THE WORKS OF THE HONOURABLE ROBERT BOYLE.

In SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE THIRD.

A NEW EDITION.

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M DCC LXXII.

THE

ORIGIN

OF

FORMS and QUALITIES,

According to the CORPUSCULAR PHILOSOPHY;

Illustrated by CONSIDERATIONS and EXPERIMENTS.

Written formerly by Way of NOTES upon an ESSAY from NITRE.

Augmented by a DISCOURSE of SUBORDINATE FORMS.

Audendum est, & veritas investiganda, quam etiamsi non assequamur, omnino tamen propriùs, quàm nunc sumus, ad eam perveniemus. GALEN.

The Publisher to the Ingenious Reader.

IN this curious and inquisitive age, when men, altogether dissatisfied and wearied out with the wranglings and idle speculations of the schools, are with equal zeal and industry so earnest in their quest and pursuit of a more solid, rational, and useful philosophy, it may prove a work very obliging and meritorious to help and guide them in their studies and researches, and to hang out a light to them (as the *Egyptians* used to do from their highly celebrated *Pharos*, for direction to the mariners that failed in those dangerous seas near *Alexandria*) whereby they may with better success steer their course through the vast ocean of learning, and make more full and perfect discoveries of hitherto unknown philosophical verities: which has been the chief design of this gentleman of honour, the most excellent and incomparable author, in this treatise now presented to your view, wherein principles are not (as was the mode and guise of former times) obtruded on the world upon the account of a great name, or involved in cloudy and mystical notions, which put the understanding upon the wreck, and yet when with all this labour and toil of the brain they are at last known, prove impertinent and useless to the making out with satisfaction, or so much as tolerably, the ordinary phænomena which nature every day presents the world with; but such as are built upon the firm and immoveable foundation of reason, sense, and experience, plain and obvious, as well to the eye, as to the understanding, and no less accurate and certain in their application. And though the most noble author hath herein, for the main, espoused the atomical philosophy (corrected and purged from the wild fancies and extravagancies of the first inventors of it, as to the origin of the universe, and still imbraced with so much kindness and tenderness by some pretenders, against which he hath so learnedly disputed in his first part *Of the Usefulness of Experimental Philo-*

sophy) in explicating the appearances; yet, considering the several alterations and additions (the happy product of his penetrating judgment) made therein, I may not scruple to call it a new hypothesis, peculiar to the author, made out by daily observations, familiar proofs and experiments, and by exact and easily practicable chymical processes; whereby one of the most abstruse parts of natural philosophy, the origin of forms and qualities, which so much vexed and puzzled the antients, and which, I would speak with the leave of the *Cartesians*, their ingenious master durst scarce venture upon, or at least was unwilling to handle at large, is now fully cleared and become manifest: so that from this very essay we may well take hope, and joyfully expect to see the noble project of the famous *Verulam* (hitherto reckoned among the *Desiderata*) receive its full and perfect accomplishment; I mean a real, useful, and experimental physiology, established and bottomed upon easy, true, and generally received principles. But I shall not forestal thy judgment either about the excellency of the author, or his subject, who hath so freely communicated to the world those treasures of learning, wherewith his mind is inriched, but shall soon refer you to the work itself, after I have given you these few advertisements.

The following discourse (as is easily perceivable by divers passages thereof) being written several years since whole and intire, as now it is, I know not whether it will be worth while to intimate, that the author casually turning over of late a very recent chymical writer found in one of his treatises (divers of which he never to this day read over) a part of the fifth experiment of the second section; but, as he professes (and sure is like to be believed) he did not dream that that chymist, or any other author whatsoever, had lighted on that part of the experiment, till a good while after he had made and examined that, among many others, concerning salts, as may be easily guessed by the peculiar uses and applications he made of it. And though he had met with so unlikely an experiment in a writer, who, whether he deserve it or no, has the ill fortune to be much accused of insincerity, and some of whose more easy processes our author (who is yet willing to spare his name, and seems to think his works not useless) could not find to succeed, he should not have taken it upon his authority, no more than he is wont to take other processes, divers of which he yet in the general supposes may be true, upon the relation of other chymists, who, by blemishing their books by things untrue and justly suspicious, are not to be relied on, nor much thanked by wary men. But it will probably appear less pertinent to add any thing further on this subject, than to take notice, that when the author had once consented to the publication of the following papers, he several times wished for an opportunity to make the experiments and observations he now presents to the publick, more full and compleat than they were when addressed to a private friend. But the contagion that drove him from the places where his accommodations for repeating experiments were, obliged him to apply himself to other studies and imployments.

And upon the same account, though he afterwards found many of his notes upon other parts of the essay of Salt-petre, and hath lying by him divers papers concerning sensible Qualities, and Sensation in general, and the Production of second Qualities, together with a collection of Notes about occult Qualities, and some other subjects of kin to those of this book; yet having, upon the freshly intimated occasion, diverted his thoughts to other subjects, he will not ingage himself to put together and communicate his collections on these subjects by any publick promise.

Furthermore, as the author has in the following disquisitions aimed not at the raising or abetting a faction in philosophy, but at the discovery of the truth; so he is not so solicitous, what every sort of reader will think of his attempts (which is easy to foresee are not like to be over-welcome to the votaries of the school-philosophy) as to refuse a compliance with the desires of his friends, who have been long since very earnest with

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him, not to spend that time in replies to particular persons, which might be more usefully imployed in pursuing further discoveries of nature by experiments. If he meet with any cogent and material objections against any of his chief opinions, he is enough a lover of truth to be disposed to think himself obliged by those who shall shew him his mistakes, and to take occasion to reform them. But if nothing new or weighty be urged, he considers, that he lives in an age, wherein he has observed (even in his own case) that truth, if recommended by real experiments, will in time make its own way, and wherein live store of ingenious men, who for the main, approve the opinions, and probably will not dislike the arguments he has proposed; and who being more at leisure than he to write polemical books, will not silently suffer what they judge truth to be triumphed over, or oppressed by those, who, imploying usually but scholastical arguments, may be confuted by answers of the like nature. And therefore he doubts not, but that some learned favourers of the corpuscularian philosophy (of which he hath endeavoured to make out those parts, wherein they almost all agree) will be both able and willing to defend those discoveries by rational disputations, that they have not opportunity to increase by new experiment.

In the mean while, I have no temptation to doubt in the least, but that this curious and excellent piece will be entertained and received by all that have any regard to the great concerns of learning, with that gust, delight, respect, and estimation, which it so highly merits.

The Author's Proemial Discourse to the Reader *.

AS it is the part of a mineralist, both to discover new mines, and to work those that are already discovered, by separating and melting the ores to reduce them into perfect metal; so I esteem, that it becomes a naturalist, not only to advise hypotheses and experiments, but to examine and improve those that are already found out. Upon this consideration (among other motives) I was invited to make the following attempt, whose productions coming to be exposed to other eyes, than those for which they were first written, it will be requisite to give the publick some account of the occasion, the scope, and some circumstances. And this I shall do the more fully, because the reasons I am to render of my way of writing, in reference to the Peripatetick philosophy, must contain intimations, which, perhaps, will not be useless to some sorts of readers (especially gentlemen) and, by being applied to most of those other parts of my writings that relate to the school-philosophy, may do them good service, and save both my readers and me some trouble of repetitions.

Having four or five years ago published a little physico-chymical tract about the different parts and redintegration of nitre, I found as well by other signs, as by the early solicitations of the stationer for a new edition, that I had no cause to complain of the reception that had been given it: but I observed too, that the discourse, consisting chiefly of reflexions that were occasionally made upon the phænomena of a single experiment, was more available to confirm those in the corpuscularian philosophy, that had already somewhat inquired into it, than to acquaint those with the principles and notions of it, who were utter strangers to it; and, as to many readers, was fitter to excite a curiosity for that philosophy, than to give an introduction thereunto. Upon this occasion it came into my mind, that about the time when I writ that essay about salt-

* The following preface being addressed only to *Pyrophilus*.

petre (which was divers years before it was publiſhed) I had alſo ſome thoughts of a hiſtory of qualities, and that having in looſe ſheets ſet down divers obſervations and experiments proper for ſuch a deſign, I had alſo drawn up a diſcourſe, which was ſo contrived, that though ſome parts of it were written in ſuch a manner, as that they may ſerve for expoſitory notes upon ſome particular paſſages of the eſſay ; yet thoſe parts with the reſt might ſerve for a general preface to the hiſtory of qualities, in caſe I ſhould ever have conveniency, as well as inclination, to make the proſecuting of it my buſineſs; and in the mean time might preſent that *Pyrophilus*, to whom I writ ſome kind of introduction to the principles of the mechanical philoſophy, by expounding to him, as far as my thoughts and experiments would enable me to do, in few words, what, according to the corpuſcularian notions, may be thought of the nature and origin of qualities and forms; the knowledge of which either makes or ſuppoſes the moſt fundamental and uſeful part of natural philoſophy. And to invite me to make uſe of theſe conſiderations and trials about qualities and forms, it opportunely happened, that though I could not find many of the notes written about particular qualities (my looſe papers having been, during the late confuſions, much ſcattered by the many removes I had then occaſion to make); yet when laſt winter, being urged to publiſh my Hiſtory of Cold (which ſoon after came forth) I rummaged among my looſe papers, I found, that the ſeveral notes of mine, that he had met with under various heads, but yet all concerning the origin of forms and qualities, together with the preface addreſſed to *Pyrophilus* (though written at diſtant times and places) had two or three years before, by the care of an induſtrious perſon, with whom I left them, been fairly copied out together (which circumſtance I mention, that the reader may not wonder to find the following book not written uniformly in one continued tenor) excepting ſome experiments, which having been of my own making, it was not difficult for me to perfect, either out of my notes and memory, or (where I doubted their ſufficiency) by repeated trials. So that if the urgency, wherewith divers ingenious men preſſed the publication of my new experiments about cold, and my unwillingneſs to protract it, till the froſty ſeaſon, that was fitteſt to examine and prove them, were all paſt, had not prevailed with me to let thoſe obſervations be made publick the laſt winter, they might have been accompanied with the preſent eſſay of the Origin of Qualities and Forms, which have been premiſed to what I have written touching any of the particular qualities, ſince it contains experiments and conſiderations fit to be preliminary to them all.

But though I was by this means diverted from putting out the following treatiſe at the ſame time with the Hiſtory of Cold, yet I was without much difficulty prevailed with not to alter my intentions of ſuffering it to come abroad; becauſe divers of my hiſtorical accounts of ſome particular qualities are to be reprinted, which may receive much light and confirmation by the things delivered in this preſent treatiſe about qualities and forms in general. To which inducement was added the perſuaſion of ſome ingenious perſons, who are pleaſed to confeſs their having received more information and ſatisfaction in theſe papers, than I durſt pretend to give them: though indeed the ſubject is ſo noble and ſo important, and does ſo much want the being illuſtrated by ſome diſtinct and experimental diſcourſe, that not only, if I did not ſuſpect my friends of partiality, I ſhould hope, that it may gratify many readers, and inſtruct more than a few; but ſuch as it is, I do not altogether deſpair, that it will prove neither unacceptable nor uſeleſs. And indeed the doctrines of forms and qualities, and generation, and corruption, and alteration, are wont to be treated of by ſcholaſtical philoſophers in ſo obſcure, ſo perplexed, and ſo unſatisfactory a way, and their diſcourſes upon theſe ſubjects do conſiſt ſo much more of logical and metaphyſical notions and niceties than of phyſical obſervations and reaſonings, that it is very difficult for any reader of but an ordinary

ordinary capacity to underftand what they mean, and no lefs difficult for any intelligent and unprejudiced reader to acquiefce in what they teach: which is oftentimes fo precarious and fo contradictious to itfelf, that moft readers (without always excepting fuch as are learned and ingenious) frighted by the darknefs and difficulties wherewith thefe fubjects have been furrounded, do not fo much as look after or read over thefe general and controverted matters, about which the fchools make fo much noife; but defpairing to find any fatisfaction in the ftudy of them, betake themfelves immediately to that part of phyficks that treats of particular bodies. So that to thefe it will not be unacceptable to have any intelligible notions offered them of thefe things, which, as they are wont to be propofed, are not wont to be underftood: though yet the fubjects themfelves, if I miftake not, may be juftly reckoned not only amongft the nobleft and moft important, but (in cafe they be duly propofed) among the ufefuleft and moft delightful fpeculations that belong to phyficks.

I CONSIDER too, that among thofe that are inclined to that philofophy, which I find I have been much imitated in calling Corpufcularian, there are many ingenious perfons, efpecially among the nobility and gentry, who having been firft drawn to like this new way of philofophy by the fight of fome experiments, which for their novelty or prettinefs they were much pleafed with, or for their ftrangenefs they admired, have afterwards delighted themfelves to make or fee variety of experiments, without having ever had the opportunity to be inftructed in the rudiments or fundamental notions of that philofophy whofe pleafing or amazing productions have enamoured them of it. And as our *Pyrophilus*, for whom thefe notes were drawn up, did in fome regards belong to this fort of virtuofi, fo it is not impoffible, but that fuch readers, as he was then, will not be forry to meet with a treatife, wherein though my chief and proper bufinefs be the giving fome account of the nature and origin of forms and qualities; yet, by reafon of the connexion and dependance betwixt thefe and divers of the other principal things that belong to the general part of phyficks, I have been obliged to touch upon fo many other important points, that this tract may in fome fort exhibit a fcheme of, or ferve for an introduction into the elements of the Corpufcularian philofophy.

AND as thofe readers, that have had the curiofity to perufe what is commonly taught in the fchools about forms, and generation, and corruption, and thofe other things we have been mentioning, and have (as is ufual among ingenious readers) quitted the ftudy of thofe unfatisfactory intricacies with difguft, will not be difpleafed to find in our notes fuch explications of thofe things, as render them at leaft intelligible; fo it will not, perhaps, prove unacceptable to fuch readers, to find thofe matters which the fchools had interwoven with *Ariftotle*'s doctrine, reconciled and accommodated to the notions of the corpufcular phyficks.

IF it be faid, that I have left divers things unmentioned, which are wont to be largely treated by the *Ariftotelians*, and particularly have omitted the difcuffion of feveral queftions, about which they are wont very folemnly and eagerly to contend; I readily acknowledge it to be true: but I anfwer further, that to do otherwife than I have done, were not agreeable to the nature of my defign, as is declared in the preface to *Pyrophilus*; and that though moft readers will not take notice of it, yet fuch as are converfant in that fort of authors, will, I prefume, eafily find that I have not left them unconfulted, but have had the curiofity to refort to feveral both of the more and of the lefs recent fcholaftical writers about phyficks, and to fome of the beft metaphyficians to boot, that I might the better inform myfelf, both what their opinions are, and upon what arguments they are grounded. But as I found thofe inquiries far more troublefome than ufeful, fo I doubt not, that my omiffions will not much difpleafe that fort of readers, for whofe fake chiefly it is that thefe papers are permitted to be made publick. For

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if I ſhould increaſe the obſcurity of the things themſelves I treat of, by adding the ſeveral obſcurer comments (rather than explications) and the perplexed and contradictious opinions I have met with among ſcholaſtic writers, I doubt that ſuch perſons, as I chiefly write for, would, inſtead of better comprehending what I ſhould ſo deliver, abſolutely forbear to read it. And there being many doctrines, to which number this we are ſpeaking of ſeems to belong, wherein the ſame innate light, or other arguments, that diſcover the truth, do likewiſe ſufficiently ſhew the erroneouſneſs of diſſenting opinions; I hope it may ſuffice to propoſe and eſtabliſh the notions that are to be imbraced, without ſolicitouſly diſproving what cannot be true, if thoſe be ſo. And indeed there are many opinions and arguments of good repute in the ſchools, which do ſo intirely rely upon the authority of *Ariſtotle*, or ſome of his more celebrated followers, that where that authority is not acknowledged, to fall upon a ſolemn confutation of what has been ſo precariouſly advanced, were not only unneceſſary, but indiſcreet, even in a diſcourſe not confined to the brevity challenged by the nature of this of ours. And there are very many queſtions and controverſies, which, though hotly and clamorouſly contended about, and indeed pertinent and fit enough to be debated in their philoſophy, do yet ſo much ſuppoſe the truth of ſeveral of their tenets, which the new philoſophers reject; or are grounded upon technical terms or forms of ſpeaking, that ſuppoſe the truth of ſuch opinions; or are expreſſions, whereof we neither do nor need make any uſe; that to have inſerted ſuch debates into ſuch a diſcourſe as mine, would have been not only tedious, but impertinent. As (for inſtance) thoſe grand diſputes, whether the four elements are endowed with diſtinct ſubſtantial forms, or have only their proper qualities in ſtead of them? and whether they remain in the mixed bodies, according to their forms, or according to their qualities? And whether the former or the latter of thoſe be or be not refracted? Theſe, I ſay, and divers other controverſies about the four elements and their manner of miſtion, are quite out of doors in their philoſophy, that acknowledge neither, that there are four elements, nor that cold, heat, dryneſs, and moiſture are, in the peripatetic ſenſe, firſt qualities, or that there are any ſuch things as ſubſtantial forms *in rerum naturâ*. And it made me the more unwilling to ſtuff theſe papers with any needleſs ſchool-controverſies, becauſe I found upon peruſal of ſeveral ſcholaſtic writers (eſpecially the recenter, who may probably be ſuppoſed to be the moſt refined) that they do not always mean the ſame things by the ſame terms, but ſome imploy them in one ſenſe, others in another, and ſometimes the ſame writer uſes them in very different ſenſes, which I am obliged to take notice of, that ſuch readers as have conſulted ſome of thoſe authors may not accuſe me of miſtaking or injuring ſome of the ſcholaſtical terms and notions he may meet with in theſe papers, when I have only imployed them in the ſenſe of other ſchool-writers, which I judged preferable. And this put me in mind of intimating, that whereas, on the contrary, I ſometimes imployed variety of terms and phraſes to expreſs the ſame thing, I did it purpoſely, though perhaps to the prejudice of my own reputation, for the advantage of *Pyrophilus*; both I and others having obſerved, that the ſame unobvious notions being ſeveral ways expreſſed, ſome readers, even among the ingenious ſort of them, will take it up much better in one of thoſe expreſſions, and ſome in another.

But perhaps it will be wondered at, even by ſome of the new philoſophers, that diſſenting ſo much as I do from *Ariſtotle* and the ſchoolmen, I ſhould overlook or decline ſome arguments, which ſome very ingenious men think to be of very great force againſt the doctrine I oppoſe. But divers of theſe arguments being ſuch, as the logicians call *ad hominem*, I thought I might well enough ſpare them. For I have obſerved *Ariſtotle* in his Phyſics to write ſo very often in ſo dark and ambiguous a way, that it is far more difficult than one would think, to be ſure what his opinion was: and the unlearned

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and too frequently jarring gloſſes of his interpreters have often made the comment darker than the text; ſo that (though in moſt it be, yet) in divers caſes it is not eaſy (eſpecially without the expence of many words) to lay open the contradictions of the Peripatetick doctrine, beſides that the urging ſuch contradictions are oftentimes fitter to ſilence an unwary adverſary, than ſatisfy a wary and judicious reader; it being very poſſible, that a man may contradict himſelf in two ſeveral places of his works, and yet not be in both of them in the wrong. For one of his aſſertions, though inconſiſtent with the other, may yet be conſiſtent with truth. But this is not all I have to ſay on this occaſion. For beſides that having, for many reaſons elſewhere mentioned, purpoſely forborne the reading of ſome very much, and, for aught I know, very juſtly eſteemed diſcourſes about general hypotheſes, it is very poſſible, that I may be a ſtranger to ſome of thoſe arguments: beſides this, I ſay, I confeſs I have purpoſely forborne to make uſe of others, which I have ſufficiently taken notice of. For ſome of thoſe ratiocinations would engage him, that ſhould imploy them, to adopt an hypotheſis or theory, in which perhaps I am not ſo thoroughly ſatisfied, and of which I do not conceive myſelf to have, on this occaſion, any neceſſity to make uſe: and accordingly I have forborn to imploy arguments, that are either grounded on, or ſuppoſe indiviſible corpuſcles called Atoms, or any innate motion belonging to them; or that the eſſence of bodies conſiſts in extenſion, or that a vacuum is impoſſible; or that there are ſuch *globuli cœleſtes*, or ſuch a *materia ſubtilis*, as the *Carteſians* imploy to explicate moſt of the phænomena of nature. For theſe and divers other notions, I (who here write rather for the Corpuſcularians in general, than any party of them) thought it improper needleſly to take in, diſcourſing either againſt thoſe to whom theſe things appears as diſputable, as the Peripatetick tenets ſeem to me; or for to ſatisfy an ingenious perſon, whom it were not fair to impoſe upon with notions that I did not myſelf think proper.

And on the like account I forbore ſuch arguments as thoſe, that ſuppoſe in nature and bodies inanimate, deſigns and paſſions proper to living, and perhaps peculiar to intelligent beings; and (ſuch as) ſome proofs that are drawn from the theology of the ſchools; (which I wiſh leſs interwoven with *Ariſtotle*'s philoſophy.) For though there be ſome things which ſeem to be of this ſort (as arguments drawn from final cauſes in divers particulars that concern animals) which in a ſound ſenſe I not only admit, but maintain; yet ſince, as they are wont to be propoſed, they are liable enough to be queſtioned, I thought it expedient for my preſent deſign to pretermit them, as things that I do not abſolutely need; though the imploying ſome of them would facilitate my taſk. And this I did the rather, becauſe I alſo forbear to anſwer arguments, that however vehemently and ſubtilly urged by many of the modern ſchoolmen of the Roman Catholick communion, are either confeſſedly, or at leaſt really built upon ſome theological tenets of theirs, which being oppoſed by the divines of other churches, and not left unqueſtioned by ſome acute ones of their own, would not be proper to be ſolemnly taken notice of by me, whoſe buſineſs in this tract is to diſcourſe of natural things as a naturaliſt, without invading the province of divines, by intermeddling with ſupernatural myſteries; ſuch as thoſe, upon which divers of the phyſico-theological tenets of the ſchoolmen, eſpecially about real qualities, and * the

* *Atque hæc ſententia* (of the diſtinction and ſeparableneſs of quantity from matter) *eſt omnino tenenda: quanquam enim non poſſit ratione naturali ſufficienter demonſtrari, tamen ex principiis theologiæ convincitur eſſe vera, maximè propter myſterium euchariſtiæ.* Suarez Diſp. Metap. 40. p. m. 341. pauciſque interjectis. —*Prima ratio pro hac ſententia eſt, quia in myſterio euchariſtiæ Deus ſeparavit quantitatem à ſubſtantiis panis & vini*, &c. Et p. m. 342.—*Hæc reſponſio & ſententia* [adverſariorum] *ſic explicata non poteſt facilè & evidenter impugnari, ſiſtendo in puro naturali; nihilominus tamen, partim ratione naturali, partim adjuncto myſterio ſufficientiſſimè improbatur.*

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ſeparableneſs of accidents from objects of inheſion, are manifeſtly, if not alſo avowedly grounded. But to return to the other things I was owning to have left unmentioned, notwithſtanding all that I have been ſaying, I readily acknowledge, that in ſome recent authors that have been imbracers of the new philoſophy, I have met with ſome paſſages that might well and pertinently be taken into the following diſcourſe, but that having been (as I formerly intimated) tranſcribed ſome years ago, I cannot now ſo conveniently alter it: which I am the leſs troubled at, becauſe theſe few additional arguments, thought fit to illuſtrate or confirm, being not neceſſary to make out what has been delivered, may ſafely be let alone, unleſs there happen (as it is not unlikely there may) an occaſion of reprinting theſe notes, with ſuch inlargements as may make them the more fit to be an introduction into the corpuſcular philoſophy.

I HOPE then, upon the whole matter, that I have pitched upon that way that was the moſt conducive to my deſign, partly by inſiſting only on thoſe opinions, whether true or falſe, which for their importance or difficulty ſeemed to deſerve to be particularly either explicated or diſproved; and partly by chuſing to employ ſuch arguments, as I thought the cleareſt and cogenteſt, and by their aſſuming the leaſt of any, ſeemed the eaſieſt to be vindicated from exceptions; without troubling myſelf to anſwer objections that appeared rather to be drawn from metaphyſical or logical ſubtilties, or to be grounded upon the authority of men, than to be phyſical ratiocinations, founded upon experience, or the nature of the things under debate; eſpecially having, in the propoſal or confirmation of the truth, ſo laid the grounds, and imitated the ways of anſwering what is like to be colourably objected againſt it, that an ingenious man may well enough furniſh himſelf with weapons to defend the truth, out of the notions, hints, and experiments, wherewith in this tract care has been taken to accompany. And by forbearing to proſecute ſome of the Peripatetick controverſies any further than I have done, will not, I hope, be blamed by them that have obſerved as well as I, how much thoſe diſputes are wont to be lengthened by ſuch frivolous diſtinctions, as do not deſerve to be ſolemnly examined, eſpecially in ſuch a compendious treatiſe as ours. For an attentive reader needs not be much converſant with the writings of the modern Peripateticks about ſuch ſubjects as ſubſtantial forms, generation, corruption, *&c.* to take notice, that it is their cuſtom, when they find themſelves diſtreſſed by a ſolid argument, to endeavour to elude it by ſome pitiful diſtinction or other; which is uſually ſo groundleſs, and ſo unintelligible, or ſo nugatory, or ſo impertinent to the ſubject, or at leaſt ſo inſufficient for the purpoſe it is alledged for, that to vouchſafe it a ſolicitous confutation might queſtion a writer's judgment with intelligent readers; who by ſuch inſignificant diſtinctions are ſatisfied of nothing ſo much, as that the framers of them had rather ſay (that which indeed amounts to) nothing, than not to ſeem to ſay ſomething. And of ſuch evaſions they may probably be emboldened to make uſe, by the practice of *Ariſtotle* himſelf, to whom ſuch obſcure and unſatisfactory diſtinctions are ſo familiar, that I remember one of his own commentators * (and he one of the moſt judicious) could not forbear, upon a certain text of his maſter's, to complain of it, and particularly to take notice, that that one diſtinction of *actu & potentia* runs through almoſt all *Ariſtotle*'s philoſophy, and is imployed to ſhift off thoſe difficulties he could not clearly explicate.

* The author here meant is the inquiſitive Peripatetick *Cabæus*, who in one place hath theſe words: *Ut hanc quæſtionem ſolvat, recurrit ad illam diſtinctionem ſibi valde familiarem, quâ utitur Ariſtoteles in tota ſua philoſophia, quoties obviam habet aliquam gravem difficultatem, diſtinguit enim actu vel potentia,* &c. In another theſe: —*Quæ eſt diſtinctio quædam familiaris Ariſtoteli, quam applicat omnibus rebus, ubi difficultates urgent, & videtur iſtis vocibus quaſi fatali gladio omnes reſcindere difficultatis nodos; vix enim eſt difficultas, cui non putat ſe ſatisfacere diſtinguendo* actu & potentiâ.

By

By which neverthelefs I would not be underftood to cenfure or decry the whole Peripatetick philofophy, much lefs to defpife *Ariftotle* himfelf; whofe own writings give me fometimes caufe a little to wonder, to find fome abfurdities fo confidently fathered upon him by his fcholaftic interpreters. For I look upon *Ariftotle* as one (though but as one amongft many) of thofe famed ancients, whofe learning about *Alexander*'s time ennobled *Greece*; and I readily allow him moft of the praifes due to great wits, excepting thofe which belong to clear-headed naturalifts. And I here declare once for all, that, where in the following tract, or any other of my writings, I do indefinitely depreciate *Ariftotle*'s doctrine, I would be underftood to fpeak of his phyficks, or rather of the fpeculative part of them (for his hiftorical writings concerning animals I much efteem) nor do I fay that even thefe may not have their ufe among fcholars, and even in univerfities, if they be retained and ftudied with due cautions and limitations (of which I have elfewhere fpoken).

But to refume the difcourfe whence the Peripatetick diftinctions tempted me to digrefs; by any thing I formerly faid I would not in the leaft difparage thofe excellent, and efpecially thofe modern authors that have profeffedly oppofed the *Ariftotelian* phyficks (fuch as *Lucretius*, *Verulam*, *Baffo*, *Des Cartes* and his followers, *Gaffendus*, the two *Boots*, *Pagnenus*, *Pemble*, *Helmont*) nor to be thought to have made no ufe of any of their cogitations or arguments. For though fome of their books I could not procure when I had occafion to have recourfe to them; and though the weaknefs of my eyes difcouraged me from perufing thofe parts of others that concerned not the fubject I was treating of, yet I hope I have been benefited by thofe I have confulted, and might have been more fo by the learned *Gaffendus*'s little, but ingenious, *Syntagma Philofophiæ Epicuri*, if I had more feafonably been acquainted with it.

But whether we have treated of the nature and origin of forms and qualities in a more comprehenfive way than others; whether we have by new and fit fimilitudes and examples and other means rendered it more intelligible than they have done; whether we have added any confiderable number of notions and arguments, towards the compleating and confirming of the propofed hypothefis; whether we have with reafon difmiffed arguments unfit to be relied on; and whether we have propofed fome notions and arguments fo warily as to keep them from being liable to exceptions or evafions, whereto they were obnoxious as others have propofed them; whether (I fay) we have done all or any of thefe in the firft or fpeculative part of this treatife, we willingly leave the reader to judge. But in the fecond or hiftorical part of it perhaps he will be invited to grant that we have done that part of phyficks we have been treating of fome little fervice; fince by the lovers of real learning it was very much wifhed that the doctrines of the new philofophy (as it is called) were backed by particular experiments; the want of which I have endeavoured to fupply, by annexing fome whofe nature and novelty, I am made believe, will render them as well acceptable as inftructive. For though, that I might not anticipate what belongs to other papers, I did not make the laft fection confift of above a decad of them; and though, for the reafons intimated in the advertifements premifed to them, I did not exprefly mention to *Pyrophilus* all that I could have told him about them; yet I have been careful fo to chufe them, and to interweave hints in delivering them, that a fagacious reader who fhall have the curiofity to try them heedfully, and make reflections on the feveral phænomena that in likelihood will occur to him, will (if I miftake not) receive no contemptible information, as of fome other things fo particularly about the nature of mixtions (which I take to be one of the moft important and ufeful, though neglected and ill underftood doctrines of the practical part of phyficks) and may probably light upon more than he expects, or I have fully delivered, and perhaps too more than I forefaw.

And though some virtuosi, more conversant perhaps with things than books, presuming the decay of the Peripatetick philosophy to be every where as great as it is among them in *England*, may think that a doctrine which they look on as expiring, need not have been so solicitously confuted; yet those that know how deep rooting this philosophy has taken (both elsewhere, and particularly) in those academies where it has flourished for many ages, and in some of which it is, exclusively to the mechanical philosophy, watered and fenced by their statutes or their superiors: and he that also knows how much more easy some (more subtile than candid) wits find it plausibly to defend an error, than ingenuously to confess it, will not wonder that I should think that a doctrine so advantaged, though it be too erroneous to be feared, is yet too considerable to be despised. And not to question whether several of those that most contemn the favourers of the Peripatetick hypothesis, as the later discoveries have reduced them to reform it, be not the least provided to answer their arguments (not to question this, I say) there are divers of our adversaries (misled only by education and morally harmless prejudices) who do so much deserve a better cause, than that which needs all their subtilty, without being worthy of it, that I shall think more pains than I have taken very usefully bestowed, if my arguments and experiments prove so happy as to undeceive persons whose parts, too unluckily confined to narrow and fruitless notions, would render them illustrious champions for the truths they are able so subtily to oppose; and who might questionless perform considerable things, if they imployed as much dexterity to expound the mysteries of nature, as the riddles of the schoolmen, and laid out their wit and industry to surmount the obscurity of her works, instead of that of *Aristotle*'s.

There might be a few other particulars fit to be taken notice of in this preface, but finding that I had already mentioned them in that which I had addressed to *Pyrophilus*, my haste makes me willing rather to refer the reader thither for them, than alter that, or lengthen this (which I should think much too long already, if it were not possible that it may hereafter prove preliminary to more papers than these it is now premised to). So that there remains but one advertisement necessary to be given here, namely, that whereas in the following notes I several times speak of the author of the essay of salt-petre, as of a third person, the occasion of that was, that when these notes, and some about particular qualities, were written, I had a design to make two distinct sorts of annotations upon that essay; in the former whereof (which now comes forth) I assumed the person of a Corpuscularian, and discoursed at that rate. But I had thoughts too (in case God were pleased to grant me life and opportunity) to take a second review both of the treatise itself, and of the notes on it, and on that occasion to add what my riper thoughts and further experience might suggest unto me. And that in my animadversions I might with the more freedom and conveniency add, explain, alter, and even retract, as I should see cause, I thought it not amiss to write them, as if they were made on the work of another. By which intimation the reader may be assisted to guess how much I intended in the following discourse (in which, as in the prefaces belonging to it, I play the Corpuscularian) to reserve myself the freedom of questioning and correcting, upon the designed review, any thing delivered in these notes; and how much more it was in them my design to bring *Pyrophilus* experiments and queries to illustrate obscure matters, than by hasty assertions to dogmatize about them.

THE

THE PREFACE.

THE origin, *Pyrophilus*, and nature of the qualities of bodies, is a ſubject that I have long looked upon as one of the moſt important and uſeful that the naturaliſt can pitch upon for his contemplation. For the knowledge we have of the bodies without us, being for the moſt part fetched from the informations the mind receives by the ſenſes, we ſcarce know any thing elſe in bodies, upon whoſe account they can work upon our ſenſes, ſave their qualities: for as to the ſubſtantial forms which ſome imagine to be in all natural bodies, it is not half ſo evident that there are ſuch as it is, that the wiſeſt of thoſe that do admit them confeſs, that they do not well know them *. And as it is by their qualities that bodies act immediately upon our ſenſes, ſo it is by virtue of thoſe attributes likewiſe that they act upon other bodies, and by that action produce in them, and oftentimes in themſelves, thoſe changes that ſometimes we call alterations, and ſometimes generation or corruption.

AND it is chiefly by the knowledge, ſuch as it is, that experience (not art) hath taught us of theſe differing qualities of bodies, that we are enabled, by a due application of agents to patients, to exerciſe the little empire that we have either acquired or regained over the creatures. But I think not the contemplation of qualities more noble and uſeful than I find it difficult; for what is wont to be taught us of qualities in the ſchools, is ſo ſlight and ill-grounded, that it may be doubted whether they have not rather obſcured than illuſtrated the things they ſhould have explained. And I was quickly diſcouraged from expecting to learn much from them of the nature of divers particular qualities, when I found that, except ſome few, which they tell you in general may be deduced (by ways they leave thoſe to gueſs at, that can) from thoſe four qualities, they are pleaſed to call the firſt, they confeſs that the reſt ſpring from thoſe forms of bodies, whoſe particular natures the judiciouſeſt of them acknowledge they cannot comprehend. And *Ariſtotle* himſelf not only doth (as we ſhall ſee anon) give us of quality in general (which yet ſeems far more eaſily definable than many a particular quality) no other than ſuch a definition, as is as obſcure as the thing to be declared by it; but I obſerve, not without ſome wonder, that in his eight books of Phyſicks, where he profeſſedly treats of the general affections of natural things, he leaves out the doctrine of qualities; as after him *Magirus* and divers other writers of the Peripatetick phyſiology have done: which (by the way) I cannot but look upon as an omiſſion, ſince qualities do as well ſeem to belong to natural bodies generally conſidered, as place, time, motion, and thoſe other things, which upon that account are wont to be treated of in the general part of natural philoſophy. The moſt ingenious *Des Cartes* has ſomething concerning ſome qualities; but though, for reaſons elſewhere expreſſed, I have purpoſely forborne to peruſe his ſyſtem of philoſophy, yet I find by turning over the leaves that he has left moſt of the other qualities untreated of; and of thoſe that are more properly called ſenſible, he ſpeaks but very briefly and generally, rather conſidering what they do upon the organs of ſenſe, than what changes happen in the objects themſelves, to make them cauſe in us a perception ſometimes of one quality and ſometimes of another. Beſides that his explications do many of them ſo depend upon his peculiar no-

* *Nego tibi ullam eſſe formam nobis notam plenè & planè; noſtramque ſcientiam eſſe umbram in ſole.* *Scaliger* (of whoſe confeſſion to the ſame purpoſe more are cited hereafter).

tions (of a *materia subtilis*, *globuli secundi elementi* and the like) and these, as it became so great a person, he has so interwoven with the rest of his hypothesis, that they can seldom be made use of without adopting his whole philosophy. *Epicurus* indeed, and his scholiast *Lucretius*, have given some good hints concerning the nature of some few qualities. But, besides that even these explications are divers of them either doubtful or imperfect, or both, there are many other qualities which are left for others to treat of. And this is the second and main difficulty which I find in investigating the nature of qualities; namely, that whatever be to be thought of the general theories of *Aristotle* or other philosophers concerning qualities, we evidently want that upon which a theory, to be solid and useful, must be built; I mean an experimental history of them. And this we so want, that except perhaps what mathematicians have done concerning sounds, and the observations (rather than experiments) that our illustrious *Verulam* hath (in some few pages) said of heat in his short *Essay de Formâ Calidi*; I know not any one quality of which any author has yet given us an any-thing competent history. These things I mention to you, *Pyrophilus*, not at all to derogate from those great men whose design seems rather to have been to deliver principles and summaries of philosophy, than to insist upon particulars; but for this purpose, that since the nature of qualities is so beneficial a speculation, my labours may not be looked upon as wholly useless, though I can contribute but a little to the clearing of it; and that since it is so abstruse a subject, I may be pardoned if I sometimes miss the mark, and leave divers things uncompleated; that being but what such great philosophers have done before me.

But, *Pyrophilus*, before I proceed to give you my notes upon this part of our author's essay, that you may rightly understand my intention in them, it will be requisite to give you three or four advertisements.

And first, Whenever I shall speak indefinitely of substantial forms, I would always be understood to except the reasonable soul that is said to inform the human body, which declaration I here desire may be taken notice of once for all.

Secondly, Nor am I willing to treat of the origin of qualities in beasts; partly because I would not be ingaged to examine of what nature their souls are, and partly because it is difficult in most cases (at least for one that is compassionate enough) either to make experiments upon living animals, or to judge what influence their life may have upon the change of qualities produced by such experiments.

Thirdly, The occasion of the following reflections being only this, that our author, in that part of his essay concerning salt-petre, whereto these notes refer, does briefly intimate some notions about the nature and origin of qualities; you must not expect that I, whose method leads me but to write some notes upon this and some other parts of this essay, should make solemn or elaborate discourses concerning the nature of particular qualities, and that I should fully deliver my own apprehensions concerning those subjects. For, as I elsewhere sufficiently intimate, that in these first notes I write as a Corpuscularian, and set down those things only that seem to have a tendency to illustrate or countenance the notions or fancies implied in our author's essay; so I must here tell you, that I neither have now the leisure, nor pretend to the skill, to deliver fully the history, or to explicate particularly the nature of each several quality.

Fourthly, But I consider that the schools have of late much amused the world with a way they have got of referring all natural effects to certain entities, that they call real qualities, and accordingly attribute to them a nature distinct from the modification of the matter they belong to, and in some cases separable from all matter whatsoever; by which means they have, as far forth as their doctrine is acquiesced in, made it thought needless or hopeless for men to imploy their industry in searching into the nature of particular qualities and their effects. As if (for instance) it be demanded how snow comes

to

to dazzle the eyes, they will anſwer, that it is by a quality of whiteneſs that is in it which makes all very white bodies produce the ſame effect: and if you aſk what this whiteneſs is, they will tell you no more in ſubſtance, than that it is a real entity which denominates the parcel of matter to which it is joined, white; and if you further inquire what this real entity, which they call a quality, is, you will find, as we ſhall ſee anon, that they either ſpeak of it much after the ſame rate that they do of their ſubſtantial forms (as indeed ſome of the moderneſt teach, that a quality affects the matter it belongs to *per modum formæ ſecundariæ*, as they ſpeak) or at leaſt they will not explicate it more intelligibly.

AND accordingly, if you further aſk them how white bodies in general do rather produce this effect of dazzling the eyes, than green or blue ones, inſtead of being told that the former ſort of bodies reflect outwards, and ſo to the eye far more of the incident light than the latter; you ſhall perchance be told that it is their reſpective natures ſo to act. By which way of diſpatching difficulties they make it very eaſy to ſolve all the phænomena of nature in general, but make men think it impoſſible to explicate almoſt any of them in particular.

AND though the unſatisfactorineſs and barrenneſs of the ſchool-philoſophy have perſuaded a great many learned men, eſpecially phyſicians, to ſubſtitute the chymiſts three principles inſtead of thoſe of the ſchools; and though I have a very good opinion of chymiſtry itſelf, as it is a practical art; yet, as it is by chymiſts pretended to contain a ſyſtem of theorical principles of philoſophy, I fear it will afford but a very little ſatiſfaction to a ſevere inquirer into the nature of qualities. For beſides that, as we ſhall more particularly ſee anon, there are many qualities which cannot with any probability be deduced from any of the three principles; thoſe that are aſcribed to one or other of them cannot intelligibly be explicated without recourſe to the more comprehenſive principles of the Corpuſcularian philoſophy: to tell us, for inſtance, that all ſolidity proceeds from ſalt, only informing us (where it can plauſibly be pretended) in what material principle or ingredient that quality reſides, not how it is produced; for this doth not teach us (for example) how water even in exactly-cloſed veſſels comes to be frozen into ice; that is, turned from a fluid to a ſolid body, without the acceſſion of a ſaline ingredient (which I have not yet found pretended) eſpecially glaſs being held impervious to ſalts). Wherefore, *Pyrophilus*, I thought it might much conduce to the underſtanding the nature of qualities, to ſhew how they are generated; and by the ſame way I hoped it might remove in ſome meaſure the obſtacle, that theſe dark and narrow theories of the Peripateticks and Chymiſts may prove to the advancement of ſolid and uſeful philoſophy. That then which I chiefly aim at, is to make it probable to you by experiments (which I think hath not yet been done) that almoſt all ſorts of qualities, moſt of which have been by the ſchools either left unexplicated, or generally referred to I know not what incomprehenſible ſubſtantial forms, may be produced mechanically; I mean by ſuch corporeal agents, as do not appear either to work otherwiſe than by virtue of the motion, ſize, figure, and contrivance of their own parts (which attributes I call the mechanical affections of matter, becauſe to them men willingly refer the various operations of mechanical engines): or to produce the new qualities exhibited by thoſe bodies, their action changes by any other way, than by changing the texture or motion, or ſome other mechanical affection of the body wrought upon. And this if I can in any paſſable meaſure do, though but in a general way, in ſome or other of each of theſe three ſorts into which the Peripateticks are wont to divide the qualities of bodies, I hope I ſhall have done no uſeleſs piece of ſervice to natural philoſophy; partly by exciting you and your learned friends to inquire after more intelligible and ſatisfactory ways of explicating qualities, and partly by beginning ſuch a collection of materials towards the hiftory of thofe qualities, that I fhall the moft largely infift on, as heat, colours, fluidity, and firmnefs, as may invite you and other ingenious men to contribute alfo their experiments and obfervations to fo ufeful a work, and thereby lay a foundation whereon you, and perhaps I, may fuperftruct a more diftinct and explicite theory of qualities than I fhall at prefent adventure at. And though I know that fome of the things my experiments tend to manifeft, may likewife be confirmed by the more obvious phænomena of nature, yet I prefume you will not diflike my chufing to entertain you with the former (though without at all defpifing, or fo much as ftrictly forbearing to imploy the latter) becaufe the changes of qualities made by our experiments will, for the moft part, be more quick and confpicuous; and the agents made ufe of to produce them being of our own applying, and oftentimes of our own preparation, we may be therefore affifted the better to judge of what they are, and to make an eftimate of what it is they do.

CONSIDERATIONS AND EXPERIMENTS TOUCHING THE ORIGIN of FORMS and QUALITIES.

THE THEORICAL PART.

THAT, before I defcend to particulars, I may, *Pyrophilus*, furnifh you with fome general apprehenfion of the doctrine (or rather the hypothefis) which is to be collated with, and to be either confirmed or difproved by the hiftorical truths that will be delivered concerning particular qualities (and forms); I will affume the perfon of a Corpufcularian, and here at the entrance give you (in a general way) a brief account of the hypothefis itfelf, as it concerns the origin of qualities (and forms); and for diftinction's fake, I fhall comprize it in the eight following particulars, which, that the whole fcheme may be the better comprehended, and as it were furveyed under one profpect, I fhall do little more than barely propofe them that either feem evident enough by their own light, or may without prejudice have divers of their proofs referved for proper places in the following part of this treatife. And though there be fome other particulars to which the importance of the fubjects, and the greatnefs of the (almoft univerfal) prejudices that lie againft them, will oblige me immediately to annex (for the feafonable clearing and juftifying of them) fome annotations; yet that they

they may, as little as I can, obſcure the coherence of the whole diſcourſe, as much of them as conveniently may be ſhall be included in [ ] paratheſes.

I. I AGREE with the generality of philoſophers ſo far as to allow, that there is one catholick or univerſal matter common to all bodies, by which I mean a ſubſtance extended, diviſible, and impenetrable.

II. BUT becauſe this matter being in its own nature but one, the diverſity we ſee in bodies muſt neceſſarily ariſe from ſomewhat elſe than the matter they conſiſt of. And ſince we ſee not how there could be any change in matter, if all its (actual or deſignable) parts were perpetually at reſt among themſelves, it will follow, that to diſcriminate the catholick matter into variety of natural bodies, it muſt have motion in ſome or all its deſignable parts: and that motion muſt have various tendencies, that which is in this part of the matter tending one way, and that which is in that part tending another; as we plainly ſee in the univerſe or general maſs of matter, there is really a great quantity of motion, and that variouſly determined, and that yet divers portions of matter are at reſt.

THAT there is local motion in many parts of matter is manifeſt to ſenſe; but how matter came by this motion was of old, and is ſtill hotly diſputed of: for the antient Corpuſcularian philoſophers (whoſe doctrine in moſt other points, though not in all, we are moſt inclinable to) not acknowledging an Author of the univerſe, were thereby reduced to make motion congenite to matter, and conſequently coeval with it. But ſince local motion, or an endeavour at it, is not included in the nature of matter, which is as much matter when it reſts as when it moves; and ſince we ſee that the ſame portion of matter may from motion be reduced to reſt, and after it hath continued at reſt, as long as other bodies do not put it out of that ſtate, may by external agents be ſet a moving again; I, who am not wont to think a man the worſe naturaliſt for not being an atheiſt, ſhall not ſcruple to ſay with an eminent philoſopher of old, whom I find to have propoſed among the *Greeks* that opinion (for the main) that the excellent *Des Cartes* has revived amongſt us, that the origin of motion in matter is from God; and not only ſo, but that thinking it very unfit to be believed that matter barely put into motion, and then left to itſelf, ſhould caſually conſtitute this beautiful and orderly world: I think alſo further, that the wiſe Author of things did, by eſtabliſhing the laws of motion among bodies, and by guiding the firſt motions of the ſmall parts of matter, bring them to convene after the manner requiſite to compoſe the world, and eſpecially did contrive thoſe curious and elaborate engines, the bodies of living creatures, endowing moſt of them with a power of propagating their ſpecies. But though theſe things are my perſuaſions, yet, becauſe they are not neceſſary to be ſuppoſed here, where I do not pretend to deliver any compleat diſcourſe of the principles of natural philoſophy, but only to touch upon ſuch notions as are requiſite to explicate the origin of qualities and forms, I ſhall paſs on to what remains, as ſoon as I have taken notice that local motion ſeems to be indeed the principal amongſt ſecond cauſes, and the grand agent of all that happens in nature: for though bulk, figure, reſt, ſituation, and texture do concur to the phænomena of nature, yet in compariſon of motion they ſeem to be in many caſes, effects, and in many others little better than conditions, or requiſites, or cauſes *ſine quibus non*, which modify the operation that one part of matter by virtue of its motion hath upon another; as in a watch, the number, the figure, and coaptation of the wheels and other parts is requiſite to the ſhewing the hour, and doing the other things that may be performed by the watch; but till theſe parts be actually put into motion, all their other affections remain inefficacious. And ſo in a key, though it were too big or too little, or if its ſhape were incongruous to that of the cavity of the lock, it would be unfit to be uſed as a key though it were put into motion; yet, let its bigneſs and figure

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be never ſo fit, unleſs actual motion intervene, it will never lock or unlock any thing, as without the like actual motion, neither a knife nor razor will actually cut, how much ſoever their ſhape and other qualities may fit them to do ſo. And ſo brimſtone, what diſpoſition of parts ſoever it have to be turned into flame, would never be kindled, unleſs ſome actual fire or other parcel of vehemently and variouſly agitated matter ſhould put the ſulphureous corpuſcles into a very briſk motion.

III. THESE two grand and moſt catholick principles of bodies, matter and motion, being thus eſtabliſhed, it will follow, both that matter muſt be actually divided into parts, that being the genuine effect of variouſly determined motion, and that each of the primitive fragments, or other diſtinct and intire maſſes of matter, muſt have two attributes, its own magnitude, or rather ſize, and its own figure or ſhape. And ſince experience ſhews us (eſpecially that which is afforded us by chymical operations, in many of which matter is divided into parts too ſmall to be ſingly ſenſible) that this diviſion of matter is frequently made into inſenſible corpuſcles or particles, we may conclude, that the minuteſt fragments, as well as the biggeſt maſſes of the univerſal matter, are likewiſe endowed each with its peculiar bulk and ſhape. For being a finite body, its dimenſions muſt be terminated and meaſurable: and though it may change its figure, yet for the ſame reaſon it muſt neceſſarily have ſome figure or other. So that now we have found out, and muſt admit three eſſential properties of each intire or undivided, though inſenſible part of matter; namely, magnitude (by which I mean not quantity in general, but a determined quantity, which we in *Engliſh* oftentimes call the ſize of a body) ſhape, and either motion or reſt (for betwixt them two there is no mean): the two firſt of which may be called inſeparable accidents of each diſtinct part of matter; inſeparable, becauſe being extended, and yet finite, it is phyſically impoſſible that it ſhould be devoid of ſome bulk or other, and ſome determinate ſhape or other; and yet accidents, becauſe that whether or no the ſhape can by phyſical agents be altered, or the body ſubdivided, yet mentally both the one and the other may be done, the whole eſſence of matter remaining undeſtroyed.

WHETHER theſe accidents may not conveniently enough be called the moods or primary affections of bodies, to diſtinguiſh them from thoſe leſs ſimple qualities (as colours, taſtes, and odours) that belong to bodies upon their account; or whether, with the *Epicureans*, they may not be called the conjuncts of the ſmalleſt parts of matter, I ſhall not now ſtay to conſider: but one thing the modern ſchools are wont to teach concerning accidents, is too repugnant to our preſent doctrine to be in this place quite omitted; namely, that there are in natural bodies ſtore of real qualities and other real accidents, which not only are no moods of matter, but are real entities diſtinct from it, and, according to the doctrine of many modern ſchoolmen, may exiſt ſeparate from all matter whatſoever. To clear this point a little, we muſt take notice, that accident is among logicians and philoſophers uſed in two ſeveral ſenſes; for ſometimes it is oppoſed to the fourth predicable (property) and is then defined; That, which may be preſent or abſent without the deſtruction of the ſubject; as a man may be ſick or well, and a wall white or not white, and yet the one be ſtill a man, the other a wall: and this is called in the ſchools *accidens prædicabile*, to diſtinguiſh it from what they call *accidens prædicamentale*, which is oppoſed to ſubſtance: for when things are divided by logicians into ten predicaments or higheſt genus's of things, ſubſtance making one of them, all the nine other are of accidents. And as ſubſtance is commonly defined to be a thing that ſubſiſts of itſelf, and is the ſubject of accidents (or more plainly a real entity or thing that needs not any (created) being, that it may exiſt): ſo an accident is ſaid commonly to be *id cujus eſſe eſt in eſſe*; and therefore *Ariſtotle*, who uſually calls ſubſtances ſimply ὄντα, *entities*, moſt commonly calls accidents ὄντος ὄντα, *entities of entities*; theſe needing

needing the exiſtence of ſome ſubſtance or other, in which they may be, as in their ſubject of inheſion. And becauſe logicians make it the diſcriminating note of ſubſtance and accident, that the former is a thing that cannot be in another, as in its ſubject of inheſion, it is requiſite to know, that, according to them, that is ſaid to be in a ſubject which hath theſe three conditions; That however it (1) be in another thing (2) is not in it as a part, and (3) cannot exiſt ſeparately from the thing or ſubject wherein it is: as a white wall is the ſubject of inheſion of the whiteneſs we ſee in it, which ſelf-ſame whiteneſs, though it be not in the wall as a part of it, yet cannot the ſelf-ſame whiteneſs, according to our logicians, exiſt any where out of the wall, though many other bodies may have the like degree of whiteneſs. This premiſed, it will not be hard to diſcover the falſity of the lately mentioned ſcholaſtick opinion touching real qualities and accidents, their doctrine about which does, I confeſs, appear to me to be either unintelligible, or manifeſtly contradictious. For ſpeaking in a phyſical ſenſe, if they will not allow theſe accidents to be modes of matter, but entities really diſtinct from it, and in ſome caſes ſeparable from all matter, they make them indeed accidents in name, but repreſent them under ſuch a notion as belongs only to ſubſtances; the nature of a ſubſtance conſiſting in this, that it can ſubſiſt of itſelf without being in any thing elſe, as in a ſubject of inheſion. So that to tell us that a quality or other accident may ſubſiſt without a ſubject, is indeed, whatever they pleaſe to call it, to allow it the true nature of ſubſtance; nor will their groundleſs diſtinctions do any more than keep them from ſeeming to contradict themſelves in words, whilſt unprepoſſeſſed perſons ſee that they do it in effect. Nor could I ever find it intelligibly made out what theſe real qualities may be that they deny to be either matter or modes of matter, or immaterial ſubſtances. When a bowl runs along or lies ſtill, that motion or reſt, or globous figure of the bowl is not nothing, and yet is not any part of the bowl; whoſe whole ſubſtance would remain, though it wanted which you pleaſe of theſe accidents: and to make them real and phyſical entities (for we have not here to do either with logical or metaphyſical ones) is, as if, becauſe we may conſider the ſame man ſitting, ſtanding, running, thirſty, hungry, weary, *&c.* we ſhould make each of theſe a diſtinct entity, as we do give ſome of them (as hunger, wearineſs, *&c.*) diſtinct names; whereas the ſubject of all theſe qualities is but the ſame man, as he is conſidered with circumſtances that make him appear different in one caſe from what he appears in the other: and it may be very uſeful to our preſent ſcope to obſerve, that not only diverſity of names, but even diverſity of definitions, doth not always infer a diverſity of phyſical entities in the ſubject whereunto they are attributed. For it happens in many of the phyſical attributes of a body, as in thoſe other caſes wherein a man that is a father, a huſband, a maſter, a prince, *&c.* may have a peculiar definition (ſuch as the nature of the thing will bear) belong unto him in each of theſe capacities; and yet the man in himſelf conſidered is but the ſame man, who, in reſpect of differing capacities, or relations to other things, is called by differing names, and deſcribed by various definitions, which yet (as I was ſaying) conclude not ſo many real and diſtinct entities in the perſon ſo variouſly denominated,

An EXCURSION about the relative Nature of PHYSICAL QUALITIES.

BUT, because I take this notion to be of no ſmall importance towards the avoiding of the grand miſtake that hath hitherto obtained about the nature of qualities, it will be worth while to illuſtrate it a little farther. We may conſider then that when *Tubal-Cain*, or whoever elſe were the ſmith that invented locks and keys, had made his firſt lock (for we may reaſonably ſuppoſe him to have made that before the key, though the compariſon be made uſe of without that ſuppoſition) that was only a piece of iron contrived into ſuch a ſhape; and when afterwards he made a key to that lock, that alſo in itſelf conſidered was nothing but a piece of iron of ſuch a determinate figure: but in regard that theſe two pieces of iron might now be applied to one another after a certain manner, and that there was a congruity betwixt the wards of the lock and thoſe of the key, the lock and the key did each of them now obtain a new capacity; and it became a main part of the notion and deſcription of a lock, that it was capable of being made to lock or unlock by that piece of iron we call a key, and it was looked upon as a peculiar faculty and power in the key, that it was fitted to open and ſhut the lock; and yet by theſe new attributes there was not added any real or phyſical entity either to the lock or to the key, each of them remaining indeed nothing but the ſame piece of iron, juſt ſo ſhaped, as it was before. And when our ſmith made other keys of different bigneſſes or with different wards, though the firſt lock was not to be opened by any of thoſe keys, yet that indiſpoſition, however it might be conſidered as a peculiar power of reſiſting this or that key, and might ſerve to diſcriminate it ſufficiently from the locks thoſe keys belonged to, was nothing new in the lock, or diſtinct from the figure it had before thoſe keys were made. To carry this compariſon a little farther, let me add, that though one that would have defined the firſt lock and the firſt key would have given them diſtinct definitions with reference to each other; and yet (as I was ſaying) theſe definitions being given but upon the ſcore of certain reſpects, which the defined bodies had one to another, would not infer that theſe two iron inſtruments did phyſically differ otherwiſe than in the figure, ſize, or contrivement of the iron whereof each of them conſiſted. And proportionably hereunto, I do not ſee why we may not conceive, that as to thoſe qualities (for inſtance) which we call ſenſible, though, by virtue of a certain congruity or incongruity in point of figure (or texture or other mechanical attributes) to our ſenſories, the portions of matter they modify are enabled to produce various effects, upon whoſe account we make bodies to be endowed with qualities; yet they are not in the bodies that are endowed with them, any real or diſtinct entities, or differing from the matter itſelf, furniſhed with ſuch a determinate bigneſs, ſhape, or other mechanical modifications. Thus, though the modern goldſmiths and refiners reckon amongſt the moſt diſtinguiſhing qualities of gold, by which men may be certain of its being true and not ſophiſticated, that it is eaſily diſſoluble in aqua regis, and that aqua fortis will not work upon it; yet theſe attributes are not in the gold any thing diſtinct from its peculiar texture, nor is the gold we have now of any other nature than it was in *Pliny*'s time, when aqua fortis and aqua regis had not been found out (at leaſt in theſe parts of the world) and were utterly unknown to the *Roman* goldſmiths. And this example I have the rather pitched upon, becauſe it affords me an opportunity to repreſent, that unleſs we admit the doctrine I have been propoſing, we muſt admit, that a body may have an almoſt infinite number of new real entities accruing

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to it without the intervention of any physical change in the body itself. As for example, gold was the same natural body immediately before aqua regis and aqua fortis were first made, as it was immediately after; and yet now it is reckoned amongst its principal properties, that it is dissoluble by the former of those two menstruums, and that it is not like other metals dissoluble or corrodible by the latter. And if one should invent another menstruum (as possibly I may think myself master of such a one) that will but in part dissolve pure gold, and change some part of it into another metalline body, there will then arise another new property, whereby to distinguish that from other metals; and yet the nature of gold is not a whit other now than it was before this last menstruum was first made. There are some bodies not cathartick nor sudorifick, with some of which gold being joined acquires a purgative virtue, and with others a power to procure sweat; and, in a word, nature herself doth sometimes otherwise, and sometimes by chance produce so many things that have new relations unto others: and art, especially assisted by chymistry, may, by variously dissipating natural bodies, or compounding either them or their constituent parts with one another, make such an innumerable company of new productions, that will each of them have new operations either immediately upon our sensories, or upon other bodies whose changes we are able to perceive, that no man can know but that the most familiar bodies may have multitudes of qualities that he dreams not of; and a considering man will hardly imagine that so numerous a croud of real physical entities can accrue to a body, whilst in the judgment of all our senses it remains unchanged and the same that it was before.

To clear this a little farther, we may add, that beaten glass is commonly reckoned among poisons; and (to skip what is mentioned out of *Sanctorius* of the dysentery procured by the fragments of it) I remember * *Cardan* hath a story, that in a cloister where he had a patient then like to die of torments in the stomach, two other nuns had been already killed by a distracted woman that, having casually got free, had mixed beaten glass with peas that were eaten by these three and divers others of the sisters (who yet escaped unharmed). Now though the powers of poisons be not only looked upon as real qualities, but are reckoned amongst the abstrusest ones; yet this deleterious faculty, which is supposed to be a peculiar and superadded entity in the beaten glass, is really nothing distinct from the glass itself (which, though a concrete made up of those innocent ingredients, salt and ashes, is yet a hard and stiff body) as it is furnished with that determinate bigness and figure of parts which have been acquired by comminution; for these glassy fragments being many and rigid, and somewhat small (without yet being so small as dust) and endowed with sharp points and cutting edges, are enabled by these mechanical affections to pierce or wound the tender membranes of the stomach and guts, and cut the slender vessels that they may meet with there; whereby naturally ensue great gripings and contorsions of the injured parts, and oftentimes bloody fluxes, occasioned by the perforation of the capillary arteries and the great irritation of the expulsive faculty, and sometimes also not only horrid convulsions, by consent of the brain and cerebellum with some of the nervous or membranous parts that happen to be hurt, but also dropsies, occasioned by the great loss of blood we were just now speaking of. And it agrees very well with this conjecture, that beaten glass hath divers times been observed to have done no mischief to animals that have swallowed it: for there is no reason it should, in case the corpuscles of the powder either chance to be so small as not to be fit to wound the guts, which are usually lined with a slimy substance, wherein very minute powders may be as it were sheathed, and by that means hindered from hurting the guts (insomuch that a fragment of glass with three

* *Cardan. Contradict.* 9. lib. 2. tract. 5. *apud Schenkium.*

An EXCURSION about the relative Nature of Physical Qualities.

BUT, becaufe I take this notion to be of no fmall importance towards the avoiding of the grand miftake that hath hitherto obtained about the nature of qualities, it will be worth while to illuftrate it a little farther. We may confider then that when *Tubal-Cain*, or whoever elfe were the fmith that invented locks and keys, had made his firft lock (for we may reafonably fuppofe him to have made that before the key, though the comparifon be made ufe of without that fuppofition) that was only a piece of iron contrived into fuch a fhape; and when afterwards he made a key to that lock, that alfo in itfelf confidered was nothing but a piece of iron of fuch a determinate figure: but in regard that thefe two pieces of iron might now be applied to one another after a certain manner, and that there was a congruity betwixt the wards of the lock and thofe of the key, the lock and the key did each of them now obtain a new capacity; and it became a main part of the notion and defcription of a lock, that it was capable of being made to lock or unlock by that piece of iron we call a key, and it was looked upon as a peculiar faculty and power in the key, that it was fitted to open and fhut the lock; and yet by thefe new attributes there was not added any real or phyfical entity either to the lock or to the key, each of them remaining indeed nothing but the fame piece of iron, juft fo fhaped, as it was before. 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And proportionably hereunto, I do not fee why we may not conceive, that as to thofe qualities (for inftance) which we call fenfible, though, by virtue of a certain congruity or incongruity in point of figure (or texture or other mechanical attributes) to our fenfories, the portions of matter they modify are enabled to produce various effects, upon whofe account we make bodies to be endowed with qualities; yet they are not in the bodies that are endowed with them, any real or diftinct entities, or differing from the matter itfelf, furnifhed with fuch a determinate bignefs, fhape, or other mechanical modifications. 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* *Cardan. Contradict.* 9. lib. 2. tract. 5. *apud Schenkium.*

very ſharp corners hath been obſerved to have for above eighteen months * lain inoffenſive even in a nervous and very ſenſible part of the body) out of which they may, with the groſſer excrements of the lower belly, be harmleſly excluded, eſpecially in ſome individuals, whoſe guts and ſtomach too may be of a much ſtronger texture and better lined or ſtuffed with groſs and ſlimy matter than thoſe of others. And accordingly we ſee that the fragments of ſaphires, cryſtals, and even rubies, which are much harder than glaſs, are innocently, though perhaps not very effectually, uſed by phyſicians (and I have ſeveral times taken that without inconvenience) in cordial compoſitions, becauſe of their being by grinding reduced to a powder too ſubtile to excoriate or grate upon the ſtomach or guts; and probably it was upon ſome ſuch account that that happened which is related by *Cardan* in the ſame place; namely, that though the three nuns we have been ſpeaking of were poiſoned by the glaſs, yet many others who eat of the other portions of the ſame mingled peas received no miſchief thereby. (But of this ſubject more † elſewhere).

And this puts me in mind to add, that the multiplicity of qualities that are ſometimes to be met with in the ſame natural bodies needs not make men reject the opinion we have been propoſing, by perſuading them that ſo many differing attributes as may be ſometimes found in one and the ſame natural body, cannot proceed from the bare texture and other mechanical affections of its matter. For we muſt conſider each body, not barely, as it is in itſelf, an entire and diſtinct portion of matter, but as it is a part of the univerſe, and conſequently placed among a great number and variety of other bodies, upon which it may act; and by which it may be acted on, in many ways (or upon many accounts) each of which men are wont to fancy as a diſtinct power or quality in the body by which thoſe actions, or in which thoſe paſſions, are produced. For if we thus conſider things, we ſhall not much wonder that a portion of matter that is indeed endowed but with a very few mechanical affections, as ſuch a determinate texture and motion, but is placed among a multitude of other bodies that differ in thoſe attributes from it and one another, ſhould be capable of having a great number and variety of relations to thoſe other bodies, and conſequently ſhould be thought to have many diſtinct inherent qualities by ſuch as look upon thoſe ſeveral relations or reſpects it may have to bodies without it, as real and diſtinct entities implanted in the body itſelf. When a curious watch is going, though the ſpring be that which puts all the parts into motion, yet we do not fancy (as an *Indian* or *Chineſe* would perchance do) in this ſpring one faculty to move the index uniformly round the dial-plate, another to ſtrike the hour, and perhaps a third to give an alarm, or ſhew the age of the moon or the tides; all the action of the ſpring (which is but a flexible piece of ſteel forcibly coiled together) being but an endeavour to dilate or unbind itſelf, and the reſt being performed by the various reſpects it hath to the ſeveral bodies (that compoſe the watch) among which it is placed, and which they have one to another. We all know that the ſun hath a power to harden clay, and ſoften wax, and melt butter, and thaw ice, and turn water into vapours, and make air expand itſelf in weather-glaſſes, and contribute to blanch linen, and make the white ſkin of the face ſwarthy, and mowed graſs yellow, and ripen fruit, hatch the eggs of ſilk-worms, caterpillars, and the like in-

* This memorable accident happened to a ſenator of *Berne*, who was cured by the experienced *Fabricius Hildanus*, that gives a long account of it to the learned *Horſtius*, among whoſe obſervations it is extant (*lib.* 2. *obſerv.* 35.) who aſcribes the indolence of the part, whilſt uncompreſſed, to ſome ſlimy juice (familiar enough to thoſe tendinous parts) wherein the glaſſy fragment was as it were bedded.

† In thoſe notes about occult qualities, where the deleterious faculty attributed to diamonds is conſidered.

ſects,

fects, and perform I know not how many other things, divers of which seem contrary effects; and yet these are not distinct powers or faculties in the sun, but only the productions of its heat (which itself is but the brisk and confused local motion of the minute parts of a body) diversified by the differing textures of the body that it chances to work upon, and the condition of the other bodies that are concerned in the operation. And therefore, whether the sun in some cases have any influence at all distinct from its light and heat, we see that all those phænomena we have thought fit to name, are producible by the heat of the common culinary fire duly applied and regulated. And so, to give an instance of another kind, when some years since, to try some experiments about the propagation of motion with bodies less capable of being battered by one another than those that have been formerly imployed, I caused some solid balls of iron, skilfully hardened and exquisitely shaped and glazed, to be purposely made; each of these polished balls was a spherical looking-glass, which, placed in the midst of a room, would exhibit the images of the objects round about it in a very regular and pleasing perspective. It would contract the image and reflect the beams of the sun after a manner differing from flat and from convex looking-glasses. It would in a neat perspective lessen the image of him that looked upon it, and bent it, and it would shew that image as if it were behind the surface and within the solid substance of the sphere; and in some it had all those distinct, and some of them wonderful properties, which either ancient or modern writers of catoptricks have demonstrated to belong to spherical specula, as such: and yet the globe, furnished with all these properties and affections, was but the iron itself reduced by the artificer to a spherical figure; (for the glass that made it specular was not distinct from the superficial parts of the iron, reduced all of them to a physically equal distance from the center). And of specula, spherical enough as to sense, you may make store in a trice, by breaking a large drop of quick-silver into several little ones, each of which will serve for objects placed pretty near it, and the smaller of which (being the least depressed in the middle by their own weight, and consequently more perfectly globous) may, with a good microscope placed in a window, afford you no unpleasant prospect of the neighbouring objects; and yet to reduce a parcel of stagnant quick-silver, which will much emulate a flat looking-glass, into many of these little spherical specula, whose properties are so differing from those of plain ones, there intervenes nothing but a slight local motion, which, in the twinkling of an eye, changeth the figure of the self-same matter.

I HAVE said thus much, *Pyrophilus*, to remove the mistake, that every thing men are wont to call a quality must needs be a real and physical entity, because of the importance of the subject; and yet I have omitted some things that might have been pertinently added, partly because I may hereafter have opportunity to take them in, and partly because I would not any farther lengthen this excursion, which yet I must not conclude, till I have added this short advertisement.

THAT I have chosen to declare what I mean by qualities, rather by examples than definitions, partly because being immediately or reductively the objects of sense, men generally understand pretty well what one another mean when they are spoken of: as to say, that the taste of such a thing is saline or sour, or that such a sound is melodious, shrill, or jarring (especially if, when we speak of sensible qualities, we add some enumeration of particular subjects wherein they do the most eminently reside) will make a man as soon understood as if he should go about to give logical definitions of those qualities; and partly because the notions of things are not yet so well stated and agreed on, but that it is many times difficult to assign their true genus's. And *Aristotle* himself doth not only define accidents without setting down their genus, but when he comes to define qualities, he tells us, that quality is that by which a thing is said to be *qualis*; where

where I would have you take notice, both that in his definition he omits the genus, and that it is no ſuch eaſy thing to give a very good definition of qualities, ſince he that is reputed the great maſter of logick, where he pretends to give us one, doth but upon the matter define the thing by the ſame thing: for it is ſuppoſed to be as little known what *qualis* is, as what *qualitas* is; and methinks he does juſt as if I ſhould define whiteneſs to be that, for which a thing is called white, or virtue that, for which a man is ſaid to be virtuous*. Beſides that, I much doubt whether his definition be not untrue as well as obſcure: for, to the queſtion, *Qualis res eſt?* anſwer may be returned out of ſome, if not all, of the other predicaments of accidents; which ſome of the modern logicians being aware of, they have endeavoured to ſalve the matter with certain cautions and limitations, which, however they may argue the deviſers to be ingenious, do, for aught I can diſcern, leave us ſtill to ſeek for a right and intelligible definition of quality in general; though to give ſuch a one be probably a much eaſier taſk than to define many qualities that may be named in particular, as ſaltneſs, ſourneſs, green, blue, and many others, which, when we hear named, every man knows what is meant by them, though no man (that I know of) hath been able to give accurate definitions of them.

IV. And if we ſhould conceive that all the reſt of the univerſe were annihilated, except any of theſe intire and undivided corpuſcles (treated of in the 3d particular foregoing) it is hard to ſay what could be attributed to it, beſides matter, motion (or reſt) bulk, and ſhape. Whence by the way you may take notice that bulk, though uſually taken in a comparative ſenſe, is in our ſenſe an abſolute thing, ſince a body would have it, though there were no other in the world. But now there being actually in the univerſe great multitudes of corpuſcles mingled among themſelves, there ariſe in any diſtinct portion of matter, which a number of them make up, two new accidents or events: the one doth more relate to each particular corpuſcle in reference to the (really or ſuppoſedly) ſtable bodies about it, namely its poſture (whether erected, inclined, or horizontal): and when two or more of ſuch bodies are placed one by another, the manner of their being ſo placed, as one beſides another, or one behind another, may be called their order; as I remember, *Ariſtotle* in his Metaphyſicks, *lib.* 1. *cap.* 4. recites this example out of the ancient Corpuſcularians, that A and N differ in figure, and AN and NA in order, Z and N in ſituation: and indeed poſture and order ſeem both of them reducible to ſituation. And when many corpuſcles do ſo convene together as to compoſe any diſtinct body, as a ſtone or a metal, then from their other accidents (or modes) and from theſe two laſt mentioned, there doth emerge a certain diſpoſition or contrivance of parts in the whole, which we may call the texture of it.

V. And if we ſhould conceive all the reſt of the univerſe to be annihilated ſave one ſuch body, ſuppoſe a metal or a ſtone, it were hard to ſhew that there is phyſically any thing more in it than matter, and the accidents we have already named. But now we

* Since the writing of this the author found that ſome of the eminenteſt of the modern ſchoolmen themſelves have been as well as he unſatisfied with the *Ariſtotelian* definition of quality: concerning which (not to mention *Revius*, a learned Proteſtant annotator upon *Suarez*) *Ariaga* ſays (*diſp.* 5. *ſect.* 2. *ſubſ.* 1.) *Per hanc nihil explicatur; nam de hoc quærimus, quid ſic eſſe quale, dices habere qualitatem; bonus circulus: qualitas eſt id, quo quis ſit qualis, & eſſe qualem eſt habere qualitatem.* And even the famous Jeſuit *Suarez*, though he endeavours to excuſe it, yet confeſſeth that it leaves the proper notion of quality as obſcure to us as before: (*Quæ definitio,* ſaith he, *licèt ea ratione eſſentialis videatur, quod detur per habitudinem ad effectum formalem, quem omnis forma eſſentialiter reſpicit, tamen quod ad nos ſpectat, æquè obſcura nobis manet propria ratio qualitatis.*) *Suarez diſputat. metaphyſ.* 42. But *Hurtadus* (in his metaphyſical diſputations) ſpeaks more boldly, telling us roundly, that it is *non tam definitio, quàm inanis quædam nugatio*; which makes me the more wonder that a famous *Carteſian* (whom I forbear to name) ſhould content himſelf to give us ſuch an inſignificant or at leaſt ſuperficial definition of quality,

are to confider, that there are *de facto* in the world certain fenfible and rational beings that we call men; and the body of man having feveral external parts, as the eye, the ear, *&c.* each of a diftinct and peculiar texture, whereby it is capable to receive impreffions from the bodies about it, and upon that account it is called an organ of fenfe; we muft confider, I fay, that thefe fenfories may be wrought upon by the figure, fhape, motion, and texture of bodies without them after feveral ways, fome of thofe external bodies being fitted to affect the eye, others the ear, others the noftrils, *&c.* And to thefe operations of the objects on the fenfories, the mind of man, which upon the account of its union with the body, perceives them, giveth diftinct names, calling the one light or colour, the other found, the other odour, *&c.* And becaufe alfo each organ of fenfe, as the eye, or the palate, may be itfelf differingly affected by external objects, the mind likewife gives the objects of the fame fenfe diftinct appellations, calling one colour green, the other blue, and one tafte fweet, and another bitter, *&c.* Whence men have been induced to frame a long catalogue of fuch things, as, for their relating to our fenfes, we call fenfible qualities; and becaufe we have been converfant with them before we had the ufe of reafon, and the mind of man is prone to conceive almoft every thing (nay, even privations, as blindnefs, death, *&c.*) under the notion of a true entity or fubftance, as itfelf is; we have been from our infancy apt to imagine that thefe fenfible qualities are real beings in the objects they denominate, and have the faculty or power to work fuch and fuch things; as gravity hath a power to ftop the motion of a bullet fhot upwards, and carry that folid globe of matter toward the center of the earth; whereas indeed (according to what we have largely fhewn above) there is in the body, to which thefe fenfible qualities are attributed, nothing of real and phyfical but the fize, fhape, and motion or reft, of its component particles, together with that texture of the whole, which refults from their being fo contrived as they are; nor is it neceffary they fhould have in them any thing more, like to the ideas they occafion in us, thofe ideas being either the effect of our prejudices or inconfideratenefs, or elfe to be fetched from the relation that happens to be betwixt thofe primary accidents of the fenfible object and the peculiar texture of the organ it affects: as when a pin being run into my finger caufeth pain, there is no diftinct quality in the pin anfwerable to what I am apt to fancy pain to be, but the pin in itfelf is only flender, ftiff, and fharp, and by thofe qualities happens to make a folution of continuity in my organ of touching, upon which, by reafon of the fabrick of the body, and the intimate union of the foul with it, there arifeth that troublefome kind of perception which we call pain, and I fhall anon more particularly fhew how much that depends upon the peculiar fabrick of the body.

VI. But here I forefee a difficulty, which being perhaps the chiefeft that we fhall meet with againft the corpufcular hypothefis, it will deferve to be, before we proceed any farther, taken notice of. And it is this, that whereas we explicate colours, odours, and the like fenfible qualities by a relation to our fenfes, it feems evident that they have an abfolute being irrelative to us: for fnow (for inftance) would be white, and a glowing coal would be hot, though there were no man or any other animal in the world. And it is plain that bodies do not only by their qualities work upon our fenfes, but upon other, and thofe inanimate, bodies; as the coal will not only heat or burn a man's hand if he touch it, but would likewife heat wax (even fo much as to melt it and make it flow) and thaw ice into water, although all the men and fenfitive beings in the world were annihilated. To clear this difficulty, I have feveral things to reprefent: and,

1. I say not that there are no other accidents in bodies than colours, odours, and the like; for I have already taught that there are fimpler and more primitive affections

of

of matter, from which theſe ſecondary qualities, if I may ſo call them, do depend: and that the operations of bodies upon one another ſpring from the ſame, we ſhall ſee by and by.

2. Nor do I ſay that all qualities of bodies are directly ſenſible; but I obſerve that when one body works upon another, the knowledge we have of their operation proceeds either from ſome ſenſible quality, or ſome more catholick affection of matter, as motion, reſt, or texture, generated or deſtroyed in one of them; for elſe it is hard to conceive how we ſhall come to diſcover what paſſes betwixt them.

3. We muſt not look upon every diſtinct body that works upon our ſenſes as a bare lump of matter of that bigneſs and outward ſhape that it appears of; many of them having their parts curiouſly contrived, and moſt of them perhaps in motion too. Nor muſt we look upon the univerſe that ſurrounds us as upon a moveleſs and undiſtinguiſhed heap of matter, but as upon a great engine, which having either no vacuity, or none that is conſiderable betwixt its parts (known to us) the actions of particular bodies upon one another muſt not be barely eſtimated, as if two portions of matter of their bulk and figure were placed in ſome imaginary ſpace beyond the world, but as being ſituate in the world, conſtituted as it now is, and conſequently as having in their action upon each other liable to be promoted or hindered or modified by the actions of other bodies beſides them: as in a clock, a ſmall force applied to move the index to the figure of XII will make the hammer ſtrike often and forcibly againſt the bell, and will make a far greater commotion among the wheels and weights, than a far greater force would do, if the texture and contrivance of the clock did not abundantly contribute to the production of ſo great an effect. And in agitating water into froth, the whiteneſs would never be produced by that motion, were it not that the ſun or other lucid body ſhining upon that aggregate of ſmall bubbles enables them to reflect confuſedly great ſtore of little and as it were contiguous lucid images to the eye. And ſo the giving to a large metalline ſpeculum a concave figure would never enable it to ſet wood on fire, and even to melt down metals readily, if the ſun-beams, that in cloudleſs days do, as to ſenſe, fill the air, were it not by the help of that concavity thrown together to a point. And to ſhew you by an eminent inſtance how various and how differing effects the ſame action of a natural agent may produce according to the ſeveral diſpoſitions of the bodies it works upon, do but conſider that in two eggs, the one prolifick, the other barren, the ſenſe can perhaps diſtinguiſh before the incubation no difference at all; and yet theſe bodies outwardly ſo like, do differ in the internal diſpoſition of their parts, that if they be both expoſed to the ſame degree of heat (whether of a hen or an artificial oven) that heat will change the one into a putrid and ſtinking ſubſtance, and the other into a chick, furniſhed with great variety of organical parts of very differing conſiſtencies, and curious as well as differing textures.

4. I do not deny but that bodies may be ſaid in a very favourable ſenſe to have thoſe qualities we call ſenſible, though there were no animals in the world: for a body in that caſe may differ from thoſe bodies which now are quite devoid of quality, in its having ſuch a diſpoſition of its conſtituent corpuſcles, that in caſe it were duly applied to the ſenſory of an animal, it would produce ſuch a ſenſible quality which a body of another texture would not: as though if there were no animals there would be no ſuch thing as pain, yet a pin may, upon the account of its figure, be fitted to cauſe pain, in caſe it were moved againſt a man's finger; whereas a bullet, or other blunt body, moved againſt it with no greater force, will not cauſe any ſuch perception of pain. And thus ſnow, though, if there were no lucid body nor organ of ſight in the world, it would exhibit no colour at all (for I could not find it had any in places exactly darkened) yet it hath a greater diſpoſition than a coal or ſoot, to reflect ſtore of light outwards, when the

the sun shines upon them all three. And so we say, that a lute is in tune whether it be exactly played upon or no, if the strings be all so duly stretched as that it would appear to be in tune, if it were played upon. But as if you should thrust a pin into a man's finger, both a while before and after his death, though the pin be as sharp at one time as at another, and maketh in both cases alike a solution of continuity; yet in the former case the action of the pin will produce pain, and not in the latter, because in this the pricked body wants the soul, and consequently the perceptive faculty: so if there were no sensitive beings those bodies that are now the objects of our senses would be but dispositively, if I may so speak, endowed with colours, tastes, and the like; and actually, but only with those more catholick affections of bodies, figure, motion, texture, *&c.*

To illustrate this yet a little farther: suppose a man should beat a drum at some distance from the mouth of a cave, conveniently situated to return the noise he makes; although men will presently conclude that that cave hath an echo, and will be apt to fancy upon that account some real property in the place to which the echo is said to belong; and although indeed the same noise made in many other of the neighbouring places would not be reflected to the ear, and consequently would manifest those places to have no echoes; yet, to speak physically of things, this peculiar quality or property we fancy in the cave is in it nothing else but the hollowness of its figure; whereby it is so disposed, as when the air beats against it, to reflect the motion towards the place whence that motion began; and that which passeth on this occasion is indeed but this, that the drum-stick falling upon the drum makes a percussion of the air, and puts that fluid body into an undulating motion, and the airy waves thrusting on one another till they arrive at the hollow superficies of the cave have, by reason of its resistance and figure, their motion determined the contrary way; namely, backwards towards that part where the drum was when it was struck. So that in that which here happens there intervenes nothing but the figure of one body and the motion of another; though if a man's ear chance to be in the way of these motions of the air, forwards and backwards, it gives him a perception of them, which he calls sound. And because these perceptions which are supposed to proceed from the same percussion of the drum, and thereby of the air, are made at distinct times one after another, that hollow body from whence the last sound is conceived to come to the air, is imagined to have a peculiar faculty, upon whose account men are wont to say that such a place hath an echo.

5. And whereas one body doth often seem to produce in another divers such qualities as we call sensible, which qualities therefore seem not to need any reference to our senses; I consider, that when one inanimate body works upon another, there is nothing really produced by the agent in the patient, save some local motion of its parts or some change of texture consequent upon that motion: and so, if the patient come to have any sensible quality that it had not before, it acquires it upon the same account upon which other bodies have it, and it is but a consequent to this mechanical change of texture, that, by means of its effects upon our organs of sense, we are induced to attribute this or that sensible quality to it. And in case a pin should chance by some inanimate body to be driven against a man's finger, that which the agent doth is but to put a sharp and slender body into such a kind of motion; and that which the pin doth is to pierce into a body that it meets with, not hard enough to resist its motion; and so that upon this there should ensue such a thing as pain, is but a consequent that superadds nothing of real to the pin that occasions that pain. So if a piece of transparent ice be, by the falling of some heavy and hard body upon it, broken into a gross powder that looks whitish, the falling body doth nothing to the ice but break it into very small fragments, lying confusedly upon one another; though by reason of the fabrick of the world and of our eyes there

doth in the day-time, upon this comminution, enſue ſuch a kind of copious reflection of the incident light to our eyes as we call whiteneſs. And when the ſun, by thawing this broken ice, deſtroys the whiteneſs of that portion of matter, and makes it become diaphanous, which it was not before, it doth no more than alter the texture of the component parts, by putting them into motion, and thereby into a new order; in which, by reaſon of the diſpoſition of the pores intercepted betwixt them, they reflect but few of the incident beams of light, and tranſmit moſt of them. Thus, when with a burniſher you poliſh a rough piece of ſilver, that which is really done is but the depreſſion of the little protuberant parts into one level with the reſt of the ſuperficies; though upon this mechanical change of the texture of the ſuperficial parts, we men ſay, that it hath loſt the quality of roughneſs, and acquired that of ſmoothneſs; becauſe that, whereas before the little exſtancies by their figure reſiſted a little the motion of our fingers, and grated upon them a little, our fingers now meet with no ſuch offenſive reſiſtance. It is true that the fire doth thaw ice, and alſo both make wax flow, and enable it to burn a man's hand; and yet this doth not neceſſarily argue in it any inherent quality of heat, diſtinct from the power it hath of putting the ſmall parts of the wax into ſuch a motion, as that their agitation ſurmounts their coheſion; which motion, together with their gravity, is enough to make them *pro tempore* conſtitute a fluid body; and aqua fortis, without any (ſenſible) heat, will make camphire caſt on it aſſume the form of a liquor diſtinct from it; as I have tried that a ſtrong fire will alſo make camphire fluid: not to add, that I know a liquor into which certain bodies being put, when both itſelf (as well as they) is actually cold (and conſequently when you would not ſuſpect it of an actual inherent heat) will not only ſpeedily diſſipate many of their parts into ſmoke, but leave the reſt black and burnt almoſt like a coal. So that though we ſuppoſe the fire to do no more than variouſly and briſkly to agitate the inſenſible parts of the wax, that may ſuffice to make us think the wax endowed with a quality of heat: becauſe if ſuch an agitation be greater than that of the ſpirit and other parts of our organs of touching, that is enough to produce in us that ſenſation we call heat; which is ſo much a relative to the ſenſory which apprehends it, that we ſee that the ſame lukewarm water, that is, whoſe corpuſcles are moderately agitated by the fire, will appear hot to one of a man's hands, if that be very cold, and cold to the other, in caſe it be very hot, though both of them be the ſame man's hands. To be ſhort, if we fancy any two of the bodies about us, as a ſtone, a metal, &c. to have nothing at all to do with any other body in the univerſe, it is not eaſy to conceive either how one can act upon the other, but by local motion (of the whole body or its corporeal effluvia); or how by motion it can do any more than put the parts of the other body into motion too, and thereby produce in them a change of ſituation and texture, or of ſome other of its mechanical affections: though this (paſſive) body being placed among other bodies in a world conſtituted as ours now is, and being brought to act upon the moſt curiouſly contrived ſenſories of animals, may upon both theſe accounts exhibit many differing ſenſible phænomena; which, however we look upon them as diſtinct qualities, are conſequently but the effects of the often mentioned catholick affections of matter, and deducible from the ſize, ſhape, motion (or reſt) poſture, order, and the reſulting texture of the inſenſible parts of bodies. And therefore though, for ſhortneſs of ſpeech, I ſhall not ſcruple to make uſe of the word Qualities, ſince it is already ſo generally received, yet I would be underſtood to mean them in a ſenſe ſuitable to the doctrine above delivered. As if I ſhould ſay that roughneſs is apt to grate and offend the ſkin, I ſhould mean, that a file or other body, by having upon its ſurface a multitude of little hard and exſtant parts, and of an angular or ſharp figure, is qualified to work the mentioned effect: and ſo if I ſhould ſay that heat melts metals, I ſhould mean, that this fuſion is effected by fire or ſome other body,

body, which, by the various and vehement motion of its infenfible parts, does to us appear hot. And hence (by the way) I prefume you will eafily guefs at what I think of the controverfy fo hotly difputed of late betwixt two parties of learned men, whereof the one would have all accidents to work only in virtue of the matter they refide in, and the other would have the matter to act only in virtue of its accidents: for confidering that on the one fide the qualities we here fpeak of do fo depend upon matter, that they cannot fo much as have a being but in and by it; and on the other fide, if all matter were but quite devoid of motion (to name now no other accidents) I do not readily conceive how it would operate at all; I think it is fafeft to conclude that neither matter nor qualities apart, but both of them conjointly, do perform what we fee done by bodies to one another, according to the doctrine of qualities juft now delivered.

Of the Nature of a Form.

VII. WE may now advance fomewhat further, and confider, that men having taken notice that certain confpicuous accidents were to be found affociated in fome bodies, and other conventions of accidents in other bodies, they did for conveniency, and for the more expeditious expreffion of their conceptions, agree to diftinguifh them into feveral forts, which they call genders or fpecies, according as they referred them, either upwards to a more comprehenfive fort of bodies, or downward to a narrower fpecies, or to individuals; as, obferving many bodies to agree in being fufible, malleable, heavy, and the like, they gave to that fort of body the name of Metal, which is a genus in reference to gold, filver, lead, and but a fpecies in reference to that fort of mixed bodies they call Foffilia: this fuperior genus comprehending both metals, ftones, and divers other concretions, though itfelf be but a fpecies in refpect of mixed bodies. Now when any body is referred to any particular fpecies (as of a metal, a ftone, or the like) becaufe men have for their convenience agreed to fignify all the effentials requifite to conftitute fuch a body by one name, moft of the writers of phyficks have been apt to think, that befides the common matter of all bodies, there is but one thing that difcriminates it from other kinds, and makes it what it is, and this, for brevity's fake, they call a form: which, becaufe all the qualities and other accidents of the body muft depend on it, they alfo imagine to be a very fubftance, and indeed a kind of foul, which, united to the grofs matter, compofes with it a natural body, and acts in it by the feveral qualities to be found therein, which men are wont to afcribe to the creature fo compofed. But as to this affair I obferve, that if (for inftance) you afk a man what gold is; if he cannot fhew you a piece of gold, and tell you this is gold, he will defcribe it to you as a body that is extremely ponderous, very malleable and ductile, fufible, and yet fixed in the fire, and of a yellowifh colour; and if you offer to put off to him a piece of brafs for a piece of gold, he will prefently refufe it, and (if he underftand metals) tell you, that though your brafs be coloured like it, it is not fo heavy nor fo malleable, neither will it like gold refift the utmoft brunt of the fire, or refift aqua fortis. And if you afk men, what they mean by a ruby, or nitre, or a pearl, they will ftill make you fuch anfwers, that you may clearly perceive, that whatever men talk in theory of fubftantial forms, yet that, upon whofe account they really diftinguifh any one body from others, and refer it to this or that fpecies of bodies, is nothing but an aggregate or convention of fuch accidents as moft men do by a kind of agreement (for the thing is more arbitrary than we are aware of) think neceffary or fufficient to make a portion of the univerfal matter belong to this or that determinate genus or fpecies of natural bodies. And therefore not only the generality of chymists, but divers philosophers, and, what is more, some schoolmen themselves, maintain it to be possible to transmute the ignobler metals into gold; which argues, that if a man could bring any parcel of matter to be yellow, and malleable and ponderous, and fixed in the fire, and upon the test, and indissoluble in aqua fortis, and in some to have a concurrence of all those accidents by which men try true gold from false, they would take it for true gold without scruple. And in this case the generality of mankind would leave the school-doctors to dispute, whether being a factitious body (as made by the chymist's art) it have the substantial form of gold, and would upon the account of the convention of the freshly mentioned accidents, let it pass current amongst them, notwithstanding most mens greater care not to be deceived in a matter of this nature than in any other. And indeed since to every determinate species of bodies there doth belong more than one quality, and for the most part a concurrence of many is so essential to that sort of bodies, that the want of any one of them is sufficient to exclude it from belonging to that species; there needs no more to discriminate sufficiently any one kind of bodies from all the bodies in the world that are not of that kind; as the chymists *luna fixa*, which they tell us wants not the weight, the malleableness, nor the fixedness, nor any other property of gold, except the yellowness (which makes them call it white gold) would by reason of that want of colour be easily known from true gold. And you will not wonder at this if you consider, that though spheres and parallelopipedons differ but in shape, yet this difference alone is the ground of so many others, that *Euclid*, and other geometricians have demonstrated I know not how many properties of the one, which do no way belong to the other; and * *Aristotle* himself somewhere tells us that a sphere is composed of brass and roundness. And I suppose it would be thought a man's own fault if he could not distinguish a needle from a file, or a key from a pair of sciffars, though these being all made of iron, and differing but in bigness and shape, are less remarkably diverse than natural bodies, the most part of which differ from each other in far more accidents than two. Nor need we think that qualities being but accidents, they cannot be essential to a natural body; for accident, as I formerly noted, is sometimes opposed to substance, and sometimes to essence. And though an accident cannot but be accidental to matter, as it is a substantial thing, yet it may be essential to this or that particular body: as in *Aristotle*'s newly mentioned example, though roundness is but accidental to brass, yet it is essential to a brazen sphere; because, though the brass were devoid of roundness (as if it were cubical, or of any other figure) it would still be a corporeal substance, yet without that roundness it could not be a sphere. Wherefore since an aggregate or convention of qualities is enough to make the portion of matter it is found in what it is, and denominate it of this or that determinate sort of bodies; and since those qualities, as we have seen already, do themselves proceed from those more primary and catholick affections of matter, bulk, shape, motion, or rest, and the texture thence resulting, why may we not say that the form of a body being made up of those qualities united in one subject, doth likewise consist in such a convention of those newly named mechanical affections of matter as is necessary to constitute a body of that determinate kind. And so, though I shall for brevity's sake retain the word Form, yet I would be understood to mean by it, not a real substance distinct from matter, but only the matter itself of a natural body, considered with its peculiar manner of existence; which I think may not inconveniently be called either its specifical or its denominating state, or its essential modification; or, if you would have me express it in one word, its stamp. For such a convention of accidents is sufficient to perform the offices that are necessarily required in what men call a form, since it makes the body such as it is, making it appertain to this, or

* *Arist.* Metaph. lib. 7. cap. 8.

or that determinate ſpecies of bodies, and diſcriminating it from all other ſpecies of bodies whatſoever: As for inſtance, ponderouſneſs, ductility, fixedneſs, yellowneſs, and ſome other qualities concurring in a portion of matter, do with it conſtitute gold, and making it belong to that ſpecies we call metals, and to that ſort of metals we call gold, do both denominate and diſcriminate it from ſtones, ſalts, marcaſites, and all other ſorts of bodies that are not metals, and from ſilver, braſs, copper, and all metals, except gold. And whereas it is ſaid by ſome, that the form alſo of a body ought to be the principle of its operation, we ſhall hereafter conſider in what ſenſe that is to be admitted or rejected; in the mean time it may ſuffice us, that even in the vulgar philoſophy it is acknowledged, that natural things for the moſt part operate by their qualities, as ſnow dazzles the eyes by its whiteneſs, and water ſcattered into drops of rain falls from the clouds upon the account of its gravity. To which I ſhall add, that how great the power may be which a body may exerciſe by virtue of a ſingle quality, may appear by the various and oftentimes prodigious effects which fire produces by its heat, when thereby its melts metals, calcines ſtones, deſtroys whole woods and cities, *&c.* And if ſeveral active qualities convene in one body (as that which in our hypotheſis is meant by form, uſually compriſes ſeveral of them) what great things may be thereby performed, may be ſomewhat gueſſed at by the ſtrange things we ſee done by ſome engines, which, being as engines, undoubtedly devoid of ſubſtantial forms, muſt do thoſe ſtrange things they are admired for, by virtue of thoſe accidents, the ſhape, ſize, motion, and contrivance of their parts. Not to mention, that in our hypotheſis, beſides thoſe operations that proceed from the eſſential modification of the matter, as the body (compoſed of matter and neceſſary accidents) is conſidered *per modum unius*, as one intire corporeal agent, it may in divers caſes have other operations upon account of thoſe particular corpuſcles, which though they concur to compoſe it, and are, in reference to the whole, conſidered but as its parts, may yet retain their own particular nature, and divers of the peculiar qualities: as in a watch, beſides thoſe things which the watch performs as ſuch, the ſeveral parts whereof it conſiſts, as the ſpring, the wheels, the ſtring, the pins, *&c.* may have each of them its peculiar bulk, ſhape, and other attributes, upon the account of one or more of which the wheel or ſpring, *&c.* may do other things than what it doth, as merely a conſtituent part of the watch. And ſo in the milk of a nurſe, that hath ſome hours before taken a potion, though the corpuſcles of the purging medicine appear not to ſenſe diſtinct from the other parts of the milk, which in far greater numbers concur with them to conſtitute that white liquor; yet theſe purgative particles, that ſeem to be but part of the matter whereof the milk conſiſts, do yet ſo retain their own nature and qualities, that being ſucked in with the reſt by the infant, they quickly diſcriminate and diſcover themſelves by purging him. But of this ſubject more hereafter.

Of Generation, Corruption, and Alteration.

VIII. IT now remains, that we declare, what, according to the tenor of our hypotheſis, is to be meant by generation, corruption, and alteration; (three names that have very much puzzled and divided philoſophers). In order hereunto we may conſider,

1. THAT there are in the world great ſtore of particles of matter, each of which is too ſmall to be, whilſt ſingle, ſenſible; and being intire or undivided, muſt needs both have its determinate ſhape, and be very ſolid. Inſomuch, that though it be mentally, and by divine Omnipotence diviſible, yet by reaſon of its ſmallneſs and ſolidity nature doth ſcarce ever actually divide it; and theſe may in this ſenſe be called *minima* or *prima naturalia*.

2. THAT

2. THAT there are alſo multitudes of corpuſcles which are made up of the coalition of ſeveral of the former *minima naturalia*; and whoſe bulk is ſo ſmall and their adheſion ſo cloſe and ſtrict, that each of theſe little primitive concretions or cluſters (if I may ſo call them) of particles is ſingly below the diſcernment of ſenſe, and though not abſolutely indiviſible by nature into the *prima naturalia* that compoſed it, or perhaps into other little fragments, yet, for the reaſons freſhly intimated, they very rarely happen to be actually diſſolved or broken, but remain intire in great variety of ſenſible bodies, and under various forms or diſguiſes. As, not to repeat what we lately mentioned of the undeſtroyed purging corpuſcles of milk, we ſee that even groſſer and more compounded corpuſcles may have ſuch a permanent texture: for quickſilver, for inſtance, may be turned into a red powder for a fuſible and malleable body, or a fugitive ſmoke, and diſguiſed I know not how many other ways, and yet remain true and recoverable mercury. And theſe are, as it were, the ſeeds or immediate principles of many ſorts of natural bodies, as earth, water, ſalt, *&c.* and thoſe ſingly inſenſible, become capable, when united, to affect the ſenſe: as I have tried, that if good camphire be kept a while in pure ſpirit of wine, it will thereby be reduced into ſuch little parts as totally to diſappear in the liquor, without making it look leſs clear than fair water; and yet, if into this mixture you pour a competent quantity of water, in a moment the ſcattered corpuſcles of the camphire will, by reuniting themſelves, become white, and conſequently viſible, as before their diſperſion.

3. THAT as well each of the *minima naturalia*, as each of the primary cluſters above mentioned having its own determinate bulk and ſhape, when theſe come to adhere to one another, it muſt always happen that the ſize, and often that the figure of the corpuſcle compoſed by their juxta-poſition and coheſion, will be changed; and not ſeldom too, the motion either of the one or the other, or both, will receive a new tendency, or be altered as to its velocity or otherwiſe: and the like will happen when the corpuſcles that compoſe a cluſter of particles are disjoined, or any thing of the little maſs is broken off. And whether any thing of matter be added to a corpuſcle or taken from it, in either caſe (as we juſt now intimated) the ſize of it muſt neceſſarily be altered, and for the moſt part the figure will be ſo too, whereby it will both acquire a congruity to the pores of ſome bodies (and perhaps ſome of our ſenſories) and become incongruous to thoſe of others; and conſequently be qualified, as I ſhall more fully ſhew you hereafter, to operate on divers occaſions, much otherwiſe than it was fitted to do before.

4. THAT when many of theſe inſenſible corpuſcles come to be aſſociated into one viſible body, if many or moſt of them be put into motion, from what cauſe ſoever the motion proceeds, that itſelf may produce great changes and new qualities in the body they compoſe; for not only motion may perform much even when it makes not any viſible alteration in it, as air put into ſwift motion (as when it is blown out of bellows) acquires a new name, and is called Wind, and to the touch appears far colder than the ſame air not ſo formed into a ſtream; and iron, by being briſkly rubbed againſt wood or other iron, hath its ſmall parts ſo agitated as to appear hot to our ſenſe: but this motion oftentimes makes viſible alterations in the texture of the body into which it is received; for always the moved parts ſtrive to communicate their motion, or ſomewhat of the degree of it, to ſome parts that were before either at reſt or otherwiſe moved, and oftentimes the ſame moved parts do thereby either disjoin or break ſome of the corpuſcles they hit againſt, and thereby change their bulk or ſhape, or both, and either drive ſome of them quite out of the body, and perhaps lodge themſelves in their places, or elſe aſſociate them anew with others. Whence it uſually follows that the texture is for a while at leaſt, and unleſs it be very ſtable and permanent for good and all, very

much altered, and especially in that the pores or little intervals intercepted betwixt the component particles will be changed as to bigness or figure, or both, and so will cease to be commensurate to the corpuscles that were fit for them before, and become commensurate to such corpuscles of other sizes and shapes, as till then were incongruous to them. Thus we see that water, by losing the wonted agitation of its parts, may acquire the firmness and brittleness we find in ice, and lose much of the transparency it had whilst it was a liquor. Thus also by very hard rubbing two pieces of resinous wood against one another, we may make them throw out divers of their looser parts into steams and visible smoke; and may, if the attrition be duly continued, make that commotion of the parts so change the texture of the whole, as afterwards to turn the superficial parts into a kind of coal. And thus milk, especially in hot weather, will by the intestine, though languid motions of its parts, be in a short time turned into a thinner sort of liquor than milk, and into cream, and this (last named) will, by being barely agitated in a churn, be turned in a short time into that unctuous and consistent body we call butter, and into thin, fluid, and sour butter-milk. And thus (to dispatch) by the bruising of fruit, the texture is commonly so changed, that, as we see particularly in apples, the bruised part soon comes to be of another nature than the sound part, the one differing from the other both in colour, taste, smell, and consistence. So that (as we have already inculcated) local motion hath, of all other affections of matter, the greatest interest in the altering and modifying of it; since it is not only the grand agent or efficient among second causes, but is also sometimes one of the principal things that constitutes the form of bodies. As when two sticks are set on fire by long and vehement attrition, local motion is not only that which kindles the wood, and so as an efficient produces the fire, but is that which principally concurs to give the produced stream of shining matter, the name and nature of flame: and so it concurs also to constitute all fluid bodies.

5. And that since we have formerly seen that it is from the size, shape, and motion of the small parts of matter and the texture that results from the manner of their being disposed in any one body, that the colour, odour, taste, and other qualities of that body are to be derived, it will be easy for us to recollect that such changes cannot happen in a portion of matter without so much varying the nature of it, that we need not deride the antient atomists for attempting to deduce the generation and corruption of bodies from the famed σύγκρισις ϰ̀ διάκρισις, the *convention* and *dissolution*, and the alterations of them, from the transposition of their (supposed) atoms. For though indeed nature is wont, in the changes she makes among things corporeal, to imply all the three ways, as well in alterations as generations and corruptions; yet if they only meant, as probably enough they did that of the three ways proposed, the first was wont to be the principal in the generation of bodies, the second in the corruption, and the third in their alterations; I shall not much oppose this doctrine: though I take the local motion or transposition of parts in the same portion of matter to bear a great stroke as well in reference to generation and corruption, as to alteration: as we see when milk, or flesh, or fruit, without any remarkable addition or loss of parts, turns into maggots, or other insects; and as we may more conspicuously observe in the precipitation of mercury, without addition, in the vitrification of metals, and other chymical experiments to be hereafter mentioned.

These things premised, it will not now be difficult to comprise in few words such a doctrine touching the generation, corruption, and alteration of bodies as is suitable to our hypothesis and the former discourse. For if in a parcel of matter there happen to be produced (it imports not much how) a concurrence of all those accidents (whether those only or more) that men by tacit agreement have thought necessary and sufficient to constitute

ſtitute any one determinate ſpecies of things corporeal, then we ſay that a body belonging to that ſpecies, as ſuppoſe a ſtone, or a metal, is generated or produced *de novo:* not that there is really any thing of ſubſtantial produced, but that thoſe parts of matter that did indeed before pre-exiſt, but were either ſcattered and ſhared among other bodies, or at leaſt otherwiſe diſpoſed of, are now brought together and diſpoſed of after the manner requiſite to entitle the body that reſults from them to a new denomination, and make it appertain to ſuch a determinate ſpecies of natural bodies, ſo that no new ſubſtance is in generation produced, but only that which was pre-exiſtent obtains a new modification or manner of exiſtence. Thus when the ſpring, and wheels, and ſtring, and balance, and index, *&c.* neceſſary to a watch, which lay before ſcattered, ſome in one part, ſome in another of the artificer's ſhop, are firſt ſet together in the order requiſite to make ſuch an engine, to ſhew how the time paſſes, a watch is ſaid to be made: not that any of the mentioned material parts is produced *de novo*, but that till then the divided matter was not ſo contrived and put together, as was requiſite to conſtitute ſuch a thing as we call a watch. And ſo when ſand and aſhes are well melted together and ſuffered to cool, there is generated by the colliquation that ſort of concretion we call glaſs, though it be evident that its ingredients were both pre-exiſtent, and do but by their aſſociation obtain a new manner of exiſting together. And ſo when, by the churning of cream, butter and butter-milk are generated, we find not any thing ſubſtantial produced *de novo* in either of them, but only that the ſerum and the fat corpuſcles being put into local motion do, by their frequent occurſions, extricate themſelves from each other, and aſſociate themſelves in the new manner requiſite to conſtitute the bodies, whoſe names are given them.

And as a body is ſaid to be generated when it firſt appears cloathed with all thoſe qualities, upon whoſe account men have been pleaſed to call ſome bodies ſtones; others, metals; others, ſalts, *&c.* ſo when a body comes to loſe all or any of thoſe accidents that are eſſential and neceſſary to the conſtituting of ſuch a body, it is then ſaid to be corrupted or deſtroyed, and is no more a body of that kind, but loſes its title to its former denomination. Not that any thing corporeal or ſubſtantial periſhes in this change, but only that the eſſential modification of the matter is deſtroyed: and though the body be ſtill a body (no natural agent being able to annihilate matter) yet it is no longer ſuch a body as it was before, but periſheth in the capacity of a body of that kind. Thus, if a ſtone, falling upon a watch, break it to pieces, as, when the watch was made, there was no new ſubſtance produced, all the material parts (as the ſteel, braſs, ſtring, *&c.*) being pre-exiſtent ſomewhere or other (as in iron and copper-mines, in the bellies of thoſe animals of whoſe guts men uſe to make ſtrings); ſo not the leaſt part of the ſubſtance of the watch is loſt, but only diſplaced and ſcattered; and yet that portion of matter ceaſes to be a watch as it was before. And ſo (to reſume our late example) when cream is by churning turned into butter, and a ſerous liquor, the parts of the milk remain aſſociated into thoſe new bodies, but the white liquor periſheth in the capacity of milk. And ſo when ice comes to be thawed in exactly-cloſed veſſels, though the corruption be produced only (for aught appears) by introducing a new motion and diſpoſition into the parts of the frozen water, yet it thereupon ceaſes to be ice, however it be as much water, and conſequently as much a body as before it was frozen or thawed. Theſe and the like examples may teach us rightly to underſtand that common axiom of naturaliſts, *Corruptio unius eſt generatio alterius*; *& è contrà:* for ſince it is acknowledged on all hands that matter cannot be annihilated, and ſince it appears by what we have ſaid above, that there are ſome properties, namely, ſize, ſhape, motion (or, in its abſence, reſt) that are inſeparable from the actual parts of matter; and ſince alſo the coalition of any competent number of theſe parts is ſufficient to conſtitute a natural body endowed

dowed with divers ſenſible qualities, it can ſcarce be otherwiſe, but that the ſame agents that ſhatter the frame, or deſtroy the texture of one body, will, by ſhuffling them together, and diſpoſing them after a new manner, bring them to conſtitute ſome new ſort of bodies: as the ſame thing that by burning deſtroys wood, turns it into flame, ſoot, and aſhes. Only I doubt whether the axiom do generally hold true, if it be meant, that every corruption muſt end in the generation of a body belonging to ſome particular ſpecies of things, unleſs we take powders and fluid bodies indefinitely for ſpecies of natural bodies; ſince it is plain there are multitudes of vegetables and other concretions, which, when they rot, do not, as ſome others do, turn into worms, but either into ſome ſlimy or watery ſubſtance, or elſe (which is the moſt uſual) they crumble into a kind of duſt or powder, which, though looked upon as being the earth into which rotten bodies are at length reſolved, is very far from being of an elementary nature, but as yet a compounded body, retaining ſome if not many qualities, which often makes the duſt of one ſort of plant or animal differ much from that of another. And this will ſupply me with this argument *ad hominem*, *viz.* That ſince in thoſe violent corruptions of bodies that are made by outward agents, ſhattering them into pieces, if the axiom hold true, the new bodies emergent upon the diſſolution of the former muſt be really natural bodies (as indeed divers of the moderns hold them to be) and generated according to the courſe of nature; as when wood is deſtroyed by fire, and turned partly into flame, partly into ſoot, partly into coals, and partly into aſhes, I hope we may be allowed to conclude, that thoſe chymical productions, which ſo many would have to be but factitious bodies, are natural ones, and regularly generated. For it being the ſame agent, the fire that operates upon bodies, whether they be expoſed to it in cloſe glaſſes or in chimnies, I ſee no ſufficient reaſon why the chymical oils and volatile ſalts, and other things, which Spagyrites obtain from mixed bodies, ſhould not be accounted natural bodies, as well as the ſoot and aſhes, and charcoal, that by the ſame fire are obtained from kindled wood.

But before we paſs away from the mention of the corruption of bodies, I muſt take ſome notice of what is called their putrefaction. This is but a peculiar kind of corruption, wrought ſlowly (whereby it may be diſtinguiſhed from deſtruction by fire and other nimble agents) in bodies: it happens to them for the moſt part by means of the air or ſome other ambient fluid, which, by penetrating into the pores of the body, and by its agitation in them, doth uſually call out ſome of the more agile and leſs intangled parts of the body, and doth almoſt ever looſen and diſlocate the parts in general, and thereby ſo change the texture, and perhaps too the figure, of the corpuſcles that compoſe it, that the body thus changed acquires qualities unſuitable to its former nature, and for the moſt part offenſive to our ſenſes, eſpecially of ſmelling and taſting: which laſt clauſe I therefore add, not only becauſe the vulgar look not upon the change of an egg into a chick as a corruption, but as a perfection of the egg; but becauſe alſo I think it not improbable, that if by ſuch ſlow changes of bodies as make them loſe their former nature, and might otherwiſe paſs for putrefaction, many bodies ſhould acquire better ſcents or taſtes than before; or if nature, cuſtom, or any other cauſe, ſhould much alter the texture of our organs of taſting and ſmelling, it would not perhaps be ſo well agreed on what ſhould be called putrefaction, as that imports an impairing alteration, but men would find ſome favourabler notion for ſuch changes. For I obſerve that medlars, though they acquire in length of time ſuch a colour and ſoftneſs as rotten apples and other putrefied fruits do, yet, becauſe their taſte is not then harſh as before, we call that ripeneſs in them, which otherwiſe we ſhould call rottenneſs. And though, upon the death of a four-footed beaſt, we generally call that change which happens to the fleſh or blood putrefaction, yet we paſs a more favourable judgment upon that which happens to the fleſh and other ſofter parts of that animal (whether it be a kind of large rabbets or very ſmall and hornleſs deer) of which in China and in the Levant they make muſk; becauſe, by the change that enſues the animal's death, the fleſh acquires not an odious but a grateful ſmell. And we ſee that ſome men, whoſe appetites are gratified by rotten cheeſe, think it then not to have degenerated, but to have attained its beſt ſtate, when having loſt its former colour, ſmell, and taſte, and, which is more, being in great part turned into thoſe inſects called mites, it is both in a philoſophical ſenſe corrupted, and in the eſtimate of the generality of men grown putrid. But becauſe it very ſeldom happens that a body by generation acquires no other qualities than juſt thoſe that are abſolutely neceſſary to make it belong to the ſpecies that denominates it; therefore in moſt bodies there are divers other qualities that may be there, or may be miſſing, without eſſentially changing the ſubject: as water may be clear or muddy, odorous or ſtinking, and ſtill remain water; and butter may be white or yellow, ſweet or rancid, conſiſtent or melted, and ſtill be called butter. Now therefore, whenſoever a parcel of matter does acquire or loſe a quality that is not eſſential to it, that acquiſition or loſs is diſtinctly called alteration (or by ſome, mutation): the acqueſt only of the qualities that are abſolutely neceſſary to conſtitute its eſſential and ſpecifical difference, or the loſs of any of thoſe qualities, being ſuch a change as muſt not be called mere alteration, but have the particular name of generation or corruption; both which, according to this doctrine, appear to be but ſeveral kinds of alteration taken in a large ſenſe, though they are diſtinguiſhed from it in a more ſtrict and limited acceptation of that term.

And here we have a fair occaſion to take notice of the fruitfulneſs and extent of our mechanical hypotheſis: for ſince, according to our doctrine, the world we live in is not a moveleſs or indigeſted maſs of matter, but an Αὐτόματον, or *ſelf-moving engine*, wherein the greateſt part of the common matter of all bodies is always (though not ſtill the ſame parts of it) in motion, and wherein bodies are ſo cloſe ſet by one another, that (unleſs in ſome very few and extraordinary and as it were preternatural caſes) they have either no vacuities betwixt them, or only here and there interpoſed and very ſmall ones: and ſince, according to us, the various manner of the coalition of ſeveral corpuſcles into one viſible body is enough to give them a peculiar texture, and thereby fit them to exhibit divers ſenſible qualities, and to become a body, ſometimes of one denomination, and ſometimes of another; it will very naturally follow, that from the various occurſions of thoſe innumerable ſwarms of little bodies that are moved to and fro in the world, there will be many fitted to ſtick to one another, and ſo compoſe concretions; and many (though not in the ſelf-ſame place) disjoined from one another and agitated apart; and multitudes alſo that will be driven to aſſociate themſelves, now with one body, and preſently with another. And if we alſo conſider on the one ſide, that the ſizes of the ſmall particles of matter may be very various, their figures almoſt innumerable; and that if a parcel of matter do but happen to ſtick to one body, it may chance to give it a new quality, and if it adhere to another, or hit againſt ſome of its parts, it may conſtitute a body of another kind; or if a parcel of matter be knocked off from another, it may barely by that leave it, and become itſelf of another nature than before: if, I ſay, we conſider theſe things on the one ſide, and on the other ſide, that (to uſe Lucretius's compariſon) all that innumerable multitude of words that are contained in all the languages of the world are made of the various combinations of ſome of the twenty-four letters of the alphabet, it will not be hard to conceive that there may be an incomprehenſible variety of aſſociations and textures of the minute parts of bodies, and conſequently a vaſt multitude of portions of matter endowed with ſtore enough of differing qualities

qualities to deserve distinct appellations; though for want of heedfulness and fit words men have not yet taken so much notice of their less obvious varieties as to sort them as they deserve, and give them distinct and proper names. So that though I would not say that any thing can immediately be made of every thing, as a gold-ring of a wedge of gold, or oil or fire of water; yet since bodies having but one common matter can be differenced but by accidents, which seem all of them to be the effects and consequents of local motion, I see not why it should be absurd to think that (at least among inanimate bodies) by the intervention of some very small addition or subtraction of matter (which yet in most cases will scarce be needed) and of an orderly series of alterations, disposing by degrees the matter to be transmuted, almost of any thing, may at length be made any thing: as, though out of a wedge of gold one cannot immediately make a ring, yet by either wire-drawing that wedge by degrees, or by melting it, and casting a little of it into a mould, that thing may easily be effected. And so though water cannot immediately be transmuted into oil, and much less into fire; yet if you nourish certain plants with water alone (as I have done) till they have assimilated a great quantity of water into their own nature, you may, by committing this transmuted water (which you may distinguish and separate from that part of the vegetable you first put in) to distillation in convenient glasses, obtain, besides other things, a true oil, and a black combustible coal (and consequently fire); both of which may be so copious as to leave no just cause to suspect that they could be any thing near afforded by any little spirituous parts, which may be presumed to have been communicated by that part of the vegetable that is first put into the water, to that far greater part of it which was committed to distillation.

But, Pyrophilus, I perceive the difficulty and fruitfulness of my subject have made me so much more prolix than I intended, that it will not now be amiss to contract the summary of our hypothesis, and give you the main points of it with little or no illustration, and without particular proofs, in a few words. We teach then (but without peremptorily asserting it)

1. That the matter of all natural bodies is the same; namely, a substance, extended and impenetrable.

2. That all bodies thus agreeing in the same common matter, their distinction is to be taken from those accidents that do diversify it.

3. That motion, not belonging to the essence of matter (which retains its whole nature when it is at rest) and not being originally producible by other accidents, as they are from it, may be looked upon as the first and chief mood or affection of matter.

4. That motion, variously determined, doth naturally divide the matter it belongs to into actual fragments of parts; and this division, obvious experience (and more eminently chymical operations) manifest to have been made into parts exceedingly minute, and very often too minute to be singly perceivable by our senses.

5. Whence it must necessarily follow, that each of these minute parts or *minima naturalia* (as well as every particular body made up by the coalition of any number of them) must have its determinate bigness or size, and its own shape. And these three, namely, bulk, figure, and either motion or rest (there being no mean between these two) are the three primary and most catholick moods or affections of the insensible parts of matter, considered each of them apart.

6. That when divers of them are considered together, there will necessarily follow here below both a certain position of posture in reference to the horizon (as erected, inclining, or level) of each of them, and a certain order or placing before or behind, or besides one another; as when in a company of soldiers, one stands upright, the

other

other stoops, the other lies along upon the ground, they have various postures; and their being placed besides one another in ranks, and behind one another in files, are varieties of their order: and when many of these small parts are brought to convene into one body from their primary affections, and their disposition or contrivance as to posture and order, there results that which by one comprehensive name we call the texture of that body. And indeed these several kinds of location (to borrow a scholastical term) attributed in this sixth number to the minute particles of bodies, are so near of kin that they seem all of them referable to (that one event of their convening) situation or position. And these are the affections that belong to a body, as it is considered in itself, without relation to sensitive beings or to other animal bodies.

7. THAT yet there being men in the world whose organs of sense are contrived in such differing ways, that one sensory is fitted to receive impressions from some, and another from other sorts of external objects or bodies without them (whether these act as intire bodies, or by emission of their corpuscles, or by propagating some motion to the sensory) the perceptions of these impressions are by men called by several names, as heat, colour, sound, odour; and are commonly imagined to proceed from certain distinct and peculiar qualities in the external objects which have some resemblance to the ideas their action upon the senses excites in the mind; though indeed all these sensible qualities, and the rest that are to be met with in the bodies without us, are but the effects or consequents of the above-mentioned primary affections of matter, whose operations are diversified according to the nature of the sensories or other bodies they work upon.

8. THAT when a portion of matter, either by the accession or recess of corpuscles, or by the transposition of those it consisted of before, or by any two, or all of these ways, happens to obtain a concurrence of all those qualities which men commonly agree to be necessary and sufficient to denominate the body which hath them, either a metal or a stone, or the like, and to rank it in any peculiar and determinate species of bodies, then a body of that denomination is said to be generated.

9. THIS convention of essential accidents being taken (not any of them apart, but all) together for the specifical difference that constitutes the body and discriminates it from all other sorts of bodies, is by one name, because considered as one collective thing, called its form (as beauty, which is made up of symmetry of parts and agreeableness of colours) which is consequently but a certain character (as I sometimes call it) or a peculiar state of matter, or, if I may so name it, an essential modification: a modification, because it is indeed but a determinate manner of existence of the matter, and yet an essential modification, because that though the concurrent qualities be but accidental to matter (which with others instead of them, would be matter still) yet they are essentially necessary to the particular body, which without those accidents would not be a body of that denomination, as a metal or a stone, but of some other.

10. Now a body being capable of many other qualities besides those whose convention is necessary to make up its form, the acquisition or loss of any such quality is by naturalists, in the more strict sense of that term, named alteration: as when oil comes to be frozen, or to change colour, or to grow rancid; but if all or any of the qualities that are reputed essential to such a body come to be lost or destroyed, that notable change is called corruption. As when oil being boiled takes fire, the oil is not said to be altered in the former sense, but corrupted or destroyed, and the emergent fire generated; and when it so happens that the body is slowly corrupted, and thereby also acquires qualities offensive to our senses, especially of smell and taste (as when flesh or fruit grows rotten) that kind of corruption is by a more particular name called putrefaction. But neither in this nor in any other kind of corruption is there any thing substantial destroyed (no such thing having been produced in generation, and

matter

matter itſelf being on all hands acknowledged incorruptible) but only that ſpecial connexion of the parts or manner of their co-exiſtence, upon whoſe account the matter (whilſt it was in its former ſtate) was, and was called a ſtone, or a metal, or did belong to any other determinate ſpecies of bodies.

AN EXAMEN OF THE ORIGIN AND DOCTRINE OF SUBSTANTIAL FORMS,

As it is wont to be taught by the PERIPATETICKS.

THE origin of forms, Pyrophilus, as it is thought the nobleſt, ſo, if I miſtake not, it hath been found one of the moſt * perplexed enquiries that belong to natural philoſophy: and, I confeſs, it is one of the things that has invited me to look about for ſome more ſatisfactory account than the ſchools uſually give of this matter, that I have obſerved that the wiſeſt that have buſied themſelves in explicating forms according to the Peripatetick notions of them, have either knowingly confeſſed themſelves unable to explain them, or unwittingly proved themſelves to be ſo, by giving but unſatisfactory explications of them.

It will not, I preſume, be expected that I, who now write but notes, ſhould enumerate, much leſs examine, all the various opinions touching the origin and nature of forms; it being enough for our purpoſe, if, having already intimated in our hypotheſis what, according to that, may be thought of this ſubject, we now briefly conſider the general opinion of our modern Ariſtotelians and the ſchools concerning it; I ſay, the modern Ariſtotelians, becauſe divers of the ancient, eſpecially Greek commentators of Ariſtotle, ſeem to have underſtood their maſter's doctrine of forms much otherwiſe, and leſs incongruouſly, than his Latin followers, the ſchoolmen and others, have ſince done. Nor

* *Formarum cognitio eſt rudis, confuſa, nec niſi per περιστάσεις; neque verum eſt, formæ ſubſtantialis ſpeciem recipi in intellectum, non enim in ſenſu uſquam fuit.* J. C. Scalig.

Formæ ſubſtantiales ſunt incognitæ nobis, quia inſenſibiles: ideo per qualitates, quæ ſunt principia immediatæ tranſmutationis, exprimuntur. Aquinas ad 1. de degenerat. & corrupt.

In hac humanæ mentis caligine æquè forma ignis ac magnetis nobis ignota eſt. Sennertus.

do

do I exprefly mention Ariftotle himfelf among the champions of fubftantial forms, becaufe though he feems in a place or two exprefly enough to reckon forms among fubftances, yet elfewhere the examples he employs to fet forth the forms of natural things by, being taken from the figures of artificial things (as of a ftatue, *&c.*) which are confeffedly but accidents, and making very little ufe, if any, of fubftantial forms to explain the phænomena of nature, he feems to me upon the whole matter either to have been irrefolved whether there were any fuch fubftances or no, or to fpeak ambiguoufly and obfcurely enough of them, to make it queftionable what his opinions of them were.

But the fum of the controverfy betwixt us and the fchools is this, whether or no the forms of natural things (the fouls of men always excepted) be in generation educed, as they fpeak, out of the power of the matter, and whether thefe forms be true fubftantial entities, diftinct from the other fubftantial principle of natural bodies, namely matter.

The reafons that move me to embrace the negative are principally thefe three: firft, that I fee no neceffity of admitting in natural things any fuch fubftantial forms; matter, and the accidents of matter, being fufficient to explicate as much of the phænomena of nature as we either do or are like to underftand. The next, that I fee not what ufe this puzzling doctrine of fubftantial forms is of in natural philofophy; the acute Scaliger, and thofe that have moft bufied themfelves in the indagation of them, having freely acknowledged (as the more candid of the Peripateticks generally do) that the true knowledge of forms is too difficult and abftrufe to be attained by them. And how like it is, that particular phænomena will be explained by a principle whofe nature is confeffedly ignored, I leave you to judge. But becaufe to thefe confiderations I often have had, and fhall have here and there, occafion to fay fomething in the body of thefe notes, I fhall at prefent infift upon the third; which is, that I cannot conceive neither how the forms can be generated, as the Peripateticks would have it, nor how the things they afcribe to them are confiftent with the principles of true philofophy, or even with what themfelves otherwife teach.

The manner how forms are educed out of the power of the matter, according to that part of the doctrine of forms wherein the fchools generally enough agree, is a thing fo inexplicable, that I wonder not it hath put acute men upon feveral hypothefes to make it out. And indeed the number of thefe is of late grown too great to be fit to be here recited, efpecially fince I find them all fo very unfatisfactory, that I cannot but think the acute fticklers for any of them are rather driven to embrace it by the palpable inconveniences of the ways they reject, than by any thing they find to fatisfy them in that which they make choice of: and for my part I confefs I find fo much reafon in what each party fays againft the explications of the reft, that I think they all confute well, and none does well eftablifh.

But my prefent way of writing forbidding me to infift on many arguments againft the doctrine wherein they moft agree, I fhall only urge that which I confefs chiefly fticks with me, namely, that I find it not comprehenfible.

I know the modern fchoolmen fly here to their wonted refuge of an obfcure diftinction, and tell us, that the power of matter in reference to forms is partly eductive, as the agent can make the form out of it; and partly receptive, whereby it can receive the form fo made. But fince thofe that fay this will not allow that the form of a generated body was not actually pre-exiftent in its matter, or indeed any-where elfe; it is hard to conceive how a fubftance can be educed out of another fubftance totally diftinct in nature from it, without being before fuch eduction actually exiftent in it. And as for the receptive power of the matter, that but fitting it to receive or lodge a form when brought to be united with it, how can it be intelligibly made out to can-

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tribute to the production of a new ſubſtance of a quite differing nature from that matter, though it harbours it when produced? And it is plain that the human body hath a receptive power in reference to the human ſoul, which yet themſelves confeſs both to be a ſubſtantial form, and not to be educed out of the power of matter. Indeed if they would admit the form of a natural body to be but a more fine and ſubtile part of the matter, as ſpirit of wine is of wine, which upon its receſs remains no longer wine, but phlegm or vinegar, then the eductive power of matter might ſignify ſomething: and ſo it might, if with us they would allow the form to be but a modification of the matter; for then it would import, that the matter may be ſo ordered or diſpoſed by fit agents as to conſtitute a body of ſuch a ſort and denomination: and ſo (to reſume that example) the form of a ſphere may be ſaid to lurk potentially in a piece of braſs, inaſmuch as that braſs may by caſting, turning, or otherwiſe, be ſo figured as to become a ſphere. But this they will not admit, leſt they ſhould make forms to be but accidents, though it is, for aught I know, as little intelligible how what is educed out of any matter, without being either pre-exiſtent, or being any part of the matter, can be a true ſubſtance, as how that roundneſs that makes a piece of braſs become a ſphere, can be a new ſubſtance in it. Nor can they admit the other way of educing a form out of matter as ſpirit is out of wine, becauſe then not only matter will be corruptible againſt their grounds, but matter and form would not be two differing and ſubſtantial principles, but one and the ſame, though diverſified by firmneſs, groſſneſs, *&c.* which are but accidental differences. I know they ſpeak much of the efficacy of the agent upon the matter in the generation of natural bodies, and tell us ſtrange things of his manner of working. But not to ſpend time in examining thoſe obſcure niceties, I anſwer in ſhort, that ſince the agent, be he what he will, is but a phyſical and finite agent, and ſince what way ſoever he works he can do nothing repugnant to the nature of things, the difficulty that ſticks with me will ſtill remain. For if the form produced in generation be, as they would have it, a ſubſtance that was not before to be found any where out of that portion of matter wherewith it conſtitutes the generated body; I ſay, that either it muſt be produced by refining or ſubtiliating ſome parts of the matter into form, or elſe it muſt be produced out of nothing, that is, created (for I ſee no third way how a ſubſtance can be produced *de novo*). If they allow the firſt, then will the form be indeed a ſubſtance, but not, as they hold it is, diſtinct from matter; ſince matter, however ſubtiliated, is matter ſtill, as the fineſt ſpirit of wine is as truly a body as was the wine itſelf that yielded it, or as is the groſſer phlegm from which it was extracted: beſides that, the Peripateticks teach that the form is not made of any thing of the matter; nor indeed is it conceivable how a phyſical agent can turn a material into an immaterial ſubſtance, eſpecially matter being, as they themſelves confeſs, as well incorruptible as ingenerable. But if they will not allow, as indeed they do not, that the ſubſtantial form is made of any thing that is material, they muſt give me leave to believe that it is produced out of nothing, till they ſhew me how a ſubſtance can be produced otherwiſe that exiſted no-where before. And at this rate every natural body of a ſpecial denomination, as gold, marble, nitre, *&c.* muſt not be produced barely by generation, but partly by generation and partly by creation. And ſince it is confeſſed on all ſides that no natural agent can produce the leaſt atom of matter, it is ſtrange they ſhould in generation allow every phyſical agent the power of producing a form, which, according to them, is not only a ſubſtance but a far nobler one than matter; and thereby attribute to the meaneſt creatures that power of creating ſubſtances which the ancient naturaliſts thought too great to be aſcribed to God himſelf; and which indeed is too great to be aſcribed to any other than him. And therefore ſome ſchoolmen and philoſophers have derived forms immediately from God; but

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is not only to desert Ariſtotle and the Peripatetick philoſophy they would ſeem to maintain, but to put Omnipotence upon working I know not how many thouſand miracles every hour to perform that (I mean the generation of bodies of new denominations) in a ſupernatural way, which ſeems the moſt familiar effect of nature in her ordinary courſe.

And as the production of forms out of the power of matter is for theſe reaſons incomprehenſible to me; ſo thoſe things which the Peripateticks aſcribe to their ſubſtantial forms are ſome of them ſuch as I confeſs I cannot reconcile my reaſon to: for they tell us poſitively that theſe forms are ſubſtances, and yet at the ſame time they teach that they depend upon matter both *in fieri* and *in eſſe*, as they ſpeak; ſo that out of the matter that ſupports them they cannot ſo much as exiſt (whence they are uſually called material forms) which is to make them ſubſtances in name, and but accidents in truth. For not to aſk how (among phyſical things) one ſubſtance can be ſaid to depend upon another *in fieri* that is not made of any part of it, the very notion of a ſubſtance is to be a ſelf-ſubſiſting entity, or that which needs no other created being to ſupport it or to make it exiſt. Beſides that, there being but two ſorts of ſubſtances, material and immaterial, a ſubſtantial form muſt appertain to one of the two, and yet they aſcribe things to it that make it very unfit to be referred to either. To all this I add, that theſe imaginary material forms do almoſt as much trouble the doctrine of corruption as that of generation: for if a form be a true ſubſtance really diſtinct from matter, it muſt, as I lately noted, be able to exiſt of itſelf without any other ſubſtance to ſupport it; as thoſe I reaſon with confeſs that the ſoul of man ſurvives the body it did before death inform: whereas they will have it that in corruption the form is quite aboliſhed and utterly periſhes, as not being capable of exiſting, ſeparated from the matter whereunto it was united. So that here again what they call a ſubſtance they make indeed an accident, and beſides contradict their own vulgar doctrine, that natural things are upon their corruption reſolved into the firſt matter; ſince, at this rate they ſhould ſay, that ſuch things are but partly reſolved into the firſt matter, and partly either into nothing, or into forms, which, being as well immaterial as the ſouls of men, muſt, for aught appears, be alſo, like them, accounted immortal.

I should now examine thoſe arguments that are wont to be employed by the ſchools to evince their ſubſtantial forms; but, beſides that the nature and ſcope of my preſent work enjoins me brevity, I confeſs that, one or two excepted, the arguments I have found mentioned as the chief, are rather metaphyſical or logical, than grounded upon the principles and phænomena of nature, and reſpect rather words than things; and therefore I, who have neither inclination nor leiſure to wrangle about terms, ſhall content myſelf to propoſe, and very briefly anſwer, two or three of thoſe that are thought the plauſibleſt.

First then they thus argue, *omne compoſitum ſubſtantiale* (for it is hard to Engliſh well ſuch uncouth terms) *requirit materiam & formam ſubſtantialem, ex quibus componatur. Omne corpus naturale eſt compoſitum ſubſtantiale: ergo*, &c. In this ſyllogiſm ſome do plauſibly enough deny the conſequence, but for brevity's ſake I ſhall rather ch.ſe to deny the minor, and deſire the propoſers to prove it. For I know not any thing in nature that is compoſed of matter, and a ſubſtance diſtinct from matter, except man, who alone is made up of an immaterial form and a human body: and if it be properly ſaid to be *compoſita ſubſtantialia*, I ſhall, rather than wrangle with them, give them leave to find out ſome other name for other natural things.

But then they argue, in the next place, that if there were no ſubſtantial forms, all bodies would be but *entia per accidens*, as they ſpeak; which is abſurd. To which I anſwer, that in the notion that divers learned men have of an *ens per accidens*, namely, that

that it is that which conſiſts of thoſe things *quæ non ordinantur ad unum*, it may be ſaid, that though we do not admit ſubſtantial forms, yet we need not admit natural bodies to be *entia per accidens*; becauſe in them the ſeveral things that concur to conſtitute the body, as matter, ſhape, ſituation, and motion, *ordinantur per ſe & intrinſicè* to conſtitute one natural body. But if this anſwer ſatisfy not, I ſhall add, that for my part that which I am ſolicitous about is, that what nature hath made things to be in themſelves, not what, logician or metaphyſician will call them in the terms of his art; it being much fitter in my judgment to alter words, that they may better fit the nature of things, than to affix a wrong nature to things that they may be acommodated to forms or words that were probably deviſed, when the things themſelves were not known or well underſtood, if at all thought on.

Wherefore I ſhall but add one argument more of this ſort; and that is, that if there were no ſubſtantial forms, neither could there be any ſubſtantial definitions; but the conſequent is abſurd, and therefore ſo is the antecedent. To which I reply, that ſince the Peripateticks themſelves confeſs the forms of bodies to be of themſelves unknown; all that this argument ſeems to me to conclude is but this, that if we do not admit ſome things that are not *in rerum naturâ*, we cannot build our definitions upon them: nor indeed could we, if we ſhould admit ſubſtantial forms, give ſubſtantial definitions of natural things, unleſs we could alſo define natural bodies by things that we know not; for ſuch * the ſubſtantial forms are (as we have ſeen already) confeſſed to be by the wiſeſt Peripateticks, who pretend not to give the ſubſtantial definition of any natural *compoſitum*, except man. But it may ſuffice us to have, inſtead of ſubſtantial, eſſential definitions of things; I mean ſuch as are taken from the eſſential differences of things, which conſtitute them in ſuch a ſort of natural bodies, and diſcriminate them from all thoſe of any other ſort.

These three arguments, Pyrophilus, for ſubſtantial forms, you may poſſibly, as well as I, find variouſly propoſed, and perhaps with ſome light alterations multiplied in the writings of the Peripateticks and ſchoolmen; but all the arguments of this kind that I have met with, may, if I miſtake not, be ſufficiently ſolved by the anſwers we have given to theſe, or at leaſt by the grounds upon which thoſe anſwers are built; thoſe ſeemingly various arguments agreeing in this, that either they reſpect rather words than things, or that they are grounded upon precarious ſuppoſitions; or laſtly that they urge that as an abſurdity, which, whether it be one or not in thoſe that admit the Peripatetick philoſophy, to me, that do as little acquieſce in many of their other principles as I do in their ſubſtantial forms, doth not appear any abſurdity at all. And it is perhaps for fear that arguments of this ſort ſhould not much prevail with naturaliſts, that ſome of the modern aſſertors of the forms we queſtion have thought it requiſite to add ſome more phyſical arguments, which (though I have not found them all in the ſame writers, yet) being in all but few, I ſhall here briefly conſider them.

First then, among the phyſical arguments that are brought to prove ſubſtantial forms, I find that the moſt confidently inſiſted on, which is taken from the ſpontaneous return of heated water to coldneſs; which effects, ſay they, muſt neceſſarily be aſcribed to the action of the ſubſtantial form whoſe office it is to preſerve the body in its natural ſtate, and when there is occaſion to reduce it thereunto: and the argument indeed might be plauſible, if we were ſure that heated water would grow cold again (without the avolation of any parts more agitated than the reſt) ſuppoſing it to be removed into ſome of the imaginary ſpaces beyond the world; but as the caſe is, I ſee no neceſſity of flying to a ſubſtantial form, the matter ſeeming to be eaſily explicable otherwiſe. The

* *Nego tibi ullam eſſe formam nobis notam plenè & planè, noſtramque ſcientiam eſſe umbram in ſole.* Scalig.

water we heat is ſurrounded with our air, or with ſome veſſel, or other body contiguous to the air, and both the air and the water in theſe climates are moſt commonly leſs agitated than the juices in our hands, or other organs of touching, which makes us eſteem and call thoſe fluids cold. Now when the water is expoſed to the fire, it is thereby put into a new agitation more vehement than that of the parts of our ſenſory, which you will eaſily grant, if you conſider that when the heat is intenſe it makes the water boil and ſmoke, and oftentimes run over the veſſel; but when the liquor is removed from the fire, this acquired agitation muſt needs by degrees be loſt, either by the avolation of ſuch fiery corpuſcles as the Epicureans imagine to be got into heated water, or by the water's communicating the agitation of its parts to the contiguous air, or to the veſſel that contains it, till it have loſt its ſurpluſage of motion, or by the ingreſs of thoſe frigorifick atoms, wherewith (if any ſuch be to be granted) the air in theſe climates is wont to abound, and ſo be reduced into its former temperature: which may as well be done without a ſubſtantial form, as if a ſhip ſwimming ſlowly down a river ſhould by a ſudden guſt of wind, blowing the ſame way the ſtream runs, be driven on much faſter than before, the veſſel upon the ceaſing of the wind may, without any ſuch internal principle, return after a while to its former ſlowneſs of motion. So that in this phænomenon we need not have recourſe to an internal principle, the temperature of the external air being ſufficient to give an account of it. And if water be kept (as is uſual in poor mens houſes that want cellars) in the upper rooms of the houſe, in caſe the climate be hot, the water will, in ſpite of the form, continue far leſs cold than, according to the Peripateticks, its nature requires, all the ſummer long. And let me here repreſent to the champions of forms, that, according to their doctrine, the fluidity of the water muſt at leaſt as much proceed from its form as the coldneſs; and yet this does ſo much depend upon the temperature of the air, that in Nova Zembla vaſt quantities of water are kept in the hard and ſolid form of ice all the year long by the ſharp cold of the ambient air, notwithſtanding all the pretended office and power of the ſubſtantial form to keep it fluid; which it will never be reduced to be, unleſs by ſuch a thawing temperature of the air as would itſelf, for aught appears, make it flow again, although there were no ſubſtantial form *in rerum naturâ*.

There is another argument much urged of late by ſome learned men, the ſubſtance whereof is this; that matter being indifferent to one ſort of accidents as well as to another, it is neceſſary there ſhould be a ſubſtantial form to keep thoſe accidents, which are ſaid to conſtitute it, united to the matter they belong to, and preſerve both them and the body in their natural ſtate: for ſince it is confeſſed that matter hath no appetite to theſe accidents more than to any others, they demand how without a ſubſtantial form theſe accidents can be contained and preſerved? To this I might repreſent that I am not ſo well ſatisfied with the notion wont to be taken for granted, not only by the vulgar, but by philoſophers, of the natural ſtate of bodies; as if it were undeniable that every natural body (for as to ſome I ſhall not now queſtion it) has a certain ſtate wherein nature endeavours to preſerve it, and out of which it cannot be put, but by being put into a preternatural ſtate. For the world being once conſtituted by the great Author of things as it now is, I look upon the phænomena of nature to be cauſed by the local motion of one part of matter hitting againſt another, and am not ſo fully convinced that there is ſuch a thing as nature's deſigning to keep ſuch a parcel of matter in ſuch a ſtate that is clothed with juſt ſuch accidents, rather than with any other. But I look upon many bodies, eſpecially fluid ones, as frequently changing their ſtate, according as they happen to be more or leſs agitated or otherwiſe wrought upon by the ſun, and other conſiderable agents in nature. As the air, water, and other fluids, if the temperature as to cold or heat, and rarefaction or condenſation,

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which they are in at the beginning of the ſpring here at London, be pitched upon as their natural ſtate; then not only in the torrid and frozen zones they muſt have other and very differing natural ſtates, but here itſelf they will almoſt all the ſummer and all the winter (as our weather-glaſſes inform us) be in a varying preternatural ſtate, becauſe they will be in thoſe ſeaſons either more hot and rarified, or more cold and condenſed, than in the beginning of the ſpring. And in more ſtable and conſtant bodies, I take in many caſes the natural ſtate to be but either the moſt uſual ſtate, or that wherein that which produces a notable change in them finds them. As when a ſlender piece of ſilver that is moſt commonly flexible, and will ſtand bent every way, comes to be well hammered, I count that flexibility to be the natural ſtate of that metal, becauſe moſt commonly ſilver is found to be flexible, and becauſe it was ſo before it was hammered; but the ſpringineſs it acquires by hammering is a ſtate which properly is no more unnatural to the ſilver than the other, and would continue with the metal as long as it, if both pieces of ſilver, the one flexible the other ſpringy, were let alone and kept from outward violence. And as the ſilver, to be deprived of its flexibleneſs, needed the violent motion of the hammer, ſo to deprive it of its ſpring it needs the violent agitation of a nealing fire. Theſe things and much more I might here repreſent; but to come cloſe to the objection, I anſwer, that the accidents ſpoken of are introduced into the matter by the agents or efficient cauſes, whatever they be that produce in it what (in the ſenſe formerly explained) we call an eſſential (though not a ſubſtantial) form. And theſe accidents being once thus introduced into the matter, we need not ſeek for a new ſubſtantial principle to preſerve them there, ſince by the general law or common courſe of nature the matter qualified by them muſt continue in the ſtate ſuch accidents have put it into, till by ſome agent or other it be forcibly put out of it, and ſo diveſted of thoſe accidents: as in the formerly mentioned example borrowed from Ariſtotle, of a brazen ſphere, when once the motion of tools impelled and guided by the artificer have turned a piece of braſs into a ſphere, there needs no new ſubſtance to preſerve that round figure, ſince the braſs muſt retain it till it be deſtroyed by the artificer himſelf, or ſome other agent able to overcome the reſiſtance of the matter, to be put into another figure. And on this occaſion let me confirm this *ad hominem*, by repreſenting that there is not an inconſiderable party among the Peripateticks themſelves who maintain, that in the elements the firſt qualities (as they call them) are inſtead of forms, and that the fire (for inſtance) hath no other form than heat and drineſs, and the water than coldneſs and moiſture. Now if theſe bodies, that are the vaſteſt and the moſt important of the ſublunary world, conſiſt but of the univerſal matter and the few accidents; and if in theſe there needs no ſubſtantial form to keep the qualities of the matter united to it, and conjoined among themſelves, and preſerve them in that ſtate as long as the law of nature requires; though beſides the four qualities that are called firſt, the elements have divers others, as gravity and levity, firmneſs and fluidity, opacouſneſs and tranſparency, &c. why ſhould the favourers of this opinion deny that in other bodies, beſides the elements, qualities may be preſerved and kept united to the matter they belong to without the band or ſupport of a ſubſtantial form? And as, when there is no competent deſtructive cauſe, the accidents of a body will by the law of nature remain ſuch as they were; ſo if there be, it cannot with reaſon be pretended that the ſubſtantial form is able to preſerve all thoſe accidents of a body that are ſaid to flow from it, and to be as it were under its care and tuition. For if, for inſtance, you expoſe a ſphere or bullet of lead to a ſtrong fire, it will quickly loſe (not to mention its figure) both its coldneſs, its conſiſtence, its malleableneſs, its colour (for it will appear of the colour of fire) its flexibility, and ſome other qualities; and all this in ſpite of the imaginary ſubſtantial form, which, according to the Peripatetical principles, in this case must still remain in it without being able to help it. And though, upon the taking the lead from off the fire, it is wont to be educed to most of its former qualities (for it will not of itself recover its sphericity) yet that may well be ascribed partly to its peculiar texture, and partly to the coldness of the ambient air, according to what we lately discoursed touching heated and refrigerated water; which temperature of the air is an extrinsical thing to the lead, and indeed it is but accidental that the lead upon refrigeration regains its former qualities: for in case the lead have been exposed long enough to a sufficiently intense fire, it will (as we have purposely tried) be turned into glass, and lose its colour, its opacity, its malleableness, and (former degree of) flexibleness, and acquire a reddishness, a degree of transparency, a brittleness, and some other qualities that it had not before: and let the supposed substantial form do what it can, even when the vessel is removed from the fire, to reduce or restore the body to its natural state and accidents, yet the former qualities will remain lost as long as these preternatural ones introduced by the fire continue in the matter; and neither the one will be restored, nor the other destroyed, till some sufficiently powerful extrinsick agent effect the change. And on the other side I consider, that the fruit, when severed from the tree it grew on, is confessed to be no longer animated (at least the kernels or seeds excepted) by the vegetative soul or substantial form of the plant; yet in an orange or lemon (for instance) plucked from the tree, we see that the same colour, the same odour, the same taste, the same figure, the same consistence, and, for aught we know, the same other qualities, whether sensible or even occult, as are its antidotal and antiscorbutical virtues that must before be said to have flowed from the soul of the tree, will continue many months, perhaps some years, after the fruit has ceased to have any commerce with the tree (nay, though the tree whereon it grew be perhaps in the mean time hewn down or burnt, and though consequently its vegetative soul or form be destroyed) as when it grew thereon, and made up one plant with it. And we find that tamarinds, rhubarb, senna, and many other simples, will, for divers years after they have been deprived of their former vegetative soul, retain their purgative and other specifick properties.

I find it likewise urged that there can be no reason why whiteness should be separable from a wall and not from snow or milk, unless we have recourse to substantial forms. But in case men have agreed to call a thing by such a name, because it has such a particular quality that differences it from others, we need go no farther to find a reason why one quality is essential to one thing and not to another. As in our former example of a brass sphere, the figure is that for which we gave it that name; and therefore, though you may alter the figure of the matter, yet by that very alteration the body perishes in the capacity of a sphere, whereas its coldness may be exchanged for heat, without the making it the less a sphere, because it is not for any such quality, but for roundness, that a body is said to be a sphere. And so firmness is an inseparable quality of ice, though this or that particular figure be not, because that it is for want of fluidity that any thing that was immediately before a liquor is called ice; and congruously hereunto, though whiteness were inseparable from snow and milk, yet that would not necessarily infer that there must be a substantial form to make it so: for the firmness of the corpuscles that compose snow is as inseparable from it as the whiteness; and yet it is not pretended to be the effect of the substantial form of the water, but of the excess of the coldness of the air, which (to use vulgar, though perhaps inaccurate expressions) puts the water out of its natural state of fluidity and into a preternatural one of firmness and brittleness. And the reason why snow seldom loses its whiteness but with its nature, seems to be, that its component particles are so disposed, that the same heat of the ambient air that is fit to turn it into a transparent body is also fit to

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make it a fluid one, which when it is become, we no longer call it ſnow, but water; ſo that the water loſes its whiteneſs, though the ſnow do not. But if there be a cauſe proper to make a convenient alteration of texture in the ſnow without melting or reſolving it into water, it may then exchange its whiteneſs for yellowneſs without loſing its right to be called ſnow: as I remember I have read in an eminent writer, that *de facto*, in the northern regions towards the pole, thoſe parcels of ſnow that have lain very long on the ground degenerated in time into a yellowiſh colour, very differing from that pure whiteneſs to be obſerved in the neighbouring ſnow lately fallen.

But there yet remains an argument for ſubſtantial forms, which, though (perhaps becauſe phyſical) it will not be overlooked or ſlightly anſwered by their oppoſers, will for the ſame reaſon deſerve to be taken notice of here; and it is, that there ſeems to be a neceſſity of admitting ſubſtantial forms in bodies, that from thence we may derive all the various changes to which they are ſubject, and the differing effects they produce, the preſervation and reſtitution of the ſtate requiſite to each particular body, as alſo the keeping of its ſeveral parts united into one *totum*. To the anſwering of this argument ſo many things will be found applicable both in the paſt and ſubſequent parts of theſe notes, that I ſhall at preſent but point the chief particulars on which the ſolution is grounded.

I consider then, firſt, that many and great alterations may happen to bodies, which ſeem manifeſtly to proceed from their peculiar texture and the action of outward agents upon them, and of which it cannot be ſhewn that they could happen otherwiſe, though there were no ſubſtantial forms *in rerum naturâ*: as we ſee, that tallow (for inſtance) being melted by the fire, loſes its coldneſs, firmneſs, and its whiteneſs, and acquires heat, fluidity, and ſome tranſparency; all which being ſuffered to cool, it preſently changes for the three firſt named qualities. And yet divers of theſe changes are plainly enough the effects partly of the fire, partly of the ambient air, and not of I know not what ſubſtantial form: and as it is both evident and remarkable, what great variety of changes in qualities, and productions in new ones, the fire (that is, a body conſiſting of inſenſible parts, that are variouſly and vehemently moved) doth effect by its heat, that is, by a modified local motion; I conſider further, that various operations of a body may be derived from the peculiar texture of the whole, and the mechanical affections of the particular corpuſcles or other parts that compoſe it, as we have often occaſion to declare here and there in this treatiſe; and particularly by an inſtance, ere long to be further inſiſted on, namely, that though vitriol made of iron with a corroſive liquor be but a factitious body, made by a convenient appoſition of the ſmall parts of the ſaline menſtruum to thoſe of the metal, yet this vitriol will do moſt, if not all, of the ſame things, that vitriol made by nature in the bowels of the earth, and digged out thence, will perform: and each of theſe bodies may be endowed with variety of differing qualities, which I ſee not why they muſt flow in the native vitriol from a ſubſtantial form, ſince in the factitious vitriol the ſame qualities belong to a form that does plainly emerge from the coalition of metalline and ſaline corpuſcles aſſociated together and diſpoſed of after a certain manner.

And laſtly, as to what is very confidently as well as plauſibly pretended, That a ſubſtantial form is requiſite to keep the parts of a body united, without which it would not be one body; I anſwer, That the contrivance of conveniently figured parts, and in ſome caſes their juxta poſition, may, without the aſſiſtance of a ſubſtantial form, be ſufficient for this matter. For not to repeat what I juſt now mentioned concerning vitriol made by art, whoſe parts are as well united and kept together as thoſe of the native vitriol, I obſerve, that a pear grafted upon a thorn, or a plumb inoculated upon an apricot, will bear good fruit, and grow up with the ſtock, as though they both

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made but one tree, and were animated but by the ſame common form; whereas indeed both the ſtock and the inoculated or grafted plant have each of them its own form, as may appear by the differing leaves, and fruits, and ſeeds they bear. And that which makes to our preſent purpoſe is, that even vegetation and the diſtribution of aliments are in ſuch caſes well made, though the nouriſhed parts of the total plant, if I may ſo call it, have not one common ſoul or form; which is yet more remarkable in the miſletoes that I have ſeen growing upon old hazle-trees, crab-trees, apple-trees, and other plants, in which the miſletoe often differs very widely from that kind of plant on which it grows and proſpers. And for the durableneſs of the union betwixt bodies, that a ſubſtantial form is not requiſite to procure it, I have been induced to think, by conſidering that ſilver and gold, being barely mingled by infuſion, will have their minute parts more cloſely united than thoſe of any plant or animal that we know of. And there is ſcarce any natural body wherein the form makes ſo ſtrict, durable, and indiſſoluble an union of the parts it conſiſts of, as that which in that factitious concrete we call glaſs ariſes from the bare commiſtion of the corpuſcles of ſand with thoſe ſaline ones wherewith they are colliquated by the violence of the fire: and the like may be ſaid of the union of the proper accidents of glaſs with the matter of it, and betwixt one another.

To draw towards a concluſion: I know it is alledged as a main conſideration on the behalf of ſubſtantial forms, that theſe being in natural bodies the true principles of their properties, and conſequently of their operations, their natural philoſophy muſt needs be very imperfect and defective, who will not take in ſuch forms; but for my part I confeſs, that this very conſideration does rather indiſpoſe than incline me to admit them. For if indeed there were in every natural body ſuch a thing as a ſubſtantial form, from which all its properties and qualities immediately flow, ſince we ſee that the actions of bodies upon one another are for the moſt part (if not all) immediately performed by their qualities or accidents, it would ſcarce be poſſible to explicate very many of the explicable phænomena of nature without having recourſe to them; and it would be ſtrange if many of the abſtruſer phænomena were not explicable by them only. Whereas indeed almoſt all the rational accounts to be met with of difficult phænomena are given by ſuch as either do not acknowledge or at leaſt do not take notice of ſubſtantial forms. And it is evident by the clear ſolutions (untouched by many vulgar philoſophers) we meet with of many phænomena in the Staticks and other parts of the Mechanicks, and eſpecially in the Hydroſtaticks and Pneumaticks, how clearly many phænomena may be ſolved without employing a ſubſtantial form. And on the other ſide, I do not remember that either Ariſtotle himſelf (who perhaps ſcarce ever attempted it) or any of his followers, has given a ſolid and intelligible ſolution of any one phænomenon of nature by the help of ſubſtantial forms; which you need not think it ſtrange I ſhould ſay, ſince the greateſt patrons of forms acknowledging their nature to be * unknown to us, to explain any effect by a ſubſtantial form, muſt be to declare (as they ſpeak) *ignotum per ignotius*, or at leaſt *per æquè ignotum*. And indeed to explicate a phænomenon being to deduce it from ſomething elſe in nature more known to us than the thing to be explained by it, how can the employing of incomprehenſible (or at leaſt uncomprehended) ſubſtantial forms help us to explain intelligibly this or that particular phænomenon? For to ſay, that ſuch an effect proceeds not from this or that quality of the agent, but from its ſubſtantial form, is to take an eaſy way to reſolve all difficulties in general, without rightly reſolving any one in particular; and would make

* *Nomina tu lapidis, qui quotidie tuis oculis obſervatur, formam, & Phillida ſolus habeto.* Scal. contra Card.

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a rare philoſophy, if it were not far more eaſy than ſatisfactory: for if it be demanded why jet attracts ſtraws, rhubarb purges choler, ſnow dazzles the eyes rather than graſs, *&c.* to ſay, that theſe and the like effects are performed by the ſubſtantial forms of the reſpective bodies, is at beſt but to tell me what is the agent, not how the effect is wrought; and ſeems to be but ſuch a kind of general way of anſwering, as leaves the curious enquirer as much to ſeek for the cauſes and manner of particular things, as men commonly are for the particular cauſes of the ſeveral ſtrange things performed by witchcraft, though they be told that it is ſome devil that does them all. Wherefore I do not think but that natural philoſophy, without being for that the more defective, may well enough ſpare the doctrine of ſubſtantial forms as an uſeleſs theory; not that men are arrived to be able to explicate all the phænomena of nature without them, but becauſe whatever we cannot explicate without them, we cannot neither intelligibly explicate by them.

And thus, Pyrophilus, I have offered you ſome of thoſe many things that indiſpoſed me to acquieſce in the received doctrine of ſubſtantial forms; but in caſe any more piercing enquirer ſhall perſuade himſelf that he underſtands it thoroughly, and can explicate it clearly, I ſhall congratulate him for ſuch happy intellectuals, and be very ready to be informed by him. But ſince what the ſchools are wont to teach of the origin and attributes of ſubſtantial forms, is that which I confeſs I cannot yet comprehend; and ſince I have ſome of the moſt eminent perſons among the modern philoſophers to join with me, though perhaps not for the ſame conſiderations, in the like confeſſion, that it is not neceſſary the reaſon of my not finding this doctrine conceivable muſt be rather a defectiveneſs in my underſtanding, than the unconceivable nature of the thing itſelf; I, who love not (in matters purely philoſophical) to acquieſce in what I do not underſtand, nor to go about to explicate things to others by what appears to me itſelf inexplicable, ſhall, I hope, be excuſed, if, leaving thoſe that contend for them the liberty of making what uſe they can of ſubſtantial forms, I do, till I be better ſatisfied, decline employing them myſelf, and endeavour to ſolve thoſe phænomena I attempt to give an account of, without them; as not ſcrupling to confeſs, that thoſe that I cannot explicate, at leaſt in a general way, by intelligible principles, I am not yet arrived to the diſtinct and particular knowledge of.

Now for our doctrine touching the origin of forms, it will not be difficult to collect it from what we formerly diſcourſed about qualities and forms together: for the form of a natural body being, according to us, but an eſſential modification, and as it were the ſtamp of its matter; or ſuch a convention of the bigneſs, ſhape, motion (or reſt) ſituation and contexture (together with the thence reſulting qualities) of the ſmall parts that compoſe the body, as is neceſſary to conſtitute and denominate ſuch a particular body; and all theſe actions being producible in matter by local motion, it is agreeable to our hypotheſis to ſay, that the firſt and univerſal, though not immediate cauſe of forms, is none other but God, who put matter into motion (which belongs not to its eſſence) and eſtabliſhed the laws of motion amongſt bodies, and alſo, according to my opinion, guided it in divers caſes at the beginning of things; and that, among ſecond cauſes, the grand efficient of forms in local motion, which, by variouſly dividing, ſequeſtering, tranſpoſing, and ſo connecting the parts of matter, produces in them thoſe accidents and qualities, upon whoſe account the portion of matter they diverſify, comes to belong to this or that determinate ſpecies of natural bodies, which yet is not ſo to be underſtood, as if motion were only an efficient cauſe in the generation of bodies, but very often (as in water, fire, *&c.*) it is alſo one of the chief accidents that concur to make up the form.

But in this laſt ſummary account of the origin of forms, I think myſelf obliged to declare to you a little more diſtinctly what I juſt now intimated to be my own opinion. And this I ſhall do by advertiſing you, that though I agree with our Epicureans in thinking it probable that the world is made up of an innumerable multitude of ſingly inſenſible corpuſcles endowed with their own ſizes, ſhapes, and motions; and though I agree with the Carteſians in believing (as I find that * Anaxagoras did of old) that matter hath not its motion from itſelf, but originally from God; yet in this I differ both from Epicurus and Des Cartes, that whereas the former of them plainly denies that the world was made by any deity (for deities he owned) and the latter of them, for aught I can find in his writings, or thoſe of ſome of his moſt eminent diſciples, thought that God, having once put matter into motion, and eſtabliſhed the laws of that motion, needed not more particularly interpoſe for the production of things corporeal, nor even of plants or animals, which, according to him, are but engines: I do not at all believe that either theſe Carteſian laws of motion, or the Epicurean caſual concourſe of atoms, could bring mere matter into ſo orderly and well contrived a fabrick as this world; and therefore I think, that the wiſe Author of nature did not only put matter into motion, but, when he reſolved to make the world, did ſo regulate and guide the motions of the ſmall parts of the univerſal matter, as to reduce the greater ſyſtems of them into the order they were to continue in; and did more particularly contrive ſome portions of that matter into ſeminal rudiments or principles, lodged in convenient receptacles (and as it were wombs) and others into the bodies of plants and animals: one main part of whoſe contrivance did, as I apprehend, conſiſt in this, that ſome of their organs were ſo framed, that, ſuppoſing the fabrick of the greater bodies of the univerſe, and the laws he had eſtabliſhed in nature, ſome juicy and ſpirituous parts of theſe living creatures muſt be fit to be turned into prolifick ſeeds, whereby they may have a power, by generating their like, to propagate their ſpecies. So that, according to my apprehenſion, it was at the beginning neceſſary that an intelligent and wiſe agent ſhould contrive the univerſal matter into the world (and eſpecially ſome portions of it into ſeminal organs and principles) and ſettle the laws according to which the motions and actions of its parts upon one another ſhould be regulated; without which interpoſition of the world's architect, however, moving matter may, with ſome probability (for I ſee not in the notion any certainty) be conceived to be able, after numberleſs occurſions of its inſenſible parts, to caſt itſelf into ſuch grand conventions and convolutions as the Carteſians call *vortices*, and (as I remember) † Epicurus ſpeaks of under the name of προσκρίσεις, καὶ δινήσεις; yet I think it utterly improbable that brute and unguided, though moving matter, ſhould ever convene into ſuch admirable ſtructures as the bodies of perfect animals. But the world being once framed, and the courſe of nature eſtabliſhed, the naturaliſt (except in ſome few caſes where God or incorporeal agents interpoſe) has recourſe to the firſt cauſe but for its general and ordinary ſupport and influence, whereby it preſerves matter and motion from annihilation or deſition; and in explicating particular phænomena conſiders only the ſize, ſhape, motion (or want of it) texture, and the reſulting qualities and attributes of the ſmall particles of matter. And thus in this great automaton, the world (as in a watch or clock) the materials it conſiſts of being left to themſelves, could never at the firſt convene into ſo curious an engine: and yet when the ſkilful artiſt has once made and ſet it a going, the phænomena it exhibits are to be accounted for by the number, bigneſs, proportion, ſhape,

* Ariſtotle, ſpeaking of Anaxagoras in the firſt chapter of his laſt book of his Phyſicks, hath this paſſage: *Dicit* (Anaxagoras) *cum omnia ſimul eſſent, atque* quieſcerent *tempore infinito, mentem* moviſſe *ac ſegregaſſe.*

† Epicurus in his epiſtle to Pythocles.

motion

motion (or endeavour) reft, coaptation, and other mechanical affections of the fpring, wheels, pillars, and other parts it is made up of: and thofe effects of fuch a watch that cannot this way be explicated, muft, for aught I know, be confeffed not to be fufficiently underftood.

But to return thither, whence my duty to the author of nature obliged me to make this fhort digreffion:

The hitherto propofed hypothefis touching the origination of forms hath, I hope, been rendered probable by divers particulars in the paft difcourfes, and will be both exemplified and confirmed by fome of the experiments that make the latter part of this prefent treatife, (efpecially the fifth and feventh of them) which containing experiments of the changing the form of a falt and a metal, do chiefly belong to the hiftorical or experimental part of what we deliver touching the origin of forms: and indeed, befides the two kinds of experiments prefently to be mentioned, we might here prefent you a third fort confifting partly of divers relations of metalline tranfmutations delivered upon their own credit by credible men that are not alchymifts, and partly of fome experiments (fome made, fome directed by us) of changing both bodies totally inflammable almoft totally into water; and a good part even of diftilled rain water without additament into earth; and diftilled liquors readily and totally mingleable with water *pro parte* into a true oil that will not mix with it: this fort of experiments, I fay, I might here annex if I thought fit in this place, either to lay any ftrefs upon thofe that I cannot my felf make out, or to transfer hither thofe experiments of changes amongft bodies not metalline that belong to another * treatife: but over and above what the paft notes and the experiments that are to follow them contain towards the making of what we teach concerning forms; we will here for further confirmation proceed to add two forts of experiments (befides the third already mentioned): the one, wherein it appears that bodies of very differing natures being put together, like the wheels and other pieces of a watch, and by their connection acquiring a new texture, and fo new qualities may, without having recourfe to a fubftantial form, compofe fuch a new concrete as may as well deferve to have a fubftantial form attributed to it by virtue of that new difpofition of its parts as other bodies that are faid to be endowed therewith; and the other, that a natural body being diffipated and as it were taken in pieces like a watch, may have its parts fo affociated as to conftitute new bodies of natures very differing from its own and from each other, and yet thefe diffipated and fcattered parts, by being re-collected and put together again like the pieces of a watch in the like order as before, may recompofe (almoft, if not more than almoft) fuch another body as that they made up before they were taken afunder.

I. *Experiments and Thoughts about the Production and Reproduction of Forms.*

It was not at random that I fpoke, when in the foregoing notes, about the origin of qualities, I intimated that it was very much by a kind of tacit agreement that men had diftinguifhed the fpecies of bodies, and that thofe diftinctions were more arbitrary

* The Sceptical Chymift.

than we are wont to be aware of; for I confess that I have not yet, either in *Aristotle* or any other writer, met with any genuine and sufficient diagnostic and boundary for the discriminating and limiting the species of things; or to speak more plainly, I have not found that any naturalist has laid down a determinate number and sort of qualities or other attributes, which is sufficient and necessary to constitute all portions of matter endowed with them, distinct kinds of natural bodies: and therefore I observe that most commonly men look upon these as distinct species of bodies that have had the luck to have distinct names found out for them, though perhaps divers of them differ much less from one another than other bodies, which (because they have been huddled up under one name) have been looked upon as but one sort of bodies. But not to lay any weight on this intimation about names, I found that for want of a true characteristick or discriminating note, it hath been and is still both very uncertain as to divers bodies, whether they are of different species or of the same, and very difficult to give a sufficient reason why divers bodies wherein nature is assisted by art should not as well pass for distinct kinds of bodies as others that are generally reckoned to be so.

WHETHER (for instance) water and ice be not to be esteemed distinct kinds of bodies is so little evident that some that pretend to be very well versed in *Aristotle*'s writings and opinions, affirm him to teach that water loses not its own nature by being turned into ice; and indeed I remember I have read a * text of his that seems express enough to this purpose, and the thing itself is made plausible by the reduciblenes of ice back again into water. And yet I remember *Galen* is affirmed to make these two distinct species of bodies, which doctrine is favoured by the differing qualities of ice and water: for not only the one is fluid and the other solid and even brittle; but ice is also commonly more or less opacous in comparison of water, being also lighter than it in specie, since it swims upon it. To which may be added, that ice beaten with common salt will freeze other bodies when water mingled with salt will not: and on this occasion I would propose to be resolved, whether must, wine, spirit of wine, vinegar, tartar, and vappa be specifically distinct bodies? And the like question I would ask concerning a hen's egg and the chick that is afterwards hatched out of it; as also concerning wood, ashes, soot, and likewise the eggs of silkworms, which are first small caterpillars, or (as some think them) but worms when they are newly hatched, and then aurelias, (or husked maggots), and then butterflies; which I have observed with pleasure to be the successive production of the prolifick seed of silk-worms. And whether the answer to these queries be affirmative or negative, I doubt, the reason that will be given for either of the two will not hold in divers cases whereto I might apply it. And a more puzzling question it may be to some, whether a charcoal being thoroughly kindled do specifically differ from another charcoal? For according to those I argue with, the fire has penetrated it quite through; and therefore some of the recent Aristotelians are so convinced of its being transmuted, that all the satisfaction I could find from a very subtile modern schoolman to the objection, that if the glowing coal were plunged into water it would be a black coal again; was that, notwithstanding that reduction, the form of a charcoal had been once abolished by the fire, and was reproduced by God upon the regained disposition of the matter to receive it.

* See *Lib.* 1. *de Gen. & Corr. t.* 80. *Idem corpus* (says he there) *quanquam continuum, aliàs liquidum, aliàs concretum videmus, non divisione aut compositione hoc passum, aut conversione, aut attactu, sicuti* Democritus *asserit: nam neque transpositione, neque* Naturæ demutatione (ἢ τὸ μεταβάλλειν τὴν φύσιν) *ex liquido concretum evadere solet.*

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NOR is it very eafy to determine whether clouds, and rain, and hail, and fnow, be bodies fpecifically diftinct from water and from each other; and the writers of meteors are wont to handle them as diftinct. And if fuch flight differences as thofe, that difcriminate thefe bodies, or that which diftinguifhes wind from exhalations, whofe courfe makes it be fufficient to conftitute differing kinds of bodies, it will be hard to give a fatisfactory reafon why other bodies that differ in more, or more confiderable particulars, fhould not enjoy the fame privilege: and I prefume that fnow differs lefs from rain than paper doth from rags, or glafs made of wood-afhes does from wood. And indeed men having by tacit confent agreed to look upon paper, and glafs, and foap, and fugar, and brafs, and ink, and pewter, and gunpowder, and I know not how many others, to be diftinct forts of bodies; I fee not why they may not be thought to have done it on as good grounds as thofe upon which divers other differing fpecies of bodies have been conftituted. Nor will it fuffice to object that thefe bodies are factitious; for it is the prefent nature of bodies that ought to be confidered in referring them to fpecies, which way foever they came by that nature; for falt that is in many countries made by boiling fea-water in cauldrons and other veffels, is as well true fea-falt as that which is made in the *Ifle of Man* (as navigators call it) without any co-operation of man, by the bare action of the fun upon thofe parts of the fea-water which chance to be left behind in hollow places after a high fpring-tide. And filk-worms which will hatch by the heat of human bodies, and chickens that are hatched in *Ægypt* by the heat of ovens or dunghills are no lefs true filk-worms or chickens than thofe that are hatched by the fun or by hens.

As for what may be objected, that we muft diftinguifh betwixt factitious bodies and natural, I will not now ftay to examine how far that diftinction may be allowed; for it may fuffice for our prefent purpofe to reprefent, that whatever may be faid of factitious bodies, where man does, by inftruments of his own providing, only give figure, or alfo contexture to the fenfible (not infenfible) parts of the matter he works upon; as when a joiner makes a ftool, or a ftatuary makes an image, or a turner a bowl: yet the cafe may be very differing in thofe other factitious productions wherein the infenfible parts of matter are altered by natural agents who perform the greateft part of the work among themfelves, though the artificer be an affiftant, by putting them together after a due manner. And therefore I know not why all the productions of the fire made by chymifts fhould be looked upon as not natural, but artificial bodies; fince the fire, which is the grand agent in thefe changes, doth not, by being employed by the chymift, ceafe to be and to work as a natural agent: and fince nature herfelf doth, by the help of the fire, fometimes afford us the like productions that the alchymift's art prefents us: as in *Ætna*, *Vefuvius*, and other burning mountains, (fome of whofe productions I can fhew you), ftones are fometimes turned into lime, (and fo an alkalizate falt is produced), and fometimes, if they be more difpofed to be fluxed than calcined, brought to vitrification; metalline and mineral bodies are by the violence of the fire colliquated into maffes of very ftrange and compounded natures. Afhes and metalline flowers of divers kinds are fcattered about the neighbouring places, and copious flowers of fulphur fublimed by the internal fire, have been feveral times found about the vents at which the fumes are difcharged into the air: (as I have been affured by ingenious vifitors of fuch places, whom I purpofely inquired of touching thefe *flores*; for of thefe travellers more than one anfwered me they had themfelves gathered, and had brought fome very good). Not to add, that I have fometimes fufpected, upon no abfurd grounds, that divers of the minerals and other bodies we meet with in the lower parts of the earth, and think to have been formed and lodged there ever fince the beginning of things, have been ſince produced there by the help of ſubterraneal fires or other heats, which may, either by their immediate action and exceedingly long application, very much alter ſome bodies by changing their texture; as when lead is turned into minium and tin into putty, by the operation of the fire in a few hours, or by elevating, in the form of exhalations or vapours, divers ſaline and ſulphureous corpuſcles or particles of unripe, or (to uſe a chymical term of art) embryonated minerals, and perhaps metals, which may very much alter the nature, and thereby vary the kind of other ſubterraneal bodies which they pervade, and in which they often come to be incorporated; or elſe may, by convening among themſelves, conſtitute particular concretions; as we ſee that the fumes of ſulphur and thoſe of mercury unite into that lovely red maſs which in the ſhops they call vermilion, and which is ſo like to the mineral whence we uſually obtain mercury, that the Latins give them both the ſame name, *Cinnabaris*, and in that are imitated by the French and Italians; in whoſe favour I ſhall add, that if we ſuppoſe this mineral to conſiſt of a ſtony concretion penetrated by ſuch mineral fumes as I have been ſpeaking of, the appellation may be better excuſed than perhaps you imagined; ſince from *Cinnabaris nativa* not only I obtained a conſiderable quantity of good running mercury (which is that men are wont to ſeek for from it) but, to gratify my curioſity ſomewhat further, I tried an eaſy way that came into my mind, whereby the *caput mortuum* afforded me no deſpicable quantity of good combuſtible ſulphur: but this upon-the-by, being not obliged to ſet down here the grounds of my paradoxical conjecture about the effects of ſubterraneal fires and heats ſince I here lay no ſtreſs upon it, but return to what I was ſaying about *Ætna* and other Volcanos. Since then theſe productions of the fire being of nature's own making, cannot be denied to be natural bodies, I ſee not why the like productions of the fire ſhould be thought unworthy that name, only becauſe the fire that made the former was kindled by chance in a hill, and that which produced the latter was kindled by a man in a furnace. And if flower of ſulphur, lime, glaſs, and colliquated mixtures of metals and minerals are to be reckoned among natural bodies, it ſeems to be but reaſonable, that, upon the ſame grounds, we ſhould admit flower of antimony, lime, and glaſs, and pewter, and braſs, and many other chymical concretes (if I may ſo call them) to be taken into the ſame number; and then it will be evident that, to diſtinguiſh the ſpecies of natural bodies, a concourſe of accidents will, without conſidering any ſubſtantial form, be ſufficient.

But becauſe I need not on this occaſion have recourſe to inſtances of a diſputable nature, I will pitch, for the illuſtration of the mechanical production of forms, upon vitriol: for ſince nature herſelf without the help of art does oftentimes produce that concrete (as I have elſewhere ſhewn by experience) there is no reaſon why vitriol, produced by eaſy chymical operations ſhould not be looked upon as a body of the ſame nature and kind: and in factitious vitriol, our knowing what ingredients we make uſe of, and how we put them together, enables us to judge very well how vitriol is produced. But becauſe it is wont to be reckoned with ſaltpetre, ſea-ſalt, and ſal-gem among true ſalts, I think it requiſite to take notice, in the firſt place, that vitriol is not a mere ſalt, but that which *Paracelſus* ſomewhere, and after him divers other Spagyriſts call a *magiſtery*, which in their ſenſe (for there are that uſe it in another) commonly ſignifies a preparation, wherein the body to be prepared has not its principles ſeparated; as in diſtillation, incineration, &c. but wherein the whole body is brought into another form by the addition of ſome ſalt or menſtruum that is united *per minima* with it. And agreeably to this notion, we find that from common vitriol, whether native or factitious, may be obtained (by diſtillation and reduction) an acid ſaline ſpirit, and a metalline ſubſtance, as I elſewhere mention, that from blue vitriol copper may be (by more

than

than one way) ſeparated. And I the rather give this advertiſement, becauſe that as there is a vitriol of iron, which is uſually green, and another of copper, which is wont to be blue, and alſo a white vitriol about which it is diſputed what it holds (though that it holds ſome copper I have found); and yet of all theſe, are without ſcruple reputed true vitriols, notwithſtanding that they differ ſo much in colour, and (as I have diſcovered) in ſeveral other qualities; ſo I ſee no reaſon why the other minerals, being reduced by their proper menſtruums into ſalt like magiſteries, may not paſs for the vitriols of thoſe metals, and conſequently for natural bodies; which if granted, will add ſome confirmation to our doctrine, though its being granted is not neceſſary to make it out. For to confine ourſelves to vitriol, it is known among chymiſts, that if upon the filings of *Mars* one put a convenient quantity of that acid diſtilled liquor, which is (abuſively) wont to be called oil of vitriol, diluting the mixture with rain or with common water, it is eaſy by filtrating the ſolution, by evaporating the aqueous ſuperfluity of it, and by leaving the reſt for a competent while in a cellar (or other cold place) to cryſtallize; it is eaſy, I ſay, by this means to obtain a vitriol of iron; which agrees with the other vitriol of vitriol-ſtones or marchaſites preſented us by nature without the help of any other menſtruum than the rain that falls upon them from the clouds, in I know not how many qualities, part obvious, and part of them occult: as (of the firſt ſort) in colour, tranſparency, brittleneſs, eaſineſs of fuſion, ſtyptical taſte, reducibleneſs to a red powder by calcination, and other qualities more obvious to be taken notice of; to which may be annexed divers qualities of the ſecond ſort, (I mean the more abſtruſe ones), as the power to turn in a trice an infuſion of galls made in ordinary water, (as alſo to turn a certain clear mineral ſolution, elſewhere mentioned) into an inky colour, to which in all probability we may add a faculty of cauſing vomits even in a ſmall doſe, when taken into the ſtomach of a man, and that remarkable property of being endowed with as exact and curious a ſhape or figure as thoſe for which ſalts have been, by modern philoſophers eſpecially, ſo much admired. But that no ſcruple might ariſe from hence, that in the *vitriolum martis* wont to be made by chymiſts, the menſtruum that is employed is the oil of common vitriol, which may be ſuſpected to have retained the nature of the concrete whence it proceeded; and ſo this factitious vitriol may not be barely a new production, but partly a recorporification, as they ſpeak, of the vitriolate corpuſcles contained in the menſtruum: to prevent this ſcruple, I ſay (which yet perhaps would not much trouble a conſidering chymiſt) I thought fit to employ a quite other menſtruum that would not be ſuſpected to have any thing of vitriol in it. And though aqua fortis and ſpirit of nitre, however they corrode *Mars*, are unfit for ſuch a work; yet having pitched upon ſpirit of ſalt inſtead of oil of vitriol, and proceeding the ſame way that has been already ſet down, it anſwered our expectation, and afforded us a good green vitriol. Nor will the great diſpoſition I have obſerved in this our vitriol to reſolve, by the moiſture of the air into a liquor, make it eſſentially differing from other vitriols, ſince it has been obſerved, and particularly by *Guntherus Belichius* more than once, that even the common vitriol he uſed in *Germany* will alſo, though not ſo eaſily as other ſalts, run (as the chymiſts phraſe it) *per deliquium*. And to make the experiment more complete, though we did not find either oil of vitriol or ſpirit of ſalt, good menſtruums to make a blue venereal vitriol out of copper (however filed or thinly laminated) and though upon more trials than one, it appeared that aqua fortis and ſpirit of nitre, which we thought fit to ſubſtitute to the above-mentioned liquors, did indeed make a ſolution of copper, but ſo unctuous a one that it was very hard to bring any part of it to dryneſs without ſpoiling the colour and ſhape of the deſired body: yet repeating the experiment with care and watchfulneſs, we this way obtained one of the

loveliest

loveliest vitriols that hath perhaps been seen, and of which you yourself may be the judge by a parcel of it I keep by me for a rarity.

To apply now these experiments, especially that wherein spirit of salt is employed, to the purpose for which I have mentioned them, let us briefly consider these two things; the one, that our factitious vitriol is a body that, as well as the natural, is endowed with many qualities (manifest and occult) not only such as are common to it with other salts, as transparency, brittleness, solubleness in water, *&c.* but such as are properties peculiar to it, as greenness, easiness of fusion, stypticity of taste, a peculiar shape, a power to strike a black with infusion of galls, an emetick faculty, *&c.*

The other thing we are to consider is, that though these qualities are in common vitriol believed to flow from the substantial form of the concrete; and may as justly as the qualities, whether manifest or occult, of other inanimate bodies, be employed as arguments to evince such a form: yet in our vitriol, made with spirit of salt, the same qualities and properties were produced by the associating and juxta-position of the two ingredients, of which the vitriol was compounded; the mystery being no more but this, that the steel being dissolved in the spirit, the saline particles of the former, and the metalline ones of the latter having each their determinate shapes, did, by their association, compose divers corpuscles of a mixed or compounded nature, from the convention of many whereof there resulted a new body of such a texture, as qualified it to affect our sensories, and work upon other bodies, after such a manner as common vitriol is wont to do. And indeed in our case, not only it cannot be made appear, that there is any substantial form generated anew, but that there is not so much as an exquisite mixture, according to the common notion the schools have of such a mixture. For both the ingredients retain their nature (though perhaps somewhat altered) so that there is, as we were saying, but a juxta-position of the metalline and saline corpuscles; only they are associated so, as by the manner of their coalition to acquire that new texture which denominates the magistery they compose, vitriol. For it is evident that the saline ingredient may either totally, or for much the greatest part be separated by distillation, the metalline remaining behind. Nay, some of the qualities we have been ascribing to our vitriol, do so much depend upon texture, that the very beams of the sun (converged) will, as I have purposely tried, very easily alter its colour, as well as spoil its transparency, turning it at first from green to white; and, if they be concentred by a good burning-glass, making it change that livery for a deep red.

Doubts and Experiments touching the curious Figures of Salts.

AND here let me take notice, that though the exact and curious figures, in which vitriol and other salts are wont to shoot, be made arguments of the presence, and great instances of the plastick skill of substantial forms and seminal powers; yet, I confess, I am not so fully satisfied in this matter, as even the modern philosophers appear to be. It is not that I deny that *Plato*'s excellent saying, γεωμετρεῖ ὁ Θεός, may be applied to these exquisite productions of nature. For though God has thought fit to make things corporeal after a much more facile and intelligible way, than by the intervention of substantial forms; and though the plastick power of seeds, which in plants and animals I willingly admit, seem not in our case to be needful; yet is the divine architect's geometry

geometry (if I may ſo call it) nevertheleſs to be acknowledged and admired. For having been pleaſed to make the primary and inſenſible corpuſcles of ſalts and metals of ſuch determinate, curious, and exact ſhapes, that as they happen to be aſſociated together, they ſhould naturally produce concretions; which, though differingly figured, according to the reſpective natures of their ingredients, and the various manners of their convening, ſhould yet be all of them very curious, and ſeem elaborate in their kinds; how little I think is fit to be allowed, that the bodies of animals, which conſiſt of ſo many curiouſly framed and wonderfully adapted organical parts, and whoſe ſtructure is a thouſand times more artificial than that of ſalts and ſtones, and other minerals), can be reaſonably ſuppoſed to have been produced by chance, or without the guidance of an intelligent author of things, I have elſewhere largely declared. But I confeſs I look upon theſe figures we admire in ſalts, and in ſome kind of ſtones (which I have not been incurious to collect) as textures ſo ſimple and ſlight, in compariſon of the bodies of animals, and oftentimes in compariſon of ſome one organical part, that I think it cannot be in the leaſt inferred, that becauſe ſuch ſlight figurations need not be aſcribed to the plaſtick power of ſeeds, it is not neceſſary, that the ſtupendous and incomparably more elaborate fabrick and ſtructure of animals themſelves ſhould be ſo. And this premiſed I ſhall add, that I have been inclined to the conjecture about the ſhapes of ſalts, that I lately propoſed by theſe conſiderations.

First, That by a bare aſſociation of metalline and ſaline corpuſcles, a concrete, as finely figured as other vitriols may be produced, as we have lately ſeen.

Secondly, Becauſe that the figures of theſe ſalts are not conſtantly in all reſpects the ſame, but may in divers manners be ſomewhat varied, as they happen to be made to ſhoot more haſtily or more leiſurely, and as they ſhoot in ſcanter or fuller proportion of liquor. This may be eaſily obſerved by any that will but with a little attention conſider the difference that may be found in vitriolate cryſtals or grains, when quantities of them were taken out of the great coolers, as they call them, wherein that ſalt, at the works where it is boiled, is wont to be ſet to ſhoot. And accordingly where the experienced mineraliſt *Agricola* deſcribes the ſeveral ways of making vitriol in great quantities, he does not only more than once call the great grains or cryſtals, into which it coagulates, cubes; but ſpeaking of the manner of their concretion about the cords or ropes, that are wont (in *Germany*) to be hanged from certain croſs bars into the vitriolate water or ſolution for the vitriol to faſten itſelf to, he compares the concretions indifferently to cubes or cluſters of grapes; *Ex his* (ſays he, ſpeaking of the croſs-bars) *pendent reſtes lapillis extentæ, ad quos humor ſpiſſus adhæreſcens denſatur in tranſlucentes atramenti ſutorii vel cubos, vel acinos, qui uvæ ſpeciem gerunt* *. I remember alſo, that having many years ſince a ſuſpicion that the reaſon why alkalies, ſuch as ſalt of tartar and pot-aſhes, are wont to be obtained in the form of white powders or calces, might be the way wherein the water of the lixiviums, that contain them, is wont to be drawn off; I fancied that by leaving the ſaline corpuſcles a competent quantity of water to ſwim in, and allowing them leiſure for ſuch a multitude of occurſions as might ſuffice to make them hit upon more congruous coalitions than is uſual, I might obtain cryſtals of them, as well as of other ſalts: conjecturing this, I ſay, I cauſed ſome well purified alkalies diſſolved in clear water to be ſlowly evaporated, till the top was covered with a thin ice-like cruſt; then taking care not to break that, leſt they ſhould (as in the ordinary way, where the water is all forced off) want a ſufficient ſtock of liquor I kept them in a very gentle heat for a good while; and then breaking the above-mentioned ice-like cake, I

* *Georg. Agricola de re metall.* lib. 12. p. 462.

had,

had, as I wiſhed, divers figured lumps of cryſtalline ſalt ſhot in the water, and tranſparent almoſt like white ſugar-candy.

I LIKEWISE remember that having on ſeveral occaſions diſtilled a certain quantity of oil of vitriol with a ſtrong ſolution of ſea-ſalt, till the remaining matter was left dry, that ſaline reſidue being diſſolved in fair water, filtered, and gently evaporated, would ſhoot into cryſtals, ſometimes of one figure, ſometimes of another, according as the quantity or ſtrength of the oil of vitriol and other ſubſtances determined. And yet theſe cryſtals, though ſometimes they would ſhoot into priſm-like figures, as roched petre; and ſometimes into ſhapes more like to allom or vitriol; nay, though oftentimes the ſame *caput mortuum* diſſolved would in the ſame glaſs ſhoot into cryſtals, whereof ſome would be of one ſhape, ſome of another; yet would theſe differing grains or cryſtals appear for the moſt part more exquiſitely figured, than oftentimes vitriol does. From ſpirit of urine and ſpirit of nitre, when I have ſuffered them to remain long together before coagulation, and freed the mixture from the ſuperfluous moiſture very ſlowly, I have ſometimes obtained fine long cryſtals (ſome of which I can ſhew you) ſo ſhaped, that moſt beholders would take them for cryſtals of ſalt-petre. And I have likewiſe tried, that whereas ſilver is wont to ſhoot into plates exceeding thin, almoſt like thoſe of *Moſcovia* glaſs, when I have diſſolved a pretty quantity of it in aqua fortis or ſpirit of nitre, and ſuffered it to ſhoot very leiſurely, I have obtained lunar cryſtals, (ſeveral of which I have yet by me), whoſe figure, though ſo pretty as to have given ſome wonder even to an excellent geometrician, is differing enough from that of the thin plates formerly mentioned; each cryſtal being compoſed of many ſmall and finely ſhaped ſolids, that ſtick ſo congruouſly to one another, as to have one ſurface that appeared plain enough, common to them all.

THIRDLY, That inſenſible corpuſcles of different, but all of them exquiſite ſhapes, and endowed with plain, as well as ſmooth ſides, will conſtitute bodies variouſly, but all very finely figured; I have made uſe of ſeveral ways to manifeſt. And firſt, though harts horn, blood, and urine, being reſolved, and (as the chymiſts ſpeak) analyzed by diſtillation, may well be ſuppoſed to have their ſubſtantial forms (if they had any) deſtroyed by the action of the fire; yet in regard the ſaline particles they contain, are endowed with ſuch figures as we have been ſpeaking of, when in the liquor, that abounds with either of theſe volatile ſalts, the diſſolved particles do leiſurely ſhoot into cryſtals, I have divers times obſerved in theſe many maſſes (ſome bigger and ſome leſs) whoſe ſurfaces had plains, ſome of figures, as to ſenſe, exactly geometrical, and others very curious and pleaſant. And of theſe finely ſhaped cryſtals of various ſizes, I have pretty ſtore by me. And becauſe (as it may be probably gathered from the event) the ſaline corpuſcles of ſtillatitious acid liquors, and thoſe of many of the bodies they are fitted to diſſolve, have ſuch kind of figures as we have been ſpeaking of, when the ſolutions of theſe bodies, upon the receſs of the ſuperfluous moiſture, ſhoot into cryſtals; theſe, though they will ſome times be differing enough, according the particular natures of the diſſolved bodies and the menſtruum, yet either the cryſtals themſelves, or their ſurfaces, or both, will oftentimes have fine and exquiſite figures; as I have tried by a menſtruum, wherewith I was able to diſſolve ſome gems, as alſo with a ſolution of coral made with ſpirit of verdigreaſe, to omit other examples. And for the ſame reaſon when I tried, whether the particles of ſilver, diſſolved in aqua fortis, would not, without coagulating with the ſalts, convene, upon the account of their own ſhapes, into little concretions of ſmooth and flat ſurfaces, I found that having (to afford the metalline corpuſcles ſcope to move in) diluted one part of the ſolution with a great many parts of diſtilled rain-water, for common water will often-times make ſuch ſolutions become white or turbid) a plate of copper being ſuſpended in the liquor, and ſuffered to lie

lie quiet there a while, (for it need not be long) there would ſettle all about it ſwarms of little metalline and undiaphanous bodies, ſhining in the water like the ſcales of ſmall fiſhes, but formed into little plates extremely thin, with ſurfaces not only flat, but exceeding gloſſy: and among thoſe, divers of the larger were prettily figured at the edges. And as for gold, its corpuſcles are ſufficiently diſpoſed to convene with thoſe of fit or congruous ſalts into concretions of determinate ſhapes, as I have found in the cryſtals I obtained from gold diſſolved in aqua regis, and after having been ſuffered to loſe its ſuperfluous moiſture, kept in a cold place; and not only ſo, but alſo when, by a more powerful menſtruum, I had ſubdivided the body of gold into ſuch minute particles, that they were ſublimable, (for that I can aſſure you is poſſible;) theſe volatile particles of gold, with the ſalts wherewith they were elevated, afforded me (ſometimes) ſtore of cryſtals, which, though not all of them near of the ſame bigneſs, reſembled one another in their ſhape, which was regular enough, and a very pretty one. But of this more elſewhere.

§ I Remember I have long ſince taken pleaſure to diſſolve two or more of thoſe ſaline bodies, whoſe ſhapes we know already, in fair water, that by a very gentle evaporation I might obtain concretions whoſe ſhapes ſhould be, though curious, yet differing from the figure of either of the ingredients. But we muſt not expect that in all caſes the ſalts diſſolved together ſhould be totally compounded: for oftentimes they are of ſuch different natures, that one will ſhoot much ſooner than another, and then it frequently happens, that a good proportion of that will be firſt cryſtallized in its own ſhape; as is conſpicuouſly to be obſerved in the refining of that impure petre, (which from the country, that affords it, the purifiers call *Barbary* nitre), from the common ſalt it abounds with: and (alſo) as *Agricola* obſerves *, that in ſome caſes, where a vitriolate matter is mingled with that which yields allom, thoſe two kinds of ſalts will ſhoot ſeparately in the ſame large veſſel; (which the trials I have made with the compounded ſolutions of thoſe two ſalts do not diſcountenance.) Now in ſuch caſes all that can be expected, or need be deſired, is, that the remaining part of the mixture, or ſome portion of it, afford cryſtals or grains of compounded ſolid figures. Though the *Venetian* borax, wont to be ſold in ſhops, be known to be a factitious body, compounded of ſeveral ſalts, that I ſhall not now ſtay to enumerate; and though when we buy it, we uſually find it to conſiſt of lumps and grains miſhapen enough, yet when I diſſolved ſome of it in a good quantity of fair water, and made it coagulate very leiſurely, I had cryſtals, upon whoſe ſurfaces I could perceive very exquiſite, and, as to ſenſe, regular and geometrical figures. And one thing I muſt not here by any means pretermit, which is, that though the *caput mortuum* of common aqua fortis conſiſts of bodies of very differing natures (for ſuch are nitre and vitriol), and has been expoſed to a great violence of the fire; yet I have ſometimes admired the curiouſneſs of thoſe figures, that might be obtained barely by frequent ſolutions and coagulations of the ſaline particles of this *caput mortuum* in fair water. But becauſe the glaſſes wherein my concretions were made, were too little to afford great cryſtals, and they ought to ſhoot very ſlowly; I choſe rather to ſhew the curious ſome large cryſtals, which I took out of the laboratory of an ingenious perſon, who, without minding the figures, had upon my recommendation made great quantity of that ſalt in large veſſels for a medicine; (it being the *Panacea duplicata*, ſo famous in *Holſtein*). For divers of theſe cryſtals have not only triangles, hexagons, and rhomboides, and other figures exquiſitely cut on their ſmooth and ſpecular ſurfaces, and others, bodies of priſmatical ſhapes: but ſome of them are no leſs accurately figured than the fineſt nitre or vitriol I remember myſelf to have obſerved, and

* *G. Agricola de Re Metallica*, lib. 12.

ſome alſo terminate in bodies almoſt like pyramids, conſiſting of divers triangles, that meet in one vertical point, and are no leſs admirably ſhaped, than the fairer ſort of *Corniſh* diamonds, that have been brought me for rarities. Beſides the producing of ſalts of new ſhapes by compounding of ſaline bodies, I have found it to be practicable not only in ſome groſs, or, as they ſpeak, corporal ſalts, ſuch as ſea-ſalt, ſaltpetre, but alſo in ſome natural and ſome chymical ſalts diſſolved together, and which perhaps you will think more conſiderable in ſaline ſpirits made by diſtillation; not that all of them are fit for this purpoſe, but that I have found divers of thoſe that work upon one another with ebullition, to be ſo. For in that conflict the ſaline corpuſcles come to be aſſociated to one another, and thereby, or by their newly acquired figure, whilſt their coalition laſts, to loſe much of their former volatility: ſo that upon evaporation of the ſuperfluous liquor, they will not fly as otherwiſe they might, but coagulate into finely ſhaped cryſtals, as I have tried, among other ſaline liquors, with ſpirit of urine, and ſpirit of nitre, and with oil of vitriol, and ſpirit of fermented urine, with ſpirit of ſheep's blood, and ſpirit of ſalt, and alſo with the ſpirits of ſalt and urine; which laſt experiment I the rather mention, becauſe it ſhews by the difference of the cryſtals, afforded by thoſe two liquors from the cryſtals reſulting from one of them, namely, the ſpirit of urine (or, if you pleaſe, the volatile ſalt wherewith it abounds), coagulated with a fit doſe of oil of vitriol, how much thoſe compound emergent figures depend upon the more ſimple figures of the ſaline corpuſcles, that happen to convene into thoſe new concretes. For the ſpirit of urine ſatiated with ſpirit of ſalt, and both very gently, and not too far evaporated, often afforded me cryſtals that differed exceedingly in ſhape from thoſe which I obtained from the ſame ſpirit of urine, ſatiated either with oil of vitriol, or with ſpirit of nitre. For (to add that upon the by) that ſalt, compounded of the two ſpirits of urine and of common ſalt, is wont to be very prettily figured, conſiſting of one long beam as it were, whence on both ſides iſſue out far ſhorter cryſtals, ſometimes perpendicular to that, and parallel to one another like the teeth in a comb, and ſometimes ſo inclining as to make the whole appear almoſt like a feather; which is the more remarkable, becauſe I have (many years ago) obſerved that common ſal-armoniack that is made of urine and common ſalt, both crude, with a proportion of ſoot, will, if warily diſſolved and coagulated, ſhoot into cryſtals of the like ſhape. How far the unknown figure of a ſalt may poſſibly (for I fear it will not eaſily) be gueſſed at by that of the figure which it makes with ſome other ſalt, whoſe figure is already known, I leave to geometricians to conſider; having, I fear, inſiſted too long on this ſubject already. But yet I muſt add one particular more, which will as well illuſtrate and confirm much of what has been ſaid above touching the organization of vitriol, as ſhew that the ſhape of vitriol depends upon the texture of the bodies whereof it is compoſed.

Fourthly, then, when I conſidered that (as I formerly noted) vitriol being but a magiſtery, made by the concoagulation of the corpuſcles of a diſſolved metal with thoſe of the menſtruum, the magiſteries of other metals might, without inconvenience, be added, as other vitriolate concretes, to the green, the blue, and white vitriol, that are without ſcruple referred to the ſame ſpecies: and when I conſidered that oil of vitriol was not a fit menſtruum to diſſolve divers of the metals, nor even all thoſe that it will corrode; and that the like unfitneſs alſo is to be found in common ſpirit of ſalt; I pitched upon aqua fortis, or ſpirit of nitre, as that menſtruum which was likelieſt to afford variety of vitriols. And accordingly I found, that beſides the lovely vitriol of copper formerly mentioned, that liquor would with quickſilver afford one ſort of cryſtals, with ſilver another, and with lead a third; all which cryſtals of vitriol, as they differed from each other in other qualities (upon which ſcore you will find this experiment elſe-

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where mentioned), ſo they did very manifeſtly and conſiderably differ in ſhape; the cryſtals of ſilver ſhooting in exceeding thin plates, and thoſe of lead and quickſilver obtaining figures, though differing enough from each other, yet of a far greater depth and thickneſs, and leſs remote from the figure of common vitriol or ſea-ſalt: and yet all theſe vitriols, eſpecial that of crude lead, when it was happily made, had ſhapes curious and elaborate, as well as thoſe we admire in common vitriol or ſea-ſalt.

If then theſe curious ſhapes, which are believed to be of the admirableſt effects, and of the ſtrongeſt proofs of ſubſtantial forms, may be the reſults of texture; and if art can produce vitriol itſelf as well as nature, why may we not think that in ordinary phænomena, that have much leſs of wonder, recourſe is wont to be had to ſubſtantial forms without any neceſſity? (matter, and a convention of accidents being able to ſerve the turn without them;) and why ſhould we wilfully exclude thoſe productions of the fire, wherein the chymiſt is but a ſervant of nature, from the number of natural bodies? And indeed ſince there is no certain diagnoſtick agreed on whereby to diſcriminate natural and factitious bodies, and to conſtitute the ſpecies of both; I ſee not why we may not draw arguments from the qualities and operations of ſeveral of thoſe that are called factitious, to ſhew how much may be aſcribed to, and performed by the mechanical characterization, or ſtamp of matter; of which we have a noble inſtance in gun-powder, wherein, by a bare comminution and blending the ingredients, nitre, charcoal, and brimſtone, which have only a new, and that an exceeding ſlight contexture, each retaining its own nature in the mixture, ſo that there is no colour afforded to the pretence of a ſubſtantial form; there is produced a new body, whoſe operations are more powerful and prodigious than thoſe of almoſt any body of nature's own compounding. And though glaſs be but an artificial concrete, yet, beſides that it is a very noble and uſeful one, nature herſelf has produced very few, if enough to make up a number more laſting and more unalterable. And indeed divers of thoſe factitious bodies that chymiſtry is able to afford us, are endowed with more various and more noble qualities than many of thoſe that are unqueſtionably natural. And if we admit theſe productions into the number of natural bodies, they will afford us a multitude of inſtances to ſhew that bodies may acquire many and noble qualities, barely by having mechanical affections introduced by outward agents into the matter, or deſtroyed there. As though glaſs be ſuch a noble body as we have lately taken notice of, yet ſince its fuſibility, tranſparency, and brittleneſs, that are its only conſtituent attributes, we can in leſs than an hour (or perhaps half that time) turn an opacous body into tranſparent glaſs without the addition of any other viſible body, by a change of texture made in the ſame matter, and by another change of texture, made without addition, as formerly, we can in a trice reduce glaſs into, or obtain from it, a body not glaſſy, but opacous, and otherwiſe of a very different nature, as it had been before. And here let me add what may not a little conduce to our preſent deſign, that even thoſe that embrace *Ariſtotle*'s principles, do unawares confeſs that a ſlight change of texture, without the introduction of a ſubſtantial form, may not only make a ſpecifical difference betwixt bodies, but ſo vaſt a one, that they ſhall have differing geniuſes, and may (as the chymiſts ſpeak) belong to different kingdoms. For coral, to paſs by all other plants of that kind that may be mentioned to the ſame purpoſe, whilſt it grows in the bottom of the ſea, is a real plant, and ſeveral times (which ſuffices for my preſent ſcope) hath been there found by an acquaintance of mine, as well as by other enquirers, ſoft and tender like another plant: nay, I elſewhere * bring very good and recent authority to prove that it is oftentimes found very ſucculent, and does propagate its ſpecies as well as other ſhrubs; and yet coral being gathered and

* In the Eſſays about things ſuppoſed to be ſpontaneouſly generated.

removed into the air, by the recefs of its foul, no new lapidifick form being fo much as pretended to, turns into a concretion, that is by many eminent writers, and others reckoned among lapideous ones: as indeed coral does not burn like wood, nor obey diftillation like it; and not only its calx is very differing from the afhes of vegetables, and is totally foluble in divers acid liquors, and even fpirit of vinegar, but the uncalcined coral itfelf will be eafily corroded by good vinegar, after the fame manner as I have feen *lapis ftellaris*, and other unqueftionably mineral ftones diffolved, fome by that liquor, and fome by the fpirit of it. A much ftranger thing may be feen in the Eaft-India ifland of *Sombrero*, not very far from *Sumatra*, if we may believe our countryman Sir *James Lancafter*, who relates it as an eye-witnefs, for which reafon, and for the ftrangenefs of the thing, I fhall add the ftory in his own words. *Here* († fays he, fpeaking of the coaft of *Sombrero) we found upon the fand by the fea fide a fmall twig growing up to a young tree; and offering to pluck up the fame, it fhrunk down into the ground, and finketh unlefs you hold very hard. And being plucked up, a great worm is the root of it: and look how the tree groweth in greatnefs, the worm diminifheth. Now as foon as the worm is wholly turned into the tree, it rooteth in the ground, and fo groweth to be great. This transformation was one of the greateft wonders I faw in all my travels. This tree being plucked up a little, the leaves ftripped off, and the peel, by that time it was dry, turned into a hard ftone, much like to white coral. So that* (concludes he) *this worm was twice transformed into different natures: of thefe we gathered and brought home many.* The induftrious *Pifo*, in his excellent hiftory of *Brafil* vouches a multitude of witneffes (not having opportunity to be one himfelf) for the ordinary transformation of a fort of animals (not much unlike grafhoppers) into vegetables, at a certain feafon of the * year.

But fince I fet down this relation of Sir *John Lancafter*, I have met with another, whofe ftrangenefs may much countenance it, in a fmall tract newly publifhed by a Jefuit, *F. Michael Boym*, whom a good critick much commended to me. For this author doth, as an eye-witnefs affirm, that which is little lefs to my prefent purpofe: ‡ *Je vis, &c.* i. e. *I faw in a fmall frefh-water and fhallow lake of the ifland* Hainan (which belongs to *China*) *crabs or craw-fifhes, which, as foon as they were drawn out of the water, did in a moment lofe both life and motion, and became petrified, though nothing appeared to be changed either in the external or internal figure of their bodies.* What he further adds of thefe fifhes is but of their virtues in phyfick, which not concerning our fubject, I fhall, *Pyrophilus*, willingly pretermit it; and even as to our countryman's relation, hoping by means of an ingenious correfpondent in the *Eaft Indies*, to receive a further information about the ftrange plant he mentions, I fhall at prefent urge only what has been taken notice of concerning coral, to countenance the obfervation for whofe fake thefe narratives have been alledged. And fo likewife as to what I was faying of glafs and gunpowder, our receiving of thofe, and the generality of factitious bodies into the catalogue of natural bodies, is not (which I formerly alfo intimated) neceffary to my prefent argument: whereto it is fufficient that vitriol is granted on all hands to be a natural body, though it be alfo producible by art. And alfo to the argument it affords us we might add that memorable experiment delivered by *Helmont*, of turning oil of vitriol into allom, by the odour (as he calls it) of mercury, if however it be not defpicable, we had found it fit to be relied on. But referving an account of that for another place, we fhall fubftitute the inftance prefented us by our author about the production of falt-petre: for

† *Purchas Pilgr.* Part the firft, p. 152.

* The paffage, which is long, I do not here tranfcribe, having had occafion to do it elfewhere. It is extant, *lib.* 5, *cap.* 21. and at the clofe of his narrative he fubjoins, *Non eft, quod quifquam de veritate dubitet, cum infinitos teftes habeat Brafilia, &c.*

‡ *Flora Sinenfis, ou Traite des Fleurs, &c.* under the Title *Lozmeoques.*

if, having diſſolved pot-aſhes in fair water, you coagulate the filtrated ſolution into a white ſalt, and on that pour ſpirit of nitre till they will not hiſs any longer; there will ſhoot when the ſuperfluous water is evaporated, cryſtals, that proclaim their nitrous nature by their priſmatical (or at leaſt priſm-like) ſhape, their eaſy fuſion, their accenſion, and deflagration, and other qualities partly mentioned by our author, and partly diſcoverable by a little curioſity in making trials.

II. *Experimental Attempts about the Redintegration of Bodies.*

THE former of thoſe two arguments, *Pyrophilus*, by which I propoſed to confirm the origin of forms, was, as you may remember, grounded upon the manner by which ſuch a convention of accidents, as deſerves to paſs for a form, may be produced: and that having been hitherto proſecuted, it now remains that we proceed to the ſecond argument, drawn not (as the former) from the firſt production, but from the reproduction of a phyſical body. And though both theſe arguments are valid, yet if this latter could, in ſpite of the difficulties intervening in making of the experiments that belongs to it, be as clearly made out as the former, you would, I ſuppoſe, like it much the better of the two. For if we could reproduce a body which has been deprived of its ſubſtantial form, you would, I preſume, think it highly probable, if not more than probable, that (to borrow our author's own expreſſion) that which is commonly called the form of a concrete, which gives it its being and denomination, and from whence all its qualities are in the vulgar philoſophy, by I know not what inexplicable ways, ſuppoſed to flow, may be in ſome bodies but a characterization or modification of the matter they conſiſt of; whoſe parts by being ſo and ſo diſpoſed in relation to each other, conſtitute ſuch a determinate kind of body, endowed with ſuch and ſuch properties: whereas if the ſame parts were otherwiſe diſpoſed, they would conſtitute other bodies of very differing natures from that of the concrete, whoſe parts they formerly were, and which may again reſult or be produced after its diſſipation, and ſeeming deſtruction, by the re-union of the ſame component particles, aſſociated according to their former diſpoſition.

BUT though it were not poſſible to make an adequate redintegration of a chymically analyzed body, becauſe ſome of the diſſipated parts will either eſcape through the junctures of the veſſels (though diligently cloſed), or if they be very ſubtile, will fly away upon the disjoining of the veſſels, or will irrecoverably ſtick to the inſide of them: yet I ſee not why ſuch a reproduction, as is very poſſible to be effected, may not ſuffice to manifeſt what we intend to make out by it. For even in ſuch experiments it appears, that when the form of a natural body is aboliſhed, and its parts violently ſcattered by the bare re-union of ſome parts after the former manner, the very ſame matter the deſtroyed was before made of, may, without addition of other bodies, be brought again to conſtitute a body of the like nature with the former, thongh not of equal bulk. And indeed the experiment recorded by our author about the reproduction of ſalt-petre, as it is the beſt and ſucceſsfulleſt I have ever been able to make upon bodies, that require a ſtrong heat to diſſipate them; ſo I hope it will ſuffice to give you thoſe thoughts about this matter that the author deſigned in alledging it; and therefore, though having premiſed thus much, I ſhall proceed to acquaint you with the ſucceſs of ſome attempts

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he intimates (in that eſſay) his intention of making for the redintegration of ſome bodies; yet doing it out of ſome hiſtorical notes, I find among my looſe papers, that which I at preſent pretend to, is but partly to ſhew you the difficulty of ſuch attempts, which ſince our author's eſſay was communicated, have been repreſented (I fear by conjecture only) as very eaſy to be accurately enough done; and partly becauſe our author does not without reaſon intimate the uſefulneſs of redintegrations, in caſe they can be effected; and does not cauſeleſly intimate that ſuch attempts, though they ſhould not perfectly ſucceed, may increaſe the number of noble and active bodies, and conſequently the inventory of mankind's good.

Upon ſuch conſiderations we attempted the diſſipation and re-union of the parts of common amber; and though chymiſts for fear of breaking their veſſels, are wont when they commit it to diſtillation, to add to it a *caput mortuum* (as they ſpeak) of ſand, brick, *&c.* (in whoſe room we ſometimes chuſe to ſubſtitute beaten glaſs;) which hinders them to judge of and employ the remanence of amber after the diſtillation is finiſhed; yet we ſuppoſed and found that if the retort were not too much filled, and if the fire were ſlowly and warily enough adminiſtered, the addition of any other body would be needleſs. Wherefore having put into a glaſs retort four or five ounces of amber, and adminiſtred a gentle and gradual heat, we obſerved the amber to melt and bubble (which we therefore mention becauſe ingenious men have lately queſtioned whether it can be melted), and having ended the operation and ſevered the veſſels, we found that there was come over in the form partly of oil, partly of ſpirit and phlegm, and partly of volatile ſalt, near half the weight of the concrete: and having broken the retort, we found in the bottom of it a cake of coal-black matter, than whoſe upper ſurface I ſcarce remember to have ſeen in my whole life any thing more exquiſitely poliſhed; inſomuch that, notwithſtanding the colour, as long as I kept it, it was fit to ſerve for a looking-glaſs: and this ſmooth maſs being broken (for it was exceeding brittle), the large fragments of it appeared adorned with an excellent luſtre. All thoſe parts of the amber being put together into a glaſs body, with a blind head luted to it, were placed in ſand to be incorporated by a gentle heat: but whilſt I ſtept aſide to receive a viſit, the fire having been increaſed without my knowledge, the fumes aſcended ſo copiouſly, that they lifted up the veſſel out of the ſand, whereupon falling againſt the ſide of the furnace, it broke at the top, but being ſeaſonably called, we ſaved all but the fumes; and the remaining matter looks not unlike tar, and with the leaſt heat may be poured out like a liquor, ſticking, even when it is cold to the fingers. Yet this opening body doth not eaſily communicate ſo much as a tincture to ſpirit of wine, which therefore ſeems ſomewhat ſtrange, becauſe another time preſuming that this would be a good way to obtain a ſolution of ſome of the reſinous parts of amber, we did, by pouring ſpirit of wine, that (though rectified) was not of the very beſt, upon the re-united parts of amber, lightly digeſted into a maſs, eaſily obtain a clear yellow ſolution, very differing from the tincture of amber, and abounding (as I found by trial) in the diſſolved ſubſtance of the amber: but in oil of turpentine we have in a ſhort time diſſolved it into a blood-red balſam, which may be of good uſe (at leaſt) to chirurgeons. And having again made the former experiment with more warineſs than before, we had the like ſucceſs in our diſtillation, but the re-united parts of the amber being ſet to digeſt in a large bolt-head, the liquor, that was drawn off, did in a few hours, from its own *caput mortuum*, extract a blood-red tincture, or elſe made a ſolution of ſome part of it, whereby it obtained a very deep red; but having been by intervening accidents, hindered from finiſhing the experiment, we miſſed the ſatisfaction of knowing to what it may be brought at laſt.

And

And as for what our author tells us of this design to attempt the redintegration of vitriol, turpentine, and some other concretes, wherein it seemed not unpracticable, he found in it more difficulty than every one would expect. For the bodies on which such experiments are likeliest to succeed, seem to be allum, sea-salt, and vitriol. And as for allum, he found it a troublesome work to take (as a Spagyrist would speak) the principles of it asunder, in regard that it is inconvenient to distil it with a *caput mortuum*, (as chymists call any fixed addittament) lest that should hinder the desired redintegration of the dissipated parts. And when he distilled it by itself without any such additament, he found that with a moderate heat the allum would scarce part with any thing but its phlegm; and if he urged it with a strong fire, he found it would so swell, as to endanger the breaking of the retort, or threaten the boiling over into the receiver. (Yet having once been able very warily to abstract as much phlegm and spirit as I conveniently could, from a parcel of roch allum, and having poured it back upon that pulverized *caput mortuum*, and left the vessel long in a quiet place, I found that the corpuscles of the liquor having had time after a multitude of occursions to accommodate and re-unite themselves to the more fixed parts of the concrete, did, by that association (or dissolution) recompose, at the top of the powder, many crystalline grains of finely figured salt, which increasing with time, made me hope that at the length the whole, or the greatest part would be reduced into allum, which yet a mischance that robbed me of the glass, hindered me to see. So likewise of sea-salt, if it be distilled, as it is usual, with thrice its weight of burned clay, or beaten brick, it will prove inconvenient in reference to its redintegration: and if it be distilled alone, it is apt to be fluxed by the heat of the fire, and whilst it remains in fusion will scarce yield any spirit at all. And as for vitriol, though the redintegration of it might seem to be less hopeful than that of the other salts, in regard that it consists not only of a saline, but of a metalline body, whence it may be supposed to be of a more intricate and elaborate texture; yet because there needs no *caput mortuum* in the distillation of it, we did, to pursue our author's intimated designs, make two or three attempts upon it, and seemed to miss of our aim, rather upon the account of accidental hindrances, than of any insuperable difficulty in the thing itself. For once we with a strong fire, drew off from a parcel of common blue vitriol, the phlegm and spirit, and some quantity of the heavy oil, (as chymists abusively call it): these liquors, as they came over without separation, we divided into several parts, and the remaining very red *caput mortuum* into as many. One of these parcels of liquor we poured over night upon its correspondent portion of the newly mentioned red powder: but having left it in a window, and the night proving very bitter, in the morning I found the glass cracked in many places by the violence of the frost, and the liquor seemed to have been soaked up by the powder, and to have very much swelled it. This mixture then I took out, and placing it in an open-mouthed glass in a window, I found after a while, divers grains of pure vitriol upon the other matter, and some little swellings, not unlike those we shall presently have occasion to speak of. I took likewise a much larger parcel of the fore-mentioned liquor, and its correspondent proportion of *caput mortuum*; and having leisurely mixed them in a large glass bason, I obtained divers phænomena, that belong not to this place, but may be met with where they will more properly fall in. In this bason (which I laid in the window, and kept from agitation) I perceived, after a while, the liquor to acquire a bluish tincture, and after ten or twelve weeks I found the mixture dry (for it seems it was too much exposed to the air); but the surface of it adorned in divers places, with grains of vitriol very curiously figured.

And

And besides these, there were store of protuberances, which consisted of abundance of small vitriolate particles, which seemed in the way to a coalition; for having let the bason alone for four or five months longer, the matter appeared crusted over, partly with very elevated saline protuberances, partly with lesser parcels, and partly also with considerable broad cakes of vitriol, some of above half an inch in breadth, and proportionably long: and indeed the whole surface was so oddly diversified, that I cannot count the trouble these trials have put me to, mispent. Another time, in a more slender and narrow mouthed glass, I poured back upon the *caput mortuum* of vitriol the liquors I had by violence of the fire forced from it; so that the liquid part did swim a pretty height above the red calx, and remained a while limpid and colourless: but the vessel having stood for some time unstopped in a window, the liquor after a while acquired by degrees a very deep vitriolate colour, and not long after there appeared at the bottom, and on the top of the calx, many fair and exquisitely figured grains of vitriol, which covered the surface of the calx, and the longer the vessel continued in the window, the deeper did this change, made upon the upper part of the powder, seem to penetrate: so that I began to hope that in process of time, almost (if not more than almost) the whole mixture would be reduced to perfect vitriol. But an accident robbed me of my glass before I could see the utmost of the event.

And on this occasion I must not pretermit an odd experiment I lately made, though I dare not undertake to make it again. I elsewhere relate how I digested for divers weeks a quantity of powdered antimony, with a greater weight by half of oil of vitriol; and how having at length committed this mixture to distillation, and thereby obtained, besides a little liquor, a pretty quantity of combustible antimonial, or antimonio-vitriolate sulphur; there remained in the bottom of the retort a somewhat light and very friable *caput mortuum*, all the upper part of which was at least as white as common wood-ashes, and the rest looked like a cinder. And now I must tell you what became of this *caput mortuum*, whereof I there make no further mention. We could not well foresee what could be made of it, but very probable it was that it would afford us some new discovery by being exposed to the fire, in regard of the copious sulphur whereof it seemed to have been deprived; provided it were urged in close vessels, where nothing could be lost. Whereupon committing it to a naked fire in a small glass retort, well coated and accommodated with a receiver, we kept it there many hours, and at length severing the vessels, we found (which need not be wondered at) no antimonial quicksilver, and much less of sulphur sublimed than we expected: wherefore greedily hastening to the *caput mortuum*, we found it fixed into a mass, covered with a thin cake of glass, whose fragments being held against the light, were not all coloured, as antimonial glass is wont to be, but were as colourless as common white glass. The lump above mentioned being broken, was found, somewhat to our wonder, to be perfect black antimony, adorned with long shining streaks, as common antimony is wont to be: only this antimony seemed to have been a little refined by the sequestration of its unnecessary sulphur; which ingredient seems by this experiment, as well as by some other observations of ours, to be more copious in some particular parcels of that mineral, than is absolutely requisite to the constitution of antimony. Though in our case it may be suspected that the reduction of part of the mass to a colourless glass was an effect of the absence of so much of the sulphur, and might in part make the remaining mass some amends for it. What we further did with this new or reproduced concrete, is not proper to be here told you: only for your satisfaction we have kept a lump of it, that you may with us take notice of what some Philosophers would call the mindfulness of nature; which, when a body was deprived of a not inconsiderable portion of its chief ingredient, and had all its other

parts

parts diffipated and fhuffled, and difcoloured fo as not to be knowable, was able to rally thofe fcattered and difguifed parts, and marfhal or difpofe them into a body of the former confiftence, colour, *&c.* though (which is not here to be overlooked) the contexture of antimony, by reafon of the copious fhining *ftiriæ* that enoble the darker body, be much more elaborate, and therefore more uneafy to be reftored than that of many other concretes.

But among all my trials about the redintegration of bodies, that which feemed to fucceed beft was made upon turpentine: for having taken fome ounces of this, very pure and good, and put it into a glafs retort, I diftilled fo long with a very gentle fire, till I had feparated it into a good quantity of very clear liquor, and a *caput mortuum* very dry and brittle; then breaking the retort, I powdered the *caput mortuum*, which when it was taken out was exceeding fleek, and tranfparent enough, and very red, but being powdered, appeared of a pure yellow colour. This powder I carefully mixed with the liquor that had been diftilled from it, which immediately diffolved part of it into a deep red balfam; but by further digeftion, in a large glafs exquifitely ftopt, that colour began to grow fainter, though the remaining part of the powder (except a very little, proportionable to fo much of the liquor, as may be fuppofed to have been wafted by evaporation and transfufion out of one veffel into another) be perfectly diffolved, and fo well re-united to the more fugitive parts of the concrete, that there is fcarce any that by the fmell, or tafte, or confiftence, would take it for other than good and laudable turpentine.

CONSIDERATIONS AND EXPERIMENTS TOUCHING THE ORIGIN OF QUALITIES and FORMS.

THE HISTORICAL PART.

SECTION I.

Containing the Obſervations.

IN the foregoing notes I have endeavoured, with as much clearneſs as the difficulty of the ſubject and the brevity I was confined to permitted, to give a ſcheme or ſummary of the principles of the corpuſcularian philoſophy, as I apprehended them, by way of a ſhort introduction to it, at leaſt as far as I judged neceſſary for the better underſtanding of what is contained in our notes and experiments, concerning the productions and changes of particular qualities. But though I hope I have not ſo affected brevity as to fall into obſcurity, yet, ſince theſe principles are built upon the phænomena of nature, and deviſed in order to the explication of them, I know not what I can do more proper to recommend them than to ſubjoin ſome ſuch natural phænomena as either induce me to take up ſuch notions, or which I was directed to find out by the notions I had embraced. And ſince I appeal to the teſtimony of nature to verify the doctrine I have been propoſing, about the origin and production of qualities (for that of forms will require a diſtinct diſcourſe); I think it very proper to ſet down ſome obſervations of what nature does, without being over-ruled by the power and ſkill of man, as well as ſome experiments wherein nature is guided, and as it were, maſtered by art, that ſo ſhe may be made to atteſt the truth of our doctrine, as well when ſhe diſcloſes herſelf freely, and, if I may ſo ſpeak, of her own accord, as when ſhe is, as it were, cited to make her depoſitions by the induſtry of man. The obſervations will be but the

the more ſuitable to our deſign for being common and familiar as to the phænomena, though perhaps new enough as to the application to our purpoſe. And as for the experiments, becauſe thoſe that belong more immediately to this or that particular quality, may be met with in the notes that treat of it, I thought it not amiſs that the experiments ſhould be both few in number, and yet ſo pregnant that every one of them ſhould afford ſuch differing phænomena as may make it applicable to more than one quality:

I.

The obſervation I ſhall begin with ſhall be fetched from what happens in the hatching of an egg. For as familiar and obvious a thing as it is, (eſpecially after what the learned *Fabricius ab Aquapendente*, and a recenter anatomiſt, have delivered about them) that there is a great change made in the ſubſtance of the egg when it is by incubation turned into a chick; yet, as far as I know, this change hath not been taken notice of for the ſame purpoſe to which I am about to apply it.

I consider then that in a prolific egg (for inſtance that of a hen) as well the liquor of the yolk as that of the white is a ſubſtance, as to ſenſe, ſimilar. For upon the ſame account that anatomiſts and phyſicians call ſeveral parts of the human body, as bones, membranes, *&c.* ſimilar, that is ſuch, as that every ſenſible part of it hath the ſame nature or denomination with the whole, as every ſplinter of bone is bone, as every ſhred of ſkin is ſkin.

And though I find by diſtilling the yolks and whites they ſeem to be diſſimilar bodies, in regard that the white of an egg (for example) will afford ſubſtances of a very differing nature, as phlegm, ſalt, oil, and earth; yet (not now to examine whether, or how far theſe may be eſteemed productions of the fire that are rather obtained from the white of the egg, than were præ-exiſtent in it; not to mention this, I ſay) it doth not appear by diſtillation that the white of an egg is other than a ſimilar body in the ſenſe above delivered. For it would be hard to prove that one part of the white of an egg will not be made to yield the ſame differing ſubſtances by diſtillation that any other part does; and bones themſelves, and other hard parts of a human body that are confeſſedly ſimilar may, by diſtillation, be made to afford ſalt, and phlegm, and ſpirit, and oil, and earth, as well as the white of an egg.

This being thus ſettled in the firſt place, we may in the next conſider that by beating the white of an egg well with a whiſk, you may reduce it from a ſomewhat tenacious, into a fluid body, though this production of a liquor be, as we elſewhere noted, effected by a divulſion, agitation, *&c.* of the parts; that is, in a word, by a mechanical change of the texture of the body.

In the third place I conſider that, according to the exacteſt obſervations of modern anatomiſts, which our own obſervations do not contradict, the rudiments of the chick, lodged in the cicatricula or white ſpeck upon the coat of the yolk, is nouriſhed till it have obtained to be a great chick, only by the white of the egg; the yolk being by the providence of nature reſerved as a more ſtrong and ſolid aliment till the chick have abſumed the white, and be thereby grown great and ſtrong enough to digeſt the yolk; and in effect you may ſee the chick furniſhed not only with all the neceſſary, but divers other parts, as head, wings, legs, and beak, and claws, whilſt the yolk ſeems yet, as it were, untouched. But whether this obſervation about the entireneſs of the yolk be preſently true, is not much material to our preſent purpoſe, nor would I be thought to build much upon it; ſince the yolk itſelf, eſpecially at that time, is wont to be fluid

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enough, and to be a liquor perhaps no leſs ſo than the white was, and that is enough for my preſent purpoſe.

For in the laſt place I conſider that the nutritive liquor of an egg, which is in itſelf a body ſo very ſoft, that by a little agitation it may be made fluid, and is readily enough diſſolvable in common cold water; this very ſubſtance, I ſay, being brooded on by the hen, will within two or three weeks be tranſmuted into a chick furniſhed with organical parts, as eyes, ears, wings, legs, *&c.* of a very differing fabrick, and with a good number of ſimilar ones, as bones, cartilages, ligaments, tendons, membranes, *&c.* which differ very much in texture from one another; beſides the liquors, as blood, chyle, gall, *&c.* contained in the ſolid parts: ſo that here we have out of the white of an egg, which is a ſubſtance ſimilar, inſipid, ſoft (not to call it fluid) diaphanous, colourleſs, and readily diſſoluble in cold water; out of this ſubſtance, I ſay, we have, by the new and various contrivement of the ſmall parts it conſiſted of, an animal, ſome of whoſe parts are not tranſparent, but opacous; ſome of them red, as the blood; ſome yellow or greeniſh, as the gall; ſome white, as the brain; ſome fluid, as blood, and other juices; ſome conſiſtent, as the bones, fleſh, and other ſtable parts of the body; ſome ſolid and frangible, as the bones; others tough and flexible, as the ligaments; others ſoft and looſely coherent, as the marrow; ſome without ſprings, as many of the parts; ſome with ſprings, as the feathers; ſome apt to mingle readily with cold water, as the blood, the gall; ſome not to be *ſo* diſſolved in it, as the bones, the claws, and the feathers; ſome well taſted, as the fleſh and blood; ſome very ill taſted; as the gall (for that I have purpoſely and particularly obſerved); in a word, we have here produced out of ſuch an uniform matter as the white of an egg,

First, new kind of qualities, as (beſides opacity) colours (whereof a ſingle feather will ſometimes afford us variety) odours, taſtes, and heat in the heart and blood of the chick, hardneſs, ſmoothneſs, roughneſs, *&c.*

Secondly, divers other qualities that are wont to be diſtinguiſhed from ſenſible ones, as fluidity (in the blood and aqueous humour of the eye) conſiſtency in the griſtles, fleſh, *&c.* hardneſs, flexibility, ſpringineſs, toughneſs, unfitneſs to be diſſolved in cold water, and ſeveral others. To which may probably be added,

Thirdly, ſome occult properties, as phyſicians obſerve, that ſome birds, as young ſwallows, young magpyes, afford ſpecifick, or at leaſt noble medicines in the falling-ſickneſs, hyſterical fits, and divers other diſtempers.

Fourthly, I very well foreſee it may be objected that the chick with all its parts is not a mechanically contrived engine, but faſhioned out of matter by the ſoul of the bird lodged chiefly in the cicatricula, which by its plaſtick power faſhions the obſequious matter and becomes the architect of its own manſion. But not here to examine whether any animal except man be other than a curious engine, I anſwer, that this objection invalidates not what I intend to prove from the alledged example. For let the plaſtick principle be what it will, yet ſtill, being a phyſical agent, it muſt act after a phyſical manner; and having no other matter to work upon but the white of the egg, it can work upon that matter but as phyſical agents, and conſequently can but divide the matter into minute parts of ſeveral ſizes and ſhapes, and by local motion variouſly context them according to the exigency of the animal to be produced, though from ſo many various textures of the produced parts there muſt naturally emerge ſuch differences of colours, taſtes, and conſiſtences, and other qualities as we have been taking notice of. That which we are here to conſider is not what is the agent or efficient in theſe productions, but what is done to the matter to effect them. And though ſome birds, by an inbred ſkill, do very artificially build their curious neſts, yet cannot nature that teaches

teaches them, enable them to do any more than ſelect the materials of their neſts, and by local motion divide, tranſport, and connect them after a certain manner. And when man himſelf, who is undoubtedly an intelligent agent, is to frame a building or an engine, he may indeed, by the help of reaſon and art, contrive his materials curiouſly and ſkilfully; but ſtill all he can do is but to move, divide, tranſpoſe, and context the ſeveral parts into which he is able to reduce the matter aſſigned him.

Nor need we imagine that the ſoul of that hen which having firſt produced the egg, does after a while ſit on it, hath any particular efficiency in hatching of a chick, for the egg will be well hatched by another hen though that which laid it be dead; and which is more, we are aſſured by the teſtimony of very good authors as well as of recent travellers, that in ſome places, eſpecially in *Ægypt*, there needs no bird at all to the production of a chick out of an egg, ſince they hatch multitudes of eggs by the regulated heat of ovens or dunghills: and indeed, that there is a motion or agitation of the parts of the egg by the external heat whereby it is hatched, is evident of itſelf, and not (as far as I know) denied by any; and that alſo the white ſubſtance is abſumed, and contexed or contrived into the body of the chick and its ſeveral parts, is manifeſt to ſenſe; eſpecially if one hath the curioſity to obſerve the progreſs of the chick's formation and increment. But as it is evident that theſe two things, the ſubſtance of the white, and the local motion wherein the external heat neceſſary to incubation puts its parts, do eminently concur to the production of the chick, ſo that the former power (whatever that be) doth any more than guide theſe motions, and thereby aſſociate the fitted particles of matter after the manner requiſite to conſtitute a chick, is that which I think will not eaſily be evinced. And I might, to what I ſaid of the egg, add ſeveral things touching the generation of viviparous animals, which the learned *Fabricius ab Aquapendente*, as well as ſome of the ancient philoſophers, would have to be generated from an imperfect kind of eggs: but I take the eggs of birds to be much fitter to inſtance in, becauſe they are things that we have more at command, and wherewith we can conveniently make more trials and obſervations; and eſpecially becauſe in perfect eggs the matter to be tranſmuted is more cloſely locked up, and being kept from any viſible ſupply of matter, confined to be wrought upon by the external heat and by its own vital principle within.

II.

Water being generally eſteemed an elementary body, and being at leaſt far more homogeneous than bodies here below are wont to be, it may make very much for our preſent purpoſe to ſhew that water itſelf, that is fluid, taſteleſs, inodorous, diaphanous, colourleſs, volatile, &c. may, by a differing texture of its parts, be brought to conſtitute bodies of attributes very diſtant from theſe. This I thought might be done by nouriſhing vegetables with ſimple water. For in caſe I could do ſo, all or the greateſt part of that which would accrue to the vegetable thus nouriſhed, would appear to have been materially but water with what exotick quality ſoever it may afterwards, when tranſmuted, be endowed.

The ingenious *Helmont*, indeed, mentions an experiment ſomewhat of this nature, though not to the ſame purpoſe, which he made by planting a branch of willow into a pot full of earth, and obſerving the increaſe of weight he obtained after divers years, though he fed the plant but with rain-water. And ſome learned modern naturaliſts have conjectured at the eaſy tranſmutableneſs of water by what happens in gardens and orchards, where the ſame ſhowers or rain, after a long drought, makes a great number of differing plants to flouriſh. But though theſe things be worthy of their authors, yet

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I thought they would not be ſo fit for my purpoſe, becauſe it may be ſpecioufly enough objected that the rain-water does not make theſe plants thrive and flouriſh, by immediately affording them the aliments they aſſimilate into their own ſubſtance, but by proving a vehicle that diſſolves the ſaline and other alimental ſubſtances of the earth, and dilutes both them and the nutritive juice which in a part of the plant itſelf it may find too much thickened by the drought or heat of the ambient air; and by this means it contributes to the nouriſhment of the plant, though itſelf afterwards exhaled into vapours. And indeed experience ſhews us, that ſeveral plants which thrive not well without rain-water, are not yet nouriſhed by it alone; ſince when corn in the field and fruit-trees in orchards have conſumed the ſaline and ſulphureous juices of the earth, they will not proſper there, how much rain ſoever falls upon the land, till the ground, by dung or otherwiſe, be ſupplied again with ſuch aſſimilable juices. Wherefore I rather choſe to attempt the making of plants grow in phials filled with water, not only to prevent the fore-mentioned objection, and alſo to make the experiment leſs tedious, but that I might have the pleaſure of ſeeing the progreſs of nature in the tranſmutation of water; and my obſervations of this kind, as novelties unmentioned by any other writer, I ſhewed divers ingenious friends, who having better opportunities than I of ſtaying in one place, have attempted the like and made ſucceſsful trials, which I ſuppoſe will not be concealed from the publick. Of my obſervations about things of this kind I can at preſent find but few among my *Adverſaria*, but in them I find enough for my preſent turn. For they and my memory inform me that vinca pervinca, raphanus aquaticus, ſpearmint, and even ranunculus itſelf, did grow and proſper very well in phials filled with fair water, by whoſe necks the leaves were ſupported, and the plant kept from ſinking; ſome of theſe were only cuttings without roots, divers of them were left in the water all the autumn and great part of the winter, and at the end of *January* were taken out verdant and with fair roots, which they had ſhot in the water. And beſides, I find that particularly a branch or ſprig of raphanus aquaticus was kept full nine months, and during that time withered not the whole winter, and was taken out of the water with many fibrous roots, and ſome green buds, and an increaſe of weight; and that a ſtump of ranunculus did ſo proſper in the water, that in a month's time it had attained to a pretty deal more than double the weight it had when it was put in. And the next note which I find concerning theſe plants informs me, that the above-mentioned crow's-foot, being taken out again at ſix months after it was put in, weighed a drachm and a half wanting a grain and a half, that is, ſomewhat above thrice as much as it did at firſt. This laſt circumſtance (of the increaſe of weight) I therefore thought fit particularly to make trial of, and ſet down upon this account among others, that having doubted the roots and leaves that ſeemed produced out of the water, might really be ſo by an oblongation and an expanſion of the plants (as I have purpoſely tried, that an onion weighed and laid up in the ſpring, though after ſome weeks keeping in the air it ſhot blades, whereof one was five inches long, inſtead of incorporating the air or terreſtrial effluviums with itſelf, and conſequently thereby growing heavier, had loſt nine grains of its former weight); it might by this circumſtance appear that there may be a real aſſimilation and tranſmutation of water into the ſubſtance of the vegetable, as I elſewhere alſo ſhew by other proofs. For this being made out, from thence I infer that the ſame corpuſcles which, convening together after one manner, compoſe that fluid, inodorous, colourleſs, and inſipid body of water, being contexted after other manners, may conſtitute differing concretes which may have firmneſs, opacity, odours, ſmells, taſtes, colours, and ſeveral other manifeſt qualities, and that too very different from one another. And beſides all this, theſe diſtinct portions of tranſmuted water may have many other qualities, without excepting thoſe that are wont to be called ſpecifick or occult; witneſs the ſeveral medicinal virtues attributed by authors to ſpearmint, and to periwinkle, to majorane, and to raphanus aquaticus. And as for ranunculus, that plant being reckoned among poiſonous ones, and among thoſe that raiſe bliſters, it will be eaſily granted that it hath, as other poiſons, an occult, deleterial faculty: and indeed it ſomewhat deſerves our wonder that ſo inſipid and innocent a thing as fair water ſhould be capable of being turned into a ſubſtance of ſuch a piercing and cauſtic nature, as by contact to raiſe bliſters on a human body. And yet perhaps that is no leſs ſtrange which we elſewhere relate, that a plant, conſiſting chiefly of tranſmuted water, did by diſtillation afford us a true oil that would not mingle with water, and conſequently was eaſily convertible into fire. But whether or no this experiment, or any ſuch like, prove that almoſt all things may be made of all things, not immediately, but by intervention of ſucceſſive changes and diſpoſitions, is a queſtion to which we elſewhere ſay ſomething, but are not willing in this place to ſay any thing. And if it be here objected that the ſolid ſubſtance that accrues to a plant rooted in water proceeds not at all from the water itſelf, but from the nitrous, fat, and earthy ſubſtances that may be preſumed to abound even in common water; not here to repeat what I elſewhere ſay about this objection, I ſhall at preſent reply, that though, as to divers plants that flouriſh after rain, I am apt to think, as I intimated above, that they may in part be nouriſhed as well by the ſaline and earthy ſubſtances, to which the rain uſually proves a vehicle, as by the rain itſelf; yet as to what the objection holds forth about the plants that grow not in the ground, but in glaſſes filled with water, it ſhould not be barely ſaid, but proved; which he will not perhaps think eaſy to be done that conſiders how vaſt a quantity of fair water is requiſite to be exhaled away to obtain as much as one ounce of dry reſidence, whether ſaline or earthy.

III.

THAT a plant growing in the earth doth by the faculties of its vegetative ſoul attract the juices of the earth that are within its reach, and ſelecting thoſe parts that are congruous to its nature, refuſe the reſt, is the general opinion of philoſophers and phyſicians; and therefore many naturaliſts are not wont much to marvel when they ſee a tree bear a fruit that is ſour or bitter, becauſe they preſume that nature hath in the root of the tree culled out ſuch parts of the alimental juice of the earth as being made to convene into one fruit, are fit to make it of ſuch a quality. But it is worth obſerving, for our preſent purpoſe, what happens both in ordinary graftings, and eſpecially in that kind of inciſion (taking the word in a large ſenſe) which is commonly called inoculation. For though we may preſume that the root of a white thorn (for inſtance) may electively attract its aliment from the earth and chooſe that which is fitteſt to produce the ignoble fruit that is proper for that plant; yet we cannot reaſonably ſuppoſe that it ſhould in its attraction of aliment have any deſign of providing an appropriate nutriment for a pear: and yet the known experience of gardeners, and our own obſervations, manifeſt that the cyons of a pear-tree will take very well upon a white-thorn ſtock, and bring forth a well-taſted fruit very differing in many qualities from that of the white-thorn. I have alſo learned from thoſe that are expert, that though apples and pears, being but vulgar fruit, are ſeldom propagated but by grafting, yet they may be propagated likewiſe by inoculation (which ſeems to be but a kind of grafting with a bud). Now in the inoculations that are made upon fruit-trees it is very obſervable, and may much countenance what we are endeavouring to prove, that a little vegetable bud (that is no ſeed properly ſo called) not ſo big oftentimes as a pea, ſhould be able ſo to tranſ-

mute

mute all the ſap that arrives at it, that though this ſap be already in the root, and in its paſſage upwards determined by nature's intention, as men are wont to ſpeak, to the production of the fruit that is natural to the ſtock, yet this ſap ſhould by ſo ſmall a vegetable ſubſtance as a bud (whether by the help of ſome peculiar kind of ſtrainer, or by the operation of ſome powerful ferment lodged in it, or by both theſe, or ſome other cauſe) be ſo far changed and over-ruled as to conſtitute a fruit quite otherwiſe qualified than that which is the genuine production of the tree, and which is actually produced by thoſe other portions of the like ſap which happened to nouriſh the prolific buds that are the genuine offspring of the ſtock; ſo that the ſame ſap that in one part of a branch conſtitutes (for inſtance) a cluſter of haws, in another part of the ſame branch may conſtitute a pear. And that which is further remarkable to our preſent purpoſe, is, that not only the fruits made of the ſame ſap do often differ from one another in ſhape, bigneſs, colour, odour, taſte, and other obvious qualities as well as occult ones; but that though the ſap itſelf be (oftentimes) a wateriſh and almoſt inſipid liquor, that appears to ſenſe homogeneous enough, and even by diſtillation affords very little beſides phlegm; yet this ſap is not only convertible by buds of ſeveral natures into differing fruits, but in one and the ſame fruit the tranſmuted ſap ſhall, by differing textures, be made to exhibit very differing, and ſometimes contrary qualities. As when (for inſtance) a peach-bud does not only change the ſap that comes to it into a fruit very differing from that which the ſtock naturally produceth, but in the ſkin of the peach it muſt be red, in the kernel white, and in other parts, of other colours; the fleſh of it muſt be fragrant, the ſtone inodorous, the fleſh ſoft and yielding, the ſtone very hard and brittle, the meat pleaſantly taſted, the kernel bitter; not to mention that peach-bloſſoms, though produced alſo by the bud, are of a colour and texture very differing from that of the fruit, and are ennobled with an occult quality which the fruit hath not, I mean a purgative virtue: ſo that from inoculations we may learn that a phlegmatic liquor that ſeems homogeneous enough, and but very ſlenderly provided with other manifeſt qualities than common water, may, by being variouſly contexted by the buds of trees, be tranſmuted into bodies endowed with new and various conſiderable ſcents, colours, taſtes, ſolidity, medicinal virtues, and divers other qualities manifeſt and occult.

If it be here ſaid that theſe qualities are the productions of the plaſtic power reſiding in prolific buds, which indeed (to me) ſeem to be but very minute boughs; I ſhall return the ſame anſwer that I did to the like objection when it was propoſed in the firſt obſervation.

Hitherto I have only argued from vulgar inoculations, but there may be others, as well more conſiderable, as leſs ordinary: and I remember I have ſeen a tree, whereof, though the ſtock was of one ſort of good fruit, there were three more and differing kinds of ſtone-fruit that had been made to take by inoculation, and two of thoſe inoculated boughs had actually fruit on them; and the third, though it had as yet no fruit, becauſe the ſeaſon for that ſort of plants to bear was not yet come, yet the ſhoot was ſo flouriſhing, that we concluded that the bloſſoms would in due time be ſucceeded by fruit. And ſince I have been ſpeaking of the differing qualities of the parts of the ſame fruit, I am content to add two things: the one that *Garcias ab Horto*, a claſſic author (and phyſician to the *Indian* viceroy) affirms * with ſome ſolemnity (as wondering that a learned man ſhould write otherwiſe) that though the fruit we call *caſſia fiſtula* be very commonly uſed both here and in the *Indies* as a purging medicine, yet the ſeeds

* *Aromat. Hiſt. lib.* 1. *cap.* 29. *de Caſſia ſolutiva.*

of

of this folutive caffia are aftringent. The other, that of late years there have been often brought into *England* from the *Caribbee* iflands certain kernels of a fruit, which thofe that have feen it grow, liken to a white pear-plumb; thefe are fo ftrongly purgative and alfo emetic, that the ingenious Mr. *Lygon* * tells us that five of them wrought with him a dozen times upwards, and above twenty downwards; and yet the fame author affures us (which is likewife here a received tradition among them that are curious of this fruit) that in the kernel, in the parting of it into halves (as when our hazle-nuts in *England* part in the middle longways) you fhall find a thin film, which looks of a faint carnation (which colour is eafily enough difcerned, the reft of the kernel being perfectly white) and that, taking out the film, you may eat the nut fafely without feeling any operation at all, and it is as fweet as a Jordan almond. [A learned man that practifed phyfic in *America*, being inquired of by me concerning the truth of this relation, anfwered, that though he had divers times given thofe nuts as cathartic remedies, yet he had not that curiofity to take out the films, finding it the univerfal belief that the purgative faculty confifted therein]. And I remember that the famous † *Monardes* doth fomewhat countenance this tradition, where, fpeaking of another purging fruit that alfo comes from *America* (from *Cartagena* and *Nombre de Dios)* he takes notice that thefe purging beans (which are like ours, but fmaller) have a thin fkin that divides them through the middle, which muft (together with the external rind) be caft away, elfe they will work fo violently, both upwards and downwards, as to bring the taker into hazard of his life: whereas he commends thefe beans, rightly prepared, not only as a pleafant medicine that doth, without trouble, purge both choler, phlegm, and grofs humours; for which it is celebrated among the *Indians*.

To thefe ftories of our countrymen and *Monardes*, I fhall fubjoin another which I find related by that great rambler about the world, *Vincent le Blanck*, who giving us an account of a publick garden which he vifited in *Africa*, in the territories of the Lord of *Caftma*, not far from the borders of *Nubia*, which he reprefents as the curioufeft garden he faw in all the Eaft, he mentions this among other rarities: ‘ There were (fays ‘ he) other forts of fruit which I never faw but there, and one among the reft, leaved ‘ like a fycamore, with fruit like the golden apple, but no gall more bitter, and within ‘ five kernels as big as almonds, the juice whereof is fweet as fugar; betwixt the fhell ‘ and the nut there grows a thick fkin of a carnation colour, which, taken before they ‘ be thoroughly ripe, they preferve with date vinegar, and make an excellent fweetmeat, ‘ which they prefent to the king as a great curiofity.’

IV.

THE fourth and laft obfervation I fhall at prefent mention is afforded me by the confideration of rotten cheefe. For if we take notice of the difference betwixt two parts of the fame cheefe, whereof the one continues found by preferving its texture, and the other hath fuffered that impairing alteration of texture we call rottennefs; we may often fee a manifeft and notable change in the feveral portions of a body that was before fimilar: for the rotten part will differ from the found in its colour, which will be fometimes livid, but moft commonly betwixt green and blue; and its odour, which will be both ftrong and offenfive; and its tafte, which will be very piquant, and to fome men much more pleafant than before, but to moft men odious; and in divers other qualities, as

* Lygon's *Hiftory of* Barbados, *pag.* 67, 68.
† *See* Nicholaus Monardes, *under the Title*, Fabæ purgatrices.

particularly its confiftence, it will be much lefs folid and more friable than before; and if with a good microfcope we look upon the moulded parts of many cheefes, we fhall quickly difcover therein fome fwarms of little animals (the mites) furnifhed with variety of parts of differing fizes, fhapes, textures, *&c.* and defcry a yet greater diverfity, both as to manifeft qualities (nor probably is it inferior as to occult ones) betwixt the mouldy part of the cheefe and the untainted, than the unaffifted eye could otherwife have difcovered.

Advertifements about the enfuing Section II.

THE author would not have the reader think that the following experiments are the fole ones, that he could have fet down to the fame purpofe with them. For they are not the only that he had actually laid afide for this occafion, till judging the enfuing ones fufficient for his prefent fcope, he thought it fitter to referve others for thofe notes about the production of particular qualities, to which they feemed properly to belong. Perhaps alfo it will be requifite for me (becaufe fome readers may think the omiffion a little ftrange) to excufe my having left divers particulars unmentioned in more than one of the enfuing experiments. And I confefs that I might eafily enough both have taken notice of more circumftances in them, and made far more reflexions on them, if I would have expatiated on the feveral experiments according to the directions delivered in other * papers. But though there, where it was my defign to give employment to the curiofity and diligence of as many votaries to nature, as (for want of better inftructions) had a mind to be fo fet on work, it was fit the propofed method fhould be fuitable; yet here, where I deliver experiments not fo much as part of natural hiftory, as inftances to confirm the hypothefes and difcourfes they are annexed to, it feemed needlefs and improper (if not impertinent) to fet down circumftances, cautions, inferences, hints, applications, and other particulars that had no tendency to the fcope for which the experiments were alledged.

AND as for the kind of experiments here made choice of, I have the lefs fcrupled to pitch upon chymical experiments rather than others on this occafion; not only becaufe of thofe advantages which I have afcribed to fuch experiments in the latter part of the † preface to my Specimens, but becaufe I have been encouraged by the fuccefs of the attempt made in thofe difcourfes. For as new as it was, when I made it four or five years ago; and as unufual a thing as it could feem to divers Atomifts and Cartefians, that I fhould take upon me to confirm and illuftrate the notions of the Particularian philofopy (if I may fo call it) by the help of an art, which many were pleafed to think cultivated but by illiterate operators or whimfical fanaticks in philofophy, and ufeful only to make medicines or difguife metals: yet thefe endeavours of ours met with much lefs oppofition than new attempts are moft commonly fain to ftruggle with. And in fo fhort a time I have had the happinefs to engage both divers chymifts to learn and relifh the notions of the corpufcular philofophy, and divers eminent embracers of that to en-

* Containing fome advices and directions for the writing of an experimental natural hiftory.

† The preface here mentioned is that premifed to the tract intituled,—*Some Specimens of an attempt to make Chymical Experiments ufeful to illuftrate the notions of the Corpufcular Philofophy.*

deavour

deavour to illuſtrate and promote the new philoſophy, by addicting themſelves to the experiments, and peruſing the books of chymiſts. And I acknowledge it is not unwelcome to me to have been (in ſome little meaſure) inſtrumental to make the corpuſcularian philoſophy, aſſiſted by chymiſtry, preferred to that which has ſo long obtained in the ſchools. For (not here to conſider, which I elſewhere do, how great an advantage that philoſophy hath of this, by having an advantage of it in point of clearneſs) though divers learned and worthy men that knew no better principles, have, in cultivating the Peripatetick ones, abundantly exerciſed and diſplayed their own wit; yet I fear they have very little, if at all, improved their reader's intellect, or enriched it with any true or uſeful knowledge of nature; but have rather taught him to admire their ſubtlety, than underſtand hers. For to aſcribe all particular phænomena that ſeem any thing difficult (for abundance are not thought ſo that are ſo) to ſubſtantial forms, and but nominally underſtood qualities, is ſo general and eaſy a way of reſolving difficulties, that it allows naturaliſts, without diſparagement, to be very careleſs and lazy, if it do not make them ſo; as in effect we may ſee, that in about two thouſand years ſince *Ariſtotle*'s time the adorers of his phyſicks, at leaſt by virtue of his peculiar principles, ſeem to have done little more than wrangle, without clearing up (that I know of) any myſtery of nature, or producing any uſeful or noble experiment: whereas the cultivators of the Particularian philoſophy, being obliged by the nature of their hypotheſis and their way of reaſoning, to give the particular accounts and explications of particular phænomena of nature, are alſo obliged, not only to know the general laws and courſe of nature, but to inquire into the particular ſtructure of the bodies they are converſant with as that wherein, for the moſt part, their power of acting and diſpoſition to be acted on does depend. And in order to this, ſuch inquirers muſt take notice of abundance of minute circumſtances; and to avoid miſtaking the cauſes of ſome of them, muſt often make and vary experiments; by which means nature comes to be much more diligently and induſtriouſly ſtudied, and innumerable particulars are diſcovered and obſerved which in the lazy Ariſtotelian way of philoſophizing would not be heeded. But to return to that decade of inſtances to which theſe advertiſements are premiſed; I hope I need not make an apology for making choice rather of chymical experiments than others, in the ſecond and concluding ſection of the hiſtorical part of the preſent treatiſe. But though I prefer that kind of inſtances, yet I would not be thought to overvalue them in their kind, or to deny that ſome artiſts may (for aught I know) be found, to whoſe chymical arcana theſe experiments may be little better than trifles. Nor perhaps are theſe the conſiderableſt that I myſelf could eaſily have communicated (though theſe themſelves would not be now divulged, if I would have been ruled by the diſſuaſions of ſuch as would have nothing of chymical made common which they think conſiderable). But things of greater value in themſelves, and of noble uſe in phyſick, may be leſs fit for our preſent purpoſe (which is not to impart medicinal or alchymiſtical proceſſes, but illuſtrate philoſophical notions) than ſuch experiments as theſe; which, beſides that they contain variety of phænomena, do not (for the moſt part) require either much time, or much charge, or much ſkill.

SECTION II.

Containing the Experiments.

EXPERIMENT I.

TAKE good and clear oil of vitriol, and cast into it a convenient quantity of good camphire grofly beaten; let it float there a while, and, without the help of external heat, it will insenfibly be resolved into a liquor, which, from time to time, as it comes to be produced, you may, by fhaking the glafs, mingle with the oil of vitriol; whereunto you may by this means impart firft a fine yellow and then a colour which though it be not a true red will be of kin to it, and fo very deep as to make the very mixture almoft quite opacous. When all the camphire is perfectly diffolved by incorporating with the menftruum, if you hit upon good ingredients, and upon a right proportion (for a flight miftake in either of them may make this part of the experiment mifcarry) you may probably obtain fuch a mixture as I have more than once had; namely fuch a one, as not only to me, whofe fenfe of fmelling is none of the dulleft, but alfo to others, that knew not of the experiment, feemed not at all to have an odour of the camphire. But if into this liquor you pour a due quantity of fair water, you will fee (perhaps not without delight) that, in a trice the liquor will become pale almoft as at the firft; and the camphire that lay concealed in the pores of the menftruum, will immediately difclofe itfelf, and immerge in its own nature and priftine form of white floating and combuftible camphire, which will fill not the phial only, but the neighbouring part of the air with its ftrong and diffufive odour.

Now the phænomena of this experiment may, befides the ufes we elfewhere make of it, afford us feveral particulars pertinent to our prefent purpofe.

I. For (firft) we fee a lighter and confiftent body brought by a comminution into particles of a certain figure, to be kept fwimming and mixed with a liquor on which it floated before, and which is by great odds heavier than itfelf: fo that as by the folution of gold in *aqua regis* it appears that the ponderoufeft of bodies, if it be reduced to parts minute enough, may be kept from finking in a liquor much lighter than itfelf; fo this experiment of ours manifefts, what I know not whether hitherto men have proved, that the corpufcles of lighter bodies may be kept from emerging to the top of a much heavier liquor. Which inftance being added to that of the gold, may teach us that when bodies are reduced to very minute parts, we muft as well confider their particular texture, as the received rules of the hydroftaticks, in determining whether they will fink, or float, or fwim.

II. This experiment alfo fhews that feveral colours, and even a very deep one, may foon be produced by a white body and a clear liquor, and that without the intervention of fire or any external heat.

III. And that yet this colour may almoft in the twinkling of an eye be deftroyed, and, as it were, annihilated; and the latitant whitenefs, as many would call it, may be as fuddenly reftored by the addition of nothing but fair water, which has no colour of its own; upon whofe account it might be furmifed to be contrary to the perifhing colour,

lour, or to heighten the other into a predominacy; nor does the water take into itſelf either the colour it deſtroyed, or that it reſtores. For

IV. The more than ſemi-opacity of the ſolution of camphire and oil of vitriol does preſently vaniſh; and that menſtruum with the water make up (as ſoon as the camphorate corpuſcles come to be afloat) one tranſparent and colourleſs liquor.

V. And it is worth noting, that upon the mixture of a liquor which makes the fluid much lighter (for ſo water is in reſpect of vitriol) a body is made to emerge that did not ſo when the fluid was much heavier. This experiment may ſerve to countenance what we elſewhere argue againſt the ſchools touching the controverſy about miſtion. For whereas though ſome of them diſſent, yet moſt of them maintain that the elements always loſe their forms in the mixed bodies they conſtitute; and though if they had dexterouſly propoſed their opinion and limited their aſſertions to ſome caſes, perhaps the doctrine might be tolerated; yet ſince they are wont to propoſe it crudely and univerſally, I cannot but take notice how little it is favoured by this experiment; wherein even a mixed body (for ſuch is camphire) doth in a further miſtion retain its form and nature, and may be immediately ſo divorced from the body to which it was united, as to turn in a trice to the manifeſt exerciſe of its former qualities. And this experiment being the eaſieſt inſtance I have deviſed of the preſervation of a body when it ſeems to be deſtroyed, and of the recovery of a body to its former conditions, I deſire it may be taken notice of, as an inſtance I ſhall after have occaſion to have recourſe to and make uſe of.

VI. But the notableſt thing in the experiment is, that odours ſhould depend ſo much upon texture; that one of the ſubtleſt and ſtrongeſt-ſcented drugs, that the Eaſt itſelf, or indeed the world afford us, ſhould ſo ſoon quite loſe its odour by being mixed with a body that has ſcarce, if at all, any ſenſible odour of its own; and this while the camphorate corpuſcles ſurvive undeſtroyed in a liquor, from whence one would think that leſs ſubtile and fugitive bodies than they ſhould eaſily exhale.

VII. Nor is it much leſs conſiderable that ſo ſtrong and piercing a ſcent as that of camphire ſhould be in a moment produced in a mixture wherein none of it could be perceived before, by ſuch a liquor as water, that is quite devoid of any odour of its own; which ſo eaſy and ſudden reſtauration of the camphire to its native ſcent, as well as other qualities, by ſo languid a liquor as common water, doth likewiſe argue that the union or texture of the two ingredients, the camphire and the oil of vitriol, was but very ſlight, upon which, nevertheleſs, a great alteration in point of qualities depended. And to confirm, that divers of the preceding phænomena depend upon the particular texture of the liquors employed to exhibit them; I ſhall add, that if inſtead of oil of vitriol you caſt the concrete into well-dephlegmed ſpirit of nitre, you will obtain no red nor dark, but a tranſparent and colourleſs ſolution. And when to the above mentioned red mixture I put, inſtead of fair water, about two or three parts of duly rectified ſpirit of wine, there would enſue no ſuch changes as thoſe formerly recited; but the ſpirit of wine that diſſolved the concrete, when it was by itſelf, without loſing its diaphaneity or acquiring any colour, did, when it diſſolved the mixture, diſſolve it with its new adventitious colour, looking like a groſs red wine ſomewhat turbid, or not yet well freed from its lees; ſo that this colour appeared to reſide in the mixture as ſuch, ſince neither of the two ingredients diſſolved in or mingled with the ſpirit of wine would have afforded that colour, or indeed any other. But if to this liquor that looked like troubled wine we poured a large proportion of fair water, the redneſs would immediately vaniſh, and the whole would, as to ſenſe, become white throughout: I ſay as to ſenſe, becauſe the whiteneſs did not indeed appertain properly to the whole mixture, but to a huge multitude

tude of little corpuſcles of the revived concrete, whereof ſome or other, which at firſt ſwam confuſedly to and fro, left no ſenſible portion of the liquor unfurniſhed with ſome of them; whereas when the camphirate corpuſcles had leiſure to emerge, as they ſoon did, they floated in the form of a white powder or froth at the top of the liquor, leaving all the reſt as clear and colourleſs as the common water.

But we have not yet mentioned all the uſe we deſigned to make of our mixture; for, by proſecuting the experiment a little further, we made it afford us ſome new phænomena.

VIII. For having kept the mixture in a moderately warm place (which circumſtance had perhaps no influence on the ſucceſs) and having diſtilled it out of a glaſs retort, the event anſwered our expectation, and the liquor that came over had a ſcent, which, though very ſtrong, was quite differing both from that of the mixture, and that of the camphire; and in the remaining body, though the liquor and the camphire it conſiſted of were either both tranſparent, or the one tranſparent as a liquor, and the other white, as tranſparent colourleſs bodies are wont to be made by contuſion; yet the remaining maſs, which amounted to a good part of the mixture, was not only opacous, but as black as coal, in ſome places looking juſt like poliſhed jet; which is the more conſiderable, becauſe, that, though vegetable ſubſtances, that are not fluid, are wont to acquire a blackneſs from the fire, yet neither do liquors, that have already been diſtilled, obtain that colour upon re-diſtillation; neither have we, upon trial purpoſely made, found that camphire, expoſed to fire in a retort, fitted with a receiver (which was the caſe of the preſent experiment) would at all acquire a jetty colour, but would either totally aſcend white, or afford *flores*, and a *caput mortuum* (as a vulgar chymiſt would call the remains) of the ſame colour, both in reſpect of one another, and in reſpect of the camphire.

IX. And our experiment afforded this notable phænomenon, that though oil of vitriol be a diſtilled liquor, and though camphire be ſo very fugitive a ſubſtance, that being left in the air it will of itſelf fly all away, and therefore phyſicians and druggiſts preſcribe the keeping it in linſeeds or millium, or other convenient bodies, to hinder its avolation; yet, by our experiment, its fugacity is ſo reſtrained, that not only the *caput mortuum*, newly mentioned, endured a good fire in the retort, before it was reduced to that pitchy ſubſtance we were lately mentioning; but having taken ſome of that ſubſtance out of the retort, and ordered it by a careful workman to be kept in a cloſely covered crucible during ſome time in the fire, when it was brought me back, after the pot had been kept red-hot above half an hour, there remained a good quantity of the matter, brittle, without any ſmell of camphire, and as black as ordinary charcoal: ſo much do the fixidity and volatility of bodies depend upon texture.

EXPERIMENT II.

Among thoſe experiments of mine, *Pyrophilus*, which tend to manifeſt, that new qualities may be produced in bodies, as the effects of new textures; I remember, ſome years ago, I writ for a friend a whole ſet of trials that I had made about the changes I could produce in metals and minerals by the intervention of ſublimate. But though the whole tract wherein they are recited might be pertinent enough to our preſent ſubject, yet reſerving other paſſages of it for other places (eſpecially for our notes upon thoſe particular qualities, which they are moſt proper to illuſtrate) it may at this time ſuffice me to ſend you a tranſcript of what that account contains relating to copper and ſilver, the one a mean and fugitive, and the other a noble and fixed metal. For thoſe changes in

 colour,

colour, consistence, fusibleness, and other qualities, which you will meet with in these experiments, will afford us divers phænomena, to shew what great changes may be made, even in bodies scarce corruptible, by one or more of those three catholic ways of nature's working according to the corpuscular principles; namely, the accesss, the recess, and the transposition of the minute particles of matter.

As for my method of changing the texture of copper, I confess it hath oftentimes seemed strange to me, that chymists, plainly seeing the notable effect that sublimate distilled from antimony has upon that mineral, by opening it and volatilizing it (as we see it do in the making of what they are pleased to call *Mercurius vitæ*) should not have the curiosity to try, whether or no sublimate might not likewise produce, if not the same, yet a considerable change in other mineral bodies; there appearing no reason, or at least there having been none given, that I know of, why the reserating operation (if I may so speak) of sublimate should be confined to antimony. Upon these considerations, we were invited to endeavour to supply the neglect we had observed in chymists of improving the experiment of *Butyrum Antimonii*: and though an indisposition in point of health, which befel us before we had made any great progress in our inquiries, made us so shy of fumes of sublimate and minerals, that we neither did make all our trials so accurately, nor prosecute them so far as we would have done, had we been to deal with more innocent materials; yet we suppose it will not be unwelcome to you to receive from us a naked, but faithful narrative of our proceedings; being apt to think that you will therein find inducements to carry on this experiment further than we have done, and to compleat what we have but begun.

First then, we took half a pound of copper plates, of about an inch broad, and the thickness of a grain of wheat (which we after found was too great) and of an arbitrary length; then casting a pound of grosly beaten Venetian sublimate into the bottom of a somewhat deep glass retort, we cast in the copper plates upon it, that the fumes of the sublimate might, in their ascension, be compelled to act upon the incumbent metal; and then placing this retort as deep as we well could in a sand furnace, and adapting to it a small receiver, we administered a gradual fire seven or eight hours, and at length for a while increased the heat, as much as we well could do in such a furnace. The success of this operation was as follows.

1. There came little or no liquor at all over into the receiver, but the neck and upper part of the retort were candied on the inside, by reason of the copious sublimate adhering to them, which sublimate weighed about ten ounces: in the retort we found about two ounces and a quarter of running mercury, which had been suffered to revive by the acid salts, which corroding the copper, forsook the quicksilver, whereto they had been in the sublimate united.

2. Upon the increase of the fire there was plainly heard a noise, made by the melting matter in the retort, not unlike that of a boiling pot, or of vitriol, when, being committed to a calcining fire, it is first brought to flow. And this noise we found to be a more constant circumstance of this experiment, than the revivification of part of the mercury contained in the sublimate: for upon another trial made with the former proportion of copper plates and sublimate, we observed, during a very long while, such a noise as hath been already mentioned; but the operation being finished, we scarce found so much as a few grains of running mercury either in the retort or receiver.

3. We found the metalline lump in the bottom of the retort to have been increased in weight somewhat more than (though not half an ounce above) two ounces; some of the copper plates, lying at the bottom of the mass, retained yet their figure and malleableness, which we ascribe to their not having been thin enough to be sufficiently wrought

upon

upon by the fublimate: the others, which were much the greater number, had wholly loft their metalline form, and were melted into a very brittle lump, which I can compare to nothing more fitly, than a lump of good benjamin. For this mafs, though ponderous, was no lefs brittle, and being broken, appeared of divers colours, which feemed to be almoft tranfparent, and in fome places it was red, in others of a high and pleafant amber colour, and in other parts of it colours more darkifh and mixt might be difcerned.

4. But this ftrange mafs being broken into fmaller lumps, and laid upon a fheet of white paper in a window, was by the next morning, where-ever the air came at it, all covered with a lovely greenifh blue, or rather bluifh green, almoft like that of the beft verdigreefe: and the longer it lay in the air, the more of the internal parts of the fragments did pafs into the fame colour; but the white paper, which in fome places they ftranded, feemed dyed of a green colour inclining unto yellow. And here we had occafion to take notice of the infinuating fubtilty of the air; for having put fome pieces of this cupreous gum (if I may fo call it) into a little box, to fhut out the air, which we have found it poffible to exclude by other means, we found that notwithftanding our care, thofe included fragments were, as well as the reft already mentioned, covered with the powder as it were of *viride æris*.

5. We muft not on this occafion omit to tell you, that having the laft year made fome trials in reference to this experiment, we obferved in one of them, that fome little copper-plates, from which fublimate had been drawn off, retained their priftine fhape and metalline nature, but were whitened over like filver, and continued fo for divers months (though we cannot precifely tell you how long, having at length accidentally loft them). And to try whether this whitenefs were only fuperficial, we purpofely broke fome of thefe flexible plates, and found that this filver colour had penetrated them throughout, and was more glorious in the very body of the metal than on its furface; which made us fufpect that the fublimate by us employed had been adulterated with arfenick (wherewith the fophifticators of metals are wont to make blanchers for copper, but not to mention that the malleablenefs continued, which arfenick is wont to deftroy) we difcovered not by trial, that the fublimate was other than fincere.

6. In this metalline gum the body of the copper appeared fo changed and opened, that we were invited to look upon fuch a change as no ignoble experiment, confidering the difficulty which the beft artifts tells us there is, and which thofe that have attempted it have found; I fay, not to unlock the fulphur of Venus, but to effect lefs changes in its texture, than was hereby made. For this gum, caft upon a quick coal and a little blown, will partly melt and flow like rofin, and partly flame and burn like a fulphur, and with a flame fo lafting, if it be rekindled as often as it leaves off burning, that we obferved it, not without fome wonder; and fo inflammable is this opened copper, that being held to the flame of a candle or a piece of lighted paper, it would almoft in a moment take fire, and fend forth a flame like common fulphur, but only that it feemed to us to incline much more to a greenifh colour, than the bluer flame of brimftone is wont to do.

To thefe phænomena of our experiment, as it was made with copper, my notes enable me to fubjoin fome others, exhibited, when we made it with fublimate and filver.

There were taken of the pureft fort of coined filver we could get, half a fcore of thin plates, on which was caft double the weight of fublimate in a fmall and ftrongly-coated retort. This matter being fublimed in a naked fire, we found (having broken the veffel) that the fublimate was almoft totally afcended to the top and neck of the retort; in the latter of which appeared in many places fome revived mercury; in the bottom of the

retort

retort we found a little fluxed lump of matter, which it was ſcarce poſſible to ſeparate from the glaſs; but having with much ado divorced them, we found this maſs to be brittle, of a pale yellowiſh colour, of near about the weight of the metal on which the ſublimate had been caſt. And in the thicker part of this lump there appeared, when it was broken, ſome part of the ſilver plates, which, though brittle, ſeemed not to have been perfectly diſſolved. This reſin of ſilver did, like that of copper, but more ſlowly, imbibe the moiſture of the air, and within about twenty-four hours was covered with a ſomewhat greeniſh duſt, concerning which we durſt not determine, whether it proceeded from that mixture of copper, which is generally to be met with in coined ſilver, or from the compounded metal: for the more curious ſort of painters do, as they inform us, by corroding coined ſilver with the fretting ſteams of ſaline bodies, or with corroſive bodies themſelves, turn it into a fine kind of azure, as we may elſewhere have opportunity more particularly to declare. I ſhall now only add, that ſome ſmall fragments of our reſin being caſt upon red-hot coals, did there waſte themſelves in a flame, not very differing in colour from that of the former mentioned reſin of copper, but much more durable than would have been eaſily expected from ſo ſmall a quantity of matter.

This is all the account I can give you of our firſt trial; but ſuſpecting that the copper wont to be mixed as an alloy with our coined ſilver might have too much influence on the recited event, coming afterwards into a place where we could procure refined ſilver, we took an ounce of that, and having laminated it, we caſt it upon twice its weight of beaten ſublimate, which being driven away from it with a ſomewhat ſtrong fire, we took out of the bottom of the glaſs retort a lump of matter, which in ſome places, where it lay next the glaſs, was as it were ſilvered over very finely, but ſo very thinly, that the thickneſs of the ſilver ſcarce equalled that of fine white paper; the reſt of the metal (except a little that lay undiſſolved almoſt in the middle of the maſs, becauſe, as we ſuppoſed, the plates had not been beaten till they were ſufficiently and equally thin) having been, by the ſaline part of the ſublimate that ſtuck to it, colliquated into a maſs that looked not at all like ſilver, or ſo much as any other metal or mineral.

And it is remarkable, that though ſilver be a fixed metal, and accounted indeſtructible, yet it ſhould by ſo ſlight an operation, and by but about a quarter of its weight of additament (as appeared by weighing the whole lump) be ſo ſtrangly diſguiſed, and have its qualities ſo altered.

For (firſt) though an eminent whiteneſs be accounted the colour which belongs to pure ſilver, and though beaten ſublimate be alſo eminently white; yet the maſs we are ſpeaking of was partly of a lemon or amber colour, or a deep amethyſtine colour, and partly of ſo dark an one, as it ſeemed black: and it was pretty, that ſometimes in a fragment, that ſeemed to be one continued and entire piece, the upper part would be of a light yellow, which abruptly ending, the lower was of a colour ſo obſcure, as ſcarce to challenge any name diſtinct from black.

Next, Whereas ſilver is one of the moſt opacous bodies in nature, and ſublimate a white one, the produced maſs was in great part tranſparent, though not like glaſs, yet like good amber.

Thirdly, The texture of the ſilver was exceedingly altered; for our maſs, inſtead of being malleable and flexible, as that metal is very much, appeared, if you went about to cut it with a knife, like horn, yet otherwiſe eaſily apt to crack and break, though not at all to bend.

Fourthly, Whereas ſilver will endure ignition for a good while before it be brought to fuſion, our mixture will eaſily melt, not only upon quick coals, but in the flame of a candle; but this refin or gum (if I may fo call it) of our fixed metal did not, like that we formerly defcribed of copper, tinge the flame of a candle, or produce with the glowing coals, on which it is laid, either a green or bluifh colour.

And, *Pyrophilus*, to difcover, how much thefe operations of the fublimate upon copper and filver depend upon the particular textures of thefe bodies, I took two parcels of gold, the one common gold thinly laminated, and the other very well refined, and having caft each of thefe in a diftinct urinal upon no lefs than thrice its weight of grofly beaten fublimate, I caufed this laft named fubftance to be in a fand-furnace elevated from the gold, but found not that either of the two parcels of that metal was manifeftly altered thereby: whether in cafe the gold had been reduced to very minute particles, fome kind of change (perhaps, if any, differing enough from thofe lately recited to have been made in the copper and the filver) might have been made in it, I am not fo abfolutely certain; but I am confident, that by what I referve to tell you hereafter of fublimate's operation upon fome other minerals, efpecially tin, it will appear, that that operation depends very much upon the particular texture of the body from whence that fublimate is elevated.

Before I difmifs this fubject, *Pyrophilus*, I muft not conceal from you, that in the papers whence thefe experiments made with fublimate have been tranfcribed, I annexed to the whole difcourfe a few advertifements, whereof the firft was, that I was reduced in thofe experiments to imploy, for want of a better, a fand-furnace, wherein I could not give fo ftrong a fire as I defired; which circumftance may have had fome influence upon the recited phænomena: and among other advertifements there being one, that will not be impertinent to my prefent defign, and may poffibly afford a not unfuccefsful hint, I fhall fubjoin it in the words wherein I find it delivered.

The next thing of which I am to advertife you, is this, that this experiment may probably be further improved by imploying about it various and new kinds of fublimate; and that feveral other things may be fublimed up together either with crude mercury or with common fublimate, he that confiders the way of making vulgar fublimate, will not, I fuppofe, deny. To give you only one inftance, I fhall inform you, that, having caufed about equal parts of common fublimate and fal armoniack to be well powdered and incorporated, by fubliming the mixture in ftrong and large urinals placed in a fand-furnace, we obtained a new kind of fublimate, differing from the former, which we manifefted *ad oculum*, by diffolving a little of it and a little of common fublimate feverally in fair water: for dropping a little refolved falt of tartar upon the folution of common fublimate, it immediately turned of an orange-tawny colour; but dropping the fame liquor upon the folution of the armoniack fublimate, if I may fo call it, it prefently turned into a liquor, in whitenefs refembling milk. And having from four ounces of copper-plates drawn fix ounces of this new fublimate after the already often recited manner, we had indeed in the bottom of the retort a cupreous refin, not much unlike that made by copper and common fublimate; and this refin did, like the other, in the moift air foon begin to degenerate into a kind of verdigreefe. But that which was fingular in this operation, was, that not only fome of the fublimate had carried up to a good height enough of the copper to be manifeftly coloured by it of a fine bluifh green, but into the receiver there was paffed near an ounce of liquor, that fmelt almoft like fpirit of fal armoniack, and was tincted like the fublimate, fo that we fuppofed the body of the *Venus* to have been better wrought upon by this, than by the former fublimate. And yet I judged not this way to be the moft effectual way of improving common fublimate, being apt to think, upon grounds not now to be mentioned, that it may by convenient liquors be fo far enriched and advanced, as to be made capable of opening the compact body of gold itſelf, and of producing in it ſuch changes (which yet perhaps will enrich but men's underſtandings) as chymiſts are wont very fruitleſly to attempt to make in that almoſt indeſtructible metal. But of this, having now given you a hint, I dare here ſay no more.

EXPERIMENT III.

THERE is, *Pyrophilus*, another experiment, which many will find more eaſy to be put in practice, and which yet may, as to ſilver, be made a kind of ſuccedaneum to the former, and conſequently may ſerve to ſhew how the like qualities in bodies may be effected by differing ways, provided a like change of texture be produced by them. Of this I ſhall give you an example in that preparation of ſilver that ſome chymiſts have called *Luna Cornea*, which I ſhall not ſcruple to mention particularly, and apply to my preſent purpoſe; becauſe, though the name of *Luna Cornea* be already to be met with in the writings of ſome alchymiſts, yet the thing itſelf, being not uſed in phyſick, is not wont to be known by thoſe that learn chymiſtry in order to phyſick; and the way that I uſe in making it is differing from that of alchymiſts, being purpoſely deſigned to ſhew ſome notable phænomena, not to be met with in their way of proceeding.

WE take then refined ſilver, and having beaten it into thin plates, and diſſolved it in about twice its weight of good aqua fortis, we filtrate it carefully to obtain a clear ſolution, (which ſometimes we evaporate further, till it ſhoot into cryſtals, which we afterwards dry upon brown paper with a moderate heat).

UPON the abovementioned ſolution we drop good ſpirit of ſalt, till we find that it will no more curdle the liquor it falls into (which will not happen ſo ſoon as you will be apt at firſt to imagine); then we put the whole mixture in a glaſs funnel lined with cap-paper, and letting the moiſture drain through, we dry with a gentle heat the ſubſtance, that remains in the filtre, firſt waſhing it (if need be) from the looſely adhering ſalts, by letting fair water run through it ſeveral times, whilſt it yet continues in the filtre. This ſubſtance being well dried, we put it into a glaſs phial, which being put upon quick coals firſt covered with aſhes, and then freed from them, we melt the contained ſubſtance into a maſs, which, being kept a while in fuſion, gives us the *Luna Cornea* we are now to conſider.

IF to make this factitious concrete, we firſt reduce the ſilver into cryſtals, and afterwards proceed with ſpirit of ſalt, as we have juſt now taught you to do with the ſolution, we have the exceedingly opacous, malleable, and hardly fuſible body of ſilver, by the convenient interpoſition of ſome ſaline particles, not amounting to the third part of the weight of the metal, reduced into cryſtals, that both ſhoot in a peculiar and determinate figure, differing from thoſe of other metals, and alſo are diaphanous and brittle, and by great odds more eaſily fuſible than ſilver itſelf, beſides other qualities, wherein having elſewhere taken notice, that theſe cryſtals differ both from ſilver and from aqua fortis, we ſhall not now inſiſt on them, but paſs to the qualities that do more properly belong to the change of the ſolution of ſilver into *Luna Cornea*.

FIRST then we may obſerve, that though ſpirit of ſalt be an highly acid liquor, and though acid liquors and alkalies are wont to have quite contrary operations, the one precipitating what the other would diſſolve, and diſſolving what the other would precipitate; yet in our caſe, as neither oil of tartar *per deliquium*, nor ſpirit of ſalt will diſſolve ſilver, ſo both the one and the other will precipitate it; which I deſire may be taken notice of againſt the doctrine of the vulgar chymiſts, and as a proof that the precipitation of bodies depends not upon acid or alkalizate liquors as fuch, but upon the texture of the bodies, that happen to be confounded.

2. WE may here obferve that whitenefs and opacity may be immediately produced by liquors, both of them diaphanous and colourlefs.

3. THAT, on the other fide, a white powder, though its minute parts appear not tranfparent, like thofe of beaten glafs, refin, *&c.* which, by comminution, are made to feem white, may yet, by a gentle heat, be prefently reduced into a mafs indifferently tranfparent, and not at all white, but of a fair yellow.

4. WE may obferve too, that though filver require fo ftrong a fire to melt it, and may be long kept red-hot, without being brought to fufion; yet by the affociation of fome faline particles conveniently mingled with it, it may be made fo fufible, as to be eafily and quickly melted, either in a thin phial, or at the flame of a candle, where it will flow almoft like wax.

5. IT may alfo be noted, that though the lunar folutions and the fpirit of falt would, either of them apart, have readily diffolved in water; yet when they are mingled, they do, for the moft part, coagulate into a fubftance that will lie undiffolved in water, and is fcarce, if at all, foluble either in aqua fortis or in fpirit of falt.

6. AND remarkable it is, that the body of filver being very flexible and malleable, (efpecially if the metal be, as ours was, refined) it fhould yet, by the addition of fo fmall a proportion of falt (a body rigid and brittle) as is affociated to it in our experiment, be made of a texture fo differing from what either of its ingredients was before, being wholly unlike either a falt or a metal, and very like in texture to a piece of horn. And to fatify my felf how much the toughnefs of this metalline horn depended upon the texture of the *compofitum* refulting from the refpective textures of the feveral ingredients, I precipitated a folution of filver with the diftilled faline liquor commonly called oil of vitriol, inftead of fpirit of falt; and having wafhed the precipitate with common water, I found, agreeably to my conjecture, that this precipitate, being fluxed in a moderate heat afforded a mafs, that looked like enough to the concrete we have been difcourfing of, but had not its toughnefs, being brittle enough to be eafily broken in pieces. But the two confiderableft phænomena of our experiment do yet remain unmentioned.

FOR, 7thly, it is odd, that whereas a folution of filver is, as we have often occafion to note, the bittereft liquor we have ever met with, and the fpirit of falt far fourer than either the fharpeft vinegar, or even the fpirit of it, thefe two fo ftrongly and offenfively tafted liquors fhould be fo eafily and fpeedily, without any other thing to correct them, be reduced into an infipid fubftance (at leaft fo far infipid, that I have licked it feveral times with my tongue, without finding it otherwife, though perhaps with much rolling it to and fro in the mouth, it may at length afford fome unpleafant tafte, but exceedingly different from that of either of the liquors that compofed it): and this, though the falts, that made both the filver and the precipitating fpirit fo ftrongly tafted, remain affociated with the filver.

8. AND laftly, it is very ftrange, that though the faline corpufcles that give the efficacy both to good aqua fortis and the like fpirit of falt, be not only fo volatile that they will eafily be diftilled with a moderate fire, but fo fugitive, that they will in part fly away of themfelves in the cold air (as our nofes can witnefs to our trouble, when the phials, that contain fuch liquors, are unftopped); yet by virtue of the new texture they acquire by affociating themfelves with the corpufcles of the filver and with one another, thefe minute particles of falt lofe fo much of their former lightnefs, and acquire fuch a degree of fixednefs, that they will endure melting with the metal they adhere to, rather than fuffer themfelves to be driven away from it. Nor do I remember, that when I melted this mafs in a thin

a thin phial, I could perceive any ſenſible evaporation of the matter; nay, having afterwards put a parcel of it upon a quick coal, though that were blown to intend the heat, yet it ſuffered fuſion, and ſo ran off from the coal, without appearing, when it was taken up again, to be other than *Luna Cornea*, as it was before.

EXPERIMENT IV.

I AM now, *Pyrophilus*, about to do a thing contrary enough both to my cuſtom and inclination; that is, to diſcourſe upon the phænomena of an experiment, which I do not teach you to make. But ſince I cannot as yet, without ſome breach of promiſe, plainly diſcloſe to you what I muſt now conceal, your equity aſſures me of your pardon. And as becauſe the qualities of the ſalt I am to ſpeak of are very remarkable, and pertinent to my preſent purpoſe, I am unwilling to paſs them by unmentioned; ſo I hope, that, notwithſtanding their being ſtrange, I may be allowed to diſcourſe upon them to you, who, I preſume, know me too well to ſuſpect I would impoſe upon you in matters of fact, and to whom I am willing (if you deſire it) to ſhew the anomalous ſalt itſelf, and ocular proofs of the chief properties I aſcribe to it.

I SHALL not then ſcruple to tell you, that diſcourſing one day with a very ingenious traveller and chymiſt, who had had extraordinary opportunities to acquire ſecrets, of a certain odd ſalt I had thought upon and made, which was of ſo differing a kind from other ſalts, that though I did not yet know what feats I ſhould be able to do with it, yet I was confident it muſt have noble and unuſual operations: this gentleman, to requite my frankneſs, told me, that I had lighted on a greater jewel than perhaps I was aware of; and that if I would follow his advice, by adding ſomething that he named to me, and proſecuting the preparation a little further, I ſhould obtain a ſalt exceedingly noble. I thanked him, as I had cauſe, for his advice, and when I had opportunity, followed it. And though I found the way of making this ſalt ſo nice and intricate a thing, that if I would, I could ſcarce eaſily deſcribe it, ſo as to enable moſt men to practiſe it; yet having once made it, I found, that beſides ſome of the things I had been told it would perform, I could do divers other things with it, which I had good cauſe to believe the gentleman of whom I was ſpeaking did not think of; and I doubt not but I ſhould have done much more with it, if I had not unfortunately loſt it ſoon after I had prepared it.

SEVERAL of the phænomena I tried to produce with it, which are not ſo proper for this place, are reſerved for another: but here I ſhall mention a few that beſt fit my preſent purpoſe.

FIRST then, though the ſeveral ingredients that compoſed this ſalt, were all of them ſuch as vulgar chymiſts muſt, according to their principles, look upon as purely ſaline, and were each of them far more ſalt than brine, or more ſour than the ſtrongeſt vinegar, or more ſtrongly taſted than either of thoſe two liquors; yet the compound, made up of only ſuch bodies, is ſo far from being eminently ſalt, or ſour, or inſipid, that a ſtranger being aſked what taſte it had, would not ſcruple to judge it rather ſweet, than of any other taſte; though its ſweetneſs be of a peculiar kind, as there is a difference even among bodies ſweet by nature; the ſweetneſs of ſugar being diverſe from that of honey, and both of them differing from that of the ſweet vitriol of lead. And this is the only inſtance I remember I have hitherto met with of ſalts, that, without the mixture of inſipid bodies, compoſe a ſubſtance really ſweet. I ſay, really ſweet, becauſe chymiſts oftentimes term the calces of metals, and other bodies dulcified, if they be freed from all corroſive ſalts and ſharpneſs of taſte, ſweet, though they have nothing at all of poſitive

ſweetfweetnefs in them; and by that licence of fpeaking do often enough impofe upon the unfkilful.

ANOTHER thing confiderable in our anomalous falt is, that though its odour be not either ftrong or offenfive (both which that of volatile falts is wont to be); yet if it be a little urged with heat, fo as to be forced to evaporate haftily and copioufly, I have known fome that have been ufed to the powerful ftink of aqua fortis, diftilled urine, and even fpirit of fal armoniack itfelf, that have complained of this fmell as more ftrong, and upon that account more unfupportable than thefe themfelves; and yet when thefe fumes fettle again into a falt, their odour will again prove mild and inoffenfive, if not pleafant.

THIRDLY, Whereas all the volatile and acid and lixiviate falts, that we know of, are of fo determinate and fpecificated a nature, if I may fo fpeak, that there is no one fort of the three but may be deftroyed by fome one or other of the other two falts, if not by both; as fpirit of urine, which is a volatile falt, being mingled with fpirit of falt or aqua fortis, or almoft any other ftrong and acid fpirit, will make a great ebullition, and lofe its peculiar tafte, and feveral of its other qualities; and, on the other fide, falt of tartar and other alkalies (that is, falts produced by incineration of mixed bodies) will be deftroyed with ebullition by aqua fortis, fpirit of falt, or almoft any other ftrong fpirit of that family: and fpirit of falt, aqua fortis, *&c.* will be (as they fpeak) deftroyed both by animal volatile falts, and by the fixed falts of vegetables; that is, will make an effervefcence with either fort of falts, and compofe with them a new liquor or falt, differing from either of the ingredients, and, as to tafte, fmell, odour, and divers other qualities, more languid and degenerous: whereas, I fay, each of thefe three families of falts may be eafily deftroyed by the other two, our anomalous falt feems to be above the being thus wrought upon by any of all the three, and is the only body I know (which is no fmall privilege, or rather prerogative): for I did not find that a folution of it made with as little water as I could, which is the way whereby we ufually make it fluid, would make any ebullition either with oil of tartar *per deliquium*, or fpirit of fal armoniack, or ftrong fpirit of falt, or even oil of vitriol, but would calmly and filently mix with thefe differing liquors, and continue as long as I had patience to look upon them, without being precipitated by them. But this is not the only way I employed, to examine whether our falt belonged to any of the three above-mentioned comprehenfive families of falts. For I found that the ftrongeft folution of it would turn fyrup of violets either red, as acid fpirits do, or green, as both fixed and volatile falts will do. Nor would our folution turn a clear one of fublimate made in common water either white, as fpirit of urine, fal armoniack, or others of the fame family, or into an orange tawny, like falt of tartar, and other alkalies; but left the folution of fublimate tranfparent, without giving it any of thefe colours, mingling itfelf very kindly with it, as it had done with the four lately mentioned liquors. And to fatisfy my felf a little further, I not only tried that an undifcoloured mixture of fyrup of violets and our folution would immediately be turned red by two or three drops of fpirit of falt, or green by as much oil of tartar; but to profecute the experiment, I let fall a drop or two of a mixture made of our anomalous folution, and fpirit of falt well fhaken together, upon fome fyrup of violets, which was thereby immediately turned red, and a little of the fame anomalous folution, being fhaken together with oil of tartar *per deliquium*, turned another parcel of the fame fyrup of violets into a delightful green; which, happening as I expected, feemed to argue that our folution, though as to fenfe it were exquifitely mingled in the feveral mixtures to which I had put it, did, as it left them their undeftroyed refpective natures, retain its own: and yet this falt is fo far from being a languid or an infignificant thing, that aqua fortis, and oil of vitriol themfelves, as operative

operative and as furious liquors as they are, are unable in divers caſes to make ſuch ſolutions, and perform ſuch other things as our calm but powerful menſtruum can, though but ſlowly effect.

FOURTHLY, Though this ſalt be a volatile one, and requires no ſtrong heat to make it ſublime into finely figured cryſtals without a remanence at the bottom; yet, being diſſolved in liquors, you may make the ſolution, if need be, to boil, without making any of the ſalt ſublime up, before the liquor be totally or almoſt totally drawn off; whereas the volatile ſalt of urine, blood, hartſhorn, *&c.* are wont to aſcend before almoſt any part of the liquor they are diſſolved in, which is in many caſes very inconvenient.

AND though this be a volatile ſalt, yet I remember not that I have obſerved any fixed ſalt (without excepting ſalt of tartar itſelf) that runs near ſo ſoon *per deliquium* as this will do; but by abſtraction of the adventitious moiſture, it is eaſily reſtored to its former ſaline form, and yet differs from ſalt of tartar, not only in fixedneſs and taſte, and divers other qualities, but alſo in this, that whereas ſalt of tartar requires a vehement fire to flux it, a gentler heat than one would eaſily imagine will melt our ſalt into a limpid liquor.

AND whereas ſpirit of wine will diſſolve ſome bodies, as ſanderick, maſtick, gum-lack, *&c.* and water, on the other ſide, diſſolves many that ſpirit of wine cannot, and oils will diſſolve ſome, for which neither of the other liquors are good ſolvents; our ſalt will readily diſſolve both in fair water, in the higheſt rectified ſpirit of wine (and that ſo little, as not to weigh more than the ſalt) and in chymical oils themſelves, with which it will aſſociate itſelf very ſtrictly, and perhaps more too than I have yet found any other conſiſtent ſalt to do.

EXPERIMENT V.

THE experiment I am, *Pyrophilus*, now about to deliver, though I have not yet had opportunity to perfect what I deſigned, when ſome notions that I have about fire and ſalt ſuggeſted it to me, is yet ſuch as may far more clearly than almoſt any of the experiments commonly known to chymiſts ſerve to ſhew us how near to a real tranſmutation thoſe changes may prove, that may be effected even in inanimate, and, which is more, ſcarce corruptible bodies, by the receſs of ſome particles, and the acceſs of ſome others, and the new texture of the reſidue. The experiment I have made ſeveral ways, but one of the lateſt and beſt I have uſed is this: Take one part of good ſea-ſalt well dried and powdered and put to it double its weight of good aqua fortis or ſpirit of nitre; then having kept it (if you have time) for ſome while in a previous digeſtion, diſtil it over with a ſlow fire in a retort or a low body, till the remaining matter be quite dry, and no more: for this ſubſtance that will remain in the bottom of the glaſs is the thing that is ſought for.

THIS operation being performable in a moderate fire, and the bodies themſelves being almoſt of an incorruptible nature, one would ſcarce think that ſo ſlight a matter ſhould produce any change in them; but yet I found, as I expected, theſe notable mutations of qualities effected by ſo unpromiſing a way.

FOR in the firſt place we may take notice that the liquor that came over was no longer an aqua fortis or ſpirit of nitre, but an aqua regis that was able to diſſolve gold, which aqua fortis will not meddle with, and will not diſſolve ſilver as it would have done before, but will rather, as I have purpoſely tried, precipitate it out of aqua fortis, if that menstruum have already dissolved it. But this change belonging not so properly to the substance itself I was about to consider, I shall not here insist on it.

2. THEN the taste of this substance comes by this operation to be very much altered; for it hath not that strong saltness that it had before, but tastes far milder; and though it relish of both, affects the palate much more like saltpetre than like common salt.

3. NEXT, whereas this last-named body is of very difficult fusion, our factitious salt imitates salt-petre in being very fusible; and it will, like nitre, soon melt by being held in the flame of a candle.

4. BUT to proceed to a more considerable phænomenon, it is known that sea-salt is a body that doth very much resist the fire; when once, by being brought to fusion, it hath been forced to let go that windy substance that makes unbeaten salt crackle in the fire, and so by blowing it accidentally increase it. It is also known that acid spirits, as those of salt, vitriol, nitre, vinegar, *&c.* are not only not inflammable themselves, but hinderers of inflammation in other bodies; and yet my conjecture leading me to expect that by this operation I should be able to produce out of two uninflammable bodies, a third that would be easily inflammable, I found upon trial not only that small lumps of this substance cast upon quick and well-blown coals, though they did not give so blue a flame as nitre, did yet, like it, burn away with a copious and vehement flame. And for further trial, having melted a pretty quantity of this transmuted sea-salt in a crucible, by casting upon it little fragments of well-kindled charcoal, it would, like nitre, presently be kindled, and afford a flame so vehement and so dazling that one that had better eyes than I, and knew not what it was, complained that he was not able to support the splendor of it. Nor were all its inflammable parts consumed at one deflagration, for by casting in more fragments of well-kindled coal the matter would fall a puffing, and flame afresh for several times consecutively according to the quantity that had been put into the crucible.

5. BUT this itself was not the chief discovery I designed by this experiment. For I pretended hereby to devise a way of turning an acid salt into an alkali, which seems to be one of the greatest and difficultest changes, that is rationally to be attempted among durable and inanimate bodies. For it is not unknown to such chymists as are any thing inquisitive and heedful, how vast a difference there is between acid salts, and those that are made by the combustion of bodies, and are sometimes called fixed, sometimes alkalizate. For whereas strong lixiviums (which are but strong solutions of alkalies) will readily enough dissolve common sulphur and divers other bodies abounding with sulphur, even those highly acid liquors aqua fortis and aqua regis, though so corrosive that one will dissolve silver and the other gold itself, will let brimstone lie in them undissolved I know not how long; though some say that in process of time there may be some tincture drawn by the menstruum from it, which I have not seen tried, and though it were true, would yet sufficiently argue a great disparity betwixt those acid spirits and strong alkalizate solutions, which will speedily dissolve the very mass of common sulphur. Besides it is observed by the inquisitive chymists, nor does my experience contradict it, that the bodies that are dissolved by an acid menstruum may be precipitated by an alkalizate; and on the contrary, solutions made by the latter may be precipitated by the former. Moreover, as litharge dissolved in spirit of vinegar will be precipitated by the oil of tartar *per deliquium*, or the solution of its salt; and, on the contrary, sulphur or antimony dissolved in such a solution will be precipitated out of it by the spirit of vinegar or even common vinegar: moreover acids and alkalizates do also differ exceedingly in taste, and in this greater disparity, that the one is volatile and the other fixed, besides other particulars not necessary here to be insisted on. And indeed if that

were true which is taught in the ſchools, that there is a natural enmity as well as diſparity betwixt ſome bodies, as between oily and wateriſh ones, the chymiſts may very ſpecioully teach (as ſome of them do) that there is a ſtrange contrariety betwixt acid and alkalizate ſalts; as when there is made an affuſion of oil of tartar upon aqua regis or aqua fortis, to precipitate gold out of the one and ſilver out of the other, their mutual hoſtility ſeems manifeſtly to ſhew itſelf not only by the noiſe, and heat, and fume that are immediately excited by their conflict, but by this moſt of all, that afterwards the two contending bodies will appear to have mutually deſtroyed one another, both the ſour ſpirit and the fixed ſalt having each loſt its former nature in the ſcuffle and degenerated with its adverſary into a certain third ſubſtance that wants ſeveral of the properties both of the ſour ſpirit and the alkali. Now to apply all this to the occaſion on which I mentioned it, how diſtant and contrary ſoever the more inquiſitive of the latter chemiſts take acid and fixed ſalts to be, yet I ſcarce doubted but that by our experiment I ſhould from acid ſalts obtain an alkali; and accordingly having, by caſting in ſeveral bits of well-kindled coal, excited in the melted maſs of our tranſmuted ſalt as many deflagrations as I could, and then giving it a pretty ſtrong fire to drive away the reſt of the more fugitive parts, I judged that the remaining maſs would be (like the fixed nitre I have elſewhere mentioned) of an alkalizate nature; and accordingly having taken it out, I found it to taſte, not like ſea-ſalt, but fiery enough upon the tongue, and to have a lixiviate reliſh. I found too that it would turn ſyrup of violets into a greeniſh colour, that it would precipitate a limpid ſolution of ſublimate made in fair water into an orange-tawny powder. I found that it would, like other fixed ſalts, produce an ebullition with acid ſpirits, and even with ſpirit of ſalt itſelf, and concoagulate with them. Nor are theſe themſelves all the ways I took to manifeſt the alkalizate nature of our tranſmuted ſea-ſalt.

I DID indeed conſider at firſt that it might be ſuſpected that this new alkalizateneſs might proceed from the aſhes of the injected coals, the aſhes of vegetables generally containing in them more or leſs of a fixed ſalt. But when I conſidered too that a pound of charcoal burned to aſhes is wont to yield ſo very little ſalt that the injected fragments of coal (though they had been, which they were not) quite burned out in this operation, would ſcarce have afforded two or three grains of ſalt (perhaps not half ſo much) I ſaw no reaſon at all to believe that in the whole maſs I had obtained, (and which was all that was left me of the ſea-ſalt I had firſt employed); it was nothing but ſo inconſiderable a proportion of aſhes that exhibited all the phænomena of an alkali.

AND for further confirmation both of this and what I ſaid a little before, I ſhall add, that to ſatisfy myſelf yet more, I poured upon a pretty quantity of this lixiviate ſalt a due proportion of aqua fortis till the hiſſing and ebullition ceaſed, and then leaving the fluid mixture for a good while to coagulate (which it did very ſlowly) I found it at length to ſhoot into ſaline cryſtals; which, though they were not of the figure of nitre, did yet, by their inflammability and their bigneſs, ſufficiently argue that there had been a conjunction made betwixt the nitrous ſpirit and a conſiderable proportion of alkali.

I CONSIDERED alſo that it might be ſuſpected that in our experiment it was the nitrous corpuſcles of the aqua fortis, that, lodging themſelves in the little rooms deſerted by the ſaline corpuſcles of the ſea-ſalt that paſſed over into the receiver, had afforded this alkali; as common ſalt-petre, being handled after ſuch a manner, would leave in the crucible a fixed or alkalizate ſalt. But to this, I anſwer, that as the ſea-ſalt which was not driven over by ſo mild a diſtillation and ſeemed much a greater part than that which had paſſed over, was far from being of an alkalizate nature; ſo the nitrous corpuſcles that are preſumed to have ſtaid behind were, whilſt they compoſed the ſpirit of nitre, of an highly volatile and acid nature, and conſequently of a nature directly oppoſite to that of alkalies. And if by the addition of any other substance that were no more alkalizate than sea-salt, an alkali could be obtained out of spirit of nitre of aqua fortis, the producibleness of an alkali out of bodies of another nature might be rightly thence inferred; so that however it appears, that by the intervention of our experiment two substances that were formerly acid are turned into one that is manifestly of an alkalizate nature; which is that we would here evince.

Perhaps it may, *Pyrophilus*, be worth while to subjoin, that to prosecute the experiment by inverting it, we drew two parts of strong spirit of salt from one of purified nitre, but did not observe the remaining body to be any thing near so considerably changed as the sea-salt, from which we had drawn the spirit of nitre; since though the spirit of salt that came over did (as we expected) bring over so many of the corpuscles of the nitre, that being heated, it would readily enough dissolve foliated gold; yet the salt that remained in the retort, being put upon quick coals, did flash away with a vehement and halituous flame, very like that of common nitre.

EXPERIMENT VI.

I come now, *Pyrophilus*, to an experiment, which, though in some things it be of kin to that which I have already taught you concerning the changing of sea-salt by aqua fortis, will yet afford us divers other instances to shew, how upon the change of texture in bodies there may arise divers new qualities; especially of that sort, which, because they are chiefly produced by chymistry, and are wont to be considered by chymists, if not by them only, may in some sense be called chymical.

The body, which, partly whilst we were preparing it, and partly when we had prepared it, afforded us these various phænomena, either is the same that *Glauberus* means by his *sal mirabilis*, or at least seems to be very like it; and whether it be the same or no, its various and uncommon properties make it very fit to have a place allowed it in this treatise; though of the many trials I made with it, I can at present find no more among my loose papers than that following part of it that I wrote some years ago to an ingenious friend, who I know will not be displeased, if, to save myself some time and the trouble of examining my memory, I annex the following transcript of it.

[To give you a more particular account of what I writ to you from *Oxford* of my trials about *Glauber*'s salt, though I dare not say that I have made the self-same thing which he calls his *sal mirabilis*, because he has described it so darkly and ambiguously, that it is not easy to know with any certainty what he means; yet whether or no I have not made salt, that, as far as I have yet tried it, agrees well enough with what he delivers of his, and therefore is like to prove either his *sal mirabilis*, or almost as good a one, I shall leave you to judge by this short narrative.

The strange things that the industrious *Glauber*'s writings have invited men to expect from his *sal mirabilis*, in case he be indeed possessed of such a thing, and the enquiries of divers eminent men who would fain learn of me what I thought of its reality and nature, invited me the next opportunity I got, to take into my hands his *Pars altera miraculi mundi*, whose title you know promises a description of this *sal artis mirificum*, as he is pleased to call it. But I confess I did not read it near all over, because a great part of it is but a transcription of several entire chapters out of *Paracelsus*, and I perceived that much of the rest did, according to the custom of chymical writings, more concern the author than the subjects: wherefore, looking upon his process of making his *sal mirabilis*, I soon perceived he had no mind to make it common, since he only bids us,

upon

upon two parts of common salt dissolved in water, to pour *A*, without telling us what that *A* is. Wherefore, reading on in the same process, and finding that he tells us that with *B* (which he likewise explains not at all, nor determines the quantity of it) one may make an aqua fortis, it presently called into my mind that some years before having had occasion to make many trials, mentioned in other tracts of mine, with oil of vitriol and salt-petre, I did, among other things, make a red spirit of nitre by the help only of oil of vitriol; remembering this, I say, I resorted to one of my *Carneades*'s dialogues *, and reviewing that experiment, as I have set it down, I concluded, that though I had not dissolved the salt-petre in water, as *Glauber* doth his common salt, yet, since on the other side I made use of external fire, it was probable I might this way also get a nitrous spirit, though not so strong. And though by calling the liquor that must make an aqua fortis *B*, whereas he had called that which is to make his spirit of salt and *sal mirabilis*, *A*, he seemed plainly to make them different things; yet relying on the experiment I had made, and putting to a solution of nitre as much of the oil of vitriol as I had taken last, though that be double the quantity he prescribes for the making of his *sal mirabilis*, I obtained out of a low glass body and head in sand an indifferent good *spiritus nitri* that even before rectification would readily enough dissolve silver, though it were diluted with as much of the common water wherein salt-petre had been dissolved, as amounted at least to double or treble the weight of the nitrous parts. The remaining matter being kept in the fire till it was dry, afforded us a salt easily reducible (by solution in fair water and coagulation) into crystalline grains, of a nature very differing both from crude nitre and from fixed nitre, and from oil of vitriol. For it coagulated into pretty big and well-shaped grains, which, you know, fixed nitre and other alkalizate salts are not wont to do; and these grains were not, like the crystals of salt-petre itself, long and hexaëdrical, but of another figure, not easy nor necessary to be here described.

Besides this vitriolate nitre (if I may so call it) would not easily, if at all, flow in the air as fixed nitre is wont to do: moreover it was easily enough fusible by heat, whereas fixed nitre doth usually exact a vehement fire for its fusion; and though crude salt-petre also melts easily, yet to satisfy you, how differing a substance this of ours was from that, we cast quick coals into the crucible, without being at all able to kindle it. Nay, and when for further trial we threw in some sulphur also, though it did flame away itself, yet it did not seem to kindle the salt that was hot enough to kindle it; much less did it flash, as sulphur is wont on such occasions to make salt-petre do. Add to all this, that a parcel of this white substance, being without brimstone made to flow for a while in a crucible with a bit of charcoal for it to work upon, grew manifestly and strongly scented of sulphur, and acquired an alkalizate taste, so that it seemed almost a coal of fire upon the tongue, if it were licked before it imbibed any of the air's moisture, and (which many perhaps will, though I do not, think stranger) obtained also a very red colour; which recalled to my mind that *Glauber* mentions such a change observable in his salt made of common salt, upon whose account he is pleased to call such a substance his *Carbunculus*.

Being invited by this success to try whether I could make his *sal mirabilis*, notwithstanding his intimating, as I lately told you, that it is done with a differing menstruum from that wherewith the salt-petre is to be wrought upon; I observed, that where he points at a way of making his salt in quantity without breaking the vessels, he prescribes that the materials be distilled in vessels of pure silver; whence I conjectured

* See the Sceptical Chemist.

that it was not aqua fortis or ſpirit of nitre that he employed to open his ſea-ſalt; and that conſequently, ſince common ſpirit of ſalt was too weak to effect ſo great a change as the experiment requires, it was very probable that he employed oil of ſulphur or of vitriol, which will ſcarce at all fret unalloyed ſilver. And however I concluded, that whatſoever the event ſhould prove, it could not but be worth the while to try what operation ſuch a menſtruum would have upon ſea-ſalt, as I was ſure had ſuch a notable one upon ſalt-petre. And I remember, that formerly making ſome experiments about the differing manners of diſſolution of the ſame concrete by ſeveral liquors, I found that oil of vitriol diſſolves ſea-ſalt in a very odd way (which you will find mentioned among my promiſcuous experiments): wherefore, pouring upon a ſolution of bay-ſalt, made in but a moderate proportion of water, oil of vitriol to the full weight of the dry ſalt, and abſtracting the liquor in a glaſs cucurbite placed in ſand, I obtained, without ſtreſs of fire, beſides phlegm, good ſtore of a liquor, which by the ſmell and taſte ſeemed to be ſpirit of ſalt. And to ſatisfy myſelf the better, mingling a little of it with ſome of the ſpirit of nitre lately mentioned, I found the mixture, even without the aſſiſtance of heat, to diſſolve crude gold. And having for further trial's ſake poured ſome of it upon ſpirit of fermented urine, till the affuſion ceaſed to produce any conflict, and having afterwards gently evaporated away the ſuperfluous moiſture, there did, as I expected, ſhoot in the remaining liquor a ſalt figured like combs and feathers, thereby diſcloſing itſelf to be much of the nature of ſal armoniack, ſuch as I elſewhere relate my having made, by mingling ſpirit of urine with ſpirit of common ſalt made the ordinary way.]

THIS, *Pyrophilus*, is all I can find at preſent of that account, of which I hoped to have found much more: but you will be the more unconcerned for my not adding divers other things that, I remember, I tried, as well before and after the writing the above tranſcribed paper (as particularly that I found the experiment ſometimes to ſucceed not ill, when I diſtilled the oil of vitriol and ſea-ſalt together, without the intervention of water, whereby much time was ſaved, and alſo when I employed oil of ſulphur, made with a glaſs bell, inſtead of oil of vitriol) if I inform you that afterwards I found that *Glauber* himſelf, in ſome of his ſubſequent pieces, had delivered more intelligibly the way of making what he, without altogether ſo great a brag as moſt think, calls his *ſal mirabilis* (which yet ſome very ingenious readers of his writings have come to us to teach them) and that thoſe experiments of his about it which I was able to make ſucceed (for ſome I was not, and ſome I did not think fit to try); you will find, together with thoſe of my own, in more proper places of other papers. Only, to apply what hath been above related to my preſent purpoſe, I muſt not here pretermit a couple of obſervations.

AND firſt, we may take notice of the power that mixtures, though they ſeem but very ſlight, and conſiſt of the ſmalleſt number of ingredients, may, if they make great changes of texture, have, in altering the nature and qualities of the compounding bodies. For in our (above-recited) caſe, though ſea-ſalt, being a body conſiderably fixed, requires a naked fire to be elevated even by the help of copious additaments of beaten bricks or clay, *&c.* to keep it from fuſion, yet the ſaline corpuſcles are diſtilled over in a moderate fire of ſand; whilſt the oil of vitriol, by whoſe intervention they acquire this volatility, though it be not (like the other) a groſs, or as the ſame chemiſt ſpeaks, corporeal ſalt, but a liquor that has been already diſtilled, is yet by the ſame operation ſo fixed as to ſtay behind not only in the retort, but, as I have ſometimes purpoſely tried, in much conſiderabler heats than that needs in this experiment be expoſed to. Nor only is the oil of vitriol made thus far fixed, but it is otherwiſe alſo no leſs changed: for

when

when the remaining falt has been expofed to a competent heat, that it may be very dry and white, to be fure of which I feveral times do, when the diftillation is ended, keep the remaining mafs (taken out of the retort and beaten) in a crucible among quick coals, you fhall have a confiderable quantity (perhaps near as much as the fea-falt you firft employed) of a fubftance which, though not infipid, has not at all the tafte of fea-falt, or any other pungent one, and much lefs the highly corrofive acidity of oil of vitriol.

And the mention of this fubftance leads me to the fecond particular I intended to take notice of, which is a phænomenon, to confirm what I formerly intimated, that notwithftanding the regular and exquifite figures of fome falts, they may, by the addition of other bodies, be brought to conftitute cryftals of very differing, and yet of curious fhapes. For if you diffolve the hitherto mentioned *caput mortuum* of fea-falt (after you have made it very dry, and freed it from all pungency of tafte) in a fufficient quantity of fair water, and having filtrated the folution, fuffer the diffolved body leifurely to coagulate, you will probably obtain, as I have often done, cryftals of a far greater tranfparency than the cubes, wherein fea-falt is wont to fhoot, and of a fhape far differing from theirs though oftentimes no lefs curious than that of thofe cubes; and, which makes mainly for my prefent purpofe, I have often obferved thofe finely-figured cryftals to differ as much in fhape from one another, as from the grains of common falt. And indeed I muft not on this occafion conceal from you, that whether it be to be imputed to the peculiar nature of fea-falt, or (which I judge more probable) to the great difparities to be met with in liquors, that do all of them pafs for oil of vitriol, whether (I fay) it be to this or to fome other caufe that the effect is to be imputed, I have found my attempts to make the beft fort of *fal mirabilis* fubject to fo much uncertainty, that though I have divers times fucceeded in them, I have found fo little uniformity in the fuccefs, as made me reckon this experiment amongft contingent ones, and almoft weary of meddling with it.

EXPERIMENT VII.*

I remember, *Pyrophilus*, I once made an experiment, which if I had had the opportunity to repeat, and had done fo with the like fuccefs, I fhould be tempted to look upon it, though not as a luciferous experiment (for it is the quite contrary) yet as fo luciferous a one, as, how much foever it may ferve to recommend chemiftry itfelf, may no lefs difpleafe envious chemifts, who will be troubled, both that one who admits not their principles, fhould devife fuch a thing, and that, having found it, he fhould not (chemift like) keep it fecret.

But to give you a plain and naked account of this matter, that you may be able the better to judge of it, and, if you pleafe, to repeat it, I will freely tell you, that fuppofing all metals, as well as other bodies, to be made of one catholick matter common to them all, and to differ but in fhape, fize, motion, or reft, and texture of the fmall

* Though this VIIth experiment, being confiderable and very pertinent, the author thought fit to mention it, fuch as it is here delivered, when he writ it but to a private friend; yet, after he was induced to publifh thefe papers, it was the (now raging) plague, which drove him from the accommodation requifite to his purpofe, that fruftrated the defign he had of firft repeating that part of the experiment which treats of the deftruction of gold: for as for that part which teaches the volatilization of it, he had tried that often enough before.

parts

parts they confift of, from which affections of matter the qualities, that difference particular bodies, refult. I could not fee any impoffibility in the nature of the thing, that one kind of metal fhould be tranfmuted into another (that being in effect no more than that one parcel of the univerfal matter, wherein all bodies agree, may have a texture produced in it like the texture of fome other parcel of the matter common to them both).

And having firft fuppofed this, I further confidered that in a certain menftruum, which, according to vulgar chemifts doctrine, muft be a worthlefs liquor, according to my apprehenfion there muft be an extraordinary efficacy in reference to gold, not only to diffolve and otherwife alter it, but to injure the very texture of that fuppofedly immutable metal.

The menftruum then I chofe to try, whether it could not diffolve gold with, is made by pouring on the rectified oil of the butter of antimony as much ftrong fpirit of nitre, as would ferve to precipitate out of it all the *bezoarticum minerale*; and then with a good fmart fire diftilling off all the liquor that would come over, and (if need be) cohobating it upon the antimonial powder. For though divers chemifts that make this liquor throw it away, upon prefumption that, becaufe of the ebullition that is made by the affufion of the fpirit to the oil and the confequent precipitation of a copious powder, the liquors have mutually deftroyed or difarmed each other; yet my notions and experience of the nature of fome fuch mixtures invite me to prize this, and give it the name of *menftruum peracutum*.

Having then provided a fufficient quantity of this liquor (for I have obferved that gold ordinarily requires a more copious folvent than filver) we took a quantity of the beft gold we could get, and melted it with three or four times its weight of copper, which metal we chufe rather than that which is more ufual among refiners, filver, that there may be the lefs fufpicion that there remained any filver with the gold after their feparation: this mixture we put into good aqua fortis or fpirit of nitre, that all the copper being diffolved, the gold might be left pure and finely powdered at the bottom; this operation with aqua fortis being accounted the beft way of refining gold that is yet known, and not fubject, like lead, to leave any filver with it, fince the aqua fortis takes up that metal. And for greater fecurity we gave the powder to an ancient chemift to boil fome more of the menftruum upon it, without communicating to him our defign. This highly refined gold being by a competent degree of heat brought, as is ufual, to its native colour and luftre, we put to it a large proportion of the *menftruum peracutum* (to which we have fometimes found caufe to add a little fpirit of falt to promote the folution) wherein it diffolves flowly and quietly enough; and there remained at the bottom of the glafs a pretty quantity (in fhew, though not in weight) of white powder that the menftruum would not touch; and, if I much mifremember not, we found it as indiffolvable in aqua regis too. The folution of gold being abftracted, and the gold again reduced into a body, did, upon a fecond folution, yield more of the white powder, but not (if I remember aright) fo much as at the firft: now having fome little quantity of this powder, it was eafy, with borax or fome other convenient flux, to melt it down into a metal, which metal we found to be white like filver, and yielding to the hammer, if not to a lefs preffure, and fome of it being diffolved in aqua fortis or fpirit of nitre, did, by the odious bitternefs it produced, fufficiently confirm us in our expectation to find it true filver.

I doubt not but you will demand, *Pyrophilus*, why I did not make other trials with this factitious metal, to fee in how many other qualities I could verify it to be filver; but the quantity I recovered after fufion was fo fmall, fome of it perhaps being left either

either in the flux or in the crucible, that I had not wherewithal to make many trials; and being well enough ſatisfied by the viſible properties and the taſte peculiar to ſilver, both that it was a metal, and rather ſilver than any other, I was willing to keep the reſt of it for a while, as a rarity, before I made further trials with it; but was ſo unfortunate as with it to loſe it in a little ſilver box, where I had ſomething of more value, and poſſibly of more curioſity.

You will alſo aſk why I repeated not the experiment? To which I ſhall anſwer, that, beſides that one may eaſily enough fail in making the menſtruum fit for my purpoſe, I did, when I had another opportunity (for I was long without it) make a ſecond attempt; and having, according to the above mentioned method, brought it ſo far that there remained nothing but the melting of the white powder into ſilver, when having waſhed it, I had laid it upon a piece of white paper by the fire's ſide to dry, being ſuddenly called out of my chamber, an ignorant maid, that in the mean time came to dreſs it up, unluckily ſwept this paper, as a foul one, into the fire; which diſcouragement, together with a multiplicity of occaſions, have made me ſuſpend the purſuit of this experiment till another opportunity. But in the mean time I was confirmed in ſome part of my conjecture by theſe things.

The firſt, by finding, that with ſome other menſtruums, which I tried, and even with good aqua regis itſelf, I could obtain from the very beſt gold I diſſolved in them ſome little quantity of ſuch a white powder as I was ſpeaking of; but in ſo very ſmall a proportion to the diſſolved gold, that I had never enough of it at once to think it worth proſecuting trials with.

The other was this, that a very experienced mineraliſt whom I had acquainted with part of what I had done, aſſured me that an eminently learned and judicious perſon that he named to me, had, by diſſolving gold in a certain kind of aqua regis, and after by reduction of it into a body, re-diſſolving it again, and repeating this operation very often, reduced a very great if not much the greater part of an ounce of gold into ſuch a white powder.

And the third thing that confirmed me was the proof given me by ſome trials, that I purpoſely made, that the *menſtruum peracutum* I employed had a notable operation upon gold, and would perform ſome things (one of which we ſhall by and by mention) which judicious men, that play the great criticks in chemiſtry, do not think feaſible; ſo that there ſeems no greater cauſe to doubt, that the above-mentioned ſilver was really obtained out of the pure gold, than only this, that men have hitherto ſo often in vain attempted to make a real tranſmutation of metals (for the better or for the worſe) and to deſtroy the moſt fixed and compacted body of gold, that the one is looked upon as an unpracticable thing, and the other as an indeſtructible metal.

To reflect then a little upon what we have been relating, if we did not miſtake nor impoſe upon ourſelves (I ſay upon ourſelves, the project being our own, and purſued without acquainting any body with our aim) it may afford us very conſiderable conſequences of great moment.

And in the firſt place, it ſeems probably reducible from hence, that however the chemiſts are wont to talk irrationally enough of what they call *tinctura auri* and *anima auri*; yet, in a ſober ſenſe, ſome ſuch thing may be admitted: I ſay, ſome ſuch thing, becauſe as on the one hand I would not countenance their wild fancies about theſe matters, ſome of them being as unintelligible as the Peripatetics ſubſtantial forms; ſo, on the other hand, I would not readily deny but that there may be ſome more noble and ſubtile corpuſcles, that being duly conjoined with the reſt of the matter whereof gold conſiſts, may qualify that matter to look yellow, to reſiſt aqua fortis, and to exhibit thoſe other

other peculiar phænomena that diſcriminate gold from ſilver ; and yet theſe noble parts may either have their texture deſtroyed by a very piercing menſtruum or by a greater congruity with its corpuſcles than with thoſe of the remaining part of the gold ; may ſtick more cloſe to the former, and by their means be extricated and drawn away from the latter. As when (to explain my meaning by a groſs example) the corpuſcles of ſulphur and mercury do, by a ſtrict coalition, aſſociate themſelves into the body we call vermilion, though theſe will riſe together in ſublimatory veſſels, without being divorced by the fire, and will act in many caſes as one phyſical body ; yet it is known enough among chemiſts that if you exquiſitely mix with it a due proportion of ſalt of tartar, the parts of the alkali will aſſociate themſelves more ſtrictly with thoſe of the ſulphur than theſe were before aſſociated with thoſe of the mercury ; whereby you ſhall obtain out of the cinnabar, which ſeemed intenſely red, a real mercury that will look like fluid ſilver. And this example prompts me to mind you, *Pyrophilus*, that, at the beginning of this paragraph I ſaid no more than that the conſequence I have been deducing might probably be inferred from the premiſes. For as it is not abſurd to think that our menſtruum may have a particular operation upon ſome noble and (if I may ſo call them) ſome tinging parts of the gold, ſo it is not impoſſible but that the yellowiſhneſs of that rich metal may proceed not from any particular corpuſcles of that colour, but from the texture of the metal ; as in our lately-mentioned example, the cinnabar was highly red, though the mercury it conſiſted of were ſilver-coloured, and the ſulphur but a pale yellow ; and conſequently the whiteneſs and other changes produced in the new metal we obtained, may be attributed, not to the extraction of any tinging particles, but to a change of texture, whereon the colour as well as other properties of the gold did depend. But that which made me unwilling to reject the way I firſt propoſed of explicating this change of colour, was, that a mineraliſt of great veracity hath ſeveral times aſſured me that a known perſon in the relator's country, the *Netherlands*, got a great deal of money by the way of extracting a blue tincture out of copper, ſo as to leave the body white; adding, that he himſelf, having procured from a friend (to ſatisfy his curioſity) a little of the menſtruum (whoſe chief ingredients his friend communicated to him and he to me) he did, as he was directed, diſſolve copper in common aqua fortis, to reduce it into ſmall parts; and then having kept the calx of the powder of this copper for ſome hours in this menſtruum, he perceived that the clear liquor, which was weak in taſte, did not diſſolve the body of the metal, but only extract a blue tincture, leaving behind a very white powder, which he quickly reduced by fuſion into a metal of the ſame colour, which he found as malleable as before ; which I the leſs wonder at, becauſe the experienced chemiſt *Johannes Agricola*, in his Dutch annotations upon *Poppius*, mentions the making of a white and malleable copper in good quantities upon his own knowledge; and that of ſuch a kind of copper I have with pleaſure made trial, I elſewhere relate. But of theſe matters we may poſſibly ſay more in a convenient place.

THE ſecond thing that ſeems deducible from our former narrative is, that however moſt (for I ſay not all) of the judicioueſt among the chemiſts themſelves, as well as among their adverſaries, believe gold too fixed and permanent a body to be changeable by art, inſomuch that it is a received axiom amongſt many eminent Spagyriſts, that *facilius eſt aurum conſtruere, quàm deſtruere* ; yet gold itſelf is not abſolutely indeſtructible by art, ſince gold being acknowledged to be an homogeneous metal, a part of it was, by our experiment, really changed into a body that was either true ſilver, or at leaſt a new kind of metal very different from gold. And ſince it is generally confeſſed, that among all the bodies we are allowed to obſerve near enough, and to try our

our skill upon, there is not any whose form is more strictly united to its matter than that of gold; and since also the operation, by which the white powder was produced, was only made by a corrosive liquor, without violence of fire; it seems at least a very probable inference that there is not any body of so constant and durable a nature, but that, notwithstanding its persisting inviolated in the midst of divers sensible disguises, its texture, and consequently its nature, may be really destroyed, in case this more powerful and appropriated agent be brought by a due manner of application to work upon the body, whose texture is to be destroyed.

But this matter we elsewhere handle, and therefore shall now proceed to the last and chief consectaries of our experiment.

Thirdly then, it seems deducible from what we have delivered, that there may be a real transmutation of one metal into another, even among the perfectest and noblest metals, and that effected by factitious agents in a short time, and, if I may so speak, after a mechanical manner. I speak not here of projection, whereby one part of an aurifick powder is said to turn I know not how many hundred or thousand parts of an ignobler metal into silver or gold, not only because, though projection includes transmutation, yet transmutation is not all one with projection, but far easier than it; but chiefly because it is not in this discourse you are to expect what I can say, and do think, concerning what men call the philosophers stone. To restrain myself then to the experiment we are considering, that seems to teach us that at least amongst inanimate bodies the noblest and constantest sort of forms are but peculiar contrivances of the matter, and may by agents, that work but mechanically, that is, by locally moving the parts and changing their sizes, shape, or texture, be generated and destroyed; since we see that in the same parcel of metalline matter which a little before was true and pure gold, by having some few of its parts withdrawn and the rest transposed or otherwise altered in their structure (for there appears no token that the menstruum added any thing to the matter of the produced silver) or by both these ways together, the form of gold, of that peculiar modification which made it yellow, indissoluble in aqua fortis, *&c.* is abolished; and from the new texture of the same matter there arises that new form or convention of accidents, from which we call a metal silver. And since ours was not only dissoluble in aqua fortis, but exhibited that excessively bitter taste which is peculiar to silver, there seems no necessity to think that there needs a distinct agent, or a peculiar action of a substantial form to produce in a natural body the most peculiar and discriminating properties. For it was but the same menstruum, devoid of bitterness, that by destroying the texture of gold changed it into another, upon whose account it acquired at once both whiteness in colour, dissolubleness in aqua fortis, and aptness to compose a bitter body with it, and I know not how many other new qualities are attributed.

I know it is obvious to object that it is no very thrifty way of transmutation, instead of exalting silver to the condition of gold, to degrade gold to the condition of silver. But a transmutation is nevertheless more or less real for being or not being lucriferous; and since that may inrich a brain that may impoverish a purse, I must look upon your humour as that of an alchymist, rather than of a philosopher, if I durst not expect that the instructiveness in such an experiment will suffice to recommend it to you And if I could have satisfied myself that good authors are not mistaken about what they affirm of the transmutation of iron into copper, though, the charge and pains considered, it be a matter of no gain, yet I should have thought it an experiment of great worth, as well as the transmutation of silver into gold. For it is no small matter to remove the bounds that nature seems very industriously to have set to the alterations of bodies;

especially

especially among those durable and almost immortal kinds, in whose constancy to their first forms nature seems to have designed the shewing herself invincible by art.

I should here, *Pyrophilus*, conclude what I have to say of the experiment that hath already so long entertained us, by recommending to you the repetition of what I had not the opportunity to try above once from end to end, were it not that I remember something I said about the *menstruum peracutum*, may seem to import a promise of communicating to you something of the efficacy of that liquor upon gold. And therefore partly for that reason, and partly to make sure that the present discourse shall not be uninstructive to you, I would add, that though not only the generality of refiners and mineralists, but divers of the most judicious cultivators of chymistry itself, hold gold to be so fixed a body, that it can as little be volatilized as destroyed, and that upon this ground, that the processes of subliming or distilling gold to be met with in divers chymical books are either mystical, or unpracticable, or fallacious (in which opinion I think them not much mistaken); though this, I say, be the persuasion even of some critical chymists, yet, upon the just expectation I had to find my menstruum very operative upon gold, I attempted and found a way to elevate it to a considerable height, by a far less proportion of additament, than one that were not fully persuaded of the possibility of elevating gold would imagine; and though I have indeed found by two or three several liquors (especially the *aqua pugilum*, enigmatically described by *Basilius*) that the fixedness of gold is not altogether invincible, yet I found the effect of these much inferior to that of our mixture; touching which I shall relate to you the easiest and shortest, though not perhaps the very best, manner of imploying it.

We take then the finest gold we can procure, and having either granulated it or laminated it, we dissolve it in a moderate heat with a sufficient quantity of the *menstruum peracutum*, and having carefully decanted the solution into a conveniently sized retort, we very gently in a sand-furnace distil off the menstruum; and if we have a mind to elevate the more gold, we either pour back upon the remaining substance the same menstruum, or, which is better, re-dissolve it with fresh. The liquor being abstracted, we urge the remaining matter by degrees of fire, and in no stronger a one than what may easily be given in a sand furnace, a considerable quantity of the gold will be elevated to the upper part of the retort, and either fall down in a golden-coloured liquor into the receiver, or, which is more usual, fasten itself to the top and neck in the form of a yellow and reddish sublimate; and sometimes we have had the neck of the retort inriched with good store of large thin crystals, not yellow but red, and most like rubies, very glorious to behold (though even these being taken out and suffered to lie a due time in the open air, would lose their saline form, and run *per deliquium* into a liquor). Nor see I any cause to doubt but that by the re-affusions of a fresh menstruum upon the dry calx of gold that stays behind, the whole body of the metal may be easily enough made to pass through the retort, though for a certain reason I forbore to prosecute the experiment so far.

But here, *Pyrophilus*, I think myself obliged to interpose a caution, as well as to give you a further information about our present experiment. For first I must tell you, that though even learned chymists think it a sufficient proof of a true tincture, that not only the colour of the concrete will not be separated by distillation, but the extracted liquor will pass over tincted into the receiver; yet this supposition, though it be not unworthy of able men, may in some cases deceive them. And next I must tell you, that whereas I scruple not in several writings of mine to teach that the particles of solid and consistent bodies are not always unfit to help to make up fluid ones, I shall now venture to say further, that even a liquor made by distillation, how volatile soever such liquors may be

2 thought,

thought, may in part conſiſt of corpuſcles of the moſt compact and ponderous bodies in the world.

Now to manifeſt both theſe things, and to ſhew you withal the truth of what I elſewhere teach, *That ſome bodies are of ſo durable a texture, that their minute parts will retain their own nature, notwithſtanding variety of diſguiſes, which may impoſe not only upon other men, but upon chymiſts themſelves*; I will add, that to proſecute the experiment, I dropped into the yellow liquor afforded me by the elevated gold a convenient quantity of clean running mercury, which was immediately coloured with a golden coloured film, and ſhaking it to and fro, till the menſtruum would gild no more, when I ſuppoſed the gold to be all precipitated upon the mercury, I decanted the clarified liquor, and mixing the remaining amalgam (if I may ſo call it) of gold and mercury with ſeveral times its weight of borax, I did as I expected; by melting them in a ſmall crucible, I eaſily recovered the ſcattered particles of the elevated metal, reduced into one little maſs or bead of corporal or yellow (though perhaps ſomewhat paliſh) gold. But yet whether the gold that tinged the menſtruum might not, before the metal was reduced or precipitated out of it, have been more ſucceſsfully applied to ſome conſiderable purpoſes than a bare ſolution of gold that hath never been elevated, may be a queſtion which I muſt not in this place determine, and ſome other things that I have tried about our elevated gold, I have elſewhere taken notice of; only this further uſe I ſhall here make of this experiment, that whereas I ſpeak in other papers as if there may be a volatile gold in ſome ores and other minerals, where the mine-men do not find any thing of that metal, I mention ſuch a thing upon the account of the paſt experiment and ſome analogies. And therefore as I would not be underſtood to adopt what every chymical writer is pleaſed to fancy concerning volatile gold; ſo I think judicious men that are not ſo well acquainted with chymical operations, are ſometimes too forward to condemn the chymiſts obſervations; not becauſe their opinions have nothing of truth, but becauſe they have had the ill luck not to be warily enough propoſed. And to give an inſtance in the opinion that ſome minerals have a volatile gold (and the like may be ſaid of ſilver) I think I may give an account rational enough of my admitting ſuch a thing, by explicating it thus: that as in our experiment, though after the almoſt total abſtraction of the menſtruum, the remaining body being true gold, and conſequently in its own nature fixed, yet it is ſo ſtrictly aſſociated with ſome volatile ſaline particles, that theſe being preſſed by the fire, carry up along with them the corpuſcles of the gold, which may be reduced into a maſs by the admiſtion of borax, or ſome other body fitted to divorce the corpuſcles of the metal from thoſe that would elevate them, and to unite them into grains, too big and ponderous to be ſublimed: ſo in ſome mineral bodies there may be pretty ſtore of corpuſcles of gold ſo minute, and ſo blended with the unfixed particles, that they will be carried up together with them by ſo vehement a heat, as is wont to be imployed to bring ores, and even metalline maſſes to fuſion. And yet it is not impoſſible but that theſe corpuſcles of gold, that in ordinary fuſions fly away, may be detained and recovered by ſome ſuch proper additament, as may either work upon and (to uſe a chymical term) mortify the other parts of the maſs, without doing ſo upon the gold; or, by aſſociating with the volatile and ignobler minerals ſome way or other, diſable them to carry away the gold with them, as they otherwiſe may do; or by its fixedneſs and cognation of nature make the diſperſed gold imbody with it. On which occaſion I remember that a very ingenious man deſiring my thoughts upon an experiment which he, and ſome others that were preſent at it, looked upon as very ſtrange; namely, that ſome good gold having for a certain trial been coupled with a great deal of lead, inſtead of being advanced in colour as in goodneſs, was grown manifeſtly paler than before; my conjecture being, that ſo great a proportion of lead

 might

might contain divers particles of volatile silver, which meeting with the fixed body of the gold by incorporating therewith was detained, was much confirmed by finding upon inquiry that the gold, instead of losing its weight, had it consideraby increased; which did much better answer my guess, than it did their expectation that made the experiment, and were much surprised at the event. But this is no fit place to prosecute the consideration of the additaments that may be used to unite and fix the particles of the nobler metals blended with volatile bodies; though perhaps what hath been said may afford some hint about the matter, as well as some apology for the chymical term, volatile gold: the possibility of which, I presume, we have evinced by the latter part of this experiment (in which I am sorry I cannot remember the proportion of the remaining salts that were able to elevate the gold) for that I have several times made, and therefore dare much more confidently rely on it, than I can press you to do on the former part (about the transmutation, or at least destruction of gold) till you or I shall have opportunity to repeat that trial.

EXPERIMENT VIII.

THOUGH, *Pyrophilus*, the experiment I am about to subjoin may at the first glance seem only to concern the *production of tastes*, and be indeed one of the principal that I devised concerning that subject, and that belongs to the notes I have made about those qualities, yet, if you do not of yourself take notice of it, I may hereafter have occasion to shew you, that there are some particulars in this experiment that are applicable to more than tastes. And since I had once thoughts (however since discouraged by the difficulties of the attempt) to make my notes extend even to divers qualities, which the operations of chymists and the practice of physicians have made men take notice of (such as the powers of corroding, precipitating, fixing, purging, blistering, stupefying, *&c.*) I presume you will not dislike that one, who had thoughts to say something even of chymical and medical qualities, if I may so call them, should give you here an experiment or two about more obvious, though particular affections of bodies, when there are several things in the experiment that may be of a general import to the doctrine of the origin of qualities and forms.

WE took then an ounce of refined silver, and having dissolved it in aqua fortis, we suffered it to shoot into crystals, which being dried, we found to exceed the weight of the silver by several drachms, which accrued upon the coagulation of the acid salts that had dissolved and were united to the metal. These crystals we put into a retort, and distilled them in sand, with almost as great a heat as we could give in a hammered iron-furnace, wherein the operation was made; but there came over only a very little sourish phlegm with an ill scent: wherefore the same retort being suffered to cool, and then coated, it was removed to another furnace, capable of giving a far higher degree of heat, namely, that of a naked fire, and in this furnace the distillation was pursued by the several degrees of heat, till at length the retort came to be red-hot, and kept so for a good while: but though even by this operation there was very little driven over, yet that sufficiently manifested what we aimed at shewing, namely, that a body extremely bitter might afford, as well as it consisted of, good store of parts that are not at all bitter, but (which is a very differing taste) eminently sour. For our receiver being taken off even when it was cold, the contained spirit smoked out like rectified aqua fortis, and not only smelt and tasted like aqua fortis, to the annoyance of the nose and tongue, but being poured upon filings of crude copper, it fell immediately to corrode them with violence, making

making much hiſſing, and ſending up thick fumes, and in a trice produced with the corroded copper a bluiſh colour, like that which that metal is wont to give in good aqua fortis.

Afterwards we took minium and aqua fortis, and made a ſolution, which being filtred and evaporated, left us a *ſaccharum Saturni* much like the common, made with ſpirit of vinegar; then taking this ſweet vitriol of lead (as we elſewhere call it) we endeavoured in the formerly mentioned ſand-furnace to drive it over in a retort; but finding that degree of fire incompetent to force over any thing, ſave a little phlegmatick liquor, we cauſed the retort to be coated and transferred to the other furnace, where being urged with a naked fire, it afforded at length a ſpirit ſomewhat more copious than the ſilver had done. This ſpirit ſmoked in the cold receiver as the other had, and did, like it, rankly ſmell of aqua fortis, and was ſo far from retaining any of the ſweetneſs of the concrete that had yielded it, that it was offenſively acid, and being poured upon minium, it did with noiſe and bubbles fall upon it, and quickly afforded us a liquor, which being filtred did, by its ſweetneſs, as well as other proofs, aſſure us, that there would have needed but a gentle evaporation (if we had leiſure to make it) to obtain from it a true ſugar of lead. And it is remarkable that the concrete, which appeared white before diſtillation, remained, for the moſt part, behind in the retort in the form of a *caput mortuum* (ſometimes we have had it in a yellowiſh lump) which was neither at all ſweet, as the vitriol of lead itſelf had eminently been, nor at all ſour, as the liquor diſtilled from it was in a high degree; but ſeemed rather inſipid, and was indeed but a calx of lead, which the heat of the fire had in part reduced into true and manifeſt lead in the retort itſelf, as appeared by many grains of ſeveral ſizes that we met with in the *caput mortuum* (the reſt of which is eaſily enough reducible by fuſion, with a convenient flux, into malleable lead).

There are ſome phænomena of this experiment, that we may elſewhere have occaſion to take notice of, as particularly, that notwithſtanding ſilver be a body ſo fixed in the fire, that it will (as it is generally known) endure the cupel itſelf, and though in the dried cryſtals of ſilver, the ſalt, that adheres to the ſilver, increaſes the weight of the metal but about a fourth or a third part; yet this ſmall proportion of ſaline corpuſcles was able to carry up ſo much of that almoſt fixedeſt of bodies, that more than once we have had the inſide of the retort, to a great height, ſo covered over with the metalline corpuſcles, that the glaſs ſeemed to be ſilvered over, and could hardly, by long ſcraping, be freed from the copious and cloſely adhering ſublimate.

But the phænomenon that I chiefly deſire to take notice of at preſent is this, that not only aqua fortis, being concoagulated with the differing bodies, may produce very differing concretes, but the ſame numerical ſaline corpuſcles, that, being aſſociated with thoſe of one metal, had already produced a body eminent in one taſte, may afterwards, being freed from that body, compoſe a liquor eminent for a very differing taſte; and after that too, being combined with the particles of another metal, would with them conſtitute a body of a very eminent taſte, as oppoſite as any one can be to both the other taſtes; and yet theſe ſaline corpuſcles, if, inſtead of this ſecond metal, they ſhould be aſſociated with ſuch a one as that they are driven from, would therewith exhibit again the firſt of the three mentioned taſtes. To prove all this, we took cryſtals of refined ſilver made with aqua fortis, and though theſe cryſtals be, as we often note, ſuperlatively bitter, yet having by a naked fire extorted from them what ſpirit we could, and found that, as we expected, extremely acid, we put one part of it upon a few filings of ſilver, of which it readily made a ſolution more bitter than gall, and the other

other part of the diftilled liquor we poured upon minium. And though whilft it had been an ingredient of the cryftals of filver committed to diftillation, it did with that metal compofe an exceffively bitter fubftance, yet the fame particles being loofened from that metal, and affociated with thofe of the lead, did with them conftitute a folution, which by evaporation afforded us a *faccharum Saturni*, or a vitriol fweet as fugar. And for further confirmation, we varied the experiment, having in a naked fire diftilled fome dried *faccharum Saturni* made with aqua fortis, the little liquor that came over, in proportion to the body that afforded it, was fo ftrong a fpirit of nitre, that for feveral hours the receiver was filled with the red fumes; and though the fmoking liquor were hugely fharp, yet part of it being poured upon a piece of its own *caput mortuum* (in which we perceived not any tafte) did at length (for it wrought but very flowly) exhibit fome little grains of a faccharine vitriol; but the other part being put upon filings of filver fell upon it immediately with noife and ftore of fmoke, and a while after concoagulated with part of it (which it had diffolved) into a falt exceffively bitter.

EXPERIMENT IX.

The artificial tranfmutation of bodies being, as the rareft and difficulteft production, fo one of the nobleft and ufefulleft effects of human fkill and power, not only the clear inftances of it are to be diligently fought for and prized, but even the probabilities of effecting fuch an extraordinary change of bodies are not to be neglected; efpecially if the verfion hoped for be to be made betwixt bodies of primordial textures (if I may fo call them) and fuch bodies as by the greatnefs of their bulk, and by their being to be found in moft of the mixed bodies here below, make a confiderable part of thofe that we men have the moft immediately to do with. Invited by thefe confiderations, *Pyrophilus*, I fhall venture to give you the account of fome obfervations and trials about the tranfmuting of water into earth, though it be not fo perfect as I wifh, and as I hope by God's bleffing to make it.

THE firft occafion afforded me to do any thing about this matter was my being confulted by a gentleman (an antient chymift, but not at all a philofopher) who relating to me how much he had (with the wonted fuccefs of fuch attempts) laboured after the grand *Arcana*, complained to me, among other things, that, having occafion to imploy great quantity of putrified rain-water, he obtained from it much lefs than he wifhed of the fubftance that he looked for, but a great deal of a certain whitifh excrementitious matter, which he knew not what to make of. This gave me the curiofity firft to defire a fight of it, in cafe he had not thrown it away (which by good fortune he had not) and then, taking notice of the unexpected plenty, and fome of the qualities of it, to afk him fome queftions which were requifite and fufficient to perfuade me, that this refidence came not from accidental foulnefs of the water, nor of the veffels it was received in. This I afterwards often thought of, and indeed it might juftly enough awaken fome fufpicions, that the little motes that have been fometimes obferved to appear numerous enough in pure rain-water, whilft it is diftilling, might not be merely accidental, but really produced, as well as exhibited by the action of the fire. I thought it then worth while to profecute this matter a little farther; and having put a pretty quantity of diftilled rain-water in a clean glafs body, and fitted it with a head and a receiver, I fuffered it to ftand in a digeftive furnace, till by the gentle heat thereof the water was totally abftracted, and the veffel left dry; which being taken out of the fand, I found the bottom of the glafs covered over with a white (but not fo very white) fubftance, which being fcraped off with

with a knife, appeared to be a fine earth, in which I perceived no manifeſt taſte, and which, in a word, by ſeveral qualities ſeemed to be earth.

THIS encouraged me to re-diſtil the rain-water in the ſame glaſs body, whoſe bottom, when the water was all drawn off, afforded me more of the like earth: but though the repetition of the experiment, and my having, for greater caution, tried it all the while in a new glaſs, that had not been imployed before to other uſes, confirmed me much in my conjecture, that unleſs it could be proved, which I think will ſcarce be pretended, that ſo inſipid a liquor as rain-water ſhould, in ſo gentle a heat, diſſolve the moſt cloſe and almoſt indeſtructible body of glaſs itſelf (which ſuch corroſive menſtruums as aqua fortis and aqua regis are wont to leave unharmed) the earthy powder I obtained from already diſtilled rain-water, might be a tranſmutation of ſome parts of the water into that ſubſtance; yet having unhappily loſt part of my powder, and conſumed almoſt all the reſt (for I kept a little by me, which you may yet ſee) I ſhould, till I had more frequently reiterated my experiments (which then I had not opportunity to do, though I had thoughts of doing it alſo with ſnow-water that I had put into chymical glaſſes for that purpoſe, and with liquor of melted hail, which I had likewiſe provided) and thereby alſo obtained ſome more of this virgin earth (as divers chymiſts would call it) to make farther trials with, have retained greater ſuſpicions, if I had not afterwards accidentally fallen into diſcourſe of this matter with a learned phyſician, who had dealt much in rain-water; but he much confirmed me in my conjecture, by aſſuring me that he had frequently found ſuch a white earth as I mentioned in diſtilled rain-water, after he had diſtilled the ſame numerical liquor (carefully gathered at firſt) I know not how many times one after another; adding, that he did not find (any more than I had done) any cauſe to ſuſpect, that if he had continued to re-diſtil the ſame portion of water, it would have yielded him more earth.

BUT the oddneſs of the experiment ſtill keeping me in ſuſpenſe, it was not without much delight, that afterwards mentioning it to a very ingenious perſon, whom without his leave, I think not fit to name, well verſed in chymical matters, and whom I ſuſpected to have, in order to ſome medicines, long wrought upon rain-water, he readily gave me ſuch an account of his proceedings, as ſeemed to leave little ſcruple about the tranſmutation we have been mentioning: for he ſolemnly affirmed to me, that having obſerved, as I had done, that rain-water would, even after a diſtillation or two, afford a terreſtrial ſubſtance, which may ſometimes be ſeen ſwimming up and down in the limpid liquor, he had the curioſity, being ſettled and at leiſure, to try how long he could obtain this ſubſtance from the water. And accordingly having freed rain-water, carefully collected from its accidental, and as it were feculent earthineſs, which it will depoſite at the firſt ſlow diſtillation (and which is oftentimes coloured) whereby it may be diſtinguiſhed from the white earth made by tranſmutation) he re-diſtilled it in very clean glaſſes, not only eight or ten times, but near two hundred, without finding that his liquor grew weary of affording him the white earth, but rather that the corpuſcles of it did appear far more numerous, or at leaſt more conſpicuous in the latter diſtillation than in the former. And when I expreſſed my curioſity to ſee this earth, he readily ſhewed me a pretty quantity of it, and preſented me with ſome, which comparing with what I had remaining of mine, I found to be exceeding like it, ſave that it was more purely white, as having been for the main afforded by rain-water that had been more frequently rectified. And to compare this welcome powder with that I made myſelf, I tried with this divers things, which I had before tried with my own and (becauſe the quantity preſented me was leſs inconſiderable) ſome others too. For I obſerved in this new powder, as I had done with my own, that being put into an excellent microſcope, and placed where the ſun-beams might

might fall upon it, it appeared a white meal, or a heap of corpuſcles ſo exceeding, not to ſay unimaginably ſmall, that in two or three choice microſcopes both I and others had occaſion to admire it: and their extreme littleneſs was much more ſenſibly diſcerned by mingling ſome few grains of ſand amongſt them, which made a mixture that looked like that of pebble ſtones, and of the fineſt flower. For our earth, even in the microſcope, appeared to conſiſt of as ſmall particles, as the fineſt hair-powder to the naked eye. Nor could we diſcern this duſt to be tranſparent, though, when the ſun ſhined upon it, it appeared in the microſcrope to have ſome particles a little gliſtering, which yet appearing but in a glaring light, we were not ſure to be no *deceptio viſus*.

2. I FOUND, that our white powder being caſt into water, would indeed for a while diſcolour it by ſomewhat whitening it, which is no more than ſpaud will do, and the fine duſt of white marble and other ſtones, whoſe corpuſcles, by reaſon of their minuteneſs, ſwim eaſily for a while in the water; but when it was once ſettled at the bottom, it continued there undiſſolved (for aught I could perceive) for ſome days and nights, as earth would have done.

3. HAVING weighed a quantity of it, and put it into a new clean crucible, with another inverted over it for a cover, I placed it among quick coals, and there kept the crucible red-hot for a pretty while, cauſing the fire afterward to be acuated with a blaſt of a bellows; but taking out the powder, I neither found it melted nor clotted into lumps, nor, when I weighed it again, did I ſee cauſe to conclude, that there was much of it waſted, beſides what ſtuck to the ſides of the crucible and to a little clay, wherewith I had luted on the cover (and which, to ſhew you that the heat had not been inconſiderable, was in ſeveral places burnt red by the vehemence of fire): and when I afterwards kept this powder in an open crucible among glowing coals, neither I, nor one that I employed to aſſiſt me, perceived it at all to ſmoke; and having put a little upon a quick coal, and blown that too, I found that which I had not blown away, to remain fixed (which ſome bodies will not do) upon quick coals, that will endure the fire in a red-hot crucible.

4. I FOUND this powder to be much heavier in ſpecie than water; for employing a nice pair of gold ſcales, and a method that would be too long here to deſcribe, I found that this powder weighed ſomewhat (though not much) more than twice ſo much common water, as was equal to it in bulk. And leſt ſome corollaries, that ſeem obviouſly contained in the common but groundleſs conceits of the Peripateticks, about the proportions of the elements in denſity, &c. ſhould make you expect that this powder ought to have been much more ponderous, I ſhall add, that having had the curioſity, which I wonder no body ſhould have before me, to examine the gravity of the earth, which ſeems the moſt elementary of any we have, I took ſome ſifted wood-aſhes, which I had cauſed to be three or four times boiled in a plentiful proportion of water, to free them from ſalt, and having put them very dry into common water, I found them but little heavier than our newly mentioned powder, ſurpaſſing in weight water of the ſame bulk but twice, and a little more than a 6th part (water and it being very little more than as 1 to $2\frac{1}{6}$). And that you may the leſs doubt of this, I will yet ſubjoin, that examining the ſpecific gravity of (white) glaſs itſelf, I found that compact body to be very little, if at all, more than two times and a half as heavy as water of equal bigneſs to it. So that the gravity of that powder, which, borrowing a chymical term, we have been calling virgin earth, being added to its fixedneſs and other qualities, it may ſeem no great impropriety of ſpeech to name it earth; at leaſt if by earth we mean not the pure elementary earth of the ſchools, which many of themſelves confeſs not to be found actually ſeparate,

but

but a body dry, cold, ponderous, induring the fire, and, which is the main, irresoluble by water and fire into other bodies specifically different.

[But to return to the guise of the powder; when I asked this learned man, whether he observed the glass he distilled in to have been fretted by the liquor, and whether this lost of its substance, according as it desposited more powder, he answered me (and he is a person of unsuspected credit) that he found not his glass to have been injured by the liquor, and that the water wasted (though he were careful it should not do so by evaporation and transfusions) by degrees so much, that there remained by his estimate but about an eighth part of the first quantity. And though for certain reasons he kept by him the liquor last distilled, yet he doubted not but that it might be very nigh totally brought into earth, since out of an ounce of distilled rain-water he had already obtained near three quarters of an ounce, if not more, of the often-mentioned earth.]

These several relations will, I suppose, persuade you, *Pyrophilus*, that this experiment is hopeful enough to be well worth your pursuing; if not, that perhaps none but such a scrupulous person as I would think the prosecution of it other than superfluous; and if you do acquiesce in what hath been already done, you will, I presume, think it no mean confirmation of the corpuscularian principles and hypotheses; for if, contrary to the opinion that is so much in request among the generality of modern physicians and other learned men, that the elements themselves are transmuted into one another, and those simple and primitive bodies, which nature is presumed to have intended to be the stable and permanent ingredients of the bodies she compounds here below, may be artificially destroyed, and (without the intervention of a seminal and plastick power) generated or produced: if, I say, this may be done, and that by such slight means, why may we not think that the changes and metamorphoses that happen in other bodies, which are acknowledged by the moderns to be far more liable to alterations, may proceed from the local motion of the minute or insensible parts of matter, and the changes of texture that may be consequent thereunto? Some bold atomists would here be determining, by what particular ways this strange transmutation of water into earth may be performed; and would perchance particularly tell you how the continually but slowly agitated parts of the water, by their innumerable occursions, may by degrees rub, and as it were grind themselves into such surfaces, as either to stick very close to one another by immediate contact (as I elsewhere observe polished pieces of glass to do) or implicate and intangle themselves together so, as to make as it were little knots; which knots (he would add) or the newly mentioned clusters of coherent particles, being then grown too great and heavy to be supported by the water, must subside to the bottom in the form of a powder, which, by reason of the same gravity of the moleculæ, and the strict union of the lesser particles that compose them, obtain an indisposition to dissolve in water, and to be elevated or dissipated by the fire; as their insipidness may be accounted for by its being but the same with that of the liquor, whence they were made, and their transparency by that of the water they were made of, and by the multitude of the little surfaces that belong to so fine a powder. But though in favour of such conjectures I could somewhat illustrate them, partly by applying to this occasion what I elsewhere observe of the reducing of the fluid body of quicksilver, by a bare circulation (which is but a repeated distillation) with a proportionable heat, into a real powder, which also will not so easily be raised by the fire, as the fluid body, whence by change of texture it was made; and partly by subjoining, among other things, how by the conjunction of two distilled liquors digested together I have obtained good store of an insipid substance, that would not melt in water, and that would long enough endure no inconsiderable degree of fire; though, I say, by these and other such particulars I could make our atomist's conjectures less improbable, yet the full disquisition of so difficult a subject is too long and intricate to be proper for this place *.

And therefore, without here examining our atomist's explication of this metarmorphosis, we will give him leave for a while to suppose the transmutation itself to be real, and thereupon to consider whether the historical part of it do not much disfavour some of the chief doctrines of the chymists, and a fundamental one of *Helmont*'s. For if the purest water may be turned into earth, it will not be easy to make it improbable that the other ingredients of mixt bodies, which the chymists call their hypostatical principles, are capable of being transmuted into one another, which would overthrow one of the main foundations of their whole philosophy; and besides, if out of the simplest water itself a moderate fire can produce a large proportion of earth, that was not formerly præexistent in it, how shall we be sure that in all the analyses which the fire makes of mixed bodies, the substances thereby exhibited are obtained by separation only, without any transmutation? As for *Helmont*, it is well enough known, that he makes water to be the material principle of all bodies here below, which he would have to be either water itself, or but water disguised by those forms, which the seeds of things have given it. I will not here examine whether this opinion, if he had restrained it to animals and vegetables, might not with some restriction and limitations be kept from appearing absurd, since my *Eleutherius* hath (though without absolutely adopting it) elsewhere pleaded for its not being so extravagant as it hath been thought.

But whereas *Helmont*'s grand argument from experience is grounded on this, that the alkahest doth, as he affirms, by being digested with and distilled from other tangible bodies, reduce them all at last into a liquor no way differing from rain-water, though we should grant the matter of fact, yet the experiment of our powder will warrant me to question their ratiocination. For if all mixed bodies be therefore concluded to be materially from water, because they are by the operation of the fire and a menstruum, after having passed through divers previous changes, reduced at length into insipid water; by the same way of arguing (and with great cogency) I might conclude, that all those bodies are materially but disguised earth, since without intervention of a seminal principle (for *Helmont* will not allow that title to fire, which he styles the artificial death of things) water itself may be turned into earth. Indeed if that acute chymist were now alive, and had such an immortal liquor, as he describes his alkahest to be, I would gladly put him upon trying whether that menstruum would reduce our white earth into water. But there being no more probability of that, than that such reproduced water, being just what it was before, might be turned into earth again; it may be probably said, that since these bodies are mutually convertible into one another (and as to the version of water into earth, by a seemingly slight operation) they are not either of them ingenerable and incorruptible elements, much less the sole matter of all tangible bodies, but only two of the primordial and of the most obvious schematisms of that, which is indeed the universal matter; which as it comes to have its minute particles associated after this or that manner, may, by a change of their texture and motion, constitute with the same corpuscles sometimes water and sometimes earth.

But, *Pyrophilus*, to leave these reflexions, to return to the bold conjectures, that they are ground on; though if I had leisure and indulgence enough, I could, I confess, add many things in favour of some thoughts: † yet I would not have you wonder, that

* What is here delivered may be for the main verified by what the reader will meet with in the (following) Xth experiment, though that be not it which the author meant.

† Of the possible ways of turning liquors into consistent bodies, by bending, breaking, twisting, and by otherwise changing the texture of the liquor, see more particularly the *History of Fluidity and Firmness*, published by the author.

whilst I was mentioning the many particulars that seem to evince the change of water into earth, I should let fall some words that intimate a diffidence about it. For to disguise nothing unto you, I must confess, that having in spite of an unusual care unluckily lost a whole paper of the powder I had made myself, and having unexpectedly been obliged to remove from my furnaces before I had made half the trials I judged requisite in so nice a case, I have not yet laid aside all my scruples.

For 1. I would gladly know whether the untransmuted rain-water, by the deposition of so much terrestrial matter, were grown lighter in specie than before, or sharp in taste. Next I would be thoroughly satisfied (which I confess I am not yet, notwithstanding all that the followers of *Angelus Sala* have confidently enough written) whether and how far insipid liquors (as rain-water is) may or may not work as menstruums upon stones or earthy bodies: not to question whether the particles of rain-water may not by their mutual attrition, or some other action upon one another, be reduced into shapes and sizes fit to compose such a menstruum as the liquor was not before; as in divers plants that seemed to be nourished only with water, the sap is endowed with a sharp taste and great penetrancy and activity of parts.

2. It were also fit to know whether the glass body, wherein all the distillations are made, do lose of its weight any thing near so much as the obtained powder amounts to over and above the decrement of weight which may be imputed to the action of the heat upon the substance of the glass, in case it appear by another glass, kept empty in an equal heat, and for the same time, that the glass loses by such operations any thing worth reckoning. And it were also not impertinent to try whether the gravity of the obtained powder be the same in specie with that of the glass wherein the distillations were made (for that it differed but about a fifth part from the weight of crystalline glass I lately mentioned). Which scruple and some of the former I might have prevented, if I had had convenient metalline vessels wherein to make the distillations instead of glass ones.

3. I could wish likewise that it were more demonstrably determined what is on all hands taken for granted (as it appears indeed highly probable) that distilled rain-water is a perfectly homogeneous body; which if it be not, divers suspicions might be suggested about its transmutation into earth; and if it be, it will be, as a very strange thing, so a matter of very great difficulty to conceive, how a perfectly and exquisitely homogeneous matter should, without any addition) or any seminal and plastick principle, be brought to afford great store of a matter of much more specifick gravity than itself; since we see that no aggregate we can make of bodies, but equiponderant in specie with water, doth by virtue of their convention grow specifically heavier than it.

4. Having had the curiosity to try whether corrosive liquors would work upon our white powder, I found that not only good oil of vitriol would corrode it, but strong and dephlegmed spirit of salt did readily work upon part of it, and that without the assistance of heat, though not without hissing and exciting great store of bubbles, as I have known such menstruums do, when put upon *Lapis Stellaris* or *Ossifragus*, or some such soft stone; as if that so much defecated rain-water, actuated by heat, had resolved some of the looser corpuscles of the sand or stone, that together with some salts compose common glass, as I have observed in some petrifying water, that some of the bodies I took up, and which were presumed to be petrified, were but crusted over with stone that seemed generated, but by the successive apposition of stony particles that, lying invisibly mingled with the running water, stuck in their passage to the conveniently disposed bodies that lay in the stream's way. But yet I must not omit, that when I suffered this mixture to settle, as much of the powder as seemed to be a very great part of

of it, remained in the lower part of the liquor, as if that had rather fretted than dissolved it; and that not becaufe the menftruum was overcharged or glutted, as I found by putting in afterwards feveral frefh parcels of powder, which it readily fell upon, not without noife and froth. Nor muft I forget that fometimes I have excited fuch an ebullition, by pouring the fame liquors upon the earthy part of wood-afhes, feveral times wafhed in boiling water (though, I confefs, I afterwards fomewhat fufpected there might remain fome little adhering alkali which might occafion thofe bubbles, notwithftanding that both I and another, whom I alfo invited to tafte it, took the earth to be quite faltlefs): I might, *Pyrophilus*, add, that fometimes alfo methought I found this powder (which yet likewife fometimes happened to me with the lately mentioned earth of wood-afhes) fomewhat gritty between my teeth, and fubjoin divers other particulars, if it were not too tedious to mention to you all the doubts and confiderations that have occurred to me about the recited change of water into earth: which yet are not fuch as ought to hinder me from giving you the hiftorical account I have fet down, fince to fome of my fcruples I could here give plaufible anfwers, but that I cannot do it in few words. And if any part of our white powder prove to be true earth, no body perhaps yet knows to what the experiment may lead fagacious men: and whether in a ftrict fenfe it be true earth or no, yet the phænomena that are exhibited in the production of it are fufficient to give this ninth experiment a place among the others (of the fame decade) with which it is affociated. For fince out of a fubftance that is univerfally acknowledged to be elementary and homogeneous, and which manifeftly is fluid, tranfparent, much lighter in fpecie than earth, moift and fugitive, there is artificially generated or obtained, a fubftance confiftent, white, and confequently opacous, comparatively ponderous, dry, and not at all fugitive; the alteration is fo great, and effected in fo fimple a way, that it cannot but afford us a confiderable inftance of what the varied texture of the minute parts may perform in a matter confeffedly fimilar. And if frequently diftilled rain-water fhould not be allowed homogeneous, our experiment will at leaft fhew us, better than perhaps any hath yet done, how little we are bound to believe what the chymifts and others tell us, when they pretend manifeftly to exhibit to us homogeneous principles and elementary bodies; and how difficult it is to be certain, when a body is abfolutely irrefoluble into fpecifically differing fubftances, and confequently what is the determinate number of the perfectly fimple ingredients of bodies (fuppofing that fuch there are): though I muft confefs that my only aim is not to relate what hath been done, but to procure the profecution of it. For if the obtained fubftance be by the rain-water diffolved out of the glafs, this will both prove a noble and furprifing inftance of what may be done by infipid menftruums, even upon bodies that are juftly reckoned among the compacteft and moft indiffoluble that we know of, and may afford us many other confiderable hints that have been partly intimated already: and if, on the other fide, this powder, whether it be true elementary earth or not, be found to be really produced out of the water itfelf, it may prove a *magnale* in nature, and of greater confequence than will be prefently forefeen, and may make the alchymifts hopes of turning other metals into gold appear lefs wild; fince that by experimentally evincing that two fuch difficult qualities to be introduced into a body, as confiderable degrees of fixity and weight (whofe requifitenefs to the making of gold are two of the principal things that have kept me from eafily expecting to find the attempts of alchymifts fuccefsful) may, without the mixture of the homogeneous matter, be generated in it, by varying the texture of its parts.

I WILL not now adventure to add any thing of what I have been attempting about the tranfmuting (without additaments) of pure alkalizate falts into earth, becaufe I do not

not yet know whether the trials will anſwer my hopes (for I do not yet call them my expectations). But upon this ſubject of tranſmutations I could, if it did not properly belong to another treatiſe, tell you ſomething about the changes that may be wrought upon highly rectified ſpirit of wine, which would perchance make you think of other things of the like kind leſs indefeaſible. For whereas it is a known thing that that ſpirituous liquor being kindled (and that, if you pleaſe, by other ſpirit of wine actually fired) will, for aught appears, burn all away, that is, be totally turned into flame; if I durſt rely in ſo important a caſe on a couple of trials, whilſt I hope for an opportunity of making farther ones, I would tell you, that by a way unthought on (that I know of) by any body, I have, without any addition, obtained from ſuch ſpirit of wine, as being kindled in a ſpoon would flame all away, without leaving the leaſt drop behind it, a conſiderable quantity of downright incombuſtible phlegm. And by another way (mentioned indeed by *Helmont*, but not taught to almoſt any of his readers) ſome ingenious perſons that you know and eſteem, working by my directions (but without knowing what each other was doing) did both of them reduce conſiderable quantities of high rectified ſpirit of wine (that would before have burnt all away) into a liquor that was for the moſt part phlegm, as I was informed, as well by my own taſte, as by the trials I ordered to be made (being forced myſelf to be moſt commonly abſent). From which change of the greateſt part of that firſt liquid ſpirit into phlegm, it ſeems deducible that the ſame portion of matter, which by being kindled may be turned all into fire, may be, by another way of handling, turned into phlegm or water, and this without the addition of any thing, and without being wrought upon by any viſible body, but one ſo extremely dry as duely prepared ſalt of tartar; and that itſelf is not ſo indiſpenſably neceſſary to the obtaining of phlegm out of totally inflammable ſpirit of wine, but that, as I was ſaying, I did by another way obtain that dull liquor, without imploying the ſalt or any other viſible body whatſoever. But I make a ſcruple to entertain you any longer with extravagancies of this nature, and yet if I were ſure you would contain your ſmiles, I would add for concluſion, that if I had time and opportunity to furniſh myſelf with any quantity of that water, I had it in my thoughts to try whether that would have afforded me ſuch a terreſtrial ſubſtance as rain-water had done, and thereby have undergone a new and further metamorphoſis.

EXPERIMENT X.

THERE is one experiment more, two of the chief phænomena of which belong to another diſcourſe (where I particularly mention them); and yet I ſhall conclude this little treatiſe with the recitation of the experiment itſelf, not only becauſe divers of the phænomena do eminently belong to our preſent ſubject, but becauſe I have ſcarce met with any experiments more ſuitable to the deſign I have of ſhewing, before I conclude this diſcourſe, what great and ſudden productions and deſtructions of qualities may be effected by the compoſition of the ſmalleſt number of ingredients, even among liquors themſelves; and ſuch too as are believed to be both of them ſimple and homogeneous, and incapable of putrefaction; that ſo it may appear what notable alterations of qualities even ſeemingly ſlight and eaſy mixtures can perform among bodies both of them fluid, as well as among thoſe that were either both of them ſtable, or one of them ſtable, and the other conſiſtent.

TAKE then of good oil of vitriol and of ſpirit of wine that will burn all away, equal parts, not in quantity, but in weight; put them together by little and little, and having placed,

placed the mixture in a bolt-head or glaſs-egg with a long neck, and carefully ſtopped it with a cork and hard wax, ſet the veſſel in a moderate heat to digeſt for a competent while (two or three weeks may do well) then pour out the mixture into a tall glaſs cucurbite, to which lute on a head and a receiver with extraordinary care, to prevent the avolation of the ſpirits, which will be very ſubtile: then with a very gentle fire abſtract the ſpirit of wine that will firſt aſcend; and when the drops begin to come over ſouriſh, ſhift the receiver, and continue the diſtillation with great care, that the matter boil not over: and when you judge that about half the acid liquor is come over, it will not be amiſs, though it be not neceſſary, to change the receiver once more: but whether you do this or no your diſtillation muſt be continued, increaſing the fire towards the latter end till you have brought over all you can, and what remains in the cucurbite muſt be put into a glaſs well ſtopped to keep it from the air.

N. B. 1. That to the production of moſt, if not all of the phænomena of this experiment, it is not abſolutely neceſſary that ſo long a digeſtion (not to ſay, not any) be premiſed; though if the time above preſcribed be allowed, the experiment will ſucceed the better.

2. THAT, I remember, I have ſometimes made uſe of oil of ſulphur *per campanam* (as they call it) inſtead of oil of vitriol, to produce the recited phænomena; and though the attempt ſucceeded not ill as to divers particulars, yet I afterwards choſe rather to employ oil of vitriol; both becauſe it did in ſome points better anſwer my expectation than the other liquor, and becauſe I would not give occaſion to ſuſpect that the odours, hereafter to be mentioned as phænomena of our experiment, were due to the common ſulphur, whence the unctuous liquor made *per campanam* was obtained as ſuch, and did no way proceed from the acid vitriolate ſalt, which that oil (as it is improperly called) doth abound with.

3. THAT I had likewiſe the curioſity to digeſt oil of vitriol with Spaniſh wine inſtead of ſpirit of wine, by which means I obtained an odd ſpirit and reſidence, and ſome other phænomena, which I content myſelf to have in this place given hint of, in regard that wine being a liquor of a much leſs ſimple nature than its ſpirit, the phænomena afforded me by this are much fitter for my preſent purpoſe.

4. THAT great care muſt be had in regulating the fire, when once a good part of the acid ſpirit mentioned in the proceſs is come over. For if the fire be not increaſed, the reſt will ſcarce aſcend; and if it be increaſed but a little too much, the matter will be more apt than one would ſuſpect to ſwell exceedingly in the cucurbite, and perhaps run over into the receiver, and ſpoil what it finds there, as it hath more than once happened to me, when I was fain to commit the management of the fire to others.

Now the oil of vitriol and the ſpirit of wine being both of them diſtilled liquors, and the latter of them ſeveral times re-diſtilled, and one of them being drawn from ſo ſimple and familiar a ſubſtance as wine, and the other from a concrete not more compounded than what nature herſelf (which, as I elſewhere ſhew, can without the help of art produce vitriol) doth divers times preſent us with; theſe liquors, I ſay, being both of them diſtilled, and conſequently volatile, one would expect that by diſtilling them they ſhould be brought over united, as I have tried, that the ſpirit of wine and of nitre, or alſo of common ſalt, may be, and as the ſpirits of differing vegetables are wont to be; or that at leaſt the diſtillation ſhould not much alter them from what it found them, after they had been well mingled together. But this, notwithſtanding theſe two liquors being of very odd textures in reference to each other, their conjunction and diſtillation will make them exhibit divers conſiderable and perhaps ſurpriſing phænomena.

For, first, whereas spirit of wine has no great scent, nor no good one, and moderately dephlegmed oil of vitriol is wont to be inodorous; the spirit that first comes over from our mixture hath a scent not only very differing from spirit of wine, but from all things else that I remember I ever smelt. And as this new odour doth, to almost all those whose opinions I have asked about it, seem very fragrant and pleasant, so I have sometimes had it so exceeding subtile, that in spite of the care that was taken to lute the glasses exactly together, it would perfume the neighbouring parts of the laboratory, and would not afterwards be kept in by a close cork covered with two or three several bladders, but smell strongly at some distance from the phial wherein it was put. I did not think it unlikely that so noble and piercing a liquor might be of no mean efficacy in physick; and though I missed of receiving an account of its effects from some ingenious physicians, into whose hands I put it to have trials made of it, yet I cannot despair of finding it a considerable medicine, when I remember partly what hath been done by some acquaintances of mine with bare phlegm of vitriol, upon the account (as is supposed) of that little sulphur of vitriol that, though but sparingly, doth enrich that liquor; and partly, what the masters of chymical arcana tell us of the wonderful virtues of the volatile sulphur of vitriol, and what I have observed myself, that may invite me to have a good opinion of remedies of that nature.

2. But to shew how much the odours of bodies depend upon their texture, I shall now add, that after this volatile and odoriferous spirit is come over, and has been followed by an acid spirit, it will usually towards the latter end of the distillation be succeeded by a liquor that is not only not fragrant, but stinks so strongly of brimstone, that I have sometimes known it almost take away the breath (as they speak) of those who, when I had the receiver newly taken off in my hand, did (either because to make sport I gave them no warning, or because they would not take it, as thinking what I told them was impossible) too boldly adventure their noses in the trial.

3. There is in this operation produced a liquor that will not mingle either with the fragrant or with the fetid spirit hitherto described, but is very differing from both of them, and is so very pleasant, subtile, and aromatical, that it is no less differing as well from spirit of wine as oil of vitriol. But of this liquor I give a further account in a more convenient place.

4. When the distillation is carried on far enough, you will find at the bottom, that the two above-mentioned diaphanous spirits (for oil of vitriol is indeed rather a saline spirit than an oil) have produced a pretty quantity of a substance, not only very opacous, but black almost like pitch or jet.

5. And this substance, though produced by two bodies that were not only fluid but distilled, will not alone be consistent, but (if the distillation have been urged far enough) brittle.

6. And though spirit of wine be reputed the most inflammable, and oil of vitriol the most corrosive liquor that is known, yet I could not find that this black substance would easily, if at all, be brought, I say, not to flame, but to burn, nor that it had any discernible taste; though both the liquors from whose mixture it was obtained, have an exceeding strong and pungent taste.

7. And whereas both oil of vitriol and spirit of wine will each of them more readily than most liquors that are yet known, mingle with common water, and diffuse itself therein, I observed that this pitchy mass, if the distillation had been continued till it was perfectly dry, would not, that I could perceive, dissolve in common water for very many hours, and, if I much misremember not, for some days.

8. And

8. And laſtly, whereas the oil of vitriol and the ſpirit of wine were both of them diſtilled liquors, and one of them exceeding volatile, and fugitive, yet the black maſs produced by them was ſo far fixed that I could not make it riſe by a conſiderably ſtrong and laſting fire that would have raiſed a much more ſluggiſh body than the heavieſt of thoſe that concurred to produce it.

The remaining particulars that I have obſerved in this experiment belong to another treatiſe, and therefore I ſhall forbear to mention them in this: nor ſhall I at preſent add any new phænomenon to thoſe I have already recited; thoſe freſhly mentioned experiments, and thoſe that preceded them, being, even without the aſſiſtance of the four obſervations I have delivered before them, ſufficient to manifeſt the truth I have been endeavouring to make out. For, in the experiments we are ſpeaking of, it cannot well be pretended, or at leaſt not well proved, that any ſubſtantial forms are the cauſes of the effects I have recited; for in moſt of the (above-mentioned) caſes, beſides that in the bodies we employed, the ſeminal virtues, if they had any before, may be ſuppoſed to have been deſtroyed by the fire, they were ſuch, as thoſe I argue with would account to be factitious bodies artificially produced by chymical operations. And it is not more manifeſt, that in the production of theſe effects there intervenes a local motion and change of texture by theſe operations, than it is evident and precarious, that they are the effects of ſuch things as the ſchools fancy ſubſtantial forms to be: ſince it is in theſe new experiments, by the addition of ſome new particles of matter, or the receſs or expulſion of ſome præ-exiſtent ones, or, which is the moſt frequent way, by the tranſpoſition of minute parts, yet without quite excluding the other two, that no more ſkilful a chymiſt than I have been able to produce by art a not inconſiderable number of ſuch changes of qualities, that more notable ones are not ordinarily preſented us by nature, where ſhe is preſumed to work by the help of ſubſtantial forms; I ſee not why it may not be thought probable that the ſame catholick and fertile principles, motion, bulk, ſhape, and texture of the minute parts of matter, may, under the guidance of nature (whoſe laws the modern Peripateticks acknowledge to be eſtabliſhed by the all-wiſe God) ſuffice likewiſe to produce thoſe other qualities of natural bodies, of which we have not given particular inſtances.

FREE CONSIDERATIONS

ABOUT

SUBORDINATE FORMS,

As they are wont to be maintained by divers Learned Moderns.

AN ADVERTISEMENT.

[The following difcourfe about fubordinate forms had come forth the laft year, annexed to the foregoing examen of fubftantial forms, as a part of, or an appendix to it, being then written, and promifed in the Preface to the Reader, if by reafon of the bookfeller's hafte, who was defirous the book might be printed and publifhed at the begining of the term, it had not been left out, and is here added in this edition, wherein no other addition is made.]

THE generality of vulgar philofophers have for many ages fo handled the doctrine of forms, as if they fufpected not that more than one form could belong to a natural body; but fome later writers, efpecially the learned *Sennertus*, and, if you will believe him, the famous Peripatetick *Zabarel* himfelf, have endeavoured to introduce an hypothefis, which teaches that in animals and plants, befides the fpecifick form, as *Sennertus* calls it, which alone is wont to be taken notice of, there may refide in thofe bodies, and efpecially in fome determinate parts of them, certain other forms proper to thofe parts, but neverthelefs fo fubjected to the predominant miftrefs form, if I may fo call it, that they deferve the title but of fubordinate forms, and. during the reign of the fpecifick form, are fubfervient to it, but in the capacity (as it were) of matter; yet fo that when the fpecifick form comes to be abolifhed or depofed, thefe fubordinate forms may come to fet up for themfelves, and in reference to thofe parts of matter they belong to, exercife the functions of fpecifick forms: as in a dog or a horfe, befides the fenfitive foul, which is the fpecifick form of the whole beaft, the flefh, and blood, and bones have their diftinct forms, which appertain to them as they are fuch bodies, though they are ruled by the foul, but as the matter which fhe animates and informs; and when by death the fenfitive foul or fpecifick form is depofed or abolifhed, the body is not prefently refolved into the four elements, much lefs reduced into the firft matter, but thofe fubordinate forms do ftill keep the flefh, flefh; and the bone, bone; the one, for a little, and the other for a much longer time.

To make out this doctrine, he ingenioufly urges the fpecifick virtues obfervable in gathered plants, and particularly the purgative faculty of rhubarb, fenna, and other cathartick vegetables. And though, as to this noble fort of examples afforded him by the fpecifick properties they are endowed with, when they are deprived of the life they enjoyed as plants, it may not be pretended by the obftinate, that, for aught has been yet tried, rhubarb, fenna, *&c.* are not purgative while they are living plants, and fo, when they are dead, do not fo much retain as require that fpecifick virtue; as wine obtains divers medicable virtues (as that of cooling, diffolving coral, pearl, *&c.*) when (by fome alteration imperceptible to fight) lofing its predominant form it turns to vinegar, which it had not before; yet it were not difficult to propofe experiments that would determine this fcruple, if it were thought important enough. And I fhall add, that it is evident that damafk-rofes, for inftance, which are purgative, retain for a confiderable time the fame colour and fragrant odour, *&c.* when they are gathered, and confequently acknowledged to be deprived of life, as when they grew upon the tree.

This doctrine of fubordinate forms has been fo well entertained, and fuppofed to be of fuch importance, and (which nearly concerns the paft difcourfe) to afford fuch countenance to fubftantial forms, that the nature of our prefent difcourfe forbids me to leave it altogether untouched; and the rather becaufe I have not found it fo much as taken notice of by the corpufcularian philofophers. But as (on the one hand) this confideration invites me to offer fomething about this matter, fo (on the other fide) joining with the difficulty and abftrufenefs of the fubject, it would deter a bolder writer than I, to pretend to give a full and fatisfactory account of fo perplexed and abftrufe a matter. And therefore I fhall think my attempt may be excufable (if not acceptable) if I can at prefent fhow that fubordinate forms may be intelligibly explicated in a general way, according to the corpufcularian principles, or are at leaft very reconcileable thereunto. And in regard that, as I juft now intimated, the patrons of thefe fubjugated forms affert fubftantial ones, and proceed upon other notions that we do not admit, I muft venture to explicate this matter in a way very differing from theirs. And it will not be amifs to begin my difcourfe with laying down fome obfervations, which may ferve partly to add fome things unmentioned to thofe that are mentioned by *Sennertus* or *Zabarel*, towards clearing up the notion and nature of fubordinate forms (a fubject not obvious nor eafy to be made plain) and partly to make way for the carrying on the fubfequent part of the difcourfe, without thofe excurfions that would elfe too much interrupt it.

First then, we may confider, that, according to what I have formerly difcourfed, the name form is a technical word or term of art, whofe fignification, as I there alfo noted, is not fo well defined as is prefumed, and were to be wifhed. But without much injury to the more obvious and ufual notion of it, we may obferve, that it is commonly fome one confiderable thing (or at moft fome few things) fuch, for the moft part, as fome confpicuous phænomenon, that is exhibited, or fome peculiar operation, that is performed by it, or fome particular ufe, to which it is applicable, upon whofe account this or that form is attributed to this or that natural body; and only upon the recefs or abolition of which it is faid to lofe its form, or, if you pleafe, denomination.

Secondly, I confider that the bodies whofe being or not being endowed with fubordinate forms is contended for, are generally either vegetables, or animals, or bodies belonging to them; and confequently thefe bodies being of a very compounded nature

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confift of parts, whether organical or not, that are not all of them of the fame nature, which I take to be true, not only of thofe parts that are unanimoufly owned to be organical, but of many of thofe that are reputed fimilar, becaufe as to fenfe they are fo. This is evident in bones, which, though believed to have as good a right as any to the title of fimilar parts, do yet by diftillation afford falt, oil, phlegm, fpirit, and afhes. And vitriol, though fimilar as to fenfe, may be (as we formerly noted) artificially produced by uniting the metalline particles of iron or copper with the faline corpufcles of diftilled falt or nitre; which inftance I the lefs fcruple to make ufe of, becaufe, that though the patrons of fubordinate forms feem to have afferted them, to give fome account of what happens in vegetables and animals, when the ultimate form is abolifhed or expelled; yet for my own part I fee not why we may not alfo attribute fubordinate forms to divers inanimate bodies. To illuftrate this matter, I will borrow an example from rhubarb (for this drug, as it is fold in the fhops, is an inanimate body) wherein the purgative faculty is affirmed to proceed from a fubftantial form; which virtue, whilft the rhubarb grew in the ground, did, as they teach us, proceed from the fpecifick form. For if from the fame rhubarb we do, by a convenient menftruum, extract, together with the finer parts of the body, all the purgative virtue (which, as *Sennertus* himfelf teaches, may very well be done) I fee not why, according to his grounds, the remaining rhubarb, which will retain divers of its former qualities, if not difclofe fome new ones, ought not to have a peculiar form diftinct from that which he and the fchools call *Forma miftionis* affigned to it; to which thofe qualities may be attributed, and which confequently may be looked upon as a fubordinate form in reference to that which the intire, though inanimate rhubarb, had before. But whatever become of this inftance, there are other bodies, wherein I fee not why, according to his grounds, a fubordinate form may not be allowed: for in an olive or an almond (for example) though when it is gathered it ceafes to be animated by the vegetative foul of the tree, yet it retains the fame fhape, colour, &c. that it had before it was gathered (which it retains upon the account of the fubordinate form that belonged to it as fuch a fruit) by virtue of which form it may be preferved found during a whole year, or perhaps much longer; fo when by barely crufhing the pulp of the olive between your fingers you may immediately fqueeze out oil, which confeffedly was pre-exiftent there (the preffure only affociating fo many parts as to make them vifible) and which is a peculiar liquor endowed with noble qualities, and capable of preferving itfelf divers years: I fee not (I fay) why the form of this oil, from whence its qualities muft be faid to flow, may not be looked upon as having been, whilft the liquor made a part of the olive, a fubordinate form to that of the entire fruit; whofe remaining part having alfo its own peculiar qualities, and that fuch, whereby, for inftance, an olive that has loft its oil much differs from an almond that has loft its alfo, may, for aught I fee, deferve to have a diftinct fubordinate form afcribed unto it. But to make this out the better, I fhall here add a couple of examples that perhaps will feem clear enough; the one is fulphur vive, wherein (to fpeak according to the chymical notions) nature has united under one form two bodies of very differing kinds, the one readily inflammable, and the other a great refifter of fire; and yet thefe two are eafily feparable, as may appear by the known chymical practice of kindling fulphur under a glafs bell; for the oleaginous part (as the combuftible is fuppofed) manifeftly burns away with a blue flame, and the faline corpufcles meeting with the moift vapours that are commonly interfperfed in the air, are condenfed againft the fides of the glafs into a highly fharp and corrofive menftruum (which may feveral ways be brought to exhibit its falt in a dry and brittle form). The other inftance I was to mention is alfo of a body that cannot be pretended to be factitious (namely, *cinnabaris foffilis*) for in this concrete under the form of a mineral ftone, nature has ranged three (if not more) complete bodies, that has each of them its own diftinct form, and that exceeding different from the others, as may appear when thefe bodies are fkilfully feparated. For thence, as we noted above, we have obtained a running mercury, an inflammable fulphur, which itfelf will be eafily allowed to be a compounded body, and a ftrange concrete, whofe properties I had not occafion to look into. To thefe inftances I might add divers others, if it were neceffary fo to do. And if it be faid that thefe forms are not fubordinate, but rather co-ordinate, it will lie upon the objectors to prove it; who perhaps will find it no eafy matter to evince, that the fame ingredient, for inftance, of fulphur, is not as much fubjugated by the form of the entire body, as that of the purgative portion of rhubarb, by the form of that drug. But if it did appear that thefe forms were more properly ftyled co-ordinate than fubordinate, it would not much trouble me, who am inclined to think that divers of the forms which *Sennertus* and his followers call fubordinate or fubjugated, may be as fitly ftyled co-ordinate or concurrent; fince I fhall fhow anon that I do not afcribe to the fpecifick or fupreme form, in reference to the reft, fuch a coercive power and dominion, as thofe learned men are pleafed to do.

Thirdly, I confider that all thefe differing bodies, whereof, as of parts, or as of ingredients, a compounded body is made up, are by virtue of the compofition and peculiar fabrick thereof fo put together or contrived, that they concur to thofe actions or operations which are proper to the body as fuch, and therefore are prefumed to flow immediately from the form of it. For an inftance of which I fhall name gunpowder, where three ingredients upon a very flight mixture (as I fhall anon fhew theirs to be) do, by a concurrent action, produce thofe wonderful effects that are fcarce to be matched by nature herfelf. And that thefe ftupendous operations really refult from the proportion of the ingredients, and the manner of their commixture, will be hereafter manifeft.

Fourthly, I confider that notwithftanding thefe feveral parts, whereof the compounded body confifts, do in the proper, and, if I may fo call them, fpecifick actions of the body fo concur, as to perform them jointly, and (as the fchools in divers cafes exprefs themfelves) *per modum unius:* yet thefe thus confpiring bodies may each of them retain thofe attributes or that modification, which made it a diftinct natural body, before it came to be affociated with thofe others, with which it makes up a more compounded body.

And if it be proper to propofe here an argument *ad hominem*, I fhall add, that the more confiderate of the modern fchoolmen themfelves do, though perhaps unawares, teach fuch things as do very well agree with the doctrine of fubordinate forms. For when in the generation of man they tell us, that, as *Ariftotle* alfo obferves, the embryo lives the life of a plant and of an animal, before he attains to live the life of a man, it is plain, that, according to them, upon the introduction of the rational foul the vegetative and fenfitive fouls, that before fucceffively informed the embryo, do fo no more, the advenient human foul becoming now the true form of the human body. And thefe pre-exiftent fouls are not abolifhed, and do not lofe their being, but only their office, which at firft was to inform the body of the embryo, but now ceafes, fo that they are not deftroyed, but only depofed. And this confideration feems to afford ground enough to admit in divers natural bodies, forms that difpofe the matter they modify for the reception of a noble ftamp, for which reafon I fometimes call them preparatory forms, befides thofe more noted forms, that the fchools ufually term fpecifick, (and which I fometimes call predominant or fupreme) by which I fuppofe is meant (to fpeak intelligible) the laft and higheft ftamp, or modification, that nature gives that parcel of matter; whereas the preparatory form is but (if I may fo fpeak) a harbinger that dif-

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poses the matter to receive a more perfect form, which, if it be not to be succeeded by any other more noble, is intitled the specifick form of that body; as in the embryo the vegetative and the sensitive soul is but preparatory to the rational, which alone is said to be the specifick form of man.

But here I would not be thought to adopt for mine all those opinions upon which I think it allowable for me to argue with those that own them. For I must not omit to intimate *in transitu*, that I elsewhere consider with what congruity to some other of their tenets they can assert, and in what sense, in regard of the nature of the thing itself, we may admit, that the souls of all living creatures be the true forms of their bodies, notwithstanding the scruples suggested to me, as by other things, so particularly by the great difference I take notice of by some of these animating forms (if I may so call them) and other natural forms in reference to the manner of their informing the respective bodies they belong to: of this to give an instance, it is evident that the reasonable soul (which some call *animus*, to distinguish it from the *anima* or sensitive) is not the architect of the human body (which they confess and teach must be organized before it be fit to have that united to it) as many other forms are said to be of theirs; nor do all the properties, or so much as all the specifick ones, flow from that soul (whose mansion was a living animal of a determinate kind before it was united thereto) as those of other natural compounds are held to flow from their forms. And even in beasts and plants (if we will rather consider the thing than mens opinions) if the soul be all the form, there will remain in the matter after the abolition of the form great store of qualities, that by their so remaining show, that they do not flow from the soul, as gravity is said to flow from the form of the earth, and transparency from that of the air, and these surviving qualities are oftentimes not only many, and several of them noble and specifick, as appears in the beauty, fragrancy, and cordial virtue of oranges, lemons, &c. but oftentimes the same, that were there in the body, for aught our senses can perceive, whilst it was said to be informed by the soul. And I believe it would puzzle a Peripatetick to discriminate an apple or an orange, which, having been plucked off from the tree, were with a slender thread artificially tied on again by the stalk, from the other fruit as yet growing on the same branch. And not only the letters that were carved on the bark of young trees, and grew with them, remain as fair and legible as ever, when the trees are cut down; but a dead body for some time after death (and it matters not how little a while, provided the soul, and consequently the specifick form, be really destroyed or departed) does oftentimes so exactly retain the shape, feature, and even colour, warmth, and other qualities, which it had whilst the soul (a little before) was there, that it often puzzles the best physicians (especially if the sick person were hysterical or apoplectical) to discern with certainty whether the patient be dead or alive. And as for that conceit of a *forma cadaveris*, whereby divers of the modern Peripateticks have attempted to decline the inconvenience of allowing, contrary to the doctrine of very many of their party, that the same qualities remain *in corrupto* (as they speak) as were *in genito*; and that in spite of their general and fundamental tenet, the matter may be for some time (how little soever it imports not) without a substantial form: this cadaverous form, I say, that seems much to disparage our senses (which witness divers of the remaining qualities to be the same they were before) seems to be deduced without any ground from the phænomena of nature, being introduced (as * *Suarez* himself, though a friend to this expedient, ingeniously confesses) but because it is consonant to the Peripatetick doctrine that it should be so (though that itself be not

* Disput. 15. Sect. 8. Sect. 16.

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ſo evident, but that *Scotus*, and I know not how many of the Ariſtotelians themſelves, reject this form, if they do not alſo deride it).

Nor need we be very ſolicitous how the parts of a dead body can be kept together, if neither the ſoul, nor ſome new ſubſtantial form that ſucceeds it, perform that office; ſince competent agents, whatever they were, having contexed a portion of matter into ſuch a human body as the ſoul left upon its departure, the fabrick of the body and connection of the parts will ſuffice to make it retain for a little while (and that is enough for our purpoſe, ſince dead bodies are not wont to remain long unaltered) their priſtine ſhape, and divers other manifeſt qualities which may continue till the action of outward agents upon the leſs ſolid parts of the body, or the internal and inordinate commotions of the juices, and the ſofter, though not fluid parts that are contained in it, do by their degeneration vitiate the texture, and conſequently the manifeſt qualities of it. And if theſe inordinate agitations of the blood, humours, &c. be hindered, though by an external cauſe, the body, notwithſtanding the loſs of the ſoul, will continue unputrefied, not only for ſome hours, but for many months together; as a learned eye-witneſs, whom I inquired of, aſſured me, he, as well as many others have obſerved in very cold countries, as *Ruſſia*, *Sweden*, &c. where they often keep thoſe bodies that die in the winter unburied, and yet ſweet, till the ſpring, when the ſun's heat makes them begin to putrefy. And it is plain, in ſome aromatick gums and fruits, that bodies that were once plants, may, after they have loſt the vegetative ſoul, not only continue many years uncorrupted, but by embalming other bodies keep them ſo too.

But, as I was ſaying, the proſecution of this inquiry *(whether in living creatures the ſoul be always the true form, to all the intents and purpoſes that the vulgar philoſophers would have it)* belongs not to this place, elſe I might alſo queſtion the congruity of what is taught as well by *Sennertus*, as the ſchools, that upon the ſupervening of the ultimate or ſpecifick form, the forms, that thereupon become ſubordinate, do but make a part of the matter informed by the new form. I grant indeed, that they may qualify and diſpoſe the ſeveral portions of matter they belong to in ſuch a way, as that they make the body they conſiſt of a fitter ſubject or receptacle for the ultimate form that is to be introduced: and there may be a neceſſity of ſuch previous diſpoſitions in the ſubject, becauſe the compounded body is of ſuch a nature, as that no other bodies but ſuch as are thus and thus qualified, are fit to make it up. But it ſeems not to me ſo eaſy to conceive how a ſubſtance diſtinct from matter (for ſuch both he and they make their ſubſtantial forms to be) can properly be ſaid to have its capacity confounded with that of the matter. And notwithſtanding the lately mentioned diſtinction betwixt ſpecifick and preparatory forms, thoſe (laſt named) ſeem to me as true ones, whilſt they are either ſole or predominant, as the ſpecifick themſelves. For bodies are what they are by the matter and modification that do for the preſent conſtitute them, whatever they may prove to be in the future; and it is extra-eſſential to the form that is ſaid to be previous, that it is to be ſucceeded by another which is ſaid to be more noble. A ſpring of ſteel is a true and perfect ſpring before it be made a part of the watch, and by becoming ſo it is not really bettered in its nature, though it be made indeed more uſeful to man: and when copper is turned into vitriol, copper was a true and complete metal before, and it is accidental to the copper, that corroſive ſpirits coagulate themſelves with it into a ſalt-like ſubſtance. And antimony is true and perfect antimony before it be turned into glaſs, whether afterwards it happen to be or be not changed, by the bare operation of the fire; from a black and opacous mineral to a fine red and tranſparent glaſs.

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AND though I know *Ariſtotle* attributes to forms τιμιότης ϰ̉ ἀτιμία; yet it is not always ſo eaſy duly to apply thoſe civil appellations to phyſical things, and to determine whether a ſucceeding form be more or leſs noble than the precedent: as when, for inſtance, pearls are reduced by ſalts into a chymical magiſtery, and vitriol is made of iron or copper; where, for divers œconomical and military uſes, the metals themſelves are fitter than the magiſteries (as in goldſmiths ſhops and on ladies necks, the intire pearls are much more prized than the prepared ones) and for other purpoſes, eſpecially in phyſick, which regards the health of man, the magiſteries are better than the crude metals. And theſe inſtances put me in mind of taking notice, that as the ſupervening of a form does not always deſtroy the old, as in vitriol (ſuch as I formerly mentioned) the copper retains its metalline nature under the diſguiſe of a ſalt; ſo upon the abolition of the ultimate form, the previous form may in divers caſes be reduced to the exerciſe of its former functions; as, out of ſuch vitriol as I am ſpeaking of, it is eaſy, without the addition of any metalline ſubſtance, to recover true and malleable copper. But I have dwelt too long upon this fifth conſideration, which will invite me to make ſhorter work with thoſe that follow.

FIFTHLY, But before I proceed to them, it will not be amiſs to intimate, that one may, if one pleaſes, make ſome diſtinction between ſubordinate forms, there being one ſort of them that may deſerve a peculiar name. For in men, horſes, ſheep, and other perfect animals, there are divers parts, eſpecially thoſe that phyſicians call ſimilar (in oppoſition to organical ones) ſuch as bones, ligaments, membranes, which ſeem evidently to challenge peculiar and diſtinct forms: for the diverſity of their nature, being very manifeſt and ſtable, perſevering oftentimes a great while (as appears in bones) after the death of the animal, thoſe that allow that a natural body is what it is upon the account of its form, cannot well deny theſe ſo diſtinct bodies diſtinct forms; which, becauſe the bodies they conſtitute are the parts of a human body, ſome modern ſchoolmen have (not very inconveniently) called partial forms. But this diſtinction being not of ſo great weight that we need inſiſt upon it, the notice already taken of it may at preſent ſuffice.

SIXTHLY, I conſider, that among the conſtituent parts of an animal or plant there may lurk ſome ſeminal principles or rudiments, that is, ſmall parcels of matter of ſuch a texture, that though whilſt they remain aſſociated with the other parts of the compounded body, they are not by ſenſe (eſpecially when that is employed with no greater attention than is uſual) diſtinguiſhable from the reſt of the compounded body, comes to have its predominant form aboliſhed, theſe ſeminal principles or rudiments being ſet at liberty, and befriended by external heat, and the ſoftneſs which uſually attends corrupting bodies, and perhaps by a lucky concourſe of other circumſtances may fall to act according to their own nature, and generate inſects, moſs, &c. as I have more amply declared in other papers *.

SEVENTHLY, I conſider, that beſides that when the ſpecifick form of a body is deſtroyed, the change is not oftentimes ſo great as vulgar philoſophers imagine; the corruption of the animal or other body ought not to be looked upon, as if it happened in ſome of thoſe imaginary and empty ſpaces that are conceived to be beyond the univerſe, but in this world of ours, where the body, which is deprived of its ſpecifick form, is ſubject to be acted upon by the ſun, the air, and I know not how many powerful agents, by whoſe various concourſe with, and operations upon the body, either the pre-exiſtent, though lately eclipſed forms, may be aſſiſted to ſet up for them-

* Eſſays about ſpontaneous generation.

ſelves,

felves, or new forms may refult from new leagues and contextures of the particles that compofed the body that loft its principal, or, if I may fo call it, fovereign form (as we have largely difcourfed in the lately mentioned papers) * .

These obfervations being laid down, to avoid the neceffity of too much interrupting our future difcourfe, by being obliged to interpofe fome of the premifed explications and other paffages, as obfcure and difficult, as we readily confefs the fubject we are treating of to be, we fhall now adventure to try, whether about *Sennertus*'s doctrine we can propofe any conjectures, that being as agreeable to the phænomena, as his are congruous to the corpufcularian hypothefis, according to which we have hitherto difcourfed of forms.

And (to begin with a conceffion) I allow the learned *Sennertus* and his followers to be in the right, who, without fearing the invidious title of innovators, afferted, that in an animal or a plant there was fomething elfe befides the bare *materia prima*, and the vegetative or fenfitive foul, with its effential faculties. And the inftances they bring to fhew, that in fome parts of fuch bodies there may lurk peculiar forms, which, when the life of the plant or animal determines, come to difclofe themfelves, are probable enough; and the inftances taken by *Sennertus* from the fpecifick virtue that furvives in gathered plants, and particularly the above confidered purgative faculty of rhubarb, are ingenioufly alledged for their purpofe. And it is probable too, what *Sennertus*, according to their grounds, teaches, that this purging property in rhubarb, fenna, &c. as it does not flow from the vital foul of the plant, which is already deftroyed, fo it does not proceed barely from the form of a mixed body as fuch; it being no way likely that fo great a variety of fpecifick properties, as roots plucked out of the ground, and fruits torn from the tree, are endowed with, fhould proceed merely from that general form that belongs in common to compounded bodies as fuch. To which argument I forbear to add that other, wherewith it is feconded by *Sennertus*, though he and others of feveral parties are wont to lay much weight upon it; namely, that thefe properties flow from the fpecifick form, which, even in inanimate bodies, is of a fublime nature, and muft be the author of fuch peculiar virtues, which, according to him, being far above the reach of elementary qualities, cannot be produced by any mixture whatever of the elements: this argument, I fay, I decline to urge in this place, becaufe I elfewhere purpofely examine it, and having declared in what fenfe only it feems to be fafely grantable, I reject the chief fuppofition on which it leans.

To proceed then to the next part of our difcourfe: though (as I was faying) there be fome things about the doctrine of fubordinate forms, wherein I diffent not from thefe learned men, yet there are others, wherein I muft confefs myfelf unfatisfied; for neither do I acquiefce in fome of the notions whereon they ground the things, wherein we agree, nor do I agree with them in fome of the main things they affert: and efpecially having in the paft difcourfe rejected fubftantial forms, it is not to be expected that we fhould either employ them in our explications, or admit thofe explications that neceffarily fuppofe them.

They teach us indeed that the fpecifick form of a body does command all the fubordinate forms, and ufe them but as inftruments to its own purpofes, thofe forms belonging then to the matter, which the fpecifick informs and rules. But for my part, that do not acknowledge in many bodies, that are or may be faid to have fubordinate forms, any thing fubftantial diftinct from matter, I confefs I do not readily conceive, which way this dominion attributed to the fpecifick form is exercifed, nor do I fee any neceffity

* Efpecially in the later part or effay.

of

of admitting any ſuch power in that form, nor that the portions of matter that are endowed with thoſe forms, that are ſaid to be ſubordinate, can, being under the degrees of ſouls, and conſequently unfurniſhed with knowledge and will, pay this preſumed ſuperintendant form any obedience; I mean any other obedience, than ſome ſuch kind of one as the parts of a clock or engine may be ſaid to yield to one another. I ſhould therefore rather conceive the matter thus; when divers bodies of differing natures or ſchematiſms come to be aſſociated, ſo as to compoſe a body of one denomination, though each of them be ſuppoſed to act according to its own peculiar nature, yet by reaſon of the coaptation of thoſe parts, and the contrivement of the compounded body, it will many times happen, that the action or effect produced will be of a fixed nature, and differing from that, which ſeveral of the parts, conſidered as diſtinct bodies or agents, tended to, or would have performed. As when in a balance, by putting in a weight into one of the ſcales, the oppoſite ſcale, though as a heavy body it will naturally tend downwards, yet by virtue of the fabrick of the inſtrument is made to mount upwards. And when an archer kills a deer with his arrow, the bow being a ſpringy body, naturally endeavours to unbend itſelf; and the ſpring being faſtened to the bow, muſt neceſſarily follow the motion of it, and the ſhaft, though a heavy body, and as ſuch, tending directly downwards, is by the forcible impulſe it receives from the ſpring, thrown with ſuch violence (not directly downwards, but in a parabolical or ſome ſuch crooked line) as far more ſtrongly to hit the mark, than it would (if left to itſelf) have ſtruck the ground. So that thoſe actions, which *Sennertus* and others attribute to the conſpiring of ſubordinate forms to aſſiſt the ſpecifick and preſiding form, we take to be but the reſultant actions of ſeveral bodies, which being aſſociated together, are thereby reduced in many caſes to act jointly, and mutually modify each other's actions; and that, which he aſcribes to the dominion of the ſpecifick form, I attribute to the ſtructure, and eſpecially to the connexion of the parts of the compounded body: as in a clock, though all the parts it conſiſts of do contribute to the performance of thoſe things that belong to a clock, as regularly if they intended ſo to do, and did not only concur, but knowingly conſpire in what they do, yet in all this there is no ſubſtantial form to ſuperintend their motion; but the lead (or other weight) tends downwards as it is wont to do; and the hand, wheels, and other parts do only perform ſuch motions as they are forcibly put into by other bodies, which by the deſcending weight (that does not in the leaſt intend what it effects) are themſelves ſet a moving. And notwithſtanding the prodigious operations that men admire in gun-powder, yet not only, as we formerly intimated, this ſtrange power is but the effect of the mechanical texture, and of the way wherein the ingredients are mingled, and as it were contexed; but this artificial mixture is far more ſlight than thoſe made by nature are wont to be. For as the efficacy of the mechanical texture in gun-powder may appear by this, that neither of the ingredients (whether the ſulphur, the nitre, or the coal) is apart able to produce effects any thing near like gun-powder; ſo to convince others how ſlightly the ingredients are mingled, I thought the beſt way was to ſhew how eaſily they may be ſeparated again; to which effect I beat good gun-powder ſmall, and having boiled it a pretty while in a conſiderable proportion of water, by exhaling a ſufficient quantity of the well-filtrated and limpid decoction I obtained ſtore of cryſtals, whoſe figure, taſte, and way of flaſhing upon a quick coal proclaimed them to be good ſalt petre; the black ſtuff left in the filter remaining, if the ſolution had been well made, inſipid enough, and when dried it will not blow up like the gun-powder, but (in great part) burn along with a blue flame like common brimſtone. And for farther proof we may, by boiling this black ſtuff in a pretty ſtrong lixivium, diſſolve the ſulphur, as will appear both by the ſmell that the lixivium will acquire, and by this, that if you filter it, though the liquor will pafs clear enough, and leave the black and coal-like part in the filter ; yet by dropping into it fome quantity of an acid liquor (I ufed fpirit of falt) the fulphureous fmell will be increafed, and the liquor will be made white by the precipitation of fulphureous corpufcles ; whereas if I put fpirit of falt into that clear folution, which (I was faying) afforded me the cryftals of nitre, the liquor, not troubled by any fuch precipitation, would continue limpid as before ; which argued that the falt-petre had not intimately incorporated any fenfible quantity of the fulphur with itfelf, but had been only flightly affociated with it.

And to illuftrate what I faid of refultant actions by an inftance purely phyfical, I fhall fubjoin what I fomewhere mention, with another aim, that by taking a couple of powders fit for my purpofe, one blue and the other yellow, and mingling them in a certain proportion, the mixture exhibited a green colour, which did not flow from any new predominant form, which made the blue and yellow corpufcles fubfervient to its purpofes (for an excellent microfcope fhewed me the blue and the yellow particles fuch as they were before) but only hence, that from the mixture of thofe bodies, the diftinct actions of the blue and the yellow corpufcles did upon the eye make a compounded impreffion, like that made by bodies to whom their fpecifick forms are fuppofed to impart, among other qualities, greennefs. And when vitriol or fublimate are made by art, there needs nothing befides the manner, wherein the faline and metalline particles are contexed, either to contain the parts together, and keep them united into body, or (notwithftanding their not only diftinct, but very differing forms) to enable the mixture they compofe to effect divers things, which neither of them fingle would have performed ; nay and fome of them fuch things (as to vomit, purge, *&c.*) as merit to be reckoned among fuch fpecifick properties, as many of thofe are, which when preferved in vegetables, are thought to argue the confpiring of feveral forms under the direction of a fuperintendent one.

And as in a watch the fpring is really a fpring, and acts as a fpring, whilft it is a part of the watch, though by reafon of its connexion with the other parts it is reduced to concur with thofe other parts towards exhibiting the phænomena proper to the whole engine ; and though the watch were taken all in pieces, the fpring would be a fpring ftill : fo in many compound bodies, befides the fpecifick form, which the body has as fuch, and which may be called its total or general form, particular bodies (by whofe affociation and conjunction it is made up) may enjoy their own diftinct forms, which may therefore be called partial ones ; and thefe bodies, though whilft the whole fubfifts they are part of it, and by their connexion with the reft concur to the operations of the body as fuch, which joint operations are wont to be thofe, that are attributed to the fpecifick form ; yet they do not always fo depend upon it, but that when it is abolifhed, they may retain their own nature ; as a bone will be a bone ftill, whole ages after the animal it belonged to is dead : whence we need not wonder, that divers forms fhould furvive in bodies deprived of their fpecifick form. For indeed thofe that are called fubordinate, may be as true and real forms (nay, and fubftantial forms, if in any living creature, befides man, there were any fuch) as that which bears the title of fpecifick ; and even whilft this is in being, there are many things which compounded bodies perform by virtue of their particular forms, rather than upon the account of the fpecifick : as (not to repeat the newly mentioned inftance of a fpring) in vitriol the friablenefs, tranfparency, and aptnefs to mingle with water, need not be attributed to the compofitum as fuch, but may, for aught we know, be due to the faline corpufcles, which not only retain their own nature (as may be argued from fome vitriols that I have made fince I have been able to feparate and recover them again out of the mixture) but to reduce the metal, they have corroded, into a falt-like body with themfelves. And in

gun-

gun-powder it is manifeft, that the blacknefs proceeds not from the compofitum as fuch, but from the coals, as the nitrous tafte does from the falt-petre. And the fragrancy of a rofe, whilft it grows upon the bufh, need not be conceived to proceed from the foul or life of the plant, fince, when it is gathered, it retains the fame grateful fmell.

And this laft inftance leads me to a farther confideration, wherewith I fhall conclude this difcourfe. We may call to mind what was obferved a little after the beginning of it, of the arbitrary, or at leaft not fufficiently fettled ufe of the word *form*; and that it not feldom happens, that thofe things, upon whofe account we attribute this or that form to a natural body, are but very few of thofe many attributes that belong to it. Now the form of a body being really no more than a convention of accidents, whereby the matter is ftamped and denominated, it is very confonant to reafon, that oftentimes hoftile agents or caufes may deprive the matter of thofe accidents, which conftituted the fpecifick form, and yet leave the reft, which, according to the law of nature, ought to continue there, till fome competent agent put the body out of that ftate, wherein, upon the form's deceafe, it was left.

And to clear up this matter, we may confider, that the fame body may have a two-fold modification, and be thereby fitted for two, if not more, ftates and kinds of operations, not neceffarily dependent upon one another. For as the fpring of a watch by virtue of its texture is an elaftic body, and upon the account of another is iron, and therefore though being caft red-hot into cold water it will become ftiff and brittle, and confequently ceafe to be a fpring, yet it will continue iron, that is, a hard metalline body eafily fubject to ruft, capable of ftriking fire with a flint, and of being attracted (as men commonly fpeak) by a load-ftone, and of attracting a magnetical needle; fo in a rofe, for inftance, we may diftinguifh or confider a two-fold modification of the matter, one, whereby it is fitted to receive from the bufh it grows on a certain peculiar and fpirituous fap, by whofe intervention and concurrence it has nourifhment and growth, and confequently exercifes vital functions as a part of a living plant; and another which does not fo much require the acceffion of fluid and moveable parts, but confifts rather in the texture of the more ftable parts: and this texture being commonly more permanent and durable than the other part of the modification (confifting much in the peculiar motion of a fluid fubftance) wherein the life participated by the rofe confifted, may laft, when the flower is deprived of its foul and fpecifick form by its avulfion from the bufh, and retain thofe qualities, as well occult as manifeft, that naturally refult from a parcel of matter fo contrived.

I may fomewhat illuftrate my meaning on this occafion, by making a comparifon betwixt a living creature and a mill. For as a mill is capable of performing divers things only when the water, that paffes through certain of its parts, put them, and, by their intervention, others into motion; fo there are divers things, that are not performable by a plant, unlefs when it is irrigated by a vital liquor. And as a mill may neverthelefs retain the nature of a ftructure ufeful for other purpofes, though the drought of fummer have perchance made it lofe, or the froft have congealed into ice the water that ufed to drive it; fo although the foul of a plant be deftroyed, or ceafe to act, the body may, upon the account of the more permanent ftructure of its ftabler parts, retain a fitnefs for diverfe of the fame purpofes it ferved for before. And if it were here pertinent, the comparifon might be carried on a little farther by adding, that as when a mill does upon either of the lately mentioned accounts ceafe to perform the peculiar operation of a mill, as the wood, iron, and other materials of that mill are not deftroyed; fo neither does the water vanifh into nothing, but either lofes its motion, and by being congealed exchanges the name of water for that of ice, or elfe is diffipated and fcattered into exhalations, which contain all the ſubſtance that ever the water had, that, which is loſt, being but the uſual manner of coexiſtence of the water and the mill it was wont to drive: ſo when a plant is pulled out of the earth, or a roſe from the buſh, as the diſmembered part of the plant may retain the texture of its more ſtable parts; ſo the ſap or juice, that were wont to enliven the body, does, though inviſibly, remain either in the form of ſteams exhaled into the air, or perhaps in parts condenſed and intercepted upon the loſs of its wonted agitation in the imperceptible cavities of the fibres, and other parts of the plant, ſo that nothing, that is ſubſtantial, periſhes, but only the particular modification, that reſulted from the peculiar kind of union of the more permanent parts with thoſe congruouſly ſhaped and fitly agitated fluid ones, that permeated them. As to ſome purpoſes, the example of a wind-mill, being ſet on work merely by the impulſe of the air, may be more appoſite than that of the water-mill; but neither of them affords any more than an imperfect compariſon in this regard, among others, that whereas the mill itſelf by loſing even for a very long time, the motion it was wont to be in, is not thereby conſiderably impaired, becauſe of the ſolidity of the materials it is made up of; in vegetable, when that fluid ſubſtance, whereof the ſoul chiefly conſiſts, quite ceaſes to be influent, one of its chief functions being to repair continually, by aſſimilated or tranſmuted aliments, the waſte that was continually made of looſer parts in the body it belonged to, the ſame agents or cauſes, that deſtroyed the life of the plant, are wont likewiſe to produce or occaſion ſuch a diſcompoſure in the texture of the remaining part (eſpecially thoſe that are more tender or more ſlightly connected) that they quickly become unfit to be animated again, though a fluid ſubſtance, like that which was wont to irrigate it whilſt the vegetative life laſted, ſhould be again communicated to it; but yet even in this regard the difference betwixt a mill and a plant is not always ſo vaſt as one would imagine. For in claſſick authors we have relations of a ſtaff or a pike made of a durable wood, that many years after the tree had been cut down, being caſually ſtruck into the ground, took root there. And as for the roſe of *Jericho*, as they call it, a late modern writer, followed, as I remember, by another naturaliſt of good account, affirms, that divers years after it is gathered, and ſeems to be quite ſhriveled up and withered, it may by the help of water be ſo far recovered, as to be plumped up again, and diſplay its leaves almoſt as if it had not been long ſince gathered. And I myſelf have, not without ſome wonder, obſerved, how very long a plant of aloes torn from the ground, and hung in the air near the cieling of my chamber, would not only continue ſucculent, but (perhaps after ſome years) be capable of being made to perform acts that are wont to be aſcribed to vitality and growth, upon the dexterous adminiſtration of a convenient liquor: and even ſome animals themſelves are not ſo very unlike to theſe plants, and conſequently to engines, as one would think. For that which children are ſaid to do for ſport about reviving drowned flies, challenges a more ſerious conſideration than were fit for me to inſiſt on now, and deſerves to be both heedfully experimented, and ſeriouſly reflected on by a naturaliſt. I choſe to try it chiefly upon waſps and bees, rather than upon flies, becauſe their bigneſs renders the phænomena more conſpicuous; and having drowned them ſo, that, if let alone, they would not in probability have ever recovered, I found that the heat of the ſun would recover them, as well as it has been obſerved, that warm aſhes would recover flies (ſo that theſe trials argue celeſtial heat to be as little more as leſs vital than elementary): and the degrees and manner of their recovering again the operations of life ſuggeſted obſervations, not unworthy to be taken notice elſewhere, though not fit to be delivered here; where I ſhall not ſo much as mention what with warm applications we have done, to revive the expired motion of the parts even of perfect and ſanguineous animals, when they ſeemed to have been killed;

killed; because I fear that the excursions I have unawares made already will be looked upon as too much a digression.

Wherefore, to take up my discourse where a while since I left it, I shall proceed now to observe, that even in a body that has lost its specifick form, the noble qualities that remain do not always flow from the form of the entire body as such, but from the peculiar form of some particular parts of that body, which being separated from it, though perhaps the more stable parts that remain, will keep the visible structure from being manifestly altered, yet this remaining body will be quite deprived of the noble properties we were mentioning; as may be gathered as well from what has been above mentioned out of *Sennertus* about drawing an extract from rhubarb, in which its whole purgative virtue resides, as in some preparations of cinnamon, and divers other substances endowed with fine parts, which upon the loss of those parts remain but the carcasses of what they were. And even in the gross bark of oak, tanners find that when the water has extracted the dissoluble parts, or time has wasted some subtile parts, and changed the texture of the rest, though the bark retains its outward form, they cannot make use of it as they might have done before. And (as I formerly intimated *) besides this pre-existent and surviving modification, it is in divers cases very possible that new qualities and properties (whose principle may be called a form) may be disclosed upon the abolishing of the specifick form, though they were not actually in any part of the matter, but are produced in it by a concurrence of the texture and dispositions left there by the late form, and the operation of external agents. As when out of the flesh of a dead animal there is generated musk; for not only those seminal rudiments that actually were latitant in the corrupted body, gain opportunity to set up for themselves, and become perfect insects, or other creatures of their own kind, but the external agents, to whose action, according to the common course of Providence, deceasing plants or animals happen to be exposed, do oftentimes (not without the foreknowledge of the most wise Author of nature) so agitate the small parts of the widowed matter, and perhaps by associating themselves with them do so alter their texture, and thereby introduce a new modification, that by the conjunction of the former dispositions that were regularly left in the matter with these new agents, promoted by a concourse of favourable circumstances, there may be produced new and noble forms (however not vital ones). As when a lime-stone, being calcined and left in the open air, will in tract of time, as I have particularly observed, by the assistance of congruous particles it meets with there, and befriended by the more catholick causes or physical mutations, afford true and inflammable salt-petre; and I have seen certain marcasites that, being burnt and exposed to the air in convenient places, would have such a change produced in their parts, as, after a due time, to afford an efflorescence, which both by the colour, taste, and operation appeared to be vitriol.

But of such matters no more at present. I will rather take notice, in prosecution of what I was not long since observing about the two-fold modification of living creatures, that I fear we sometimes attribute to the specifick form or soul things that may be well enough performed without it by the more stable modification of the body, befriended by an easy concourse of natural agents. Thus, though the exclusion of excrements be unanimously ascribed to the soul, which for that purpose is said to be endowed with a peculiar faculty, that they call expulsive; yet it has been observed and affirmed by many, that divers times the excrements have been discharged out of the bodies of men a good while after they were unquestionably dead; so much (it seems) of the former structure of

* Especially in the 5th and 6th considerations.

the parts remaining, as sufficed to co-operate with the excrements themselves, changed by the death of the animal, to that exclusion. And thus (to add an instance of another kind) though the maturation of fruit be a great, and, as the schools speak, a perfective alteration, which is supposed to be wrought by the vegetative soul of the plant; yet it has been vulgarly observed, that apples and grapes gathered before they be ripe, and laid on heaps together, will ripen well enough afterwards (and the example were more eminent in medlars, if what some call their ripeness, others did not call their rottenness). And very remarkable is that account which the inquisitive *Oviedo* gives the emperor *Charles* the fifth of the Ananas, if I mistake not the name; which having mentioned as one of the considerablest fruits he met with in the *West-Indies*, he takes notice that though, notwithstanding their largeness, they grow in clusters, yet they must be gathered whilst all but one are green. For as soon as the first begins to be yellow, the whole cluster must be taken off, leaving the rest to ripen, and attain to the same colour in the chamber, which they will very well do. The learned † *Josephus Acosta* speaks thus of the fruit of the plane-tree, to the same: "This fruit (says he) inclines more to "cold than heat."——They are accustomed to gather the boughs or clusters (as I have said) being green, and put them into vessels, wherein they ripen being well covered, especially when there is a certain herb mingled with it, which serves for this effect. But the diligent *Piso* speaking of those Brasilian plants, which he calls Pacoeira and Bananiera, punctually relates that which comes up yet more fully to our present purpose. For not only speaking of the fruit, he says *, *continentur plerumque in uno ramo quatuordecem aut sedecem numero, ut ita una planta proferat septuaginta aut octuaginta, qui subinde virides avulsi, nunc in ædibus, nunc in navibus suspenduntur, donec justam maturitatem & flavedinem consequantur:* but adds this memorable passage concerning the lopped boughs themselves; *Ramus autem ille fructibus onustus, interea dum illi maturescunt, augetur, floresque semper protrudit ex corpore illo foliaceo, &c.* On this occasion I might here add, that even in our cold climate, onions and some other bulbous plants will in the spring-time shoot out of their own accord. And I have taken pleasure to keep potatoes in the air, to observe how at that season, when they usually begin to sprout in the ground, they would put forth leaves at so many of the little holes or dimples, as to give themselves a verdant livery: but that not being willing now to examine, whether or how far an animated seed may have an interest in these last mentioned productions, I will rather take notice, that even in animals some things that are confidently presumed to be the proper effects of the animal's soul, may be really performed by the texture of the body, and the ordinary and regular concourse of external causes. For (not here to repeat what I lately noted of the exclusion of excrements in dead bodies) though the nails of a man are nourished, and do grow as well as other parts; and though the hair in most animals be sometimes, even in determinate parts of the body, peculiar to the species of animals to which it belongs; and though in man hairs do not only grow, but in the disease called the *plica Polonica* it appears to participate of blood (since the hairs being cut, weep out that liquor) yet nails themselves are observed to grow in dead men. And that they do not so only (as is supposed) for a little time, whilst the impressions left by the soul upon the carcass are yet vivid and recent, but for a much longer while than has been imagined, I have been with pleasure informed by a memorable observation I met with in the experienced ‡ *Paræus*, who speaking of a body that he by imbalming preserved for more than twenty-five years, he affirms it still remained whole and sound,

† *Acosta, lib.* 4. *p.* 269. * *Piso, Natur. Hist. cap.* 21. *pag.* 155. ‡ *Paræus*, lib. 28.

and

and that, as to the nails, he found, that *having often pared them, he ſtill obſerved them to grow again to their former bigneſs.*

I KNOW the patrons of *Sennertus*'s opinion look upon it as a clear and cogent argument, to prove the ſoul's performing almoſt all things done in the body, that in the corpſe of a man or other animal newly dead, though the organization remain the ſame, yet all the animal and vital functions perfectly ceaſe. But beſides that I have already taken notice, that ſome things wont to be attributed to the ſenſitive ſoul may be obſerved in a body avowedly dead, I confeſs, that this argument ſeems to me, though very ſpecious, yet grounded upon what is but precarious. For though it may be true, that the viſible fabrick may continue for a while without any manifeſt alteration, yet who can aſſure us that the internal organization is not conſiderably changed and vitiated? For the body of an animal is an engine, that conſiſts not only of ſolid and ſtable parts, as bones, muſcles, ſkin, *&c.* but of divers ſoft ones, as the brain, nerves, *&c.* and of ſome that are fluid, as the blood and other liquors; and, which is in our caſe exceedingly conſiderable, requires a convenient coaptation, or compoſition of all theſe: whence it follows, that the external frame of the body remaining unaltered, yet upon death there may be great and ſad alterations in the texture of the blood and humours, and the contexture or ſtructure of other internal parts. And theſe changes may quite ſpoil the organization of the body, and make it unfit to perform the wonted functions of ſuch an engine. Thus we ſee, that in dead bodies, even whilſt they are warm, the blood oftentimes coagulates in the veſſels, whereby the circulation, that grand wheel of life, is ſtopped. And in ſudden palſies, though there be uſually no viſible change in the affected limb, yet it loſes ſenſe, or motion, or both: and not only in ſyncopes or great ſwoonings, and in apoplexies, a great part of the animal functions are for the time ſuſpended or unperformed; but even in ſo natural a ſtate as ſleep, the body appears not to move, nor do ſounds and odours affect the ſenſes, as when one is awaked, though the ſoul be preſent in the body, and the ears and noſtrils be open. And how great changes in the nature of liquors, and conſequently of the blood, may be produced without any viſible alteration, may be gueſſed at by what often happens in wine upon thunder; for that liquor, which was pleaſant, ſpirituous, inflammable before, ſpeedily degenerates into a ſour and uninflammable vinegar. Which inſtance will not, I ſuppoſe, appear inconſiderable to thoſe many modern philoſophers and phyſicians, that would have life maintained by a *biolychnium*, or vital flame continually burning in the heart, and fed by the ſpirituous parts of the circulating blood.

IT were not perhaps time miſpent to proſecute ſuch inquiries, as we have lately touched. But though I did not want leiſure, I ſhould be diſcouraged by conſidering, that even ſome of the things I have already delivered, may be queſtioned by thoſe who take not the word *life*, and ſome other terms by me imployed, in the ſame ſenſe that I do. And indeed it is very difficult that men ſhould avoid falling either into miſtakes, or into unprofitable diſputes, if they diſcourſe largely of ſuch themes, where the names that are of a very common and neceſſary uſe, have (yet) their ſignifications very little ſtated or agreed on. For *life*, for inſtance, is a word, whoſe meaning is not yet defined, and is applied to ſubjects, that are exceeding different; for it is aſcribed not only to all ſorts of animals and plants, but by many chymiſts and mineraliſts to ſtones and metals growing in the bowels of the earth. Nor is it attributed only to things corporeal, but to thoſe that differ *toto genere*, as they ſpeak, from them; namely, to ſeparate ſouls, angels good and bad, and to God himſelf. Nay, what that is even among animals, wherein it conſiſts, is not yet ſufficiently agreed on. as may appear by the diſpute among the modern naturaliſts, whether prolifical, but as yet unhatched, eggs have life

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or no? and whether flies be really dead in winter, which some affirm them to be, not only because those insects seem to be devoid of sense and motion, but because they place the notion of life in a constant circulation of the blood, or some analogous juice, and a distribution of the aliment thereby performed to repair the wastes of the body; whilst others, on the contrary, think them to be rather benumbed than dead, because regularly recovering the manifest actions of life in the spring (or oftentimes before, if a due application of heat be made unto them) it cannot be supposed, that they were during the winter really destitute of life: death being a privation, which, by physical means, admits not of a return to the former state. Nor are the boundaries and differences betwixt the life of a plant and that of an animal so settled and defined, but that divers not impertinent questions may be made about them; and particularly it may be doubted, whether some, as nails, hair, horns, &c. that belong to the body of an animal, may not for all that (even whilst he is alive) have the nature of a plant, to which the part where it grows serves as the stock does to a graft for a soil, and is but an appropriated one. But to do more than point at such matters, would add too much to the digressions, of which I fear the passed discourse may be thought to be guilty already. I shall not therefore add any thing at present further about the subordinate forms of plants and animals: but in regard I ventured, about the beginning of this little tract, to ascribe subordinate forms to divers bodies that never had life, which I doubt will seem a paradox to many, I think it will not be amiss to apply the chief points of our doctrine about subordinate forms to inanimate bodies, because this course, as it will invite me to make some new, though short additions, to illustrate and enlarge some points, so it will help to call to your memory most of the heads of that doctrine, which the several excursions whereto divers subjects tempted me, may have kept you from taking a distinct view of. And for order's sake I shall cast the main things, I would have considered, into distinct propositions, with short comments annexed to them; having only intimated in general once for all, that you will not, I hope, wonder, that I should often use for examples, such bodies as are looked upon as factitious, if you recal to mind what I have formerly said to shew, that the difference betwixt them and those that are confessedly natural, is not always near so great as men are wont to imagine. To which I shall now add, that in the following discourse they are often imployed, not so much to prove, as to illustrate the notions on whose occasion they are alledged; which sure they may very properly do. And among the bodies themselves, in whose productions man's power or skill has a share, I reckon that there is a great difference between those, wherein man gives an outward shape, such as himself designs, by tools of his own making, that are always external to the produced body, and those (such as are most chymical productions, besides others) wherein his chief work is to apply physical agents to patients, by which means it oftentimes comes to pass, that (as in productions, that all allow to be natural) the instruments he works by are parts of the matter itself he works upon, or at least intrinsical to it. But of this more perchance elsewhere; I come to the propositions themselves.

THE word *form* is of a sufficiently indeterminate signification.

THIS I have already had occasion to shew, that it can scarce be denied by them that shall consider, though it be a technical word or term of art, yet men have not intelligibly defined and agreed, on how many, or what things they are sufficient to intitle a portion of matter to a determinate and distinct form. For besides that there are I know not how many bodies, such as treacle, beer, gun-powder, coal, ink, &c. about which men seem not so much as to have considered whether they ought to have particular forms ascribed to them (or to be looked upon only as factitious things) there are other bodies, that have been taken notice of, about which even the Peripateticks dispute, whether

whether they ought to have particular forms allowed, or no. For not only ice is by ſome made to be a diſtinct kind of natural body, whereas others will have it to be only water altered, and thereby deprived of its fluidity, not its form; but even touching the elements themſelves the ſchool-men fiercely diſpute, a whole party denying them to have any other forms, than the firſt qualities by which they are wont to be diſtinguiſhed. If I affected paradoxes, I might here add, that perchance there may be bodies, which, as they may be diverſly conſidered, ſeem to have a title to more than one form, and upon that ſcore may puzzle the ſchools about the aſſignation of their forms. When, for inſtance, I have (though not without ſome difficulty) reduced lead *per ſe* into a body like that which chymiſts call *vitrum Saturni*, and which they make by the addition of flints ſand; and it is not eaſy to determine whether this ſhall be one of thoſe kind of bodies that are called metals, and in our inſtance is only diſguiſed, or belongs to that other kind of bodies we call glaſs; for it ſeems to have the properties of both. For, like lead, it is very ponderous, and diſſoluble in aqua fortis and ſpirit of vinegar, which diſſolve not common glaſs; it affords a very ſweet ſolution, as lead is wont to do; and which is more, it may without addition by bare heat be quickly reduced in great part into true and malleable lead. On the other ſide, it is a body fuſible, tranſparent, and brittle, which are the three grand properties of glaſs; beſides which, I have obſerved in it ſome others that will be more properly taken notice of elſewhere. So likewiſe when miſletoe grows (as I have ſometimes ſeen it) to a very great bigneſs on a hazle, which (you know) is but a very ſmall tree; or when an apricot or peach is inoculated, and proſpers upon the bough of a plum-tree, the reſt of whoſe branches bear plums as they did before (to which I might add ſome inſtances of trees that I have ſeen to bear more kinds of fruit); and when red or blue amel is made, which conſiſts of calcined tin, which they call putty, and of the ſalt and ſand (or fuſible ſtones) whereof the glaſſmen make what they call their fritta, and of ſome burnt copper, or ſome other metalline pigment, moſt, if not all, of which ſo differing ingredients may perchance be re-obtained out of the amel, which has divers properties of the reſpective bodies it conſiſts of, and yet wants others of them: if, I ſay, ſuch examples as theſe, to which I could add ſeveral others, were propoſed, it would perhaps ſomewhat perplex the ſchoolmen to accommodate them fairly to the vulgar doctrine of forms, at whoſe framing probably ſuch inſtances were not dreamed of.

II. It is not eaſy to decide the nobleneſs of forms.

THIS point alſo has been partly handled already, which will make it the leſs needful to inſiſt long upon it; and indeed, beſides that nobleneſs is rather a civil or political than a phyſical qualification, it is oftentimes difficult enough to determine even in this ſenſe of nobleneſs, which of the two forms is the moſt noble. Of this difficulty we have already elſewhere given ſome examples, to which we may add, beſides the lately mentioned inſtance of the glaſs of lead, that of antimony made *per ſe*; crude antimony being fitter for ſeveral purpoſes, both mechanical and medical, than the vitrified calx; and this again being better for divers other uſes than that which has not been freed from its more fugitive parts.

IT ſeems it was diſputable among the ancients, whether or no their electrum (which learned men tell us was a compoſition of gold and ſilver) was a nobler thing than either of thoſe metals. And it may be queſtioned, whether, when chymiſts have made a precipitate of gold and mercury, the produced powder be a nobler thing than the gold alone. For chymiſts think it worth while to put themſelves to much trouble and ſome charge to bring gold, by the addition of mercury, to this new ſtate; and therefore if a ſpagyrical phyſician were judge, he would think that in ſuch a medicine the gold is improved by the change; but if a goldfmith were to be judge he would conclude that gold, being the nobleft of metals, an alloy muft needs imbafe it; and he would take the pains, by melting it with borax or fome other additament, to free the gold from the quickfilver, and reftore it to its priftine form.

It may alfo be difputed, whether, though in living creatures the ultimate form be wont to be more noble than its previous harbinger, it may not be fometimes otherwife in bodies inanimate, as well as in the productions of art. I will not urge for an example to this purpofe, that when corn is ground in a horfe-mill, though the whole aggregate, confifting of the horfe, the wheels, grindftones, and other parts of the mill, be looked upon as but one engine in reference to the ufe of the whole, which is comminution of grain; yet the horfe, though contained in the mill, and looked on as a part of it, is of a much nobler nature than the engine, to whofe effects it co-operates. This example, I fay, and others of the like kind (as that of a turnfpit-dog, included in a wheel, to make it go round) I fhall not prefs, but rather give this for an inftance, that when an artificer, who makes filver fodder, adds to the filver a certain proportion (which I have elfewhere fpecified) of brafs or copper, and melts them together, though he thereby obtains a mixture of good and frequent ufe for joining together the pieces of brafs and filver inftruments, and ftopping holes and cracks in them, yet it may be much queftioned, whether this brittle fubftance be not lefs noble than the filver alone was. And when a plant, that grows by fome petrifying fpring, by imbibing that water is at length turned into a ftone; though the rarity of fuch things makes men prize them, yet it may well be queftioned, whether the fupervenient form be not lefs noble than that which the plant had before.

III. In divers bodies the form is attributed upon the account of fome one eminent property or ufe; which, if it be prefent and continue, though many other things fupervene or chance to be wanting, the matter is neverthelefs looked upon as retaining its form, and is wont to be allowed its ufual denomination.

An example of this we may be furnifhed with by our lately mentioned inftance of *vitrum antimonii:* for the account, upon which we take a body to be a glafs, being chiefly fufiblenefs and tranfparency, this antimonial preparation, by virtue of thofe qualities, paffes without fcruple for glafs, and would in effect be taken for yellow or red glafs (according as it happens to have more or lefs of tincture) by an indifferent fpectator, who were afked what it is. And yet this fubftance is not only by its folubility, unfixednefs, and difpofition to afford a regulus, very differing from common glafs, but retains fo much of an antimonial nature and properties, as to be vomitive and purgative, as well as *crocus metallorum.* It can fcarce be unknown to chymifts that there is a vaft difference between thofe liquors that are expreffed out of olives, almonds, and other unctuous vegetables, and thofe fine effential oils, as they call them, which are drawn by the help of water in limbecks; and even of thefe, fome are wont to fwim upon water, as oil of annifeeds or nutmegs; others to fubfide, as the oil of cloves and cinnamon, &c. well drawn; and all thefe effential oils are very differing from the oil of guaiacum or of box, and other empyreumatical oils that are diftilled in retorts by the violence of the fire; and thefe do as much differ from expreffed oils as from effential ones: and yet all thefe fo differing liquors are reckoned among oils, becaufe they agree in this, that they are fluid bodies, unctuous to the touch, and mingle not with water. And fo although fome forts of falts be very fugitive (as the volatile falts of hartfhorn, urine, &c.) others very fixed (as that drawn from the calx of tartar, and from the afhes of wormwood, afh, celandine, and other plants); and though fome faline liquors, as vinegar and juice of lemons, are acid, and diffolve pearl, coral, &c. which lixiviate

falts,

ſalts, whoſe taſte is fiery, will precipitate what the others have diſſolved; yet all theſe are numbered among ſalts, becauſe they agree in the accounts upon which we allow bodies that denomination; namely, their being very ſapid, and readily diſſoluble in water. Examples to the ſame purpoſe with the foregoing I could give you in flame, ſmoke, glaſs, coal, and divers other ſorts of bodies. And indeed, by reaſon of the unſettled notion and almoſt arbitrary uſe of the word *form*, I have obſerved it to be ſo uncertainly applied to the conſtituting of the diſtinct claſſes or kinds of bodies, that I have doubted whether divers of thoſe forms by which ſuch kinds are conſtituted, be not a kind of metaphyſical conceptions, by virtue of which, bodies very differing in nature are comprized in the ſame denomination, becauſe they agree in a fitneſs for ſome uſe, or in ſome other thing that is common to them all; as whether a bullet be ſilver, or braſs, or lead, or cork, if it ſwing at the end of a ſtring, it is enough to make it a pendulum; and whether a burned body be chalk, or rag-ſtone (which is very hard and coarſe) or alabaſter, which is a ſoft and fine ſtone, or an oyſter-ſhell, or a cockle-ſhell, or a piece of coral; yet if it have been calcined to whiteneſs, it is lime, rather than ſuch true phyſical forms, as are ſaid to make the bodies that have forms of the ſame denomination, to be of the ſame ſpecifick nature. However theſe forms ſeem to be very generical things, and more ſuch than is commonly heeded. And I have alſo ſometimes queſtioned, whether ſome of thoſe things, upon whoſe ſcore men conſtitute bodies in this or that ſpecies or claſſis, be ſo properly the true and intrinſick forms of thoſe bodies, as certain ſtates of matter, wherein bodies very differing in nature may agree. As water, wine, and I know not how many other differing liquors, may each of them apart be made, by congelation, to paſs into that ſort of body we call ice. And not only the tallow and greaſe of animals, and the expreſſed oils and ſpirits of fermented vegetables (ſome whereof differ exceedingly among themſelves) but alſo (as I have tried) divers mineral and even metalline concretes may be made (ſome of them without deſtruction of their nature) to paſs into that claſſis of body we call flame.

IV. By reaſon of the conjunction or connexion of the parts that make up a *totum* (or at leaſt an aggregate of bodies that, for their connexion, are looked upon as ſuch) it will often happen that ſeveral things will be performed by the joint or concurrent action of theſe united (or coherent) parts.

These kinds of operations are of kin to thoſe mentioned by the ſchoolmen, when they tell us that ſome things are done by divers agents *actione communi*; as when a man diſputes *vivâ voce* ſyllogiſtically, the rational ſoul, which dictates the words, and the vocal organs that pronounce them, are *actione communi* the efficients of the pronounced ſyllogiſm. But to give an inſtance nearer to our purpoſe; when a bullet is let fall upon a level pavement, though it touches the body it falls on but in a very ſmall part of its ſuperficies (geometricians having demonſtrated that a perfect ſphere can touch a perfect plane but in one point); yet the plane receives the action of the gravity of the whole body; thoſe parts that do not immediately come to touch it, ſtriking it nevertheleſs by the intervention of thoſe that do. And ſo likewiſe in a boat, the limbs and clothes of a man that ſtands upon the deck, and all the parts of a watch, if he carry one in his pocket, gravitate concurrently on the veſſel, though only the ſoles of his feet or ſhoes do immediately preſs upon it, and the wheels and other parts of the watch may be moving at the ſame time very differing ways. Now in organical bodies, and divers others, both natural and factitious, thoſe things that are performed by the parts as in a ſtate of conjunction, and, as it were, conſpiring, or (if you will have it ſo expreſſed) *actione communi* (which action may ſometimes be ſucceſſive) are oftentimes aſcribed to the form: as in a watch, moſt of the chief of the phænomena do ſo depend upon the concurrent action of the ſeveral parts, that few of them can be out of order, but that they will hinder thoſe phænomena to be at all, or at leaſt to be well and regularly produced.

V. We may yet in a ſound ſenſe admit, that in ſome bodies there may be ſubordinate forms.

What I mean by *a ſound ſenſe* in this propoſition may be clearly collected from ſeveral paſſages of the paſt and remaining parts of this diſcourſe, where we carefully exclude thoſe ſenſes, in which we do not allow the received doctrine of ſubordinate forms. Wherefore, having met with a couple of plauſible objections ſtarted ſince the death of *Sennertus*, againſt the admitting them at all, we ſhall here briefly examine them, not only to make them appear not to be cogent, but becauſe ſome of our anſwers may ſerve for reaſons why we admit the forms diſputed of.

The firſt argument we are to conſider, is, that a body can have but one form, being but one body.

But though to this I might frame an anſwer from the looſe and indeterminate ſignification of the word *form*, yet it may be directly replied, that though a body can have but one total and adequate form, yet nothing hinders but that its parts may have their partial forms ſubordinate to that: as the ſteel-ſpring and the braſs-wheels of a watch may retain their diſtinct metalline forms though the watch they compoſe be but one. And it is not wholly to be pretermitted on this occaſion, that among the ſchoolmen themſelves there has long been a conſiderable party who aſſerted with many of the ancients, that in compounded bodies the elements retained their reſpective forms notwithſtanding the new form that belonged to the mixed body as ſuch.

Secondly, it is objected againſt the ſupervening of a higher form, that a body being already complete in its own kind by its own form, no other form can accrue to it without making that which they call *Ens per accidens*.

To this I anſwer, that the notion of an *Ens per accidens* belongs rather to metaphyſicks than natural philoſophy; and in what its eſſence conſiſts, is ſtill ſo hotly diſputed among the moderns, that till the buſineſs be agreed on, or at leaſt more clearly ſtated, an argument drawn from thence will not much preſs us. And indeed when I conſider that the ſchools themſelves are fain to allow the ſoul and the body, that is, an immaterial ſubſtance and a corporeal, to make up a man, who according to them is *unum per ſe*, and not *per accidens*, and that the ſame ſchools ſcruple not to teach, that the rational ſoul, which is a ſubſtance, and the underſtanding and will, which are ſaid to be its faculties, and ſo its accidents, to make *unum per ſe*; I cannot but think, that, by a parity of reaſon, that name, predicate, or qualification, may well be taken in as large a ſenſe as is requiſite for our purpoſe. And indeed if the parts of a body, whether merely natural or factitious, be by their union or conjunction brought to become the principle of a property or operation, which belongs to neither of them ſingle; I ſee not why ſuch a body may not paſs for *unum per ſe*, as well as divers bodies that are wont to be looked upon under that notion. But to proceed to our further anſwer, not here to urge that a whole ſect of the Peripateticks themſelves maintained (as we newly noted) the forms of the elements to remain in the mixed body, which notwithſtanding they hold to be very conſiſtent with the unity or oneneſs of that compounded body, it may be anſwered further, that though a body by its own form be complete in its own kind, yet it may be ſuch, as to be capable of being advanced to a nobler ſtate by an acceſſion that ſhall not ruin its former nature, but enable it to cooperate to nobler actions than its former could reach to. As when a ſpring is made part of a watch, it does thereby, without loſing the nature of a ſpring, mainly contribute to the nobleſt phænomena of ſo curious an engine; and

and the ingredients of gun-powder, by the superinduction of the form or new contexture they acquire by being compounded into that concrete, are each of them enabled to cooperate to the performance of things far exceeding the utmost it could do before. Nor will it follow from this superinduction of forms, that there may be a form of a form as well as of matter, but only that to a body that has already a form, an ulterior form may supervene, wherein we see no absurdity. But of this point more elsewhere; only in this place it will not be amiss to take notice, that in our proposition we thought to employ the words, *some bodies*, and, *may be*, because that though in living creatures we may often meet with subordinate forms either properly or less improperly so called, yet that in bodies inanimate this happens not so often, you will be induced to think by what you will find said upon the last proposition of this discourse.

VI. The supervening of the new form is often but accidental to the præexistent form, and (then) does not at all destroy its nature, but modify its operations.

For illustration sake, let us consider a needle that is not yet touched by a loadstone: this needle has its own form as a piece of steel, as well as its figuration as a needle; but when afterwards it comes to be excited by the loadstone, there are then new and wonderful properties superinduced, and this needle is able to point regularly north and south, and attract other needles, and communicate a verticity to them, and is fit for much nobler uses than it was before. And this new modification does so regulate its motions, that whereas before it was indifferent, if it were nicely poised, to rest at east and west, or at any other point of the compass, it is now determined to keep moving till it points north and south, and to rest in that position. And yet by drawing this magnetick needle after a certain manner upon the pole of a vigorous loadstone, you may in a trice deprive it of all its accessional faculties; notwithstanding which it will remain as true a steel-needle as it was first.

And perhaps we shall need to add but a little reflection on the formerly mentioned instance of the spring of a watch, to declare intelligibly what it is, that the structure or modification, whence the *forma totius* according to us results, does to a body endowed already with its own form. For as the spring, though it retains its own nature, and acts according to it, yet by the contrivance of the watch, it is not only so pent in, that it cannot fly out to its full extent as else it would, but by the same contrivance has its incessant endeavour to stretch itself so moderated and managed by the wheels and balance, that it mainly concurs to set all the other parts a moving, and perform what is done by a watch as such: so in natural bodies, that which is performed by the supervening of a higher and total form is, that by virtue of the connection and structure of the parts introduced with this new form, the action of the particular parts, though they retain their own partial forms, and act as far as they can, according to them, is so mastered or otherwise modified, that they are brought to concur to those things that are done by the whole body as one agent, and become subservient to the operations that are proper to the body in its new and ultimate capacity. So when a piece of lead is without addition vitrified by the mere action of the fire, this happens to the body upon its acquiring the form of glass; that whereas before the metalline particles were so inconveniently situated, and perhaps shaded, that they denied passage to the beams of light, and by reason of their contexture composed a body that was very flexible, they become now to be so ranged and otherwise altered, that they freely admit the light to traverse them, but admit not of being freely bent as before. And when salt-petre, by the addition of a small proportion of brimstone and coal, is made into gun-powder, this accrues to it from its acquired modification, that if a little fire fall on it, it will not, as before, leisurely consume, and leave behind it a considerable portion of the whole body (perhaps a third part or more)

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in the form of a fixed or alkalizate salt, but will fly away all at once, and leave little or nothing behind it.

VII. Besides the specifick actions of a body that harbours subordinate forms, there may be divers others, wherein some of the parts or ingredients may act according to their particular and pristine nature.

This might be well enough gathered from what we lately delivered, when we shewed that the total and specifick form has not such a dominion over the partial and subordinate ones, as the patrons of these forms have imagined. For though, by virtue of the modification of the whole, the operations of the parts or ingredients are so compounded and guided, and in some cases, as it were, over-ruled, as to concur to those operations that belonged to the *totum* as such, and are requisite to be performed *actione communi*; yet in other respects, and as to other purposes, as it is not necessary that such bodies as we speak of should have their parts entirely under the dominion of the ultimate form (that is subservient only to the operations and uses of the *totum*, as such); so those parts may in such cases act according to their distinct and particular qualifications. This answer, I say, may be deduced from what has been above delivered; but I chuse rather to clear the matter by two or three particular instances, which may shew that the same may happen to several bodies that is manifest in a watch, where, though the form of the engine do in many things make the spring and other parts concur to perform the operations proper to such an engine, yet the wheels may look bright and yellow, the spring may move a magnetick needle freely placed, and other parts may do other things, not by virtue of the form of the watch, but by virtue of their own qualities. An example to this purpose may be afforded, by what I remember I have not long since mentioned concerning gun-powder. To which I shall now add, that whereas in pills and divers other medicines made up of several ingredients, the *compositum* has, if the physician do his part well, some resulting virtues distinct from those of the ingredients, and belonging to the *compositum* as such; it may oftentimes happen, that notwithstanding the emergent form of the compounded medicine, some particular ingredient may not only retain its former nature, but so retain it, that the *compositum* is endowed with that quality only upon the score of that ingredient. This I have divers times observed in certain pills wherein good ambergrease, being mingled with some purgative ingredients, retained its own grateful smell, and communicated it to the whole mass whereof the pills are made: and the most sort of purging pills in our apothecaries shops taste strong of the aloes, whatever the other ingredients be. And a further instance (and that a considerable one) we may take from treacle that hath not been too long kept. For though it be acknowledged that opium works by a specifick, and, as they call it, occult quality, and though it be in *(Venice)* treacle blended with above threescore other ingredients, most of which enter that famous composition in far greater quantity than does the opium; yet in spite of the *forma compositi*, which so elaborate a mixture produces, and to which such great peculiar virtues are ascribed by Peripatetick as well as other physicians, yet it is noted by many, that before treacle grows old it manifestly derives an opiate quality from the little opium admitted in it, and upon that account is a potent remedy in fluxes, and divers other distempers where quieting medicines are proper. A no less evident example to our purpose we have in the precipitate of gold and mercury made by heat alone. For though by virtue of the union of the ingredients the resulting powder may have divers qualities, as particularly, a red colour, which neither the gold nor the quicksilver had apart; yet the salivating faculty, which this precipitate usually, though not always, exercises, though it be reckoned among occult qualities, as not having by any been deduced from the first or second, yet it belongs to this medicine barely upon the account of of the mercurial ingredient; the gold, without that, having no ſuch faculty, and mercury alone without gold being ſufficient (by more ways than one of application) to cauſe a flux of ſpittle.

VIII. In divers bodies that which is called or looked upon as the ſpecifick form, is often not ſo much as the preſiding, but only the eminenteſt.

To make out this, we may take notice of the following particulars. 1ſt, We obſerved above, that the word *form* has not a ſettled and determinate ſignification, but it is employed arbitrarily enough; ſo that divers bodies, to whom particular forms are uſed to be aſſigned, deſerve not that privilege better than many other, in which (perhaps for want of men's having particularly conſidered them) they are not wont to conſider any peculiar and diſtinct form. 2dly, We have alſo elſewhere ſhown that the forms of inanimate bodies (which we here ſpeak of) are wont to be but reſpective things reſulting from the co-exiſtence of ſuch corpuſcles or parts after ſuch a determinate manner. 3dly, It may likewiſe be remembered, what we have already noted, that it is uſually from ſome particular reſpect, or for a fitneſs to ſome particular uſe, that men aſcribe this or that form to this or that body; as we exemplified in oils, ſalt, &c. as well as in watches, burning-glaſſes, and the like. 4thly, To theſe things it will be agreeable, that the nature and fabrick of a body may be ſuch, that it may have a manifold ſtructure (if I may ſo ſpeak) anſwerable to more than one of thoſe reſpects, on whoſe ſcore bodies are denominated, or may be fit for more than one of thoſe uſes; an aptitude for which, when it is found ſingle in another body, is ſufficient to make it be referred to this or that diſtinct kind or claſſis of things corporeal. I cannot in few words expreſs this notion more clearly, and therefore ſhall illuſtrate it by the example of antimonial glaſs: for one that would make beads or microſcopes with it would readily find in it fuſibleneſs and tranſparency; which, when they are found in common glaſs or *vitrum Saturni*, are enough to refer them to that ſort of bodies that are comprehended under the name of glaſs. But beſides this combination, or (if many convene) this conjugation of qualities, or (to expreſs it in one word) beſides this modification, the body we ſpeak of has another, upon whoſe account it is yet to work upwards and downwards in a human body; upon which ſcore, as the artificer conſiders it only as glaſs, ſo the chymiſt and phyſician look upon it as a medicine. 5thly, Nor is it neceſſary that theſe conjugations of qualities, or (theſe) modifications, ſhould have a ſtrict dependency upon one another; as for inſtance, the emetick and cathartick properties of the antimonial glaſs belong not to it as glaſs, or (if you pleaſe) do not flow immediately from the form it hath of glaſs; for neither has common glaſs, nor (that we know of) glaſs of lead any ſuch properties, nor is it neceſſary, that if this very portion of matter had not the form of glaſs, it ſhould want or loſe theſe properties; for the calx of antimony, before its vitrification, had them; and you may, even without addition, obtain from this glaſs a regulus, that is not, like glaſs, tranſparent, but looks like a metalline body, under which form it may yet preſerve the virtues of the calcined antimony. 6thly, To theſe things it will alſo be congruous, that ſince, as was ſaid above, the nobleneſs or ignobleneſs of forms is not eaſy to be decided, and is wont to be meaſured by men, by the greater or leſſer uſe, that the eſtimated body affords them, one man may in the ſame body look upon one kind of modification, and another upon a quite differing one, as the higheſt form of that body. As in the lately mentioned example of the melted calx of antimony, an artificer may think its nobleſt form to be that of glaſs, and a chymiſt or a phyſician that of antimony. And ſo if an ordinary watch, that ſhows only the hours and their quarters, being hung at a ſtring, were made to ſwing as a pendulum, to an aſtronomer or ſome other that were to make nice obſervations, it would be moſt uſeful in the capacity of a pendulum, because, as that, it may divide a minute into seconds, and a second itself into half or fourth parts; but for other men, who, though they need an instrument to measure time, need not such minute subdivisions of it, the little engine we speak of will be much more useful and considerable in the capacity of a watch than of a pendulum. 7thly, From all which particulars it will be reasonable to collect, that it may often happen in inanimate bodies, whether confessedly natural, or such as are called factitious, that that which is looked upon as the ultimate, or at least the chief form, is not the presiding, but only the eminentest: by which I mean, not simply the noblest (for that were hard to determine, and according to men's estimate would not be always true) but that, which in that body is at least for the time the most considered; or, if that expression will please better, we may say that sometimes the most regarded form is not so much the predominating as the denominating form.

IX. The lately mentioned forms seem to be rather concurrent than subordinate.

This, as I was saying, follows well enough from what has been freshly discoursed. For if a body may have divers such conjugations of accidents or modifications, as may intitle it in differing respects to differing forms, and that form, which is considered as the eminentest, be not a presiding form, nor so much as always the noblest; what will remain but that these forms (for I have granted above that some bodies may have subordinate ones) that happen to coexist in the same body, be more fitly termed concurrent or coincident than subordinate?

And indeed, though I cannot now stay to examine how far what I shall say may be applied to bodies in general, I confess that, as to inanimate bodies, this dominion and subjection, that is imagined between forms, seems to me, at least in many cases, neither well established, nor easy to be well explicated. And I doubt that sometimes we mistake names for things; and because when a body, by the action of proper agents, obtains such a modification as fits it for such and such actions and uses, we are wont to call it by such a name, and attribute a form to it, we are prone to conclude that the faculties and qualifications it enjoys, and the things it is able to perform, are due to this form we have assigned it; as if this form were some distinct and operative substance that were put into the body as a boy into a pageant, and did really begin, and guide, and over-rule the motions and actions of the *compositum*. Whereas indeed what we call the form, if it be not sometimes little more than one of those airy things that schools call an external denomination, seems oftentimes to be rather a metaphysical conception in our mind than a physical agent that performs all things in the body it is ascribed to: as when a conveniently shaped piece of steel is, by having a due temper given it, turned into the spring of a watch, not only the motions of the watch, though proceeding from this spring, proceed not from the form of the iron (for a spring made of another elastical body, though it would not be so convenient, might set a watch a moving) but, which is here the main observable, the springiness itself flows not immediately from the form (for steel is not less steel, when it is not springy than when it is) but from the mechanical and adventitious texture that is superinduced in the metal, and may be given it by several outward agents, as the fire, the hammer, &c. And it is so far from being evident, that in bodies inanimate and compounded the eminentest and most considered form must have a dominion over, and an efficacy in all operations and actions of the *compositum*, that even in bodies not so compounded it is not always necessary that the specifick form should have so much as a concurrent stroke in what is performed; for external agents may introduce such qualities into the body we speak of, as, being once there, will suffice for actions and productions suitable to their own nature, whether the form be active in assisting them or no. We see that boiling water taken off the fire will

will raiſe bliſters on one's hands, and dreſs meat, and perform other things wont to be the effects of the fire, only by virtue of the adventitious heat it has received, though, according to the Peripateticks, the form of water, which is an element naturally cold as well as moiſt, ought rather to oppoſe than further the action of the preternatural heat. Another example to the ſame purpoſe may be given in the operations of a heated iron taken from the fire (nay, though that be quite put out) to which divers other inſtances might be added. I know it may be pretended in favour of the ſchools, that it is the fire that was got in, and yet remains in the iron, that was the cauſe of theſe effects. But beſides that this ſubterfuge would involve the makers in very perplexing difficulties, I will, to prevent the allegation, put a caſe, where it cannot be pretended, by ſuppoſing the iron to be heated not by the fire, but by forcible ſtrokes between a hammer and an anvil, both of them actually cold. When a piece of ſilver is, by being hammered or drawn into wire, made to be a ſpringy body, it will be able to act many things by that acquired elaſticity, which do not at all flow from the form peculiar to the metal. For not only copper, ſteel, and many other bodies may be made ſpringy too; but, if you heat it in the fire the goldſmith will aſſure you that it is as true and as good ſilver as before, and yet it will ceaſe to be a ſpring. And ſo when a ſmith makes a file, by making in it many little impreſſions acroſs one another, and afterwards hardening the ſteel, by virtue of this roughneſs which is given it by external agents, it acquires a durable aſperity, upon whoſe account it is qualified to perform many and conſiderable things, whereto the form of the metal as ſuch does not, that appears, concur. And though the hardneſs contribute to the making a good file, yet not only the iron was as true and perfect iron before it became rough, as afterwards; but even that degree of hardneſs, which qualifies our inſtrument to be a good file, flows not immediately from the form of the metal, for that was true iron, when it was ſoft, and its eminent degree of hardneſs was (as I freſhly intimated) given it by the temper it received from the ſmith. I could eaſily increaſe the number of theſe inſtances, if it were neceſſary; and I ſhould here add ſome examples to ſhew, that even in occult qualities, which are ſo generally preſumed to flow from the ſpecifick form of a body, it is not always neceſſary that this form ſhould have any great intereſt (or perhaps any at all) in the operations; but that the matter need but be duly excited and diſpoſed by outward agents to be enabled to perform them; this, I ſay, I ſhould here make probable, were it not that ſuch inſtances do more properly belong to our notes about particular, and eſpecially about occult qualities.

BUT that I may at length conclude this diſcourſe, I ſhall now in the cloſe, as I have done in ſome of the paſſages of it, complain that the uncertain ſignification and uſe of terms, wont to be employed about the points I have been handling, are apt to occaſion much darkneſs and difficulty in our inquiries into the things themſelves; and I am apt to think that if the meaning of the words *form*, *life*, *ſoul*, *animal*, *vegetative*, and ſome few other terms, were clearly defined and agreed on, a great part of the perplexing controverſies that are agitated about ſubordinate forms, and points relating unto them, would appear to be diſputes about words or terms. And I am not ſure but that ſome parts of the paſſed diſcourſe would not be looked upon as of the ſame kind too, not out of choice, but a neceſſity impoſed upon me, by the nature of my deſign, that I was drawn to meddle with any controverſy that I think may hereafter look like a verbal one; ſo if I have not miſſed my aim, I have both diſcovered ſome errors and deficiencies in the received doctrine I took upon me to conſider, and contributed ſomething towards the future eſtabliſhing of a clearer, as well as truer theory about theſe matters.

A way of preſerving BIRDS taken out of the EGG, and other ſmall FOETUSES.

Firſt printed in the *Philoſophical Tranſactions*, N° XII. p. 199.

For Monday, May 7, 1666.

THIS was imparted in a letter, as follows: The time of the year invites me to intimate to you, that among the other uſes of the experiment I long ſince preſented the ſociety of preſerving whelps taken out of the dam's womb, and other *fœtuſes*, or parts of them, in ſpirit of wine; I remember I did, when I was ſolicitous to obſerve the proceſs of nature in the formation of a chick, open hen's eggs, ſome at ſuch a day, and ſome at other days after the beginning of the incubation; and carefully taking out the embryos, embalmed each of them in a diſtinct glaſs (which is wont to be carefully ſtopt) in ſpirit of wine; which I did, that ſo I might have them in readineſs to make on them, at any time, the obſervations I thought them capable of affording; and to let my friends at other ſeaſons of the year ſee, both the differing appearances of the chick at the third, fourth, ſeventh, fourteenth, or other days, after the eggs had been ſat on, and (eſpecially) ſome particulars not obvious in chickens that go about; as the hanging of the guts out of the abdomen, &c. How long the tender embryo of the chick ſoon after the *punctum ſaliens* is diſcoverable, and whilſt the body ſeems but a little organized gelly, and ſome while after that, will be this way preſerved, without being too much ſhrivelled up, I was hindered by ſome miſchances to ſatisfy myſelf: but when the fœtuſes I took out were ſo perfectly formed as they were wont to be about the ſeventh day, and after, they ſo well retained their ſhape and bulk, as to make me not repent of my curioſity; and ſome of thoſe, which I did very early this ſpring, I can yet ſhew you. I know I have mentioned to you an eaſy application of what I, ſome years ſince, made publick enough; but not finding it to have been yet made by any other, and being perſuaded by experience, that it may be extended to other fœtuſes, which this ſeaſon (the ſpring) is time to make proviſion of, I think the advertiſement will not ſeem unſeaſonable to ſome of our friends; though being now in haſte, and having in my thoughts divers particulars relating to this way of preſerving birds taken out of the egg, and other ſmall fœtuſes, I muſt content myſelf to have mentioned that which is eſſential, leaving divers other things, which a little practice may teach the curious, unmentioned. Notwithſtanding which, I muſt not omit theſe two circumſtances; the one, that when the chick was grown big, before I took it out of the egg, I have (but not conſtantly) mingled with the ſpirit of wine a little ſpirit of ſal armoniac,

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made (as I have * elsewhere delivered) by the help of quick lime: which spirit I chuse, because, though it abounds in a salt not sour, but urinous, yet I never observed it (how strong soever I made it) to coagulate spirit of wine. The other circumstance is, that I usually found it convenient to let the little animals I meant to embalm lie for a little while in ordinary spirit of wine, to wash off the looser filth that is wont to adhere to the chick, when taken out of the egg; and then, having put either the same kind of spirit or better upon the same bird, I suffered it to soak some hours (perhaps some days, *pro re nata)* therein, that the liquor, having drawn, as it were, what tincture it could, the fœtus being removed into more pure and well dephlegmed spirit of wine, might not discolour it, but leave almost as limpid, as before it was put in.

An Account of a new kind of Baroscope, which may be called Statical; and of some Advantages and Conveniences it hath above the Mercurial.

First printed in the *Philosophical Transactions*, No. XIV. p. 256.

For Monday, July 2, 1666.

As for the new kind of baroscopes, which, not long ago † I intimated to you, that my haste would not permit me to give you an account of; since your letters acquaint me that you still design a communicating to the curious as much information as may be, in reference to baroscopes, I shall venture to send you some account of what I did but name (in my former letter) to you.

Though by a passage you may meet with in the 19th and 20th pages of my thermometrical experiments and thoughts you may find that I did some years ago think upon this new kind of baroscope; yet the changes of the atmosphere's weight not happening to be then such as I wished, and being unwilling to deprive myself of all other use of the exactest balance ‡ that I or perhaps any man ever had, I confess to you that successive avocations put this attempt for two or three years out of my thoughts; till afterwards returning to a place, where I chanced to find two or three pair of scales I had left there, the sight of them brought it into my mind; and though I were then unable to procure

* In the Usefulness of Experimental Philosophy.

† See No. XI. p. 185. Phil. Transactions.

‡ The scales here meant were before competent eye-witnesses made to turn manifestly with the thousandth part of a grain.

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exacter, yet my deſire to make the experiment ſome amends for ſo long a neglect, put me upon conſidering, that if I provided a glaſs-bubble, more than ordinary large and light, even ſuch balances, as thoſe might in ſome meaſure perform, what I had tried with the ſtrangely nice ones above mentioned.

I caused then to be blown at the flame of a lamp, ſome glaſs-bubbles, as large, thin, and light, as I could then procure; and chuſing among them one that ſeemed the leaſt unfit for my turn, I counterpoiſed it in a pair of ſcales, that would loſe their æquilibrium with about the 30th part of a grain, and were ſuſpended at a frame. I placed both the balance and the frame by a good baroſcope, from whence I might learn the preſent weight of the atmoſphere. Then leaving theſe inſtruments together; though the ſcales being no nicer than I have expreſſed, were not able to ſhew me all the variations of the air's weight, that appeared in the mercurial baroſcope, yet they did what I expected, by ſhewing me variations no greater than altered the height of quick-ſilver half a quarter of an inch, and perhaps much ſmaller than thoſe: nor did I doubt, that, if I had either tender ſcales, or the means of ſupplying the experiment with convenient accommodations, I ſhould have diſcerned far ſmaller alterations of the weight of the air, ſince I had the pleaſure to ſee the bubble ſometimes in an æquilibrium with the counterpoiſe; ſometimes, when the atmoſphere was high, preponderate ſo manifeſtly, that the ſcales being gently ſtirred, the cock would play altogether on that ſide at which the bubble was hung; and at other times (when the air was heavier) that which was at the firſt but the counterpoiſe, would preponderate, and, upon the motion of the balance, make the cock vibrate altogether on its ſide. And this would continue ſometimes many days together, if the air ſo long retained the ſame meaſure of gravity; and then (upon other changes) the bubble would regain an æquilibrium, or a preponderance: ſo that I had oftentimes the ſatisfaction, by looking firſt upon the ſtatical baroſcope (as for diſtinction's ſake it may be called) to foretel, whether in the mercurial baroſcope the liquor were high or low. Which obſervations, though they hold as well in winter, and ſeveral times in ſummer (for I was often abſent during that ſeaſon) as the ſpring, yet the frequency of their viciſſitudes (which perhaps was but accidental) made them more pleaſant in the latter of theſe ſeaſons.

So that the matter of fact having been made out by variety of repeated obſervations, and by ſometimes comparing of thoſe new baroſcopes together, I ſhall add ſome of theſe notes about this inſtrument which readily occur to my memory, reſerving the reſt to another opportunity.

And firſt, if the ground on which I went in framing this baroſcope be demanded, the anſwer in ſhort may be; 1. That though the glaſs-bubble, and the metalline counterpoiſe, at the time of their firſt being weighed, be in the air, wherein they both are weighed, exactly of the ſame weight, yet they are nothing near of the ſame bulk; the bubble, by reaſon of its capacious cavity (which contains nothing but air, or ſomething that weighs leſs than air) being perhaps a hundred or two hundred times (for I have not conveniency to meaſure them) bigger than the metalline counterpoiſe. 2. That according to a hydroſtatical law (which you know I have lately had occaſion to make out) if two bodies of equal gravity, but unequal bulk, come to be weighed in another medium, they will be no longer equiponderant; but if the new medium be heavier, the greater body, as being lighter in ſpecie, will loſe more of its weight than the leſſer and more compact; but if the new medium be lighter than the firſt, then the bigger body will out-weigh the leſſer: and this diſparity, ariſing from the change of mediums, will be ſo much the greater, by how much the greater inequality of bulk there is between the bodies formerly equiponderant. 3. That laying theſe two together, I conſidered that

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it would be all one, as to the effect to be produced, whether the bodies were weighed in mediums of differing gravity, or in the same medium, in case its (specifick) gravity were considerably altered; and consequently, that since it appeared by the baroscope that the weight of the air was sometimes heavier and sometimes lighter, the alterations of it, in point of gravity, from the weight it was of at first counterpoising the bubble of it, would unequally affect so large and hollow a body as the bubble, and so small and dense a one as a metalline weight: and when the air, by an increase of gravity, should become a heavier medium than before, it would buoy up the glass more than the counterpoise; and if it grew lighter than it was at first, would suffer the former to preponderate. (The illustrations and proof can scarce be added in a few words; but if it be desired, I may, God permitting, send you them at my next leisure). And though our *English* air be about a thousand times lighter than water, the difference in weight of so little air, as is but equal in bulk to a bubble, seemed to give small hopes that it would be sensible upon a balance; yet by making the bubble very large and light, I supposed and found the event I have already related.

SECONDLY, The hermetically sealed glass-bubble I employed, was of the bigness of a somewhat large orange, and weighed about 1 drachm and 10 grains. But I thought it very possible, if I had been better furnished with conveniences (wherein I afterwards found I was not mistaken) to make among many, that might be expected to miscarry, some that might be preferable to this, either for capacity or lightness, or both; especially if care be taken, that they be not sealed up whilst they are too hot. For though one would think that it were advantageous to rarify and drive out the air as much as is possible, because in such sealed bubble the air itself (as I have elsewhere shewn) has a weight; yet this advantage countervails not the inconvenience of being obliged to increase the weight of the glass, which when it includes highly rarified air, if it be not somewhat strong, will be broken by the pressure of the external air, as I have sufficiently tried.

THIRDLY, I would have tried whether the dryness and moisture of the air would in any measure have altered the weight of the bubble, as well as the variation of gravity produced in the atmosphere by other causes; but the extraordinary constant absence of fogs kept me from making observations of this kind; save that one morning early, being told of a mist, I sent to see (being myself in bed) whether it made the air so heavy as to buoy up the bubble; but did not learn that that mist had any sensible operation on it.

FOURTHLY, By reason of the difficulties and casualties that may happen about the procuring and preserving such large and light bubbles as I have been lately mentioning, it may in some cases prove a convenience to be informed, that I have sometimes, instead of one sufficiently large bubble, made use of two that were smaller. And though a single bubble of competent bigness be much preferable, by reason that a far less quantity and weight of glass is requisite to comprise an equal capacity, when the glass is blown into a single bubble, than when it is divided into two; yet I found that the imploying of two instead of one did not so ill answer my expectations, but that they may for a need serve the turn instead of the other; than which they are more easily to be procured. And if the balance be strong enough to bear so much glass without being injured, by imploying two or a greater number of larger bubbles, the effect may be more conspicuous, than if only a single bubble (though a very good one) were imployed.

THIS instrument may be improved by divers accommodations. As

First, There may be fitted to the anſa (or cheeks of the balance) an arch (of a circle) divided into 15 or 20 degrees (more or leſs) according to the goodneſs of the balance) that the cock reſting over againſt theſe diviſions, may readily and without calculation ſhew the quantity of the angle, by which, when the ſcales propend either way, the cock declines from the perpendicular, and the beam from its horizontal parallelifm.

Secondly, Thoſe that will be ſo curious may, inſtead of the ordinary counterpoiſe (of braſs) imploy one of gold, or at leaſt of lead, whereof the latter being of equal weight with braſs, is much leſs in bulk, and the former amounts not to half its bigneſs.

Thirdly, Theſe parts of the balance that may be made of copper or braſs, without any prejudice to the exactneſs, will, by being made one of thoſe metals, be leſs ſubject than ſteel (which yet, if well hardened and poliſhed, may laſt good a great while) to ruſt with long ſtanding.

Fourthly, Inſtead of the ſcales, the bubble may be hung at one end of the beam, and only a counterpoiſe to it at the other, that the beam may not be burthened with unneceſſary weight.

Fifthly, The whole inſtrument, if placed in a ſmall frame, like a ſquare lanthorn with glaſs-windows, and a hole at the top for the commerce of the internal and external air, will be more free from duſt, and irregular agitations; to the latter of which it will otherwiſe be ſometimes incident.

Sixthly, This inſtrument being accommodated with a light wheel and an index (ſuch as have been applied by the excellent Dr. *Chriſtopher Wren* to open weather-glaſſes, and by the ingenious Mr. *Hook* to baroſcopes) may be made to ſhew much more minute variations than otherwiſe.

Seventhly, And the length of the beam, and exquiſiteneſs of the balance, may eaſily, without any of the foregoing helps (and much more with them) make the inſtrument far exacter than any of thoſe I was reduced to imploy. And to theſe accommodations divers others may be ſuggeſted, by a farther conſideration of the nature of the thing, and a longer practice.

Though in ſome reſpects this ſtatical baroſcope be inferior to the mercurial; yet in others it has its own advantages and conveniences above it.

And, 1. It confirms *ad oculum* our former doctrine, that the falling or riſing of the mercury depends upon the varying weight of the atmoſphere; ſince in this baroſcope it cannot be pretended, that a *fuga vacui*, or *funiculus*, is the cauſe of the changes we obſerve. 2. It ſhews not only that the air has weight, but a more conſiderable one, than ſome learned men, who will allow me to have proved, it has ſome weight, will admit; ſince even the variation of weight in ſo ſmall a quantity of air, as is but equal in bulk to an orange, is manifeſtly diſcoverable upon ſuch balances as are none of the niceſt. 3. This ſtatical baroſcope will oftentimes be more parable than the other: for many will find it more eaſy to procure a good pair of gold-ſcales, and a bubble or two, than a long cane ſealed, a quantity of quickſilver, and all the other requiſites of the mercurial baroſcope; eſpecially if we compriſe the trouble and ſkill that is requiſite to free the deſerted part of the tube from air. 4. And whereas the difficulty of removing the mercurial inſtrument has kept men from ſo much as attempting to do it, even to neighbouring places; the eſſential parts of the ſcale-baroſcope (for the frame is none of them) may very eaſily in a little room be carried, whither one will, without the hazard of being ſpoiled or injured. 5. There is not in ſtatical baroſcopes, as in other, a danger of uncertainty as to the goodneſs of the inſtruments, by reaſon that in thoſe the air

is,

is, in ſome more, and in ſome leſs perfectly excluded; whereas in thoſe, that conſideration has no place. (And by the way, I have ſometimes, upon this account, been able to diſcover by our new baroſcope, that an eſteemed mercurial one, to which I compared it, was not well freed from air). 6. It being, as I formerly intimated, very poſſible to diſcover hydroſtatically both the bigneſs of the bubble, and the contents of the cavity, and the weight and dimenſions of the glaſſy ſubſtance (which together with the included air make up the bubble) much may be diſcovered by this inſtrument, as to the weight of the air abſolute or reſpective. For, when the quickſilver in the mercurial baroſcope is either very high, or very low, or at a middle ſtation between its greateſt and leaſt height, bringing the ſcale-barometer to an exact æquilibrium (with very minute diviſions of a grain) you may, by watchfully obſerving, when the mercury is riſen or fallen juſt an inch, or a fourth, or half an inch, *&c.* and putting in the like minute diviſions of a grain to the lighter ſcale, till you have again brought the balance to an exquiſite æquilibrium; you may, I ſay, determine what known weight in a ſtatical baroſcope anſwers ſuch determinate altitudes of the aſcending and deſcending quickſilver in the mercurial. And if the balance be accommodated with a divided arch, or a wheel and index, theſe obſervations will aſſiſt you for the future to determine readily, by ſeeing the inclination of the cock or the degree marked by the index, what pollency the bubble hath, by the change of the atmoſphere's weight, acquired or loſt. Some obſervations of this nature I watchfully made, ſometimes putting in a 64^{th}, ſometimes a 32^{d}, ſometimes a 16^{th}, and ſometimes heavier parts of a grain, to the lighter ſcale. But one that knew not for what uſes thoſe little papers were, coming to a window where my baroſcopes ſtood, ſo unluckily ſhook them out of the ſcales, and confounded them, that he robbed me of the opportunity of making the nice obſervations I intended, though I had the ſatisfaction of ſeeing that they were to be made. 7. By this ſtatical inſtrument we may be aſſiſted to compare the mercurial baroſcopes of ſeveral places (though never ſo diſtant) and to make ſome eſtimates of the gravities of the air therein. As if, for inſtance, I have found by obſervation, that the bubble I imploy (and one may have divers bubbles of ſeveral ſizes, that the one may repair any miſchance that may happen to another) weighed juſt a drachm, when the mercurial cylinder was at the height of $29\frac{1}{2}$ inches (which in ſome places I have found a moderate altitude); and that the addition of the 16^{th} part of a grain is requiſite to keep the bubble in an æquilibrium, when the mercury is riſen an 8^{th}, or any determinate part of an inch above the former ſtation: when I come to another place, where there is a mercurial barometer, as well freed from air as mine (for that muſt be ſuppoſed) if taking out of my ſcale inſtrument, it appear to weigh preciſely a drachm, and, the mercury in the baroſcope there ſtand at juſt $29\frac{1}{2}$ inches, we may conclude the gravity of the atmoſphere not to be ſenſibly unequal in both thoſe two places, though very diſtant. And though there be no baroſcope there, yet if there be an additional weight, as for inſtance, the 16^{th} part of a grain requiſite to be added to the bubble, to bring the ſcales to an æquilibrium, it will appear that the air at this ſecond place is, at that time, ſo much heavier than the air of the former place was when the mercury ſtood at $29\frac{1}{2}$ inches.

But in making ſuch compariſons we muſt not forget to conſider the ſituation of the ſeveral places, if we mean to make eſtimates not only of the weight of the atmoſphere, but of the weight and denſity of the air. For though the ſcales will ſhew (as has been ſaid) whether there be a difference of weight in the atmoſphere at the two places; yet if one of them be in a vale or bottom, and the other on the top or ſome elevated part of a hill, it is not to be expected that the atmoſphere in this latter place ſhould gravitate as much

much as the atmosphere in the former, on which a longer pillar of air does lean or weigh.

AND the mention I have made of the differing situation of places, puts me in mind of something that may prove another use of our statical baroscope, and which I had thoughts of making trial of, but was accidentally hindered from the opportunity of doing it; namely, that by exactly poising the bubble at the foot of a high steeple or hill, and carrying it in its close frame to the top, one may, by the weight requisite to be added to counterpoise there, to bring the beam to its horizontal position, observe the difference of the weight of the air at the bottom, and at the top; and in case the hill be high enough at some intermediate stations. But how far this may assist men to estimate the absolute or comparative height of mountains, and other elevated places; and what other uses the instrument may be put to when it is duly improved; and the cautions that may be requisite in the several cases that shall be proposed, I must leave to more leisure and farther consideration.

A NEW

FRIGORIFICK EXPERIMENT,

SHEWING

How a considerable degree of COLD may be suddenly produced without the help of SNOW, ICE, HAIL, WIND, or NITRE, and that at any time of the year.

First printed in the *Philosophical Transactions*, N°. XV. p. 255. For Wednesday, July 18, 1666.

AS for the experiment you saw the other day at my lodgings, though it belongs to some papers about cold, that (you know) could not be published when the rest of the history came forth, and therefore was reserved for the next edition of that book; yet the weather having been of late very hot, and threatening to continue so, I presume, that to give you here, in compliance with your curiosity, an account of the main and practical part of the experiment, may enable you to gratify not only the curious among your friends, but those of the delicate, that are content to purchase a coolness of drinks at a somewhat chargeable rate.

You may remember that the fpring before the laft, I fhewed you a particular account of a way wherein, by a certain fubftance obtained from fal armoniac, I could prefently produce a confiderable degree of cold, and that with odd circumftances, without the help of fnow, ice, nitre, *&c.* But that experiment being difficult and coftly enough, and defigned to afford men information, not accommodations, I afterwards tried what fome more cheap and facile mixtures of likely bodies with fal armoniac would do towards the production of cold, and afterwards I began to confider, whether to that purpofe alone (for my firft experiment was defigned to exhibit other phænomena too) thofe mixtures might not without inconvenience be omitted. And I was much confirmed in my conjecture by an accident which was cafually related to me by a very ingenious phyfician of my acquaintance, but not to be repeated to you in a few words, though he complained, he knew not what to make of it.

Among the feveral ways by which I have made infrigidating mixtures with fal armoniac, the moft fimple and facile is this: take one pound of powdered fal armoniac and about three pints (or pounds) of water; put the falt into the liquor, either all together, if your defign be to produce an intenfe, though but a fhort coldnefs; or at two, three, or four feveral times, if you defire that the produced coldnefs fhould rather laft fomewhat longer than be fo great. Stir the powder in the liquor with a ftick or whalebone (or fome other thing that will not be injured by the fretting brine that will be made) to haften the diffolution of the falt, upon the quicknefs of which depends very much the intenfity of the cold, that will enfue upon this experiment. For the clearing up whereof, I fhall annex the following particulars.

1. That a confiderable degree of cold is really produced by this operation, is very evident: Firft, to the touch; Secondly, by this, that if you make the experiment (as for this reafon I fometimes chufe to do) in a glafs-body or a tankard, you may obferve, that whilft the folution of the falt is making, the outfide of the metalline veffel will, as high as the mixture reaches within, be bedewed (if I may fo fpeak) with a multitude of little drops of water, as I have † elfewhere fhown, that it happens when mixtures of fnow and falt being put into glaffes or other veffels, the aqueous vapours that fwim to and fro in the air, and chance to glide along the fides of the veffels, are by the coldnefs thereof condenfed into water. And in our armoniac folution you may obferve, that if you wipe off the dew from any particular part of the outfide of the veffel, whilft the folution does yet vigoroufly go on, it will quickly collect frefh dew, which may be fometimes copious enough to run down the fides of the veffel. But thirdly, the beft and fureft way of finding out the coldnefs of our mixture is that which I fhewed you, by plunging into it a good fealed weather-glafs furnifhed with tincted fpirit of wine; for the ball of this being put into our frigorifick mixture, the crimfon liquor will nimbly enough defcend much lower than when it was kept in the open air, in common water, of the fame temper with that wherein the fal armoniac was put to diffolve. And if you remove the glafs out of the mixture into common water, the tincted fpirit will (as you may remember it did) haftily enough re-afcend for a pretty while, according to the greater or leffer time that it continued in the armoniac folution. And this has fucceeded with me, when inftead of removing the mixture into common water, I removed it into water newly impregnated with falt-petre.

The duration of the cold produced by this experiment depends upon feveral circumftances; as, firft, upon the feafon of the year, and prefent temperature of the air; for in fummer and hot weather the cold will fooner decay and expire. Secondly, upon the quan-

† In the *Hiftory of Cold.*

tity of the salt and water; for, if both these be great, the effect will be as well more lasting, as more considerable. Thirdly, for aught I yet know, we may here add the goodness and fitness of the particular parcel of salt that is imployed: for though it be hard to discern beforehand, which will be the more, and which the less proper; yet some trials have tempted me to suspect that there may be a considerable disparity, as to their fitness to produce cold, betwixt parcels of salt, that are without scruple looked upon as sal armoniac; of which difference it were not perhaps very difficult to assign probable reasons from the nature of the ingredients of this compound concrete, and the ways of preparing it. But the duration of the cold may be conceived to depend also, fourthly, upon the way of putting the salt into the water; for if you cast it in all at once, the water will sooner acquire an intense degree of coldness, but it will also the sooner return to its former temper; whereas, if you desire but an inferiour degree of quality, but that may last longer (which will usually be the most convenient for the cooling of drinks) then you may put in the salt by little and little. For keeping a long weather-glass for a good while in our impregnated mixture, I often purposely tried, that, when the tincted liquor subsided but slowly, or was at a stand, by putting in from time to time two or three spoonfuls of fresh salt, and stirring the water to quicken the dissolution, the spirit of wine would begin again to descend, if it were at a stand, or rising, or subside much more swiftly than it did before. And if you would lengthen the experiment, it may not be amiss, that part of the sal armoniac be but grosly beaten, that it may be the longer in dissolving, and consequently in cooling the water. Whilst there are dewy drops produced on the outside of the vessel, it is a sign that the cold within continues pretty strong; for when it ceases, these drops, especially in warm weather, will by degrees vanish. But a surer way of measuring the duration of the cold is, by removing from time to time the sealed weather-glass out of the saline mixture into the same common water, with part of which it was made. And though it be not easy to determine any thing particularly about this matter, yet it may somewhat assist you in your estimates, to be informed, that I have in the spring by a good weather-glass found a sensible adventitious cold, made by a pound of sal armoniac at the utmost, to last about two or three hours.

3. To cool drinks with this mixture, you may put them in thin glasses, the thinner the better; which (their orifices being stopped, and still kept above the mixture) may be moved to and fro in it, and then be immediately poured out to be drunk: though when the glass I imployed was conveniently shaped, as like a sugar-loaf, or with a long neck, I found it not amiss to drink it out of that, without pouring it into any other; which can scarce be done without lessening the coldness. The refrigeration, if the glass-phial be convenient, is quickly performed: and if one have a mind to cool his hands, he may readily do it by applying them to the outside of the vessel that contains the refrigerating mixture; by whose help, pieces of crystal, or bullets for the cooling of the mouths or hands of those patients, to whom it may be allowed, may be potently cooled, and other such refreshments may be easily procured.

4. How far sal armoniac, mingled with sand or earth, and not dissolved, but only moistened with a little water sprinkled on it, will keep bottles of wine or other liquors more cool than the earth or than sand alone will do, I have not yet had opportunity by sufficient trials fully to satisfy myself, and therefore resign that inquiry to the curious.

5. For the cooling of air, and liquors, to adjust weather-glasses, to be able to do which, at all times of the year, was one of the chief aims that made me bethink myself of this experiment; or to give a small quantity of beer, &c. a moderate degree of cool-

coolnefs, it will not be requifite to imploy near fo much as a whole pound of fal armoniac at a time. For you may eafily obferve by a fealed weather-glafs, that a very few ounces, well powdered and nimbly diffolved in about four times the weight of water, will ferve well enough for many purpofes.

6. And that you may the lefs fcruple at this, I fhall tell you, that even before and after Midfummer, I have found the cold producible by our experiment to be confiderable, and ufeful for refrigerating of drinks, *&c.* but if the fal armoniac be of the fitteft fort (for I intimated above, that I fufpected it is not equally good) and if the feafon of the year do make no difadvantageous difference, the degree of cold that may be produced by no more than one pound (if not by lefs) fal armoniac may, within its own fphere of activity, be much more vehement, than, I prefume, you yet imagine, and may afford us excellent ftandards to adjuft fealed weather-glaffes by; and for feveral other purpofes. For I remember that in the fpring, about the end of *March*, or beginning of *April*, I was able with one pound of fal armoniac, and a requifite portion of water, to produce a degree of cold much greater, than was neceffary the preceding winter, to make it frofty weather abroad; nay, I was able to produce ice in a fpace of time almoft incredibly fhort. To confirm which particulars, becaufe they will probably feem ftrange to you, I will here annex the tranfcript of an entry, that I find in a note-book of phænomena and fuccefs of one of thofe experiments, as I then tried it; though I fhould be afhamed to expofe to your perufal a thing fo rudely penned, if I did not hope you would confider that it was haftily written only for my own remembrance. And that you may not ftop at any thing in the immediately annext note, or the two that follow, it will be requifite to premife this account of the fealed thermofcope (which was a good one) wherewith thefe obfervations were made; that the length of the cylindrical pipe was 16 inches, the ball about the bignefs of a fomewhat large walnut, and the cavity of the pipe by guefs about an eighth or ninth part of an inch diameter.

The firft experiment is thus regiftered. *March* the 27th, in the fealed weather-glafs, when firft put into water, the tincted fpirit refted at $8\frac{5}{8}$ inches: being fuffered to ftay there a good while, and now and then ftirred to and fro in the water, it defcended at length a little beneath $7\frac{5}{8}$ inches: then the fal armoniac being put in, within about a quarter of an hour, or a little more, it defcended to $2\frac{1}{16}$ inches, but before that time, in half a quarter of an hour it began manifeftly to freeze the vapours and drops of water on the outfide of the glafs. And when the frigorifick power was arrived at the height, I feveral times found, that water thinly placed on the outfide, whilft the mixture within was nimbly ftirred up and down, would freeze in a quarter of a minute (by a minute watch). At about three quarters of an hour after the infrigidating body was put in, the thermofcope that had been taken out a while before, and yet was rifen but to the loweft freezing mark, being again put in the liquor, fell an inch beneath the mark. At about $2\frac{1}{2}$ hours from the firft folution of the falt, I found the tincted liquor to be in the midft between the freezing marks, whereof the one was at $5\frac{1}{2}$ inches (at which height when the tincture refted, it would ufually be fome, though but a fmall froft abroad; and the other at $4\frac{3}{4}$ inches; which was the height to which ftrong and durable frofts had reduced the liquor in the winter. At three hours after the beginning of the operation, I found not the crimfon liquor higher than the upper freezing mark newly mentioned; after which, it continued to rife very flowly for about an hour longer; beyond which time I had not occafion to obferve it.

Thus far the note-book; wherein there is mention made of a circumftance of fome former experiments of the like kind, which I remember was very confpicuous in this newly recited. For the frigorifick mixture having been made in a large glafs-body (as they call it) with a large and flattish bottom, a quantity of water which I (purposely) spilt upon the table, was, by the operation of the mixture within the glass, made to freeze, and that strongly enough, the bottom of the cucurbite to the table; that stagnant liquor being turned into solid ice, that continued a considerable while unthawed away, and was in some places about the thickness of a half-crown piece.

ANOTHER observation made the same spring, but less solemn, as meant chiefly to shew the duration of cold in a high degree, is recorded in these terms: the first time the sealed weather-glass was put in, before it touched the common water, it stood at $8\frac{1}{8}$, having been left there a considerable while, and once or twice agitating the water, the tincted liquor sunk but to $7\frac{7}{8}$, or at furthest $7\frac{6}{8}$; then the frigorifick liquor being put into the water with circumstances disadvantageous enough, in (about) half a quarter of an hour the tincted liquor fell beneath $3\frac{3}{4}$; and the thermoscope being taken out, and then put in again, an hour after the water had been first infrigidated, subsided beneath 5 inches, and consequently within a quarter of an inch of the mark of the strongly freezing weather.

7. WHEREAS the grand thing that is like to keep this experiment from being as generally useful, as perhaps it will prove lucriferous, is the dearness of sal armoniack, two things may be offered to lessen this inconvenience. For first, sal armoniac might be made much cheaper, if instead of fetching it beyond sea, our countrymen made it at home; which it may easily be, and I am ready to give you the receipt, which is no great secret. But next I considered, that probably the infrigidating virtue of our mixture might depend upon the peculiar texture of the sal armoniac, whereby, whilst the water is dissolving it, either some frigorifick particles are extricated and excited, or rather some particles, which did before more agitate the minute parts of the water, are expelled (or invited out by the ambient bodies) or come to be clogged in their motion: whence it seemed reasonable to expect that upon the re-union of the saline particles into such a body, as they had constituted before, the redintegrated sal armoniac, having near upon the same texture, would, upon its being redissolved, produce the same, or a not much inferiour degree of coldness: and hereupon, though I well enough foresaw that an armoniac solution, being boiled up in earthen vessels (for glass ones are too chargeable) would, by piercing them, both lose some of the more subtile parts, and thereby somewhat impair the texture of the rest; yet I was not deceived in expecting that the dry salt remaining in the pipkins, being re-dissolved in a due proportion of water, would very considerably infrigidate it; as may further appear by the notes, which for your greater satisfaction you will find here subjoined, as soon as I have told you, that, though for want of other vessels I was first reduced to make use of earthen ones, and the rather, because some metalline vessels will be injured by the dissolved sal armoniac, if it be boiled in them; yet I afterwards found some conveniencies in vessels of other metal, as of iron; whereof you may command a farther account.

March the 29th the thermoscope in the air was at $8\frac{7}{8}$ inches; being put into a somewhat large evaporating glass, filled with water, it fell (after it staid a pretty while, and had been agitated in the liquor) to 8 inches: then about half the salt, or less, that had been used twice before, and felt much less cold than the water, being put in and stirred about, the tincted spirit subsided with a visible progress, till it was fallen manifestly beneath four inches; and then, having caused some water to be freshly pumped and brought in, though the newly mentioned solution were mixt with it, yet it presently made the spirit of wine manifestly to ascend in the instrument, much faster than one would have expected, &c.

AND

And thus much may suffice for this time concerning our frigorifick experiment; which I scarce doubt but the *Cartesians* will lay hold on as very favourable to some of their tenets; which you will easily believe it is not to the opinion I have elsewhere opposed of those modern philosophers, that would have salt-petre to be the *primum frigidum* (though I found by trial, that whilst it is actually dissolving, it gives a much considerabler degree of cold, than otherwise). But about the reflections that may be made on this experiment, and the variations and improvements, and uses of it, though I have divers things lying by me; yet, since you have seen several of them already, and may command a sight of the rest, I shall forbear the mention of them here, not thinking it proper to swell the bulk of this letter with them.

The Method observed in Transfusing the Blood out of one Animal into another.

First printed in the *Philosophical Transactions* No. XX. *p.* 353.

For *Monday*, December 17, 1666.

THIS method was promised in the last of these papers. It was first practised by Dr. *Lower* in *Oxford*, and by him communicated to the author, who imparted it to the Royal Society, as follows:

First, Take up the carotidal artery of the dog or other animal whose blood is to be transfused into another of the same or a differing kind, and separate it from the nerve of the eighth pair, and lay it bare above an inch; then make a strong ligature on the upper part of the artery, not to be untied again; but an inch below, *viz.* towards the heart, make another ligature of a running knot, which may be loosened or fastened as there shall be occasion. Having made these two knots, draw two threads under the artery between the two ligatures; and then open the artery, and put in a quill, and tie the artery upon the quill very fast by those two threads, and stop the quill with a stick. After this, make bare the jugular vein in the other dog about an inch and a half long; and at each end make a ligature with a running knot, and in the space betwixt the two running knots draw under the vein two threads, as in the other; then make an incision in the vein, and put into it two quills, one into the descendent part of the vein, to receive the blood from the other dog and carry it to the heart; and the other quill put into the other part of the jugular vein which comes from the head (out of which the second dog's own blood must run into dishes). These two quills being put in and tied fast, stop them with a stick till there be occasion to open them.

All things being thus prepared, place the dogs on their sides towards one another so conveniently that the quill may go into each other; (for the dog's necks cannot be brought so near but that you must put two or three several quills more into the first two, to convey the blood from one to another). After that unstop the quill that goes down into the first dog's jugular vein, and the other quill coming out of the other dog's

artery;

artery; and by the help of two or three other quills put into each other, according as there ſhall be occaſion, inſert them into one another; then ſlip the running knots, and immediately the blood runs through the quills, as through an artery, very impetuouſly. And immediately, as the blood runs into the dog, unſtop the other quill, coming out of the upper part of his jugular vein (a ligature being firſt made about his neck, or elſe his other jugular vein being compreſſed by one's finger); and let his own blood run out at the ſame time into diſhes (yet not conſtantly, but according as you perceive him able to bear it) till the other dog begin to cry, and faint, and fall into convulſions, and at laſt die by his ſide.

Then take out both the quills out of the dog's jugular vein, and tie the running knot faſt, and cut the vein aſunder (which you may do without harm to the dog, one jugular vein being ſufficient to convey all the blood from the head and upper parts, by reaſon of a large anaſtomoſis, whereby both the jugular veins meet about the larynx). This done, ſew up the ſkin and diſmiſs him, and the dog will leap from the table and ſhake himſelf, and run away, as if nothing ailed him.

And this I have tried ſeveral times before ſeveral in the univerſities, but never yet upon more than one dog at a time for want of leiſure and convenient ſupplies of ſeveral dogs at once. But when I return I doubt not but to give you a fuller account, not only by bleeding ſeveral dogs into one, but ſeveral other creatures into one another, as you did propoſe to me before you left *Oxford*; which will be very eaſy to perform, and will afford many pleaſant, and perhaps not unuſeful experiments.

But becauſe there are many circumſtances neceſſary to be obſerved in the performing of this experiment, and that you may better direct any one to do it, without any danger of killing the other dog that is to receive the other's blood, I will mention two or three.

First, that you faſten the dogs at ſuch a convenient diſtance, that the vein nor artery be not ſtretched; for then, being contracted, they will not admit or convey ſo much blood.

Secondly, that you conſtantly obſerve the pulſe beyond the quill in the dog's jugular vein (which it acquires from the impulſe of the arterious blood) for if that fails, then it is a ſign the quill is ſtopt by ſome congealed blood, ſo that you muſt draw out the arterial quill from the other, and with a probe open the paſſage again in both of them, that the blood may have its free courſe again. For this muſt be expected when the dog that bleeds into the other hath loſt much blood, his heart will beat very faintly, and then the impulſe of blood being weaker, it will be apt to congeal the ſooner, ſo that at the latter end of the work you muſt draw out the quill oftener, and clear the paſſage; if the dog be faint-hearted, as many are, though ſome ſtout fierce dogs will bleed freely and uninterruptedly, till they are convulſed and die. But to prevent this trouble, and make the experiment certain, you muſt bleed a great dog into a little one, or a maſtiff into a cur, as I once tried; and the little dog bled out at leaſt double the quantity of his own blood, and left the maſtiff dead upon the table; and after he was untied he ran away, and ſhaked himſelf, as if he had been only thrown into water. Or elſe you may get three or four ſeveral dogs prepared in the ſame manner; and when one begins to fail and leave off bleeding, adminiſter another; and I am confident one dog will receive all their blood (and perhaps more) as long as it runs freely, till they are left almoſt dead by turns; provided you let out the blood proportionably, as you let it go into the dog that is to live.

Thirdly, I ſuppoſe the dog that is to bleed out into diſhes, will endure it better, if the dogs that are to be adminiſtered to ſupply his blood, be of near an equal age, and fed

fed alike the day before, that both their bloods may be of a near ſtrength and temper.

THERE are many things I have obſerved upon bleeding dogs to death, which I have ſeen ſince your departure from *Oxford*, whereof I ſhall give you a relation hereafter. In the mean time ſince you were pleaſed to mention it to the Royal Society, with a promiſe to give them an account of this experiment, I could not but take the firſt opportunity to clear you from that obligation, &c.

So far this letter; the preſcriptions whereof having been carefully obſerved by thoſe who were employed to make the experiment, have hitherto been attended with good ſucceſs; and that not only upon animals of the ſame ſpecies (as two dogs firſt, and then two ſheep); but alſo upon ſome of very differing ſpecies (as a ſheep and a dog, the former emitting the other receiving).

NOTE only, that inſtead of a quill, a ſmall crooked thin pipe of ſilver or braſs, ſo ſlender, that the one end may enter into the quill, and having at the other end, that is to enter into the vein and artery, a ſmall knob for the better faſtening them to it with a thread, will be much fitter than a ſtrait pipe or quill, for this operation; for ſo they are much more eaſy to be managed.

IT is intended that theſe trials ſhall be proſecuted to the utmoſt variety the ſubject will bear; as by exchanging the blood of old and young, ſick and healthy, hot and cold, fierce and fearful, tame and wild animals, &c. and that not only of the ſame but alſo of differing kinds. For which end, and to improve this noble experiment, either for knowledge, or uſe, or both, ſome ingenious men have already propoſed conſiderable trials and inquiries; of which perhaps an account will be given hereafter. For the preſent we ſhall only ſubjoin ſome.

Conſiderations about this kind of Experiments.

1. IT may be conſidered in them, that the blood of the emittent animal may, after a few minutes of time, by its circulation, mix and run out with that of the recipient. Wherefore, to be aſſured in theſe trials that all the blood of the recipient is run out, and none left in him but the adventitious blood of the emittent, two, or three, or more animals (which was alſo hinted in the method above) may be prepared and adminiſtered to bleed them all out into one.

2. IT ſeems not irrational to gueſs aforehand that the exchange of blood will not alter the nature or diſpoſition of the animals, upon which it ſhall be practiſed; though it may be thought worth while, for ſatisfaction and certainty, to determine that point by experiments. The caſe of exchanging the blood of animals ſeems not like that of graffing, where the cyon turns the ſap of the ſtock, graffed upon, into its nature; the fibres of the cyon ſo ſtraining the juice which paſſes from the ſtem to it, as thereby to change it into that of the cyon: whereas in this transfuſion there ſeems to be no ſuch percolation of the blood of animals, whereby that of the one ſhould be changed into the nature of the other.

3. THE moſt probable uſe of this experiment may be conjectured to be, that one animal may live with the blood of another; and conſequently thoſe animals that want blood, or have corrupt blood, may be ſupplied from others with a ſufficient quantity, and of ſuch as is good, provided the transfuſion be often repeated, by reaſon of the quick expence that is made of the blood.

Trials

Trials proposed to Dr. LOWER,

To be made by him, for the Improvement of transfusing Blood out of one live Animal into another.

(Promised Numb. XX. *p.* 357.)

First Printed in the *Philosophical Transactions*, No. XXII. *p.* 385.

For *Monday*, February 11, 1666.

THE following queries and trials were written long since and read about a month ago in the Royal Society, and do now come forth against the author's intention, at the earnest desire of some learned persons, and particularly of the worthy doctor to whom they were addressed; who thinks, they may excite and assist others in a matter, which to be well prosecuted will require many hands. At the reading of them, the author declared that of divers of them he thought he could foresee the events, but yet judged it fit not to omit them, because the importance of the theories they may give light to may make the trials recompence the pains, whether the success favour the affirmative or the negative of the question, by enabling us to determine the one or the other upon surer grounds than we could otherwise do. And this advertisement he desires may be applied to those other papers of his that consist of queries or proposed trials.

The Queries themselves follow.

1. WHETHER by this way of transfusing blood, the disposition of individual animals of the same kind may not be much altered? (as whether a fierce dog, by being often quite new stocked with the blood of a cowardly dog, may not become more tame) *&* *vice versa, &c.*

2. WHETHER immediately upon the unbinding the dog, replenished with adventitious blood, he will know, and fawn upon his master, and do the like customary things as before? and whether he will do such things better or worse at some time after the operation?

3. WHETHER those dogs that have peculiarities will have them either abolished, or at least much impaired by transfusion of blood? (as whether the blood of a mastiff, frequently

frequently transfufed into a blood-hound, or a fpaniel, will not prejudice them in point of fcent)?

4. WHETHER acquired habits will be deftroyed or impaired by this experiment? (as whether a dog taught to fetch and carry, or to dive after ducks, or to fett, will, after frequent and full recruits of the blood of dogs unfit for thofe exercifes, be as good at them as before?)

5. WHETHER any confiderable change is to be obferved in the pulfe, urine, and other excrements of the recipient animal by this operation, or the quantity of his infenfible tranfpiration?

6. WHETHER the emittent dog, being full fed at fuch a diftance of time before the operation, that the mafs of blood may be fuppofed to abound with chyle, the recipient dog, being before hungry, will lofe his appetite more than if the emittent dog's blood had not been fo chylous? and how long, upon a vein opened of a dog, the admitted blood will be found to retain chyle?

7. WHETHER a dog may be kept alive without eating by the frequent injection of the chyle of another, taken frefhly from the receptacle, into the veins of the recipient dog?

8. WHETHER a dog that is fick of fome difeafe, chiefly imputable to the mafs of blood, may be cured by exchanging it for that of a found dog? and whether a found dog may receive fuch difeafes from the blood of a fick one, as are not otherwife of an infectious nature?

9. WHAT will be the operation of frequently ftocking (which is feafible enough) an old and feeble dog with the blood of young ones, as to livelinefs, dulnefs, drowfinefs, fqueamifhnefs, &c. *& vice verfa.*

10. WHETHER a fmall young dog, by being often frefh ftocked with the blood of a young dog of a larger kind, will grow bigger than the ordinary fize of his own kind?

11. WHETHER any medicated liquors may be injected, together with the blood, into the recipient dog? and in cafe they may, whether there will be any confiderable difference found between the feparations made on this occafion, and thofe which would be made, in cafe fuch medicated liquors had been injected with fome other vehicle, or alone, or taken in at the mouth?

12. WHETHER a purgative medicine being given to the emittent dog a while before the operation, the recipient dog will be thereby purged, and how? (which experiment may be hugely varied).

13. WHETHER the operation may be fuccefsfully practifed, in cafe the injected blood be that of an animal of another fpecies, as of a calf into a dog, &c. and of a cold animal, as of a fifh, or frog, or tortoife, into the veffels of a hot animal? *& vice verfa?*

14. WHETHER the colour of the hair or feathers of the recipient animal, by the frequent repeating of this operation, will be changed into that of the emittent?

15. WHETHER by frequently transfufing into the fame dog, the blood of fome animal of another fpecies, fomething further, and more tending to fome degrees of a change of fpecies, may be effected, at leaft in animals near of kin (as fpaniels and fetting dogs, Irifh grey-hounds and ordinary grey-hounds, &c.)?

16. WHETHER the transfufion may be practifed upon pregnant bitches, at leaft at certain times of their gravidation? and what effect it will have upon the whelps?

THERE were fome other queries propofed by the fame author; as the weighing of the emittent animal before the operation, that (making an abatement for the effluviums, and

and for the excrements, if it voids any) it may appear how much blood it really loſes. To which were annexed divers others, not ſo fit to be peruſed but by phyſicians, and therefore here omitted.

PROPOSALS

To try the Effects of the Pneumatick Engine exhauſted, in Plants, Seeds, Eggs of Silk-worms.

Firſt Printed in the *Philoſophical Tranſactions*, No. XXIII. p. 424.

For *Monday*, March 11, 1666.

THE ingenious Dr. *Beale* did formerly ſuggeſt, as follows:

IT would be, I think (ſaith he) very well worth the trial, to ſee what effects would be produced on plants put into the pneumatick (or rarefying) engine of Mr. *Boyle*, with the earth about their roots, and flouriſhing; whether they would not ſuddenly wither if the air were totally taken from them. And particularly to try in the ſeaſon cherry-bloſſoms, when partly opened, partly not opened, upon a branch; to wit, whether the air may be ſo attenuated as to blaſt. But it may be noted, that the bloſſoms do not forthwith diſcover the blaſt; an old experienced countryman having once given me notice of a blaſty noon (it being then ſultry weather and ſomewhat gloomy with the thickneſs of exhalations, almoſt like a very thick miſt) and within a day or two ſhewing the proof upon the cherry-bloſſoms then flagging, but not much altering their colour till two days more were paſt.

THE noble Mr. *Boyle* ſuggeſts, as proper for the approaching ſeaſon, that it may be tried,

1. WHETHER ſeeds (eſpecially ſuch as are of a haſty growth, *viz.* orpin, lettice, garden-creſs-ſeeds, &c.) will germinate and thrive in the exhauſted receiver of the ſaid engine?

2. WHETHER the excluſion of air from the ſenſitive plant would be harmful to it?

3. WHETHER the graffing of pears upon ſpina cervina (the almoſt only purgative vegetable known in England) will produce the effect of communicating to the fruit that purging quality, or not?

4. WHETHER ſilk-worms eggs will be hatched in ſuch an exhauſted receiver, in the ſeaſon proper for hatching?

To which may be added the trials of putting in a phial, full of water, ſome of thoſe herbs that will ſhoot and grow in water alone, including them in ſuch a receiver, and pumping out what air you can, to ſee whether they will then ſhoot or not?

AND

And though fome of thefe propofals have been formerly begun to be experimented, yet ought they to be diligently profecuted, to fee how far the air is neceffary to vegetation; and whether plants do indeed live as much upon the air, as the earth; and the branches of them are rooted (as it were) in, and quickened by the air, as their roots are planted and nourifhed in and by the earth?

The experiment hitherto made of this kind was, that fome lettice-feed being fown upon fome earth in the open air, and fome of the fame feed at the fame time upon other earth in a glafs-receiver of the above mentioned engine, afterwards exhaufted of air; the feed expofed to the air was grown up an inch and a half high within eight days; but that in the exhaufted receiver, not at all. And air being again admitted into the faid emptied receiver, to fee whether any of the feed would then come up, it was found that in the fpace of one week it was grown up to the height of two or three inches.

A Confirmation of the Experiments mentioned in No. XXVII. to have been made by Signor Fracaffati in *Italy*, by injecting Acid Liquors into Blood.

First printed in the *Philofophical Tranfactions*, No. XXIX. p. 551.

For *Monday*, November 11, 1667.

[THE author having feen the particulars inferted in Numb. XXVII. concerning fome experiments made by Signor *Fracaffati*, and recollecting what himfelf had experimented of that nature feveral years ago, was pleafed to give to the publifher the following information about it by the favour of a letter, written to him from *Oxford*, Oct. 19, 1667.]

SIR,

I Hinted to you in my laft fomething about the original of the experiments made in *Italy*, by injecting acid liquors into blood; to explain which, I fhall now tell you, that about this time three years *, I mentioned, at *Grefham* college, to the Royal Society, an odd experiment I had formerly made (not by chance, but defign) upon blood

* The Journals of the Royal Society being looked into by the publifher (who, by the honour of his relation to that illuftrious body, hath the advantage of perufing them, as he by his office hath the care of feeing them faithfully managed) do fully agree with the affirmation of this noble perfon, as well in the circumftance of the time, as the fubftance of the matter in queftion; it being in the month of December of An. 1664, when what is now alledged in this letter was publickly related by its author.

yet warm, as it came from the animal, *viz.* That by putting into it a little aqua fortis, or oil of vitriol, or ſpirit of ſalt (theſe being the moſt uſual acid menſtruums) the blood not only would preſently loſe its pure colour, and become of a dirty one, but in a trice be alſo coagulated; whereas if ſome fine urinous ſpirit, abounding in volatile ſalt, ſuch as the ſpirit of ſal armoniac, were mingled with the warm blood, it would not only not curdle it, or imbaſe its colour, but make it rather more florid than before, and both keep it fluid, and preſerve it from putrefaction for a long time.

This experiment I deviſed, among other things, to ſhew the amicableneſs of volatile ſpirits to the blood; and I remember it was ſo much taken notice of, that ſome very inquiſitive members of the Society came preſently to me and deſired me to acquaint them more particularly with it; which I readily did, though afterwards I made ſome further obſervations about the ſame experiment that I had no occaſion to relate.

This having been ſo publickly done, though I ſhall not ſay that Signor *Fracaſſati* may not have hit, as well as I, upon the experiments publiſhed in his name, yet there is ſo little difference between the warm blood of an animal out of his veins and in them, that it is not very improbable that he may have had ſome imperfect rumour of our experiment without knowing whence it came; and ſo may, without any diſingenuity, have thence taken a hint to make and publiſh what now is engliſhed in the Tranſactions. If it be thought fit that any mention be made of what I related ſo long ſince, I think I can ſend you ſome other circumſtances belonging to it; for I remember I tried it with other liquors (as ſpirit of wine, oil of tartar, oil of turpentine) and I think alſo I can ſend you ſome remarks upon the colour of the upper part of the blood. And I ſhall on this occaſion add, in reference to anatomical matters in general, that after I ſaw how favourably *the Uſefulneſs of Experimental Philoſophy* was received, I was invited to inlarge it in another edition; and for that I provided divers anatomical as well as other experiments, and deſigned many more; ſo that I have by me divers things that would not perhaps be unwelcome to anatomiſts, *&c.*

NEW

NEW EXPERIMENTS

Concerning the Relation between Light and Air, in ſhining Wood and Fiſh; made by the Author, and by him addreſſed from *Oxford* to the Publiſher, and ſo communicated to the Royal Society.

Firſt Printed in the *Philoſophical Tranſactions*, No. XXXI. p. 581.

For *Monday*, January 6, 1667-8.

SIR,

TO perform now the promiſe I made you the other day, I muſt acquaint you with what will perhaps ſomewhat ſurprize you, by giving you an account of what I tried on *Tueſday* night laſt (*October* 29, 1667) and the two or three following nights, about the relation between air and light, as this is to be found in ſome bodies.

THE occaſion of theſe trials was this: Having, as you know, long ſince made ſome notes, chiefly hiſtorical, upon particular qualities, and finding light to be (how juſtly I now diſpute not) reckoned by the generality of philoſophers among qualities, I huddled together what obſervations I had either made myſelf or received from ſome ingenious travellers (to whom I recommended my inquiries) about ſhining bodies; and had alſo prepared ſeveral trials about them, to be made when I ſhould have opportunity and requiſite inſtruments to put them in practice; which, as to ſome of thoſe deſigned experiments, have been long denied me. But having at length got hither one of my little engines, and having alſo procured, after much inquiry, a few ſmall pieces of ſhining wood, I began on the day above-mentioned to try with them an experiment I found in my liſt: and though the main experiment be but one; I intend to ſet down what occurred to me about it but as ſeveral phænomena of it; yet finding it requiſite to acquaint you with ſome trials that are not ſo properly parts of it, I ſhall, for diſtinction ſake, propoſe them as ſeveral experiments; the narratives whereof are taken for the moſt part *verbatim* out of the notes I ſet down for my own uſe, when the things to be regiſtered were freſhly done. Which advertiſement I give you both to excuſe the careleſſneſs of the ſtyle, and to induce you not to diſtruſt a narrative that was made only to ſerve my memory, not an hypotheſis.

EXPERIMENT I.

To try whether or no a piece of ſhining wood, being put into a receiver of our pneumatick engine, would, upon the withdrawing and re-admitting of the air, ſuffer ſuch changes, as I have often obſerved a live coal, placed there, to do. Having at length procured a piece of ſuch wood, about the bigneſs of a groat or leſs, that gave a vivid light (for rotten wood) we put it into a middle-ſized receiver, ſo as it was kept from touching the cement; and the pump being ſet a work, we obſerved not during the five or ſix firſt exſuctions of the air, that the ſplendor of the included wood was manifeſtly leſſened (though it never was at all increaſed); but about the ſeventh ſuck it ſeemed to grow a little more dim, and afterwards anſwered our expectation, by loſing of its light more and more, as the air was ſtill farther pumped out; till at length about the tenth exſuction (though by the removal of the candles out of the room, and by black clothes and hats we made the place as dark as we could) yet we could not perceive any light at all to proceed from the wood.

EXPERIMENT II.

Wherefore we let in the outward air by degrees, and had the pleaſure to ſee the ſeemingly extinguiſhed light revive ſo faſt and perfectly, that it looked to us all, almoſt like a little flaſh of lightning, and the ſplendour of the wood ſeemed rather greater, than at all leſs, than before it was put into the receiver. But partly for greater certainty, and partly to enjoy ſo delightful a ſpectacle, we repeated the experiment with the like ſucceſs as at firſt. Wherefore being deſirous to ſee how ſoon theſe changes might be produced, we included the wood in a very ſmall receiver of clear glaſs, and found that in this the light would begin to grow faint at the ſecond, or at leaſt at the third exſuction of the air, and at the ſixth or ſeventh would quite diſappear. And we found, by a minute-watch, that the ſending the candles out of the room, the pumping out the air till the wood would ſhine no more, the re-admitting of the air (upon which in a trice it would recover its light) and the ſending in for the candles to conſult the watch, did in all take up but ſix minutes.

EXPERIMENT III.

The fore-mentioned experiment, without taking notice how long it laſted, being reiterated twice in this new receiver, we had a deſire to ſee whether this luminous wood would more reſemble a coal, or the life of a perfect animal, in being totally and finally extinguiſhed, in caſe the air were kept from it a few minutes; or elſe the life of inſects, which in our exhauſted receiver I had obſerved to loſe all appearance of its continuing, and that for a much longer time than a few minutes, and yet afterwards, upon the reſtitution of air, to recover preſently, and ſhew manifeſt ſigns of life? Wherefore having exhauſted the receiver till the wood quite diſappeared, we ſtayed ſomewhat above a quarter of an hour in the dark without perceiving that the wood had regained any thing of light, though about the end of this time we made the place about it as dark as we could; and then it being too late at night to protract the experiment, we let in the air, upon whoſe admiſſion the wood preſently recovered light enough to be conſpicuous at a diſtance;

distance; though it seemed to me somewhat less vivid than before: which yet may be either a weakness in my sight, or an effect of the steams of the cement, unfriendly perhaps to the luminousness of the wood.

Thus far we proceeded yester-night, to which we this night added these observations.

We put in a piece of wood bigger than the former (this being above an inch long) and that shone very vigorously. And having by a few sucks quite deprived it of light, we left it in the exhausted receiver for full half an hour, and then coming into the dark room again, we found all had not continued so staunch, but that some small portion of air had insinuated itself into the receiver. This we concluded to be but a small portion of air, because the wood was but visible to an attentive eye: and yet, that it was really some air which was got in that caused the little glimmering light which we perceived, may appear by this, that it did presently (as we expected) vanish at the first or second suck; and then the air being let into the dark receiver, the included wood presently shone again as before; though I suspected I discerned some little diminution of its brightness; which yet, till further trials of the like kind, and for a longer time, have been made, I dare not affirm. Before the receiver was sufficiently emptied at the begining of the experiment made with this great piece of wood, a small leak accidentally sprung, which, letting in a little air, did, sooner than we intended, recall the almost disappearing light.

EXPERIMENT IV.

There is an experiment of affinity with the former which we thought it not altogether impertinent to try; for having observed on another occasion, that sometimes the operation which the withdrawing the air hath upon a body included in the receiver, proves more considerable some minutes after we have ceased pumping, than immediately after the exercise is left off; I imagined that even in such cases where the light is not made wholly to disappear (though it be made almost quite to do so) by the emptying of the pneumatical glass, the suffering the body to remain a while there, though without any pumping (unless now and then a very little to remove the air that might have stolen in, in the mean time the remaining light of the body might be further impaired, if not reduced quite to vanish. To examine this conjecture we put in a body that was not wood, which had some parts far more luminous than the rest; and having drawn out the air, all the others disappeared, and even the formerly brighter ones shone but faintly when the pneumatical glass seemed to be exhausted. But keeping the included body a while in that unfriendly place we perceived the parts that had retained light to grow more and more dim, some of them disappearing, and that which was formerly the most conspicuous, being now but just visible to an attentive eye, and that scarce without dispute. For if we had not known beforehand, that a shining matter had been included in the receiver, perhaps we should not have found it out, and he that had the youngest eyes in the company could not at all discern it (the air being let in, the body began to shine again): but this being a single trial, which the lateness of the night hindered us from re-iterating, is to be further prosecuted, and in differing substances, before much be built upon it.

EXPERIMENT V.

The rarefaction or expanſion of the air having ſo notable an operation upon our ſhining wood, I thought it would not be amiſs to try what the compreſſion of the air would do to it. For which purpoſe we included a piece of it in ſuch a little inſtrument to compreſs, which you may remember to have been deviſed and propoſed by Mr. *Hook*. But though we impelled the air forcibly enough into the glaſs, yet, by reaſon of the thickneſs requiſite in ſuch glaſſes, and the opacity thence ariſing, we were not able then to determine whether or no any change was made in the luminouſneſs of the wood.

Which I thought the leſs ſtrange, becauſe by ſome experiments purpoſely deviſed (at one of which I remember you were preſent) I had long ſince obſerved that even a great preſſure, from a fluid body which preſſeth more uniformly againſt all the parts it toucheth of the conſiſtent body, does work a far leſs manifeſt change, even on ſoft or tender ſubſtances, than one would expect from the force wherewith it compreſſeth.

And were it not that one contrary oftentimes minds us of another, I might have forgot that I had divers thoughts about finding ſome good ways of trying, whether any ſuch change of texture might be diſcovered to be made in the ſhining wood by the abſence and return of the ambient air, as might with any probability have the loſs or recovery of the wood's ſplendour attributed to it. For I had formerly (if I were not miſtaken) found by ſeveral circumſtances, which I ſhall not now ſtay to name, that a ſlight (ſo it be an appropriated) variation of the texture of this wood, and which may ſeem mainly to reſpect the pores (which perhaps ought to be of a determinate ſhape and ſize, and filled with a determinate matter) will have a great operation upon its ſplendour. And I formerly found by other trials, that even conſiſtent bodies, if ſoft ones, may have their pores enlarged and vitiated, and their bulk, and conſequently their texture (at leaſt as to their pores) manifeſtly enough altered by having the air withdrawn from about them (whereby the aërial particles within them were able to expand themſelves) and let in again; whereby, as to ſenſe, they ſeemed pretty well reſtored to their former ſtate. But the ſucceſs of my endeavours, either with microſcopes (through which a vivid piece of wood will ſhine by its own light) or otherwiſe, was not conſiderable enough to deſerve a particular account; eſpecially in this paper, where I am not to venture at matter of theory.

EXPERIMENT VI.

Thinking fit to try whether a ſmall quantity of air, without being ventilated or renewed, might not ſuffice to maintain this cold fire, though it will not that of a live coal, or a piece of match, we cauſed a piece of ſhining wood to be hermetically ſealed up in a pipe of clear and thin glaſs: but though, carrying it into the dark, we found it had quite left its light, yet imagining that that might proceed from its having been overheated (being ſealed up in a pipe not long enough to afford it a due diſtance from the flame of the lamp we employed to ſeal it); we cauſed two or three pieces of freſh wood, amounting all of them to the length of about two inches, to be ſealed up in a ſlender pipe between four or five inches in length, which being warily done, the wood retained its light very well when the operation was over: and afterwards laying it by my bed-ſide,

ſide, when the candles were carried away out of the room, I conſidered it a while before I fell aſleep, and found it to ſhine vividly.

The next morning when I awaked, though the ſun was riſen, yet forbearing to draw open the curtains of my bed till I had looked upon the ſealed glaſs, which I had fenced with a piece of cloth held between it and the window, my eyes having not yet been expoſed to the day-light, ſince the darkneſs they had been accuſtomed to during the night, made me think the wood ſhined brighter than ever. And this night, after ten of the clock, looking on it in a dark place, it appeared luminous all its length, though not ſo much as in the morning.

The morning after, and the night after that, the ſame wood did likewiſe manifeſtly, though not vigorouſly ſhine; eſpecially one piece whoſe light was much more vivid than the reſt: and, for aught I know, I might have obſerved them to ſhine longer, if one of the ſealed ends of the glaſs had not been accidentally broken.

EXPERIMENT VII.

Whilst the former trials were making, I was wiſhing for a good *Bolonian* ſtone to try what effect the withdrawing of the air would have upon it. For though I knew it might be objected that the experiments of light performable in our engine muſt be made in the night, whereas the *Bolonian* ſtone gains its light by being expoſed to the ſun-beams; yet that objection did not hinder my wiſh, ſince the better ſort of *Bolonian* ſtones may be indued with a luminouſneſs by the flame of fire or of large candles.

I also wiſhed for ſuch a ſhining diamond as is now in the hands that beſt deſerve ſuch a rarity, our Royal Founder's. For you may remember, that in the obſervations I made of that ſtone, and annexed to the concluſion of the book of colours, I ſhew how it may ſeveral ways be brought to ſhine; ſo that by one or other of thoſe ways, eſpecially that of external heat, I thought it very likely I ſhould be able to make the light continue four or five minutes, which would be long enough to try in a very ſmall receiver, exhauſtible at a ſuck or two, whether the withdrawing and reſtoring the air would have any viſible operation on it?

I also wiſhed for ſome of the glow-worms with which I formerly made other trials. For though I forgot not what operation the withdrawing of the air, by our engine, is wont to have upon living creatures, yet that made me not forbear my wiſh; not only becauſe of the different effect I have found the engine to have on inſects in reſpect of other animals, but becauſe I am not of the opinion of thoſe modern writers who will have the light of the glow-worms depend altogether upon their life, and end with it. But being not likely by my wiſhes to procure any new ſubject to make trials on, I thought fit at leaſt to do what was in my power; and accordingly (to gratify them, who, I preſumed, would, if preſent, propoſe ſuch a trial) cauſed a piece of iron to be forged whoſe top was of the bigneſs of a nut-meg; the reſt being a ſtem of an inch, or an inch and half long, for which we provided a little candle-ſtick of tobacco-pipe clay, which would not yield any ſmoke to fill and darken the receiver: then having heated the iron red-hot, and placed it in this clay, ſo that the round part was clearly protuberant, we conveyed it into a receiver of white glaſs, which was ſo placed as to keep the ſides at as good a diſtance as we could from the iron, leſt the exceſſive heat ſhould (as we much feared it would) break the glaſs; then ſending away the candles, and making the room dark, we haſtily pumped out the air, but could not perceive the withdrawing of it had any operation on the glowing iron. And though it continued ſhining long enough to give us opportunity to pump out and let in the air three ſeveral times, yet we could not obſerve that the air had any manifeſt operation one way or other. For though, upon the withdrawing of the air, the iron grew dimmer and dimmer, yet that I attributed to the cooling of it; and the rather, becauſe, having (to examine the conjecture) let in two or three times the air when the receiver had been exhauſted, there appeared no manifeſt increaſe of light upon the ſudden admiſſion of it.

EXPERIMENT VIII.

HAVING formerly, in our *Phyſico-mechanical* experiments about the ſpring of the air, obſerved that the air is thus far a vehicle of ſound, that a body but faintly ſounding, being placed in our receiver, gave a yet weaker ſound when the air was withdrawn from about it, than when the receiver was full of air; I preſumed ſome curious perſons would, if they had been preſent, deſire to have a trial made, whether or no a ſmall piece of ſhining wood being ſo included in the receiver, as that the pumping out of the air ſhould have no injurious operation upon the body of it, its light would upon the withdrawing of the air be manifeſtly diminiſhed. And this I was the leſs backward to try, becauſe (not to mention the relation which the former experiments ſhew there may be in ſome caſes between light and air) it did not readily occur to my memory, that by any manifeſt experiment (for I know there are probable reaſons to prove it) it appeared that a body more thin than air will or can tranſmit light, as well as other diaphanous mediums. And thoſe modern atomiſts that think there is in our exhauſted receiver very many times more vacuum than body, would, I preſumed, be glad to be ſupplied with an argument againſt the Peripateticks, to ſhew that the motion of bodies, *viz.* the corpuſcles of light may be freely made *in vacuo*, and proceed without the aſſiſtance of a vehicle.

WHEREFORE, having hermetically ſealed up a ſmall piece of ſhining wood in a ſlender pipe, and placed it in a ſmall receiver that was likewiſe made of clear glaſs, we exhauſted it of air, and afterwards let in again that which we had excluded; but by neither of the operations could we perceive any ſenſible decrement or increaſe of the light of the wood; though by that very obſervation it appeared that the glaſs had been well ſealed, ſince otherwiſe the included air would have got out of the pipe into the receiver, and have left the wood without light.

EXPERIMENT IX.

I HAD alſo a mind to try both what degree of rarefaction of the air would deprive the wood of its ſplendour in ſuch and ſuch meaſures, and whether or no the ſelf-ſame air, which, when rarefied, would not ſuffer the wood to ſhine, would, when reduced to its former denſity, allow it to ſhine as much as before.

THIS I propoſed to do by putting ſome ſhining wood into a clear and conveniently ſhaped glaſs, that the long ſtem or pipe being ſo far filled with quickſilver, as that there might be about half a ſpoonful of air left at the cloſed end where the wood was placed, it might be inverted into a little glaſs of ſtagnant quickſilver, and therewith conveyed into a ſlender receiver, out of which, as the air ſhould come to be pumped, that included in the glaſs which held the wood might be rarefied, and afterwards, upon the

the admission of the outward air (which must impel up the quicksilver to its former height) might be restored to its former state. But when we came to trial of this, we had no receiver conveniently shaped that was so clear and thin as that we could see the wood shine through both the glasses; and though we would for an expedient have substituted a fine thin bladder, wherein the wood was to be put, and a convenient quantity of air strongly tied up with it, yet for want of a bladder fine enough for our turn, that expedient also proved useless to us. But being desirous to make what trial we could by the least unfit means we had in our power, we got an old but thin glass, sealed at one end, whose shape was pretty cylindrical, and whose bore was about the bigness of a man's little finger, and whose length was about a foot or more; into this pipe, near the sealed end, we put a piece of shining wood, wedged in with a piece of cork, to keep it from falling; and having inverted the nose of it into another slender glass, but not cylindrical, wherein was pretty store of quicksilver, we put them both into a long receiver shaped almost like a glass churn; and having pumped a while, that the air included in the pipe, expanding itself, might depress the quicksilver, and so make escapes into the receiver as long as we thought fit; we then let in the outward air that the stagnant quicksilver might be impelled into the cavity of the pipe, now freed from much of the air, to the height requisite for our purpose.

This done, we plied the pump again, and observed that, as the air in the pipe did by its own spring expand itself more and more and grow thinner and thinner, the shining wood grew dimmer and dimmer, till at length it ceased to shine, the internal air being then got a good way lower than the surface of the external quicksilver; whereupon opening the commerce between the cavity of the receiver and the atmosphere, the quicksilver was driven up again, and consequently the air above it was restored to its former density; upon which the rotten wood also recovered its light: what the greatest expansion of this air was we could not certainly determine, because the expansion raised the external quicksilver so high as to hinder us to see and measure it; but we guessed that the air reached to about a foot or more from the top of the pipe to the surface of the quicksilver near the bottom of it. But when that rarefied air was impelled into its former dimensions, we measured it, and found that the upper part of the tube, unpossessed by the quicksilver, was about three inches; and the wood being about an inch long, there remained two inches, or somewhat better, for the air. But this experiment is to be repeated when exacter instruments can be procured.

EXPERIMENT X.

Thinking it fit to try, as well whether stinking fish that shines be of the same nature, as to luminousness, with rotten wood that shines too; as whether the withdrawing of the air will extinguish or eclipse the light of a considerable bulk of luminous matter, as in the experiments hitherto made we found it would do to a small one; we took a fish that we had kept and caused to be watched till it was almost all over luminous; though much more in the belly and some parts of the head than elsewhere; and having suspended him in a conveniently-shaped receiver, we found him to give so great a light, that we suspected beforehand that the withdrawing of the air would hardly have its full operation upon a body whose bulk was considerable, as well as its light very vivid, and which had many luminous parts retired to a pretty distance from the air. Accordingly, having exhausted the receiver as much as we were wont, it appeared, indeed, especially towards the latter end of the operation, that the absence of the air did confiderably leffen, and in fome places eclipfe the light of thofe parts that fhone lefs ftrongly; but the belly appeared not much lefs luminous than before: wherefore, fuppofing that, upon the turning of the ftop-cock the air coming in much more haftily than it could be drawn out, we fhould have the beft advantage to difcern what intereft it had in the luminoufnefs of the fifh, we re-admitted it; and, upon its rufhing in, perceived the light to be, as it were, revived and increafed; thofe parts of the fifh that were fcarce vifible before or fhone but dimly, receiving prefently their former fplendour.

AND not to leave un-profecuted the remaining part of the experiment, which was to try whether it was the kind of the luminous body, or only the greatnefs of the bulk, and the vividnefs of light, and, if I may fo fpeak, the tenacity of the fubftance it refided in that made the difference between the fifh and the wood, we put part of the fifh of another kind that fhone much more faintly than that hitherto fpoken of, and but in fome places; and by the withdrawing the air, we made fome of the luminous parts difappear, and the others fo dim as fcarce to be difcerned; and yet both the one and the other regained their former light upon the return of the air.

AND to purfue the experiment a little further, we put in fuch a piece of the firft fifh, as though it were bright, was yet but thin, and not confiderably great; and upon pumping out the air we found it, according to our expectation, quite eclipfed, though it recovered its light upon the air's re-entry.

THESE, Sir, are the experiments I have lately made about the fhining bodies in our engine. More I would have tried, notwithftanding the trouble we found in managing the engine in the dark, if rotten wood had not failed us, and I were not in a place where the glafs-men's fhops are not near fo well furnifhed as the ftationers.

I SCARCE doubt but thefe experiments will occafion among the virtuofi feveral queries and conjectures, according to the differing hypothefes and inquifitions to which men are inclined: and, particularly, it is probable that fome will make ufe of this difcourfe to countenance their opinion, that notwithftanding the coldnefs (at leaft as to fenfe) of fifhes and other animals, there may be in the heart and blood a vital kind of fire which needs air, as well as thofe fires that are fenfibly hot; which may leffen the wonder that animals fhould not be able to live when robbed of air: and if I had now time, I could poffibly furnifh you with fome other trials that feem much to favour the comparifon, though, as to the opinion itfelf of a vital flame, I fhall not now tell you my thoughts about it. And though not only the Cartefians will perhaps draw an argument from the paft phænomena in favour of their theory of light, but divers others will difcourfe upon them, and propofe further queftions, and perhaps inquiries, fuitable to their feveral hypothefes; yet I fhall content myfelf at prefent to have faithfully delivered the hiftorical part of thefe appearances, without making, at leaft at this time, any reflections on them. And the rather, indeed, becaufe I enjoyed fo little health when I was making the experiments, that it was not fit for me to engage in fpeculations that would much exercife my thoughts; which, I doubt, have been more gratified than my health hath been by the bare trials, which are moft feafonably made at hours unfeafonable for one that is not well.

Poftfcript fent by the fame noble Author from the fame place, December 6, 1667.

My condition, in point of health, being not much improved fince I writ to you in October *laft, when I fhall have added, that I have not thefe five or fix weeks been able to procure any fhining wood (except one fingle piece, which though large, was fo ill conditioned that it afforded me but one trial) you will not, I hope, expect that I fhould add much to the experiments I formerly fent you about the relation betwixt light and air. But however, fince the fubject is new and noble, and fince your curiofity about other matters has been fo welcome and ufeful to the Virtuofi, I fhall not decline, even on this occafion, to comply with it; and the rather, becaufe I half promifed you fome additionals a good while fince, and becaufe too, that though what I fhall acquaint you with may feem to be but a confirmation of two or three of the former experiments, yet, befides that it is of them which moft needed a confirmation, thefe trials will alfo afford fome circumftances that will not, I think, be unwelcome.*

EXPERIMENT XI.

To examine then the conjecture mentioned in the laft experiment, that the durablenefs of the light in the fhining fifh, in fpite of the withdrawing the air, might proceed in great part from the vividnefs of it, and the beauty of the matter it refided in, rather than from the extent of the luminous body in comparifon of the fmall pieces of fhining wood I hitherto had made my trials with; I put in the above-mentioned piece of wood, whofe luminous fuperficies might be perhaps ten or twelve times as great as that which the eye faw at once, of the furface of fuch fragments of fhining wood as I was wont to employ; and though fome parts of this large fuperficies fhined vividly (for the light was ufually enough for rotten wood inferior to that of our fifh) yet this great piece, being put into a convenient receiver, was, upon the withdrawing of the air, deprived of light, as the fmaller ones had been formerly; the returning air reftoring its light to the one, as it had done to the other.

EXPERIMENT XII.

But this is not the chief thing I intended to acquaint you with; that being the fuccefs of fome trials which we made in profecution of thefe two neighbouring experiments.

In the firft of thefe I told you I had been able to try but for half an hour, or a little more, that a fhining piece of wood deprived in our engine of light, would yet retain a difpofition to be, as it were, rekindled upon the frefh accefs of the air; wherefore, though I could have wifhed to have made a further trial with the fame kind of bodies, yet being able to procure none, I fubftituted in their room fmall pieces of rotten fifh that fhone, fome of them more faintly, and fome of them more vividly; in reference to one another, but none as ftrongly as fome that I could have employed: and having, in a very fmall and clear receiver, fo far drawn off the air as to make the included body difappear, we fo ordered the matter, that we kept out the air for about 24 hours; and

and then allowing the air to re-enter in a dark place, and late at night, upon its firſt admittance the fiſh regained its light.

EXPERIMENT XIII.

This, compared with ſome of my former obſervations about putrefaction, put me upon a trial, which, though it miſcarried, I ſhall here make mention of, that in caſe you, who are better furniſhed with glaſſes, think it worth while, you may get reiterated by the Society's operator. Conſidering then, how great an intereſt putrefaction hath in the ſhining of fiſhes, and air in the phænomena of putrefaction, I thought it might be ſomewhat to the purpoſe to take a fiſh that was, according to the common courſe I had obſerved in animals, not far from the ſtate at which it would begin to ſhine; and having cut out a piece of it, I cauſed the reſt to be hung up again in a cellar, and the exſected piece to be put into a ſmall and tranſparent receiver, that we might obſerve, if a day or two, or more, after the fiſh in the cellar ſhould begin to ſhine, that in the exhauſted receiver would either alſo ſhine, or (becauſe that ſeemed not likely) would, notwithſtanding the check which the abſence of the air might be preſumed to give the putretrefaction, be found to ſhine too, either immediately upon the admiſſion of the air, or not long after it.

But this experiment, as I lately intimated, was only deſigned and attempted, not completed, the receiver being ſo thin, that upon the exhauſtion of the internal air the weight of the external broke it; and we could ill ſpare another of that kind from trials we were more concerned to make: notwithſtanding which we made one trial more, which ſucceeded no better than the former, but miſcarried upon a quite differing account, *viz.* becauſe neither the included piece of fiſh, nor the remaining, though it were of the ſame ſort with the fiſhes I uſually employed, would ſhine at all, though kept a pretty while beyond the uſual time at which ſuch fiſhes were wont to grow luminous.

If this experiment had ſucceeded, I had ſome others to try in proſecution of it, which I ſhall not now trouble you with the mention of. But that this paragraph may not be uſeleſs to you, I will take this occaſion to give you a couple of Advertiſements that may relate not only to this experiment, but alſo more generally to thoſe, whether precedent or ſubſequent, where ſhining fiſh are employed.

ADVERTISEMENT I.

IN the firſt place then, I will not undertake that all the experiments you ſhall make with rotten fiſh ſhall have juſt the ſame ſucceſs with theſe I have related. For, as I elſewhere obſerved (in a diſcourſe written purpoſely on that ſubject) that the event of divers other experiments is not always certain; ſo I have had occaſion to obſerve the like about ſhining of fiſhes. And beſides what I lately took notice of at the cloſe of the tenth Experiment, I remember, that having once deſigned to make obſervations about the light of rotten fiſhes, and having in order thereunto cauſed a competent number of them to be bought, not one of them all would ſhine, though they were bought by the ſame perſon I was wont to employ, and hung up in the ſame place where I uſed to have them put, and kept not only till they began to putrefy, but beyond the time that others uſed to continue to ſhine; although a parcel of the ſame kind of fiſhes bought the week before, and another of the ſame kind bought not many days after, ſhined according to expectation.

expectation. What the reason of this disappointment was, I could not determine; only I remember that at the time it happened, the weather was variable, and not without some days of frost and snow. Nor is this the oddest observation I could relate to you about the uncertain shining of fishes, if I thought it necessary to add it in this place.

ADVERTISEMENT II.

Notice must also be taken in making experiments with shining fish, that their luminousness is not wont to continue very many days. Which advertisement may be therefore useful, because without it we may be apt sometimes to make trials that cannot be soon enough brought to an issue; and so we may mistake the loss of light in the fish, to be a deprivation of it caused by the experiment; which indeed is but a cessation, according to the usual course of nature.

EXPERIMENT XIV.

I know not whether you will think it worth while to be told of a trial that we made to save those criticks a labour, that else might perhaps demand why it was not made. We put therefore a piece of shining fish into a wide-mouthed glass, about half filled with fair water, and having placed this glass in a receiver, we exhausted the air for a good while, to observe whether when the pressure of the air was removed, and yet (by reason of the water that did before keep the air from immediately touching the fish) the exhaustion of the receiver did not deprive the fish of that contact of air, which it had lost before; whether, I say, in this case the absence of the air would have the same influence on the shining body, as in the former experiments; and here, as far as the numerous bubbles excited in the water would give us leave to discern it (for they did, though not unexpectedly, somewhat disturb the experiment, which inconvenience we might have prevented, if we had thought it worth while) we could not perceive, that either the absence or return of the air had any great operation upon the light of the immersed body: which yet did not keep me from intending to make a somewhat like trial with shining wood (when I can get any) fastened to the lower part of a clear glass, and covered over, but not very deep, with quicksilver. Of which practice I shall not now stay to give you the reasons, having elsewhere fully enough expressed them.

And that this Section may acquaint you with something besides the (seemingly) insignificant experiment related in it, I shall here inform you (since I perceive I did not in the first papers I sent you) that though when I formerly put together some notes about luminous bodies, I confined not my observations to one or two sorts of fishes, yet the experiments sent you since *October* last, were all of them (except a collateral one or two) made with whitings, which, among the fishes I have had occasion to take notice of, is (except one sort that I cannot procure) the fittest for such trials, and consequently fit to be named to you, to facilitate their future ones, in case you think it requisite to make any upon such subjects.

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EXPERIMENT XV.

The other of the two neighbouring experiments I lately mentioned (*viz.* the ninth) I told you, when I fent it you, needed a reiteration to confirm it, fince we had but once tried it (and that without all the conveniency we defired) that a fhining body, which upon the firft withdrawing the air lofeth much, but not all its light, may be deprived of the reft by continuing in that unfriendly place, though the air be no farther exhaufted. To profecute therefore both the experiments in one trial, we took fomewhat late at night a piece of rotten fifh, which we judged to fhine too ftrongly to be quickly deprived of all its light, and having put it into a fmall and clear receiver, we found (as we had forefeen) that the light was much impaired, but nothing near fuppreffed by the withdrawing of the air. Wherefore, having removed the receiver into a convenient place, I caufed it to be brought to me about midnight (after I was a-bed) and having by clofe drawing the curtains, and other means, made the place pretty dark, I perceived the included body to continue to fhine more vividly than one would have expected (and, if I miftake not, I faw it fhining in the morning, whilft it was dark); but the night after, coming to look upon it again, its light appeared no more: notwithftanding which I made a fhift to keep out the air about 24 hours longer, and fo after 48 hours in all, we opened the receiver in a dark place, and prefently upon the ingrefs of the air were pleafingly faluted with fo vivid an apparition of light, that the included body continued to fhine when carried into a room where there was both fire and candle, if it were but by a hat fcreened from their beams.

Being encouraged as well as pleafed with this fuccefs, we forthwith exhaufted the air once more out of the fame receiver, and having kept it about four hours longer, we looked upon it again in a dark place, and finding no appearance of light, let the air in upon it, whereby it was made to fhine again, and that vigoroufly enough, fo that I caufed the receiver to be exhaufted once more; but that it being *Sunday* night, I was unwilling to fcandalize any, by putting my fervants upon a laborious and not neceffary work.

The fuddennefs with which the included body appeared to be, as it were, re-kindled upon the firft contact of the air, revived in me fome fufpicions I have had about the poffible caufes of thefe fhort-lived apparitions of light (for I fpeak not now of real lamps found in tombs, for a reafon to be told you another time) which difclofing themfelves upon men's coming in, and confequently letting in frefh air into vaults, that had been very long clofe, did foon after vanifh. Thefe thoughts, as I was faying, occurred to me upon what I had been relating, by reafon of the fudden operation of the frefh air upon a body, that but a minute before difclofed no light. For though the lights reported to have been feen in caves quickly difappeared, which that of our fifh did not; yet that difference might poffibly proceed from the tenacity, or fome other difpofition of the matter wherein the luminoufnefs of the fifh refides. For I remembered that I had more than once obferved a certain glimmering and fmall light to be produced in a fort of bodies upon putting them out of their former reft, and taking them into the air, which fparks would vanifh themfelves fometimes within one minute, fometimes within a few minutes. But as thefe thoughts were but tranfient conjectures, fo I fhall not entertain you any longer about them, but rather contenting myfelf with the hint already given, take notice of what may be more certainly deduced from our experiment; which is, that the air may have a much greater intereft in divers odd phænomena of nature, than we are hitherto aware of.

An-

And for confirmation of our experiment I ſhall add, that, having in another receiver eclipſed a piece of fiſh that ſhone, when it was put in, more languidly than divers others that we had tried, I kept it about three days and three nights in a receiver, which (receiver) being ſomewhat like another, at firſt ſuggeſted to me, when I came to take it, ſome ſcruple; but afterwards, upon farther examination, concluded it to be the ſame: wherefore I opened it in the dark, and upon letting in the air on this body, that ſhined but faintly at firſt, it immediately recovered its long ſuppreſſed light. And having included another piece, that was yet more faint than this when it was put into the receiver, I thought fit to try at once the experiment hitherto confirmed, and the converſe of it. And therefore having kept this piece alſo three days and three nights in the exhauſted glaſs, I let in the air upon it, and notwithſtanding the darkneſs of the place, nothing of light was thereupon revived. But this being little other than I expected from a body that ſhined ſo faintly when it was put into the receiver, and had been kept there ſo long, I reſolved to exerciſe my patience a while as well as my curioſity, and try whether the appulſe and contact of the air would have that operation after ſome time, that it had not at firſt; and accordingly, after having waited a while, I obſerved the fiſh to diſcloſe a light, which, though but dim, was manifeſt enough; but having conſidered it for ſome time, I had not leiſure to watch whether it would increaſe, or how long it would continue.

I know not, Sir, whether you are weary with reading, but I am ſure I am quite tired with making ſo many experiments upon one ſubject; and therefore I ſhall here conclude this paper, as ſoon I have added this confirmation, as well of what I laſt related, as of ſomething that I obſerved before, that having included in ſome receivers two pieces of rotten whitings, whereof the one, before it was put in, ſcarce ſhone ſo vividly, as did the other after the receiver was exhauſted; and having ordered the matter ſo, that we were able to keep out the air for ſome days, at the end of about 48 hours we found, that the more ſtrongly ſhining body retained yet a deal of light. But afterwards looking upon them both in a dark place, we could not perceive in either any ſhow of light. Wherefore having let in the air into that receiver, whereinto the body that at firſt ſhined the faintlier had been put, there did not enſue any glimmering of light for a pretty while; nay, upon the ruſhing in of the air into the other glaſs (then alſo made acceſſible to the atmoſphere) the body that at firſt ſhone ſo ſtrongly, and that continued to ſhine ſo long, ſhewed no glimmering of light. But being reſolved to expect the iſſue a while longer, our patience was rewarded in leſs than a quarter of an hour with the ſight of a manifeſt light in the body laſt named; and a while after the other became viſible, but by a light very dim. The more luminous of theſe bodies I obſerved to retain ſome light twenty-four hours after: and the hitherto recited experiment had this peculiar circumſtance in it, that the two receivers were un-interruptedly kept exhauſted no leſs than four days, and as many nights.*

* What method the noble author of theſe experiments uſed in keeping out the air for ſo long a time, will probably be made known ere long by himſelf.

OBSERVATIONS and TRIALS about the Reſemblances and Differences between a BURNING COAL and SHINING WOOD.

Firſt Printed in the *Philoſophical Tranſactions*, No. XXXII. p. 605.

For *Monday*, February 10, 166⅞.

THESE particulars were already in our hands, when we publiſhed the experiments made on ſhining wood and fiſh, in the laſt papers, imparted then by the ſame noble author that thoſe were; but wanted then room enough to contain theſe, which now follow, as they were ſent in a letter from Oxford, *viz.*

AND now, Sir, ſeeing the want of ſhining wood hath kept me ever ſince I ſent you the former experiments from making any new ones on that ſubject, I ſhall, by way of amends, ſubjoin ſome of the obſervations that I heretofore intimated to you I had made of the reſemblances and differences between a live coal and a piece of ſhining wood; in peruſing of which you will eaſily diſcern, that to thoſe particulars, which my memory and the former obſervations, I had noted down about light and luminous bodies, had ſuggeſted to me, I have added ſome that have been afforded me by thoſe late trials made in my engine, whereof I ſent you an account.

RESEMBLANCES.

THE things wherein I obſerved a piece of wood and a burning coal to agree or reſemble each other, are principally theſe five:

1. *Both of them are luminaries, that is, give light, as having it (if I may ſo ſpeak) reſiding in them; and not like looking-glaſſes or white bodies, which are conſpicuous only by the incident beams of the ſun, or ſome other luminous body which they reflect.*

THIS is evident, becauſe both ſhining wood and a burning coal ſhine the more vividly, by how much the place, wherein they are put, is made the darker by the careful excluſion of the adventitious light. It is true that the moon and Venus appear brighteſt at or about midnight, and yet have but a borrowed light; but the difference between thoſe planets and the bodies we treat of, in reference to the difficulty we are conſidering, is obvious enough. For though the beholder's eye, that looks upon thoſe ſtars, be advantaged by being in the dark, which enlarges the pupil of the eye, yet the object itſelf is freely expoſed to the beams of the ſun; which, if they were intercepted, thoſe planets would quickly be darkened, as experience manifeſts in eclipſes.

2. *Both*

2. *Both shining wood and a burning coal need the presence of the air, and are too of such a density, to make them continue shining.*

THIS has been proved as to a coal, by what I long since published in my *Physico-mechanical Experiments*, where I relate how quickly a coal would be extinguished upon the withdrawing the air from about it: and as to shining wood, the experiments I lately sent you, make it needless for me to add any other proof of the requisiteness, not only of air, but of air of such a thickness, to make its light continue. How far this is applicable to flame, it is not necessary here to determine; though when I have the satisfaction of seeing you again, I may tell you something about that question, which perhaps you do not expect.

3. *Both shining wood and a burning coal, having been deprived for a time of their light, by the withdrawing of the contiguous air, may presently recover it by letting in fresh air upon them.*

THE former part of this particular, trials have often shown you to be true, when kindled coals, that seem to be extinguished in our exhausted receivers, were presently revived when the air was restored to them: and the latter part is abundantly manifest by the experiments, to which this paper is an appendance.

4. *Both a quick coal and shining wood will be easily quenched by water and many other liquors.*

THE truth of this, as to coals, is too obvious to need a proof; and therefore I shall confirm it only as to wood. For which purpose you may be pleased to take the following transcript of some of my notes about light.

I TOOK a piece of shining wood, and having wetted it with a little common water in a clear glass, it presently lost all its light *.

THE like experiment I tried with strong spirit of salt, and also with weak spirit of sal armoniac; but in both the light did, upon the wood's imbibing of the liquor, presently disappear.

AND lest you should think that in the words, *many other liquors*, I intended not to comprise any that consist of soft and unctuous parts, or that are highly inflammable, I shall subjoin a couple of notes that I find next to those just now transcribed.

I MADE the like trial with rectified oil of turpentine, with a not unlike success. The same experiment I tried more than once with high rectified spirit of wine, which did immediately destroy all the light of the wood that was immersed in it; and having put a little of that liquor with my finger upon a part of the whole piece of wood, that shone very vigorously, it quickly did, as it were, quench the coal, as far as the liquor reached; nor did it in a pretty while regain its luminousness (which whether it recovered at all, I know not; for this trial being made upon my bed, I fell asleep before I had waited long enough to finish the observation).

5. *As a quick coal is not to be extinguished by the coldness of the air, when that is greater than ordinary, so neither is a piece of shining wood to be deprived of its light by the same quality of air.*

As much of this observation as concerns the coal will be readily granted; and for proof of the other part of it, I could relate to you more trials than one, but that I suppose one may suffice, circumstanced like that which I shall now relate.

* From hence you will easily gather the reason why, when I lately told you of the trial I made with a piece of shining fish under water in the unexhausted receiver, I did not propose to have the like trial made with shining wood and water, but for this liquor substituted mercury.

I TOOK a ſmall piece of ſhining wood, and put it into a ſlender glaſs-pipe, ſealed at one end, and open at the other, and placed this pipe in a glaſs-veſſel, where I cauſed to be put a ſtrongly frigorifick mixture of ice and ſalt; and having kept it there full as long as I thought would be requiſite to freeze an aqueous body, I afterwards took it out, and perceived not any ſenſible diminution of its light. But to be ſure the frigorifick mixture ſhould not deceive me, I had placed by this pipe another almoſt filled with water, which I found to be turned into ice; and though I ſuffered the wood to remain a pretty while after, expoſed to ſo intenſe a cold, yet when I took it out, it continued ſhining, and, if I miſtake not, it ceaſed not to do ſo, when I looked on it, twenty-four hours after. But though the light of ſhining fiſh be uſually (as far as I have obſerved) more vigorous and durable than that of ſhining wood; yet I cannot ſay that it will hold out againſt cold ſo well as the other. For having ordered one of my ſervants to cut off a good large piece of the luminous whiting, and bury it in ice and ſalt, when I called for it in leſs than half an hour after, I found it much ſtiffened by the cold, and to have no light, that I could diſcern in a place dark enough. And for fear that this effect may have proceeded not barely from the operation of the cold, but alſo from that of the ſalt (for which ſuſpicion you would ſee reaſon enough, if I could ſhew you my trials about ſhining fiſh) I cauſed another time a piece of whiting to be put in a pipe of glaſs ſealed at one end, and having ſeen it ſhine there, I looked upon it again, after it had ſtayed but a quarter of an hour, by my eſtimate, in a frigorifick mixture, which the glaſs kept from touching the fiſh; and yet neither I, nor a youth that I imployed to look on it, could perceive in a dark place, that it retained any light; which whether the cold had deprived it of by that great change of texture, that the congelation of the aqueous juice of the fiſh (which I have ſeveral times obſerved to be luminous) may be ſuppoſed to have made in the body invaded by it; or whether the effect depend more principally on ſome other cauſe, I ſhall not now examine.

DIFFERENCES.

1. *The firſt difference I obſerved betwixt a live coal and ſhining wood, is that whereas the light of the former is readily extinguiſhable by compreſſion (as is obvious in the practice of ſuddenly extinguiſhing a piece of coal by treading upon it) I could not find that ſuch a compreſſion as I could conveniently give, without loſing ſight of its operation, would put out or much injure the light even of ſmall fragments of ſhining wood:* one of my trials about which I find thus ſet down among my notes about light.

I TOOK a piece of ſhining wood, and having preſſed it between two pieces of clear glaſs (whereof the one was pretty flat, and the other convex) ſo that I could clearly ſee the wood through the glaſs, I could not perceive that the compreſſion, though it ſometimes broke the wood into ſeveral fragments, did either deſtroy or conſiderably alter the light.

THIS experiment I repeated with the ſame ſucceſs. But what a ſtronger or more laſting compreſſion may do in this caſe, I had not opportunity to try.

2. *The next unlikeneſs to be taken notice of betwixt rotten wood and a kindled coal is, that the latter will in a very few minutes be totally extinguiſhed by the withdrawing of the air; whereas a piece of ſhining wood, being eclipſed by the abſence of the air, and kept ſo for a time, will immediately recover its light, if the air be let in upon it again within an hour after it was firſt withdrawn.*

THE

The former part of this obfervation is eafily proved by the experiments that have been often made upon quick coals in the pneumatical engine; and the truth of the latter part appears by an experiment about fhining wood made by us in *October* laft. Neither is it unprobable that if I had had the conveniency to try it, I fhould have found that a piece of fhining wood, deprived of its light by the removal of the ambient air, would retain a difpofition to recover it upon the return of the air, not only for half an hour (which is all that I lately afferted) but for half a day, and perhaps a longer time.

3. *The next difference to be mentioned is, that a live coal being put into a fmall clofe glafs will not continue to burn for very many minutes, but a piece of fhining wood will continue to fhine for fome whole days.*

The firft part of the affertion I know you readily grant; and the rather, becaufe it contains matter of fact, without at all determining, whether the coal's not continuing to burn proceeds from its being, as it were, ftifled by its own fmoak and exhalations (which can have no vent in a fmall clofe glafs) or from the want of frefh air, or from any particular caufe, which I muft not here debate; though I have fometimes made experiments fomewhat odd, to facilitate that enquiry. The other part of our obfervation may be eafily made out by what I tried upon fhining wood, fealed up hermetically in very fmall glaffes, where the wood did for feveral days (though I remember not precifely how many) retain its light.

4. *A fourth difference may be this; that whereas a coal, as it burns, fends forth ftore of fmoke or exhalations, luminous wood does not fo.*

5 *A fifth, flowing from the former, is, that whereas a coal in fhining waftes itfelf at a great rate, fhining wood does not.*

These two unlikeneffes I mention together, not only becaufe of their affinity, but becaufe what concerns the coal in both, will need no proof; and as for what concerns rotten wood, it may be verified by an obfervation, that, I find by my notes, I made in a fmall clear glafs; where, after it had continued luminous fome days, I looked on it in the day-time to perceive if any ftore of fpirits or other fteams had, during all that while, exhaled from the wood; but could not find any on the infide of the glafs, fave that in one place there appeared a kind of a dew, but confifting of fuch very fmall drops (if at leaft their fize were not below that name) that a multitude of them would go to the making up of one ordinary drop. But in pieces of fhining fifh I found the cafe much otherwife, as was to be expected.

6. *The laft difference I fhall take notice of betwixt the bodies hitherto compared, is, that a quick coal is actually and vehemently hot; whereas I have not obferved fhining wood to be fo much as fenfibly lukewarm.*

What is faid of the coal's heat being as manifeft as its light, I fhall need only to make out what relates to the fhining wood. To affift me wherein, I meet among my notes that, whofe tranfcript I fhall fubjoin, when I have premifed that (if my memory do not deceive me) the piece of wood to be mentioned was one that fhone fo vividly, that waking in the night fome hours before I tried it, and perceiving, as it lay near me on the bed, how luminous it was, I was invited to reach out to a place near the bed's-head, where there ftood feveral books, and laying the wood on that which came to hand, I could difcern by the light of it, that the book was an Hebrew bible, and that of the page I lighted on, the wrong end was turned upwards: to which intimation having added, that the little glafs inftrument, mentioned in the note, is fuch an one as you may find defcribed in my preliminaries to the hiftory of cold, fave that part of this was a little bending inward at the bafis, that it may fometimes ftand by itfelf, and fometimes receive a fmall body into the dimple at its bafis; having, I fay, premifed this,

this, and that as ſhining wood did not feel at all warm to me, ſo I alſo found ſhining fiſh palpably cold, I ſhall conclude your trouble with the premiſed note, which ſpeaks thus:

[I PUT upon a large piece of wood which was partly ſhining, and, as near as I could, upon one of the moſt luminous parts of it, one of thoſe thermoſcopes that I make with a pendulous drop of water. But as I had formerly tried, that by laying the tip of my noſe or finger upon it, when it ſhone vividly enough to enable me to diſcern both the one and the other, at the time of contact I could not perceive the leaſt of heat, but rather an actual coldneſs; ſo by this trial I could not ſatisfy myſelf that it did viſibly raiſe the pendulous drop, though the inſtrument were ſo tender, that by approaching one finger near it, yet without actually touching of it, it would manifeſtly be impelled up, and upon the removal of my finger, would preſently deſcend again.]

AND I remember, that having put ſuch an inſtrument upon a ſhining fiſh that was pretty large, I could not perceive that it had any degree of heat, but rather the contrary. For having divers times taken off the glaſs to apply it with the more advantage to ſeveral parts of the luminous fiſh, I divers times (for I remember not whether it were always) took notice that upon the removal of the glaſs into the air, the pendulous drop would manifeſtly riſe a little, and ſubſide again, when the glaſs was applied tō the fiſh. But whether this part of the experiment will hold in all temperatures of the air, I had not opportunity to try.

A

CONTINUATION

OF

NEW EXPERIMENTS

PHYSICO-MECHANICAL,

TOUCHING THE

SPRING and WEIGHT of the AIR, and their EFFECTS.

THE FIRST PART.

Written by way of LETTER, to the Right Honourable the Lord CLIFFORD AND DUNGARVAN.

WHERETO IS ANNEXED

A Short Difcourfe of the ATMOSPHERES of CONSISTENT BODIES.

The PREFACE.

HAVING at the beginning of the treatife, whereof this is a continuation, acquainted my readers with feveral things that belong, in common, as well to the following experiments, as to thofe there publifhed; it will not be neceffary for me to trouble the reader with a repetition of what he may have met with there already, nor to acquaint him in this addrefs with any other particulars than thofe that concern the experiments I am now about to prefent him.

I doubt not but it will be remembered by fome, that I feemed in the above-mentioned book to have promifed a fecond part to it, or a large appendix to it; but intimations

mations of that kind do many times refpect only the thing itfelf, leaving the giver of them free in point of time; and I wanted not fufficient inducements to delay a while to perform my promife, if I made any. I had, indeed, partly before the book already referred to came from the prefs, and partly fome time after, made divers other trials, in order to a fupplement of it: but being obliged to make fome journeys and removes, which allowed me no opportunity to profecute the experiments, I had made no very great progrefs in my defign before the convening of an illuftrious affembly of virtuofi, which has fince made itfelf fufficiently known under the title of THE ROYAL SOCIETY. And having then thought fit to make a prefent, to perfons fo like to employ it well, of the great engine I had then made ufe of in the phyfico-mechanical experiments about the air; and being unable afterwards to procure another fo good, I applied my ftudies to other fubjects, and gave over, for a great while, the care of making more experiments of that kind; and the rather, becaufe that finding by the very favourable reception thofe I had publifhed had met with among the curious in feveral parts of *Europe*, that they were like to be confidered and perufed, I thought I might fafely leave the profecution of them to others, who would probably come more frefh and untired to fuch an exercife of their curiofity.

BUT obferving that the great difficulties men met with in making an engine that would exhauft and keep out a body fo fubtle as air, and fo ponderous as the atmofphere (befides, perhaps, fome other impediments) were fuch, that in five or fix years I could hear but of one or two engines that were brought to be fit to work, and of but one or two new experiments that had been added by the ingenious owners of them; I began to liften to the perfuafions of thofe that fuggefted that unlefs I refumed this work myfelf, there would fcarce be much done in it. And therefore having (by the help of other other workmen than thofe I had unfuccefsfully employed before) procured a new engine, lefs than the other, and differing in fome circumftances from it, we did (though not without trouble enough) bring it to work as well as the other, and, as to fome purpofes, better; and having once got this, I made hafte to try with it thofe experiments that belonged to the defigned continuation, and do now make up this book.

I HOPE that to fuch readers as the following papers are principally intended for, I fhall not need to make an apology either for the plainnefs of my ftile (wherein I aimed at perfpicuity, not eloquence) or for my not having adorned or ftuffed this treatife with authorities, or fentences of claffic authors, which I had neither the leifure to feek, nor thought I had any great need to employ, though it had been far more eafy, than perhaps it would have proved, to borrow from them things that would have been very proper to a treatife, where my main defign was to make out, by practical experiments, divers things, among others, that have not hitherto been advantaged by that way of probation, nor perchance thought very capable of it; fo that I fhall have obtained a great part of what I aimed at, if I have fhewn that thofe very phænomena, which the fchool-philofophers and their party urge, and fometimes triumph in, as clear proofs of nature's abhorrency of a vacuum, may be not only explicated, but actually exhibited, fome by the gravity, and fome alfo by the bare fpring of the air; which latter I now mention as a diftinct thing from the other; not that I think it is actually feparated in thefe trials (fince the weight of the upper parts of the air does, if I may fo fpeak, bend the fprings of the lower) but becaufe that having in the already publifhed experiments, and even in fome of thefe, manifefted the efficacy of the air's gravitation on bodies, I thought fit to make it my tafk in many of thefe, to fhew that moft of the fame things that are done by the preffure of all the fuperincumbent atmofphere acting as a weight,

may

may be likewife performed by the preffure of a fmall portion of air, included indeed (but without any new compreffion) acting as a fpring.

The prefent firft part of our continuation might, I confefs, have been, not inconveniently, divided into two parts. For firft, it contains fome experiments that are already related in the printed book, though they be here fo repeated as to be confirmed, illuftrated, or improved, by being reiterated either with better inftruments, or with better fuccefs, than when they were made in my large receiver, which holding (if I mifremember not) about eight gallons, could not eafily be fo well exhaufted as thofe fmall receivers I often fince employed. And fecondly, the other, and far more numerous fort of experiments, related in this firft part, are new, and fuperadded. And yet I forbear to affign each of thefe two forts a place by itfelf, becaufe I could not conveniently fet down my trials otherwife than as they came to hand among my notes; and I confidered that in divers places the new ones and the old ones being mentioned together, might ferve by their neighbourhood to illuftrate or confirm each other. And however, at another edition of our Continuation, it will be a very eafy tafk, if it appear to be a requifite one, to give the improvements of the former experiments, and the fuperadded new ones, diftinct titles and places.

As for the mechanical contrivances I employed in making the following experiments, though moft of them have had the good fortune to meet with an approbation, and fome of them with more than that, from no mean virtuofi and mathematicians, yet as I expect that critical readers will judge that in fome experiments more artificial inftruments might have been made ufe of, fo I hope that they will not look upon thofe I was reduced to employ, as always the beft that ever I could have directed, fince it fufficiently appears by divers paffages of the following experiments, that they were not made at *London*, but in places where the want of a glafs-houfe, and other accommodations reduced me to make my trials not after the beft manner I could devife, but in the beft way I could then and there put in practice. And let me add on this occafion, to what I have elfewhere faid to the like purpofe, that it is both a great difcouragement to many ingenious men, and no fmall hindrance to the advancement of natural philofophy, that fome nice criticks are fo cenforious in exacting from attempters the very beft contrivances, and many that would be attempters, ftand too much in awe of fuch mens judgments; for though in very nice experiments the exactnefs of inftruments is not only defireable and ufeful, but, in fome cafes, neceffary; yet in many others, where the production of a new phænomenon is the thing aimed at, they are to be looked upon as benefactors to the hiftory of nature, that perform the fubftantial part of a difcovery, though they do it not by the moft eafy and compendious ways devifeable, or attain not to the utmoft precifenefs that might be wifhed, and is poffible. For fuch performances, notwithftanding their being fhort of perfection, make difcoveries to the world of new and ufeful things; which though others, that are more lucky at contrivances, and have better accommodations, may compafs by more compendious ways, or with greater precifenefs, yet ftill the world is beholden to the firft difcovery for the improvement of it, as we are to *Archimedes* for the firft devifing a way to find, by weighing bodies in water, how much gold or how much filver a mixture of thofe metals does contain, though (if hiftorians have not injured that great man in the relation) he went a more laborious and lefs accurate way to work than modern hydroftatians, who (as I elfewhere fhew) may perform the fame thing by a far better way, which yet, probably, we fhould not have thought of, if that attributed to *Archimedes* had not preceded and afforded us a fundamental notion. And that the not being fo dexterous at contriving the ways to effect a thing, is no fure argument that a man has not a true and

 folid

ſolid knowledge of it, we may eaſily learn from *Euclid*, whom our geometricians generally and juſtly acknowledge to be their maſter, and to have enriched the world with many uſeful truths, and ſolidly demonſtrated all his propoſitions, though divers of his modern commentators have found out more compendious ways for effecting ſeveral of his problems, as well as of demonſtrating divers of his theorems, eſpecially ſince the excellent invention of ſpecious algebra, by whoſe help that accurate mathematician Dr. *Wallis* has, beſides other ſpecimens upon intricate propoſitions, clearly demonſtrated the ten firſt and for the moſt part perplexing theorems of the ſecond element, in little more than as few lines. In ſum, in experiments that are very nice, accurate contrivances and inſtruments are induſtriouſly to be ſought, and highly to be valued; and even in ſuch other experiments, as are frequently to be reiterated, the moſt commodious and eaſy ways of performing them are very deſireable: but thoſe practical compendiums, though very welcome to them that would repeat trials, are not ſo important to the generality of readers, as being but uſeful to ſave pains, not neceſſary to diſcover truths, to which men may oftentimes do good ſervice without any peculiar gift at mechanical contrivances, ſince in moſt caſes they may be looked upon as promoters of natural philoſophy, who deviſe experiments fit to diſcover a new truth, if the attempt ſucceeds, and propoſe ways of bringing it to trial, which, though perhaps not the moſt ſkilful or expeditious, are yet ſufficient and practicable, the increaſe of phyſical knowledge being the product of the things themſelves that are diſcovered, whatever were the inſtruments men employed about making the diſcoveries.

As for the cuts I endeavoured to make their relations and deſcriptions of moſt of the experiments ſo full and plain, as to need as few ſchemes as might be to illuſtrate them: but though I hope that they who either were verſed in ſuch kind of ſtudies, or have any peculiar facility of imagining, would well enough conceive my meaning only by words; yet leſt my own accuſtomance to deviſe ſuch trials, and to ſee theſe made, ſhould make me think them more eaſily intelligible than moſt readers will find them, I adviſed with a learned friend or two, fit to be conſulted on ſuch an occaſion, what experiments were requiſite to be illuſtrated with diagrams, and to ſuch I took care they ſhould be annexed. Only I forbore to add to the figure of each inſtrument alphabetical explications of its parts, as judging that troubleſome work leſs eaſy for me, than it would be for ſuch readers as this tract is deſigned for, to underſtand what is delivered by the help of a little attention in conferring the ſchemes of the inſtruments with the verbal accounts of the experiments they relate to. But there is one particular about the cuts may require both to be given notice of and excuſed; which is, that having occaſion to alter the method of my experiments, when I began to foreſee that I ſhould be obliged to reſerve divers things for another opportunity; and being myſelf abſent from the engraver for a good part of the time he was at work, ſome of the cuts were miſplaced, and not graven in the plates, in which, according to the preſent ſeries of experiments, they might moſt properly have been put.

But perhaps I may (for I am not ſure of it) more need the reader's pardon for (unknowingly) troubling him in this continuation with ſome paſſages, that he may have already met with in the book it refers to; which, though I had not read over for ſome years before, I chanced not to have at hand, when divers of the following papers were written; and though afterwards I recovered it, yet the indiſpoſition of my eyes made me think it unfit rather to tire them by reading over the whole book, than to truſt to the reader's good nature (in caſe I ſhould need it) for the pardon of a few unintended repetitions.

I DOUBT

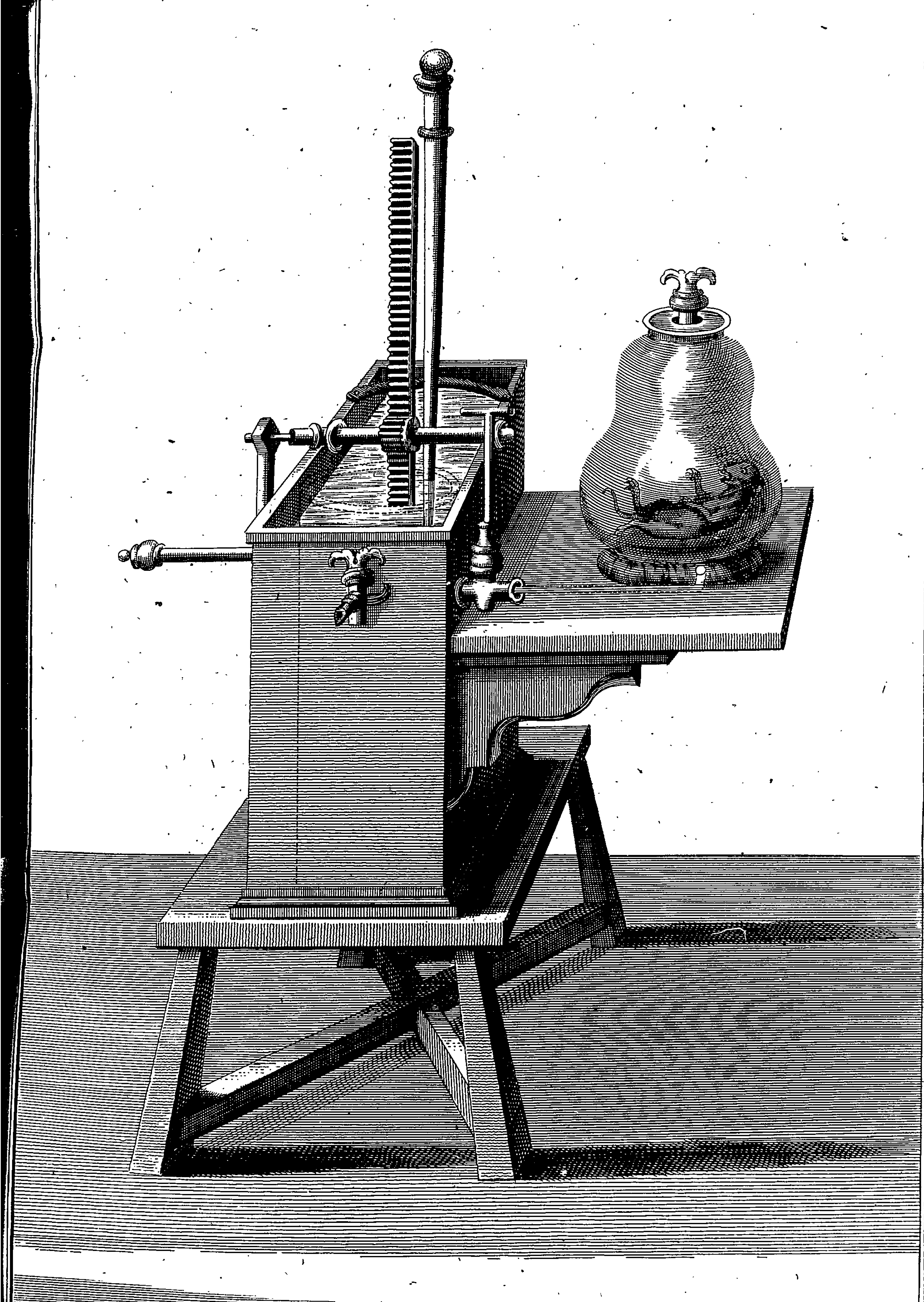
I DOUBT not many readers will be inquisitive to know why this treatise is stiled the first part of a Continuation. To give these some account of the title, I must put them in mind, that in the already published experiments I intimated that two sorts of trials might be made by the help of our engine: the one, such as needed but a short absence of the air, and the other such as required that the air should not only be withdrawn for a while, but kept out for a considerable time, from the bodies whereupon the trial is made. Of the former sort of experiments are these this present book does (as well as that heretofore published did) consist of. And though I have been so much called upon, and troubled for certain writings, whereof I have made such mention in those that passed the press, as some readers interpreted to be an engagement, that it made me think fit, when I satisfied their demands, to be thenceforward very shy of making the publick any promise; yet I was induced not to alter the title of this treatise, partly because it may intimate to the curious, that there are yet a great many things to be performed by our engine, besides the productions of it I have hitherto presented them; and partly because though I still persist in my former averseness to make promises to the world; yet it is very possible, that if God grant me life and health, I may, in due time, present my friends with what may serve for a second part of our Continuation, consisting of experiments that require a longer absence of the air from the bodies to be wrought upon: and I shall think if this first part prove not unacceptable to the curious, that the latter will be not unwelcome to them, as being designed to consist of sets of experiments, which by their being most of them new, and some of them odd enough, may perchance afford some not despicable hints to the speculative. But the very nature of these experiments requiring that some of them should be long in making, my friends could not reasonably expect a quick dispatch of work of this kind, though I should not meet for the future with such intervening impediments, as have hitherto disturbed it (as want of instruments, of health, of leisure, and of the liberty, which is so requisite in this case, of staying long enough in one place): notwithstanding all which difficulties I have by snatches been able, through God's blessing, to make forty or fifty of designed trials, being such as require the least of time to be performed in, though I now think not fit to mention any of them, as well for other reasons, as because though they be made by the help of our engine, yet they require a peculiar apparatus of instruments, very differing from those we have hitherto mentioned, and not to be intelligibly described without many words and divers figures. In the mean time, lest the industrious should be discouraged by a surmise, that there is nothing left for them to do by the help of our engine, at least as to the first sort of experiments, I shall inform them, that I had thoughts to have added divers others of that kind to these that now come forth, and particularly two clusters of pneumatical trials, the one about respiration, and the other about fire and flames; but several of my notes and observations being at present out of the way, my having neither health nor leisure to repair these inconveniencies, and prosecute trials of that sort with any assiduity, makes me chuse rather to reserve them for an appendix, than to make those that now come abroad stay for them; which will not, I presume, be the more disliked, because by taking this course I may, in delivering of the phænomena of nature, imitate nature herself, of whom it is the Roman philosopher's saying, *rerum natura sacra sua non simul tradit.* Seneca quæst nat. lib. 7. c. 31.

Some ADVERTISEMENTS *touching the* ENGINE *itself.*

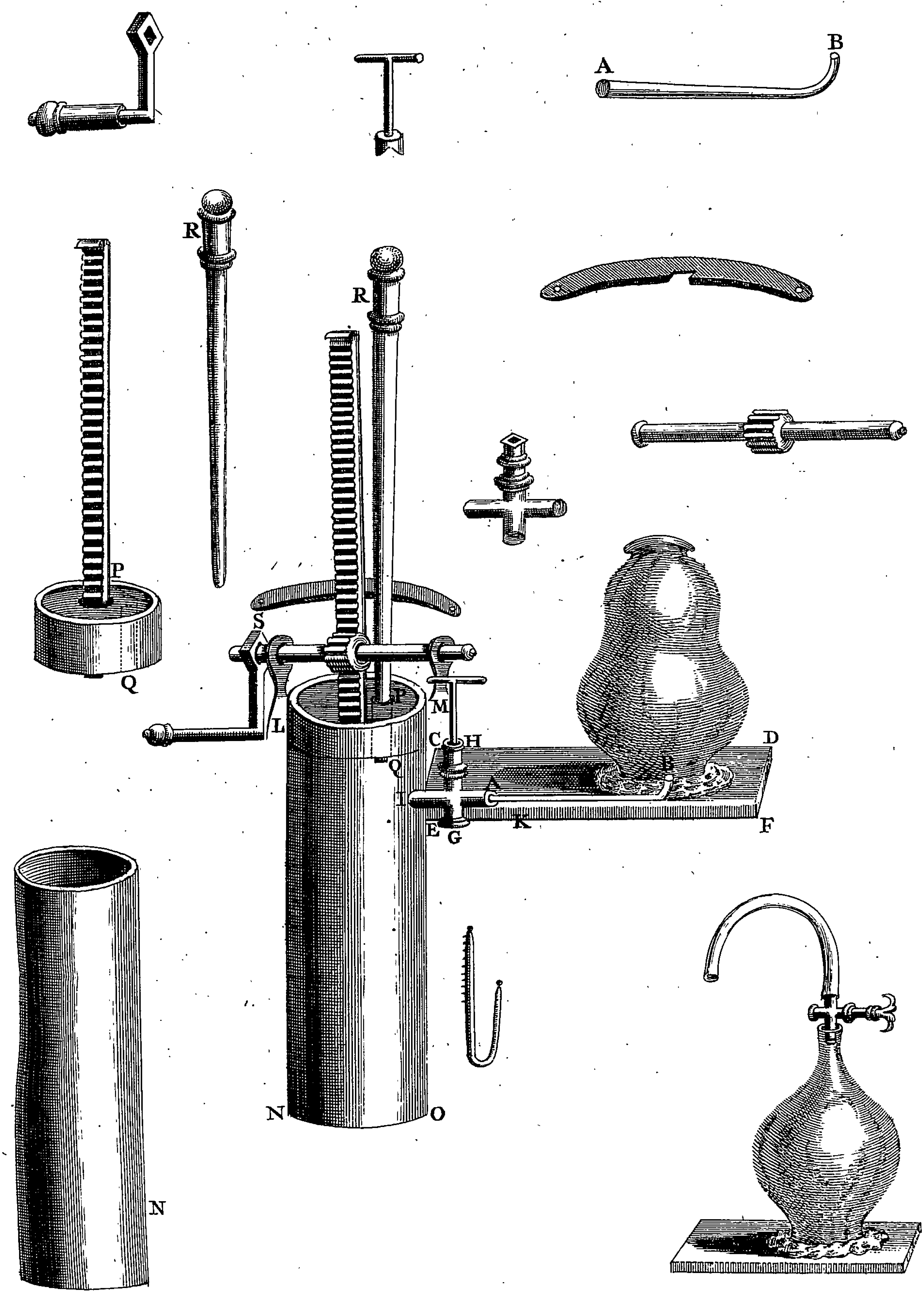
THOUGH the engine already publiſhed, and that which I employed in the following trials, have the ſame uſes, and agree both in the ground and the main part of their conſtruction, yet they differ in ſome particulars fit to be taken notice of: for after I had preſented the great engine I formerly made uſe of to the Royal Society, partly the difficulty of procuring ſuch another of that ſize and make, and partly the deſire of making ſome improvements, invited me to make ſome alterations in the ſtructure; ſome of them ſuggeſted by others (eſpecially by the ingenious Mr. *Hook*) and ſome of them that I added myſelf, as finding that without them I could not do my work. Wherefore it will not be amiſs to point at the chief differences between the former and the latter engine, and to intimate ſome of the conveniences and inconveniences that attend them.

As for the conſtruction of the ſecond engine itſelf, ſince it is preſumed that the readers of this book have already peruſed that of which this is a Continuation, and underſtood the contrivance of the inſtrument that belongs to it, it was preſumed ſufficient to exhibit in the firſt plate the delineation of the entire engine ready to be ſet at work; and in the ſecond, the figure of the ſeveral metalline parts, that compoſe it, before they are ſet together. For though theſe have not verbal and alphabetical explications annexed to them, yet the ſight of them may ſuffice to make thoſe that have an imagination fitted to conceive mechanical contrivances, and are acquainted with the former engine, comprehend the ſtructure of this; which alphabetical explications would ſcarce make ſuch readers do, as are not ſo qualified: only two things there are, which being of ſome difficulty, as well as of importance to be conceived, I ſhall here particularly take notice of. The firſt of them is, that in regard the ſucker is to be always under water, and the perforation *p q*, that paſſes perpendicularly quite through it, and ſerves together with the ſtick *r s* for a valve, is to be ſtopt at the bottom of the cylinder, as at *n o*, when it is full of water, it was requiſite to make the ſtick *r p* of a conſiderable length, as two or three feet. The other and chief thing is, that in the ſecond plate, the pipe *A B*, whoſe end *B* bends upward, is made to lie in a groove or gutter purpoſely made in the flat wooden board *c d e f*, on which the receivers are to reſt; which ſquare board I cauſed to be overlaid with very good cement, on which I took care to apply a ſtrong plate of iron, of the bigneſs and ſhape of the board, leaving only a ſmall hole for the erected part of the pipe to come out at, which I added, not only to keep the wooden board the better from warping, but becauſe I knew (what will perhaps be thought ſtrange) that the preſſure of the atmoſphere on one ſide of the board, when there is no preſſure, or but very little, on the other, will enable many aerial particles to ſtrain through the very wood, though of a good thickneſs, and imbued with oil to choak the pores. To this iron plate we ſometimes fit a lip turning up about it, to hinder the water that on ſome occaſions will come from the receiver from falling on the room; and (to add that upon the by) though the ſtop-cock *g h i k*, that belongs to the hitherto mentioned pipe, may be inſerted at *I*, into the barrel or cylinder *l m n o*, by the help of ſoder, yet we choſe as a much better way to have the branch *I*, of the

ſtop-



J. Mynde sc.



J. Mynde sc.

ſtop-cock, made like a ſcrew, which being once firmly ſcrewed into the barrel, is not apt to be broken off, and may be more eaſily mended, if any thing happen to be out of order, which the engine is the moſt liable to be in or about the pipe; partly becauſe it may fall out (though but very rarely, if due care be but taken) that the air will inſinuate itſelf between the wooden board and the iron-plate, and ſo get up (where the pipe bends upwards) into the cavity of the receiver; and partly becauſe the pipe being for a juſt reaſon made but ſlender, and the part of it that looks upwards very ſhort, it happens not very unfrequently, that when we imploy receivers with narrow orifices, where the cement muſt lie cloſe to the opening of the pipe, it happens, I ſay, that the cement, eſpecially if it be much ſoftened by heat, is ſucked (as they ſpeak) into the pipe, and ſo choaks it up; or elſe that ſome part of the body included in the receiver is drawn to the orifice of the pipe, and lying upon it as a cover hinders the free paſſage of the air into the barrel; againſt which inconvenience, to add that upon the by, we uſe amongſt other expedients to place juſt about the orifice of the pipe a ſmall cover of tin, like that of a little box, which covers it at the top, to hinder any thing from lying immediately upon the pipe, and has a ſmall opening or two in the ſide, to give the air of the receiver free acceſs to the pipe.

THE ſquare and hollow wooden part of this engine, diſcernible in the firſt plate, is ſo made, that it may contain not only the cylinder, but ſo much water as will always keep the cylinder quite covered with that liquor; by which means the ſucker, lying and playing always under water, is kept ſtill turgid and plump, and the water being ready at hand to fill up any little interval or chink that may happen to be between the ſucker and the inſide of the barrel, together with the newly mentioned plumpneſs of the ſucker, very much conduce to the exact keeping out of the air. But this advantage is not without ſome inconvenience; for divers times, if great care be not taken in turning the ſtop-cock, the water will be impelled into the receiver, and much prejudice ſundry experiments, when the included bodies are ſuch that may be ſpoiled or impaired (at leaſt for the preſent) by that liquor. The ſmallneſs of our cylinder is a convenience in regard of the facility it affords to make and diſpatch thoſe many experiments that may be performed in ſmall receivers, though it make thoſe more troubleſome and tedious that require the exhauſtion of large and capacious ones.

THE flat plate (mentioned a little above) has this great conveniency in many experiments, that the receiver needs no ſtop cock of its own; for ſuch a veſſel being made all of an entire piece of glaſs, and whelmed on upon the plate well covered with cement, can better keep out the air, than if there were a ſtop-cock, at which the air does but too frequently get in: but beſides that in divers experiments ſuch receivers do uſually require to be wide mouthed, whereby a greater compaſs is to be fenced againſt the ingreſs of the air, ſeveral experiments cannot ſo conveniently be tried in this ſort of receiver.

BUT becauſe that though this ſecond form of our engine hath as to ſeveral purpoſes its peculiar conveniences and advantages, yet ſome virtuoſi may be furniſhed with the other already, and ſome may conceive it the more clearly of the two, or may judge it preferable for their particular deſigns; I ſhall here intimate, that for moſt of the experiments, if not all, that follow in this treatiſe, they may make uſe of, or at leaſt make a ſhift with the firſt engine, with a few alterations; whereof the chief is to be this, that to the upper part of the great cylinder, on the ſide oppoſite to the iron rack, there is to be faſtened ſuch a ſquare board, and ſuitable iron-plate, as is uſed in the ſecond engine, betwixt which board and plate is to be lodged ſuch a pipe as was lately deſcribed;

deſcribed, between either a continuation of the outward branch of the ſtop-cock, or elſe firmly faſtened to it by ſodering or ſcrewing; for by this means when the ſucker is depreſſed, the air will through the cavity of this pipe, and the ſtop-cock whereto it is annexed, paſs freely by virtue of its ſpring, out of the receiver into the exhauſted cylinder; though this and the ſucker that moves in it, being not kept, as in the ſecond form of the engine, under water, the greater care will be needed to keep the air from inſinuating itſelf between them. A good cement to faſten the receivers to the often mentioned plate of iron, is a thing of no ſmall moment in making the following experiments, of which we imploy differing compoſitions for differing purpoſes, ſome of which are not neceſſary to be mentioned in that part of this work that now comes forth; but that, which in almoſt all the following trials we chiefly make uſe of, is a well wrought mixture of yellow bees-wax and turpentine, which compoſition, as it ſerves better than moſt others to keep out the air, ſo it has the conveniency, which is no ſmall one, of ſeldom needing to be heated, and ſeldomer to be much ſo; eſpecially if we imploy a little more turpentine in winter than in ſummer, in the former of which ſeaſons, as much, or very near as much of that ingredient as of the wax does well, whereas in ſummer a mixture of three parts of wax to about two of turpentine is more proper.

A CON-

A CONTINUATION OF

NEW EXPERIMENTS

PHYSICO-MECHANICAL,

TOUCHING THE

SPRING AND WEIGHT OF THE AIR, AND THEIR EFFECTS.

MY DEAR LORD,

SINCE I have already in proper places of the phyſico-mechanical experiments about the air, which I formerly preſented your lordſhip, given you a ſufficient account of ſeveral things touching the ſcope, occaſion, &c. of my attempt; it will not be neceſſary to make a ſolemn preface to the enſuing experiments. And therefore preſuming upon an acceptance which the favourable entertainment which your lordſhip, as well as the publick, was pleaſed to give my firſt trials of this kind, encourages me to expect I ſhall, without troubling you with any further preface, immediately fall upon a continuation; eſpecially ſince your lordſhip will perhaps wonder that you have not received it much ſooner, as indeed you ſhould have done if I had been befriended with accommodations and leiſure.

EXPERIMENT I.

About the raiſing of mercury to a great height in an open tube, by the ſpring of a little included air.

DIVERS ways have been propoſed to ſhew both the preſſure of the air, as the atmoſphere is a heavy body, and that the air, eſpecially when compreſſed by outward force, has a ſpring that enables it to ſuſtain or reſiſt a preſſure equal to that of as much of the atmoſphere as can come to bear againſt it; and alſo to ſhew that ſuch air as we live in, and is not condenſed by any human or adventitious force, has not only a reſiſting, refifting fpring, but an active fpring (if I may fo fpeak) in fome meafure, as when it diftends a flaccid or breaks a full-blown bladder in our exhaufted receiver.

BUT obferving that there feems to want a vifible experiment to convince thofe that are not fo eafily fatisfied with reafons, though drawn by juft confequence from phyfical or mechanical truths, or even from other experiments; taking notice, I fay, hereof, I made the following experiments; not fo much to prevent or remove a fcruple no better grounded, as to have a new way of making an eftimate by fome known and determinate meafure of the force of the bare fpring of the air, both in its natural ftate (as it is faid to be, when not compreffed nor rarefied more than the free air we breathe) and according to its feveral degrees of expanfion.

WE took then a phial with a neck not very large; and having filled about a fourth part of it with quick-filver, we fo erected and faftened a long and flender pipe of glafs, opened at both ends in the neck of the phial, with hard fealing-wax, that the lower end reached almoft to the bottom of the quick-filver, and the upper more than a yard above the phial; then having blown in a little air, to try whether the inftrument did not leak, (which it is very difficult to keep fuch inftruments from doing) we conveyed it into a long and flender receiver fit for fuch an ufe; and having withdrawn the air as well as we could, we found, according to our expectation, that the fpring of the air, included in the phial, impelled up the quick-filver into the erected pipe, to the height of 27 inches; and having fuffered the external air to return into the receiver, the quick filver fubfided in the tube, fometimes almoft, and fometimes quite as low as the ftagnant quick-filver in the phial.

See Plate III. Fig. 1.

FOR the better illuftration of this experiment, thus fummarily related, but with the like fuccefs, as to the main, feveral times repeated, we will fubjoin the following obfervations and notes:

I. THAT we tried this experiment feveral times, and the laft time in the prefence of the famous Savilian geometer, Dr. *Wallis*, who faw the quick-filver in the pipe impelled up to 27 inches, being one himfelf of the meafurers. And though at other times we found it to be much about the fame height with the laft, yet once it feemed plainly to be a pretty deal higher; which yet we fpecified not, becaufe a mifchance took off the mark which we had made to meafure the height by.

II. HAVING once, to try the ftanchnefs of the phial, blown in fo much air (without taking out any thing as we ufe to do in the like cafe) that the air in the cavity of the phial raifed and kept the quick-filver three inches high in the pipe, when we went on with the reft of the experiment, according to the way above defcribed, we found, by emptying the receiver of air, that we were able to raife the quick filver in the cane 30 inches, or fomewhat above that in the phial.

III. SOMETIMES it may happen that the mercury, when taken very foon out of the receiver, will not appear to have fubfided to its firft lownefs, which perhaps it will not fink to in fome while after; which is not to be wondered at, fince in fuch a receiver, which contains but little air, the heat of the cement and the iron imployed to melt it quite round the receiver, may impart a little warmth to the air in the phial, which will after return to its former temper. But this accident is neither conftant nor neceffary to the experiment.

IV. IT is very remarkable, that if the receiver be fitly ftopped and flender enough, upon the turning of the ftop-cock, to let out the air at the firft exfuction, the mercury will be impelled up by the fpring of the air in the phial, fuddenly flying abroad or ftretching itfelf, fo that it will be raifed feveral inches above the height it will reft at afterwards, and will make feveral vibrations up and down before it come to fettle, juft as the resisting spring, but an active spring (if I may so speak) in some measure, as when it distends a flaccid or breaks a full-blown bladder in our exhausted receiver.

But observing that there seems to want a visible experiment to convince those that are not so easily satisfied with reasons, though drawn by just consequence from physical or mechanical truths, or even from other experiments; taking notice, I say, hereof, I made the following experiments; not so much to prevent or remove a scruple no better grounded, as to have a new way of making an estimate by some known and determinate measure of the force of the bare spring of the air, both in its natural state (as it is said to be, when not compressed nor rarefied more than the free air we breathe) and according to its several degrees of expansion.

We took then a phial with a neck not very large; and having filled about a fourth part of it with quick-silver, we so erected and fastened a long and slender pipe of glass, opened at both ends in the neck of the phial, with hard sealing-wax, that the lower end reached almost to the bottom of the quick-silver, and the upper more than a yard above the phial; then having blown in a little air, to try whether the instrument did not leak, (which it is very difficult to keep such instruments from doing) we conveyed it into a long and slender receiver fit for such an use; and having withdrawn the air as well as we could, we found, according to our expectation, that the spring of the air, included in the phial, impelled up the quick-silver into the erected pipe, to the height of 27 inches; and having suffered the external air to return into the receiver, the quick silver subsided in the tube, sometimes almost, and sometimes quite as low as the stagnant quick-silver in the phial.

See Plate III. Fig. 1.

For the better illustration of this experiment, thus summarily related, but with the like success, as to the main, several times repeated, we will subjoin the following observations and notes:

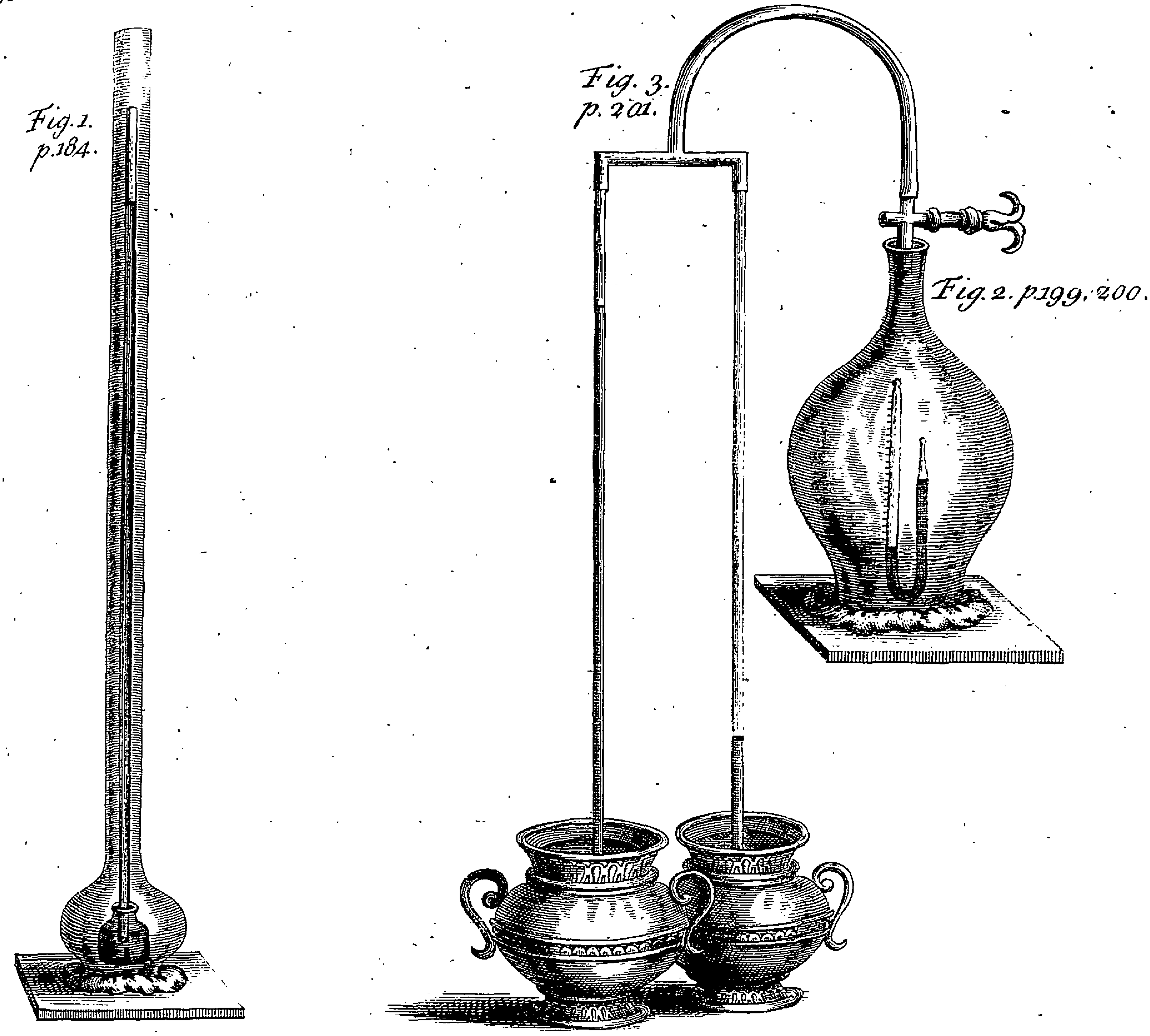
I. That we tried this experiment several times, and the last time in the presence of the famous Savilian geometer, Dr. *Wallis*, who saw the quick-silver in the pipe impelled up to 27 inches, being one himself of the measurers. And though at other times we found it to be much about the same height with the last, yet once it seemed plainly to be a pretty deal higher; which yet we specified not, because a mischance took off the mark which we had made to measure the height by.

II. Having once, to try the stanchness of the phial, blown in so much air (without taking out any thing as we use to do in the like case) that the air in the cavity of the phial raised and kept the quick-silver three inches high in the pipe, when we went on with the rest of the experiment, according to the way above described, we found, by emptying the receiver of air, that we were able to raise the quick silver in the cane 30 inches, or somewhat above that in the phial.

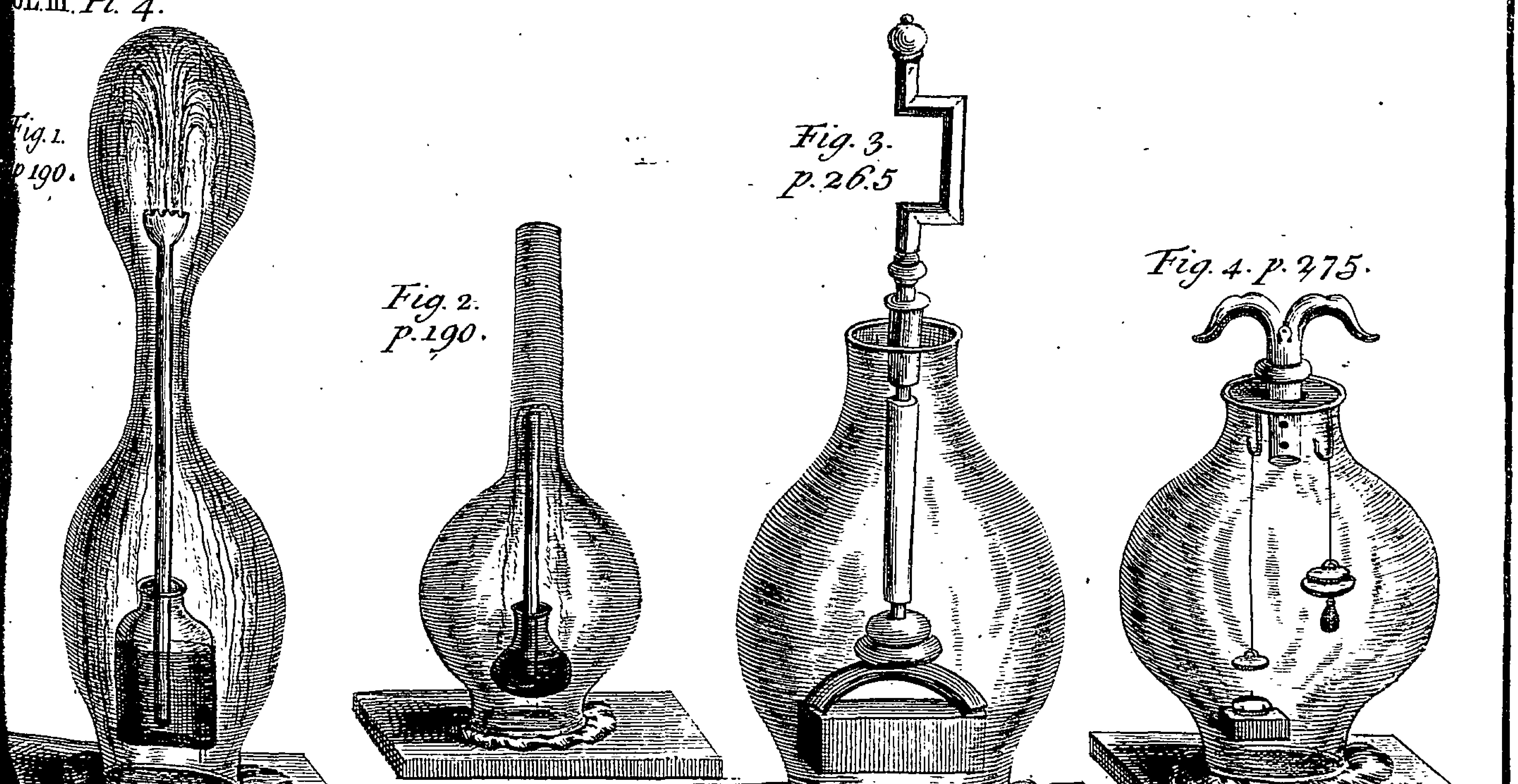
III. Sometimes it may happen that the mercury, when taken very soon out of the receiver, will not appear to have subsided to its first lowness, which perhaps it will not sink to in some while after; which is not to be wondered at, since in such a receiver, which contains but little air, the heat of the cement and the iron imployed to melt it quite round the receiver, may impart a little warmth to the air in the phial, which will after return to its former temper. But this accident is neither constant nor necessary to the experiment.

IV. It is very remarkable, that if the receiver be fitly stopped and slender enough, upon the turning of the stop-cock, to let out the air at the first exsuction, the mercury will be impelled up by the spring of the air in the phial, suddenly flying abroad or stretching itself, so that it will be raised several inches above the height it will rest at afterwards, and will make several vibrations up and down before it come to settle, just as the

Vol. III. Pl. 3.



Vol. III. Pl. 4.



the mercury does in the Torricellian experiment (the bare preſſure of the little air doing here to the mercury what the weight of the atmoſphere does there); and ſuch motions of the mercury will be made four or five ſubſequent exſuctions, upon the withdrawing of the air in the receiver. But as theſe grow leſſer and leſſer as the ſpring of the included air grows fainter, ſo none of them is any thing near ſo conſiderable as the vibrations made upon the firſt ſuck.

V. Agreeable hereunto we obſerve that at the firſt exſuction, when the ſpring of the included air was yet ſtrong, the mercury would be raiſed by our eſtimate above half, if not $\frac{2}{3}$ of the whole height, whereto it will at length be brought (though that muſt be according to the bigneſs of the receiver, and other circumſtances) and the ſubſequent exſuctions do ſtill add leſs and leſs proportions of height to the mercurial cylinder, and that for two reaſons; the one, becauſe the more there is of mercury impelled into the tube, the greater weight of mercury preſſes upon the included air; and the other, becauſe the air has ſo much the more room in the phial to expand itſelf, whereby its ſpring muſt be proportionably weakened.

Lastly, when we made moſt of theſe trials, I had the curioſity to obſerve the height of the mercury in a good barometer, and thereby found that the air was then but light; its greateſt height reaching but to 29 inches and $\frac{1}{8}$, and its height ſoon after the trial, whereof Dr. *Wallis* was a witneſs, amounting but to 29 inches.

To make an eſtimate of the quantity of air that had raiſed the quickſilver to 27 inches, we took the phial that was employed about this experiment, and having counterpoiſed it, whilſt it was empty, we afterwards filled it with water, and found the liquor to weigh 5 ounces, 2 drachms, and about 20 grains; and then having poured out the water till it was ſunk to a mark which we had made on the outſide of the glaſs, to take notice how high the quickſilver reached that we poured in; and laſtly, weighing the remaining water equal in bulk to the quickſilver, we found it to amount to 1 ounce, 2 drachms, 14 grains; ſo that the air that had raiſed up the mercury, poſſeſſed (before its expanſion) in the phial the place but of 4 ounces, and a few odd grains, i. e. of about $\frac{1}{4}$ of a pint of water. And as for the pipe alſo, employed about the ſame experiment, we found its cavity to have about $\frac{1}{8}$ part of an inch in diameter.

It was one of the uſes I hoped to make of this experiment, that by comparing the ſeveral degrees of expanſion of air included in the phial with the reſpective and increaſing heights of the mercury that was impelled up into the pipe, ſome eſtimate might be made of the force of the ſpring of the air weakened by ſeveral degrees of dilatation; but for want of conveniencies I forbore to venture upon ſuch nice obſervations, eſpecially becauſe the preſſure of the dilated air that remains in the receiver, and is external to the air included in the phial, muſt alſo be taken into conſideration.

Another uſe of our experiment may be this; that it may ſupply us with a conſiderable argument againſt ſome learned men, who attribute the ſuſpenſion of the quickſilver in the Torricellian experiment to a certain rarefied matter which ſome call a funiculus, and whereto others give other names; which rarefied ſubſtance they ſuppoſe to draw up and ſuſtain the quickſilver, in compliance of nature's abhorrency of a vacuum. For in the experiment under conſideration, the quickſilver, being not only ſuſtained at the height of 27 inches in the tube, but elevated thither; if the cauſe of this be demanded, it will be anſwered, according to their hypotheſis, that the air in the receiver, external to that of the phial, being, by reaſon of the ſucking out of ſome of it by the pump, more rarefied than that in the phial, it draws up to it the quickſilver in the cane; and the more it is rarefied the higher it is enabled to draw it. But then I demand, whence it comes to paſs, that though we can, by perſevering to pump, more and more rarefy the little remaining air, or the aëreal ſubſtance in the receiver, that in the phial not appearing to be alſo rarefied, yet the air in the receiver does not, by virtue of its ſuperadded rarefaction, whereby it exceeds that of the air in the phial, pull up the quickſilver to a greater height in the tube than 27 inches; for, that this is not the greateſt height to which mercury may be raiſed by this rarefied ſubſtance, our adverſaries muſt not deny, who tell us, that in the Torricellian experiment it ſuſtains a mercurial cylinder of 29 inches and $\frac{1}{2}$, and can raiſe a cylinder of 29 inches to $29\frac{1}{2}$, or higher, in caſe that the cylinder be made to vibrate up and down in the tube.

See the latter part of the following Experiment.

And as for thoſe that will in ſuch caſes, as our experiment ſuggeſts, have recourſe only to that which they call the *fuga vacui*, they may pleaſe alſo to conſider, that ſince the quickſilver remains the ſame, its aſcenſion in the tube will not be available for what they think to be nature's purpoſe; for, whether it reach higher or lower in the tube, it will adæquately fill no more ſpace in one poſture, or in one figure, than in another, in what part ſoever of the cavity of the receiver it be placed.

EXPERIMENT II.

Shewing that much included air raiſed mercury in an open tube no higher than the weight of the atmoſphere may in a baroſcope.

In the former experiment, by reaſon of the ſmallneſs of the phial that was employed about it, there was ſo little air included, that the expanſion of it, ſo far as was requiſite to impel up the mercury in the pipe to the above-mentioned height of 27 inches, may be probably ſuſpected to have very much weakened its ſpring, and therefore it may be thought that (eſpecially conſidering the great force that ſeveral of our experiments manifeſt impriſoned air to have) if there were a greater quantity of air included in the veſſel; ſo that the expanſion, ſufficient to raiſe the mercury to the former height, would not need to be conſiderable (becauſe that the capacity of the tube being but the ſame, the whole included air will be ſo much the leſs expanded, by how much the more of it there is); it ſeemed probable that the ſpring of the air, being but a little weakened by ſo ſmall a dilatation, would remain ſtrong enough to raiſe a much taller cylinder of mercury in the tube, and perhaps make the liquor run over into the receiver.

But though this ſuggeſtion ſeem probable enough, yet when I conſidered that the weight of the atmoſphere is able to ſuſtain a cylinder of quickſilver but of 30 inches or thereabouts (in perpendicular height) and conſequently that the preſſure of ſuch a mercurial cylinder is equivalent to that of an atmoſpherical cylinder of the ſame bore; it was not difficult to conclude, that ſince the air in a phial, before the mouth is cloſed, has a ſpring but equal in ſtrength to the weight of the atmoſpherical pillar that leans upon it (for if the ſpring were too ſtrong for the weight that leans on it, ſome of the air would get out of the phial) a greater phial, and conſequently a greater quantity of included air would not be able by its ſpring to elevate and ſuſtain a longer cylinder of mercury, than the weight of the atmoſphere is able to do; nor indeed altogether ſo much, becauſe of ſome little (though but little) diminution of the ſpring by ſome (though but a ſmall) expanſion that the included air ſuffers by ſucceeding in the place of mercury that is impelled up.

To clear, therefore, this matter by an experiment, we took a ſtrong glaſs-bottle, capable of holding about a quart of liquor, and having put into it a convenient quantity of quickſilver, we erected in it a very long and ſlender pipe of glaſs open at both the ends,

ends, and reaching at the lower end beneath the ſurface of the ſtagnant mercury; and having faſtened this pipe in the neck of the bottle, by choaking up that neck very accurately with good cement, that none of the included air might be able to get out, we conveyed the whole into a receiver, like that employed about the firſt experiment in ſhape, but much larger, that it might be able to contain ſo great a veſſel; and then the engine being ſet at work, we quickly raiſed the quickſilver to a greater height than formerly; and when we ſaw it come to a ſtand, we did by the help of ſome marks, made before-hand on the pipe, and by the help of a very long and well divided ruler, meaſure, with as much care and accurateneſs as the figure of the veſſels would allow us to do, the height of the mercurial cylinder, which we found to be 29 inches and about $\frac{7}{8}$, to which abating half an inch, which was raiſed before the pump was employed, by ſome air that had been blowed into the bottle, to try whether it were ſtanch; deducting, I ſay, this half inch of quickſilver which remained in the tube after the external air was let in (as well as it had been there before the receiver was exhauſted) out of the newly mentioned number there remained 29 inches, and near $\frac{3}{8}$, for the height of the mercury, raiſed by the ſpring of the air ſhut up in the bottle; and then conſulting with the above-mentioned baroſcope which ſtood in a window in another part of the houſe, I found that the weight of the atmoſphere did bear a mercurial cylinder of about 29 inches and $\frac{1}{2}$, which was higher by $\frac{1}{8}$ than that to which the ſpring had raiſed the quickſilver in the exhauſted receiver; and the difference perhaps would have been greater, if the place where the experiment was made, had not by its warmth added ſome little matter to the ſpring of the air; and if alſo we could have kept the mercury ſo long elevated as to give it leave to diſcharge itſelf of thoſe ſmall bubbles, which it is almoſt impoſſible, in ſuch experiments as this, to free quickſilver from, without ſome help from time.

LASTLY, though we cauſed the pump to be plied, to try whether we could not, by the more diligent exſuction of the receiver, raiſe the quickſilver above the height of that which the atmoſphere kept ſuſtained in the baroſcope; yet our labour gave us a confirmation that the ſpring of the air would not raiſe the mercury higher than did the weight of the atmoſphere; which may not a little confirm the ſecond obſervation.

N. B. THIS was not the only nor the firſt experiment we made of this kind; but this being carried on without miſchances (with which divers others were attended) and made with much care, I thought fit to ſet down this inſtead of all, intimating generally about the reſt, that they ſeemed to agree well for the main with that which is here recited. Only there is one thing relating to thoſe other experiments that ſeems not altogether unworthy to be taken notice of; which is, that when our trials were made in veſſels that contained a conſiderable quantity of air, though upon the exhauſtion of the receiver the ſpring of the included air could not raiſe the quickſilver to the top of the pipe, yet ſometimes by other effects it manifeſted itſelf to be very ſtrong, as once or twice by the blowing out or breaking the cork or cement and other matter that was employed to ſtop the glaſs it was ſhut in; and once by an accident too memorable to be here paſſed over in ſilence.

I HAD one day invited Dr. *Wallis* to ſee ſuch an experiment as I have been relating made with (not a phial, but) a bottle of green glaſs (ſuch as we uſe now for wine) and four or five pounds of mercury. After this learned perſon and I had continued ſpectators as long as we thought fit, we withdrew into another room, where we had not ſat long by the fire before we were ſurprized by a ſudden noiſe, which the perſon that occaſioned it preſently came running in to give us an account of, by which it appeared that this ingenious young man (whom I often employ about pneumatical experiments, and whom I mentioned to your Lordſhip, becauſe *J. M.* has the honour to be ſomewhat known to you) being desirous in our absence to satisfy the curiosity he had to know whether the quicksilver could not be raised higher in the pipe than I had foretold, plyed the pump so obstinately, that at length the bottle being not, it seems, every where equally strong, the imprisoned air found it more difficult to make the quicksilver run over at the top of the pipe, than to break the bottle in the weakest place; and accordingly did not only throw off a piece of the bottle, but threw it with such violence against the large and strong receiver, as broke that also, and rendered it unserviceable for the future. But the doctor and I laying together the pipe, which happened to be broken into but few pieces, concluded by the place, to which we were told it reached when this accident happened, that it had not exceeded nor indeed fully equalled the height to which the weight of the atmosphere might have raised it.

EXPERIMENT III.

Shewing that the spring of the included air will raise mercury to almost equal heights in very unequal tubes.

Having shewn in the two former experiments that the active strength of the air's spring is very considerable, I thought good also to examine whether or no to the other resemblances in operation between the weight of the free air and the pressure of the included air, this also may be added, that as the gravitation of the atmosphere is able (as we shall hereafter prove) to sustain the mercury at the same height in lesser and greater tubes sealed at the top; so the pressure of the included air may be able to sustain the mercury at the same height in slender and in larger tubes, though in the latter it must sustain a far greater weight of mercury than in the former; provided allowance be made for the weakening, which the spring of the included air must be subject to, by reason that, to succeed in the place of a large cylinder of mercury impelled up into the greater tube, it must expand itself more, and consequently have its spring more weakened than if the tube were slender.

To prosecute this experiment, I thought on a peculiar shape of vessels, which, if I had been where there is a glass-house, I would have caused to be blown for the more convenient trying of two pipes of different bores at the same time: but though I wanted this accommodation, I thought it might well enough shew what I intended by employing successively two tubes of very differing sizes, provided the vessel for the including of the air were the same.

Wherefore, taking the glass bottle made use of to try the former experiment, and erecting in it, after the manner above described, a cylindrical pipe of glass a good deal larger than the former (if not as large again) we prosecuted the experiment as we had made it, with the slender tube above-mentioned, and found that we were able, by the spring of the air in the bottle, to raise the quicksilver to a considerable height, which, measuring as well as the vessel would allow us, was, by the least estimate that was made of it (which was mine) 28 inches and $\frac{1}{8}$, by which it appeared to want somewhat above an inch of the height of the mercurial cylinder, which the weight of the atmosphere could have sustained, as appeared by the barometer, wherein the quicksilver at that time was about 29 inches and $\frac{1}{4}$ high; which difference was no more than I expected, considering, that, whereas the weight of the atmosphere is still the same, when the mercury is at its full height (and that whether the pipe be great or small) in a sealed tube; the spring of our included air must needs be weakened the larger the tube is, and the higher

higher the liquid metal is impelled in it; ſo that it ſeemed a conſiderable phænomenon that the ſpring of ſo little air ſhould be able to raiſe the mercury as high within an inch or thereabouts in a wider as in a ſlenderer tube, ſince the diameter of the cavity of the former being, by our eſtimate, double to that of the latter (into which the ſlender pipe could eaſily be put as into a caſe too big for it); the greater mercurial cylinder may be ſuppoſed to have weighed near four times as much as the leſſer; I ſay, near, becauſe there was an inch difference in their heights, but in caſe theſe had been equal, then the ſolidities of the cylinders would have been to one another as their baſes; and ſince theſe, being circular, are in duplicate proportion to their diameters, that is, as the ſquares of their diameters; it is plain that if the diameters be as one to two, the ſquares of them muſt be as one to four; and theſe cylinders conſiſting of the ſame mercury, their weights will have the ſame proportions with their ſolidities, and conſequently would be as one to four, making the abatement formerly intimated for the inch and a little more of mercury, by which the larger cylinder came ſhort of the height of the former.

N. B. 1. This and the two former experiments tried by us with quickſilver, may be alſo tried with water; but beſides that we could hardly procure tubes long enough for ſuch trials, we were not very ſolicitous about it: for if we attentively enough conſider what has been already delivered, and the proportion in ſpecifick gravity betwixt water and quickſilver (whereof the latter is near 14 times as heavy, bulk for bulk, as the former) it will not be difficult to foreſee the event of ſuch experiments, which he that has a mind to make ſhould be furniſhed not only with long tubes, but with capacious veſſels to ſhut up the air in; elſe the air will be ſo far expanded before the water has attained near the height to which the weight of the atmoſphere may raiſe it, that the experiments will not ſeem to ſucceed near ſo well with water as ours did with quickſilver.

2. We thought it worth trying, whether, when the included air had raiſed the great cylinder of mercury to the utmoſt height it could elevate it to by the ſpring it then had, it would not be brought to raiſe the quickſilver yet higher, if, notwithſtanding the expanſion it had already, there were an agitation made by the heated corpuſcles of the ſame air. And, in purſuance of this curioſity, having cauſed an hot iron and a ſhovel of kindled coals to be held near the oppoſite parts of the receiver, we perceived after a while that the mercury aſcended $\frac{1}{8}$ of an inch or better above the greateſt height it had reached before: but conjecturing that it would have riſen higher, were it not, that whilſt the application of the hot bodies was making, ſome particles of air had unperceivably ſtolen into the receiver, I cauſed the pump to be plied again to withdraw the air I ſuſpected to have got in, by which means the mercury was quickly raiſed $\frac{5}{8}$ of an inch, or better, by virtue of this adventitious ſpring (if I may ſo call it) which the included air acquired by heat, and I made no doubt that it might have been raiſed much higher; but I was unwilling, by applying a leſs moderate heat, to hazard the breaking of my glaſſes, in the place I then was in, where ſuch a miſchance could ſcarce have been repaired.

EXPE-

EXPERIMENT IV.

About a new hydraulo-pneumatical fountain, made by the spring of uncompressed air.

I SHALL now add such an application of the principle whereon the former experiment was grounded, as I should scarce think worth mentioning in this place, were it not, that besides that divers virtuosi seem not a little delighted with it, it may, for aught I know, prove to be of some philosophical use (to be pointed at hereafter).

WE took a glass bottle with a convenient quantity of water in it, and fitted this bottle with a slender glass-pipe open at both ends, and about three foot long, which was so placed that the lower orifice was a good way beneath the surface of the water, and the pipe itself passed perpendicularly upwards through the neck of the bottle, which neck was, by the pipe and by good hard cement, employed to fill the space betwixt the pipe and the inside, so well and firmly closed that no water or air could get out of the bottle, nor no external air could get into it, but by passing through the pipe. This instrument was conveyed into a large receiver shaped like a pear, of which a good part of the blunt end, and a small part of the sharp end are cut off by sections parallel to the horizon, and consequently to one another. And because this receiver was not (nor ought to be) long enough to receive the whole pipe, there was cemented on to the upper part of it a smaller receiver of white glass, of such a length and bigness, that the upper end of the pipe might reach to the middle of its cavity, or thereabouts, and that the motions of the springing water might have a convenient scope, and so be the better taken notice of.

See Plate IV. Fig. 2. This figure was designed only to make some representation of the difference, that would appear, if, instead of making the fourth experiment with water, as in the foregoing figure, the trial was made with quicksilver.

THIS double receiver being cemented on to the engine, a little of the air was by one suck of the pump drawn out from it, by which the pressure of the remaining air being weakened, it was necessary, that since the air included in the bottle had not its spring likewise weakened it should expand itself, and consequently impel up the water in the same bottle through the pipe, which it did so vigorously, as to make it strike briskly at first against that part of the top of the smaller receiver, which was just over the orifice of the pipe. But after it had a while made the water thus shoot up in a perpendicular line, as the spring of the air in the bottle grew by that air's dilatation to be weakened, the water would be impelled up less strongly and less directly, till the air in the bottle being as much expanded as that in the receiver, the ascent of the water would quite cease, unless by pumping a little more air out of the receiver we renewed it again.

ABOUT the making of this experiment these particulars may be noted.

I. It is convenient that the upper part of the pipe be made (as it easily may be at the flame of a lamp) very slender, that the water having but a very small orifice to issue out at, may be spent but slowly, and thereby make the experiment last so much the longer.

II. You may, if you please, instead of making the upper part of the pipe slender, as was just now directed, cement on to it a top either of glass or brass, consisting of three or more very slender pipes, with a pin-hole at the end of each, that one of these pointing directly upwards, and the others to the right hand and to the left, the water may spin out several ways at once, by which kind of branched pipes we have sometimes imitated the *Jets d'eau* (as the French call them) and artificial fountains of gardens and grottoes.

III.

III. In regard that so short a cylinder of water, as exceeded not the length of our glass pipe, could not make any considerable resistance to the expansion of the included air, it was thought and found safe enough to employ, instead of a strong glass-bottle, a much larger phial, without being solicitous about its shape, or that it should be very strong, and by this means we could make this pleasant spectacle last a great while, especially if we also made use of the expedient to be mentioned in the following note.

4. If you find that the included air has by expanding itself too much weakened its spring, whilst there yet remains with it a good quantity of water in the bottle or phial, you may reinforce the pressure of the air by only turning the stop-cock, and letting in what air you think fit to the exhausted receiver: for upon the admission of this new air, the air in the receiver will press upon the water in the pipe, and having driven it into the bottle again, will follow it thither till the air in the bottle and that in the receiver have attained an equal spring, and then by pumping out a convenient quantity of the air contained in the latter, the air shut up in the former will be able to impel up the water as before, till the stagnant liquor be depressed to the lower orifice of the pipe, at which, when the air of the bottle can get out, the course of the water upwards must cease.

The uses I made of this new hydraulo-pneumatical fountain (for in it I aim not only at a ludicrous experiment) were principally these.

The first was to make it the more probable, that if we had had convenient vessels we might, by the pressure of the air included in the bottle, have raised water about fourteen times as high as we did quicksilver in the former experiment, since upon but a little weakening of the pressure of the air in the double receiver, the air in the bottle was able to impel the water forcibly enough, and for a pretty while, to the top of a pipe of about a yard long, and a good deal higher (but this is but a slight use).

The next thing therefore we designed to shew by this experiment was, that in those hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be set a moving as well by the mere dilatation of one of the parcels of the air, as by giving a new force, by heat or compression, to the other; and whether this mechanical principle of motion may hereafter prove not altogether useless in engines, we refer to further consideration.

Another use we made of this experiment was to shew somewhat relating to the spring of the air, which may be worth considering, though we shall now but barely mention it. If then, when some of the air had been pumped out of the receiver, we removed that double vessel from the bottle, the external air would, by its weight, hastily depress the water in the pipe, till having driven it to the very bottom it got up in numerous bubbles through the water, and joined itself with the air incumbent on that liquor; but that which was here observable was, that all the external air that was able to get into the bottle did not do it suddenly, but after the first irruption we could perceive that from time to time there would new portions of air leisurely insinuate themselves through the pipe into the bottle, and emerge through the stagnant water in bubbles, that succeeded one another so slowly as to beget some wonder, as if the spring of the included air having been once put out of its wonted constitution by its late expansion, could not be reduced to it, but by degrees, by the weight of the atmosphere, which was still the same; or, rather, as if between the spring of the included and the pressure of the external air counterbalancing each other, there happened some such thing as is observed in an ordinary pair of scales, of which one is too much depressed, where the motion (which was swift enough at first) becomes so much the slower, by how much the weights come nearer to the æquilibrium which their equality disposes them to rest in.

But

EXPERIMENT IV.

About a new hydraulo-pneumatical fountain, made by the spring of uncompressed air.

I SHALL now add such an application of the principle whereon the former experiment was grounded, as I should scarce think worth mentioning in this place, were it not, that besides that divers virtuosi seem not a little delighted with it, it may, for aught I know, prove to be of some philosophical use (to be pointed at hereafter).

WE took a glass bottle with a convenient quantity of water in it, and fitted this bottle with a slender glass-pipe open at both ends, and about three foot long, which was so placed that the lower orifice was a good way beneath the surface of the water, and the pipe itself passed perpendicularly upwards through the neck of the bottle, which neck was, by the pipe and by good hard cement, employed to fill the space betwixt the pipe and the inside, so well and firmly closed that no water or air could get out of the bottle, nor no external air could get into it, but by passing through the pipe. This instrument was conveyed into a large receiver shaped like a pear, of which a good part of the blunt end, and a small part of the sharp end are cut off by sections parallel to the horizon, and consequently to one another. And because this receiver was not (nor ought to be) long enough to receive the whole pipe, there was cemented on to the upper part of it a smaller receiver of white glass, of such a length and bigness, that the upper end of the pipe might reach to the middle of its cavity, or thereabouts, and that the motions of the springing water might have a convenient scope, and so be the better taken notice of.

See Plate IV. Fig. 2. This figure was designed only to make some representation of the difference, that would appear, if, instead of making the fourth experiment with water, as in the foregoing figure, the trial was made with quicksilver.

THIS double receiver being cemented on to the engine, a little of the air was by one suck of the pump drawn out from it, by which the pressure of the remaining air being weakened, it was necessary, that since the air included in the bottle had not its spring likewise weakened it should expand itself, and consequently impel up the water in the same bottle through the pipe, which it did so vigorously, as to make it strike briskly at first against that part of the top of the smaller receiver, which was just over the orifice of the pipe. But after it had a while made the water thus shoot up in a perpendicular line, as the spring of the air in the bottle grew by that air's dilatation to be weakened, the water would be impelled up less strongly and less directly, till the air in the bottle being as much expanded as that in the receiver, the ascent of the water would quite cease, unless by pumping a little more air out of the receiver we renewed it again.

ABOUT the making of this experiment these particulars may be noted.

I. It is convenient that the upper part of the pipe be made (as it easily may be at the flame of a lamp) very slender, that the water having but a very small orifice to issue out at, may be spent but slowly, and thereby make the experiment last so much the longer.

II. You may, if you please, instead of making the upper part of the pipe slender, as was just now directed, cement on to it a top either of glass or brass, consisting of three or more very slender pipes, with a pin-hole at the end of each, that one of these pointing directly upwards, and the others to the right hand and to the left, the water may spin out several ways at once, by which kind of branched pipes we have sometimes imitated the *Jets d'eau* (as the French call them) and artificial fountains of gardens and grottoes.

III.

III. In regard that ſo ſhort a cylinder of water, as exceeded not the length of our glaſs pipe, could not make any conſiderable reſiſtance to the expanſion of the included air, it was thought and found ſafe enough to employ, inſtead of a ſtrong glaſs-bottle, a much larger phial, without being ſolicitous about its ſhape, or that it ſhould be very ſtrong, and by this means we could make this pleaſant ſpectacle laſt a great while, eſpecially if we alſo made uſe of the expedient to be mentioned in the following note.

4. If you find that the included air has by expanding itſelf too much weakened its ſpring, whilſt there yet remains with it a good quantity of water in the bottle or phial, you may reinforce the preſſure of the air by only turning the ſtop-cock, and letting in what air you think fit to the exhauſted receiver: for upon the admiſſion of this new air, the air in the receiver will preſs upon the water in the pipe, and having driven it into the bottle again, will follow it thither till the air in the bottle and that in the receiver have attained an equal ſpring, and then by pumping out a convenient quantity of the air contained in the latter, the air ſhut up in the former will be able to impel up the water as before, till the ſtagnant liquor be depreſſed to the lower orifice of the pipe, at which, when the air of the bottle can get out, the courſe of the water upwards muſt ceaſe.

The uſes I made of this new hydraulo-pneumatical fountain (for in it I aim not only at a ludicrous experiment) were principally theſe.

The firſt was to make it the more probable, that if we had had convenient veſſels we might, by the preſſure of the air included in the bottle, have raiſed water about fourteen times as high as we did quickſilver in the former experiment, ſince upon but a little weakening of the preſſure of the air in the double receiver, the air in the bottle was able to impel the water forcibly enough, and for a pretty while, to the top of a pipe of about a yard long, and a good deal higher (but this is but a ſlight uſe).

The next thing therefore we deſigned to ſhew by this experiment was, that in thoſe hydraulo-pneumatical engines, where water is placed between two parcels of air, the water may be ſet a moving as well by the mere dilatation of one of the parcels of the air, as by giving a new force, by heat or compreſſion, to the other; and whether this mechanical principle of motion may hereafter prove not altogether uſeleſs in engines, we refer to further conſideration.

Another uſe we made of this experiment was to ſhew ſomewhat relating to the ſpring of the air, which may be worth conſidering, though we ſhall now but barely mention it. If then, when ſome of the air had been pumped out of the receiver, we removed that double veſſel from the bottle, the external air would, by its weight, haſtily depreſs the water in the pipe, till having driven it to the very bottom it got up in numerous bubbles through the water, and joined itſelf with the air incumbent on that liquor; but that which was here obſervable was, that all the external air that was able to get into the bottle did not do it ſuddenly, but after the firſt irruption we could perceive that from time to time there would new portions of air leiſurely inſinuate themſelves through the pipe into the bottle, and emerge through the ſtagnant water in bubbles, that ſucceeded one another ſo ſlowly as to beget ſome wonder, as if the ſpring of the included air having been once put out of its wonted conſtitution by its late expanſion, could not be reduced to it, but by degrees, by the weight of the atmoſphere, which was ſtill the ſame; or, rather, as if between the ſpring of the included and the preſſure of the external air counterbalancing each other, there happened ſome ſuch thing as is obſerved in an ordinary pair of ſcales, of which one is too much depreſſed, where the motion (which was ſwift enough at firſt) becomes ſo much the ſlower, by how much the weights come nearer to the æquilibrium which their equality diſpoſes them to reſt in.

But

But the chief use designed in this experiment was to observe, whether the lines made by the water in its effluxions, would be of the same figure, notwithstanding the rarefaction of the air in the upper part of the receiver, as if the air had not been at all rarefied; and for this purpose it is best to make one's observations towards the latter end of the experiment, because then the receiver being most exhausted, and consequently having the least of air left in it, the difference made by the change of the density of the *medium*, in which the beams of water (if I may so call them) move, is like (in case there be any) to be best discerned. And this convenience we had by our way of experimenting, that we could take notice of the lines described by the salient water, as the ejaculation of that liquor grew still fainter and fainter. But though I afterwards invited Dr. *Wallis* to favour me with his opinion about the curve lines of the salient water, yet for want of an upper receiver large enough, even he professed himself (as I had done) not satisfied about them. Only he sometimes (as I also did) observed the salient water to describe part of a line perfectly enough parabolical, with which sort of curves he has been particularly conversant.

This made me resolve, for further satisfaction, to attempt by another contrivance (of whose success, if I can procure the implements I need, your lordship may expect an account) what the figures will be not only of salient water, but mercury, and other liquors; and that when the receiver is much better exhausted, than it was necessary it should be in the foregoing experiment.

EXPERIMENT V.

About a way of speedily breaking flat glasses, by the weight of the atmosphere.

For the more easy understanding of some of the subsequent trials, it will be requisite in this place to mention, among experiments about the spring of the air, the following phænomenon belonging to its weight.

This is one of those that is the most usually shewn to strangers, as a plain and easy proof, both that the weight of the incumbent air is considerable, and that the round figure of a receiver doth much more conduce to make an exhausted glass support that weight, than if the upper part of the receiver were flat.

To make this experiment we provided a hoop or ring of brass of a considerable thickness, whose height was $2\frac{1}{2}$ or 3 inches, and the diameter of whose cavity as well at the upper as lower orifice (should have been just 3 inches, but through the error of the workman) was 3 inches and $\frac{2}{10}$. To this hoop we successively fastened with cement divers round pieces of glass, such as is used by glasiers (to whose shops we sent for it) to make panes for windows, and thereby made the brass-ring with its glass-cover a kind of receiver, whose open orifice we carefully cemented on to the engine; and then we found, as we had conjectured, that usually at the first exsuction (though sometimes not till the second) the glass-plate would be broken inwards with such violence as to be shattered into a great multitude of small fragments, and (which was remarkable) the irruption of the external air driving the glass inwards did constantly make a loud clap, almost like the report of a pistol: which phænomenon, whether it may help us to discover the cause of that great noise that is made upon the discharging of guns (for the recoil seems to depend upon the dilatation and impulse of the powder) I must not stay to consider.

EXPE-

EXPERIMENT VI.

Shewing, that the breaking of glaſs-plates in the foregoing experiment, need not to be aſcribed to the fuga vacui.

THOUGH I long ſince informed you, that in the experiments I then preſented your lordſhip, it was not my purpoſe to deliver my own opinion, whether there be a vacuum, or no; and though I do not in this tract intend to declare myſelf either way; yet, that I may on this occaſion alſo ſhew, that the preſſure of the air may ſuffice to account for divers phænomena, which, according to the vulgar philoſophers, muſt be referred to nature's abhorrency of a vacuum, I will illuſtrate the foregoing experiment by another, the ſubſtance whereof is this.

THAT if, inſtead of the above-mentioned braſs-hoop, both whoſe orifices are of equal breadth, you may employ a hollow (but taller) piece of braſs, or (which is more eaſily made) of latten, ſhaped like a conus truncatus, or a ſugar-loaf, whoſe upper part is taken off parallel to the bottom; and if you make the two orifices of a breadth ſufficiently unequal, as if the larger being made as wide as that of our braſs hoop, the ſtraiter were leſs than an inch in diameter; you will find, that if this piece of metal be made uſe of, as the other was in the foregoing experiment, the flat glaſs cemented on to the orifice will be eaſily broken, as formerly when it is faſtened to the wider orifice; but if the ſtraiter orifice be turned upward, the glaſs that covers it, if it be of a due thickneſs (though no thicker than the former) will remain entire, notwithſtanding the withdrawing of the air from beneath it: which ſeems ſufficiently to argue, that it is not preciſely nature's abhorrency of a vacuum that is the cauſe why glaſſes are uſually broken in ſuch experiments, ſince, whether the wider or the narrower orifice be uppermoſt, and covered (the metalline part of the veſſel being the ſame, and only varying its poſture) the capacity of the exhauſted veſſel will be equal; and therefore nature ought to break the glaſs as well in one caſe as the other, which yet the experiment ſhews ſhe does not.

WHEREFORE this diverſity ſeems much better explicable by ſaying, that when the wider orifice is uppermoſt, the glaſs that covers it muſt ſerve for the baſis of a large atmoſpherical pillar, which by its great weight may eaſily force the reſiſtance of the glaſs; whereas when the ſmaller orifice is uppermoſt, there leans upon its cover but ſo ſlender a pillar of the atmoſphere, that the natural tenacity of mutual cohæſion of parts in the glaſs is not to be ſurmounted by a weight that is no greater.

EXPERIMENT VII.

About a convenient way of breaking blown bladders by the ſpring of the air included in them.

THE foregoing experiments having ſufficiently manifeſted the ſtrength of the air's ſpring upon fluid bodies, I next thought fit to try whether the force of a little included air would alſo upon conſiſtent and even ſolid bodies emulate the operations of the weight of the atmoſphere. In the proſecution of which enquiry we thought fit to make two ſorts of trials: the one, where the air is included in the bodies on which its ſpring does work; and the other, where it is external to them. Of the firſt ſort are this ſeventh and the two following experiments; and of the second sort are some other trials, to be comprehended under the tenth experiment.

Having formerly mentioned to your lordship, that we were several times able (though sometimes not without much difficulty) to make a blown bladder break with the spring of its own air; I should not think it worth while to say any thing here about the same phænomenon, but that (besides that it seems odd enough, and is not unpleasant to many spectators) it may deserve not to be wholly neglected, because a good way to break bladders in the much exhausted receiver may sometimes prove an useful expedient, especially in such cases, where the experimenter (who sometimes either is not skilful enough, or well enough furnished with accommodations to regulate the ingress of the air) would very suddenly supply the receiver with fresh air, when it has been much emptied, without danger of letting in too much air from without. Not to mention, that the air included in the bladder to be broken may be so mingled with steams, or imbued with divers qualities, as to be much fitter than common air for some particular purposes.

We shall then, for the affinity's sake between this trial and the former, subjoin now the way, by which we seldom failed of breaking bladders in our emptied receivers. For this purpose the blown bladder that was to be burst, having the neck very closely and strongly tied, was kept a pretty while in the receiver, whilst the air was pumping out, and then taken out again, that now the fibres were stretched and relaxed, the capacity being lessened by a new ligature that I ordered to be strongly made near the neck, the bladder might be lessened though the air were but the same, and the membrane being not so capable of yielding as before, upon the second exhaustion of the receiver the bladder in it would break far more easily than otherwise, and perhaps be oddly enough lacerated.

We sometimes also varied this way of disposing bladders to be burst, by omitting the preparatory putting in of the bladder into the receiver, and only taking it in a little near the neck, that, the bladder having not been blown very full at first, the tension of the included air might be greater. But this last way is to be made use of, when the thing we desire is, that the bladder by breaking at a certain time may part with its air, and not when it is only to give an instance of the force of the spring of uncompressed air against the sides of the vessel that contain it.

EXPERIMENT VIII.

About the lifting up a considerable weight by the bare spring of a little air included in a bladder.

You will easily believe, that the force employed (in the following experiment) by the air, to break the well blown bladders it is included in, is considerable, if I here add, that a small quantity of air which will not fill $\frac{1}{4}$ of a bladder, will not only serve to blow it quite up, but will manifestly swell it, though that effect be opposed not only by the resistance of the bladder itself, but by a considerable weight tied to the bottom of it, as in the following experiment.

We took a middle sized bladder (of a hog or sheep) and having pressed out the air, till there remained but a fourth or fifth part (by guess) we caused the neck to be very strongly tied up again; also round about the opposite part of the bladder, within about an inch of the bottom, we so strongly tied another string, that it would not be made to

slip

ſlip off by a not inconſiderable weight we hung at it; then faſtening the neck of the bladder to the turning key, we conveyed the bladder and the weight hanging at it into a large receiver, in which when it began to be pretty well exhauſted, the air within the bladder being freed from the wonted preſſure of the air without it, did by its own ſpring manifeſtly ſwell, and thereby notably ſhorten the bladder that contained it, and by conſequence viſibly lifted up the weight (that reſiſted that change of figure) which exceeded fifteen pounds of ſixteen ounces to the pound.

After that we took a large bladder, and having let out ſo much air, that it was left lank enough, we faſtened the two ends of it to the upper part of the receiver (for which elſe it would have been too long) and tied a weight (but not the ſame) ſo as that it hung down from the middle of the bladder; then exhauſting the receiver as before, though the bladder, and this new weight which ſtretched it, reached ſo low as that for a while we could ſcarce ſee whether it hung in the air or no, yet at length we perceived the bladder to ſwell, and concluded that it had lifted up its clog about an inch; which was confirmed by the return we permitted of the air into the receiver, upon which the bladder became more wrinkled than before, and the weight deſcended, which being taken off, and weighed in a ſtatera, amounted to about 28 pounds. We would have reiterated the experiment, but ſo heavy a weight having broken the bladder, we were diſcouraged from proceeding any further, eſpecially in regard of the difficulty of bringing by this contrivance the ſtrength of the air's ſpring to any exact computation; though it ſufficiently ſhews what I deſigned it ſhould, namely, that the ſpring of a little included air may be able even in ſo ſlight a contrivance to raiſe a great weight.

Whether this experiment may any way illuſtrate the motion of muſcles made by inflation, contraction, &c. it belongs not to this place to conſider.

EXPERIMENT IX.

About the breaking of hermetically ſealed bubbles of glaſs by the bare ſpring of their own air.

I shall premiſe to the following trials an experiment, wherein uncompreſſed air is made by its own bare ſpring to break the ſolid body itſelf it is ſhut up in. And this I the rather ſet down before the ſubſequent trials, becauſe in our already publiſhed phyſico-mechanical experiments mention has been made of this trial, as of one that we could not then make to ſucceed; we have ſince, employing ſmaller receivers, made it often enough proſperouſly, ſomewhat to the wonder of eminent virtuoſi, who confeſſed to me they had made frequent and divers attempts to perform the ſame thing, without ever ſucceeding in any of them.

But it will not be requiſite to multiply relations about this particular, and therefore I ſhall ſet down but this one, which I meet with among my looſe notes.

A large glaſs bubble hermetically ſealed being put into the receiver, and the air drawn out as much as in uſual operations, and ſomewhat more, though I told the company beforehand, that I had ſeveral times obſerved that ſuch bubbles would not break immediately, but ſome while after the withdrawing the air from about them; yet this continued ſo long entire after we had left off pumping, that preſuming it had been blown too ſtrong, I began to deſpair of the experiments ſucceeding; when, whilſt we were providing ſomething elſe to put into the receiver, and, as I gueſſed, four minutes after the pump had been let alone, the bubble ſurprized us with its being broken with ſuch violence by the spring of the included air, that the fragments of it were dashed every way against the sides of the receiver, and broken so very small, that when we came to take it up, the powder was by the by-standers compared to the small sand wont to be employed to dry papers that have been newly writ upon with ink. The reason why the bubble broke so slowly I cannot now stay to propose, no more than to examine whether the difficulty of breaking vessels of glass, no thicker than these bubbles, proceed from some weakening of the spring of imprisoned air, by its stretching a little the including glass (for in another case we have observed this glass to be stretchable by the pressure of air) or from hence, that it was very hard, as I have elsewhere mentioned, to avoid rarefying the air a little, and consequently weakening its spring, by the heat that was necessary to be employed about the sealing up the bubble.

EXPERIMENT X.

Containing two or three trials of the force of the spring of our air uncompressed upon stable and even solid bodies (whereto it is external).

In prosecution of the inquiry proposed in the title, we made (among others) the following trials.

The First Trial.

I. We took the brass-hoop, mentioned in the fifth experiment (whose diameter is somewhat above three inches) and having caused a glazier to cut some plates of glass, such as are used for making the quarrels of windows, till he had brought them to a size and a roundness fit to serve for covers to that brass-hoop, we carefully fastened one of them with cement to the upper orifice of the hoop or ring, and then cementing the lower orifice to the engine, so that the vessel, composed of the metal and glass, served for a small receiver; we whelmed over it a large and strong receiver, which we also fastened on to the engine with cement after the usual manner. By which contrivance it was necessary, that when the pump was set on work, the included receiver (of brass and glass) should have its air withdrawn, and yet the air in the larger receiver should not be pumped out but by breaking through the glass, so that the internal air of the metalline receiver (as we may call it for distinction sake) being pumped out, the glass plate that made part of that receiver, must lie exposed to the pressure of the ambient air shut up in the other receiver without having the former assistance of the now withdrawn air to resist the pressure; wherefore, as we expected, at the first or second exsuction of the air, included in the small metalline receiver, the glass-plate was, by the pressure of the incumbent air contained in the great receiver, broken into an 100 pieces, which were beaten inwards into the cavity of the hoop.

The Second Trial.

This done, to shew that there needed not the spring of so great a quantity of included air to break such glasses, we took another roundish one, which, though wide enough at the orifice to cover the brass ring and the new glass-plate that we had cemented on it, was yet so low that we estimated it to hold but a sixth part of what the large receiver formerly employed is able to contain; and having whelmed this smaller vessel, which was

shaped

ſhaped like thoſe cups they call tumblers, over the metalline receiver, and well faſtened it to the engine with cement, we found, that though this external receiver had a great part of its cavity filled by the included one, yet when this internal one was exhauſted by an exſuction or two, the ſpring of the little air that remained was able to break the plate into a multitude of fragments.

The Third Trial.

III. Because the glaſs-plates hitherto mentioned ſeemed not ſo thick but that the preſſure of the included air might be able to give conſiderable inſtances of its force; inſtead of the metalline receivers hitherto employed, we took a ſquare bottle of glaſs, which we judged to be able to contain about a pint (or pound) of water, and which had been provided to keep ſubtle chymical liquors in, for which uſe we are not wont to chuſe weak ones. This we inverted, and applied to the engine as a receiver, over which we whelmed the large receiver formerly mentioned; and having cemented it on, as in the foregoing experiments, we ſet the pump on work to empty the internal receiver (or ſquare bottle) by which means the withdrawing of the air, and the figure of the veſſel (which was inconvenient for reſiſting) ſuffered the preſſure of the air included in the external receiver to cruſh the phial into a great number of pieces.

And to vary this experiment, as we did that of breaking the metalline receivers, we took another glaſs of the ſhape and about the bigneſs of the former, and having applied it to the engine as before, and covered it with a receiver that was little higher than itſelf, we found that upon the exhauſtion of the air the ſecond ſquare glaſs was likewiſe broken into many fragments, ſome of which were of ſo great a thickneſs as moved ſome wonder, that the bare preſſure of the air was able to break ſuch a veſſel, though probably the cracks that reached to them were begun in much weaker parts of the glaſs.

N. B. 1. The bottoms and the necks of both theſe ſquare bottles were entire enough; by which it ſeemed probable that the veſſels had been broken by the preſſure of the air againſt the ſides, which were not only thinner than the parts above named, but expoſed a larger ſuperficies to the lateral preſſure of the air, than to the perpendicular.

2. We obſerved in one of the two laſt experiments, that the veſſel did not break preſently upon the laſt exſuction that was made of the included air, but a conſiderable time after, which it ſeems was requiſite to allow the compreſſed parts of the glaſs time to change their places; and this phænomenon I therefore mention, becauſe the ſame thing that here happened in the breaking a glaſs inwards by the ſpring of the air, I elſewhere obſerved to have happened in breaking a glaſs outwards by the ſame ſpring.

3. To confirm that it is the ſpring of the external receiver's air, that is the agent in thoſe fractures of glaſſes, and to prevent or remove ſome ſcruples, we thought fit to make this variation in the experiment. We applied a plate of glaſs, juſt like thoſe formerly mentioned, to the braſs-hoop; but in the cementing of it on, we placed in the thickneſs of the cement a ſmall pipe of glaſs about an inch long, whoſe cavity was not ſo big as that of a ſtraw, and which being left open at both the ends, might ſerve for a little channel, through which the air might paſs from the external receiver to the internal; over this we whelmed one of the ſmall receivers above mentioned, and then, though we ſet the pump on work much longer than would have needed, if this little pipe had not been made uſe of, we found, as we expected, that the internal receiver continued entire, becauſe the air, whoſe ſpring ſhould have broken it, having liberty to paſs through the pipe, and conſequently to expand itſelf into the place deſerted by the

air pumped out, did by that expanſion weaken its ſpring too much to retain ſtrength enough to break the metalline (or internal) receiver

But here it is to be noted, that either the pipe muſt be made bigger than that lately mentioned, or the exſuction of the air muſt not be made by the pump as nimbly as we can, or otherwiſe the plate of glaſs may be broken, notwithſtanding the pipe; becauſe the air contained in the external receiver having a force much greater than is neceſſary to break ſuch a plate, it may well happen (as I have ſometimes found it do) that if the air be haſtily drawn out of the internal receiver, that air, which ſhould ſucceed in its room, cannot get faſt enough out of that external receiver through ſo ſmall a pipe; and the air remaining in that external receiver will yet retain a ſpring ſtrong enough to break the glaſs. To illuſtrate which I ſhall propoſe this experiment; that ſometimes, when I have at the flame of a lamp cauſed glaſs bubbles to be blown with exceeding ſlender ſtems, if they were nimbly removed out of the flame whilſt they were ignited, they would, according to my conjecture, be either broken, if they cooled too faſt, or compreſſed inward, if they long enough retained the ſoftneſs they had given them by fuſion. For the air in the bubble being exceeding rarefied and expanded, whilſt the glaſs is kept in the flame, and coming to cool haſtily when removed from thence, loſes upon refrigeration the ſpring the heat had given it; and ſo if the external air cannot preſs in faſt enough through the too ſlender pipe, there will not get in air enough to reſiſt the preſſure of the atmoſphere; and therefore if this preſſure find the bubble yet ſoft, it will preſs it a little inwards, and either flatten it, or make a dimple in it, though the orifice of the pipe be left open.

EXPERIMENT XI.

Shewing that mercury will in tubes be raiſed by ſuction no higher than the weight of the atmoſphere is able to impel it up.

It is ſufficiently known that the common opinion of philoſophers, and eſpecially of thoſe which follow *Ariſtotle*, has long been, and ſtill is, that the cauſe of the aſcenſion of water upon ſuction, and particularly in thoſe pumps where the water ſeems of its own accord to follow the riſing ſucker, is nature's abhorrency of a vacuum. Againſt this received opinion divers of the modern philoſophers have oppoſed themſelves. But as ſome of them were vacuiſts, and others pleniſts, they have explicated the aſcenſion of water in ſucking-pumps upon very different grounds; ſo that many ingenious men continue yet irreſolved in this noble controverſy. Wherefore though I have formerly made, and now renew a ſolemn profeſſion, that I do not in this treatiſe intend to declare either for or againſt the being of a vacuum; and though I have * elſewhere occaſionally acknowledged myſelf not to acquieſce fully in what either the ancient or the modern philoſophers have taught about the adequate cauſe of ſuction (in the aſſigning of which, I think, I have ſhewn them to have been ſomewhat deficient) yet, ſince I think ſome experiments, of importance to this controverſy, may be better made by the help of our engine, than they have been by any inſtrument I have yet heard of, I ſhall now add the trials I made to ſhew both that whether there be, or may be a vacuum or not, there is no need to have recourſe to a *fuga vacui* to explicate ſuction; and alſo that whatever other cauſes have by *Gaſſendus* and *Carteſius* been ingeniouſly propoſed to explicate

* The place here meant is a paſſage in the Author's *Examen* of Mr. *Hobbes*'s *Dialogue* about the air.

ſuction,

fuction, it feems to depend clearly upon the weight of the atmofphere, or in fome cafes upon the fpring of the air; though I deny not that other caufes may contribute to that preffure of the air, which I take to be the grand and immediate agent in thefe phænomena.

We took a brafs pipe bended like a fiphon, and fitted at the bigger end with a ftop-cock, &c. as is delineated in the figure (which inftrument for brevity fake, I often call an exhaufting or fucking fiphon) and to the flender end of this we faftned with good cement the upper end of a cylindrical pipe of glafs, of about fifty inches long, and open at both ends, and having the lower end open into a glafs of ftagnant quickfilver, whofe upper fuperficies reached a pretty deal higher than the immerfed orifice of the glafs cane. Thefe things being thus prepared, we caufed the pump to be fet on work, whereby the air being by degrees drawn out of the exhaufting fiphon, and confequently of the glafs cane that opened into it; the ftagnant mercury was proportionably impelled up into the glafs-pipe, until it had attained to its due height, which exceeded not 30 inches; and then, though there remained in the upper part of the pipe above 20 inches unfilled with quickfilver, yet we could not by farther pumping raife that fluid metal any higher.

See Plate III. Fig. 2. and the annotations at the clofe of this experiment.

By which it feems manifeft enough, that whatever many learned men have taught, or others do yet believe, about the unlimited power that nature would exercife to prevent what they call a vacuum; yet this power has its bounds, and thofe depend not fo much upon the exigency of that principle, which the fchoolmen call a *fuga vacui*, as upon the fpecifick gravity of the liquor to be raifed by fuction. For confirmation of which, we fubftituted inftead of the ftagnant mercury, a bafon of water; and though inftead of the many fucks we had fruitlefly imployed to raife the quickfilver above the lately mentioned height, we now employed but one exfuction (or lefs than a full one) which did but in part empty the exhaufting fiphon: yet the water upon the opening of the ftop-cock was not only impelled to the very top of the glafs-cane, but likewife continued running for a good while through the exhaufting fiphon, and thence fell upon the plate of the engine; fo that it feemed an odd fpectacle to thofe that knew not the reafon of it, to fee the water running very brifkly of its own accord, as they imagined, out of the fhorter leg of a fiphon; efpecially that leg being perhaps not above a quarter fo long as the other. And here I muft not omit this confiderable circumftance, that though fometimes in the Torricellian experiment I have obferved the mercury to ftand at thirty inches, and now and then above it, yet the height of the mercury elevated in our glafs-cane appeared not, when meafured, to reach fully 29 inches and a quarter; which I thought it was not difficult to render a reafon of, from the varying weight of the atmofphere; and accordingly confulting the barofcope (that ftood in another room) I found the atmofphere to be at that time fomewhat light, the quickfilver in it being in height but 29 inches and an eighth, which probably would have been the very height of the quickfilver raifed by the engine, if it had had time by ftanding to free itfelf from bubbles.

From whence we may conclude, that fuction will elevate liquors in pumps no higher than the weight of the atmofphere is able to raife them, fince the clofenefs requifite in the pump of our engine to be ftaunch makes it very unlikely, that by any ordinary pump a more accurate fuction can be effected.

I have nothing to add about the related experiment but this one; that it may afford us a noble confirmation of the argument we formerly propofed againft them, that afcribed the elevation and fuftentation of the quickfilver in the Torricellian experiment to a certain rarefied air, which the more highly it is rarefied, the greater power

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it acquires to attract quickſilver, and other contiguous bodies; for in our experiment, though by continuing to pump we can rarefy or diſtend more and more the air in the exhauſting ſiphon, yet we were not able to raiſe the mercury above thirty inches (which exceeds not the height to which the atmoſphere is able to elevate it) and this, though the ſtagnant mercury being expoſed to the free air, it cannot be pretended (as in ſome other caſes it may, though not ſatisfactorily, be done) that the mercury cannot be raiſed higher, without offering violence to the body incumbent on the ſtagnant mercury: for, in the experiment, we are conſidering if nature ſhould raiſe the quickſilver higher and higher in the pipe, to ſucceed in the room of the air that is withdrawn, the formerly ſtagnant mercury, that would on this occaſion be raiſed, might be immediately ſucceeded by the free and undilated air; ſo that nature would be put to offer violence to the quickſilver only, which if ſhe were ſcrupulous to do, what ailed her to raiſe it (as ſhe did in our trial) againſt the inclinations of ſo ponderous a body, to above 29 inches high?

ANNOTATION.

Though the exhauſting ſiphon, mentioned at the beginning of this experiment, may be eaſily enough conceived by an attentive inſpection of the figure; yet, becauſe I frequently made uſe of it in pneumatical experiments, it will not be amiſs to intimate here once for all theſe three particulars about it. 1. That though the bending pipe itſelf may be for ſome uſes more conveniently made of glaſs than of metal, becauſe the tranſparency of the former may enable us to diſcover what paſſes in it, yet for the moſt part we chuſe to employ pipes of the latter ſort, becauſe the others are ſo very ſubject to break. 2. That it is convenient to make the longer leg of the ſiphon a little larger at the bottom than the reſt of the pipe uſually needs to be, that it may the more commodiouſly admit the ſhank of a ſtop-cock, which is to be very carefully inſerted with cement; by ſeaſonably turning and returning of which ſtop-cock, the paſſage (for the air) between the engine and the veſſel to be exhauſted is to be opened and ſhut. 3. That though we ſometimes content ourſelves to apply immediately the braſs ſiphon itſelf to the engine, by faſtening with cement the external ſhank of the ſtop-cock to the orifice of the little pipe through which the exſuction of the air is made, yet the bended pipe alone, if it be not almoſt conſtantly held, is ſo apt to be looſened by the motion of the engine, and the turning of the ſtop-cock (which frequently occaſions leaks, and diſturbs the operation) that for the moſt part we make uſe of a ſiphon, conſiſting of a braſs-pipe, and ſtop-cock, and a glaſs of 6, 8, or 10 inches in height, and of ſome ſuch ſhape (for it need not be the very ſame) as that repreſented in the figure: for by this means, though the exhauſtion is becauſe of this additional glaſs ſomewhat longer in making, yet it is more ſecurely and uninterruptedly carried on by reaſon of the ſtability which the breadth of the lower orifice of the glaſs gives to the whole inſtrument. Beſides which, we have theſe other conveniencies, that not only the ſiphon is hereby much lengthened, which in divers trials is very fit; but alſo that we may commodiouſly place in the glaſſy part of this compounded ſiphon a gage whereby to diſcern from time to time how much the air is drawn out of the veſſel to be exhauſted.

See Plate III. Fig 2.

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EXPERIMENT XII.

About the differing heights whereto liquors will be elevated by suction, according to their several specifick gravities.

If when I was making the foregoing experiment I had been able to procure a pipe long enough, I had tried to what height I could raise water by suction, though I would have done it rather to satisfy others than myself, who scarce doubted but that as water is (bulk for bulk) about 14 times lighter than quicksilver, so it would have been raised by suction to about four or five-and-thirty feet (which is 14 times as high as we were able to elevate the quicksilver) and no higher. But being not furnished for the trial I would have made, I thought fit to substitute another, which would carry the former experiment somewhat further. For whereas in that we shewed how high the atmosphere was able by its whole gravitation to raise quicksilver; and whereas likewise that which appears in Monsieur *Paschal*'s experiment is, at what height the whole weight of the atmosphere can sustain a cylinder of water: by the way that I thought on, it would appear (which hath not yet that I know of been shewn) how a part of the pressure of the air would in perpendicular pipes raise not only the two mentioned liquors, but others also to heights answerable to the degree of pressure, and proportionable to the specifick gravities of the respective liquors.

To make this trial the more clear and free from exceptions, I caused to be made and inserted to the shorter leg of the above-mentioned exhausting siphon a short pipe, which branched itself equally to the right hand and the left, as the adjoining figure declares. In which contrivance I aimed at these two conveniences; one that I might exhaust two glass-canes at the same time; and the other, to prevent its being surmised that the engine was not equally applied to both the glasses to be exhausted. This additional brass-pipe, being carefully cemented into the sucking siphon, we did to each of its two branches take care to have well fastened with the same cement a cylindrical glass of about 42 inches in length (that being somewhat near the height of our exhausting siphon above the floor) the lower orifice of one of these two glasses being immersed in a vessel of stagnant mercury, and that of the other in a vessel of water, where care was taken by those I employed, that as the tubes were chosen near of a bigness (which yet was not necessary) so the surfaces of the two different liquors should be near of a height. This being done, we began to pump warily and slowly, till the water in one of the pipes was elevated about 42 inches; and then measuring the height of the quicksilver in the other pipe above the surface of the stagnant quicksilver, we found it to be almost three inches; so that the water was about 14 times as high as the quicksilver. And to prosecute the experiment a little further, we very warily let in a little air to the exhausting siphon, and had the pleasure to see the two liquors proportionably descend, till turning the stop-cock when the water was about 14 inches high, we thereby kept them from sinking any lower, till we had measured the height of the quicksilver, which we found to be about one inch. Plate III. Fig. 3.

We tried also the proportion of these two liquors at other heights, but could not easily measure them so well as we did at those newly mentioned; and therefore though there seemed to be some slight variation, yet we looked upon it but as what might be well imputed to the difficulty of making such experiments exactly; and this displeased me not in these trials, that whereas it was observed, and somewhat wondered at, that

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the quickfilver for the moft part feemed to be fomewhat (though but a very little) higher than the proportion of 1 to 14 required, I had long before by particular trials found that though 14 and 1 be the neareft of fmall integer numbers that exprefs the proportion between the fpecifick gravities of quickfilver and water, yet the former of thofe fluids (or at leaft that which I made my trials with) is not quite fo heavy as this proportion fuppofes, though I fhall not here ftay to determine precifely the difference, having done it in another tract, where the method I employed in the inveftigation of it is alfo fet down.

THE above-mentioned experiment, made by the help of our engine, as to quickfilver and water being confirmable by trials (to be by and by mentioned) made in other liquors, affords our hypothefis too confiderable advantages above the vulgar doctrine of the fchools (for I do not apply what follows to all the plenifts) who afcribe the afcenfion of liquors by fuction to attraction made *ob fugam vacui*, as they are wont to fpeak.

FOR firft it is manifeftly agreeable to our doctrine, that fince the air, according to it, is a fluid that is not void of weight, it fhould raife thofe liquors that are lighter, as water, higher than thofe that are ponderous, as quickfilver; and that anfwerably to the difparity of their weights. And fecondly, there is no reafon why, if the air be withdrawn by fuction from quickfiver and water, there fhould be lefs left a vacuum above the one than above the other, in cafe either of them fucceed not in the place deferted by the air; and confequently when the air is withdrawn out of both the forementioned glafs-pipes, if there would be no vacuum in cafe no liquor fhould fucceed it, why does nature needlefsly to prevent a vacuum make the water, that is an heavy body, afcend contrary to its own nature, according to which it tends towards the center of the earth? And if the fucceeding of a liquor be neceffary to prevent a vacuum, how chance that nature does not elevate the quickfilver as well as the water; efpecially fince it is manifeft by the foregoing experiment, that fhe is able to raife that ponderous liquor above 26 inches higher than fhe did in the experiment we are now difcourfing of.

PERHAPS it would not be amifs to take notice on this occafion, that among other applications of this experiment it may be made fomewhat ufeful to eftimate the differing gravities of liquors; to which purpofe I caufed to be put under the bottom of the fore-mentioned glafs-pipes two veffels, the one with frefh water, and the other with the like water impregnated with a good proportion of fea-falt that I had caufed to be diffolved in it, for want of fea-water, which I would rather have employed. And I found that when the frefh water was raifed to about 42 inches, the faline folution had not fully reached to 40.

BUT though this difference were double to that which the proportion and gravity betwixt our fea-water and frefh water would have required; yet to make the difparity more evident, and alfo becaufe I would be able the better to guefs at the proportion of the diffolved falt, by making it as great as I could, I caufed an unufual brine to be made, by fuffering fea-falt to deliquate in the moift air; and having applied this liquor and frefh water to the two already mentioned pipes, and proceeded after the former manner, we found that when the pure water was elevated to near 42 inches, the liquor of fea-falt wanted about 7 inches and a quarter of that height; and when the water was made to fubfide to the middle of its pipe, or thereabouts, the faline liquor in the other pipe was between three and four inches lower than it.

I WOULD have tried the difference between thefe liquors and oil, but the coldnefs of the weather was unfavourable to fuch a trial: but to fhew a far greater difparity than that would have done betwixt the height of liquors of unequal gravities, I took fair water, and a liquor made of the falt of pot-afhes fuffered to run in a fellar *per deliquium* (this

(this being one of the ponderousest liquors I ever prepared) and having proceeded as in the former trials, I found that when the common water was about 42 inches high, the newly mentioned solution wanted somewhat of 30 inches; and when the water was made to subside to the middle of its pipe, or thereabouts, the deliquated liquor was between 6 and 7 inches lower than it.

I HAD some thoughts, when I applied myself to make these trials, to examine how well we could by this new way compare the saltness of the waters of several seas, and those also of salt-springs; and likewise whether and (if any thing near) how far we might by this method determine the proportion of the more simple liquors that may be mingled in compounded ones, as in the mixture of water and wine, vinegar and water, &c. but being not provided with instruments fit for such nice trials, and a mischance having impaired the glasses lately mentioned before the last trials were quite ended, and having soon after broken one of them, I laid aside those thoughts.

EXPERIMENT XIII.

About the heights to which water and mercury may be raised, proportionably to their specifick gravities, by the spring of the air.

IN prosecution of the parallel formerly begun betwixt the effects of the weight of the atmosphere, and the spring of included air, we thought fit, after the foregoing, to make the following experiment.

WE took a strong glass-bottle, capable to hold above a pint of water, and having in the bottom of it lodged a convenient quantity of mercury, we poured on it a greater quantity of water (because this liquor was to be impelled up many times higher than the other) and having provided two slender glass-pipes, each open at both ends, we so placed and fastened them by means of the cement, wherewith we choaked the upper part of the neck of the bottle, that the shorter of the pipes had its lower orifice immersed beneath the surface of the quicksilver, and the longer pipe reached not quite so low as that surface, and so was immersed but in the water, by which contrivance we avoided the necessity of having two distinct vessels for our two stagnant liquors, which would have been inconvenient in regard of the slenderness of the upper part of our receiver. This done, we conveyed the bottle into a fitly shaped receiver (formerly described at the first experiment, and having begun to pump out the air, we took notice to what heights the quicksilver and water were impelled up in their respective tubes, on which we had before made marks from inch to inch with hard wax (that they might not be removed by wet or rubbing) and we observed that when the quicksilver was impelled up to two inches, the water was raised to about eight-and-twenty; and when the quicksilver was about one inch high, the water was about fourteen. I say, about, partly because some allowances must be made for the sinking of the superficies of the stagnant quicksilver, and the greater subsidence of that of the stagnant water, by reason of the liquors impelled into the two pipes; partly because that the breadth of the mark of wax was considerable, when the quicksilver was but about an inch high, and so made it difficult to discern the exact height of the metal, when the water was fallen down to fourteen inches; especially in regard that the quicksilver never ascending so high as the neck of the bottle (which the water left far beneath it) the thickness of the receiver, and that of so strong a bottle, made it difficult to discern so clearly the station of the quicksilver as I could have wished.

EXPERIMENT XIV.

About the heights answerable to their respective gravities, to which mercury and water will subside, upon the withdrawing of the spring of the air.

For the further illustration of the doctrine proposed in the last, and some of the foregoing experiments, about the raising and sustentation of liquors in pipes by the pressure of the air, I thought it not unfit to make the following trial, though it were easy to foresee in this peculiar experiment a peculiar difficulty.

We caused then to be conveyed into a fitly shaped receiver two pipes of glass very uneven in length, but each of them sealed at one end: the shorter tube was filled with mercury, and inverted into a small glass jar, wherein a sufficient quantity of that liquor had been before lodged; the longer pipe was filled with common water, and inverted into a larger glass, wherein likewise a fit proportion of the same liquor had been put.

Then the receiver being closely cemented on to the engine, the air was pumped out for a pretty while before the mercury began to subside; but when it was so far withdrawn, that its pressure was no longer able to keep up a mercurial cylinder of that height, that liquid metal began to sink; the water in the other tube, though this were three times as long, still retaining its full height. But when the quicksilver was fallen so low, as to be but between three and four inches above the surface of the stagnant quicksilver, the water also began to subside, but sooner than according to the laws of mere staticks it ought to have done, because many aerial particles emerging from the body of the water to the upper part of the glass, did by their spring concur with the gravity of the water to depress this liquor. And so when the quicksilver was three inches above the stagnant mercury, the water in the other pipe was fallen divers inches beneath 42, and several inches beneath 28, when the mercury had subsided an inch lower. But this being no more than was to be expected, after we had caused the pumping to be a while continued, to free the water the better from the latitant air, we let in the external air; and having thereby impelled up again both the liquors into their pipes, and removed the receiver, we took out those pipes, and inverting each of them again to let out the air (for even that which held the quicksilver had got a small bubble, though inconsiderable in comparison of the air that had got up out of the water) we filled each of them with a little of the restagnant liquor belonging to it; and inverting each tube once more into its proper liquor, we repeated the experiment, and found it, as it seemed, to require more pumping than before to make the liquors begin to subside; so that when the mercury was fallen to three inches, or two or one, the water subsided so near to the heights of 42, 28, or 14 inches, that we saw no sufficient cause to hinder us from supposing that the little differences that appeared between the several heights of the quicksilver, and fourteen times as great heights of the water (which fell somewhat lower than its proportion in gravity required) proceeded from some aerial corpuscles yet remaining, in spite of all we had done, in the water, and by their spring, though but faint, when once they had emerged to the upper part of the glass, furthering a little the depression of it: not now to mention lesser circumstances, particularly the surface of the stagnant water did not inconsiderably rise by the accession of the water lately in the pipe; whereby the cylinder of water, raised above that surface, became by so much the shorter. However your lordship may, if you think fit, cause the experiment to be reiterated, which I could not well do, by reason of a mischance that befel the receiver.

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EXPERIMENT XV.

About the greateſt height to which water can be raiſed by attraction or ſucking pumps.

SINCE the making and the writing of the foregoing experiments, having met with an opportunity to borrow a place ſomewhat convenient to make a trial to what height water may be raiſed by pumping, I thought not fit to neglect it. For though both by the conſideration of our hypotheſis, to whoſe truth ſo many phænomena bear witneſs; and though particularly by the conſequences deducible from the three laſt recited experiments I were kept from doubting what the event would be, yet I thought it worth while to make the trial.

I KNOW what is ſaid to have been the complaint of ſome pump-makers: but I confeſs the phænomenon it was grounded on, ſeemed not to me to be certainly enough delivered by a writer or two, that mention what they complained of; and their obſervation ſeems not to have been made determinately or carefully enough for a matter of this moment; ſince that which they complain of ſeems to have been in general, that they could not by pumping raiſe water to what height they pleaſe, as the common opinion of philoſophers about nature's *fuga vacui* made them expect they might. And it may well have happened, that as they endeavoured only to raiſe it to the height their occaſions required, ſo all that their diſappointment manifeſted was, that they could not raiſe it to that particular height; which did not determine, whether, if the pump had been a foot or a yard ſhorter, the water would then have been elevated to the upper part of it or no: but that which I chiefly conſider is, that theſe being but tradeſmen that did not work according to the dictates of, or with deſign to ſatisfy a philoſophical curioſity, we may juſtly ſuſpect that their pumps were not ſufficiently ſtanch, nor the operation critically enough performed and taken notice of.

WHEREFORE, partly becauſe a trial of ſuch moment ſeemed not to have yet been duly made by any, and partly becauſe the varying weight of the atmoſphere was not (that appears) known, nor (conſequently) taken into conſideration by the ingenious Monſieur *Paſchal* in his famous experiment, which yet is but analogous to this; and partly becauſe ſome very late, as well as learned writers, have not acquieſced in his experiment, but do adhere to the old doctrine of the ſchools, which would have water raiſeable in pumps to any height, *ob fugam vacui* (as they ſpeak) I thought to make the beſt ſhift I could to make the trial, of which I now proceed to give your lordſhip an account.

THE place I borrowed for this purpoſe was a flat roof about 30 feet high from the ground, and with rails along the edges of it. The tube we made uſe of ſhould have been of glaſs, if we could have procured one long and ſtrong enough; but that being exceeding difficult, eſpecially for me who was not near a glaſs-houſe, we were fain to cauſe a tin-man to make ſeveral pipes of above an inch bore (for of a great length it was alledged they could not be made ſlenderer) and as long as he could, of tin or latten, as they call thin plates of iron tinned over; and theſe being very carefully ſodered together made up one pipe of about one or two-and-thirty feet long, which being tied to a pole, we tried with water whether it was ſtanch, and by the effluxions of that liquor finding where the leaks were, we cauſed them to be ſtopped with ſoder; and then for greater ſecurity, the whole pipe, eſpecially at the commiſſures, was diligently caſed over with our cloſe black cement, upon which plaiſter of Paris was ſtrewed

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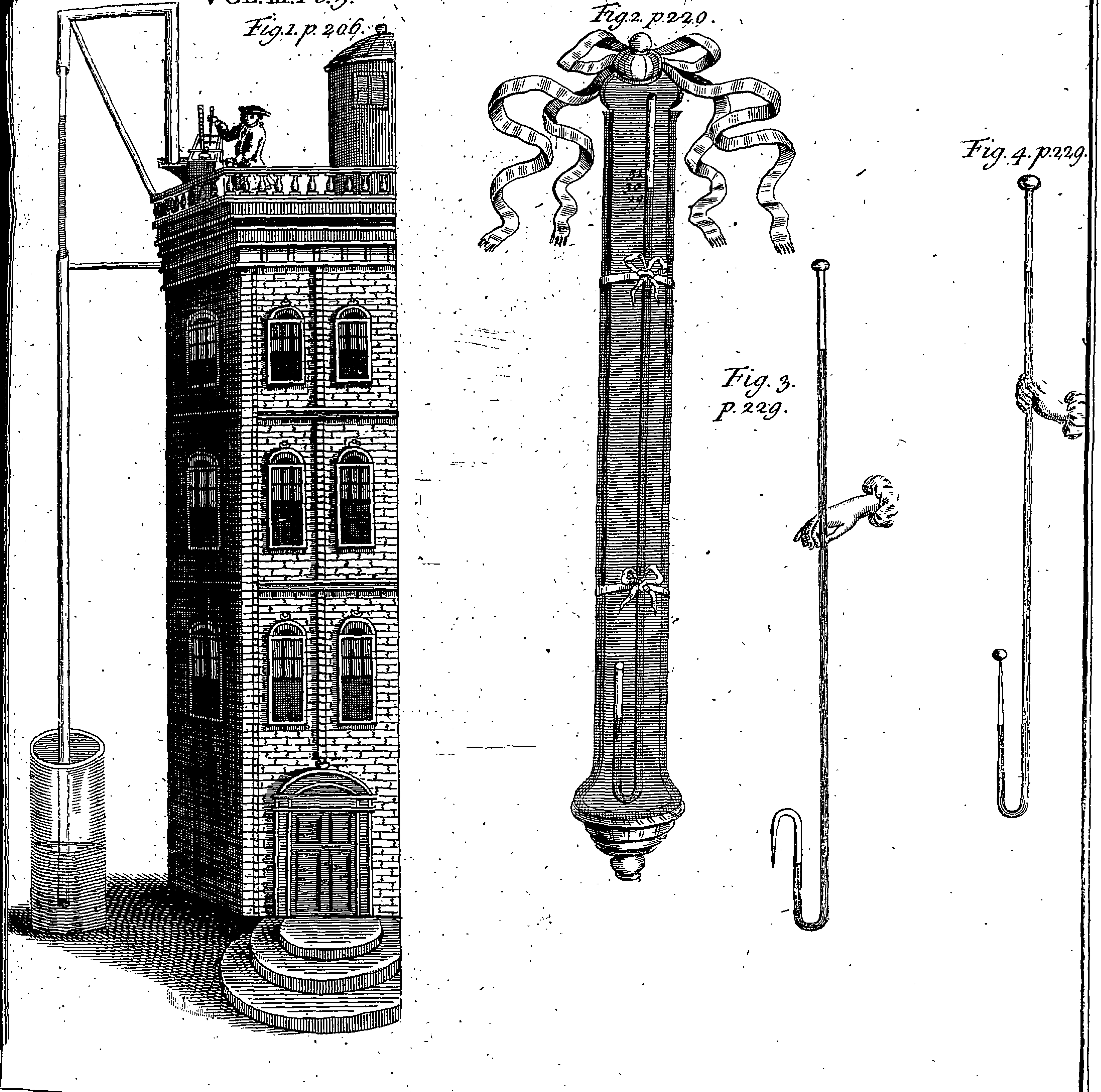
to keep it from ſticking to their hands or cloaths that ſhould manage the pipe; at the upper part of which was very carefully faſtened with the like cement a ſtrong pipe of glaſs, of between 2 and 3 feet in length, that we might ſee what ſhould happen at the top of the water; and to the upper part of this pipe was (with cement, and by the means of a ſhort elbow of tin) very cloſely faſtened another pipe of the ſame metal, conſiſting of two pieces, making a right angle with one another, whereof the upper part was parallel to the horizon, and the other, which was parallel to the glaſs-pipe, reached down to the engine, which was placed on the flat roof, and was to be with good cement ſolicitouſly faſtened to the lower end of this deſcending part of the pipe, whoſe horizontal leg was ſupported by a piece of wood nailed to the above mentioned rails; as the tube alſo was kept from overmuch ſhaking by a board faſtened to the ſame rails, and having a deep notch cut in it, for the tube to be inſerted into.

THIS apparatus being made, and the whole tube with its pole erected along the wall, and faſtened with ſtrings and other helps, and the deſcending pipe being carefully cemented on to the engine, there was placed under the bottom of the long tube a convenient veſſel, whereinto ſo much water was poured, as reached a great way above the orifice of the pipe, and one was appointed to ſtand by to pour in more as need ſhould require, that the veſſel might be ſtill kept competently full.

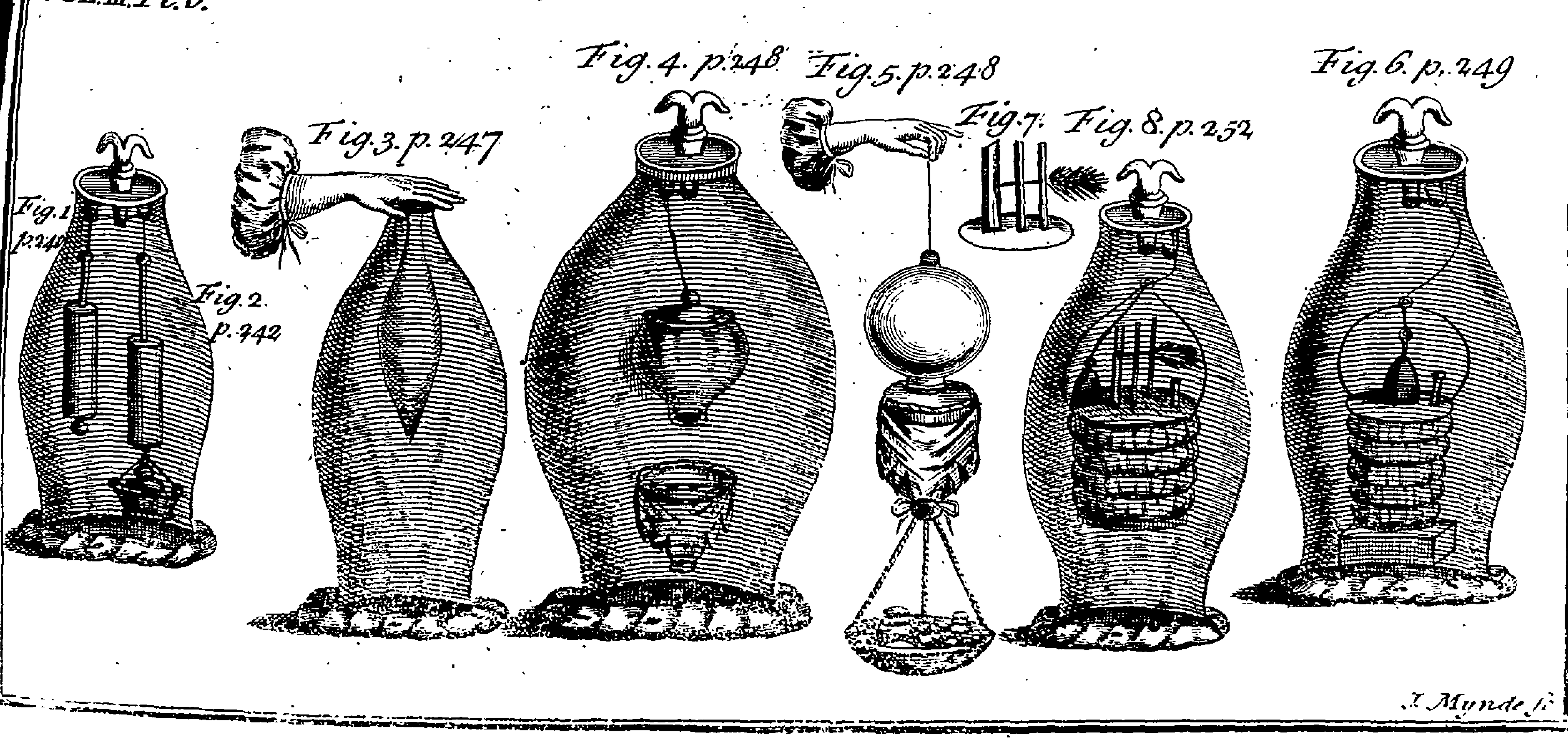
See Plate V. Fig. 1. AFTER all this, the pump was ſet on work; but when the water had been raiſed to a great height, and conſequently had a great preſſure againſt the ſides of the tube, a ſmall leak or two was either diſcovered or made, which, without moving the tube, we cauſed to be well ſtopped by one that was ſent up a ladder to apply ſtore of cement where it was requiſite.

WHEREFORE, at length we were able, after a pretty number of exſuctions, to raiſe the water to the middle of the glaſs-pipe abovementioned, but not without great ſtore of bubbles, made by the air formerly concealed in the pores of the water, and now emerging, which for a pretty while kept a kind of foam upon the ſurface of it (freſh ones continually ſucceeding thoſe that broke). And finding the engine and tube as ſtanch as could be well expected, I thought it a fit ſeaſon to try what was the utmoſt height to which water could by ſuction be elevated; and therefore, though the pump ſeemed to have been plied enough already, yet for further ſatisfaction, when the water was within few inches of the top of the glaſs, I cauſed twenty exſuctions more to be nimbly made, to be ſure that the water ſhould be raiſed as high as by our pump it could be poſſibly. And having taken notice where the ſurface reſted, and cauſed a piece of cement to be ſtuck near it (for we could not then come to reach it exactly) and deſcending to the ground where the ſtagnant water ſtood, we cauſed a ſtring to be let down, with a weight hanging at the end of it, which we applied to a mark that had been purpoſely made at that part of the metalline tube, which the ſuperficies of the ſtagnant water had reſted at, when the water was elevated to its full height: and the other end of the ſtring being by him that let it down applied to that part of the glaſs, as near as he could gueſs, where the upper part of the water reached, the weight was pulled up; and the length of the ſtring, and conſequently the height of the cylinder of water was meaſured, which amounted to 33 feet, and about 6 inches. Which done, I returned to my lodging, which was not far off, to look upon the baroſcope, to be informed of the preſent weight of the atmoſphere, which I found to be but moderate, the quickſilver ſtanding at 29 inches, and between 2 and 3 eighths of an inch. This being taken notice of, it was not difficult to compare the ſucceſs of the experiment with our hypotheſis. For if we ſuppoſe the moſt received proportion in bulk between cylinders of quickſilver and of water of the ſame weight, namely that of 1 to 14, the height

Vol. III. Pl. 5.



Vol. III. Pl. 6.



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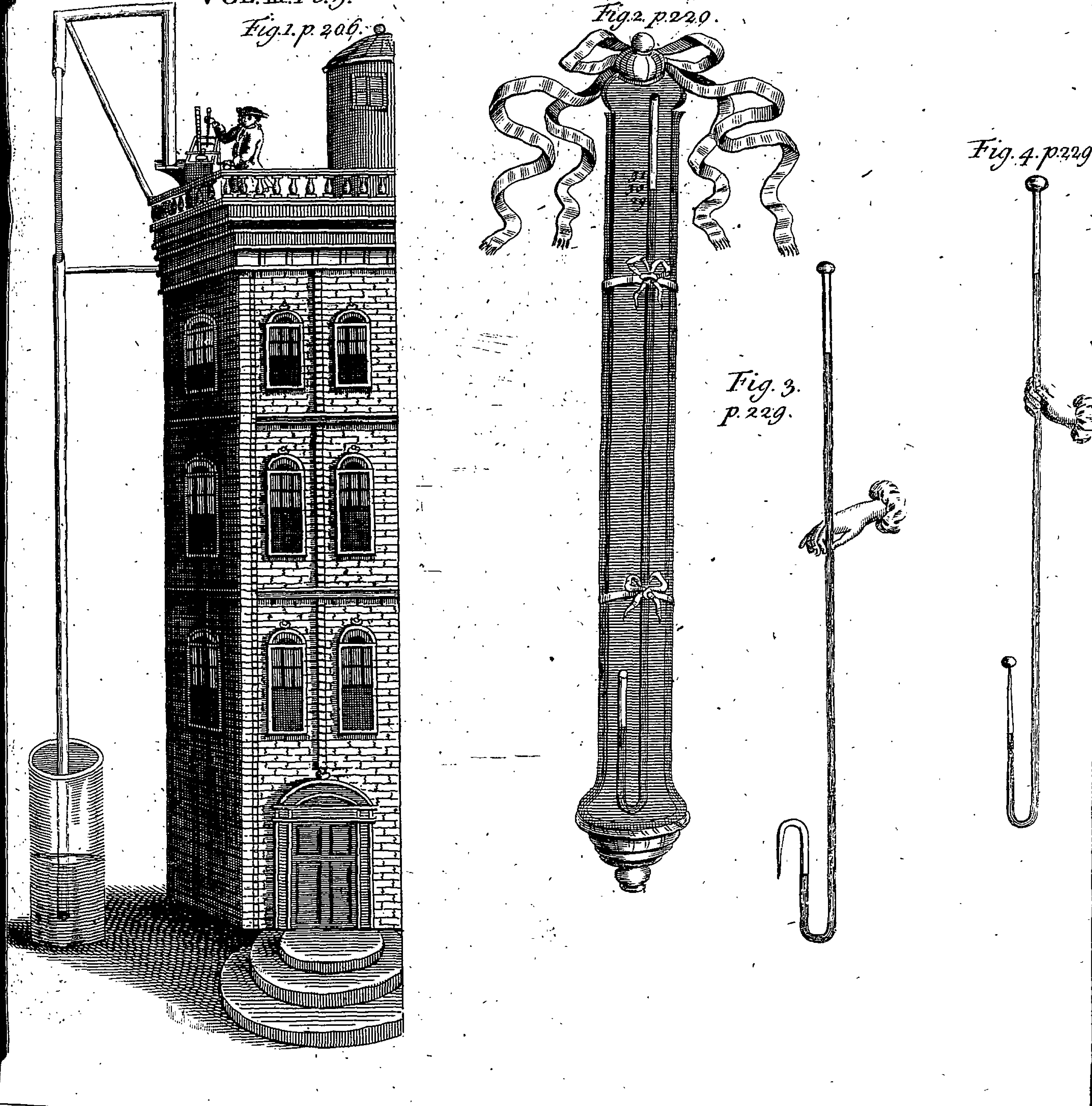

VOL. III. Pl. 6.

height of the water ought to have been 34 feet and about 2 inches, which is about 8 inches greater than we found it. But then your lordſhip may be pleaſed to remember that I formerly noted, before ever I made this experiment, that I did not allow the proportion betwixt mercury and water (at leaſt ſuch water as I made my trials with) to be altogether ſo great; and though in ordinary experiments we may with very little inconvenience make uſe of that proportion to avoid fractions, yet in ſo tall a cylinder of water as ours was, the difference is too conſiderable to be neglected. If therefore, inſtead of making an inch of quickſilver equivalent to 14 inches of water, we abate but a quarter of an inch, which is but a 56th part of the height of the water, this abatement being repeated 29 times and a quarter, will amount to 7 inches and above a quarter; which added to the former height of the water, namely 33 feet and 6 inches, will make up 34 feet and above an inch; ſo that the difference between the height of the mercury ſuſtained by the weight of the atmoſphere in the baroſcope, and that of the water raiſed and ſuſtained by the preſſure of the ſame atmoſphere in the long tube, did not appear to differ more than an inch or two from the proportion they ought to have had, according to the difference of their ſpecifick gravities. And though in our experiment the difference had been greater, provided it exceeded not 8 or 10 inches, it would not have been ſtrange; partly, becauſe of the difficulty of meaſuring all things ſo exactly in ſuch an experiment; partly, becauſe as waters are not all of the ſame weight, ſo a little diſparity of it in ſo long a cylinder may be conſiderable; and partly, and perhaps chiefly, becauſe the air flying out of the bubbles that roſe out of ſo great a quantity of water, and breaking at the top of it, and ſo near that of the tube, might, by its ſpring, though but very weak, aſſiſting the weight of ſo much water, ſomewhat (though not much) hinder the utmoſt elevation of that liquor. But our experiment did not make it needful for me to inſiſt on theſe conſiderations; and the inconſiderable difference that was betwixt the height of the water we found, and that which might have been wiſhed, did rather countenance than at all disfavour the thing to be made out by our experiment, ſince by no pumping we could raiſe the water quite ſo high (though I confeſs it wanted but very little) as the weight of the atmoſphere was able to keep up a cylinder of mercury proportionable to it in height, and equivalent in weight: and yet I preſume your lordſhip will eaſily grant that there was at leaſt as much care uſed in this experiment to keep the things about it tight, as has been wont to be uſed by tradeſmen in their pumps, where it is not ſo eaſy either to prevent a little inſinuation of the air, or to diſcern it.

It is not that I am ſure, that even all our care would have kept the water for any long time at its full height; but that the air was ſufficiently exhauſted for our purpoſe, when we determined the height of the water, I was induced to conclude by theſe circumſtances.

I. As well the conſtruction of the engine, as the many formerly related experiments that have been ſucceſsfully tried with it, ſhew, that it is not like it ſhould be inferior in cloſeneſs to the great water-pumps made by ordinary tradeſmen; and particularly the XIth experiment foregoing manifeſts, that by this pump quickſilver was raiſed to as great a height as the atmoſphere is able to ſupport in the Torricellian experiment.

II. The ſtanchneſs of the pipe appeared by the diminution (as to number) of bubbles that appeared at the top of the water, and by their ſize too; for when there was a leak (though but ſo very ſmall, that the water could not get out at in the tube) it might uſually be taken notice of by the attentive ear of him that ſtood to watch upon the ladder erected by the ſide of the tube; and the air that got in did eaſily diſcover itſelf to the eye by large bubbles, manifeſtly differing from thoſe that came from the aërial particles belonging

belonging to the water; and if the leak were not ſo very ſmall, the air that got in would ſuddenly lift up the water above it, and perhaps fill with it the deſcending p pe.

III. Though there had been ſome imperceptible leak, yet that would not have hindered the ſucceſs of the experiment for the main; for in leaks that have been but ſmall, though manifeſt enough, we have often, by cauſing the pump to be plied leſs nimbly than it now was, been able to proſecute our trials; becauſe the pump carried off ſtill more air than could get in at a leak that was no greater.

IV. And that little or no intruding air was left in the upper part of our tube, was evident by thoſe marks, whereby it was eaſy for them that are well acquainted with the pump, to eſtimate what air is left in the veſſel it ſhould exhauſt; and particularly towards the end of our operation I obſerved, that when the ſucker was depreſſed, there came out of the water that covered the pump ſo very few bubbles, that they might be imputed to the air afforded by the bubbles ſpringing from the water in the tube; whereas if any adventitious air had got into that cylinder of water, it would have appeared in the water that covered the pump.

V. Lastly, it were very ſtrange, that if the water was but caſually hindered by ſome leak from aſcending any higher, it ſhould be ſo eaſy to raiſe it to the very number of feet that our hypotheſis requires, and yet we ſhould be unable by obſtinate pumping to raiſe it one foot higher.

N. B. 1. As ſoon as we had made our experiment, and thereby found that what was requiſite to it was in order; I ſent to give notice of it to Dr. *Wallis* and Dr. *Wren*, as perſons whoſe curioſity makes them as well delighted with ſuch trials, as their deep knowledge makes them moſt competent judges of them. But before they could be found, and come, it being grown ſomewhat late and windy, I, that was not very well, and had tired myſelf with going up and down, could not ſtay with them ſo long as I intended, but leaving the reſt of the repeated experiments to be ſhewn them by *J. M.* (who had been very induſtrious in fitting and erecting the tube) they and their learned friend (whom they brought with them) Dr. *Millington* told me a while after, that they alſo had found the greateſt height to which they could raiſe the water to be 33 feet and an half.

2. When the water began firſt to appear in the glaſs, the bubbles would be, as I had foretold, exceeding numerous ſo as to make a froth of near a foot high, if the water were newly brought, and had never been raiſed in the tube before. But if the pumping were long continued, the number and height (or at leaſt one of the two) of the aggregate of bubbles would (as there remained fewer and fewer aërial particles in the water) be leſſer and leſſer; but their emerging did never, that I remember, wholly ceaſe.

3. At the beginning alſo there would appear great vibrations of the water in the upper part of the tube; the riſing and the falling amounting ſometimes to a foot, or near half a yard: but theſe grew leſſer and leſſer, as thoſe of the quickſilver in the Torricellian experiment uſe to do.

4. One may uſe an ordinary pail to hold the ſtagnant water; but we rather employed a veſſel of earth, made for another purpoſe, ſomewhat ſlender and of a cylindrical ſhape, becauſe in a narrow veſſel it is more eaſy to gueſs by the riſing and falling of the liquor, how the pump is plied, and to perceive even ſmaller leaks.

5. I must not forget to take notice, that though the newly named gentlemen came to me (when they had ſeen the experiment tried) within leſs than an hour after the time I had looked upon the baroſcope, and obſerved the quickſilver to ſtand ſomewhat beneath 29 inches and three eighths; yet, when preſently upon their return I conſulted

the ſame inſtrument again, the mercury appeared to be ſenſibly riſen, being ſomewhat (though but very little) above nine and twenty inches and three eighths; and five or ſix hours after (at bed-time) I found it to be yet more conſiderably riſen. Which may keep your lordſhip from wondering at what I intimated a little above, touching Monſieur *Paſchal*'s experiment, as well as touching the diſappointment of the pump-makers endeavours. For it is not only poſſible, that (as I have elſewhere noted) water may be raiſed in the ſame pump, though we ſuppoſe it ſtill equally ſtanch, higher at one time than at another: but it was contingent, that, in Monſieur *Paſchal*'s noble attempt to imitate the Torricellian experiment with water inſtead of quickſilver, the proportion betwixt the heights of thoſe two liquors in their reſpective tubes anſwered ſo well to their ſpecifick gravities; for the varying weight of the atmoſphere being not then, that appears, known, or conſequently taken into conſideration; if Monſieur *Paſchal*, having tried the Torricellian experiment when the air was, for inſtance, very heavy, had tried his own experiment when the atmoſphere had been as light as I have often enough obſerved it to be, he might have found his cylinder of water to have been half a yard or two feet ſhorter than the formerly meaſured height of the quickſilver would have required.

I HAVE now no more to add about this fifteenth experiment, but that it may ſerve for a ſufficient confirmation of what I note in another treatiſe, againſt thoſe hydraulical and pneumatical writers who pretend to teach ways of making water paſs by inflected pipes, and by the help of ſuction, from one ſide of a mountain to the other, be the mountain never ſo high. For if the water be to aſcend as it were ſpontaneouſly above 35 or 36 feet, a ſucking pump will not ordinarily, at leaſt here in *England*, be able to raiſe it.

AND now I ſpeak of mountains, it will not be altogether impertinent to add, that if it had not been for unſeaſonable weather, I had thought fit to make the foregoing eleventh experiment (of elevating mercury by ſuction) to be tried at the top of an hill not far from the place I then was at. For by what has been already delivered, it appears, that we might have eſtimated the height to which the water may be there elevated by ſuction, without repeating the experiment with a thirty-five feet tube (which we could not hope for conveniency to do) by the utmoſt height to which our engine could have raiſed mercury; and it may be of ſome uſe to be able from experiments to make ſome eſtimate (for it can ſcarce be an accurate one) how much it may be expected that pumps ſhall (*cæteris paribus*) loſe of their power of elevating water by ſuction, by being employed at the top of an hill, inſtead of being ſo at the bottom, or on a plain. Remembering always what I lately intimated, that even in the ſame place liquors will be brought to aſcend by ſuction to a greater or leſs height at one time than another, according to the varying gravity of the atmoſphere.

EXPERIMENT XVI.

About the bending of a ſpringy body in the exhauſted receiver.

THE cauſe of the motion of reſtitution in bodies, and conſequently of that which makes ſome of them ſpringy, which far the greater part of them are not, has been ingeniouſly attempted by ſome modern corpuſcularians, and eſpecially Carteſians. But ſince divers learned and judicious men do ſtill look upon the cauſe of elaſticity as a thing that needs to be yet farther inquired into; and becauſe I am not myſelf ſo well ſatisfied as to blame their curiosity, I held it not unfit to examine by the help of our engine their conjecture, who imagine that the air may have a great stroke in the making of bodies springy; and this I the rather did, because I had * elsewhere shewn that there is no need to assert, that in all bodies that have it, the elastical power flows immediately from the form, but that in divers of them it depends upon the mechanical structure of the body.

To make some trial, therefore, whether the air have any great interest in the motion of restitution, we took a piece of whalebone of a convenient bigness and length; and having fastened one end of it in a hole made in a thick and heavy trencher, to be placed on the plate of the engine, we tied to the other end a weight, whereby the whalebone was moderately bent, the weight reaching down so near to a body placed in a level position under it, that if the spring were but a little weakened, the weight must either lean upon, or at least touch the horizontal plain; or if on the other side the spring should grow sensibly stronger, it might be easily perceived by the distance of the weight, which was so near the plain, that a little increase of it must be visible.

This done, we conveyed these things into the receiver, and ordered those that pumped to shake it as little as they could, that the weight might not knock against the body that lay under it, or so shake it, as to hinder us from discerning whether or no it were depressed by the bare withdrawing of the air.

And when the air had been well pumped out I watched attentively, whether any notable change in the distance of the weight from the almost contiguous plain would be produced upon its being let in again; for the weight was then at rest, and the returning air flowing in much more speedily than it could before be drawn out, I thought this the likeliest time to discover whether the absence of the air had sensibly altered the spring of the whalebone: but though the experiment were made more than once, I could satisfy myself only in this, that the depression or elevation of the weight that was due to the true and mere change of the spring, was not very considerable, since I did not think myself sure that I perceived any at all: for though it be true that sometimes, when the receiver was well exhausted, the weight seemed to be a little depressed, yet that I thought was very little, if any thing more than what might be ascribed to the absence of the air, not considered as a body that had any thing to do directly with the spring, but as a body that had some (though but a little) weight; upon which account it made the medium, wherein the experiment was tried, contribute to support the weight that bent the spring; which weight, when the air was absent, must (being now in a lighter medium) have its gravitation increased by as much weight as a quantity of the exhausted air, equal to it in bulk, could amount to: but this experiment being tried only with whalebone, and in a receiver not very great, may deserve to be further tried in taller glasses, with springs of other kinds, and by the motions of a watch, and other more artificial contrivances.

* In notes about the history of Elasticity.

EXPE-

EXPERIMENT XVII.

About the making of mercurial and other gages, whereby to eſtimate how the receiver is exhauſted.

Because the air being inviſible, it is not always eaſy to know whether it be ſufficiently pumped out of the receiver that was to be exhauſted, we thought it would be very convenient to have ſome inſtrument within the receiver that might ſerve for a gage or ſtandard, whereby to judge whether or no it were ſufficiently exhauſted.

To this purpoſe divers expedients were thought on, and ſome of them put in practice; which, though not equally commodious, may yet all of them be uſefully employed, one on this occaſion, and another on that.

The firſt (if I miſremember not) that I propoſed was a bladder (which may be greater or leſs, according to the ſize of the veſſel it is to ſerve for) to be very ſtrongly tied at the neck, after having had only ſo much air left in the folds of it as may ſerve to blow up the bladder to its full dimenſions, when the receiver is very well exhauſted, and not before. But though your lordſhip will hereafter find that I yet make uſe of ſmall bladders on certain occaſions, in which they are peculiarly convenient, yet in many caſes they do, when the glaſſes are well exhauſted, take up too much room in them, and hinder the objects included in the receiver from being obſerved from all the ſides of it.

Another ſort of gage was made with quickſilver poured into a very ſhort pipe, which was afterwards inverted into a little glaſs of ſtagnant quickſilver, according to the manner of the Torricellian experiment. For this pipe being but a very few inches long the mercury in it would not begin to deſcend till a very great proportion of air was pumped out of the receiver; becauſe till then the ſpring of the remaining air would be ſtrong enough to be able to keep up ſo ſhort a cylinder of mercury. And this kind of gage is no bad one. But becauſe, to omit ſome other little inconveniencies, it cannot eaſily be ſuſpended (which in divers experiments it is fit the gage ſhould be) and the mercury in it is apt to be too much ſhaken by the motion of the engine, there was another kind of gage by ſome ingenious man (whoever he were) ſubſtituted in its place, conſiſting of a kind of ſiphon whoſe ſhorter leg hath belonging to it a large bubble of glaſs, moſt commonly made uſe of at an illuſtrious meeting of virtuoſi; where your lordſhip having ſeen it, I ſhall not need to deſcribe it more particularly.

But none of the gages I had formerly uſed, nor even this laſt, having the conveniences that ſome of my experiments require, I was fain to deviſe another, which is that I moſt make uſe of, as having advantages, ſome or other of which each of the gages already mentioned wants; for even that with ſpirit of wine, not to mention leſſer diſadvantages, hath a bubble too great to let it be uſeful in veſſels ſo ſlender, as for ſome purpoſes I divers times employ; and this ſhort cylinder of ſo light a liquor as ſpirit of wine makes the ſubſidence of the liquor be indeed a good ſign, that the receiver is well exhauſted, but gives us not an account what quantity of air may be in the receiver, till it be arrived at that great meaſure of rarefaction; and the ſame liquor, being upon a very ſmall leak (ſuch as would not be prejudicial to many experiments) impelled up to the top of the gage, we cannot afterwards by this inſtrument take any meaſure of the air that gets in at the leak: but now there are divers experiments where I deſire to ſee the phænomena that will happen, not only (or perhaps not at all) upon the uttermoſt exhaustion of the air, but when the pressure of it is withdrawn to such or such a measure, and also when the air is gradually re-admitted.

See Plate III. Fig. 4. To make the gage we are speaking of, take a very slender and cylindrical pipe of glass of six, eight, ten, or more inches in length, and not so big as a goose-quill (but such as we employ for the stems of sealed weather-glasses) and having at the flame of a lamp melted it, but not too near the middle, to make of it by bending it a siphon, whose two legs are to be not only parallel to one another, but as little distant any where from one another as conveniently may be. In one (which is usually the longer of these legs, there is to be left at the top either half an inch or a whole inch, or more or less than either (according to the length of the gage or the scope of the experimenter) of air in its natural state, neither rarefied nor condensed; the rest of the longer leg, and as great a part of the shorter as shall be thought fit, being to be filled with quicksilver. This done there may be marks placed at the outside of the longer or sealed leg, whereby to measure the expansion of the air included in the same leg; and these marks may be either little glass knobs about the bigness of pins heads, fastened by the help of a lamp at certain distances to the longer leg of the siphon, or else the divisions of an inch made on a list of paper and pasted on, either to the siphon itself, or to the slender frame which on some occasions we fasten the gage to.

This instrument being conveyed into a receiver (which for expedition sake we choose as small as will serve the turn) the air is to be very diligently pumped out, and then notice is to be taken to what part of the gage the mercury is depressed, that we may know, when we shall afterwards see the mercury driven so far, that the receiver the gage is placed in, is well exhausted. And if it be much desired to know more accurately (for one may arrive pretty near the truth by guess) what stations of the mercury in the gage are answerable to the degrees of the rarefaction of the air in the receiver, that may be compassed either by calculation (which is not so easy, and supposes some hypotheses) or, though not without some trouble, by letting in the water as often as is necessary into a receiver, whose entire capacity is first measured, and in which there may be marks made to shew, when the water to be let in shall fill a fourth part, or half, or three quarters, &c. of the cavity. For if (for instance) when the quicksilver in the gage is depressed to such a mark you let in the water, and that liquor appears to fill a fourth part of the receiver, you may conclude that about a fourth part of the air was pumped out, or that a fourth part of the spring that the whole included air had was lost by the exhaustion, when the quicksilver in the gage was at the mark above-mentioned. And if the admitted water do considerably either fall short of, or exceed the quantity you expected, you may the next time let in the water either after the mercury has a little passed the former mark, or a little before it is arrived at it. And when once you have this way obtained one pretty long and accurate gage, you will not need to take so much pains to make others, since you may divide them by the help of that one; for this being placed with any other in a small receiver, when the mercury in the standard-gage (if I may so call it) is depressed to any of the determinate divisions obtained by observation, you may thence conclude how much the air in the receiver is rarefied, and consequently by taking notice of the place where the mercury rests in the other gage, you may determine what degree of exhaustion in a receiver is denoted by that station of the mercury in this gage.

Perhaps I need not tell your lordship that the ground of this contrivance was, that whereas in divers other gages, when the pump came to be obstinately plied, the expansion of the included air would be so great, that it would either drive out the liquor, especially if it were light, or in part make an escape through it; I judged that in such an instrument as that newly described, those inconveniencies would be avoided, because that

that the more the air ſhould come to be dilated, the greater weight of quickſilver it would in the ſhorter leg have to raiſe, which would ſufficiently hinder it from making that heavy liquor run over; and the ſame ponderouſneſs of the liquor, together with the ſlenderneſs of the pipe, would likewiſe hinder the included air from getting through in bubbles.

N. B. 1. For moſt experiments where exact meaſures are not required, it will not be ſo neceſſary to mark the gage at any other ſtation of the quickſilver than that which it is brought to by the exhauſtion of the receiver; for by that alone we may know, when the air is well pumped out of the receiver wherein the gage is included: and when one is a little uſed to ſome particular gage, one may by the ſubſidence of the mercury gueſs at the degree of the air's rarefaction, ſo near as may ſerve the turn in ſuch experiments. But when this inſtrument is to be uſed about nice trials, where it may be thought requiſite to have it divided according to one of the ways formerly propoſed, it will on divers occaſions be more ſecure (in caſe the maker of the gage has ſkill to do it) to put to the diviſions rather by little knobs of glaſs than by paper; becauſe this will on ſuch occaſions be in danger either to be rubbed off or wetted. And if glaſs-marks be uſed, it will be convenient that every fifth, or tenth, or ſuch ordinal number as ſhall be judged fit, be made of glaſs of a differing colour, for diſtinction ſake, and the more eaſy reckoning: we ſometimes for a need apply, inſtead of theſe glaſs-knobs, little marks of hard ſealing-wax, which will not be injured by moiſture, as thoſe papers will that are paſted on; but theſe of wax, though in many caſes uſeful, are not comparable to the other in all; ſince if they be very ſmall, they are eaſily rubbed off; and if large, they make not the diviſion exact enough, and often hide the true place of the quickſilver.

2. I shall here, about the mercurial gages, add only this hint, that what I propoſed to myſelf in that contrivance was not only to eſtimate the air pumped out of the receiver, or that remaining in it; but alſo, by the help of this inſtrument (as elſewhere by another experiment) to meaſure (ſomewhat near) the ſtrength of the ſpring of rarefied air, according to its ſeveral degrees of rarefaction; and by this obſervation, in concurrence with other things, I hoped we might (according to what I have elſewhere inſinuated) be aſſiſted to eſtimate, by the cylinder of mercury raiſed in the open leg, the expanſion of the air included in the ſealed leg: but of theſe things I deſigned in this place to give but an intimation.

3. That leg of the gage that includes the air, may be ſealed up either at the begining, before the pipe be bent into a ſiphon, or (which is much better) after the following manner. Before you bend the pipe, draw out the end of it, which you mean to ſeal, to a ſhort and very ſlender thread; then having made the pipe a ſiphon, pour into the leg, which is to remain open, as much quickſilver as you ſhall judge convenient, which will riſe to an equal height in the other leg; out of which, by gently inclining the ſiphon, you may pour out the ſuperfluous mercury (if there be any) and when you ſee that there is an inch, or half an inch (or what part you deſigned to leave for air) unfilled with mercury next to the end that is to be cloſed, and that the reſt of that leg, and as much (as you think fit) of the other is full of quickſilver, you may, by keeping the ſiphon in the ſame poſture, and warily applying the ſlender apex above-mentioned to the upper part of the flame of a lamp, blown horizontal, eaſily ſeal up that apex without cracking or prejudicing the open leg, or conſiderably injuring the air-hole that was to be ſealed up in the other. And this ſealing of one leg muſt (as it is evident) keep the mercury ſuſpended in it, though it be higher by divers inches than that in the open leg, till the withdrawing of the external air enable the included, by expanding itſelf, to depreſs the mercury in the ſealed leg, and raiſe it in the open.

4. How

4. How the length of these mercurial gages is to be varied, according to the bigness and shape of the slender receivers they are to be employed in; and how they may easily be made either to stand upright at the bottom of the receiver, or be kept hanging in the middle, or near the top of it (as occasion may require); and how the open end may be made to secure the mercury, in cases where that is needful, belongs not so properly to this treatise as to the second part of the Continuation; where, if ever I trouble your lordship with it, the usefulness of this sort of gages, and the circumstances that may advantage them, will best appear.

5. There being some experiments, wherein it is not desired that the receiver should be near exhausted, but rather that the degrees of the air's rarefaction, which ought not to be very great, should be well measured; we may in such cases make use of gages shaped like those hitherto described, but made as long as the receiver will well admit, and furnished instead of quicksilver either with spirit of wine coloured with cochineal, or else with the tincture of red rose-leaves, drawn only with common water made sharp by a little either of the oil, or spirit of vitriol, or of common salt. For the lightness of these liquors in comparison of quicksilver will allow the expansions of the air included in the gage to be very manifest and notable enough, though not half or perhaps a quarter of the air be pumped out of the receiver.

6. You may also in such cases as these, where the receiver is large enough, and is not to be quite exhausted, make use of a mercurial gage, differing from those above described only in this, that the shorter leg need not to be above an inch or half an inch long before it expand itself into a bubble of about half an inch or an inch in diameter, and having at the upper part a very short and slender unsealed pipe, at which the air may get in and out; by which contrivance you may have this convenience, that you need not include so much air, as otherwise would be requisite, at the top of the longer leg, because the mercury in the shorter cannot, by reason of the breadth of the bubble, whereinto the expansion of the air drives it, be considerably raised; upon which account it becomes more easy to estimate by the eye the degrees of the included air's rarefaction, which may be done almost as easily as if there were water instead of mercury, provided it be remembered that quicksilver, by reason of its ponderousness, does far more assist the dilatation of the air, than so much water would do.

EXPERIMENT XVIII.

About an easy way to make the pressure of the air sensible to the touch of those that doubt of it.

Though several of our experiments sufficiently manifest to the skilful that the pressure of the air is very considerable; yet, because some of them require peculiar glasses and other instruments, which are not always at hand, and because there are many that think it surer to estimate the force of pressure by what they immediately feel than by any other way, I was invited for the sake of such to employ an easy experiment, which usually proved convincing, because it operated on that sense whereon they chiefly relied.

I caused then to be made a hollow (but strong) piece of brass, not above two or three inches high (that it might be in a trice exhausted) and open at both ends, whose orifices were circular and parallel, but not equal (the instrument being made tapering, so that it might be represented by an excavated *conus truncatus*, or a gigg, with the lower part

part cut tranſverſely off. This piece of braſs being cemented on, as if it were a ſmall receiver to the engine, the perſon, that would not believe the preſſure of the air to be near ſo conſiderable as was repreſented, was bidden to lay the palm of his hand upon the upper orifice; and being ordered to lean a little upon it, that ſo the lower part of his hand might prove a cloſe cover to the orifice, one exſuction of the air was made by the help of the pump; and then upon the withdrawing of the greateſt part of the preſſure of the internal air, that before counterbalanced that of the external, the hand being left alone to ſupport the weight of the ambient air, would be preſſed inwards ſo forcibly, that though the ſtronger ſort of men were able (though not without much ado) to take off their hands, yet the weaker ſort of triers could not do it, eſpecially if by a ſecond ſuck the little receiver were better exhauſted, but were fain to ſtay for the return of the air into the receiver to aſſiſt them.

THIS experiment being deſigned rather to convince than to puniſh thoſe that were to make it, we took care, not only that the braſs ſhould be ſo thick and the orifices ſo ſmooth, that no ſharpneſs nor roughneſs of the metal ſhould offend the hand; but alſo that the narrower orifice (which was the ofteneſt made uſe of) ſhould be but about an inch and a quarter in diameter. But if any were deſirous of a more ſenſible conviction, it was very eaſy to give it him by making the larger orifice the uppermoſt, which was the reaſon why the inſtrument was, as we formerly noted, made tapering; but yet this larger orifice ought not to exceed two inches, or two inches and a half in wideneſs, leſt the great weight of the air endanger the breaking or conſiderably hurting the hand of the experimenter: which caution I am put in mind of giving, by remembering that I once much endangered my own hand, through the miſtake of him that managed the pump, who, unawares to me, ſet it on work, when for another purpoſe I had laid my hand upon the orifice of an inſtrument of too great a diameter.

The famous experiment of Torricellius, *mentioned in the 17th of our already publiſhed trials, is of that nobleneſs and importance, that though divers learned men have (but upon very differing principles) diſcourſed of it in print, which gives me the leſs mind to inſiſt long upon it here, yet I ſhall not ſcruple to ſubjoin ſome notes concerning trials that I made (though for want of opportunity I could not repeat them according to my cuſtom) which I had not met with in others, and which may ſerve to confirm the hypotheſis made uſe of in this Continuation and the treatiſe it belongs to.*

EXPERIMENT XIX.

About the ſubſidence of mercury in the tube of the Torricellian experiment to the level of the ſtagnant mercury.

A BAROSCOPE being included in a receiver made of a long bolt head with the lower part of the ball cut circularly off, upon the firſt exſuction of the air the quickſilver that before ſtood at 29 inches (the atmoſphere appearing then by a conſtant baroſcope very light) would fall ſo low as to reſt at 9 or 10 inches (for once I meaſured the ſubſidence beneath its former elevation) and in about three ſucks more it would be brought quite down to the level of the ſtagnant quickſilver, and ſomewhat below (as it is the property of quickſilver, quite contrary to water, to riſe leſs in a ſlender pipe than in a wide).

The

The air being let into the receiver, the quickſilver would be impelled up ſlowlier or faſter, as we pleaſed, to the former height of 29 inches or thereabouts.

N. B. That if the air were ſuffered to go haſtily out of the receiver, the mercury would, by virtue of the accelerated motion acquired in its deſcent, at the very firſt ſuck deſcend till it reached within an inch or two of the ſtagnant mercury, though it would preſently after a few riſings and fallings ſettle at the height of 9 or 10 inches, till the next brought it down lower.

2. If when the mercury was reimpelled up to its due height, thoſe that managed the pump did, inſtead of rarefying the air, a little compreſs it, the quickſilver would by the compreſſed air be eaſily made to riſe an inch or more above the former ſtandard of 29 inches; which circumſtance I mention, not as a new thing, but to confirm (what ſome think ſtrange) a paſſage printed in *New Experiments*, Exper. XVII. where I mention, that if the air in the receiver, inſtead of being rarefied in the engine, were a little compreſſed by it, the preſſure of the included air being ſomewhat increaſed by having its ſpring thus bent, would ſuſtain the mercury in the Torricellian tube at a greater than the wonted height.

And to confirm another paſſage in the ſame page, where I obſerved, that if the preſſure of the air upon the ſtagnant mercury be not ſo great as it is wont to be, the mercury will begin to ſubſide in a (filled and inverted tube, which wants of the uſual height; we took a glaſs cane (ſealed at one end) much ſhorter than the due length, and having filled it with mercury, and inverted it into a glaſs full of ſtagnant mercury, we placed all in the former receiver; where the mercurial cylinder, for want of the requiſite height, remained totally ſuſpended, but upon the firſt or ſecond ſuck it would ſubſide, and in two or three ſucks more it would fall to the level of the ſtagnant mercury, or a little below it. Upon the letting in of the air it would be impelled to the very top of the tube, bating an aërial bubble, which ſeemed to come from the mercury itſelf, and was ſo little as not to be at all diſcerinble, ſave to a very attentive eye.

Experiment XVII. This experiment I ſhould not think fit here to relate, ſince I formerly acquainted your lordſhip with the ſubſidence of the mercury upon the withdrawing of the air from the receiver, were it not that, in the mention of that trial, I remember I confeſſed to you that I could not ſo free the great receiver I then uſed from air, but that the little that remained or leaked in, made me unable to bring the mercury in the tube totally to ſubſide or fall much nearer than within an inch of the ſurface of the ſtagnant mercury, with which in our preſent trials that in the tube was brought to a level.

EXPERIMENT XX.

Shewing that in tubes open at both ends, when no fuga vacui *can be pretended, the weight of water will raiſe quickſilver no higher in ſlender than in large pipes.*

Because I find it, even by learned and very late writers, urged as a clear and cogent argument againſt thoſe that aſcribe the phænomena of the Torricellian experiment to the weight of the external air; that it is impoſſible that the air, though it were granted to be a heavy body, could ſuſtain the quickſilver at the ſame height in tubes of very differing bigneſs, ſince the ſame air cannot equally counterpoiſe mercurial cylinders of ſuch unequal weights; and becauſe this objection is wont very much to puzzle thoſe that are not well acquainted with the hydroſtaticks, I preſume your lordſhip will allow me, till I can ſhew you ſome hydroſtatical papers, by which the objection may appear

to

to be but ill grounded upon the true theorems of that art, to annex the tranſcripts of a couple of experiments (that I once made to remove this ſuppoſedly inſuperable difficulty) juſt as I find them regiſtered in my note-books.

The First Trial, Sept. 2, 1662.

We took a very large glaſs-tube, hermetically ſealed at one end, and about two feet and a half in length; into this we poured quickſilver to the height of three or four fingers; then we took a couple of cylindrical pipes of very unequal ſizes, the wider being as big again as the ſlenderer, and open at both ends. The lower ends of theſe two pipes we thruſt into the quickſilver, and faſtened them near their upper ends to the tube with ſtrings, that they might not be lifted up nor moved out of their poſture, in which the convex ſurface of the mercury in both the pipes ſeemed to lie almoſt in a level, the tube alſo itſelf being placed upright in a frame. This done, by the help of a funnel we poured in water by degrees at the top of the tube, and obſerved, that as the water gravitated more and more upon the ſtagnant mercury, ſo the included mercury roſe equally in both the pipes, until the tube being almoſt filled with water, the mercury appeared to be impelled up to, and ſuſtained at as great a height in the big tube as in the leſſer, being in either raiſed about two inches above the ſurface of the ſtagnant quickſilver.

N. B. 1. Having cauſed about half the water (having no conveniency to withdraw any more) in the tube to be ſucked out at the top, we obſerved the quickſilver in both the tubes to ſubſide uniformly, and to re-aſcend alike upon the re-affuſion of the water.

2. We endeavoured to try the experiment (for their ſake, who have not the conveniency to have ſuch tubes purpoſely made) in a wooden veſſel, into which, when it was filled with water, we let down a flat glaſs furniſhed with ſtagnant mercury, whereinto the ends of the two pipes were immerſed. But the opacouſneſs of the cylinder (which reduced us to ſee only from the top the reflection of the ſtagnant mercury) and other impediments, diſabled us to perceive the motions and ſtations of the mercury in the pipe, though we once made uſe of a candle the better to diſcern them.

The Second Trial.

We took a very wide tube of glaſs, of about a foot long, and into it poured a convenient quantity of quickſilver; we took alſo two pipes of about equal length, and of that diſparity in bigneſs that we newly mentioned (thoſe pipes lately deſcribed being indeed cut off from theſe we are now to ſpeak of) and theſe being filled with quickſilver, after the manner of the Torricellian experiment, were by a certain contrivance let down into the tube, and unſtopped under the ſurface of the ſtagnant mercury, and then the quickſilver in the pipes falling down to its wonted ſtation, and reſting there, we poured into the tube about a foot height (by gueſs) of water, whereupon the quickſilver, as it before ſtood, as it were, in a level in both the pipes, ſo it was, for aught appeared to us, equally impelled up beyond its wonted ſtation, and ſuſtained there, both in the ſlender and in the bigger pipe, and upon the withdrawing of ſome of the water it began to ſubſide alike, as to ſenſe, in them both, falling no lower in the bigger than in the ſlenderer. And water being a ſecond time poured down into the tube, the mercury did in both pipes riſe uniformly as before. By which and the former experiment it ſufficiently appeared that a gravitating liquor, as air or water, may impel or keep up mercury to the ſame height in tubes that are of very differing capacities; and that liquors balance each other according to their altitude, and not barely according to their weight. For in this laſt experiment, the additional cylinder of one inch of mercury was manifeſtly raiſed and kept up by the water incumbent on the ſtagnant mercury, the other cauſe, whatever it were, of the mercury's ſuſpenſion, being able to ſuſtain but a cylinder ſhorter by an inch. And the ſame parcel of water did counterpoiſe in the differing pipes two mercurial cylinders, which though but of the ſame altitude (namely about an inch) were of very unequal weight.

EXPERIMENT XXI.

Of the heights at which pure mercury, and mercury amalgamed with tin, will ſtand in barometers.

Considering with myſelf, that if the ſuſtentation of the quickſilver in the Torricellian experiment at a certain height depends upon the æquilibrium, which a liquor of that ſpecifick gravity does at ſuch a height attain to with the external air; if that peculiar and determinate gravity of the quickſilver be altered, the height of it, requiſite to an æquilibrium with the atmoſphere, muſt be altered too; conſidering this, I ſay, I thought it might ſomewhat confirm the hypotheſis hitherto made uſe of, if a phænomenon ſo agreeable to it were actually exhibited. This I ſuppoſed performable two differing ways, namely, by mixing, or as chymiſts ſpeak, amalgamating mercury either with gold, to make it a mixture more heavy, or with ſome other metal, that might make it more light than mercury alone is. But the former of thoſe two ways I forbore to proſecute, being, where I then was, unfurniſhed with a ſufficient quantity of refined gold, for that which is coined is generally allayed with ſilver, or copper, or both; and therefore amalgamating mercury with a convenient proportion of pure tin, or, as the tradeſmen call it, block-tin, that the mixture might not be too thick to be readily poured out into a glaſs-tube, and to ſubſide in it, we filled with this amalgam a cylindrical pipe ſealed at one end, and of a fit length, and then inverted it into a little glaſs furniſhed with the like mixture. Of which trial the event was, that the amalgam did not fall down to 29, nor even to 30 inches, but ſtopped at 31 above the ſurface of the ſtagnant mixture.

N. B. 1. That though one may expect that the event of the experiment would be the more conſiderable, the greater the quantity is that is mingled of the light metal, yet care muſt be taken that the amalgam be not made too thick, leſt part of it ſtick here and there (as we did to our trouble find it apt to do) to the inſide of the pipe, by which means ſome aërial corpuſcles will meet with ſuch convenient receptacles, as to make it very difficult, if not almoſt impoſſible, to free the tube quite from air.

2. It may perhaps be worth while to try, whether by comparing the height of the amalgam to what it ought to be upon the ſcore of the ſpecifick gravities of the mercury, and the tin, mingled in a known proportion in the amalgam, any diſcovery may be made, whether thoſe two metals do penetrate one another after ſuch a manner (for there is no ſtrict penetration of dimenſions among bodies) as copper and tin have, as I elſewhere note, been by ſome chemiſts obſerved to do, when being melted down together, they make up a more cloſe and ſpecifically ponderous body than their reſpective weights ſeemed to require.

3. That

3. THAT by comparing this 21ſt experiment with the 18th of thoſe formerly publiſhed it may appear, that the height of the liquor, ſuſpended in the Torricellian experiment, depends ſo much upon its æquilibrium with the outward air, that it may be varied by a change of gravity in either of the two bodies that counterbalance each other, whether the change be of weight in the atmoſphere, or of ſpecifick gravity in the ſuſpended liquor.

ADVERTISEMENT.

I SHOULD here acquaint your lordſhip with what I have ſince tried, in reference to the 18th of the printed experiments, where I mention that I obſerved by long keeping the ſame inſtrument, with which I once made the Torricellian experiment, in the ſame place, that the height of the ſuſpended mercury would vary according as the weight of the atmoſphere happened to change. But though about the barometer (as others have by their imitation allowed me to call the inſtrument hitherto mentioned, put into a frame) I made in the year 1660 ſeveral obſervations that would not perhaps be impertinent in this place, yet having long ſince left them with a friend who lives far off, and not having them now in my power, I muſt beg your lordſhip's permiſſion to reſerve them for a part of the appendix, which I doubt I ſhall be engaged to add to this epiſtle; and in the mean time I ſhall not forbear to preſent your lordſhip thoſe other papers that I have by me, relating to the barometer; ſome of which will, I preſume, ſufficiently confirm my lately mentioned conjecture about the cauſe of the variation obſerved in the height of the ſuſpended mercury.

EXPERIMENT XXII.

Wherein is propoſed a way of making barometers that may be tranſported even to diſtant countries.

THINKING it a deſireable thing (as I have elſewhere intimated) to be able to compare together, by the help of barometers, the weight of the atmoſphere at the ſame time, not only in differing parts of the ſame country, as of *England*, but in differing regions of the world; I could not but foreſee that it would be very difficult to accompliſh my deſire without altering the form of the barometers I had hitherto made uſe of. For as theſe be unfit to be tranſported far, becauſe that ſtagnant mercury would be ſo apt to ſpill; ſo the procuring them to be made in the places where they are to be uſed, though it be no bad expedient, and ſuch as I have divers times made uſe of, is liable to this inconvenience, that, beſides that few will take the pains, and have the ſkill requiſite to make baroſcopes well, though they be ſufficiently furniſhed with glaſſes and mercury for that purpoſe; beſides this, I ſay, except men be more than ordinary diligent and ſkilful (and perhaps though they be) it will be very difficult to be ſure, that the baroſcope newly made in a remote country is as good (and but as good) as that which a man makes uſe of in this; in regard that at the making of the former, they are ſuppoſed to have no other baroſcope to compare it with; and to be ſure they have not the ſame with which it is to be compared here.

BEING by theſe conſiderations invited to attempt the making of portable or traveling baroſcopes (if I may ſo call them) I thought it requiſite to endeavour theſe three things: the firft, to make the veffel that fhould contain both the fuftained and the ftagnant mercury, all of one piece of glafs, of a like bignefs; the next, to place this veffel when filled, in fuch a frame as may be eafy to be tranfported, and yet in a reafonable meafure defend the glafs from external violence, no part of it ftanding quite out of the frame, as in all other barofcopes; and the third, fo to order the veffel, that it may not be fubject to be eafily broken by the violent motion of the mercury contained in it.

The firft of thefe will not feem practicable to thofe that imagine (without any warrant from the hydroftaticks) that it is as well neceffary as ufual, that the ftagnant mercury fhould have a veffel much wider than the tube, wherein the mercurial cylinder is fuftained; but to us the difficulty feemed much lefs to make the glafs part of our tube of one piece, and of a convenient fhape, than afterwards to fill it.

But to do both, we took a glafs cylinder fealed at one end, and of a convenient length (as about four or five feet) and caufed it by the flame of a lamp to be fo bent, that to thofe that did not take notice it was fealed at one end, it feemed to be a fiphon of very unequal legs, the one being three or four times longer than the other; by virtue of which figure the fhorter leg may ferve inftead of the diftinct veffel ufually employed to contain the ftagnant mercury. To fill this, which is not eafy, one may proceed after this manner: take a fmall funnel of glafs with a long and flender fhank, fo that it may reach three or four inches, or further, into the fhorter leg of our barometrical fiphon (if I may fo call it) and by this funnel pour into this fhorter leg as much mercury as may reach about two or three inches in both legs; then ftopping the orifice with your finger, and flowly inclining the tube, the mercury in the longer leg will gently fall to the fealed end; and the air that was there before, will pafs by it, and fo make it room. The mercury in the fhorter leg (which leg ought to be held uppermoft) will by the fame inclination of the tube fall towards the orifice; but being by the finger that ftops that, kept from falling out, if you do flowly re-erect the glafs, and then make it ftoop again as much as before, the mercury will pafs out of the fhorter leg into the longer, and join with that which was there before; and if all the mercury do not fo pafs, the orifice is to be ftopped again with your finger, and the tube inclined as formerly. This done, the tube is to be erected, and by the help of the funnel more mercury is to be poured in, and the foregoing procefs of ftopping the orifice, inclining the tube, &c. is to be repeated, till all the mercury poured into the fhorter leg be brought to join with that in the longer; and then the open leg is to be furnifhed with frefh mercury, obferving this, that the nearer the longer leg comes to the being filled, the lefs you muft raife it from time to time, when you pour mercury into the fhorter; as alfo that when you fee the longer leg quite full of mercury (though there be but little in the fhorter) you need not pour in any more, if the longer do much exceed a yard; becaufe upon the reftoring of the tube to an erected pofture, there will fubfide from the taller leg into the other a pretty quantity of mercury, by reafon of the fpace at the fealed end, which will be deferted by the mercury that was there. But becaufe it is difficult by this way, as well as by that practifed already, to fill a tube with mercury without leaving any vifible bubbles; to free it from fuch (if any happen to be) you muft once more ftop the orifice with your finger, and incline and re-erect the tube divers times, till you have thereby brought moft of the fmaller bubbles into one greater (which you may if you pleafe increafe, by letting in a little air); for by making this great bubble pafs leifurely two or three times from one end of the tube to the other, it will in its paffage as it were lick up all the fmall bubbles, and unite them to itfelf; which may

may afterwards by one inclination more of the tube be made to paſs into the ſhorter leg, and thence into the free air.

But there is another ſort of funnels, which if one have the ſkill and conveniency to make (as *I. M.* eaſily doth) one may very expeditiouſly fill the bended tubes of our portable barometers. For if you make the ſlender part of the funnel not ſtraight, but bended in the form of an obtuſe angle, and of ſuch a length, that the part which is to go into the ſhorter leg of our ſiphon, may reach to the flexure (of the ſiphon) then you may, by ſo holding the tube that the ſealed end be ſomewhat lower than the other, and by pouring in mercury at the obtuſe end of the angular funnel, eaſily make it run over the flexure into the longer leg of the ſiphon; provided you do now and then, as occaſion requires, erect a little and ſhake the tube, to help the mercury to get by the air, and expel it.

By ſuch ways as theſe we have found by experience that it is poſſible (though not eaſy) to do in ſuch a bended glaſs as our purpoſe requires, what, beſides a very late learned writer, the diligent *Merſennus* himſelf admoniſhes his reader, that it is not a practicable thing to do in the ordinary glaſſes of the Torricellian experiment, viz. to free the mercury of a ſtraight tube from air and bubbles, ſo as to be able by inclining the glaſs to make the liquor aſcend to the very top.

The firſt of our three above-mentioned ſcopes being thus attained, it was not difficult to compaſs the ſecond by the help of a ſolid piece of wood, which is to be ſomewhat longer than the tube, and a good deal broader in the lower part than in the upper, that it may receive the ſhorter leg of the ſiphon. In ſuch a piece of wood, which was about an inch thick, we cauſed to be made a gutter or channel of ſuch a depth and ſhape, that our ſiphon might be placed in it ſo deep, that a flat piece of wood (like a plained lath) might be laid upon it, without at all preſſing upon, or ſo much as touching the glaſs; ſo that this piece of wood may ſerve for a cover to defend the glaſs, to be put on when the inſtrument is to be tranſported, and taken off again, when it is to be hung up to make obſervations with; the channel piece of wood ſerving both for a part of a caſe, and for an entire frame; which may for ſome uſes be a little more commodious if the cover be joined (as it may eaſily be) to the reſt of the frame, by two or three little hinges and a haſp, by whoſe help the caſe may be readily opened and ſhut at pleaſure.

The third thing we propoſed to ourſelves is nothing near ſo eaſy as the ſecond; nor have we yet had opportunity to try, whether the way we made uſe of will hold, if the barometer be tranſported into very remote parts, though by ſmaller removes we found cauſe to hope that it will ſucceed in greater.

The grand difficulty to be obviated was this; that though it were eaſy to hinder the ſpilling of the mercury, by ſtopping the orifice of the ſhorter leg of our ſiphon, yet that would not ſerve the turn; for the upper part of the tube being deſtitute of air, if the mercury be by the motion of the inſtrument put to vibrate, it will be apt (for want of meeting with any air in the upper part of the tube to check its motions) to hit ſo violently againſt the top of the glaſs as to beat it out, or to crack ſome of the neighbouring parts.

To obviate this great inconvenience, our way is to incline the tube till the mercury be impelled to the very top of it, and yet there will remain a competent quantity in the ſhorter leg of the glaſs, if that be not at firſt made too ſhort. This done, the remaining part of the ſhorter leg is to be quite filled up either with water or mercury, and the orifice of it is to be very carefully and firmly ſtopped, for which purpoſe we uſe our ſtrong black cement; for by this means the mercury in the longer leg having no room

room to play, cannot ftrike with violence as before againft the top of the glafs. But though by many times fucceffively fhaking the barofcope we did not perceive that it was very like to be prejudiced by the fhakes it might neceffarily endure in tranfportation to remote places, if due care be had of it by the way, yet till further trial have been made I fhall not pretend to be certain of the event. But thus much of conveniency we have already found in this contrivance, that we fent it fome miles off, to the top of a hill, and had it brought home fafe again, the phænomena at the top and bottom of the hill being anfwerable to what we might have expected if we had employed another barofcope.

See the whole barofcope. Plate V. Fig. 2.

When the inftrument is to be fent away, the height of the mercurial cylinder (to be meafured from the furface of the ftagnant mercury in the fhorter leg) being taken for that place, day, and hour, and compared (if it may be) with that of another good barofcope, which is to continue in that place; as much of the gutter as is unfilled by the glafs may be well ftuffed with cotton, or fome fuch thing, to keep the glafs the more firm in its pofture; and that the tube be not fhaken or preffed againft the wood, fome of the fame matter may be put between the reft of the frame and the cover, which ought to be well bound together. And when the inftrument is arrived at the remote place where it is to be employed (for if it be to be fent but a little way, it may be carried fafely without ufing any adventitious liquor) the water that is added may be taken off again, by foaking it up with pieces of fpunge, linen, &c. but if inftead of water you put in mercury, as it ought to have been put in by weight, fo it is to be taken out, till you have juft the weight that was put in; and it is not difficult to take out the mercury by degrees, by the help of a fmall glafs pipe, fince you may either fuck up little by little as much as remains of the additional mercury, when by erecting the barometer, and warily unftopping the orifice of the lower leg, as much mercury as will of itfelf flow out is effluxed; or elfe you may take out the fuperfluous mercury, by thrufting the lower end of the pipe into that liquor, and when it has taken in enough, ftopping the upper end clofe with your finger, to keep it from falling back again when you remove the pipe.

N. B. If it fhould happen in a long voyage, that by the numerous fhakings of the inftrument there fhould, from the additional water or mercury in the fhorter leg, get up into the longer any little aerial bubble, which feems the only, but I hope not likely, danger in this contrivance, he that is to ufe the inftrument at the end of the voyage may, if he be fkilful, free the mercury from it by the fame way that we lately prefcribed to free it from air, when the inftrument was firft filled.

I presume I need not tell your lordfhip that the chief ufe of this travelling barofcope is, that he that ufes it in a remote part, keeping a diary of the heights of the mercury, by comparing thefe heights with thofe at which the mercury ftood at the fame times in the barometer that was not removed, the agreement or difference of the weight of the atmofphere in diftant places may be obferved. To which this may be added, the conveniency which the ftructure of thefe inftruments gives them to be fecurely let down into wells or mines, and to be drawn up to the top of towers, and fteeples, and other elevated places; not here to confider whether by a convenient addition, thefe, as well as fome other barometers, may not be made to difcover even very minute alterations of the atmofphere's preffure.

Whether this travelling barofcope, being furnifhed at its upper end with a very good ball and focket, and at the lower end with a great weight (which way of keeping things fteady in a fhip has been happily ufed by the Royal Society on another occafion) whether, I fay, our inftrument may by this contrivance, or fome other that might be fuggested to the same purpose, be made any thing serviceable at sea, notwithstanding the differing motions of the ship, I have had no opportunity to try; but whether it may or may not be useful in spite of the rolling of the ship, it may at least be made use of in flat calms (which divers times happen in long voyages, especially to the *East Indies*, and to *Africk*) and then the instrument which at other times may lie by without being at all cumbersome, may be made use of, as long as the calm lasts, to acquaint the observer with the weight of the atmosphere in the climate where he is, and that upon the sea; which may give some welcome information to the curiosity of speculative naturalists, and perhaps prove either more directly, or in its consequences of some use to navigators themselves, as by enabling them by its sudden changes to foretel the end of the calm. Besides that having one of these instruments ready at hand, wherever they set foot on shore, though it be but upon a small island, or a rock, they can presently and easily take notice of the gravity of the atmosphere in that place; which whether or no, if compared with other observations, it may in time prove not altogether useless to the guessing whereabouts they are, and the foreseeing some approaching changes of weather, I leave to future experience, if it shall be thought worth the making to determine.

Besides the ordinary baroscope, and this travelling one, I have employed two or three other instruments of quite differing kinds, to discover the varying gravities of the atmosphere; but though they have hitherto succeeded well for the main, yet being willing to make further observations about them, I reserve one of them for another opportunity, and think fit to leave the other in a tract it belongs to.

A Postscript Advertisement.

Since the writing of the foregoing and the following experiments about the travelling baroscope, having had occasion to make one at a place about fifty miles distant from that where I was when I writ them, I took notice that the mercury in the travelling baroscope was not by $\frac{1}{4}$ of an inch so high as that in another baroscope made the ordinary way; and yet it was not easy to perceive that the former had been less carefully filled than the latter. So that I yet know not well to what cause to impute the difference, unless it should perhaps depend upon this circumstance; that the pipe whereof the travelling baroscope was made, was very slender, and much more so than the tube of the other; and I have already elsewhere observed that mercury, contrary to what happens in water, is less apt to rise in very slender pipes. And though I remember that at the place where I writ the experiment to which this postscript belongs, in the tube I then employed to make the travelling baroscope, the mercury ascended as high as in a noted one made the common way; yet not being in the other place furnished with a tube long and big enough, I think myself obliged, till I can clear the doubt by further trial, to give your lordship this advertisement, lest either the cause already suspected, or some other unheeded thing, may in some cases make these travelling baroscopes somewhat differing from others. But though they should prove to be so, yet it would not follow that they cannot be made serviceable; for keeping a pretty while that instrument which suggested the scruple to me, just by the other with which I had compared it, and carefully taking notice of the respective heights at which the mercury rested in both, I observed, that when it rose or fell in the other barometer, it did also rise and fall in the portable one; and when it rested at its first station in the former, it did so in the latter; and though there seemed to be an inequality in the quantity of the ascent, and subsidence of the mercury in the two instruments, yet that seemed to be accountable

countable for by some circumstances, especially the very unequal breadth of the vessel that contained the stagnant mercury in the other barometer, and that shorter leg which answered to that vessel in the travelling barometer. But till the formerly proposed scruple be by further observations removed, the safest way will be to make the barometer to be sent to remote places as like as may be (in bigness and length of the tube) to another portable one kept at home, that so when they are once adjusted, the collations may be made betwixt two instruments of the same kind, whereof that which is kept at home may also, if it be thought fit, be compared, when the observations are made, with a baroscope made the ordinary way.

EXPERIMENT XXIII.

Confirming that mercury in a barometer will be kept suspended lower at the top than at the bottom of a hill. On which occasion something is noted about the height of mountains, especially the pic of Teneriff.

To give your lordship some instance (till I can present you with a noble one) of the use of our travelling barometer, I shall now add, that when I writ the foregoing experiment, chancing to be within two or three miles of a hill, which, though not high, was the least low in that country, I thought our instrument might be safely, and not altogether uselesly, carried on horseback to the top of it, which was too remote from the bottom to be conveniently reached by me on foot in the midst of winter. This trial therefore I resolved to make, because though I formerly told you of a considerable one that had been made in *France* by some eminent virtuoso of that country, yet I was willing not only to have a proof, how safely our baroscope might be transported, but to confirm to your lordship upon our own observation made in another region, so considerable an argument as these kind of experiments afford to our hypothesis.

And though when I came to try the experiment I happened to have an indisposition that forbid me to do it all myself, yet, having carefully marked on the edge of the frame the height to which the suspended quicksilver reached, and compared it with a good baroscope made the ordinary way, I committed our instrument to a couple of servants that I had often employed about pneumatical and mercurial experiments, giving them particular instructions what to do. And the instrument being such as might be safely carried on horseback, I had in two or three hours an account brought me back, the sum of which was, that they found the suspended mercury fall a little as they ascended the hill, at whose top they gave the liquor leave to settle, and carefully took notice, by a mark, of the place it rested at; which was, as I afterwards found, $\frac{1}{4}$ of an inch, or somewhat better, beneath the mark I had made; and this notwithstanding the hill was not high, and the air and wind seemed to them to be much colder at the top of it than beneath. But though as they descended more and more, they observed the mercury to rise again higher and higher (as being pressed against by a taller column of the atmosphere) and though consequently the experiment agreed very well with our hypothesis, and may serve for a confirmation of it; yet by reason of the small height of the mountain, the decrement of the height of the mercurial cylinder was not so considerable, but that I should perhaps have omitted the mention of this trial, if it did not shew that our travelling baroscopes may be fit to be employed about such experiments; and therefore, when I can recover some of my scattered papers, I shall by way of ap-

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pendix, ſubjoin to this ſome other obſervations that I procured to be made by ingenious men who had the opportunity of living near higher mountains.

Some further trials I have recommended to be hereafter made by ſome other inquiſitive perſons; and to make them the more inſtructive, I could wiſh that others would do what I ſhould have done, if opportunity had befriended me; for I deſigned to make the experiment at the bottom, the top, and the intermediate part of the whole, at three differing conſtitutions of air, when it ſhould appear by a good ordinary baroſcope, that the atmoſphere was very heavy, when it ſhould be found to be very light, and when it ſhould have a moderate degree of gravity: and I hoped that if ſagacious experiments ſhould make theſe diverſified obſervations on diſtant and unequal hills, good hints may reſult from the collations that may be made of the varying decrements of the mercurial cylinder's height, according to the differing gravities of the atmoſphere at ſeveral times, and the differing heights of the hills and ſtations where the obſervations ſhould be made.

I also endeavoured to get a baroſcope carried down to the bottoms of deep mines; partly to try whether the atmoſpherical pillar being longer there than at the top, the mercury in the tube would not be impelled up higher; and partly in order to other diſcoveries. But ſome impediments in the ſtructure of thoſe mines made it not very practicable to employ barometers there; which yet makes me not deſpair of ſucceſs in ſome other mines, where the ſhafts or pits are ſunk more perpendicularly.

Perhaps I told your lordſhip already by word of mouth, that I have been ſolicitouſly endeavouring to get the Torricellian experiment tried upon the pic of *Teneriff*; but hitherto I have had no account of the ſucceſs of my endeavours; for which I am the more concerned, becauſe of the eminent (if not matchleſs) height of that mountain, of which you may receive ſome ſatisfaction by what I am going to ſubjoin about it.

An Appendix about the height of Mountains.

Forasmuch as on the one hand not only *Kepler*, but divers other modern writers of note, do endeavour to ſtraiten the atmoſphere, and make it lower by half than the leaſt height to which, according to our eſtimation, it ſhould reach; and to countenance their opinion, will not allow the clouds to be often above a mile high, nor even the higheſt mountains to exceed two miles. And foraſmuch as on the other ſide other learned men ſeem to make the clouds and the mountains of a ſtupendous height; we, who take a middle way of eſtimating the height of the one and the other, hold it not unfit to ſubjoin on this occaſion ſome uncommon obſervations in favour of our opinion, that we have obtained from inquiſitive travellers.

But firſt I will ſubjoin a paſſage I have ſomewhere met with in *Ricciolus*'s *Almigeſtum novum*, where he, if I well remember, relates that the rector *Metenſis*, as he calls him, of the Jeſuits college, affirmed to him ſome years ſince that he had meaſured the height of many clouds, without having found any of them higher than 5000 paces; which argues, that he met with ſome ſo high, though indeed the height of clouds muſt needs be very various, according to the gravity or lightneſs, denſity or thinneſs, reſt or agitation of the air, and the condition of the vapours and exhalations they conſiſt of. And if either that be true which we have formerly had occaſion to mention concerning *Maignan*'s obſervation; or if it be true that ſublunary comets (for I ſpeak not of celeſtial ones) are generated of exhalations of the terreſtrial globe, we may well conjec-

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ture that the atmosphere (especially if its height be not uniform) and even clouds, especially those that have most fumes and fewest vapours, may reach much higher than *Cardan*, *Kepler*, and others have defined.

But of the height of clouds, which we have sometimes attempted to take geometrically, we may have elsewhere occasion to speak again; and therefore I shall now proceed to what I have to say concerning the height of mountains; which being an enquiry curious and difficult enough in itself, and of some importance in the disquisition about the height of the atmosphere, it being evident that that must reach at least as high as the tops of mountains, upon whose tops men can live; I hope it will not be unacceptable to your lordship, if having a while since, as I was intimating, had the opportunity to discourse with some credible persons that have been upon the top of exceeding high mountains, particularly of the pic of *Teneriff*, and especially with one gentleman, who was a few days before brought to satisfy the curiosity of our inquisitive and discerning monarch, by giving him an account of his journey, I acquaint you with those of the particulars which I learned from thence, that are the most pertinent to our present purpose. First then, whereas divers late mathematicians will not allow above two miles or half a *German* league, and some of them not half so much, to the height of the highest mountain; the mountain we speak of, in the island of *Teneriff*, one of the Canaries or Fortunate Islands, is so high, that though perhaps I think those travellers I have taken notice of speak with the most when they write, that the top of this mountain is to be seen at sea, four degrees off, i. e. at least threescore *German* leagues; yet having asked the ingenious gentleman lately mentioned, Mr. *Sydenham*, from what distance the top of the sugar-loaf, or highest part of the hill, so called from its figure, could be seen at sea, according to the common opinion of seamen, he answered, that that distance was wont to be reckoned 60 sea-leagues, of three miles to a league; adding, that he himself had seen it about 40 leagues off, and yet it appeared exceeding high, and like a bluish pyramid, manifestly a great deal higher than the clouds. And what he related to me about the distance was afterwards confirmed by the answers I received from observing men of differing nations, who had sailed that way; and particularly by a noble virtuoso, skilled in the mathematicks, who was then admiral of a brave English fleet. And the abovementioned gentleman Mr. *S.* also told me, that sometimes men could from thence see the island of *Madeira*, though distant from it 70 leagues; and that the great Canary, though 18 leagues off, seemed to be very near them that were on the top of the sugar-loaf, as if they might leap down upon it. Thus far Mr. *Sydenham*, by whose relation it appears, that this pic must be far higher than *Kepler* and others allow mountains to be; for else it could not be seen at sea from so great a distance. And the learned *Ricciolus* supposing it to be (as some navigators report it to be) discoverable at sea four degrees off, calculates its height measured by a perpendicular line, and allowing too for refraction, to amount to ten miles, which altitude also the accurate *Snellius* assigns it. But I fear this learned man may have been somewhat misinformed by the navigators he relies on, or else that the way of allowing for refractions is not yet reduced to a sufficient certainty; for I do not find by those who have purposely gone to the top of it, that the mountain is so high as his calculation makes it. And whereas the same eminent writer resolutely pronounces that the height of mount *Caucasus*, deduction being made for refraction, is 51 *Bolognian* miles, which are considerably greater than the *Roman* miles; I doubt not here likewise, though I question not his supputations, if you grant him the grounds of them, he makes this mountain far higher than indeed it is. For the passage of *Aristotle*, on which he founds his opinion, is obscure enough; and *Aristotle* himself does sometimes take up reports upon hear-say, without over-

over-ſtrictly examining their truth or probability; whereas all the navigators and travellers I have hitherto met with (and your lordſhip knows that I have, upon a publick account, the opportunity of meeting often with ſuch men) do almoſt unanimouſly agree that the pic of *Teneriff* is the higheſt mountain hitherto known in the world; and yet that is ſo far from being 15 leagues high, as ſome eminent and even late writers would perſuade us, that it is ſcarce a ſeventh part ſo high as *Ricciolus* computes mount *Caucaſus* to be. For having aſked Mr. *Sydenham* and others what was the eſtimate made by the moſt knowing perſons of the iſland of the height of the hill, he told me that his guides accounted it to be one-and-twenty miles high from the town called *L'oretava*, ſeated on the lower part of the hill, from which town to the ſea there is three miles of way always deſcending. But in regard that the way which amounted to 21 miles in length is, as other ways whereby ſteep places are wont to be aſcended, made to wind and turn for the conveniency of travellers; I can ſcarce deduct leſs than two thirds for the crookedneſs of the way; and accordingly having aſked him, whether the perpendicular height of it had been accurately taken by any with mathematical inſtruments, he anſwered, that he could ſay nothing to that upon his own knowledge, but that a ſeaman with great confidence affirmed himſelf to have accurately enough meaſured it by obſervations made in a ſhip, and to have found the perpendicular height of the hill to be about ſeven miles. Which eſtimate agrees well enough with the calculations of *Ricciolus* and *Snellius*, if we leſſen the diſtance from which the top of the hill is to be diſcovered, from 60 *German* leagues of four miles to a league, to the like number of common leagues at three miles to a league.

And becauſe eminent writers have ſo confidently delivered prodigious things touching the height of this mountain, I will here, to confirm the eſtimate already made, add theſe particulars, which I took from the gentleman's own mouth, and which were afterwards confirmed to me by another that went with him, and partly alſo by a third, who went up to the top at another time of the year, viz. that they begun their journey from *L'oretava* on the 18th of *Auguſt*, about 10 of the clock at night, and travelled till five in the afternoon on the *Monday* following, reſting two hours by the way, and travelling about 10 miles of the way upon mules, which afterwards they were forced to leave, and betake themſelves to their feet. Reſting upon *Monday* till midnight, they reſumed their journeying, and travelled until about nine the next morning, at which time they arrived at the top of the Sugar-loaf, or higheſt pile of the mountain; ſo that they travelled in all but 26 hours, in which, conſidering the ſteepneſs and ruggedneſs of the ways, and that they were forced to go above half way on foot, to which they were unaccuſtomed, it is likely enough that the length of the way did not much, if at all, exceed the computation of the guides.

We have ſince endeavoured, but without yet knowing what will be the ſucceſs, to have the height of this mountain carefully taken by ſkilful men; in the interim I ſhall not deny, but that if what *Ariſtotle* and other authors report of mount *Caucaſus* be true, there may be far higher mountains than the pic of *Teneriff*; eſpecially ſince there is one conſideration, which perhaps you will not think deſpicable, that I find not taken notice of by thoſe that have written of the height of mountains, viz. that of two mountains, that meaſured by geometrical inſtruments may appear to be of the ſame height, there may yet be a great inequality; becauſe the meaſurer meaſures only from ſome plain piece of ground at the bottom of the hill to the top, whereas it may be, that the country wherein one of thoſe mountains ſtands, may be exceedingly much higher than that wherein the other is placed; which difference of heights in the ſeveral countries he that is to meaſure only the height of one of the mountains, is not wont to take any notice

The like conſideration I ſince found to have been had, before me, by the learned *Ricciolus*.

notice of; and consequently, though in respect of the plains adjacent to the feet of the mountains, their altitudes may be equal yet in respect of the level or superficies of the terraqueous globe, considered as having no mountains at all but those two, the height of the one may far exceed that of the other; and so the pic of *Teneriff* being looked upon from the level of the sea, may be much less high than some other hills, but may appear much higher than some other hills, which yet protuberating above the level part of some country, which is itself generally exceeding high, may have its top more remote from the centre of the earth than that of the pic, and would appear higher than it, if as well the one as the other were looked upon from the same superficies of the sea.

But to return to the height of the atmosphere; in order to the making an estimate of what we have considered as to the height of mountains, I shall add, that though by what has been already said touching the height of the pic and other hills, it appears that the atmosphere reaches far higher than many learned men would hitherto allow; yet we are not to think that the atmosphere may not reach almost incomparably higher than the tops of mountains: nor do I suffer myself to be concluded by what many commentators of *Aristotle* and other writers are wont to teach, touching the distinct narrow extent they allow to that sphere, within whose limits they would have the steams of the terrestrial globe to produce meteors. How far the height of mountains may make the air at the tops of them inconvenient for respiration, shall be (God permitting) considered, when I come to acquaint your lordship with my loose trials about respiration.

EXPERIMENT XXIV.

Shewing that the pressure of the atmosphere may be exercised enough to keep up the mercury in the Torricellian experiment, though the air press upon it at a very small orifice.

By a very slight variation of the foregoing 22d experiment we may both confirm one of the most important, and the least likely truths of the hydrostaticks, and remove an objection, which, for want of the knowledge of this truth, is wont to be urged against our hypothesis even by learned men. For divers of these, when they see the same phænomena happen in the Torricellian experiment, whether it be made in the open air or in a chamber, are forward to object, that if it were, as we say it is, the weight of the air incumbent on the stagnant mercury, which keeps that suspended in the tube from falling down, the mercury would not be sustained at any thing near the same height in the open air, where the pillar that is supposed to lean upon the stagnant mercury, may reach up to the top of the atmosphere, as in a close room, where they imagine that no more air can press upon it than what reaches directly up to the roof or cieling. And when to this it is answered, that, though, if a room were indeed exactly closed, the sustentation of the mercury ought to be ascribed to some other cause than the weight of the imprisoned air, which other cause I have elsewhere shewn to be its spring, yet in ordinary rooms there is still a communication between the internal and external air, either by the chimney, or, if the room have none, by some crevice in the window, or by some chink between the wall and the door, or at least by the key-hole. And when to this it is objected, that the orifice of the key-hole is much narrower than the superficies of the stagnant mercury, and consequently, though the atmosphere were not reduced to press obliquely on the mercury, yet, entering at so small an orifice, it could not press sufficiently upon it; when I say, in answer to this objection I have alledged that hydro-

statical

ftatical theorem, that the preffure, in fuch cafes as ours, is to be eftimated by the heights of the liquors, and not the breadths, the affertion has been thought unlikely and precarious.

To confirm, therefore, this hydroftatical truth, one may take the bended tube mentioned in the 22d experiment, and inclining it till the greateft part of the mercury pafs from the fhorter leg into the longer, the upper end of this fhorter leg may by the flame of a lamp be drawn out fo flender, that the orifice of it fhall not be above an eighth or tenth part (not to fay a much lefs) as big as it was before. For this being done, and the tube erected again, if the tall cylinder of mercury be of the ufual or former height, as we have found it, it will appear congruous to our hypothefis that the weight of the external air may exercife as much preffion upon the ftagnant mercury through a little hole, as when all the upper fuperficies of that mercury was directly expofed to it.

And if one have not the conveniency to draw out the fhorter leg, as is prefcribed, one may neverthelefs make the trial, by carefully ftopping up the orifice with a cork and cement, leaving only, or afterwards making a very fmall hole for the air to pafs in and out. If I had not wanted a fit inftrument, I would have tried to exemplify the truth of what has been delivered, by adding to the glaffes we employed to make the fifth experiment fuch a cover as might be cemented on to the edge of the glafs, having only a very fmall hole in the midft, at which the atmofphere would be reduced to exercife its preffure; and the like cover I would have made ufe of in the tenth experiment, about the breaking of glafs-plates in the unexhaufted receiver, by the bare fpring of the air.

EXPERIMENT XXV.

Shewing that an oblique preffure of the atmofphere may fuffice to keep up the mercury at the wonted height in the Torricellian experiment, and that the fpring of a little included air may do the fame.

By adding a couple of little circumftances to the trials lately propofed, we may confirm two confiderable articles of our hypothefis. For, 1, if, inftead of drawing the fhorter leg of our barometrical fiphon, if I may fo call it, directly upwards or parallel to the longer leg, as in the foregoing experiment, you make the flender part bend off fo, as that, if it were continued, it would make a right angle with the longer leg of the fiphon, or elfe an acute angle, tending downwards; this being done, I fay, if when the tube is erected the mercury reft at its wonted ftation, it will appear that the preffure of the atmofphere may be exercifed upon it as well obliquely, when the pipe that conveys it is either horizontal or opens downwards. See Plate V. Fig 3.

And, 2. if, inftead of bending this flender pipe, one feal it up hermetically, the continuance of the mercurial cylinder at the fame height will fhew, that the fpring of a very little air, fhut up with the preffure of the atmofphere upon it (though no more than what the air here below is ordinarily expofed to by the weight of the incumbent air) is able to fupport as tall a cylinder of mercury as the weight of the whole atmofphere, i. e. of as much of it as can come to exercife its preffure againft the mercury. See Plate V. Fig 4.

N. B. If when the fhorter leg of the barofcope is fealed up, you move the inftrument up and down, the mercury will vibrate, by reafon of the fomewhat yielding fpring of the imprifoned air; but becaufe of the refiftance of the fpring the motion will be diverfified after an odd and pretty manner; which may be eafily perceived by the impreffion it makes upon the hand, but not so easily described. And because that, when the shorter leg is drawn out slender enough, after the instrument is furnished with quicksilver, it is easy to seal it up with the flame of a candle, without the help of any instrument at all, I shall here take notice to your lordship (which I could not reasonably do before) that it may on some occasions be convenient to seal up the barometer before it be transported, and, in some cases, to incline the tube beforehand till the quicksilver have quite filled the longer leg; by this means the vibrations of the quicksilver will be less than otherwise they would be, and it will be no trouble at all, when the instrument is brought to the designed place, to break off the slender apex of the shorter leg, and so expose again the mercury to the pressure of the atmosphere.

As about the former experiments, so about these two this advertisement may be given, viz. That the same trials, for the main, may be made without confining one's self to the proposed ways of making them.

1. For the first of these new trials may be made by cementing very carefully on to the orifice of the shorter leg (which need not be altered) a short pipe of glass, whose upper end may be drawn out very slender, and bent either horizontally or downwards, which is far easier to be done than to draw out the shorter leg, when the glass is furnished with mercury.

2. And as for the second trial, that may be well enough made by carefully stopping the unaltered orifice of the shorter leg with a good cork, and our close cement, or with the latter only; and when you would afterwards use this instrument as a baroscope, you need but heat a pin or slender wire red hot, and so burn a hole through the stoppel.

And this expedient, which I could not conveniently advertise your lordship of sooner, may be of use, when a travelling baroscope is to be often removed; because having once stopped the whole orifice well, it is far more easy to stop and open a pin-hole accurately, than to close and unstop the whole orifice of the tube.

Note, I endeavoured to confirm more than one of the foregoing particulars by this one experiment. Having caused a portable barometer to be made with the shorter leg of a somewhat more than ordinary length, I afterwards caused the upper part of this leg to be drawn out very slender, as in this 25th experiment; and lastly, I caused the same shorter leg to be either about or somewhat above the middle bended downwards, so that the small orifice of the slender apex pointed towards the ground. This done, I was to have measured the height of the suspended mercury, but not having a fit ruler at hand, I then deferred, and afterwards forgot to do it; but I remember, that neither I, nor some others versed in such experiments, to whom I shewed it, took any notice, that the mercury was less high than in ordinary barometers; whence it was concluded, that the atmosphere could exercise his pressure not only at a very small orifice, which in our experiment did little, if at all exceed a pin-hole, but when the air must at this little orifice press upwards to be able to press upon the surface of the stagnant mercury.

EXPERIMENT XXVI.

About the making of a baroscope (but of little practical use) that serves but at certain times.

To shew some ingenious men by a medium that has not hitherto (that I know of) been made use of, that the not subsiding of quicksilver in an inverted tube, that is a little

little ſhorter than thirty inches or thereabouts, does not proceed from ſuch a *fuga vacui* as the ſchools aſcribe to nature, but from the gravity of the external air, I deviſed the following experiment.

Having made choice of a time, when it appeared by a good baroſcope, which I had frequently conſulted for that purpoſe, that the atmoſphere was conſiderably heavy, I cauſed a glaſs-pipe, hermetically ſealed at one end, and in length about two feet and a half, to be filled with quickſilver, ſave a very little, wherein ſome drops of water were put, that we might the better diſcern the bubbles, if any ſhould be left after the inverſion of the tube into an open glaſs with ſtagnant mercury in it. Having by this means, though not without difficulty, freed the tube from bubbles, we ſo ordered the matter, that the quickſilver and the little water that was about it filled the tube exactly, without leaving any interval, that we could diſcern at the top, and yet the mercurial cylinder was but very little higher than that of our baroſcope was at that time.

This done, the newly filled pipe was left erected in a quiet place, where the liquors retained their former height for divers days. But though an ordinary ſchool-philoſophet would confidently have attributed this ſuſtentation of ſo heavy a body to nature's fear of admitting a vacuum; yet it ſeems, that either ſhe is not always equally ſubject to that fear, or ſome other cauſe of the phænomenon muſt be aſſigned; for when (a pretty while after) I had obſerved by the baroſcope, that the atmoſphere was grown much lighter than before, repairing to my ſhort tube, I found, that according to my expectation the quickſilver was not inconſiderably ſubſided, and had left a cavity at the top, which afterwards grew leſſer according as the atmoſphere grew heavier.

N. B. 1. The tube employed about this experiment may be brought to the requiſite ſhortneſs, either by wearing off a little of the glaſs at the orifice of it, or by increaſing the height of the ſtagnant mercury into which it hath been inverted.

2. When the quickſilver in our ſhort tube was much ſubſided, there appeared in the water that ſwam upon it a little bubble, about the bigneſs of a ſmall pin's head; but, conſidering how careful we had been to free the tube from bubbles before we ſet it to reſt, it may very well be, that this ſo ſmall a bubble was not produced till after the ſubſiding of the quickſilver, whereupon the aërial particles in the water became leſs compreſſed than before; not to mention that the bubble (ſuch as it was) appeared very much greater than it would have done, if the preſſure of the atmoſphere had not been kept from it by the weight of the ſubjacent pillar of mercury.

EXPERIMENT XXVII.

About the aſcenſion of liquors in very ſlender pipes in an exhauſted receiver.

What I related to your lordſhip, in the 35th of the publiſhed experiments, about the ſeemingly ſpontaneous aſcenſion of water in ſlender pipes, has occaſioned the making of many trials by the curious, whereby that experiment has been not a little diverſified. But becauſe among thoſe I have yet heard of, none have been made in our engine, it may not be amiſs to add the following trial, which may be of uſe in the examen of one or two of the chief conjectures that have hitherto been propoſed about the cauſe of that odd phænomenon.

We tinged ſome ſpirit of wine with cochineal, which being put into the receiver and the air withdrawn, did exceedingly bubble for a pretty while; then little hollow pipes of of different ſizes being put into it, the red liquor aſcended higher in the ſlenderer than the others; but upon the withdrawing of the air there ſcarce appeared any ſenſible difference in the heights of the liquor, nor yet upon the letting it in again.

AFTERWARDS two ſuch pipes of differing ſizes being faſtened together (at a diſtance) with cement, were let down into the ſame ſpirit of wine, when the receiver was well exhauſted, notwithſtanding which the liquor aſcended in them, for aught we could plainly ſee, after the ordinary manner; only when the air was let in again, there ſeemed to be ſome little (and but very little) riſing, at leaſt, in one of the pipes. In this trial, this phænomenon was noted, that though there appeared no bubbles at all in the veſſeled ſpirit of wine (notwithſtanding that we continued to pump) yet there did for a pretty while ariſe bubbles in that part of the liquor that was got into the ſlender pipes; which I gueſſed to proceed from the ſuſtentation (in part) of the ſpirit of wine, made by the inſide of the pipe whereto it adhered.

EXPERIMENT XXVIII.

About the great and ſeemingly ſpontaneous aſcenſion of water in a pipe filled with a compact body, whoſe particles are thought incapable of imbibing it.

UPON occaſion of the (ſeemingly) ſpontaneous aſcenſion of water in ſlender pipes of glaſs, I conſidered, that it would be eaſy by another way to make it riſe to a far greater height than hitherto had been done; for ſince we had found by obſervation, that, *cæteris paribus*, the ſlenderer the little pipes were that we employed, the higher the liquor would riſe in them; and ſince the hydroſtaticks had taught us, that oftentimes, even in very crooked pipes, water would be made to aſcend by the ſame ways (of raiſing it) to the ſame perpendicular height (or thereabouts) as in ſtraight ones; I thought, that I might well ſubſtitute a powder, conſiſting of ſolid corpuſcles heaped upon one another and included in a glaſs-cane, inſtead of the little pipes I had hitherto uſed. For I conſidered the little intervals that would neceſſarily be left between theſe differingly ſhaped and confuſedly placed corpuſcles, would allow paſſage to the water, as did the cavities of the little pipes, and yet would in many places be ſtraighter than the ſlendereſt pipes I had uſed: and though beaten glaſs, or fine ſand, &c. might have been employed about this experiment, yet I judged it far more convenient to make uſe of ſome metalline calx, becauſe the operation of the fire making a more exquiſite comminution of ſolid bodies than our peſtles are wont to do, is fit to ſupply us with exceeding minute grains, that intercept proportionable cavities between them.

UPON this conſideration, therefore (beſides others to be hereafter hinted) I took a ſtraight pipe of glaſs open at both ends, and of a moderate wideneſs (for it need not be very ſlender) and having tied a linen-rag to one end of it, that the water might have free paſſage in, and the powder not be able to fall out, we carefully, and as exactly as we could, filled the cavity with minium (which is lead calcined, without addition to redneſs); and then having erected the tube, ſo that the bottom of it reſted upon that of a ſomewhat ſhallow and open-mouthed glaſs, containing water enough to ſwim an inch or two above the bottom of the tube, into whoſe cavity it did, as I expected, inſinuate itſelf by degrees, as appeared by a little change of colour in that part of the minium which it reached, till (the open glaſs being from time to time ſupplied with freſh liquor) it attained to the height of about 30 inches; and then, our Society ex-

This was (if I forget not) about the latter end of the year 1662.

preſſing

preſſing a curioſity to ſee it and have it placed among better things, I was hindered from making any further obſervations with that particular glaſs.

Wherefore, taking afterwards another tube and ſome minium carefully prepared, I proſecuted the experiment, ſo as to make the water riſe in the pipe about 40 inches above the ſurface of the ſtagnant water. I gueſſed it had riſen higher, but by reaſon that, at the upper part of the minium, the difference of colour was ſo ſmall, as not to be eaſily diſtinguiſhable with certainty, I forbore to allow a greater height to the aſcenſion of the water; nor could I, where I then was, much promote the experiment, for want of ſuch accommodations as I deſired; but about the experiment, as I tried it, I ſhall take notice of the following particulars.

I tried ſome other powders beſides red lead (as beaten glaſs, pieces of fine ſpunge, putty, &c.) but did not find any of them do ſo well; which ſucceſs was yet perhaps but accidental, and therefore the trial may be repeated, eſpecially with putty, becauſe that being a metalline calx as well as minium, conſiſts of very ſmall grains, and, by reaſon of its great whiteneſs, receives a greater change of colour by wetting than minium does; in which, eſpecially if it be very fine, the diſcoloration that water makes towards the upper part of the tube, is ſometimes not ſo eaſy to be clearly diſcerned.

2. I did indeed endeavour to remedy this inconvenience by uſing, inſtead of mere water, tincted liquors, as ink, tincture of ſaffron, &c. but they ſeemed not to riſe near ſo high as water alone, as if the diſſolved ingredients did by degrees choak the pores of the minium.

3. To have the grains of our powder more minute, and the ſmaller intervals between them, I choſe not only to uſe the fineſt ſort of minium I could procure, but alſo to ſift it through a very fine ſearce, and to put it but by little and little into the tube, that by ramming it from time to time it might be made to lie the cloſer; which expedients ſucceeded not ill.

4. It ſeemed by a trial or two (for I am not ſure the obſervation will always hold) that if the tube were very ſlender (as about the bigneſs of a ſwan's quill) the experiment ſucceeded not well.

5. It may be worth while to obſerve in what times the water aſcends to ſuch and ſuch heights; for at the beginning it will aſcend much faſter than afterwards, and ſometimes it will continue riſing 24 or 30 hours, and ſometimes perhaps much longer.

6. One of the ſcopes I propoſed to myſelf in this experiment was to diſcover a miſtake in the explication, that ſome learned modern writers have given us of the cauſe of filtration; for, whereas they teach, that the parts of filter that touch the water, being ſwelled by the ingreſs of it to their pores, are thereby made to liſt up the water till it touch the ſuperior parts of the filter that are almoſt contiguous to them; by which means, theſe being alſo wetted and ſwelled, raiſe the water to the other neighbouring parts of the filter till it have reached to the top of it, whence its own gravity will make it deſcend: but in our caſe we have a filter made of ſolid metalline corpuſcles, where it will be very hard to ſhew that any ſuch intumeſcence is produced as the recited explication requires.

7. Water aſcends ſo few inches even in very ſlender pipes, as to ſeem much to favour their judgment, who diſallow the conjecture lately entertained by ſome ingenious men (particularly Mr. *H.*) about the raiſing of the ſap in trees, after the like manner that water is raiſed in ſlender pipes. But without fully delivering yet my thoughts of that ſpeculation, I may take notice, that in the laſt trial above recited, I made water to aſcend near if not above 3 feet $\frac{1}{2}$; and if, by ſo ſlight an expedient, water may be made to riſe as high as is neceſſary for the nutrition of ſome thouſand of plants (for ſuch a

 number

number there is, that exceed not 3 feet ½ in height) one may without absurdity ask, Why it is not possible that nature, or rather the most wise author of it, may have made such contrivances in plants, as to make liquors ascend in them to the tops of the tallest trees; especially since, besides divers things that we may already suspect (as heat, and something equivalent to well placed valves) many others that perhaps are not yet dreamt of, may probably concur to the effect.

8. As I formerly made, by bending the slender pipes we have been talking of, short siphons, through which the water runs, without being at first assisted by suction, so I thought fit to try, whether I could not in larger pipes, by the help of minium, make much longer siphons. But though when the orifices were turned upwards, fine minium were rammed into both the legs, and the orifices were both of them closed; yet when they came to be again turned downwards the weight of the minium would somewhere or other (and, for the most part, at or near the flexure) make some such chink or discontinuation, as to hinder the farther progress of the water. Which impediment, though I judged it superable enough (especially by making at the flexure a little pipe or socket, by which both legs might be closely filled) yet for want of accommodations and leisure it was left unsurmounted: upon which account also, I did not satisfy myself about the success of some former trials, as of the ascension of water into pieces of wood of differing sorts, the operation of the vicissitudes of the sun's beams, and the absence of them, upon liquors ascending in tubes filled with minium, &c.

9. WHETHER the pressure of the outward air be the cause of the ascension of liquors in our tubes furnished with minium is a problem, in order to whose solution I could acquaint your lordship with a contrivance wherewith to make some trials in our engine. But since it can scarce be well described without many words, unless you express a particular curiosity to know it, I shall not trouble you with it; and the rather, because the best way I know of examining this difficulty belongs to the second part of this Continuation, where mention is made of an attempt about it, which did not, I confess, displease me.

EXPERIMENT XXIX.

Of the seemingly spontaneous ascension of salts along the sides of glasses, with a conjecture at the cause of it.

To the same cause, or the like, with that of the ascension of water in slender pipes, may be probably referred an odd phænomenon, which, though I remember not to have been mentioned by any chymical or other writer, I have not unfrequently observed, as well by chance as in trials purposely made to satisfy myself, and others, about the truth of it.

THE phænomenon, in short, was this: that having, in wide-mouthed glasses (which should not be very deep) exposed to the air a strong solution of common sea-salt, or of vitriol, which reached not, by some inches, to the top of the glass; and having suffered much of the aqueous part to exhale away very slowly, the coagulated salt would, at length, appear to have lined the inside of the glass, and to have ascended much higher, not only than the place where the surface of the remaining water then rested at, but than the place to which the liquor reached when it was first poured in; and if the experiment were continued long enough, I sometimes observed this ascension of the salt to amount to some inches, and that the salt did not only line the inside of the glass, but, getting over

over the brim of it, covered the outfide of it with a faline cruft; which made them that faw how little liquor remained in the glafs, admire how it could poffibly get thither.

And though I have mentioned but the folution of vitriol and fea-falt, becaufe they are much eafier than others to be procured, and yet the experiment fucceeds better in them than in fome other far lefs parable falts; yet they are not the only ones, by whofe folutions the recited phænomenon may be exhibited.

As for the caufe of this odd effect, though I fhall not propofe any thing about it with confidence till I have further inquired into it, and efpecially till I have tried whether the phænomenon may be produced in an exhaufted receiver; yet, by what I have hitherto obferved, I am inclined to conjecture that it may be referred to fuch a caufe as that of the afcenfion of liquors in pipes, after fome fuch manner as this.

First, I obferved, that in water and aqueous liquors, that part of the furface which is next the fides of the glafs, is (whatever the reafon of it be) fenfibly more elevated than the reft of the fuperficies; and, if very little clippings of ftraw or other fuch minute and light bodies, floating upon the water, chance to approach near enough to the fides of the glafs, they will be apt (which one would not expect) to run up, as it were, this afcent of water, and reft againft the fides of the glafs.

Next we may take notice, with the falt-boilers and chymifts, that fea-falt is ufually wont to coagulate at the top of the water in fmall and oblong corpufcles, fo that as to thefe, it is eafy to conceive to them that have confidered the firft obfervation, how numbers of them may faften themfelves round about to the infide of the glafs. And befides fea-falt, I have found by trial divers others, if their folutions be flowly enough evaporated, that will, whilft yet there remains a good proportion of liquor, afford faline concretions at the top of the water; and the faftening of faline particles to the fides of the glafs may perhaps be promoted by the coldnefs that may be communicated to the corpufcles contiguous to the glafs, by reafon of the coldnefs which the glafs may be fufpected to have, upon the fcore of its denfity, in comparifon of water. But to proceed: I confider, that by the evaporation of the aqueous parts of the folution, the furface of the remaining liquor muft neceffarily fubfide, and thofe faline particles that were contiguous to the infide of the glafs and the more elevated part of the water, having no longer enough of liquor to keep them diffolved, will be apt to remain fticking to the fides of the glafs, and upon the leaft farther evaporation of the water will be a little higher than the greater part of the fuperficies of that liquor; by which means it will come to pafs, that, by reafon of the little inequalities that will be on the internal furface of the adhering corpufcles of the falt, and perhaps alfo on the internal fuperficies of the glafs, there will be intercepted between the falt and the glafs little cavities, into which the water contiguous to the bottom will afcend or be impelled, upon fuch an account as that whereon it is raifed in flender pipes. And when the liquor is thus got to the top of the falt, and comes to be expofed to the air, the faline part may, by the evaporation of the aqueous, be brought to coagulate there, and confequently to increafe the height of the faline film, if I may fo call it; which, by the like means, may be at length brought to reach to the very top of the glafs, whence it may eafily be brought over to the outfide of the veffel, where the natural weight of the folution will facilitate its progrefs downwards; and the fkin of falt, together with the contiguous furface of the glafs, may at length conftitute a kind of fiphon.

To this explication it agrees well, that I have ufually obferved the faline film hitherto mentioned to be with great eafe feparable from the glafs in large fleaks; which argues that they did not ſtick cloſe to one another, except in ſome few places, but had a thin cavity intercepted between them through which the water might aſcend.

Nor is it repugnant to this explication, that in caſe the water aſcended, it ſhould, as it ſeems, diſſolve the ſalt; for the liquor being already upon the point of concretion, is ſo glutted with ſalt that it can diſſolve no more. Whence we may alſo render a reaſon, why, when the ſaline film chances to reach to the outſide of the glaſs, the liquor divers times does not run down to the bottom, but is coagulated by the way: and I have alſo had a ſuſpicion (though I could not ſeaſonably take notice of it before now) that when the concretion is once begun, the film may be raiſed and propagated, not only by the motion of the liquor between the inſide of it and the glaſs, but by the ſame liquor's inſinuating itſelf on the outſide of the film into the ſmall chinks and crevices intercepted between the ſaline corpuſcles, as ink (eſpecially if ſomewhat thin) riſes into the ſlit, and along the ſides of the nib of a pin, though nothing but its very point be dipt in the ſurface of the liquor; and by this means the impregnated ſolution may, as it were, climb up to the top of the ſaline concretion, and by coagulating there add to its height.

Some other circumſtances I have noted of our phænomenon that agree with the propoſed explication; but perhaps it would not be worth while to ſpend more time about it. Not to examine here, whether what has been related, ſo as to make it probable, that aſcending water may carry up wherewithal to heighten and increaſe the pipes or veſſels through which it riſes, may contribute any thing more than was ſuggeſted in the former 28th experiment, towards the explication of the riſing and diffuſing of the ſap in trees.

EXPERIMENT XXX.

About an attempt to meaſure the gravity of cylinders of the atmoſphere, ſo as that it may be expreſſed by known and common weights.

Whilst I was making the former experiments, it was more than once my wiſh, that by knowing the juſt weight of a cylinder of quickſilver of a determinate diameter, and of 29 or 30 inches high, which is near the height that the air does uſually counterbalance, I might the better eſtimate the weight of a cylinder of the atmoſphere of that diameter, and conſequently make the better gueſſes how near the effects of the ſpring of the air, as well as of its weight, produced by the help of our engine, approached to the utmoſt of what might have been expected, in caſe all the inſtruments employed had been perfect, and all concurrent circumſtances had been favourable. And upon this account, I ſeveral times regretted my want of a long inſtrument of ſteel or hardened iron, wherewith I many years ſince made an obſervation that was more carefully regiſtered than preſerved, of the weight of a mercurial cylinder of a determinate height, as well as diameter; which weight I did not think ſo ſafe to determine by the help of glaſs tubes, becauſe it is very difficult to have them uniformly cylindrical, and to know that they are ſo, in regard that they are formed but by blowing and drawing out; and, beſides the inequality that may happen to the cavity upon other accounts, it is very difficult to make the ſides of the glaſs equally thick, and to examine whether they be ſo or no.

But at length lighting upon (what I had too often wanted in the foregoing experiments) a dexterous artificer that chanced to come for a while to the place where I then was,

was, I endeavoured to repair my loſs, as well as he could help me to do it, by cauſing him to turn very carefully a cylindrical piece of braſs of an inch in diameter, and three inches in length, and open (that it might be the better wrought) at both ends, to one of which was exactly fitted a flat bottom of the ſame metal faſtened very cloſe to it with little ſcrews on the outſide, this being judged a better way than if it had been turned all of a piece.

THIS inſtrument being diligently counterpoiſed in a truſty pair of ſcales, was carefully filled with mercury which (for greater caution) we took out of a new parcel, that we had not yet employed about other experiments, and finding it to weigh 17 ounces, 1 drachm, 45 grains, Troy weight (or 137 drachms 45 grains) multiplying that by 10, there will come for the weight of a mercurial cylinder of one inch in diameter and 30 inches in height (and ſo high I have divers times ſeen the mercury to be in a good barometer) about 14.2 lb. (i. e. 14 lb. 2 ounces, and above three drachms, Troy weight); and almoſt 11.8 lb. Avoirdupoiſe weight (i. e. 11 lb. 12 ounces, and above 6 drachms) which is a greater weight than, without ſuch a trial, one would eaſily imagine, that ſo ſhort a cylinder of mercury, and much leſs that a cylinder of ſo light a body as air, being neither of them above an inch diameter, could amount to.

NOTE firſt, to examine at the ſame time the weight of the mercury, and its proportion to water, we did, before the mercury was poured into the braſs-veſſel, fill it with water (after which we wiped it dry before the mercury was put into it) and this liquor weighing 10 drachms and 15 grains, the proportion between the mercury and the water appeared to be that of 13 $\frac{8}{41}$ to 1; which, though it ſeem ſomewhat of the leaſt, yet your lordſhip may remember that I formerly told you I had ſeveral times found the received proportion of 14 to 1, between mercury and water, to be ſomewhat too great; and beſides that, in a veſſel whoſe orifice was no leſs than an inch in diameter, it is exceeding difficult to be ſure when it is preciſely full, either of water or mercury, becauſe the former has a ſuperficies conſiderably concave, and the other one that is notably convex; and though we uſed ſome little artifices (which would be troubleſome here to mention) to eſtimate the protuberance of the one liquor and the deficience of the other, as near the truth as could be, yet I am not ſure but there may have been a few mercurial corpuſcles more than there ſhould have been, and that conſequently ſome ſmall abatement may have been made of the weight newly attributed to the whole mercurial cylinder of 30 inches.

2. I HAD thoughts of making uſe of the barrel of a gun, of a convenient length, to find the weight of a mercurial cylinder of 2 feet and $\frac{1}{2}$; but I preferred the inſtrument already made uſe of (eſpecially not being where I could have one bored after a peculiar way) not only becauſe I could not meet with one whoſe diameter was juſt an inch, and conſequently as convenient for calculations, and becauſe that the barrels of guns are often bored a little tapering; but becauſe a ſkilful artificer confeſſed to me that they ſcarce ever bore ſuch barrels but with a four-ſquare bit (as they call it) which leaves the cavity too angular, or too imperfectly round; whereas if an hexahedrical bit be employed, it will, as he affirmed, make the cavity almoſt as cylindrical as can be reaſonably deſired. I ſay nothing here of making uſe, for our purpoſe, of a trunk, as they call a hollow cylinder of wood, becauſe I elſewhere ſhew that wood (at leaſt ſuch as the trunks to ſhoot pellets with are wont to be made of) is not of a texture cloſe enough for ſuch an uſe.

3. BECAUSE in cylinders of mercury, 30 inches is a height which the atmoſphere is ſeldom heavy enough to be able to counterpoiſe; and becauſe 29 inches is ſomewhat nearer the middle between the greateſt and the leaſt heights, at which I have obſerved the

the mercury at differing times to ſtand in good barometers; your lordſhip may, if you pleaſe, abate a thirtieth part of the weight aſſigned above to a mercurial cylinder of 30 inches (though I take 29 and $\frac{1}{4}$, or thereabouts, to be ſomewhat a more uſual height of the mercury than preciſely nine-and-twenty).

4. The weight of a mercurial cylinder in an æquilibrium with the atmoſphere, and of one inch in diameter, being thus ſettled, we may, by the help of the doctrine of proportions and a few propoſitions, eſpecially the 14th of the 11th book of *Euclid*'s Elements, eaſily enough calculate the weight of a cylinder of mercury of another diameter, and conſequently the force of the preſſure of an atmoſpherical pillar of the ſame diameter. For ſince according to the forenamed 14th propoſition of the 11th, cylinders of equal heights are to one another as their baſes; and ſince by the ſecond propoſition of the ſame 11th element, circles (ſuch as are the baſes of cylinders) are to one another as the ſquares of their diameters; and ſince, laſtly, we ſuppoſe that mercury being a homogeneous body, at leaſt as to ſenſe, the mercurial cylinders will have the ſame proportion to each other in weight that they have in bulk; ſince, I ſay, theſe things are ſo, if, for inſtance, we deſire to know what will be the weight of a cylinder of 30 inches high whoſe diameter is two inches, the rule will be this:

As the ſquare of the diameter of the ſtandard cylinder (as I call that whoſe weight is already known) is to the ſquare of the diameter of the cylinder propoſed, ſo will the bulk of the former cylinder be to that of the latter, and the weight of that to the weight of this.

According to which rule, the ſquare of one inch (which is the diameter of the ſtandard cylinder) being but one (whereby your lordſhip may perceive how much the meaſure I pitched on facilitates computations) and the ſquare of two (which is the diameter of the propoſed cylinder) being four, the bulk or ſolid contents of this latter cylinder, and conſequently its weight, will be four times as great as thoſe of the ſtandard cylinder; and ſo, ſince the leſſer has been already ſuppoſed to weigh 11.8 lb. avoirdupoiſe, the mercurial cylinder of two inches in diameter will weigh 47.2 lb. of the ſame weight.

EXPERIMENT XXXI.

About the attractive virtue of the loadſtone in an exhauſted receiver.

Some learned modern philoſophers that have attempted to explicate the cauſe and manner of magnetical attraction or coition, give ſuch an account of it, as ſuppoſes that the air between the two magnetical bodies, being driven away by their effluviums from between them, preſſes them on the parts oppoſite to thoſe where the contact is to be made; and upon ſome ſuch ſcore (for I muſt not now ſtay to deliver their theories circumſtantially) the air is ſuppoſed to contribute very much to the attraction and ſuſtentation of the iron by the loadſtone; wherefore, partly to examine this opinion, and partly for ſome other purpoſes (not neceſſary now to be mentioned) we thought fit to make the following experiment.

We took a ſmall, but vigorous loadſtone, capped and fitted with a looſe plate of ſteel, ſo ſhaped that when it was ſuſtained by the loadſtone, we could hang at a little crook, that came out of the midſt of it, and pointed downward, a ſcale wherein to put what weights we ſhould think fit; into this ſcale we put ſometimes more and ſometimes leſs weight; and then by ſhaking of the loadſtone as much as we gueſſed it would be ſhaken by the motion of the engine, we found the greateſt weight that we preſumed it would

would be able to ſupport, in ſpite of the agitation it would be expoſed to, which proved to be, beſides the iron-plate and the ſcale, ſix ounces Troy weight, to which if we added half an ounce more, the whole weight appeared too eaſy to be ſhaken off: this done, we hung the loadſtone, with all the weight it ſuſtained, at a button of glaſs, which we had procured to be faſtened on to the top of the inſide of a receiver, when it was firſt blown; and though in about 12 exſuctions we uſually emptied ſuch receivers as much as was requiſite for moſt experiments, yet this time, to exhauſt it the more accurately, we continued pumping till we had exceeded twice that number of exſuctions; at the end of which time, ſhaking the engine ſomewhat rudely, without thereby ſhaking off the weight that hung at the loadſtone, the iron ſeemed to be very near as firmly ſuſtained by it, as before the air began to be pumped out: I ſaid very near, rather than altogether, becauſe that the withdrawing of the air, though it be not ſuppoſed to weaken at all the power of the loadſtone preciſely conſidered, yet it muſt leſſen its power to ſuſtain the ſteel; becauſe this in ſo thin a medium muſt weigh heavier than in the air, by the weight of as much air as is equal in bulk to the appended body.

Some other magnetical trials (and alſo ſome electrical ones) I remember I attempted to make by the help of our engine; but not having the notes I took of them now at hand, I ſhall ſuſpend the mentioning them, till I can give your lordſhip a more punctual account of them.

EXPERIMENT XXXII.

Shewing that when the preſſure of the external air is taken off, it is very eaſy to draw up the ſucker of a ſyringe, though the hole, at which the air or water ſucceed, be ſtopped.

Having taken notice that ſome learned oppoſers of the modern doctrine about the weight of the atmoſphere think themſelves more than ordinarily befriended by the difficulty we find in drawing up the embolus or ſucker of a ſyringe, when the hole at which the air or water ſhould ſucceed is ſtopped, and by the violence with which, as ſoon as it is let go, it is, as they imagine, drawn back; and ſuppoſing the reaſon of this confidence of theirs to be that men have not yet been able in theſe phænomena (as in ſome others) to prove the intereſt of the atmoſphere's gravity by direct or confeſſedly analagous experiments; I preſumed it will not be unwelcome to your lordſhip, if I here fortify the ſpeculations that have been or may be propoſed to explicate theſe things according to the hypotheſis of the weight of the air, by what we tried to that purpoſe, among others, when we were making uſe of a ſyringe in our engine.

The First Trial.

We took a ſyringe of braſs (that metal being cloſer and ſtronger than pewter, of which ſuch inſtruments are uſually made) being in length (in the barrel) about ſix inches, and in diameter about one inch $\frac{3}{8}$; and having, by putting a thin bladder about the ſucker, and by pouring a little oil into the cavity of the cylinder (or barrel) brought the inſtrument to be ſtanch enough, and yet the ſucker to move to and fro without much difficulty, we thruſt this to the bottom (or baſis) of the barrel to exclude the air; and having unſcrewed and laid aſide the ſlender pipe of the ſyringe (which in this and ſome other trials was like to prove not only needleſs but inconvenient) we carefully ſtopped the orifice

orifice to which the pipe in these instruments is wont to be screwed, and then drawing up the sucker we let it go, to judge by the violence with which it would be driven back again, whether the syringe were light enough for our purpose; and finding it to be so, we fastened to the barrel a ponderous piece of iron to keep it down, and then fastening to the handle of the rammer (or axle-tree of the sucker) one end of a string, whose other end was tied to the often-mentioned turning key, we conveyed this syringe, and the weight belonging unto it, into a receiver; and having pumped out the air, we then began to turn the key, thereby to shorten the string that tied the handle of the syringe to it; and, as we foretold, that the pressure of the air lately included in the receiver being withdrawn, we should no more find the wonted resistance in drawing up the sucker from the bottom of the cylinder, so we found upon trial that we could very easily pull it up without finding any sensible resistance.

See Plate VI. Fig 1.

However, having thought fit to repeat the experiment (which we did with the like success) lest it might be objected that this want of resistance might proceed, as partly from our employing the turning key to raise the sucker, so principally from some unperceived leak at which the air may be supposed to have got into the cavity of the cylinder, I thought fit not only to examine by trial, after the receiver was removed from off the pump, whether the syringe were not stanch (upon which I found that I could not, without some straining, draw up the sucker even a little way, and that it would be violently beaten back again) but also in one of these experiments to make this variation, that when the receiver being exhausted, we had drawn up the sucker almost to the top of the barrel, by such a string as was purposely chosen somewhat weak, we kept the parts of the syringe in that posture, till we had opened a passage to the outward air, upon whose ingress the sucker was (as we intended it should be) so forcibly depressed, that it broke the string by which it was tied to the turning key, and was violently driven back to the lower part of the barrel, and that notwithstanding these two disadvantageous circumstances; one, that the string was not so weak but that one whom I employed to try it before it was fastened to the syringe, made it sustain a lump of iron that weighed between four and five pounds; and the other, that yet this string was broken long before all the air that flowed in to fill the receiver had got in; so that the pressure of all the admitted air would doubtless have broken a much stronger string, if we had employed such a one to resist the depression of the sucker, which will yet be more evident by a phænomenon of our syringe, that I shall presently have occasion to relate.

The Second Trial.

Containing a variation of the foregoing.

We took the syringe employed in the foregoing experiments, and having found by trial that it was, though not perfectly, tight, nor altogether so much so as before, yet enough so for our present purpose (since, when the orifice of the vent in the basis was stopped, if the sucker were more forcibly drawn up a little way, and then let go, it would hastily return, or rather violently be impelled back towards the bottom of the barrel) we made it serve us as well as we could for the following experiment. Of this syringe we did very carefully with a cork and our cement close the vent; and then having tied to the barrel of the syringe a weight that happened to be at hand (and to amount to two pounds and as many ounces) we suspended the rammer of the syringe by a string in a large receiver; and then causing the pump to be applied, we made 11 or 12

exsuctions

exſuctions of the air, without any appearance of change in the ſyringe. But becauſe I had judged the above-mentioned weight ſufficient, and ſuppoſed, that the little air ſtill remaining in the receiver had yet too ſtrong a preſſure to be ſurmounted by it, I cauſed the pumping to be continued, and within two or three exſuctions more I perceived the cylinder to begin to be drawn down, though but very ſlowly, by the weight hanging at it (aſſiſted by its own gravity) and likewiſe tried (after having purpoſely ſtopped a-while the working of the pump) that juſt upon a freſh ſuck the deſcent would be manifeſtly accelerated. And when we had ſuffered the barrel and weight to ſlide down as far as we thought fit, we let in the external air, which, as was to be expected, raiſed them both again much faſter than they had ſubſided.

N. B. There would not have needed any thing near ſo great a weight to depreſs the barrel of the ſyringe, but that it is difficult in ſuch an inſtrument to make the ſucker fill it accurately enough, without making it ſomewhat uneaſy to be moved to and fro; upon which account it was neceſſary that a weight ſhould be added, not only to ſurmount the preſſure of the air remaining in the receiver (which was not, nor needed to be diligently exhauſted in this experiment) but to overcome that reſiſtance which we juſt now noted the inequalities of the inſide of the cylinder, and thoſe of the ſucker to give to the motion of the one in or over the other. And yet for all this it is not eaſy, though it be not impoſſible, to make one of theſe ſyringes very tight, eſpecially when the noſe is well ſtopped and the ſucker drawn up, there being often ſome little air that ſtrains in between the ſucker and the barrel, and ſome that will be harboured between the ſucker, though thruſt home, and the bottom of the barrel, beſides what may lurk between the ſame ſucker and the cork that ſtops the orifice of the vent: nor were we confident that our ſyringe did not at length let ſome aërial particles inſinuate themſelves into the cavity, which the depreſſion of the barrel had made betwixt the baſes of that barrel and the ſucker; and in ſuch caſes we ought not to wonder, if upon the return of the air the barrel and weight be not impelled up altogether to the ſame height they reſted at when they were firſt ſuſpended in the receiver.

2. It agreed very well with our doctrine, that as the cylinder and weight began not to fall till a great quantity of air had been pumped out of the receiver, ſo they did not begin to move upwards preſently upon the freedom that was allowed the air to return into the receiver; for till it had continued a pretty while flowing in, there was not enough of it entered to reſtore, by its preſſure, the cylinder and the annexed weight to their former ſituation.

3. What has been delivered about our experiment may be confirmed by this variation which we made of it; that having ſubſtituted a far heavier weight inſtead of that lately mentioned, the depreſſion of the barrel of the ſyringe ſucceeded two or three times one after another much ſooner than formerly, viz. about the ſixth, or at moſt the ſeventh exſuction.

EXPERIMENT XXXIII.

About the opening of a ſyringe whoſe pipe was ſtopped in the exhauſted receiver, and by the help of it making the preſſure of the air lift up a conſiderable weight.

Though the trial I am about to relate had not all the ſucceſs I deſired, yet perhaps it will not be impertinent to make mention of it, becauſe there is not any ſort of experiments that is wont ſo much to perſuade the generality of ſpectators of the great force of

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the pressure of the air, as those wherein they plainly see heavy and solid bodies made to ascend (upon the operation of the air on them) without seeing any other thing lift them up.

We took the often-mentioned syringe, and having closed up the hole at the bottom with good cement, we tied to the barrel a hollow piece of iron that served us for a scale, into which we put divers weights one after another, trying from time to time, whether, when the sucker was forcibly drawn up and held steadily in its highest station, the weight tied to the barrel (which was held down whilst the sucker was drawn up, and afterwards let go) would be considerably raised. And when we perceived that the addition of half a pound or a pound more would make the weight too great to be so raised, we forbore to put in that increase of weight; and having tied the handle of the rammer to the turning-key, we conveyed the syringe, together with its clog, into a receiver, out of which a convenient quantity of air being pumped, we were thereby enabled easily to draw up the sucker without the cylinder; after which, having let in the air, the by-standers concluded that the weight was raised a little, which yet I would not have allowed, if we had not been able, by inclining the engine and the receiver, to make the syringe and weight a little to swing. But to make the effect more evident, I caused a two pound weight to be taken out, and then the receiver being somewhat exhausted, and the air re-admitted, the clog, when all the air was come in, was swiftly raised, and, as it were, snatched up from the middle to the upper part of the suspended rammer.

See Plate VI. Fig. 2.

It is no easy matter to measure, with any certainty and exactness by a syringe, the weight of an atmospherical pillar equal to it in diameter, especially if there be any imperfection in the syringe, either because the sucker does not go close enough, in which case it can scarce be stanch, or because by its pressure against the inside of the barrel, which often happens if it be too close, it hinders the sucker and barrel from sliding without resistance by one another, and consequently there is an undue resistance made to the endeavour of the atmosphere to raise the barrel and weight; and therefore, though our syringe being, upon the account of some ill accident, less in order than it was in some of the foregoing experiments, I must not conclude that a cylinder of the atmosphere of the same wideness with it is equipollent to no greater a weight than that which was taken up in our trial, yet we may safely conclude, that so slender a pillar of the atmosphere is able to raise by a syringe at least such a weight, as in our experiment it actually lifted up, which amounted to above sixteen pounds (avoirdupoise weight) for it exceeded fifteen pounds and three quarters, besides the weight of the syringe's barrel itself.

EXPERIMENT XXXIV.

Shewing that the cause of the ascension of liquors in syringes is to be derived from the pressure of the air.

I shall not here trouble your lordship with what I have elsewhere proposed about the explicating of suction; but as by the lately recited experiments (I mean the 31st, 32d, and 33d) it has appeared, that it is to the pressure of the external air that we should ascribe the difficulty of drawing up the sucker of a syringe when the pipe or the vent is stopped; so I shall now endeavour to shew that the ascension of liquors which follow the sucker when it is drawn up, the pipe being open, depends also upon the pressure of the air incumbent on that liquor.

It

If I had been furnished with very tall receivers and such other glasses as I could have wished, I had tried the following experiments with water, as well as quicksilver; but for want of those accommodations I was reduced to make my experiment with the latter only of those liquors, which yet will, I hope, sufficiently make out what was intended.

The First Trial.

We took a small receiver shaped almost like a pear, cut off horizontally at both ends (being the same capped glass that is elsewhere mentioned in the accounts of other experiments); we also took the syringe formerly described, and, having fastened on to it with good cement, instead of its own brass-pipe, a small glass-pipe of about half a foot in length, we put this syringe in at the narrower end of the receiver; to whose orifice was afterwards carefully cemented on the brass-cap with the turning-key, whereto was tied by a string the handle of the rammer; then having conveniently placed upon the engine a very short thick glass shaped like a sugar-loaf (which was made use of for want of a better) with a sufficient quantity of quicksilver in it, we so placed the receiver over it, that the lower end of the pipe of the syringe reached almost to the bottom of this glass, and consequently was immersed a pretty way beneath the surface of the quicksilver: we had also poured a little water in the upper part of the syringe, that no air might get in between the sucker and the cylinder, notwithstanding that, by some accident or other, the syringe was become somewhat less tight than before: and last of all, we cemented the receiver to the engine after the usual manner.

See Plate VII. Fig. 2. which, tho' made primarily for the 39 experiment, may facilitate the conceiving of this.

That which now remained being to try the experiment itself, in order to which all this had been done, the air was pumped out of the receiver (and consequently out of the little glass that held the mercury) and then the sucker being warily drawn up, we could not see the quicksilver ascend to follow it, though a little water, which, it seems, the outward air had thrust in between the sucker and the cylinder, was either raised or stopped in the glass-pipe of the syringe (whereof yet much the greatest part remained unfilled); of which the reason, according to our hypothesis, was manifest, namely, that the air being pumped out of the receiver, the little that remained had not strength enough to press up so ponderous a liquor as the quicksilver into the pipe (though even that little unexhausted air might have spring enough left to raise a little water); and since it appeared by this, that without the pressure of the air the quicksilver would not be elevated, we thought it seasonable to shew, that by the pressure of the air it would: whereupon, the air being let slowly into the receiver, the mercury was quickly impelled up at least to the top of the glass-pipe (though, by reason of some unperceived leak it was not long sustained there).

And for further satisfaction, when the experiment was to be tried over again, we ordered it to be so made that might plainly be observed, that though, when the receiver not being yet exhausted, the sucker was drawn up but one inch, the mercury would be raised to the upper part of the glass-pipe of the syringe, yet after the exhausting of the receiver, though the sucker was drawn up twice as high, there appeared no ascension of the mercury in the pipe, whose lower part only was darkened by the little glass which contained that fluid metal.

Before I dismiss this experiment, I must, to make good a promise I made your lordship, acquaint you with a phænomenon which does not a little confirm our doctrine, according to which it was easy both to foresee, and to explain it; the phænomenon was, that if, when the air was diligently pumped out of the receiver, the sucker were endeavoured to be pulled up, it could not be so without much difficulty and resistance, such as was formerly found when the vent of the syringe was stopped, of which in our hypothesis the reason may be clearly this; that there being no common air in the receiver to assist by its pressure (whether immediate or mediate) the raising of the sucker, this could not be raised but by a force great enough to surmount the weight of the external air or atmospherical pillar that leaned upon it, so that as the other phænomena of our experiments manifest, that the raising of liquors by a syringe, which is commonly ascribed to attraction, depends upon the pressure of the air; so by this phænomenon it appears that the difficulty of opening a syringe whose pipe is stopped need not be attributed to such a *fuga vacui* as vulgar philosophers refer it to; since, in our case, the same difficulty was found, though the pipe were open and the liquor it was immersed in might have had free access to the place deserted by the sucker.

The Second Trial.

Being a prosecution of the former attempt.

To vary as well as confirm the foregoing experiment, we caused the syringe to be tied fast to a competently ponderous body that might keep the cylinder unmoved, when the sucker should be drawn up; we also cemented on to the vent or screw at the bottom of the syringe, a pipe of glass of about two inches in length (which should have been longer, but that then there would not have been room in the receiver for pulling up of the sucker) and having placed the heavy body whereto the syringe was tied upon a pedestal of a convenient height, that the glass-pipe might be all seen beneath it, and a very low phial almost filled with quicksilver might be so placed underneath the pipe, that the stagnant mercury reached a good way above the immersed orifice of the said pipe. These things being thus provided, and the handle of the syringe's rammer being tied with a string to the turning-key that belonged to the brass-cover of the receiver, this vessel was cemented on to the engine, and by it exhausted after the usual manner.

When this was done, we looked upon the syringe's glass-pipe above mentioned, and being able to see through it (whereby we were certain that it was not yet full of quicksilver) we did, by the string, draw up the sucker to a good height, but could not perceive the pipe to be filled with any succeeding mercury; wherefore, warily letting in some air, we quickly saw the mercury impelled to the very top of the pipe; and we concluded, from the quantity of quicksilver that was raised, that a pretty deal was also driven into the cavity of the cylinder.

N. B. I had once before seen the mercury ascend into the pipe upon the letting in of the air into the emptied receiver; but it seeming somewhat difficult to me to determine whether the sucker had been raised, because there was no mark to guide my estimate by, I thought it might be suspected, that in case the sucker had not been raised, the ascension of the quicksilver might have proceeded from hence, that the air contained in the glass-pipe breaking out through the stagnant mercury upon the exhausting of the receiver, the quicksilver might, upon the return of the air into the receiver, be pressed up into the place deserted by the air that broke out of the pipe; wherefore we caused a string to be tied about the rammer, as near as we could to the top of the cylinder, by which means, when the receiver was the next time exhausted, we perceived, that by drawing up the sucker we had raised it about two inches, if not more, and yet we we could not difcern any mercury to follow it (the glafs-pipe ftill continuing tranfparent) until we had let fome air return into the receiver.

THIS experiment, joined with thofe we have formerly related to have been tried with our fyringe, may teach us, that if a fyringe were made ufe of above the atmofphere, neither the ftopping of the pipe would hinder the eafy drawing up of the fucker, nor the drawing up of the fucker, though the pipe were not ftopped, would raife by fuction the liquor which the pipe was immerfed in.

POSTSCRIPT.

SINCE the laft-recited experiment was made and written, finding fome of our inftruments to be in better order than they were when that trial was made, we thought fit to endeavour, by that which follows, to repair an omiffion or two that formerly we could not well avoid.

HAVING then caufed fuch a glafs-pipe, as has been lately mentioned, to be well cemented on to the fyringe (whofe fucker did now move more eafily, and yet fill the barrel more exactly than before) I ordered (being to be abfent for a while myfelf) that the pipe fhould be filled with fpirit of wine tincted with cochineal, that the liquor and its motions might be the better difcerned, and that the pipe being filled, that air might be excluded which would elfe be harboured in the pipe, which caution was omitted in the foregoing experiment: and this the perfon to whom I committed it, affirmed to have been carefully done, though when he inverted the pipe thus filled into the reft of the red liquor, that was put into a phial, he could not poffibly do it fo well but that a bubble of air got into the pipe, and took up fome (though but a little) room there: by that time I was called upon to fee the event of the trial, and could come to look upon it, the receiver was almoft quite exhaufted; wherefore, after I had made the pumping be continued a little longer, and perceived that the tincted fpirit was fallen down out of the pipe, and that which lay in the phial feemed almoft to boil at the top, by reafon of the emerfion of numerous bubbles, I caufed the fucker to be, by the help of the turning-key, drawn up by our eftimate about two inches and a half, notwithftanding which we could not perceive the fpirit of wine to rife in the pipe, though the pumping were before left off: for which reafon I ordered the air to be let in very leifurely, upon which we could plainly fee that the red fpirit was quickly driven up to the top of the pipe; and that it was fo likewife into the cavity of the barrel, appeared, when the receiver was removed, by the fmall quantity of liquor that remained in the phial, and the plenty of it which came out of the fyringe.

N.B. THAT if I had not wanted dexterous artificers to work according to a contrivance I had defigned, I had attempted to imitate, by the help of the bare fpring of the air, fuch experiments as in the lately recited trials were made to fucceed, by the help of the preffure exercifed by the air upon the account of its weight.

EXPE-

EXPERIMENT XXXV.

Shewing, that upon the pressure of the air depends the sticking of cupping-glasses to the fleshy parts they are applied to.

It is sufficiently known, that if the air within a cupping-glass be rarefied by the flame of tow, flax, or the like, burned for a little while in it, and the glass be presently clapped upon some fleshy part of a man's body, there will quickly ensue a painful and visible swelling of the part covered by the cupping-glass.

It is also known, that this experiment is wont to be urged by the schools, as a clear proof of that abhorrence of a vacuum they ascribe to nature; for, say they, the reason of this phænomenon is plainly, that the internal air of the cupping-glass, præternaturally rarefied by heat when the instrument is applied, that heat after a while ceasing, the succeeding cold must again necessarily condense the air; and so this contracted air being no longer able to fill the whole space it replenished before, there would ensue a vacuum, if the flesh covered by the cupping-glass, or adjoining to it, did not swell into the cavity of it, to fill the place deserted by the air.

Those moderns that assert the weight of the atmosphere, do thence ingeniously endeavour to deduce the phænomenon; and indeed, if to their hypothesis about the air's weight the consideration of its spring be added, it will be easy enough to explicate the phænomenon, by saying, that when the cupping-glass is first set on, though much of the air it formerly contained were a little before expelled by the heat, yet the same heat, increasing the pressure of the remaining air, is the cause that the absence of the air driven out of the glass does not immediately occasion so sensible a pain; but when that adventitious agitation of the included air ceases, that air having now (because of the paucity of its corpuscles) but a weak spring, can no longer press upon the part covered by the cupping-glass near so strongly, as the outward air does by its weight press upon all the neighbouring parts of the flesh; by which means, according to what we have more than once explicated already, some of the yielding flesh or other body covered by the skin must be forcibly thrust into the cavity of the cupping-glass, where there is less pressure than at the outside of it: and the fibres and membranous parts being thus violently stretched, there must needs follow a sensible pain as well as tumour; which tumour yet does not fill up the cupping-glass, not only because of the resistance of the skin to be so far distended, but also, if the included air have not been much rarefied, because of the spring of the imprisoned air, which grows so much the stronger by how much the swelling flesh reduces the air into less room, as I have sometimes tried, by applying a cupping-glass to quicksilver, or even to water, which will rise in it but to a certain height.

But though by this, or some such explication, the argument urged by the schools in favour of the *fuga vacui* may be sufficiently enervated; yet it suited better with the design of this treatise to propose some new experiment to illustrate our hypothesis; and though it seemed to be far more difficult to do it in reference to cupping-glasses than to other subjects, yet I pitched upon two different ways of experimenting, whose success not disappointing me, I shall now give your lordship an account of them.

We took a glass of about one inch and a half in diameter, but a good deal longer than an ordinarily shaped cupping-glass of that breadth would have been, that there might be the more room for the flame to burn in it and rarefy the air; we also provided a receiver shaped almost like a pear; this receiver was open at both ends; at the sharper whereof

whereof there was but a ſmall orifice, but at the obtuſe end there roſe up a ſhort neck, Plate VI. Fig. 3. whoſe orifice was wide enough to admit with eaſe the newly mentioned cupping-glaſs, without touching the ſides of it, and we were not willing it ſhould be much larger, leſt it ſhould not be ſo exactly covered by the palm of the hand that ſhould be laid upon it, and leſt alſo the hand ſhould be broken or hurt by the too great weight of the atmoſphere, when the included air ſhould be withdrawn from under it.

These things being thus prepared, and the ſmaller orifice of the receiver being faſtened with cement to the engine, I cauſed the cupping-glaſs to be faſtened, with the mouth upwards, to the palm of the hand of a youth whom your lordſhip may remember to have ſeen with me, whoſe hand ſeemed framed by nature for this experiment, being broad, ſtrong, and very plump; and having pulled the glaſs to try whether it ſtuck well on, I cauſed him to put it into the receiver and lay his hand ſo upon the orifice lately mentioned, that it might ſerve for a cover to it, and hinder any air from getting in between them.

That which we pretended was, that the receiver being but ſmall (that it might be quickly exhauſted, and ſo not put the youth to a long pain) upon an exſuction or two made with the pump of the air about the cupping-glaſs, the remaining air ſhould have its preſſure ſo far weakened, as not to be able to ſupport the cupping-glaſs; eſpecially ſince if the air without the cupping-glaſs, but yet in the receiver, ſhould be more rarefied by the removal of that which had been pumped out, than the air included in the cupping-glaſs was by the precedent heat, this laſt-mentioned air having a ſtronger ſpring (or tendency to expand itſelf) than the external air of the receiver, the glaſs muſt needs fall down, or rather be thruſt off, though, in caſe there had been no air at all left in the cavity of the cupping-glaſs, the air in the receiver would by its preſſure ſuſtain a far greater weight.

The event of our trial agreed very well with our conjecture. For upon the firſt ſuck the cupping-glaſs fell off, the weight of the atmoſphere preſſing ſo hard upon the young man's hand, that, though he be more than ordinary ſtrong, he complained he could very hardly take it off the glaſs, it was almoſt thruſt into, and, a while after, that his hand was very ſore: but this laſt inconvenience became not ſo quickly very ſenſible but that we had time to repeat our experiment by faſtening the cupping-glaſs more ſtrongly than before; ſo that he complained that it drew in his hand very forcibly; and though that part be not wont to be fleſhy, yet the tumour occaſioned by the cupping-glaſs was manifeſt enough to the eye; but as before, ſo now, at the very firſt turning of the ſtop-cock, to let out the air of the receiver, the cupping-glaſs fell off.

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We took the braſs-hoop or ring, mentioned in the fifth and ſixth experiments, and covered it with a bladder, which was wetted to make it the more limber, and was ſo tied on to it (which was eaſy to do) that the bottom of the bladder covered the upper orifice of the hoop, and was ſtretched, though not ſtrongly, upon it, almoſt like the membrane that makes the head of a drum; and the neck of the bladder was tied with a ſtring,

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THE event of our trial agreed very well with our conjecture. For upon the firſt ſuck the cupping-glaſs fell off, the weight of the atmoſphere preſſing ſo hard upon the young man's hand, that, though he be more than ordinary ſtrong, he complained he could very hardly take it off the glaſs, it was almoſt thruſt into, and, a while after, that his hand was very ſore: but this laſt inconvenience became not ſo quickly very ſenſible but that we had time to repeat our experiment by faſtening the cupping-glaſs more ſtrongly than before; ſo that he complained that it drew in his hand very forcibly; and though that part be not wont to be fleſhy, yet the tumour occaſioned by the cupping-glaſs was manifeſt enough to the eye; but as before, ſo now, at the very firſt turning of the ſtop-cock, to let out the air of the receiver, the cupping-glaſs fell off.

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Plate VI. Fig. 4. ſtring near the middle of the lower orifice of the hoop, and in this lower part of the bladder we made two or three ſmall holes for the air to paſs in and out at; then having placed at the bottom of the often mentioned capped receiver a thick piece of wood that had a hole in it, to receive the neck of the bladder, we ſo placed the covered hoop upon this piece of wood, that the upper part of the bladder lay parallel to the horizon. This done, we ſuſpended, at the turning-key belonging to the cap of our receiver, a blind head, as chemiſts call it, of glaſs, which, for want of a true cupping-glaſs, we were fain to ſubſtitute, and which indeed was not very unlike one either for ſhape or ſize; and to the upper part of this glaſs we faſtened a large ring of metal, the better to depreſs it and make it lean ſtrongly on the bladder.

These things being thus made ready, and the receiver cemented on to the engine, we did by the help of the turning-key let down the cupping-glaſs (for ſo we ſhall hereafter call it) till it came almoſt to touch the level ſuperficies of the bladder; and when the receiver was as far exhauſted as we thought fit, but not near as far as it might have been, we let down the cupping-glaſs a little lower, ſo that it leaned upon the bladder, and touched it with all the parts of its orifice; ſo that the cupping-glaſs with the ſubjacent bladder was become an internal receiver, if I may ſo call it, whoſe air was conſiderably expanded, and conſequently weakened as to its ſpring. All this being done, we warily let the air into the receiver, and thereby the air that did ſurround the cupping-glaſs, which we juſt now called the internal receiver, having now a ſtronger preſſure than the air in the cupping-glaſs could reſiſt, the bladder, on which the cupping-glaſs reſted, was, as we looked for, thruſt up a pretty way into the cavity of the glaſs, in which it made a conſpicuous tumour, and was made to ſtick ſo cloſe to the orifice of it, that one would have thought that the bladder had been violently drawn in, as the ſkin is wont to be in the ordinary applications of cupping-glaſſes.

And becauſe we took notice, that though this glaſs were not capacious, for it ſcarce held a pint of water, yet the orifice of it was not very narrow, being in diameter an inch and $\frac{4}{7}$, we thought fit in repeating the experiment to add ſomething that ſeemed odd enough, and was fit to manifeſt, that cupping-glaſſes may, without heat, by the bare preſſure of the external air, be more ſtrongly faſtened, than, for aught we know, they are by the help of flame: having then reiterated the former experiment with this only variation, that we exhauſted the receiver further than before, we took out the cupping-glaſs and the bladder, which together with the included braſs-hoop was hanging at it; and then having tied the glaſs to the hook of a good ſtatera, and tied a large Plate VI. Fig 5. ſcale to the neck of the bladder, we put in by degrees weights into the ſcale, till we had loaded it enough to force off the bladder from the glaſs; which happened not till the whole weight that tended to draw down the bladder amounted to 35 pounds, if not better, of ſixteen ounces in the pound: nor did we doubt but that the preſſure of the atmoſphere would in our experiment have kept up a much greater weight, if we had, before we let in the outward air, diligently exhauſted the receiver, which we had purpoſely forborn to do, for fear the too diſproportionate preſſure of the external air ſhould break the bladder; which puts me in mind of adding, upon the by, that as more weight was put into the ſcale, the bladder (ſtretched more and more by the weight on one ſide, and the air on the other) appeared to ſwell higher in the cavity of the glaſs.

EXPE-

EXPERIMENT XXXVII.

Shewing that bellows, whofe nofe is very well ftopped, will open of themfelves when the preffure of the external air is taken off.

It is wont by the Peripateticks and others to be made a great argument for the *fuga vacui* which they attribute to nature, that if the nofe of a pair of bellows be well ftopped, one cannot open them by raifing the upper board from the lower. But of this another reafon may be eafily affigned, without determining whether there be a vacuum or no, namely, the weight and preffure of the air; for when the nofe of a pair of bellows that are tight enough is well ftopped, no air being able to infinuate itfelf upon the disjoining of the boards into the cavity made by that disjunction, this cannot be effected, but by fuch a force as is almoft able (I fay almoft, becaufe ordinary bellows cannot be fo well fhut, but that there will remain fome air in them whofe fpring will facilitate the opening of them) to raife an atmofpherical pillar, whofe bafis fhall be the upper board, which is commonly fo large that a lefs force may ferve to break common bellows than to raife fo great a weight; but if they were made ftrong enough, and there were applied a fufficient force to lift fo great a weight as the newly mentioned pillar of the atmofphere, the fides might be disjoined, how clofe and ftanch foever the inftrument were made.

Thus far one may argue upon the bare principle of the weight of the air, but taking in the fpring of it too, I thought one might proceed fo much further, that I ventured to foretel divers ingenious men, that if the preffure of the ambient air were taken off, not only it would be eafy to open the bellows in fpite of their being carefully ftopped at the nofe, but that they would fly open, as it were, of their own accord, without the application of any external force at all; and it was partly to juftify this prediction, as well as to make a trial I thought more confiderable, that we made the following experiment.

We caufed, then, to be made a pair of bellows, differing from ordinary ones in thefe particulars. Firft, that the boards were circular (and fo without handles) and of about fix inches in diameter: 2. That there was no clack or valve: 3. That the nofe was but an inch long, or lefs, being to be lengthened, if occafion required, with a pipe: 4. That the leather, which was not fpared, that the inftrument might be the more capacious, was not horny or very ftiff, but limber. The reafon of the firft and third diverfity was, that the bellows might be capable to be conveyed into our receiver (for which purpofe alfo, if there had appeared need, the nofe might have been made in the uppermoft of the two boards); the reafon of the fecond variation was, that the inftrument might be the more ftanch; and of the fourth, that the bafes of the bellows might, as in organ-bellows, be clapped clofer together, and harbour lefs air in the wrinkles and cavity: fo that when the bellows were opened to their full extent, by drawing up the upper bafis at a button purpofely made in the midft of it, the bellows looked like a cylinder of fixteen or eighteen inches high; upon which refemblance I take the liberty to call both the boards, as geometricians do both the circular parts of a cylinder, bafes. See Plate VI. Fig. 6.

But though thefe were made by an artificer, otherwife dexterous, yet it not being his trade to make bellows, nor any other man's in the town I then was in, he could not make them fo tight, but that in fpite of our oiling the leather and choaking the feams with good cement, there was fome little and unperceived hole or cranny, whereby ſome air had paſſage when the noſe was accurately ſtopped; but this was not ſo conſiderable, but that if we drew up the upper baſis from the lower, the external air would on all ſides preſs the leather inwards, and ſo make the ſhape of the inſtrument very far from being ſo cylindrical as it would be if the noſe were left open.

Wherefore, concluding, that notwithſtanding this imperfection the bellows would ſerve, though not for both the experiments I deſigned, yet for one of them, we carefully ſtopped the noſe, after we had approached the baſes to one another, and conveying them into a large receiver, it quickly appeared, when the pump was ſet on work, that at every exſuction of the incumbent air, the air harboured in the folds of the leather, and the reſt of the little cavity that could not but be left between the baſes, made the upper of thoſe baſes manifeſtly riſe, though its weight (becauſe of the thickneſs and ſolidity of the wood) would ſoon after depreſs it again, either by driving out ſome of the air at ſome place where the inſtrument was not ſufficiently tight, or by making it, as it were, ſtrained through the leather itſelf; and if the pump were agitated ſomewhat faſter than ordinary, the expanſion of the internal air would be greater than could be rendered quite ineffectual by ſo ſmall a leak, and the upper part of the bellows would be ſoon raiſed to a conſiderable height, as would appear more evidently, if we haſtily let in the external air, upon whoſe ingreſs the baſes would be clapped together, and the upper of them a good way depreſſed: ſo that the imperfection of the bellows made the experiment rather more than leſs concluding; for ſince there was no external force applied to open them, if notwithſtanding that ſome of the included air could get out of them, yet the ſpring of the internal air was ſtrong enough to open the bellows, when the ambient air was withdrawn, much more would the effect have been produced, if the bellows had been perfectly ſtanch.

EXPERIMENT XXXVIII.

About an attempt to examine the motions and ſenſibility of the Carteſian Materia ſubtilis, *or the* Æther, *with a pair of bellows made of a bladder, in the exhauſted receiver.*

I will not now diſcuſs the controverſy betwixt ſome of the modern atomiſts and the Carteſians; the former of whom think, that betwixt the earth and the ſtars, and betwixt theſe themſelves, there are vaſt tracts of ſpace that are empty, ſave where the beams of light do paſs through them; and the latter of whom tell us, that the intervals betwixt the ſtars and planets, among which the earth may perhaps be reckoned, are perfectly filled, but by a matter far ſubtiler than our air, which ſome call celeſtial, and others æther. I ſhall not, I ſay, engage in this controverſy; but thus much ſeems evident, that if there be ſuch a celeſtial matter, it muſt make up far the greateſt part of the univerſe known to us. For the interſtellar part of the world, if I may ſo ſtile it, bears ſo very great a proportion to the globes, and their atmoſpheres too, if other ſtars have any, as well as the earth, that it is almoſt incomparably greater in reſpect of them, than all our atmoſphere is in reſpect of the clouds, not to make the compariſon between the ſea and the fiſhes that ſwim in it.

Wherefore I thought it might very well deſerve a heedful inquiry, whether we can by ſenſible experiments (for I hear what has been attempted by ſpeculative arguments) diſcover any thing about the exiſtence, or the qualifications of this ſo vaſt æther; and I hoped our curioſity might be ſomewhat aſſiſted by our engine, if I could manage in it ſuch a pair of bellows as I deſigned: for I propoſed to myſelf to faſten a convenient weight

weight to the upper basis, and clog the lower with another great enough to keep it horizontal and immoveable; that when by the help of the turning-key frequently above mentioned, the upper basis should be raised to its full height, the cavity of the bellows might be brought to its full dimensions: this done, I intended to exhaust the receiver, and consequently the thus opened bellows, with more than ordinary diligence, that so both the receiver and they might be carefully freed from air: after which I purposed to let go the upper base of the bellows, that, being hastily depressed by the incumbent weight, it might speedily enough fall down to the lower basis, and by so much, and so quickly lessening the cavity, might expel thence the matter (if any where) before contained in it, and that (if it could by this way be done) at the hole of a slender pipe fastened either near the bottom of the bellows, or in the upper basis; against, or over the orifice, of which pipe there was to be placed at a convenient distance, either a feather, or (if that should prove too light) the sail of a little windmill made of cards, or some other light body, and fit to be put into motion by the impulse of any matter that should be forced out of the pipe.

By this means it seemed not improbable that some such discovery might be made, as would not be altogether useless in our inquiry. For if, notwithstanding the absence of the air, it should appear by the effects, that a stream of other matter capable to set visible bodies a moving, should issue out at the pipe of the compressed bellows, it would also appear that there may be a much subtiler body than common air, and as yet unobserved by the vacuists, or (their adversaries) the schools, that may even copiously be found in places deserted by the air; and that it is not safe to conclude from the absence of the air in our receivers, and in the upper part of those tubes where the Torricellian experiment is made, that there is no other body left but an absolute vacuity, or (as the atomists call it) a *vacuum coacervatum*. But if, on the other side, there should appear no motion at all to be produced, so much as in the feather, it seemed that the vacuists might plausibly argue, that either the cavity of the bellows was absolutely empty, or else that it would be very difficult to prove by any sensible experiment that it was full; and if, by any other way of probation, it be demonstrable that it was replenished with æther, we, that have not yet declared for any party, may by our experiment be taught to have no confident expectations of easily making it sensible by mechanical experiments; and may also be informed, that it is really so subtle and yielding a matter that does not either easily impel such light bodies as even feathers, or sensibly resist, as does the air itself, the motions of other bodies through it, and is able, without resistance, to make its passage through the pores of wood and leather, and also of closer bodies, which we find not that the air doth in its natural or wonted state penetrate.

To illustrate this last clause, I shall add, that to make the trial more accurate, I waved the use of other bellows (especially not having such as I desired) and caused a pair of small bellows to be made with a bladder, as a body, which some of our former experiments have evinced to be of so close a texture, that air will rather break it than pass through it; and that the bladder might no where lose its entireness by seams, we glued on the two bases, the one to the bottom and the other to the opposite part of it, so that the neck came out at a hole purposely made for it in the upper basis; and into the neck it was easy to insert what pipe we thought fit, binding the neck very close to it on the outside. We had likewise thoughts to have another pair of tight bellows made with a very light clack in the lower basis, that by hastily drawing up the other basis, when the receiver and bellows were very carefully exhausted, we might see by the rest, as the lifting up of the clack, whether the subtle matter that was expelled by the upper basis in its aſcent would, according to the modern doctrine of the circle made by moving bodies, be impelled up or not.

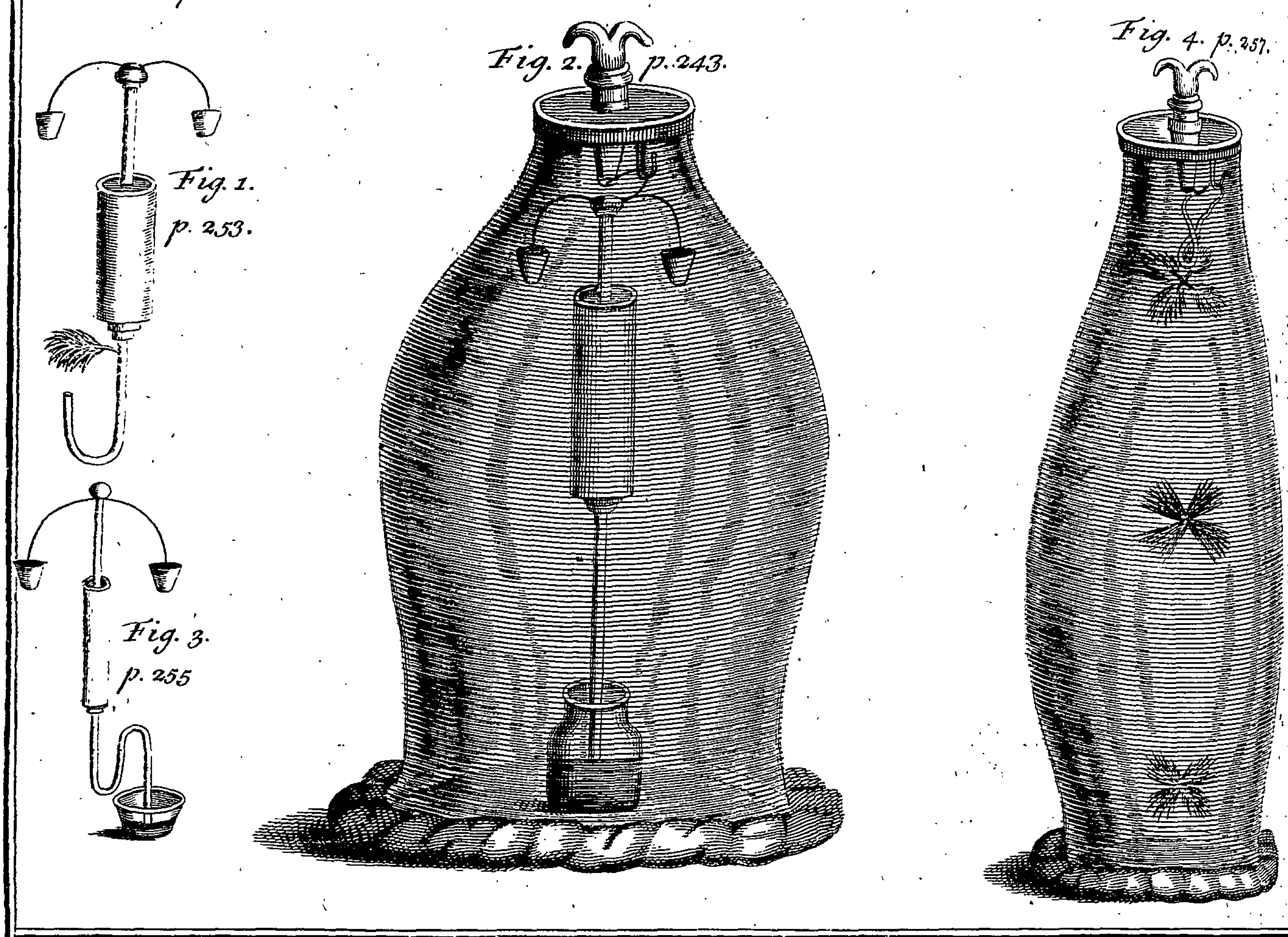
We alſo thought of placing the little pipe of the bladder-bellows (if I may ſo call them) beneath the ſurface of water exquiſitely freed from air, that we might ſee, whether upon the depreſſion of the bellows by the incumbent weight, when the receiver was carefully exhauſted, there would be any thing expelled at the pipe that would produce bubbles in the liquor wherein its orifice was immerſed.

To bring now our conjectures to ſome trial, we put into a capped receiver the bladder accommodated as before is mentioned; and though we could have wiſhed it had been ſomewhat larger, becauſe it contained but between half a pint and a pint, yet in regard it was fine and limber, and otherwiſe fit for our turn, we reſolved to try how it would do; and to depreſs the upper baſis of theſe little bellows the more eaſily and uniformly, we covered the round piece of paſteboard that made the upper baſis with a pewter-plate (with a hole in it for the neck of the bladder) which nevertheleſs, upon trial, proved not ponderous enough, whereby we were obliged to aſſiſt it by laying on it a weight of lead. And to ſecure the above-mentioned feather (which had a ſlender and flexible ſtem, and was left broad at one end, and faſtened by cement at the other, ſo as to ſtand with its broad end at a convenient diſtance juſt over the orifice of the pipe) from being blown aſide to either hand, we made it to move in a perpendicular ſlit in a piece of paſteboard that was faſtened to one part of the upper baſis, as that which the feather was glued to was to another part. Theſe things being thus provided, the pump was ſet a-work; and as the ambient air was from time to time withdrawn, ſo the air in the bladder expanded itſelf ſo ſtrongly, as to lift up the metalline weight, and yet in part to ſally out at the little glaſs-pipe of our bellows, as appeared by its blowing up the feather and keeping it ſuſpended till the ſpring of the air in the bladder was too far weakened to continue to do as it had done. In the mean time we did now and then, by the help of a ſtring faſtened to the turning-key and the upper baſis of the bellows, let down that baſis a little, to obſerve how upon its ſinking the blaſt againſt the feather would decreaſe as the receiver was further and further exhauſted: and when we judged it to be ſufficiently freed from air, we then let down the weight, but could not perceive that by ſhutting of the bellows, the feather was at all blown up, as it had been wont to be, though the upper baſis were more than uſually depreſſed: and yet it ſeems ſomewhat odd, that when, for curioſity, in order to a further trial, the weight was drawn up again, as the upper baſis was raiſed from the lower, the ſides of the bladder were ſenſibly (though not very much) preſſed, or drawn inwards. The bellows being thus opened, we let down the upper baſis again, but could not perceive that any blaſt was produced; for though the feather that lay juſt over and near the orifice of the little glaſs pipe had ſome motion, yet this ſeemed plainly to be but a ſhaking and almoſt vibrating motion (to the right and left hand) which it was put into by the upper baſis, which the ſtring kept from a ſmooth and uniform deſcent, but not to proceed from any blaſt iſſuing out of the cavity of the bladder: and for further ſatisfaction we cauſed ſome air to be let into the receiver, becauſe there was a poſſibility, that unawares to us the ſlender pipe might by ſome accident be choaked; but though upon the return of the air into the receiver, the baſes of the bellows were preſt cloſer together, yet it ſeemed, that, according to our expectation, ſome little air got through the pipe into the cavity of the bladder: for when we began to withdraw again the air we had let into the receiver, the bladder began to ſwell again, and upon our letting down the weight, to blow up and keep up the feather, as had been done before the receiver had been ſo well exhauſted.

See Plate VI. Fig 7.

What

VOL. III. Pl. 7.



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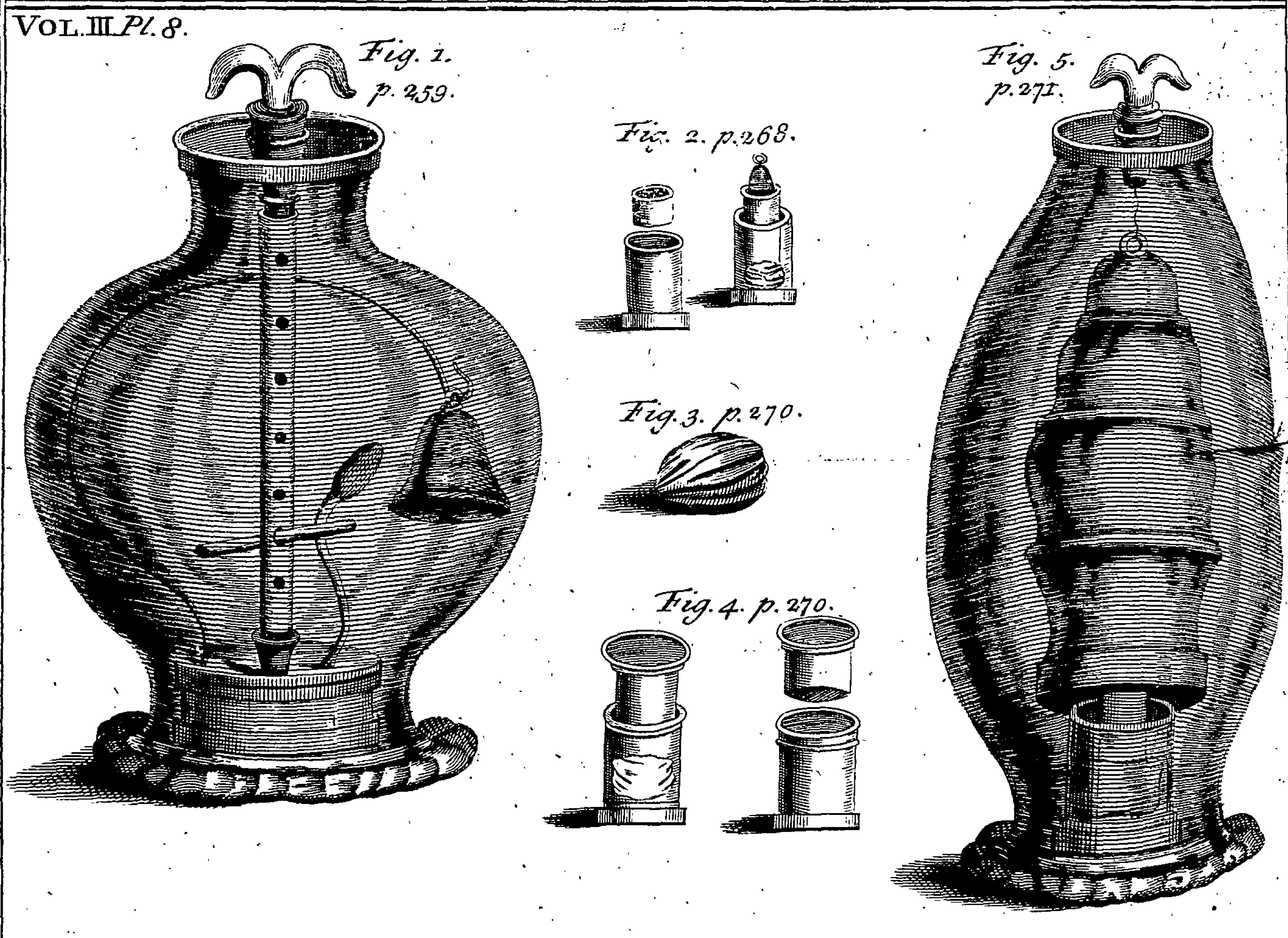

What conjecture the opening and shutting of our little bellows, more than once or twice, without producing any blast sensible by the raising of the feather, gave some of the by-standers, may be easily guessed by the preamble of this experiment; but whilst I was endeavouring to prosecute it for my own farther information, a mischance that befel the instrument kept me from giving myself the desired satisfaction.

EXPERIMENT XXXIX.

About a further attempt to prosecute the inquiry proposed in the foregoing Experiment.

Considering with myself, that by the help of some contrivances not difficult, a syringe might be made to serve, as far as our present occasion required, instead of a pair of bellows; I thought it would not be improper to try a differing, and, in some regards, a better way to prosecute an attempt which seemed to me to deserve our curiosity.

I caused then to be made for the formerly mentioned syringe, instead of its straight pipe, a crooked one, whose shorter leg was parallel to the longer; and this pipe was for greater closeness, after it was screwed on carefully, fastened with cement to the barrel; and because the brass-pipe could scarce be made small enough, we caused a short and very slender pipe of glass to be put into the orifice of the shorter leg, and diligently fastened to it with close cement: then we caused the sucker (by the help of oil, water, and moving it up and down) to be made to go as smoothly as might be, without lessening the stanchness of the syringe. After this there was fastened to the handle of the rammer a weight, made in the form of a ring or hoop, which, by reason of its figure, might be suspended from the newly mentioned handle of the rammer, and hang loose on the outside of the cylinder, and which, both by its figure and its weight, might evenly and swiftly enough depress the sucker, when that being drawn up the weight should be let go. This syringe, thus furnished, was fastened to a broad and heavy pedestal, to keep it in its vertical posture, and to hinder it from tottering, notwithstanding the weight that clogged it. And besides all these things, there was taken a feather which was about two inches long, and of which there was left at the end a piece about the breadth of a man's thumb-nail (the rest on either side of the slender stalk, if I may so call it, being stript off) to cover the hole of the slender glass-pipe of the syringe; for which purpose the other extreme of it was so fastened with cement to the lower part of the syringe (or to its pedestal) that the broad end of the feather was placed (as the other feather was in the foregoing experiment) just over the little orifice of the glass, at such a convenient distance, that when the sucker was a little (though but very little) drawn up and let go again, the weight would depress it fast enough to blow up the broad part of the feather, as high as was permitted by the resistance of the stalk (and that was a good way) the spring of which would presently restore the whole feather to its former position.

See Plate VII. Fig. 1.

All these things being done, and the handle of the rammer being tied to the turning-key of a capped receiver, the syringe and its pedestal were inclosed in a capacious receiver (for none but such an one could contain them, and give scope for the rammer's motions) and the pump being set on work, we did, after some quantity of air was drawn out, raise the sucker a little by the help of the turning-key, and then turning the same key the contrary way, we suffered the weight to depress the sucker, that we might see at what rate the feather would be blown up; and finding that it was impelled forcibly enough, we caused the pumping to be so continued that a pretty many pauses were made,

made, during each of which we raised and depressed the sucker as before, and had the opportunity to observe, that as the receiver was more and more exhausted of the air, so the feather was less and less briskly driven up, till at length, when the receiver was well emptied, the usual elevations and depressions of the sucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully myself, but I made one, whom I had often employed about pneumatical experiments, to watch attentively, whilst I drew up and let down the sucker; but he affirmed that he could not discern the least beginning of ascension in the feather. And indeed to both of us it seemed that the little and inconsiderable motion that was sometimes (not always) to be discerned in the feather, proceeded not from any thing that issued out of the pipe, but from some little shake, which it was difficult not to give the syringe and pedestal, by the raising and depressing of the sucker.

And that which made our phænomenon the more considerable was, that the weight that carried down the sucker being still the same, and the motions of the turning-key being easy to be made equal at several times, there seemed no reason to suspect that contingencies did much (if at all) favour the success; but there happened a thing which did manifestly enough disfavour it. For I remember, that before the syringe was put into the receiver, when we were trying how the weight would depress it, and it was thought, that though the weight were conveniently shaped, yet it was a little of the least, I would not alter it, but foretold, that when the air in the cavity of the syringe (that now resisted the quickness of its descent, because so much air could not easily and nimbly get out at so small a pipe) should be exhausted with the other air of the receiver, the elevated sucker would fall down more easily, which he that was employed to manage the syringe whilst I watched the feather, affirmed himself afterwards to observe very evidently: so that when the receiver was exhausted, if there had been in the cavity of the syringe a matter as fit as air to make a wind of, the blast ought to have been greater, because the celerity that the sucker was depressed with was so.

After we had long enough tried in vain to raise the feather, I ordered some air to be let into the receiver; and though when the admitted air was but very little, the motions of the sucker had scarce, if at all, any sensible operation upon the feather, yet when the quantity of air began to be somewhat considerable, the feather began to be a little moved upwards, and so by letting in air not all at once, but more and more from time to time, and by moving the sucker up and down in the intervals of those times of admission, we had the opportunity to observe, that as the receiver had more air in it, the feather would be more briskly blown up.

But not content with a single trial of an experiment of this consequence, we caused the receiver to be again exhausted, and prosecuted the trial with the like success as before, only this one circumstance that we added, for confirmation, may be fit to be here taken notice of. Having, after the receiver was exhausted, drawn up and let fall the sucker divers times ineffectually, though hitherto we had not usually raised it any higher at a time, than we could by one turn of the hand, both because we could not so conveniently raise it higher by the hand alone, and because we thought it unnecessary, since that height sufficed to make the air briskly toss up the feather; yet *ex abundanti* we now took an instrument that was pretty long, and fit so to take hold on the turning-key, that we could easily raise the sucker between two and three inches, by our estimate, at a time, and nimbly depress it again; and for all this, which would much have increased the blast, if there had been a matter fit for it in the cavity of the syringe, we could not sensibly blow up the feather till we had let a little air into the receiver.

To

To be able to make an eſtimate of the quantity of air pumped out, or let in, when the feather was ſtrongly or faintly, or not at all raiſed by the fall of the ſucker, we took off the receiver, and conveyed a gage into it, but though for a while we made ſome uſe of our gage, yet a miſchance befalling it before the operation was quite ended, I ſhall forbear to add any thing concerning that trial, and proceed to ſay ſomething of another attempt, wherein, though I foreſaw and met with ſuch difficulties, as kept me from doing altogether what I deſired, yet the ſucceſs being almoſt as good as could be expected, I ſhall venture to acquaint your lordſhip with the trial, which was this.

Instead of the hitherto employed pipe of braſs, there was well faſtened, with cement, to the ſyringe, a pipe of glaſs, whoſe figure differed from that of the other in this particular, that the ſhorter or remoter leg of our new pipe, after it had for a while been carried parallel to the other leg, was bent off ſo, that above an inch and a half of it tended downwards, that the orifice of it might be immerſed into water contained in a ſmall open jar: the deſign of which contrivance was, that when the receiver ſhould be well exhauſted, we might, according to what I told your lordſhip was at firſt deſigned, try whether by the raiſing and depreſſing of the ſucker any ſuch matter would be driven out at the noſe of the pipe, as would produce bubbles in the incumbent water; which air (though highly rarefied, perhaps to ſome hundreds of times beyond its wonted dimenſions) is capable of doing: and I chooſe to employ rather water than quickſilver, becauſe, though by uſing the latter, I might hope to be leſs troubled with bubbles, yet the ponderouſneſs and opacity of it ſeemed to outweigh that convenience. See Plate VII. Fig. 3.

I need not tell your lordſhip, that in other reſpects this experiment was made like the former; ſo that I ſhall mention only its peculiarities, which were, that as the air was pumped out of the receiver, that in the glaſs-pipe made its way through the water in bubbles; and a little air having once by a ſmall leak got in, and forced ſome of the water out of the jar into the pipe, when the receiver was again well emptied, both that water, and even the little quantity of ſtagnant water that was contained in the immerſed part of the pipe, produced ſo many bubbles of ſeveral ſizes, as quite diſturbed our obſervations. Wherefore we let alone the receiver, exhauſted as it was, for ſix or ſeven hours, to give the water time to be freed from air; and then cauſing what air might have ſtolen in to be again pumped out, till we had perceived by the gage that the receiver was well exhauſted, we cauſed the ſucker of the ſyringe to be raiſed and depreſſed divers times; and though even then a bubble would now and then make our obſervations troubleſome and leſs certain, yet it ſeemed to us, that when we were not thus confounded, we ſometimes obſerved that the elevation and fall of the ſucker, though reiterated, did not drive out at the pipe any thing that made any diſcernible bubbles in the incumbent water; for though there would appear now and then ſome ſmall bubbles on the ſurface of the water, yet I could not perceive that the matter that made them iſſued out at the pipe; and ſome of them manifeſtly proceeded from aërial particles, till then lurking in the water, as I concluded from the place and time of their riſing. But this non eruption of bubbles at the noſe of the pipe was not that which gave me the moſt ſatisfaction; for at length both I and another had the opportunity to obſerve the water in the immerſed part of the pipe, which was very ſlender, to be about an inch higher than the reſt of the ſtagnant water, and to continue at that height or place in the pipe, though the ſucker were divers times together raiſed and depreſſed, by gueſs, between two and three inches at a time; which ſeemed to argue either that there was a vacuum in the cavity of the ſyringe, or elſe, that if it were full of æther, that body was ſo ſubtle that the impulſe it received from the falling ſucker would not make it diſplace a very little thread (perhaps not exceeding a grain in weight) of water that was in the ſlender pipe, though it

appeared

appeared by the bubbles, that sometimes disclosed themselves in the water, after the receiver had been exhausted, that far more water would be displaced and carried up by a small bubble, consisting of such rarefied air, that according to my estimate, the aërial particles of it did not, before the pump was begun to be set on work, take up in the water a five-hundredth part of the quantity of a pin's head.

But whilst we were considering what to do further in our trial, a little air that strained in at some small undiscoverable leak, drove the water into the emptied part of the pipe, and put an end for that time to our trial, which had been too toilsome to invite us then to reiterate it.

I had indeed thoughts of prosecuting the inquiry by dropping from the top of the exhausted receiver light bodies conveniently shaped, to be turned round or otherwise put out of their simplest motion of descent, if they met with any resistance in their fall; and by making such bodies move horizontally and otherwise in the receiver, as would probably discover whether they were assisted by the medium. And other contrivances and ways I had in my thoughts, whereby to prosecute our enquiry; but wanting time for other experiments, I could not spare so much as was necessary to exhaust large receivers so diligently as such nice trials would exact; and therefore I resolved to desist till I had more leisure than I then had, or have since been master of.

In the interim, thus much we seem to have already discovered by our past trials, that if when our vessels are very diligently freed from air, they are full of æther, that æther is such a body as will not be made sensibly to move a light feather by such an impulse as would make the air manifestly move it, not only whilst it is no thinner than common air, but when it is very highly rarefied (which, if I mistake not, it was in our experiment so much, as to be brought to take up above an hundred times more room than before).

And one thing more we gained by the trial made with water, namely, a clear confirmation of what I have delivered in the 34th experiment, about the cause of the suction that is made by syringes; for your lordship may remember, that at the close of the experiment we have all this while been reciting, I observed, that when the external air was so very well withdrawn, the pulling up of the sucker would not make the stagnant water that the pipe of the syringe was immersed in, to ascend one inch, or so much as the tenth part of it.

EXPERIMENT XL.

About the falling, in the exhausted receiver, of a light body, fitted to have its motion visibly varied by a small resistance of the air.

Partly to try, whether in the space deserted by the air, drawn out of our receivers, there would be any thing more fit to resist the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpose to be mentioned by and by, we made the following trials.

We took a receiver, which, though less tall than we would have had, was the longest we could procure; and that we might be able, not so properly to let down as to let fall a body in it, we so fastened a small pair of tobacco-tongs to the inside of the receiver's brass-cover, that by moving the turning-key we might, by a string tied to one part of them, open the tongs, which else their own spring would keep shut. This being done, the next thing was to provide a body which would not fall down like a stone, or another dead

dead weight through the air, but would, in the manner of its defcent, fhew, that its motion was fomewhat refifted by the air; wherefore, that we might have a body that would be turned about horizontally, as it were, in its fall, we thought fit to join crofs-wife four broad and light feathers (each about an inch long) at their quills with a little cement, into which we alfo ftuck perpendicularly a fmall label of paper, about an 8th of an inch in breadth, and fomewhat more in height, by which the tongs might take hold of our light inftrument without touching the cement, which elfe might ftick to them.

By the help of this fmall piece of paper the little inftrument, of which it made a part, was fo taken hold of by the tongs, that it hung as horizontal as fuch a thing could well be placed; and then the receiver being cemented on to the engine, the pump was diligently plied, till it appeared by a gage which had been conveyed in, that the receiver had been carefully exhaufted; laftly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little inftrument let fall, which, though in the air it had made fome turns in its defcent from the fame height which it now fell from, yet now it defcended like a dead weight, without being perceived by any of us to make fo much as one turn, or a part of it: notwithftanding which I did, for greater fecurity, caufe the receiver to be taken off and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhaufted, they made fome turns in their defcent, as they alfo did being a fecond time let fall after the fame manner. See Plate VII. Fig. 4.

But when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-ftanders could perceive any thing of turning in the defcent of the feathers; and yet for further fecurity we let them fall twice more in the unexhaufted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhaufted receiver, they fell after the fame manner as they had done formerly, when the air, that would by its re-fiftance have turned them round, was removed out of their way.

N. B. 1. Though, as I intimated above, the glafs wherein this experiment was made, were nothing near fo tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. One that had more leifure and conveniency might have made a more commodious inftrument than that we made ufe of; for being accidentally vifited by that fagacious mathematician Dr. *Wren*, and fpeaking to him of this matter, he was pleafed with great dexterity as well as readinefs to make me a little inftrument of paper, on which, when it was let fall, the refiftance of the air had fo manifeft an operation, that I fhould have made ufe of it in our experiment, had it not been cafually loft when the ingenious maker was gone out of thefe parts.

3. Though I have but briefly related our having fo ordered the matter that we could conveniently let fall a body in the receiver when very well exhaufted; yet, to contrive and put in practice what was neceffary to perform this, was not fo very eafy, and it would be difficult to defcribe it circumftantially without very many words; for which reafon I forbear an account that would prove too tedious to us both.

4. What has been hitherto related was done in profecution of but one of the two defigns I aimed at in the foregoing contrivance, by which I intended, if I could have procured a receiver tall enough, to try whether bodies (fome very light, and fome heavier) being let fall, when the air was very diligently pumped out, would not defcend fomewhat fafter than if the receiver were full of air: but though I had provided a pendulum that vibrated quarters of feconds, yet the glafs being no higher than it was, the

appeared by the bubbles, that ſometimes diſcloſed themſelves in the water, after the receiver had been exhauſted, that far more water would be diſplaced and carried up by a ſmall bubble, conſiſting of ſuch rarefied air, that according to my eſtimate, the aërial particles of it did not, before the pump was begun to be ſet on work, take up in the water a five-hundredth part of the quantity of a pin's head.

But whilſt we were conſidering what to do further in our trial, a little air that ſtrained in at ſome ſmall undiſcoverable leak, drove the water into the emptied part of the pipe, and put an end for that time to our trial, which had been too toilſome to invite us then to reiterate it.

I had indeed thoughts of proſecuting the inquiry by dropping from the top of the exhauſted receiver light bodies conveniently ſhaped, to be turned round or otherwiſe put out of their ſimpleſt motion of deſcent, if they met with any reſiſtance in their fall; and by making ſuch bodies move horizontally and otherwiſe in the receiver, as would probably diſcover whether they were aſſiſted by the medium. And other contrivances and ways I had in my thoughts, whereby to proſecute our enquiry; but wanting time for other experiments, I could not ſpare ſo much as was neceſſary to exhauſt large receivers ſo diligently as ſuch nice trials would exact; and therefore I reſolved to deſiſt till I had more leiſure than I then had, or have ſince been maſter of.

In the interim, thus much we ſeem to have already diſcovered by our paſt trials, that if when our veſſels are very diligently freed from air, they are full of æther, that æther is ſuch a body as will not be made ſenſibly to move a light feather by ſuch an impulſe as would make the air manifeſtly move it, not only whilſt it is no thinner than common air, but when it is very highly rarefied (which, if I miſtake not, it was in our experiment ſo much, as to be brought to take up above an hundred times more room than before).

And one thing more we gained by the trial made with water, namely, a clear confirmation of what I have delivered in the 34th experiment, about the cauſe of the ſuction that is made by ſyringes; for your lordſhip may remember, that at the cloſe of the experiment we have all this while been reciting, I obſerved, that when the external air was ſo very well withdrawn, the pulling up of the ſucker would not make the ſtagnant water that the pipe of the ſyringe was immerſed in, to aſcend one inch, or ſo much as the tenth part of it.

EXPERIMENT XL.

About the falling, in the exhauſted receiver, of a light body, fitted to have its motion viſibly varied by a ſmall reſiſtance of the air.

Partly to try, whether in the ſpace deſerted by the air, drawn out of our receivers, there would be any thing more fit to reſiſt the motion of other light bodies through it, than in the former experiment we found it to impel them into motion; and partly for another purpoſe to be mentioned by and by, we made the following trials.

We took a receiver, which, though leſs tall than we would have had, was the longeſt we could procure; and that we might be able, not ſo properly to let down as to let fall a body in it, we ſo faſtened a ſmall pair of tobacco-tongs to the inſide of the receiver's braſs-cover, that by moving the turning-key we might, by a ſtring tied to one part of them, open the tongs, which elſe their own ſpring would keep ſhut. This being done, the next thing was to provide a body which would not fall down like a ſtone, or another

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dead weight through the air, but would, in the manner of its defcent, fhew, that its motion was fomewhat refifted by the air; wherefore, that we might have a body that would be turned about horizontally, as it were, in its fall, we thought fit to join crofs-wife four broad and light feathers (each about an inch long) at their quills with a little cement, into which we alfo ftuck perpendicularly a fmall label of paper, about an 8th of an inch in breadth, and fomewhat more in height, by which the tongs might take hold of our light inftrument without touching the cement, which elfe might ftick to them.

By the help of this fmall piece of paper the little inftrument, of which it made a part, was fo taken hold of by the tongs, that it hung as horizontal as fuch a thing could well be placed; and then the receiver being cemented on to the engine, the pump was diligently plied, till it appeared by a gage which had been conveyed in, that the receiver had been carefully exhaufted; laftly, our eyes being attentively fixed upon the connected feathers, the tongs were by the help of the turning-key opened, and the little inftrument let fall, which, though in the air it had made fome turns in its defcent from the fame height which it now fell from, yet now it defcended like a dead weight, without being perceived by any of us to make fo much as one turn, or a part of it: notwithftanding which I did, for greater fecurity, caufe the receiver to be taken off and put on again, after the feathers were taken hold of by the tongs; whence being let fall in the receiver unexhaufted, they made fome turns in their defcent, as they alfo did being a fecond time let fall after the fame manner. See Plate VII. Fig. 4.

But when after this, the feathers being placed as before, we repeated the experiment by carefully pumping out the air, neither I nor any of the by-ftanders could perceive any thing of turning in the defcent of the feathers; and yet for further fecurity we let them fall twice more in the unexhaufted receiver, and found them to turn in falling as before; whereas when we did a third time let them fall in the well exhaufted receiver, they fell after the fame manner as they had done formerly, when the air, that would by its refiftance have turned them round, was removed out of their way.

N. B. 1. Though, as I intimated above, the glafs wherein this experiment was made, were nothing near fo tall as I would have had it, yet it was taller than any of our ordinary receivers, it being in height about 22 inches.

2. One that had more leifure and conveniency might have made a more commodious inftrument than that we made ufe of; for being accidentally vifited by that fagacious mathematician Dr. *Wren*, and fpeaking to him of this matter, he was pleafed with great dexterity as well as readinefs to make me a little inftrument of paper, on which, when it was let fall, the refiftance of the air had fo manifeft an operation, that I fhould have made ufe of it in our experiment, had it not been cafually loft when the ingenious maker was gone out of thefe parts.

3. Though I have but briefly related our having fo ordered the matter that we could conveniently let fall a body in the receiver when very well exhaufted; yet, to contrive and put in practice what was neceffary to perform this, was not fo very eafy, and it would be difficult to defcribe it circumftantially without very many words; for which reafon I forbear an account that would prove too tedious to us both.

4. What has been hitherto related was done in profecution of but one of the two defigns I aimed at in the foregoing contrivance, by which I intended, if I could have procured a receiver tall enough, to try whether bodies (fome very light, and fome heavier) being let fall, when the air was very diligently pumped out, would not defcend fomewhat fafter than if the receiver were full of air: but though I had provided a pendulum that vibrated quarters of feconds, yet the glafs being no higher than it was, the

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descent even of our feathers took up so little time, that even this pendulum was of no use; only it seemed to all of us that were present at making the above recited trials, that when the feathers were let fall at such times as the air that would have turned them round in their descent was removed, they came to the bottom sensibly sooner than at other times. But when we shall have opportunity to repeat the experiment in taller glasses, and to make some variation of it, I hope to be able to give your lordship a fuller satisfaction about this particular. And in the mean while I shall forbear to examine whether the air might somewhat retard the descent of the feathers upon some other account, or merely upon that of its being a medium not quite devoid of gravity.

ANNOTATIONS.

1. BUT here I must be so sincere as to inform your lordship, that this fortieth experiment seemed not to prove so much as did the foregoing made with the syringe; for being suspicious, that, to make the feathered body above mentioned turn in its fall, there would need a resistance not altogether inconsiderable, I caused the experiment to be repeated, when the receiver was, by our estimate, little or nothing more than half exhausted, and yet the remaining air was too far rarefied to make the falling body manifestly turn.

2. AND yet perchance it would have happened otherwise, if the receiver had been tall enough; which, though I had not then leisure and conveniency to make it, yet it will not be amiss to let your lordship know by what means we did, that it might be somewhat fit to make the recited experiment and some others, bring it to the height it had, which did considerably exceed that of the tallest glass we could then procure.

To lengthen our receiver, therefore, we thought fit to try, whether we could not close enough fasten to the bottom of it with very good cement a cylindrical pipe of latten whose upper orifice should have near the same breadth with the bottom of the glass: and though this contrivance seemed liable to a couple of not mean difficulties; the one, that the latten being every where bended, and in some places necessary to be soddered, it would be very hard, as indeed we found it, to avoid some small cracks and leaks; and the other, that if the metalline pipe were wide enough, so great and heavy a pillar of the atmosphere would come to bear against it as to press it inwards, if not also to break it; yet we hoped we should be able to obviate both of these inconveniencies. Against the first of which our remedy was, to coat over very carefully the whole pipe with the same close cement wherewith we fastened it to the glass receiver: and against the second, we provided a little frame, consisting of divers small iron bars fastened together; which frame (though it were not too wide to go into the cylinder of latten, yet it) was wide enough to be so near it on the inside, that (though the weight of the atmosphere should, as we feared, press the latten so as to make it yield inward, yet) it could make it bend no further than the iron-frame would permit; which was not far enough to spoil either the receiver or the experiment: and this not unpleasant phænomenon would somewhat surprize unaccustomed spectators, that when after the receiver had been very well exhausted, the external air was permitted to return, there would be heard during some time, from the metalline part of the receiver, divers sounds brisk enough, which would make an odd cracking noise proceeding from the latten-plate, which having been forcibly, though but slowly, bent inwards by the predominant pressure of the atmosphere, was now assisted by the pressure of the returning air to regain its former figure: and as I thought not fit to omit this circumstance, because it confirms the practicableness of the remedy proposed against the second inconvenience; so I thought fit to mention this way of enlarging

larging and heightening receivers, becaufe what we have related feems to give grounds of hoping that this contrivance may be made good ufe of in divers other trials, and particularly in attempts to make receivers capacious enough to contain larger animals, and perhaps even a boy or a man. In order to fome of which purpofes we endeavoured to get an improvement made of our metalline cylinder by additional contrivances; but could not, where we then were, get artificers that would perform what was directed.

EXPERIMENT XLI.

About the propagation of founds in the exhaufted receiver.

To make fome further obfervation than is mentioned in the * publifhed experiments, about the production and conveying of founds in a glafs whence the air is drawn out, we employed a contrivance, of which, becaufe we make ufe of it in divers other experiments, it will be requifite to give your lordfhip here fome fhort defcription.

We caufed to be made at the turner's a cylinder of box, or the like clofe and firm wood, and of a length fuitable to that of the receiver it was to be employed in. Out of the lower bafis of this cylinder (which might be about an inch and a half in diameter) there came a fmaller cylinder or axle-tree, not a quarter fo thick as the other, and lefs than an inch long; this was turned very true, that it might move to and fro; or, as the tradefmen call it, ride very fmoothly in a little ferrule or ring of brafs, that was by the fame turner made for it in the midft of the fixed trencher (as we call a piece of folid wood, fhaped like a mill-ftone) being four or five inches, more or lefs (according to the widenefs of the receiver) in breadth, and between one and two in thicknefs; and in a large and round groove or gutter, purpofely made in the lower part of this trencher, I caufed as much lead as would fill it up to be placed and faftened, that it might keep the trencher from being eafily moved out of its place or pofture, and in the upper part of this trencher it was intended that holes fhould be made at fuch places as fhould be thought fit, to place bodies at feveral diftances as occafion fhould require. The upper bafis of the cylinder had alfo coming out of the midft of it another axle-tree, but wider than the former, that, into a cavity made in it, it might receive the lower end of the turning-key divers times already mentioned, to which it was to be faftened by a flender peg of brafs thruft through two correfpondent holes, the one made in the key, and the other in the newly-mentioned focket (if I may fo call it) of the axle-tree. Befides all which, there were divers horizontal perforations bored here and there in the pillar itfelf, to which this axis belonged, which pillar we fhall, to avoid ambiguity, call the vertical cylinder. The general ufe of this contrivance (whofe other parts need not to be mentioned before the experiments where they are employed) is, that the end of the turning-key being put into the focket, and the lower axis of the vertical cylinder into the trencher, by the motion of the key a body faftened at one of the holes to the cylinder may be approached to, or removed from, or made to rub or ftrike againft another body faftened in a convenient pofture to the upper part of the trencher.

See Plate VIII. Fig. 1.

To come now to our trial about founds, we caufed a hand-bell (whofe handle and clapper were taken away) to be faftened to a ftrong wire, that, one end of the wire being made faft in the trencher, the other end, which was purpofely bent downwards, took hold of the bell. In another hole made in the circumference of the fame trencher was

See the figure laft referred to.

* Page 105, 106,

wedged in (with a wooden peg) a steel-spring, to whose upper part was tied a gad of iron or steel, less than an inch long, but of a pretty thickness. The length of this spring was such, as to make the upper part of the hammer (if I may so call the piece of iron) of the same height with the bell, and the distance of the spring from the bell was such, that when it was forced back the other way, it might at its return make the hammer strike briskly upon the outside of the bell.

The trencher being thus furnished and placed in a capped receiver (as you know, for brevity sake, we use to call one that is fitted with one or other of the brass covers, often mentioned already) the air was diligently pumped out; and then, by the help of the turning-key, the vertical cylinder was made to go round, by which means as often as either of a couple of stiff wires or small pegs that were fastened at right angles into holes, made not far from the bottom of the cylinder, passed (under the bell, and) by the lately mentioned spring, they forcibly did in their passage bend it from the bell, by which means, as soon as the wire was gone by, and the spring ceased to be pressed, it would fly back with violence enough to make the hammer give a smart stroke upon the bell: and by this means we could both continue the experiment at discretion, and make the percussions more equally strong, than it would otherwise have been easy to do.

The event of our trial was, that, when the receiver was well emptied, it sometimes seemed doubtful, especially to some of the by-standers, whether any sound were produced or no; but to me, for the most part, it seemed, that after much attention I heard a sound, that I could but just hear; and yet, which is odd, methought it had somewhat of the nature of shrilness in it, but seemed (which is not strange) to come from a good way off. Whether the often turning of the cylindrical key kept the receiver from being so stanch as else it would have been, upon which score some little air might insinuate itself, I shall not positively determine; but to discover what interest the presence or the absence of the air might have in the loudness or lowness of the sound, I caused the air to be let into the receiver, not all at once, but at several times, with competent intervals between them; by which expedient it was easy to observe, that the vertical cylinder being still made to go round, when a little air was let in, the stroke of the hammer upon the bell (that before could now and then not be heard, and for the most part be but very scarcely heard) began to be easily heard; and when a little more air was let in, the sound grew more and more audible, and so increased, until the receiver was again replenished with air; though even then (that we omit not that phænomenon) the sound was observed to be much less loud than when the receiver was not interposed between the bell and the ear.

And whereas in the already published physico-mechanical experiments, I acquainted your lordship with what I observed about the sound of an ordinary watch in the exhausted receiver, I shall now add, that that experiment was repeated not long since, with the addition of suspending in the receiver a watch with a good alarum, which was purposely so set, that it might, before it should begin to ring, give us time to cement on the receiver very carefully, exhaust it very diligently, and settle ourselves in a silent and attentive posture. And to make this experiment in some respect more accurate than the others we made of sounds, we secured ourselves against any leaking at the top, by imploying a receiver that was made all of one piece of glass (and consequently had no cover cemented on to it) being furnished only within (when it was first blown) with a glass-knob or button, to which a string might be tied. And because it might be suspected, that if the watch were suspended by its own silver chain, the tremulous motion of its sounding bell might be propagated by that metalline chain to the upper part of the

the glaſs, to obviate this as well as we could, we hung the watch, not by its chain, but a very ſlender thread, whoſe upper end was faſtened to the newly mentioned glaſs-button.

These things being done, and the air being carefully pumped out, we ſilently expected the time, when the alarum ſhould begin to ring, which it was eaſy to know by the help of our other watches; but not hearing any noiſe ſo ſoon as we expected, it would perhaps have been doubted whether the watch continued going, if for prevention we had not ordered the matter ſo, that we could diſcern it did not ſtand ſtill: wherefore I deſired an ingenious gentleman to hold his ear juſt over the button at which the watch was ſuſpended, and to hold it alſo very near to the receiver; upon which he told us, that he could perceive, and but juſt perceive ſomething of ſound that ſeemed to come from far; though neither we that liſtened very attentively near other parts of the receiver, nor he, if his ears were no more advantaged in point of poſition than ours, were ſatisfied that we heard the watch at all. Wherefore ordering ſome air to be let in, we did, by the help of attention, begin to hear the alarum, whoſe ſound was odd enough, and, by returning the ſtop-cock to keep any more air from getting in, we kept the ſound thus low for a pretty while, after which a little more air, that was permitted to enter, made it become more audible; and when the air was yet more freely admitted, the by-ſtanders could plainly hear the noiſe of the yet continuing alarum at a conſiderable diſtance from the receiver.

From what has hitherto been related, we may learn what is to be thought of what is delivered by the learned *Merſennus*, in that book of his Harmonicks, where he makes this to be the firſt propoſition. *Sonus à campanis, vel altis corporibus non ſolum producitur in illo vacuo (quicquid tandem illud ſit) quod fit in tubis hydrargyro plenis, poſteaque depletis, ſed etiam idem acumen, quod in aere libero vel clauſo penitus obſervatur & auditur.* For the proof of which aſſertion, not long after, he ſpeaks thus: *porro variis tubis, quorum extremis lagenæ vitreæ adglutinantur, obſervari campanas in illo vacuo appenſas propriiſque malleis percuſſas idem penitus acumen retinere, quod in aere libero habent: atque ſoni magnitudinem ei ſono, qui fit in aere quem tubus clauſus includit, nihil cedere.* But though our experiments ſufficiently manifeſt, that the preſence or abſence of the common air is of no ſmall importance as to the conveying of ſounds, and that the interpoſition of glaſs may ſenſibly weaken them; yet ſo diligent and faithful a writer as *Merſennus* deſerves to be favourably treated; and therefore I ſhall repreſent on his behalf, that what he ſays may well enough have been true, as far as could be gathered from the trials he made. For, firſt, it is no eaſy matter, eſpecially for thoſe that have not peculiar and very cloſe cements, to keep the air quite out for any conſiderable time in veſſels conſiſting of divers pieces, ſuch as he appears to have made uſe of; and next, the bigneſs of the bell in reference to the capacity of the exhauſted glaſs, and the thickneſs of the glaſs, and the manner whereby the bell was faſtened to the inſide of the glaſs, and the hammer or clapper was made to ſtrike, may much vary the effect of the trial, for reaſons eaſy to be gathered out of the paſt diſcourſe, and therefore not needful to be here inſiſted on. And upon this account we choſe to make our experiment with ſounds that ſhould not be ſtrong or loud, and to produce them after ſuch a manner, as that as little ſhaking as could be might be given by the ſounding body to the glaſs it was included in. The propoſal made by the ſame *Merſennus*, to have thoſe that have induſtry enough try whether a bag-pipe will be made to afford the ſame ſound as in the open air, in ſuch veſſels as he uſed for his bells, though he ſeems to think it would ſucceed, is that which your lordſhip will not, I preſume, ſolicit me to make trial of, if you remember what is related in the almoſt immediately foregoing experiments, ſhewing, that

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we could make nothing come out of the cavity of a pair of bellows that had force enough to blow away a feather, when that cavity was freed from air; as the bagpipe would be by the ſame operation, that empties the glaſs that contains it, or elſe the ſound would not be made in ſuch a vacuum as the ſcope of the experiment requires.

If I had had conveniency, I would have made ſome trials by conveying a ſmall ſtringed inſtrument (perhaps ſome ſuch as they commonly call a kit) exactly tuned, into a large receiver, and then upon briſkly ſtriking the ſtring of a bigger inſtrument (tuned, as they ſpeak, to an uniſon to (or with) that of the ſmaller inſtrument) I ſhould have taken notice, whether the ſound would have been ſo uniformly propagated notwithſtanding the interpoſition of the glaſs receiver, as ſenſibly to ſhake the included ſtring; in order to the diſcerning of which, a bended piece of ſtraw or feather, or ſome ſuch light body, was to be horſed upon the ſtring to be ſhaken. I alſo intended, in caſe the ſtring were made to move, to make the like trial after the receiver was diligently exhauſted. And laſtly, I deſigned to try whether two uniſon ſtrings of the ſame inſtruments, or of a couple to be placed in the ſame receiver, would, when the air (which is the uſual medium of ſounds) was well pumped out, yet maintain ſuch a ſympathy, as it is called, that upon the motion of the one, the other would alſo be made to ſtir; which trials may be varied by employing for the external inſtrument another inſtead of a ſtringed one.

And becauſe contraries, as is vulgarly noted, ſerve to illuſtrate each other, I thought to ſubjoin, to the trials above related about the propagation of ſounds in a thinner medium than the air, ſome obſervations about the conveyance of them through that thicker medium, water; but having unluckily miſlaid my notes upon that ſubject, I cannot at preſent acquaint your lordſhip with what I intended, but muſt defer the doing it till I ſhall have recovered them.

EXPERIMENT XLII.

About the breaking of a glaſs-drop in an exhauſted receiver.

You know that among the cauſes that have been propoſed of the ſtrange flying of a glaſs-drop into a multitude of pieces, when the ſlender ſtem of it comes to be broken off, one of the leaſt improbable was taken from the preſſure of the air; as if that within the porous (and as it were honey-combed) inſide of the glaſs, being highly rarefied when the drop of melted glaſs fell into the water at its firſt formation, it was forced to continue in that preternatural ſtate of expanſion by the hardneſs and cloſeneſs of the external caſe of glaſs that incloſed the pith-like part (if I may ſo call it) ſo that upon the breaking off a part of this ſolid caſe at the ſtem, the external air gaining acceſs, and finding in the ſpungy part very little reſiſtance from the highly rarefied and conſequently weakened air included there, ruſhes in with ſuch violence as to ſhiver the glaſs-drop into a multitude of pieces.

I shall not now trouble your lordſhip with the mention of what may be alledged to queſtion this hypotheſis, eſpecially if it be compared with that accurate account of the phænomena of ſuch glaſs-drops, which was ſometimes ſince preſented to the ſociety by that great ornament of it Sir *Robert Moray*; but I ſhall only ſay in this place, that when I conſidered, that if the diſſilition of the glaſs would ſucceed when the air was pumped out of it, it would be hard to aſcribe that effect to the eruption of the external air. I thought fit to try what would happen if a glaſs-drop were broken in our exhauſted receiver,

receiver; and accordingly did, though not without ſome difficulty, ſo order the matter that the blunter part of the glaſs-drop was faſtened to a ſtable body (conveyed into the receiver) and the crooked ſtem was tied to one end of a ſtring, whoſe other end was faſtened to the turning-key; by which means, when the air had been diligently pumped out, the ſtem was (by ſhortening the ſtring) broken off, and the glaſs-drop was ſhattered into a thouſand pieces.

THIS experiment was long after repeated with the like ſucceſs; and having at that time no gage to try how far the air had been drawn out, we let the external air impel up the water out of the pump into the receiver, and thereby found that that veſſel had not been negligently exhauſted.

EXPERIMENT XLIII.

About the production of light in the exhauſted receiver.

I PRESUME, I need not put your lordſhip in mind, that divers attempts were made to try whether either a flame, or kindled coals, would be made to continue for ſome time burning in our receiver. But thoſe trials making it evident that it would be either impoſſible or very difficult to produce any durable light, without the preſence of the air, by the burning of bodies, I thought it not amiſs, conſidering the nobleneſs of light, to make trial, whether it might be otherwiſe produced in our exhauſted receiver; ſince whether or no the attempts ſhould prove ſucceſsful, the event would probably be inſtructive. For as it is the property of light, when it is produced, to be diſcoverable by itſelf; ſo in ſuch a trial as we intended, it would teach ſomething concerning light, to find that the abſence of the air would or would not hinder it from being produced. In proſecution of this deſign, knowing that hard ſugar, being nimbly ſcraped with a knife, will afford a ſparkling light, ſo that now and then one would think that ſparks of fire fly from it, we cauſed a good lump of hard loaf-ſugar to be conveniently and firmly placed in the cavity of our capped receiver, and to the vertical cylinder forementioned we cauſed to be faſtened ſome pieces of a ſteel-ſpring, which, being not very thick, might in their paſſage along the ſugar grate, or rub forcibly againſt it, and then the receiver being diligently exhauſted in the night-time, and in a dark room, the vertical cylinder (whoſe lower axis was inſerted into the often-mentioned trencher) was made for a pretty while to move round by the help of the turning-key, managed by a hand ſteady and ſtrong enough. By which means the irons that came out of the vertical cylinder, making in their paſſage vigorous impreſſions upon the ſugar that ſtood ſomewhat in their way, there were manifeſtly produced a good number of little flaſhes, and ſometimes too, though not frequently, there ſeemed to be ſtruck off little ſparks of fire.

The contrivance here mentioned may be conceived by conſidering the figure belonging to the 41 experiment.

EXPERIMENT XLIV.

About the production of a kind of halo and colours in the exhauſted receiver.

WE took a large inverted cucurbite for a receiver, which being ſo well wiped both within and without as to be very clear, allowed me to obſerve, and to make others do ſo too, that when the pump began to be ſet a work, if I cauſed a pretty large candle-to be held on the other side of the glass, upon the turning of the stop-cock to let the air out of the receiver into the cylinder, the glass would seem to be full of fumes, and there would appear about the flame of the candle, seen through them, a kind of halo, that at first commonly was between blue and green, and after some sucks would be of a reddish or orange colour, and both very vivid. The production of this meteor, if I may so call it, was, according to my conjecture, made on some such score as this: that the cement being somewhat soft and new, as is convenient for this experiment, abounds with turpentine; and having a little, as well to fasten on the receiver, as for the other purpose, applied to it a hot iron, whereby the cement was both softened and heated, it seemed rational to expect, that upon the withdrawing of the air in the receiver, the aërial particles in the cement, freed from their former pressure, would extricate themselves, and with the looser steams of the turpentine, and perhaps of the bees-wax, would with a kind of explosion expand themselves in the receiver, and by their interposition between the light and the eye exhibit those delightful colours we had seen. To confirm which, I afterwards found, that by watchfully observing it, I could plainly enough perceive the colouring steams, just upon the turning of the stop-cock, to fly up from the cement towards the top of the glass; and if we continued pumping, the receiver would grow clearer, and the colours more dilute, till we had occasion to put on the receiver, and heat the cement afresh; of which the reason might be, partly that the aërial and volatile particles of the upper part of the cement did in that tract of time spend themselves more and more; and partly, because the agitation they received from the heat communicated by the iron did continually decay: not to mention, that when the receiver is more exhausted, the want of air makes it more difficult for steams to be supported, and, as it were, swim up and down in it.

For farther confirmation, I caused some cement to be put into a small crucible warm enough to melt it; and conveying this into a clear receiver of a convenient shape and size, I caused the pump to be set a work; whereupon it appeared manifestly enough, that upon the opening of the stop-cock to let out the air, the steams would copiously be thrown about from the crucible into the capacity of the receiver, and would, after having a little played there, fall down again. But in these apparitions the vividness, and sometimes the kind of the exhibited colours, seemed much to depend upon divers circumstances, such as the degrees of heat, the bigness and shape of the receiver, the quantity of air that yet remained unpumped out, and the nature of the cement itself; which last particular I the rather mention, because, though I were hindered from doing it, I had thoughts to try a suspicion I had, that by varying the materials exposed to this kind of operation, some pretty variety might be made in the phænomena of the experiment.

Whether or no the apparition or whiteness, or light, that we sometimes happened to take notice of divers years ago, and have mentioned in the already published part of our Physico-mechanical Experiments, may be partly (though not entirely) referred to some of the cements I then employed, differing from those I now use most, and to the unheeded temper of those cements, as to warmth and degrees of softness, is a doubt that further observation may possibly enable us to determine.

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EXPERIMENT XLV.

About the production of heat by attrition in the exhausted receiver.

THE opinion that afcribes the incalefcence of folid bodies, ftruck or rubbed hard againft one another, to the attrition or vehement agitation of the intercepted air, is famous and received enough to feem worthy of a particular examination; but I confefs to your lordfhip, that it was not any thing relating to this opinion that chiefly induced me to make the experiment I am now about to give an account of; for I thought it might be ufeful to more purpofes than one to be able to produce, by attrition, a fomewhat durable heat, even in our exhaufted receiver; and therefore, though it were to forefee that it would prove no eafy tafk, yet we thought fit to attempt it fpight of the difficulties met with at our firft trials. In what way and with what fuccefs we afterwards made this attempt, I now proceed to relate.

CROSS the ftable trencher, formerly mentioned, there was faftened a pretty ftrong fpring of fteel or iron, fhaped almoft like the lath of a crofs-bow; and to the midft of this fpring was ftrongly faftened, on the outfide, a round piece of brafs hollowed almoft like a concave burning-glafs, or one of thofe tools wherein they ufe to grind eye-glaffes for telefcopes: to this piece of brafs, which was not confiderably thick, nor above two inches diameter, was fitted a convex piece of the fame metal, almoft like a gage for a tool to grind glaffes in, which had belonging to it a fquare handle, whereinto as into a focket was inferted a fquare piece of wood, proceeding from the bafis of a fquare wooden pillar, which we made ufe of on this occafion inftead of our vertical cylinder. By the help of another piece of wood coming from the other bafis of the fame pillar, the turning-key was joined to this pillar, which was made of fuch a length, that when the turning-key was forcibly kept down as low as the brafs cover it was a part of would permit, the convex piece of metal lately defcribed did deprefs the concave piece a pretty way, notwithftanding a vigorous refiftance of the fubjacent fpring. See Plate IV. Fig. 3.

BESIDES thefe things, a little fine powder of emery was put between the convex and concave pieces of brafs, to make them more congruous, and facilitate the motion that was to be made; and there was faftened to the upper part of the turning-key a good wimble, without which we prefumed the turning of the key would not produce a fufficient motion; in order to the making of which, it was, after the firft trial, judged requifite to have a ftrong man that was ufed to exercife his hands and arms in mechanical labours, upon which account we fent for a certain lockfmith that was a lufty and dexterous fellow.

ALL things that were thought neceffary being thus in readinefs, and a mercurial gage being conveyed into the receiver, we caufed the air to be diligently pumped out; and then the fmith was ordered to turn the wimble, and to continue to lean a little on it, that he might be fure to keep the turning-key from being at all lifted up by the former mentioned fpring.

WHILST this man with much nimblenefs and ftrength was moving the wimble, I watched the gage, to obferve whether the agitation of the ftop-cock, and confequently the engine, did not prejudice the experiment; and for greater caution I caufed the pump to be almoft all the while kept at work, though that feemed not fo neceffary.

WHEN the turner of the wimble was almoft out of breath, we let in for hafte the air at the cover of the receiver by lifting up the turning-key, and nimbly removing the

 receiver,

receiver, we felt the pieces of brass betwixt whom the attrition had been made, and, as we expected, found both of them very sensibly warm.

But being willing to confirm the experiment by a second trial, which we hoped might, after the experience taught us by the first, be somewhat better performed, we caused the smith, after he had well refreshed himself with rest and drink, to lay hold of the wimble again, when the gage made it appear that the receiver was well exhausted, so that by further pumping the quicksilver seemed not to be further depressed: and in this second trial the nimble smith played his part so well, the pump in the mean while not being neglected, that when we did as before hastily let in the air and take out the bodies that had been rubbed against one another, they were both of them, especially the uppermost, so hot that I could not endure to hold my hand on either of them, and they did for a considerable time retain a not inconsiderable degree of warmth.

The same day I caused to be made at the turner's two bodies of wood, for size and shape like those of brass we had just before employed; the upper of these was of hard oak, the other of beach, such a difference between woods, to be heated by mutual attrition, being thought to be an advantageous circumstance: but though the wimble was swiftly turned as before, and that by the same person, nevertheless the wood seemed not to me (for all the by-standers were not of my opinion) to have manifestly acquired any warmth; and yet that there had been a considerable attrition, appeared by the great polish which part of the wood had evidently acquired, which made me suspect that though the wood seemed dry enough, yet it might not really be so, notwithstanding the contrary was affirmed to me. But not being willing to sit down with a single trial, I caused the experiment to be repeated with more obstinacy than before; the effect of which was, that the wood, especially the upper piece of it, was brought to a warmth unquestionably sensible.

EXPERIMENT XLVI.

About the slacking of quick-lime in the exhausted receiver.

The several scopes I aimed at in making the following trial are not necessary to be here particularly taken notice of; but one of them may be guessed at by the subsequence of this experiment to that immediately foregoing, and the phænomena of it may be mentioned in this epistle, upon the account of their being exhibited by our engine.

We took in an evaporating glass a convenient quantity of water, and having conveyed it into a receiver and well drawn out the air, we let down into it by the turning-key a lump of strong lime about the bigness of a pippin, and observed not, that at the first immersion, nor for some while after, there appeared any considerable number of bubbles; but within about $\frac{1}{4}$ of an hour, as I guessed it, the lime began (the pump having been and being still plied from time to time) to slack with much violence, and with bubbles wonderfully great, that appeared at each new exsuction, so that the inside of the receiver, though pretty large, was at length lined with lime-water, and a great part of the mixture did from time to time overflow the vessel that had purposely been but little filled; nor did any thing but our weariness put a period to the bubbling of the mixture, whose heat was sensible, even on the outside of the receiver, and which continued considerably hot in the evaporating glass for $\frac{1}{4}$ of an hour, as I conjectured, after the receiver was removed.

Note,

Note, That the lime employed about this experiment was of a very good and ſtrong kind, made of hard ſtones, and not ſuch lime, made of chalk, as is commonly uſed at *London*, which probably would not have been ſtrong enough to have afforded us the ſame phænomenon.

EXPERIMENT XLVII.

About an attempt made to meaſure the force of the ſpring of included air, and examine a conjecture about the difference of its ſtrength in unequally broad-mouthed veſſels.

Though ſeveral of the foregoing trials have ſufficiently manifeſted that the ſpring of the air in its natural or wonted ſtate hath a force very conſiderable, and indeed much greater than men ſeem to have hitherto believed; yet I could not hope by any of theſe experiments to determine by any known weight how great that force is, ſo as to conclude that it is equivalent to ſuch a weight, as ſo many pounds, ounces, &c. and to no more. Wherefore, among the uſes I had deſigned to make of our ſyringe formerly often mentioned, it was one to try if by the help of that inſtrument we could determine ſomewhat near (for no more was to be expected) how much weight a cylinder of uncompreſſed air included in it, and conſequently of the ſame diameter with the cavity of the barrel, would be able to ſuſtain, or alſo to lift up.

In order to this trial, 1. we provided a ſtable pedeſtal or frame, wherein the ſyringe might be kept firm and erected; next we alſo provided a weight of lead ſhaped like our braſs-hoop, or ring formerly deſcribed, that by the advantage of its figure it might Exper. V. be made to hang down by ſtrings from the top of the handle of the rammer, and ſo preſs evenly enough on all ſides, without making the upper part of the inſtrument top-heavy. 2. We took care to leave between the bottom of the ſyringe (which was firmly cloſed with ſtrong cement) and that part of it where the ſucker was, a convenient quantity of air to expand itſelf and lift up the weight, when the air external to that included air ſhould be pumped out of the receiver. And laſtly, the handle of the rammer (from which the annular weight lately ſpoken of depended) was ſo faſtened to the turning-key of the cover of the receiver, that the weight might not compreſs the air included in the ſyringe, but leave it in its natural ſtate or wonted laxity, till the air were withdrawn from the receiver.

But notwithſtanding all this, when we actually tried the experiment, that happened which I feared. For though by this method the included air would well enough lift up a weight of ſeven or eight pounds, yet, when the rammer came to be clogged with ſo conſiderable a weight as my ſcope in making the experiment required, the inſtrument proved not ſo ſtanch, but that it was eaſier for ſome particles of air to force themſelves a paſſage, and get away between the ſucker and the inſide of the barrel, than to heave up ſo great a weight. And yet I have thought fit to relate the experiment thus particularly, becauſe if an exact ſyringe can be procured, which I fear will be very difficult, but do not think impoſſible, this ſeems to be one of the likelieſt and leaſt exceptionable ways I know of meaſuring the force of the air's ſpring.

But deſpairing to get ſuch a ſyringe as I deſired in the place where I then was, I bethought myſelf of another way, by which I hoped to be able, though not to arrive at an exact knowledge of the full force of the air's ſpring, yet at leaſt to approach nearer it than I have been able to do by the help of the ſyringe. For this purpoſe, conſidering with myſelf that if a convenient quantity of air were included in a fine ſmall bladder, the

the ſides of it would hinder the air from getting away, and the limberneſs of them would permit the air to accommodate itſelf and the bladder to the figure of a cylindrical veſſel into which it might be put:

Wherefore with much ado I procured to be made by a perſon exerciſed in turning, a couple of hollow cylinders, whoſe ſides were of a ſufficient thickneſs, that they might reſiſt the preſſure of the air to be impriſoned in them, and of ſuch differing breadths, that the firſt had but one inch in diameter, and the ſecond two; their depths being alſo unequal, that the one might receive a much larger bladder than the other.

With the leſſer of theſe, which was very carefully turned, I made a diligent trial; whoſe circumſtances I cannot now acquaint your lordſhip with, the paper wherein they were amply recorded having been, with other notes belonging to this continuation, unluckily loſt; but the moſt conſiderable things in the event were, that it was very difficult to procure a bladder ſmall and fine enough for that little cylinder; and that one which at length we procured, would not continue ſtanch for many trials, but would after a while part with a little air in the well exhauſted receiver, when it was clogged with the utmoſt weight it could ſuſtain; but whilſt it continued ſtanch we made one fair trial with it, from whence we concluded that a cylinder of air of but an inch in diameter, and leſs than two inches in length, was able to raiſe viſibly, though but a little, a weight of above ten pounds (I ſpeak of avoirdupoize weights, where a pound contains ſixteen ounces). The manner of making this experiment, and the cautions uſed in judging of it, your lordſhip may learn by the recital of the ſubſequent trial; my notes about which were not ſo unfortunate as thoſe that concerned the former.

See Plate VIII. Fig 2 and 4. Into a hollow cylinder of wood of four inches in depth, and two in diameter, furniſhed with a broad and ſolid bottom or pedeſtal to make it ſtand the firmer, was put a lamb's or ſheep's bladder very ſtrongly tied at the neck, on which was put a wooden plug, marked with ink where the edge of the cylinder was contiguous to it: this plug being loaden with weights, amounting to 35 pounds, (the uppermoſt of which weights was faſtened to the turning-key to keep it upright, and to help to raiſe it at firſt) the receiver was exhauſted till the mark appeared very manifeſtly above the brim of the cylinder; and then, though the ſtring were by turning the key quite ſlackened, yet the mark on the plug continued very viſible: and when ſo much air was let into the receiver, as made the weight depreſs the plug quite beneath the mark, upon the repumping out of the air, the weight was without the help of any turning-key lifted up, and by degrees all the mark on the plug was raiſed about $\frac{1}{8}$ above the edge of the cylinder.

Wherefore we ſubſtituted for a ſeven pound weight, one that was eſtimated at 14 (for then we had not a balance ſtrong enough to weigh it with) and uſing the ſame bladder we repeated the experiment, only having a care to ſupport a little the uppermoſt weight by the turning-key, till the bladder had attained its expanſion; and then the weight being gently let go, depreſſed not the plug ſo low, but that we could yet ſee the mark on it (which yet was all we could do) though that part of the plug where the mark was, were manifeſtly more depreſſed than the other.

For the clearing up of ſome particulars relating to this trial, we will ſubjoin the following notes.

1. The plug is to be ſo fitted to the cavity of the cylinder, as eaſily to ſlip up and down it, without grating againſt the ſides of it, leſt it needleſsly increaſe the reſiſtance of the weight to be raiſed; and this plug ought to be of a convenient length, as about an inch and a half at leaſt, that it may be the fitter to help to reduce the bladder by compreſſion into a ſomewhat cylindrical ſhape, and yet that it may not be thruſt in too deep by the incumbent weight; and that the weight might reſt more firmly upon it, there was

a broad

a broad and ſtrong ledge made at the top of it, by which it might lean on every ſide upon the brim of the hollow cylinder.

2. Before the inſtrument was conveyed into the receiver, the bladder (which ought to be of a juſt ſize, and not full blown, and of a fine and limber contexture) was put into the cylinder, and by divers gradual (but not immoderate) compreſſions was reduced to conform itſelf as much as might be to the cylindrical ſhape of the containing veſſel. and then the weight being put on, and taken off again, there was a mark (in the form of an horizontally placed arch) made with ink, where the edge of the brim of the hollow cylinder did almoſt touch the plug. This we thought neceſſary to do to avoid a miſtake; for we muſt not judge that all the weight that might be raiſed by our bladder, may paſs for the weight ſought after by our experiment; ſince the air in the bladder is by reaſon of the incumbent weight more compreſſed than it was before, and conſequently its being able to heave up a great weight will not infer that our common air is able in its natural ſtate (as they call it) to exert ſo great a ſtrength; that weight being only to be looked on as raiſed or ſuſtained by the uncompreſſed air, that is raiſed or ſuſtained when the plug is lifted up to the mark, ſince till then the ſpring of the air does but bring it back from its new ſtate of adventitious compreſſion to its natural or wonted laxity.

3. When, after the operation was ended, we took the bladder out of the veſſel, it had obtained a form cylindrical enough; and though it could be but two inches in diameter, yet it was ſo little, as to be but half an inch more long than broad.

The reaſon why I choſe to have the two cylinders made of the unequal diameters above-mentioned, was to examine, as far as by this way I could, a conjecture I had that the force of the ſpring of differing cylinders of air to lift up ſolid weights would, at the very firſt raiſing of the weights, be in duplicate proportion to the diameters of their cylinders (thoſe diameters being proportionable to the areas of the plain ſuperficies againſt which the air does immediately preſs) without very much conſidering the inequality that may be between the quantity of the ſeveral parcels of air whoſe preſſures are compared. But it is to be remembered, that I ſaid at the very firſt raiſing of the weights, becauſe preſently after that the quantity of the parcels of air may be very conſiderable; for as I have ſhewn in another treatiſe, two very unequal quantities of air being made by their expanſion to poſſeſs two equal ſpaces, the leſſer quantity of air muſt be much more rarefied in proportion than the greater; and conſequently, to bring this home to our preſent argument, though both be lifted up a quarter or half of an inch, the ſpring of a very little air muſt be much more weakened than that of a very conſiderable quantity, and ſo it cannot continue to lift up its weight as the above-mentioned proportion would (if it were not for this advertiſement) ſeem to require.

4. Taking then our conjecture in the ſenſe now declared, the ſucceſs of our trials is agreeable to it, inviting us to conclude that the air in the bladder, which was but two inches in diameter, was able by its preſſure to countervail the weight of 42 pounds, which is about four times the weight that we lately obſerved the ſpring of a cylinder of air of one inch in diameter to be able to lift up. For though, according to what we have formerly ſaid of a duplicate proportion, 42 pounds ſeems to be ſomewhat more than ought to have been lifted up in the cylinder of two inches bore, when that of one inch lifted up not above 10 pounds; yet this diſagrees not with the hypotheſis, if we conſider that the ſubſtance of the bladder ſtraightens the cavity of the ſmaller cylinder in a greater proportion than that of the bigger.

5. Though we have thus (as far as the inſtruments we were able to procure would aſſiſt us) meaſured the preſſure of included air, yet I muſt not forbear to advertiſe your lordſhip, that conſidering what I formerly obſerved to you about the weight of an atmoſphe-

mospherical pillar of an inch in diameter, I cannot but think that if a cylinder or other convenient instrument, exactly tight, can be procured, the spring of an aerial cylinder will appear to be greater than we found it by the foregoing trials; in which I consider that, not to mention the resistance of the bladder itself, the membraneous substance that lined the cylinders (though it were very thin and fine) could not but somewhat straighten their cavities, and consequently somewhat (though not much) lessen the diameters of the included aerial cylinders.

6. To all these notes I must add this advertisement, that it may be therefore the more difficult in such trials as ours to ascertain the force of the air's spring, because that air itself when it is concluded, being shut up with the pressure of the atmosphere upon it, it is probable that since that pressure, as we have shewn, is not at all times the same, the spring of the included air will accordingly be varied. And if my memory fail me not, when the lately recited experiments were made, our barometer declared the atmosphere to be somewhat light.

From what has been hitherto delivered, this may result, that it is likely that the spring of an aerial cylinder an inch broad may be able to sustain, if not raise a pretty deal more than ten pounds weight; and that the past trials, without determining that the air can raise no more in them than it did, do at least prove that it can raise up as much weight as we have related, since we actually found it to do so.

EXPERIMENT XLVIII.

About an easy way of making a small quantity of included air raise in the exhausted receiver 50 or 60 pounds or a greater weight.

I would very willingly have further prosecuted the foregoing trials, to see how far the lately proposed conjecture or hypothesis would hold; but was hindered by the want of receivers tall and capacious enough to contain the weights that such an attempt required; but remembering that there were not any experiments made in our engine, that appeared more strange to the generality of spectators, and served more to give them a high opinion of the air's spring, than those wherein they saw solid bodies actually lifted up by it; and remembering that I had lying by me a brass vessel which had been bespoken for another experiment, for which the workmen had not made it fit, I thought it not amiss to employ it about making a trial very easy, and yet fit to be shewn to strangers, to convince them that the spring of the air is a much more considerable thing than they imagined.

See Plate VIII. Fig. 4. We took then a brass vessel made like a cylinder, and having one of its orifices exactly covered over with a flat plate very firmly fastened to it, the other orifice being wide open. The depth of this vessel was four inches, and the diameter should have been precisely, but wanted about a quarter of an inch of, four inches. To this hollow cylinder we fitted a wooden plug, like one of those described in the foregoing experiment, save that it was not quite so long, and that it was furnished with a rim or lip, which was purposely made of a considerable breadth, that it might afford a stable basis See Plate VIII. Fig. 3. to the weight that should lean upon it; and then taking a middle-sized and limber bladder, strongly tied at the neck, but not near full blown, we pressed it by the help of the plug into the cylinder to make it the better accommodate itself to the figure of it; then taking notice by an inky mark how much the plug was extant above the orifice of the vessel, we laid the weights upon the plug, whose rim or lip hindered it from

from being depreſſed too deep into the cavity of the veſſel; and having conveyed them into the receiver, we found as we expected, that if we had loaded the plug but with a ſingle weight, as to avoid trouble and the danger of breaking the glaſs we uſually thought fit to do, though that were a common half-hundred weight, which you know amounts to 56 pounds, it would very quickly be manifeſtly heaved up by the ſpring of the included air. For confirmation of more than which, I ſhall ſubjoin the enſuing trial, as I find it recorded among my looſe notes. See Plate VIII. Fig. 5.

The weight that was lifted up by the bladder in the cylinder four inches broad, was 75 pounds; this weight was lifted up till the wooden plug diſcloſed the mark that was to ſhew the height at which the air kept the ſaid plug before it was compreſſed; diſcloſed it, I ſay, viſibly at the fifth exſuction, and at the ſeventh that mark was $\frac{1}{8}$, or rather $\frac{3}{16}$ above the edge of the cylinder. In the gage where the mercury in the open air was wont to ſtand about $\frac{1}{8}$ above the uppermoſt glaſs-mark, it was depreſſed till it was $\frac{1}{8}$ below the ſecond mark. When the air was let in, it was a pretty while before the weight did manifeſtly begin to ſubſide; the bladder being taken out, and the place it had poſſeſſed in the cylinder being ſupplied with a ſleeve, or ſome ſuch thing, and the weight laid again upon the plug, we found that at twenty-four exſuctions the mercury was depreſſed to the loweſt mark of the gage; and it was the thirty-fourth or thirty-fifth exſuctions before the receiver appeared to be ſo exhauſted, as to put an end to the ſinking of the mercury, which was then above $\frac{1}{8}$ beneath the loweſt mark.

Your lordſhip will eaſily believe that moſt of the ſpectators of ſuch trials thought it ſomewhat ſtrange to ſee a ſmall quantity of air, which was not only uncompreſſed in the bladder, but did not near fill it, and left it very ſoft and yielding to the leaſt touch, lift up ſo eaſily by its bare ſpring ſuch great weights as endeavoured to oppreſs it. But this not being any thing near a ſufficient trial, how far the conjecture or hypotheſis formerly propoſed will hold, I thought fit to make the utmoſt trials the talleſt receivers I could procure would admit; and having cauſed leaden weights to be purpoſely caſt flat like cheeſes, and as broad as we could conveniently put into the receiver, that by the advantage of this ſhape we might be able to pile up the more of them, without much danger that any of them ſhould be ſhaken down; we laid divers of them one upon another, and then the upper part of the receiver growing too narrow to admit more of them, we added a leſs broad weight or two; and then exhauſting the receiver till we perceived by the gage that the air was manifeſtly withdrawn, we found, as near as we could meaſure by the help of a mark and a pair of compaſſes, that the plug was ſo far raiſed, as that it was concluded that the elevation would have been much greater, if the included air being put upon ſo great a conatus, had not found it eaſier to produce ſome leak at the neck of the bladder, than to lift up ſo great a weight, which by our reckoning came to about 100 pounds of 16 ounces to the pound. But this laſt experiment, for want of ſome requiſite accommodations, we were hindered from repeating and promoting; though the above-mentioned hypotheſis made me preſume that a far greater weight might this way have been raiſed, if the bladder had been ſtanch, and the receiver high enough.

I need not tell your lordſhip that if a larger bladder be employed and included in a braſs veſſel of a ſufficiently wide orifice, a far greater weight may be lifted up by the ſpring of the internal air; but yet it will not be amiſs to give your lordſhip on this occaſion this advertiſement, which may be fit to be taken notice of on divers others; that care muſt be had not to make receivers that ought to be well emptied, too large, and eſpecially too wide at the orifice; for otherwiſe they will be expoſed to ſo great a preſſure of the atmoſphere, that they need be of an extraordinary ſtrength to reſiſt it; and even receivers that seemed thick enough proportionably to their bulk, and which held out very well till the close of the operation, yet when they came to be very diligently exhausted, they did, by reason of the wideness of their orifices, begin to crack at the bottom.

EXPERIMENT XLIX.

Viz. the XXXVI. In one of my published experiments I long since told your lordship that when I endeavoured, by the help of a sealed bubble, weighed in an exhausted receiver, to compare the gravity of air and water, I was hindered by the casual breaking of the glass from compleating the experiment; wherefore I afterwards thought fit to repeat the trial; and though when I had done so twice or thrice, having given away the large receiver I had made use of about them, and not being able ever since to procure a good one that was capacious enough for the tender scales I thought so nice an experiment required, I did not prosecute that attempt so far as I intended; yet this very difficulty I met with to procure the requisites of making the trial, invites me to subjoin the two following notes, which I find among my loose papers.

April the 19, 1662. We weighed a bubble in the receiver, which we found to weigh above half a grain heavier, when much of the air was exhausted, than when it was full. Afterwards we took out this sealed bubble, and weighing it, found it to weigh 68 grains and a half; then breaking off the small tip of it under water, we found that the heat by which it was sealed up, had rarefied its included air, so that it admitted 125 grains of water, for the admitted water and glass weighed $193\frac{1}{2}$ grains. Then filling it full with water, we found it to contain in all 739 grains of water, for it weighed $807\frac{1}{2}$ grains; whence it is evident that the difference between the weight of water and air was less than 1228 to 1.

We weighed in the receiver a bubble, the glass of which weighed 60 grains; the air that filled it weighed *in vacuo* $\frac{27}{32}$ of a grain; the water that filled it weighed $720\frac{1}{4}$ grains; so that by this experiment the proportion of the weight of air to water is as one to $853\frac{17}{27}$.

May 26, 1662. The trials mentioned in these notes, though they were too few for me to acquiesce in, yet being made in a new way, and which has some advantages above those that have been hitherto employed to weigh the air, may yet serve to keep us from the contrary extremes that have not been avoided by such eminent mathematicians as *Galileo* and *Ricciolus*; the former of which makes water to be about 400 times as heavy as the air; and the latter, whose conjecture is much remoter from the truth, 10,000 times heavier.

But it is so desireable a thing, and may prove of such importance, to know the proportion in weight betwixt air and water, that I shall not scruple to acquaint your lordship with an attempt or two, that I made to discover it by another way; for though at first sight this experiment may seem to be the same with one published a pretty while ago in the learned *Schottus* his *Mechanica hydrolico-pneumatica*; yet your lordship will easily perceive this difference between them; that whereas the industrious author of that experiment contents himself to shew, by the diminution of the weight of a glass, when the air has been drawn out of it, that the air, before it was drawn out, was not devoid of gravity; the following trial does not only perform the same thing, and by a superadded circumstance confirm the truth to be thereby proved, but it endeavours also to shew the proportion in gravity betwixt the air and water. The trials themselves were registered among my *Adversaria* as follow:

A small

A SMALL receiver being exhaufted of air by the engine, and counterpoifed whilft it continued fo, the ftop-cock was turned, and the air re-admitted, which made it weigh 36 grains more than it did before; and to prevent jealoufies, we caufed it to be applied the fecond time to the engine, by which the air being emptied once more, the glafs was put into the other fcale of the former balance, and fo counterpoifed; and then the external air being re-admitted (which rufhed in as formerly with a whiftling noife) there was found 36 grains or better requifite to reftore the balance to an æquilibrium.

WE took a fmall glafs receiver fitted with a ftop-cock, and having exhaufted it of the air, and counterpoifed it, and let in the outward air, we found the weight of the veffel to be increafed by that admiffion 36 grains. This done, we took the receiver, after having well counterpoifed it, out of the fcale, and having applied it the fecond time to the engine, we once more withdrew the air, and then turning the ftop-cock to keep out the external air, we took care that none of the cement employed to join it to the engine, fhould ftick to it, as we had diligently freed it from adherent cement before we laft applied it to the engine. Then weighing it again, we found it to weigh either 35 or 36 grains (but rather the former) heavier than it did when it was laft counterpoifed in the fame balance. This being alfo done, we immerfed the ftop-cock into a bafon of fair water, and let in the liquor, that we might find how much water would fucceed in place of the air we had drawn out. When no more water was impelled in, we turned the ftop-ftock once more to keep it from falling out, and then weighing it in the fame fcales (after we had wiped the ftop-cock, that no water might ftick to it on the outfide) we found the water (without computing the veffel) to weigh 47 ounces, 3 drachms, and 6 grains, which divided by 35 grains (which I took to be the weight of the air that was equal in bulk to this water that fucceeded it) the quotient was (wanting a very little) 650 grains, for the proportion of the weight between air and water of the fame bignefs, at the time when the experiment was made; which circumftance I therefore take notice of, becaufe the atmofphere appeared by the barofcope (wherein the mercury ftood then at 29 inches and three quarters) to be very heavy; which made me the lefs wonder to find this proportion not fo great, as at other times I had obferved it to be between water and air in point of weight; though I fufpected, that becaufe this odd experiment cannot be nimbly difpatched, fome little air may have got in at the ftop-cock, befides the air that difclofed itfelf in numerous bubbles in the water that was admitted, where, though it lay in fuch fmall particles as not to be difcerned before, yet thefe particles, by this opportunity to expand themfelves, extricated themfelves from the water, and by getting together might fomewhat refift the ingrefs of more; which is a difficulty whereto the meafuring the proportion between water and air in a heated æolipile is liable. But the ftealing in of any air before the water was let in, is mentioned but as a fufpicion.

YOUR lordfhip may perhaps think it fomewhat ftrange that I fhould prefent you trials whofe events do not fo well agree together as perchance you expected; but this very difagreement was one of the motives that induced me to acquaint you with them; for all thofe comprized in thefe experiments being made faithfully, and not without at the leaft an ordinary diligence, as they feem to make it probable, that one may without any great error eftimate the proportion of our Englifh air to water to be as one to fome number between 600 and 1100; fo it is not to be expected that the proportion, whatever it be that fhould be pitched upon, fhould be accurate and ftable. For though learned men feem to have hitherto taken it for granted that it may fuffice once for all diligently to inveftigate the proportion betwixt thofe two bodies, yet not only that I am apt to believe that a determinate quantity of air (as a pint or quart) may be unequally heavy in diſtant countries, and even in differing places of the ſame country; but what I have taken notice of in the 17th of the printed experiments, and afterwards frequently obſerved of the great inequalities of the weight of the atmoſphere, inclines me to think that in the ſelf ſame place two experiments may be made with the ſame inſtruments and equal diligence, and yet the weights of the air may be found differing enough; which may keep your lordſhip from much wondering, that in the 36th printed experiment, made when I had the variations of the atmoſphere's gravity in my eye, I found the air to be leſs ponderous in reference to water, than in theſe latter trials. But of this I hope I ſhall, if God permit, make further trial with the ſame veſſels, at times when I ſhall perceive by the baroſcope that the gravity of the atmoſphere is very great and very ſmall; and I wiſh the curious would make the like trials in other regions. I do not forget that not only the ſchool-philoſophers, but moſt of the moderns, deny that air hath any weight in air, no more than water in water; but having * elſewhere declared and explained my ſenſe about this received opinion, I ſhall not here ſpend any of the little time I have remaining to juſtify my diſſent; for which your lordſhip may find ſufficient grounds in the newly related experiments, eſpecially if you pleaſe to conſider, that though the opinion I diſallow have been chiefly and generally grounded upon ſome arguments ſuppoſed to evince that water has no weight in water, I have † elſewhere ſhewn thoſe proofs not to be cogent, and taught a practical way of weighing water in water with a pair of ordinary ſcales ‡.

EXPERIMENT L.

About the disjoining of two marbles (not otherwiſe to be pulled aſunder without a great weight) by withdrawing the preſſure of the air from them.

In our formerly publiſhed experiments about the air ‖, I did, if I miſremember not, acquaint your lordſhip with an attempt I had made to make a couple of coherent marbles fall aſunder by withdrawing the air from them. But though I then eſteemed that their coheſion depended upon the preſſure of the air, yet not being at that time furniſhed with all the accommodations requiſite to make an experiment not eaſy to be performed ſucceed, I thought fit, when I had afterwards opportunity, to proſecute what I then began, and add ſome circumſtances that I could not then make trial of, and yet whoſe ſucceſs will not, I preſume, be unwelcome, ſince it ſupplies us with no leſs than matters of fact; whence we may argue this, that this experiment of coherent marbles (which not only the Ariſtotelian pleniſts have much triumphed in, but which ſome recent favourers of our hypotheſis have declared themſelves to be troubled with) is not only reconcileable to our doctrine, but capable of being made a confirmation of it; notwithſtanding what has lately been publiſhed (upon the ſuppoſition of a caſe, which at firſt bluſh may ſeem ſomewhat of a kin to our experiment) by a very learned § writer, to whoſe objection

* In the Hydroſtratical Paradoxes.

† In an Appendix to thoſe Paradoxes.

‡ This method was omitted in the Engliſh edition of the newly mentioned Appendix, but not in the Latin Verſion.

‖ Experiment XXXI. See alſo the cauſe of this phænomenon diſcourſed of in the author's Hiſtory of Fluidity and Firmneſs.

§ Dr. *H. M.* in the ſecond chap. of the ſecond book of the new edition in folio, of his *Antidote againſt Atheiſm.*

againſt

against our hypothesis, though as well confidently as very civilly proposed, an answer may in due place, if your lordship desire it, be returned.

We took two flat round marbles, each of them of two inches and about three quarters in diameter, and having put a little oil between them to keep out the air, we hung at a hook fastened to the lowermost a pound weight, to surmount the cohesion which the tenacity of the oil and the imperfect exhaustion of the receiver might give them; then having suspended them in the cavity of a receiver, at a stick that lay horizontally a-cross it; when the engine was filled and ready to work, we shook it so strongly, that those that were wont to manage it, concluded it would not be near so much shaken by the operation. Then beginning to pump out the air, we observed the marble to continue joined, until it was so far drawn out, that we began to be diffident whether they would separate; but at the 16th suck, upon the turning of the stop-cock (which gave the air a passage out of the receiver into the pump) the shaking of the engine being almost, if not quite over, the marble spontaneously fell asunder, wanting that pressure of the air that formerly had kept them together: which event was the more considerable, not only because they hung parallel to the horizon, but adhered so firmly together when they were put in, that having tried to pull them asunder, and thereby observed how close they stuck together, I foretold it would cost a good deal of pains so far to withdraw the air, as to make them separate; which conjecture your lordship will the less wonder at, if I add that a weight of 80 and odd pounds, fastened to the lowermost marble, may be drawn up together with the uppermost, by virtue of the firmness of their cohesion.

N. B. This is not the only time that this experiment succeeded with us; for sometimes, when they were not so closely pressed together before they were put in, the disjunction was made at the 8th suck, or sooner, and we seemed to ourselves to observe, that when we hung but half a pound weight to the lower marble, it required a greater exhaustion of the receiver to separate them, than when we hung the whole pound.

After having proceeded thus far with the instruments we then had, meeting with an artificer that was not altogether unskilful, we directed him to make (what we wanted before in that place) such a brass-plate to serve for a cover or cap to the upper orifice of receivers open at the top, as we have divers times had occasion to mention already in giving accounts of some of the foregoing trials; by the help of which contrivances we prosecuted the newly related experiment much farther than we could do before, as may appear by the following account.

We fastened to the lowermost of the two marbles a weight of a very few ounces (for I remember not the precise number) and having cemented the capped receiver with the marbles in it, as before to the pump, we did by a string, whereof one end was tied to the bottom of this turning-key, and the other to the uppermost marble, and which (string) passed through the crank or hook belonging to the brass-cover; we did, I say, by the help of this string, and by turning round the key, draw up the superior marble, and by reason of their coherence the lowermost also, together with the weight that hung at it: by which means being sure that the two marbles stuck close together, we began to pump out the air that kept them coherent; and after a while, the air being pretty well withdrawn, the marbles fell asunder. But we having so ordered the matter that the lowermost could fall but a little way beneath the other, we were able by inclining and shaking the engine to place them one upon another again, and then letting in the air somewhat hastily, that by its spring it might press them hard together, we found the expedient to succeed so well, that we were not only able by turning the abovementioned cylindrical key, to make the uppermost marble take up the other, and the annexed weight;

See Plate IV. Fig. 4.

weight; but we were fain to make a much more laborious and diligent exhauſtion of the air to procure the disjunction of the marbles this ſecond time, than was neceſſary to do it at the firſt.

And for further prevention of the objections or ſcruples that I foreſaw ſome prepoſſeſſions might ſuggeſt, I thought fit to make this further trial; that when the marbles were thus aſunder, and the receiver exhauſted, we did, before we let in the air, make the marbles fall upon one another as before; but the little and highly expanded air that remained in the receiver having not a ſpring near ſtrong enough to preſs them together, by turning the key we very eaſily raiſed the uppermoſt marble alone, without finding it to ſtick to the other as before; whereupon we once more joined the marbles together, and then letting in the external air, we found them afterwards to ſtick ſo cloſe, that I could not without inconvenience ſtrain any farther, than I fruitleſly did, to pull them fairly aſunder; and therefore gave them to one that was ſtronger than I, to try whether he could do it, which he alſo in vain attempted to perform.

And now, my lord, though I had thoughts of adding divers other experiments to thoſe I have hitherto entertained you with, yet (upon a review) finding theſe to amount already to fifty, I think it not amiſs to make a pauſe at ſo convenient a number; and the rather, becauſe an odd quartianary diſtemper that I ſlighted ſo long, as to give it time to take root, is now grown ſo troubleſome, that I fear it may have too much influence upon my ſtyle; which apprehenſion obliges me as well to avoid abuſing or diſtreſſing your lordſhip's patience, as to allow myſelf ſome ſeaſonable refreſhment, to reſerve the mention of the deſigned additions until they can with leſs trouble to us both be preſented you by,

My dear lord,

Your lordſhip's moſt humble ſervant, and affectionate uncle,

Oxford, March 24.
1667.

ROBERT BOYLE.

NOTES,

N O T E S, &c.

ABOUT THE

ATMOSPHERES of CONSISTENT BODIES here below.

SHEWING,

That even hard and ſolid Bodies (and ſome ſuch as one would ſcarce ſuſpect) are capable of emitting EFFLUVIA, and ſo of having ATMOSPHERES.

ADVERTISEMENT.

HE that ſhall take the pains to peruſe the following paper, will eaſily believe me, when I tell him that it was not deſigned to come abroad with the experiments, in whoſe company it now appears: but the ſtationer earneſtly repreſenting, that divers experiments being reſerved by me for another occaſion, the remaining ones alone would not give the book a thickneſs any thing proportionable to its breadth; I conſented, at his ſolicitation, to annex to them the following obſervations, becauſe of ſome affinity between the ſmall atmoſpheres of leſſer bodies, and the great atmoſphere that ſurrounds the terreſtrial globe; in which the other that do at leaſt help to compoſe it, are loſt and confounded, as brooks and rivers are in the ocean: and to ſave the reader the pains of making gueſſes to what kind of writing the enſuing diſcourſe may belong, I ſhall here intimate, that it is diſmembered from certain papers about occult qualities in general, which make part of the notes I long ſince deſigned, and alſo partly publiſhed, about the origin of qualities, of which notes thoſe that concerned effluviums, being the moſt copious, I referred them to four general heads; whereof the firſt only is treated of in the following diſcourſe, the others being with-held, as having not affinity enough with the atmoſphere to accompany this, whereon they have no ſuch abſolute dependence, but that they may well enough ſpare it: and I make the leſs ſcruple to let it appear without them, becauſe the inducements already mentioned are not a little ſtrengthened by this ſuperadded conſideration, that the following notes may give light to ſeveral of the obſervations I have made, of ſome leſs heeded phænomena of the alterations of the air, in caſe they be allowed to enter into the Appendix to this Continuation.

Of

Of the Atmospheres *of* Consistent Bodies.

THE school-philosophers and the vulgar, in considering the more abstruse operations and phænomena of nature, are wont to run into extremes, which, though opposite to one another, do almost equally contribute to keep men ignorant of the true causes of those effects they admire: for the vulgar, being accustomed to converse with sensible objects, and to conceive grosly of things, cannot easily imagine any other agents in nature than those that they can see, if not also touch and handle; and as soon as they meet with an effect that they cannot ascribe to some palpable, or at least sensible efficient, they are, and stick not to confess themselves utterly at a loss: and though the vulgar of philosophers will not acknowledge themselves to be posed by the same phænomena with the vulgar of men, yet in effect they are so; but the school-philosophers, on the contrary, do not only refuse to acquiesce in sensible agents, but, to solve the more mysterious phænomena of nature, nay and most of the familiar ones too, they scruple not to run too far to the other side, and have their recourse to agents that are not only invisible but inconceivable, at least to men that cannot admit any save rational and consistent notions: they ascribe all abstruse effects to certain substantial forms, which, however, they call material, because of their dependence on matter, they give such descriptions to, as belong but to spiritual beings; as if all the abstruser effects of nature, if they be not performed by visible bodies, must be so by immaterial substances; whereas betwixt visible bodies and spiritual beings there is a middle sort of agents, invisible corpuscles; by which a great part of the difficulter phænomena of nature are produced, and by which may intelligibly be explicated those phænomena, which it were absurd to refer to the former, and precarious to attribute to the latter. Now, for method's sake, I will refer the notes that occur to me about effluviums to four heads; whereof the first is mentioned in the title of this paper, and each of the other three shall be successively treated of in as many distinct ones.

That fluid bodies, as liquors, and such as are manifestly either moist or soft, should easily send forth emanations, will, I presume, be granted without much difficulty; especially considering the sensible evaporation that is obvious to be observed in water, wine, urine, &c. and the loose contexture of parts that is supposed to be requisite to constitute soft bodies (as flowers, balsams, and the like); but that even hard and ponderous bodies, notwithstanding the solidity and strict cohesion of their component parts, should likewise emit steams, will to many appear improbable enough to need to be solemnly proved.

Whether you admit the atomical hypothesis or prefer the Cartesian, I think it may be probably deduced from either, that very many of the bodies we are treating of may be supposed exhaleable as to their very minute parts; for, according to the doctrine of *Lucippus*, *Democritus*, and *Epicurus*, each indivisible particle of matter hath essentially either a constant actual motion or an unlooseable endeavour after it; so that though it may be so complicated in some concretions with other minute parts, as to have its avolation hindered for a while; yet it can scarce otherwise be, but by this incessant endeavour of all the atoms to get loose, some of them should from time to time be able to extricate themselves and fly away: and though the Cartesians do not allow matter to have any innate motion, yet, according to them, both vegetables, animals, and minerals consist of little parts so contexed that their pores give passage to a celestial matter; so that this matter continually streaming through them, may well be presumed to shake the

the corpuscles that compose them ; by which continued concussion now some particles, and then others, will be thrown and carried off into the air, or other contiguous body fitted to receive them: but though by these and perhaps other considerations, I might endeavour to shew *à priori*, as they speak, that it is probable consistent bodies themselves are exhaleable, yet I think it may be as satisfactory, and more useful, to prove it *à posteriori*, by particular experiments and other examples.

That then a dry and consistent form does not necessarily infer, in the bodies that are endowed with it, an indisposition to send forth steams, which are, as it were, little colonies of particles, is evident, not only in the leaves of damask-roses, whether fresh or dried, as also in wormwood, mint, rue, &c. but in amber-gris, musk, storax, cinnamon, nutmegs, and other odoriferous and spicy bodies : but more eminent examples to our present purpose may be afforded us by camphire and volatile salts, such as are chemically obtained from hartshorn, blood, &c. for these are so fugitive, that sometimes I have had a considerable lump of volatile salt (either of fermented urine, or of hartshorn) fly away by little and little out of a glass that had been carefully stopped with a cork, without leaving so much as a grain of salt behind it : and as for camphire, though by its being uneasy to be powdered, it seem to have something of toughness or tenacity in it, yet I remember, that having for trial's sake counterpoised it in nice scales, even a small lump of it would in a few hours suffer a visible loss of its weight by the avolation of strongly scented corpuscles, and this, though the experiment were made both in a north window and in winter.

But I expect you should require instances of the effluviums of bodies of a close or solid texture; wherefore I proceed to take notice, that amber, hard wax, and many other electrical bodies do, when they are rubbed, emit effluviums : for though I will not now meddle with the several opinions about the cause and manner of electrical attraction, yet besides that almost all the modern naturalists that aim at explicating things intelligibly, ascribe the attraction we are speaking of to corporeal effluxes ; and besides that I shall ere long have occasion to shew you, that there is no need to admit with Cartesius, that because some electrical bodies are very close and fixed, what they emit upon rubbing is not part of their own substance, but somewhat that was harboured in their pores: besides these things, I say, I have found that many electrical bodies may by the very nostrils be discovered, when they are well rubbed, to part with store of corpuscles, as I have particularly, but not without attention, been able to observe in amber, resin, brimstone, &c.

I know not, whether it will be worth while to take notice of the great evaporation I have observed, even in winter, of fruits, as apples, and of bodies that seem to be better covered, as eggs ; which, notwithstanding the closeness of their shells, did daily grow manifestly lighter and lighter ; as I observed in them, and divers other bodies, that I kept long in scales, and noted their decrements of weight: but perhaps you will be pleased to hear, that having a mind to shew how considerable an evaporation is made from wood, I caused a thin cup, capable of holding about a pint or more, to be turned of a wood that was chosen by the turner as solid and dry enough, though it were not of the closest sort of woods, such as are lignum vitæ and box ; and as I caused the shape of a cup to be given it, that it might have a greater superficies exposed to the air, and consequently might be the fitter to emit store of steams into it ; so the success did not only answer my expectation, but exceed it: for though the trial were made some time in winter, there was so thick and plentiful an evaporation made from the cup, that I found it no easy matter to counterpoise it ; for whilst grains were putting into the opposite scale, to bring the tender balance to an æquilibrium, the copious avolation of invisi-

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ble steams from the wood (which had so much of superficies contiguous to the air) would make the scale that held it sensibly too light. And I remember, that for further satisfaction, being afterwards in a city where there were both good materials and workmen, I ordered to be made a bowl about the same bigness with the former, of well seasoned wood, which being suspended in the chamber I lay in (which circumstance I therefore mention, because the weather and a little physic I had taken obliged me to keep a fire there) it quickly began manifestly to lose of its weight; and though the whole cup wanted near two drachms of near two ounces, yet in twelve hours, viz. from ten of the clock in the morning to the same hour at night, it lost about 40 grains (for it was above 39): but of such experiments and the cautions belonging to them, I may elsewhere speak farther.

It were not difficult for me to multiply instances of the continual emanation of steams from vegetable and animal substances; but I am not willing to enlarge myself upon this subject, because I consider that there are other bodies which seem so much more indisposed to part with effluviums, that a few instances given in such may evince what I would prove, much more than a multitude produced in other bodies: and since I consider that those substances are the most unlikely to afford effluvia that are either very cold or very ponderous, and very solid and hard, or very fixed; if I can shew you that neither of these qualifications can keep a body from emitting steams, I hope I shall have made it probable, that there is no sort of bodies here below that may not be thought capable of affording the corporeal emanations we speak of.

And first I remember, that I have not only taken eggs, and in a very sharp winter found them, notwithstanding the coldness of the air where I kept them, to grow sensibly lighter, in a faithful pair of scales, in not very many hours; but because ice is thought the coldest visible body we know, I thought fit to shew, that even this body will lose by evaporation; for having counterpoised a convenient quantity of ice in a good balance, and forthwith exposed it therein to the cold air of a frosty night, that the evaporations should be from ice not from water, I found the next morning, that though the scale, wherein the ice were put, was dry, which argued as well as the coldness of the weather, that the exposed concretion had not thawed; yet I found its weight to be considerably diminished, and this experiment I successfully made in more than one winter, and in more than one place; and it is now but a few days since, exposing not long before midnight less than two ounces of ice in a good balance to a sharply freezing air, I sent for it before I was up in the morning; and though by the dryness of the scales, the ice, that was in one of them, appeared not to have thawed, yet it had lost about ten grains of its former weight; so that here the evaporation was made in spite of a double cold, of the ice and of the air.

I should now proceed to the mention of ponderous and solid bodies; but before I do so it may be expedient to give you notice, that, to make the proof of what I have proposed more satisfactory and more applicable to our future purposes, I shall forbear to give you any examples of the exhalations of bodies, where so potent an agent as the fire is made to intervene.

But though I purposely forbear to insist on such examples, yet it may not be amiss to intimate, that in explicating some occult qualities, even such exhalations as are produced by the help of the fire, may be fit to be taken into consideration, as we may hereafter have occasion to shew; and therefore we may observe, in general, that the fire is able to put the parts of bodies into so vehement a motion, that except gold, glass, and a very few more, there are not any bodies so fixed and solid, that it is not thought capable to dissipate either totally or in part: it is known to those that deal in the

the fufion of metals, that not only lead and tin, but much harder bodies will emit copious and hurtful fteams; and there are fome kinds of that iron which our fmiths call cold-fhare iron, about whofe fmell, whilft it was red hot, when I made inquiry, the ingenioufeft fmith I had then met with told me, that he had found it feveral times to be fo ftrong and rank that he could fcarce indure to work with his hammer thofe parcels of metal whence it proceeded: and even without being brought to fufion, not only brafs and copper will, being well heated, become ftrongly fcented, but iron will be fo too, as is evident by the unpleafing fmell of many iron ftoves: and on this occafion I might not impertinently add here a trial we made to obferve, whether the fteams of iron may not be made, though not immediately vifible, yet perceptible to the eye itfelf, though the metal had not a red, much lefs a white heat; but having elfewhere related it at large, in a difcourfe you may command a fight of, I fhall rather refer you to it than lofe the time it would take up to tranfcribe it.

These things premifed, I proceed now to the mention of ponderous bodies; and concerning them, to reprefent, that if you will admit, what almoft all the corpufcularians affert, and divers of the peripateticks do not now think fit to deny, that the magnetical operations are performed by particles iffuing forth of the body of the loadftone or other magnetical agent; I fhall not need to go far for an inftance to our prefent purpofe, fince I have hydroftatically found that fome loadftones (for I have found thofe minerals very differing in gravity) are fo ponderous as to exceed double the weight of flints or other ftones of the fame bulk.

But not to infift on loadftones, ftone-cutters will inform you, as they did me, that black marble, and fome other folid and heavy ftones, will, upon the attrition they are expofed to when the workmen are polifhing them, efpecially without water, emit, and that without the help of external heat, a very fenfible fmell; which I found to be much more ftrong and offenfive, when, to make it fo, I had the curiofity to caufe a piece of folid black marble to have divers fragments ftruck off from it with a chizel and a hammer; for the ftrokes fucceeding one another faft enough to make a great concuffion of the parts of the black marble (for in white, which is not fo folid, the trial will not fucceed well) there quickly followed, as I expected, a rank unpleafant fmell; and you will grant me, I know, that odours are not diffufed without corporeal emanations. I remember alfo, that having procured fome of thofe acuminated and almoft conical ftones that pafs among the vulgar for thunder-ftones, by rubbing them a little one againft the other, I could eafily, according to my expectation, excite a ftrong fulphureous ftink. I have alfo tried upon a certain mineral mafs that was ponderous almoft as a metal, but to me it feemed rather an unufual kind of marchafite, that I could in a trice, without external heat, make it emit more ftrongly fcented exhalations than I could contentedly endure; to which I fhall add this example more, that having once made a chemical mixture of a metalline body and coagulated mercury, which you will believe could not but be ponderous, though this mixture had already endured as violent a fire as was neceffary to bring it to fufion, in order to caft it into rings; yet it was fo difpofed to part with corporeal effluxes, that a very ingenious perfon that practifed phyfick, and was there when I made it, earneftly begged a little of it of me for fome patients troubled with diftempers in the eyes and other parts remote enough from the hand; which he affirmed himfelf to have very happily cured, by making the patient wear a ring of this odd mixture, or wearing a little of it as an appenfum near the difaffected part. If you make a *vitrum Saturni* with a good quantity of minium in reference to the fand or cryftal, which it helps to bring to fufion, you fhall have a glafs exceeding ponderous, and yet not devoid of electricity: and I remember, that having fometimes caufed brafs itfelf to be turned like wood, that I might try whether so great though invisible a concussion of all the parts would not throw off some steams that might be smelled, I was not reduced to forego my expectation; but yet because it was not fully answered, and because also there is great difference of brass upon the score of the *lapis calaminaris*, whereof, together with copper, it is made, I enquired of the workman who used to turn great quantities of brass, whether he did not often after find it more strong; and he informed me that he did, the smell being sometimes so strong as to be offensive to strangers that came to his shop and were not used to it.

I PROCEED now to the effluviums of solid and hard bodies; of which, if most of our corpuscularian philosophers and divers others be not much mistaken, I may be allowed to give instances in all electrical bodies, which, as I have already noted, must according to their doctrine be acknowledged to operate by substantial emanations: now among electrical bodies I have observed divers that are of so close a texture that aqua fortis itself, nor spirit of salt, will work upon them, and to be so hard, that some of them will strike fire like flints: of the former sort I have found divers gems which I named in my notes about electricity; and even the cornelian itself, which I found to attract hairs, though it be thought to be of a much slighter texture than precious stones, did yet resist aqua fortis, as I tried in a large ring brought out of the East-Indies, which I purposely broke, and reduced some part of it to powder, that I might make these and some other trials with it: rock crystal also, though it have a very manifest attractive virtue, as they call it, I have yet found it so hard as to strike fire rather better than worse than ordinary flints: and to shew that no hardness of a body is inconsistent with its being electrical, I shall add, that though diamonds be confest to be the hardest bodies that are yet known in the world, yet frequent experience has assured me, that even these, whether raw or polished, are very manifestly and sometimes vigorously enough electrical.

AND to let you see that I need not to have recourse to this kind of bodies, to prove that very solid ones are capable of effluvia, I will, to what I have formerly noted about the odour of black marble, subjoin two or three examples of the like nature.

THE first shall be taken from a sort of concretions very well known in divers parts of *Italy* by the name of cugoli, because of the great use that is made of it by the glassmen; these concretions, you will easily believe, are very hard, as other minerals of that sort are wont to be; and yet being invited by my conjectures about the atmospheres of bodies to try them by rubbing them one against the other, I found, as I expected, that they afforded not only a perceptible but a very strong smell, which was far from that of a perfume.

AND this brings into my mind, that having met with some stones cut out of human bladders, whose texture was so close that I could not with corrosive menstruums make any sensible solution of one, whereon I made my trial, though, to facilitate the liquors operation, part of it were reduced to fine powder; yet by a little rubbing of one of these so closely contexed stones, it would presently afford a rank smell, very like the stink of stale urine.

I REMEMBER I have caused iron to be turned with a lath, to examine, whether by the internal commotion that would by that operation be produced in the corpuscles of the metal, even that solid as well as ponderous body would not become capable of being smelled; and though by reason of the nature of that parcel of iron whereon we made our trial, or some accidental disposition which was at that time (being winter) in my organs of smelling, the odour seemed to me but very faint; yet upon the inquiry I made of the artificers, whether in turning greater pieces of iron they did not find the smell ſtronger; they told me that they often found it very ſtrong, and ſometimes more ſo than they deſired.

AND this brings into my mind what I have carefully obſerved in grinding of iron; for there are many grindſtones ſo qualified, that in caſe iron inſtruments be held upon the ſtone, whilſt it is nimbly turned under it, though the water that is wont to be uſed on ſuch occaſions ſtifles, if I may ſo ſpeak, the ſmell, and keeps it from being commonly taken notice of; yet if you purpoſely cauſe, as I remember I have done, the uſe of water to be forborn, your ſucceſs will not be like mine, if you do not find that ſtore of fœtid exhalations will be produced: and though it be not always ſo eaſy to diſcern by the ſmell from which of the two bodies they iſſue, or whether they proceed from both, yet it ſeems probable enough that ſome of the ſteams come from the iron, and it is more than probable that if they proceed not from that metal, they muſt from a body that is ſo hard as to be able to make impreſſions, in a trice, upon iron and ſteel themſelves.

THE laſt example I ſhall name under this head, is furniſhed me by marchaſites, ſome of which would, after a ſhort concuſſion without external heat, be made to exhale for a pretty while together a ſtrong ſulphureous odour, and yet were ſo hard, that when ſtruck with a ſteel-hammer (which would not eaſily break them) they afforded us ſuch a number of ſparks as appeared ſtrange enough; and it is known that it is from their diſpoſition to ſtrike fire (which yet I dare not attribute to all ſorts of marchaſites) that this kind of mineral is, by a name frequently to be met with in writers, called pyrites: and in this example we may take notice, that a body capable of being the ſource of corporeal emanations may be at once both very ſolid and very ponderous.

IT remains now that I manifeſt, that even the fixedneſs of bodies is not incompatible with their diſpoſition to emit effluviums.

I MIGHT alledge on this occaſion that the regulus of antimony and alſo its glaſs, though they muſt have endured fuſion to attain their reſpective forms; yet they will without heat communicate to liquors antimonial expirations, with which thoſe liquors being impregnated, become emetick and purgative; I might alſo add, that divers electrical bodies are very fixed in the fire, and particularly that cryſtal, as we have more than once tried, will endure ſeveral ignitions and extinctions in water, without being truly calcined, being indeed but cracked into a great multitude of little parts; but becauſe the above named antimonial bodies will after a while fly away in a ſtrong fire, and becauſe the effluviums of cryſtal are not ſo ſenſible as thoſe which can immediately affect our eyes or noſtrils, I will here ſubjoin one inſtance, ſuch as I hope will make it needleſs for me to add any more, it being of a body which muſt have ſuſtained any exceeding vehement fire, and is looked upon by moſt of the chemiſts as more undeſtroyable than gold itſelf; and that is glaſs, which is able, as you know, to endure ſo great a brunt of the fire, that you did not perhaps imagine I ſhould of all bodies name it on this occaſion, but my conjectures about the atmoſpheres of bodies leading me to think that glaſs itſelf might afford me a confirmation of them, I quickly found, that by rubbing a very little while two ſolid pieces of it (not, as I remember, of the finer ſort) one againſt the other, they would not only yield a ſenſible odour, but ſometimes ſo ſtrong an one as to be offenſive: by which you will eaſily perceive why I told you above that I did not acquieſce in the Carteſian argument againſt electrical bodies performing their operations by emanations of their own ſubſtance, drawn from hence, that glaſs does attract light bodies, as indeed it does, though but weakly; and yet is too fixed to emit effluviums, the contrary of which ſuppoſition the lately mentioned experiment, and by us often repeated, does ſufficiently evince.

From what other solid bodies, and that will endure the fire, I have, or have not been able to obtain such odorous steams, it is not necessary to declare in this place, but may perhaps be done in another.

You may, I presume, have taken notice, that, according to what I intimated a while ago, I have forborn in the precedent examples to mention those effluvia of solid bodies that need the action of the fire to be obtained: but since the sun is the grand agent of nature in the planetary world, and since during the summer, and especially at noon, and in southern climates, his heat makes many bodies have little atmospheres that we cannot so well discern that they have constantly; I see not why I may not be allowed to ascribe atmospheres to such bodies as I have observed to have them when the sun shines upon them; and also to think, that the like may be attributed, at least sometimes, to such other bodies as will do the things usually performed by effluviums when yet they are excited but by an external heat, which exceeds not that of the hot sun.

Of these two sorts of bodies I shall for brevity's sake name but two or three examples, and then hasten to a conclusion.

The first of these I must make bold to borrow from my observations about electricity, among which this is one, that to shew that the particular and usual manner of exciting such bodies, namely by rubbing them, is not always necessary, I took a large piece of good amber, and having in a summer morning, whilst the air was yet fresh, tried, that it would not without being excited attract a light body I had exposed to it, I removed it into the sun's beams, till they had made it moderately hot, and then I found according to my expectation, that it had acquired an attractive virtue, and that not only in one particular place, as is usually observed, when it is excited by rubbing, but in divers and distant places at once; at any of which it would draw to it the light body placed within a convenient distance from it; so that even in this climate of ours a solid body may quickly acquire an atmosphere by the presence of the sun, and that long before the warmest part of the day.

The next instance you will perchance think somewhat strange, it being that, when for want of an opportunity to make the like trial in the warm sun, I took a little but thick vessel made of glass, and held it near the fire till it had got a convenient degree of heat (which was not very great, though it exceeded that which I had given the amber) I found, as I had imagined, that the heat of fire had made even this body attractive, as that of the sun had made the other.

What degree of heat I have observed to be either necessary or the most convenient to excite electrical bodies according to their different natures (for the same degree will not indifferently serve for them all) this is not the properest place to declare, that it will be more to our present purpose to make some short reflection on what has been hitherto delivered.

It seems then probably deducible from the foregoing experiments and observations, that a very great number, if not the greatest part, even of consistent bodies, whether animal, vegetable, or mineral, may emit effluviums, and that even those that are solid may, at least sometimes, have their little atmospheres, though the neighbouring solids will often keep the evaporations from being every way ambient in reference to the bodies they issue from.

For as the instances hitherto alledged (which are not all that I could have named) do plainly shew that divers bodies, and some that have not been thought very likely, are such as we speak of; so several things induce me to believe that there may be many more of the like nature.

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For first, very few, if any, have (that I know of) had the curiosity to make use of nice scales, which such trials require, to examine the expirations of inanimate bodies, which if they shall hereafter do, I make little doubt but they will light on many things that will confirm what we have been proposing, by their finding that some bodies which are not yet known to yield exhalations, do afford them, and that many others do part with far more copious ones than is imagined: for one would not easily have thought that so extremely cold a body as a solid piece of ice should make a plentiful evaporation of itself in the cold air of a freezing night; or that a piece of wood that had long lain in the house and was light enough to be conveniently hung for a long time at a balance that would lose its æquilibrium with, as I remember, half a quarter of a grain, should in less than a minute of an hour send forth steams enough to make the scales manifestly turn, and that in winter.

But supposing (which is my second consideration) that trials were made with good instruments for weighing; though it will follow, that in case the exposed body grow lighter, something exhales from it, yet it will not follow, that if no diminution of weight be discovered by the instrument nothing that is corporeal recedes from it. I will not urge that it is affirmed, not only by the generality of our chemists, but by learned modern physicians, that when either glass of antimony or crocus metallorum impregnate wine with vomitive and purgative particles, they do it without any decrement of their weight, because the scales in apothecaries shops, and the little accurateness wont to be employed in weighing things, by those that are not versed in statical affairs, made me, though not deny the tradition, which may perchance be true, yet unwilling to build upon observations, which to be relied on are to be very nicely made; and therefore I shall rather take notice, that though the loadstone be concluded to have constantly about it a great multitude of magnetical effluvia, which may be called its atmosphere, yet it has not been observed to lose any thing of its weight by the recess of so many corpuscles; but because, if the Cartesian hypothesis about magnetisms be admitted, the argument drawn from this instance will not be so strong as it seems, and as it otherwise would be, I shall add a more unexceptionable example; for I know you will grant me that odours are not diffused to a distance without corporeal emanations from the odorous body; and yet, though good ambergris be, even without being excited by external heat, constantly surrounded by a large atmosphere, you will in one of the following discourses find cause to admire how inconsiderable the waste of it is.

If it be said, that in tract of time a decrement of weight may appear in bodies, that in a few hours or days discovers not any; the objection, if granted, overthrows not our doctrine; it being sufficient to establish what we have been saying, if we have evinced that the effluvia of some bodies may be subtle enough not to make the body by their avolation appear lighter in statical trials that are not extraordinarily (and as it were obstinately) protracted: and this very objection puts me in mind to add, that, for aught we know, the decrement of bodies in statical experiments long continued may be somewhat greater than even nice scales discover to us; for how are we sure that the weights themselves, which are commonly made of brass (a metal very unfixed) may not in tract of time suffer a little diminution of their weight, as well as the bodies counterpoised by them? and no man has, I think, yet tried whether glass, and even gold, may not in tract of time lose of their weight, which, in case they should do, it would not be easily discovered unless we had bodies that were perfectly fixed, by comparison to which we might be better assisted, than by comparing them with brass weights, or the like, which being themselves less fixed will lose more than gold and glass.

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My third and last consideration is, that there may be divers other ways, besides those furnished us by staticks, of discovering the effluvia of solid bodies, and consequently of shewing that it is not safe to conclude, that because their operation is not constant or manifest, such bodies do never emit any effluvia at all, and so are uncapable to work by their intervention on any other body, though never so well disposed to receive their action; and this I the rather desire that you would take notice of, because my chief (though not only) design in those notes is (you know) to illustrate the doctrine of occult qualities; and it may conduce to explicate several of them to know that some particular bodies emit effluvia, though perhaps they do it not constantly and uniformly; and though, perchance too, they do not appear to emit any at all, if they be examined after the same manner with other exhaleable bodies, but only may be made to emit them by some peculiar way of handling them, or appear to have emitted them by some determinate operation on some other single body, or at most small number of bodies.

Perchance you did not think, until you read what I lately told you about glass, that from a body that had endured so violent a fire, there could, by so slight a way as rubbing a little while one piece against another, be obtained such steams as may not only affect but offend the nostrils: nor should we easily believe, if experience did not assure us of it, that a diamond, that is justly reputed the hardest known body in the world, should by a little rubbing be made to part with electrical effluvia; nay (that I may give some kind of confirmation to that part of the last paragraph that seems most to need it) I shall add, that I once had a diamond not much bigger than a large pea, which had never been polished or cut, whose electrical virtue was sometimes so easily excited, that if I did but pass my fingers over it to wipe, the virtue would disclose itself; and if, as soon as I had taken it out of my pocket, I applied a hair to it, though I touched not the stone with my fingers, that I might be sure not to rub it, that hair would be attracted at some distance, and many times one after another, especially by one of the sides of the stone (whose surface was made up of several almost triangular planes); and though this excitation of the diamond seemed to proceed only from the warmth that it had acquired in my pocket, yet I did not find that that warmth, though it seemed not to be altered, had always the same effect on it, though the wiping it with my finger failed not (that I remember) to excite it: something like this uncertainty I always observed in another diamond of mine, that was much nobler than the first and very well polished, and in a small ruby that I have yet by me, which would sometimes be considerably electrical without being rubbed, when I but wore the ring it belonged to on my little finger; and sometimes again it seemed to have lost that virtue of operating without being excited by friction, and that sometimes within a few minutes, without my knowing whence so quick a change should proceed. But I must insist no longer on such particulars of which I elsewhere say something; and therefore I proceed to take notice that we should scarce have dreamed, that when a partridge or hunted deer has casually set a foot upon the ground, that part where the footstep hath been (though invisibly) impressed, should continue for many hours a source of corporeal effluxes, if there were not setting-dogs, and spaniels, and blood-hounds, whose noses can take notice at that distance of time of such emanations, though not only other sorts of animals, but other sorts of dogs are unable to do so.

I saw a stone in the hands of an academick, an acquaintance of mine, which I should by the eye have judged it to be an agate, not a blood-stone; and consequently I should not have thought that it could have communicated medicinal effluvia appropriated to excessive bleedings, if the wearer of it had not been subject to that disease, and had not often cured both

both himſelf and others by wearing this ſtone about his neck; which if he left off, as ſometimes he did for trial's ſake, his exceedingly ſanguine complexion (to which I have rarely ſeen a match) would in a few days caſt him into relapſes: what I have elſewhere told you about the true virtues of ſome ſtones (for I fear that moſt of thoſe that are wont to be aſcribed to them are falſe) may give ſome confirmation to what I have been delivering, which I cannot now ſtay to do, being to draw to a concluſion as ſoon as I have put you in mind, that it would not probably have ever been expected that ſo ponderous and ſolid a body as the loadſtone ſhould be environed by an atmoſphere, if iron had been a ſcarce mineral, and had not chanced to have been placed near it.

AND with this inſtance I ſhall put an end to theſe notes, becauſe it allows me to make this reflection, that ſince ſolid bodies may have conſtant atmoſpheres about them, and yet not diſcover that they have ſo, but by their operation upon one particular body, or thoſe few which participate of that; and ſince there are already (as we have ſeen) very differing ways whereby bodies may appear to be exhaleable, it is not unlikely that there may be more and more bodies (even of thoſe that are ſolid and hard) found to emit effluvia, as more and more ways of diſcovering that they do ſo, ſhall either by chance or induſtry be brought to light.

AN INVENTION FOR Eſtimating the Weight of Water in Water, with ordinary Balances and Weights.

First Printed in the PHILOSOPHICAL TRANSACTIONS, No. L. p. 1001.

"THE author of this invention is the noble *Robert Boyle*; who was pleaſed to
" comply with our deſires of communicating it in Engliſh to the curious in *Eng-*
" *land*, as by inſerting the ſame in the Latin tranſlation of his Hydroſtatical Paradoxes
" he hath gratified the ingenious abroad: and it will doubtleſs be the more welcome,
" for as much as no body we know of hath ſo much as attempted to determine, how
" much water may weigh in water; and poſſibly if ſuch a problem had been propoſed
" it would have been judged impracticable.

" THE method or expedient he made uſe of to perform it as near as he could, may
" be eaſily learned by the enſuing account of a trial or two he made for that purpoſe,
" which among his notes he cauſed to be regiſtered in the following words."

A glaſs-bubble of about the bigneſs of a pullet's egg was purpoſely blown at the flame of a lamp, with a ſomewhat long ſtem turned up at the end, that it might the more conveniently be broken off: this bubble being well heated to rarefy the air and thereby drive out a good part of it, was nimbly ſealed at the end, and by the help of the figure of the ſtem was by a convenient weight of lead depreſſed under water, the lead and glaſs being tied by a ſtring to one ſcale of a good balance, in whoſe other there was put ſo much weight as ſufficed to counterpoiſe the bubble as it hung freely in the midſt of the water, then with a long iron forceps I carefully broke off the ſealed end of the bubble under water, ſo as no bubble of air appeared to emerge or eſcape through the water, but the liquor by the weight of the atmoſphere ſprung into the unrepleniſhed part of the glaſs-bubble, and filled the whole cavity about half full; and preſently, as I foretold, the bubble ſubſided and made the ſcale it was faſtened to preponderate ſo much

much that there needed four drachms and 38 grains to reduce the balance to an æquilibrium; then taking out the bubble with the water in it, we did, by the help of the flame of a candle, warily applied, drive out the water (which otherwiſe is not eaſily excluded at a very narrow ſtem) into a glaſs counterpoiſed before; and we found it, as we expected, to weigh about four drachms and thirty grains, beſides ſome little that remained in the egg, and ſome ſmall matter that may have been rarefied into vapours, which, added to the piece of glaſs that was broken off under water and loſt there, might very well amount to ſeven or eight grains; by which it appears not only that water hath ſome weight in water, but that it weighs very near * or altogether as much in water as the ſelf ſame portion of liquor would weigh in the air.

The ſame day we repeated the experiment with another ſealed bubble larger than the former, being as big as a great hen-egg, and having broken this under water, it grew heavier by ſeven drachms and thirty-four grains; and having taken out the bubble and driven out the water into a counterpoiſed glaſs, we found the tranſvaſated liquor to amount to the ſame weight, abating ſix or ſeven grains, which it might well have loſt upon ſuch accounts as have been newly mentioned.

* This expreſſion was added, to leave liberty for a further inquiry, whether the experiment which hereby appears convincing as to the main thing intended to be proved, may not admit the having ſomething further debated and annexed about ſome circumſtantial thing or other.

TRACTS

ABOUT

The Cofmical Qualities of Things.

Cofmical Sufpicions.

The Temperature of the Subterraneal Regions.

The Temperature of the Submarine Regions.

The Bottom of the Sea.

To which is prefixed,

An Introduction to the Hiftory of PARTICULAR QUALITIES.

An ADVERTISEMENT of the PUBLISHER to the READER.

THough the noble author of the following tracts hath alfo written divers other fhort difcourfes upon feveral occafions, yet, had he not been diverted from his purpofe, he had continued to let them lie by him, intending, in cafe he fhould fuffer them to come abroad, to difpofe of them agreeably to a defign that it is not neceffary the reader fhould be now acquainted with.

IN the mean while, feveral virtuofi, to whom fome of thefe tracts had been fhewn, and with whom the matters handled in fome others had been difcourfed, did, out of a concern (as they gave out) for the commonwealth of learning, preffingly reprefent to the author:

FIRST, That divers of thefe loofe tracts, having little or no dependency upon one another, might without inconvenience be publifhed apart, in what number and order the author fhould pleafe.

SECONDLY, That fince his main defign in thefe, as well as his other phyfical writings, was to provide materials for the hiftory of nature, it would be thought enough that they be be ſubſtantial and fit for the work, in what order or aſſociation ſoever they ſhould happen to be brought into the philoſophical repoſitory.

THIRDLY, That the communicating theſe tracts to the curious would be the beſt way to ſecure them from being loſt or embezzled, as ſome others of his papers have been, not only formerly but very lately.

FOURTHLY, That the kind reception the curious had given to what he had hitherto preſented them, might well invite, if it did not oblige him, not to envy them the early uſe of thoſe experiments and hints, which will, probably before the time wherein his deſign would ſuffer them to come abroad, prove ſerviceable to philoſophy, by ſetting divers inquiſitive heads on work, exciting the curioſity of ſome, and exerciſing the induſtry of others.

LASTLY, That, as of the pieces, he had hitherto publiſhed (except where his own backwardneſs had expreſſedly interpoſed) the firſt edition had not long been the only, ſo probably within a moderate ſpace of time, another edition of thoſe tracts he ſhould firſt put out, would both allow him to increaſe their number and change their order as he ſhould judge moſt expedient, and (in caſe he ſhould in the mean while return to his library) recruit his diſcourſes with thoſe paſſages that he deſigned to borrow for them thence.

BUT though theſe conſiderations, joined to the earneſtneſs of the perſons that made them, and the juſt reſpect he had for them, rendered it uneaſy for him to reſiſt their perſuaſions, yet they never obtained an actual compliance, until they were aſſiſted by ſuch an unhappy juncture of ſickneſs and buſineſs, as, leaving him ſmall hopes of accompliſhing his firſt intentions in any reaſonable time, made him conſent to ſend away to the preſs ſome of thoſe tracts that he found the leaſt unready for it, in the order wherein they chanced to come to his hands; which being thus repreſented, the conſidering and ingenious reader will ſoon find what cauſe there is, and how much it concerns the advancement of valuable philoſophy, that, ſince this excellent author hath (to the publiſher's knowledge, as alſo was inſinuated above) many other rare tracts of a philoſophical nature in ſtore, he be ſolicited from time to time that he would be pleaſed, according to the meaſure of health he ſhall enjoy, to impart with all poſſible ſpeed thoſe diſcourſes which tend to the enlargement and progreſs of uſeful knowledge, maugre all envy and malice.

H. O.

THE HISTORY OF PARTICULAR QUALITIES.

CHAPTER I.

THE paſt diſcourſe has, I hope, *Pyrophilus*, given you ſome tolerable account both of the nature and of the origin of qualities in general; wherefore it now follows that we proceed to qualities in particular, and conſider how far the manner whereby they are produced, and thoſe other phænomena of them that we ſhall have occaſion to take notice of, will accord with, and thereby confirm the doctrine I have hitherto propoſed; and whether they will not, at leaſt, much better comport with that, than with the opinions either of the peripateticks or the chymiſts.

I SHALL not ſpend time to enquire into all the ſeveral ſignifications of the word quality, which is uſed in ſuch various ſenſes, as to make it ambiguous enough; ſince by the ſubſequent diſcourſe it will ſufficiently appear in which of the more uſual of thoſe ſignifications we employ that term. But thus much I think it not amiſs to intimate in this place, that there are ſome things that have been looked upon as qualities which ought rather to be looked upon as ſtates of matter, or complexions of particular qualities, as animal, inanimal, &c. health and beauty, which laſt attribute ſeems to be made up of ſhape, ſymmetry, or comely proportion, and the pleaſantneſs of the colours of the particular parts of the face. And there are ſome other attributes, namely, ſize, ſhape, motion, and reſt, that are wont to be reckoned among qualities, which may more conveniently be eſteemed the primary modes of the parts of matter; ſince from theſe ſimple attributes, or primordial affections, all the qualities are derived. But this conſideration relating to words and names, I ſhall not inſiſt upon it.

NOR do I think it worth while to enumerate and debate the ſeveral partitions that have been made of qualities (of which I have met with divers, and could perchance myſelf increaſe the number of them) for though one that were diſpoſed to criticize upon them, would not perhaps acquieſce in any of them, but look upon them as being more arbitrary, than grounded upon an attentive conſideration of the nature of the things themſelves; yet becauſe it ſeems not to me ſo eaſy to make an accurate diſtribution of qualities, till ſome things that concern them be better cleared up than yet they are, I ſhall content myſelf for the preſent, to propoſe to you one of the more received diviſions of phyſical qualities (for you know I do not pretend to treat of any other) allowing myſelf the liberty of making, where there ſeems cauſe, the members of the diſtribution ſomewhat more comprehenſive. We will then, with many of the moderns, divide phyſical qualities into manifeſt and occult; and reſerving the latter to be treated of apart, we will diſtribute the former into firſt, ſecond, and third; to the two laſt of which we will reſerve divers qualities not wont to be treated of by ſchool writers of phyſical ſyſtems, which, for diſtinction ſake, we may without much inconveniency ſtile ſome of them the chemical qualities of things, becauſe as *Ariſtotle* and the ſchool-men were not acquainted with them, ſo they have been principally introduced and taken notice of by means of chemical operations and experiments; ſuch as are fumigation, amalgamation, cupellation, volatilization, precipitation, &c. by which operations, among other means, corporeal things come to appear volatile or fixed, ſoluble or inſoluble in ſome menſtruums, amalgamable or unamalgamable, capable or uncapable to precipitate ſuch bodies, or be precipitated by them, and, in a word, acquire or loſe ſeveral powers to act on other bodies, or diſpoſitions to be wrought on by them; which attributes do as well deſerve the name of qualities, as divers other attributes to which it is allowed. And to theſe chemical qualities we may add ſome others, which, becauſe of the uſe that phyſicians either only, or above other men make of them, may be called medical, whereby ſome bodies taken into that of a man are deoppilating, others inciding, reſolving, diſcuſſing, ſuppurating, abſterſive of noxious adherences, and thickening the blood and humours, being aſtringent, anodinous or appeaſing pain, &c. for though ſome of the faculties of medicines, as thoſe of heating, cooling, drying, attenuating, purging, &c. may be conveniently enough referred to the firſt, ſecond, or third qualities wont to be mentioned by naturaliſts, and others are wont to be reckoned among occult ones; and though theſe medical qualities are wont to be treated of by phyſicians, yet it ſeems to me that divers of them ought not to be referred to the qualities to which they are wont to be ſo; and the handling of them may be looked upon as a deſideratum in natural philoſophy, and may well enough deſerve a diſtinct place there; ſince the writers of that ſcience are not wont to treat of them at all, and phyſicians handle them as phyſicians, whom it concerns but to know what bodies are endowed with them, and what good or ill effects they may have upon human bodies, not as naturaliſts, whoſe buſineſs it is to enquire into the production and cauſes of thoſe as well as of other qualities.

C H A P. II.

Before we deſcend to the mention of any of theſe particular qualities, I think it very expedient to ſpend a little time in conſidering three grand ſcruples about our and the corpuſcularian doctrine touching qualities, which three difficulties, though I remember to have found them expreſly objected by the adverſaries of the corpuſcularian philoſophy, nor (perhaps only for that reaſon) to have been purpoſely ſolved by the patrons of it, are yet ſuch, that having been ſuggeſted to me by conſidering the nature of the thing, I cannot but fear that they alſo may occur to and trouble you; ſince they ſeem to me of that importance, that unleſs they be removed, they may very much prejudice the reception of a good part of what I am to deliver about particular qualities.

The firſt of the above mentioned objections is grounded upon the received opinion of vulgar and Ariſtotelian philoſophers, that diverſity of qualities muſt needs flow from ſubſtantial forms, either becauſe it is part of their nature to be the principals of properties, and peculiar operations in the bodies they inform; or elſe becauſe divers of them are ſuch, that no mixture of the elements is capable of producing them.

Of

Of the two suppositions whereon this difficulty is founded, we have already shewed the former to be unfit to be admitted, by what has been said in our examen of substantial and subordinate forms; and therefore it will only remain that we examine also this second supposition, which may therefore deserve the greater consideration, because it is much pressed and relied on by the learned *Sennertus* (and his followers) who improves the argument by this addition, that as no bare mixture of the elements, so no general *forma mistionis* (such as divers of the moderns have introduced to help out the hypothesis) is sufficient to give an account of divers qualities, which he somewhere reckons up.

But, in the first place, whereas the proposers of this difficulty take it for granted that there are four elements, from whose various mixtures all other sublunary bodies spring, and are therefore only solicitous to prove that such and such qualities cannot flow from their mixture; I need not much concern myself for their whole discourse, since I admit not that hypothesis of the four elements that is supposed in it; and yet I may be allowed to observe from hence, that by the confession of those modern peripateticks that urge this argument, those ancient and other Aristotelians were mistaken, who ascribed to the mixture of the elements effects, for which these maintain them to be incompetent.

But since replies of this nature do rather concern the objectors than the objection, I proceed to consider the difficulty itself, not only as it may be proposed by peripateticks, but by chemists, who, though some of them do not with others of their sect allow of the four elements, do yet agree with the schools in this, that there is a determinate number of ingredients of compounded bodies, from whose mixture and proportion many qualities must be derived; and those that cannot, must be resolved to flow from a higher principle, whether it be a substantial form, or something for which chemists have several names, though, I doubt, no settled and intelligible notion.

To consider then the difficulty itself, I shall for the removal of it present to you four principal considerations.

But before I begin by any of these to answer the objection, I shall readily acknowledge, that in some respects, and in some cases, it may not be ill grounded; but I shall add, that in those cases I look upon it rather as a part of the corpuscularian doctrine than an objection against it; for when it happens that there is a strict connexion betwixt that modification of matter which is requisite to exhibit one phænomenon, and that from which another will necessarily follow; in such case we may not only grant, but teach, that he, who by a change of its texture gives a portion of matter the former modification, does likewise qualify it by the same change to exhibit the congruous phænomenon; though one would not perchance suspect them to have any such dependence upon one another. As for instance, strong spirit of distilled vinegar, by virtue of its being an acid spirit, hath the faculty to turn syrup of violets red; but if by making with this spirit as strong a solution as you can of coral, or some such body, you destroy the acidity of the spirit of vinegar; this liquor, as it has quite another taste, so it may, and indeed will, have another operation than formerly upon syrup of violets. For I remember that upon a trial I purposely devised to illustrate this matter, I found that the lately mentioned solution, and some others made with spirit of vinegar, would presently, like an alkalizate or urinous salt, turn syrup of violets from its native blue, not any longer into a red, but into a lovely green; and prosecuting the experiment a little farther, I found that spirit of salt itself deflegmed by a fit concrete, though the solution were horribly strong, had yet the same effect on syrup of violets. But because the cases where the above-mentioned connexion of qualities and modifications occur, are comparatively but few, I shall here consider them no farther, but proceed to the four particulars I was lately proposing.

And

And in the firſt place, I ſay, that things may acquire by mixture very differing qualities from thoſe of any of the ingredients.

Of this I ſhall have occaſion to give a multitude of inſtances in the following notes upon particular qualities; and therefore it may now ſuffice to mention two or three, that are the more obvious in the laboratories of chemiſts; as, that ſugar of lead is extremely ſweet, though the minium, and the ſpirit of vinegar of which it is made, be the former of them inſipid, and the latter ſour. And though neither aqua regis, nor crude copper, have any thing in them of blue, yet the ſolution of this metal in that liquor is of a deep blue; and ſometimes I have had the ſolution of crude mercury in good aqua fortis of a rich green, though it would not long continue ſo: and of ſuch inſtances you will, as I was ſaying, hereafter meet with plenty. So that they are much miſtaken who imagine either that no manifeſt qualities can be produced by mixture, except thoſe that reſide in the elements, or reſult immediately from the combinations of the four firſt qualities. For not to repeat what variations the mixtures of the moſt ſimple ingredients only may produce; it is manifeſt that nature and art muſt continually make mixtures of bodies, both of already compounded bodies, as when aſhes and ſand compoſe the common coarſe glaſs, and when nature combines ſulphur with unripe vitriol, and perhaps other ſubſtances in a marchaſite; and alſo of bodies already decompounded, as native vitriol is made in the bowels of the earth of an aqueous liquor impregnated with an acid ſalt, and of a cupreous or martial mineral, ſtrictly united both to a combuſtible ſulphureous ſubſtance, and to another body of a more fixed terreſtrial nature. And thus artificers may eaſily, as trial hath aſſured me, produce new and fine colours, by ſkilfully mixing in the flame two pieces of ammels (which are already decompounded bodies) of colours more ſimple or primary than that which reſults from their colliquation. And this way of ſo combining bodies, not ſimple or elementary, will be acknowledged capable of being made much more fertile in the production of various qualities and phænomena of nature, if you conſider how much the variation of the proportion of the ingredients in a mixed body may alter the qualities and operations of it, and that proportion is capable of being varied almoſt *in infinitum*. Thus much may ſuffice for our firſt conſideration; eſpecially ſince divers things by which it may be much confirmed, will be met with in the two following chapters.

In the ſecond place I obſerve, that it is but an ill-grounded hypotheſis to ſuppoſe that new qualities cannot be introduced into a mixed body, or thoſe that it had before be deſtroyed, unleſs by adding or taking away a ſenſible portion of ſome one or more of the Ariſtotelian elements, or chemical principles; for there may be many changes as to quality produced in a body without viſibly adding or taking away any ingredient, barely by altering the texture, or the motion of the minute parts it conſiſts of: for when (for inſtance) water hermetically ſealed up in a glaſs is by the cold of the winter turned into ice, and thereby both loſeth its former fluidity and tranſparency, and acquires firmneſs, brittleneſs, and oftentimes opacity, all which qualities it loſeth again upon a thaw; in this caſe, I ſay, I demand what element or hypoſtatical principle can be proved to get into, or out of this ſealed glaſs, and by its intruſion and receſs produce theſe alterations in the included body. And ſo in that fixed metal, ſilver, what ſenſible acceſſion or decrement can be proved to be made as to ingredients, when by barely hammering it (which doth but change the ſituation and texture of the parts) it acquires a brittleneſs, which by ignition, wherein it doth not ſenſibly loſe any thing, it may preſently be made to exchange for its former malleableneſs? And the ſame experiment gives us an inſtance alſo that the inviſible agitation of the parts may alone ſuffice to give a body, at leaſt for a while, new qualities; ſince a thick piece of ſilver nimbly hammered will quickly acquire

quire a confiderable degree of heat, whereby it will be enabled to melt fome bodies, to dry others, and to exhibit divers phænomena that it could not produce when cold. I might add, that fpirit of nitre moderately ftrong, though when included in a well-ftopped phial in the form of a liquor it will appear diaphanous, and without any rednefs, will yet fill the upper part of the phial with red fumes, if the warm fun-beams or any fit heat (though but externally applied, and though the glafs continue clofe ftopped) do put the nitrous fpirits into a fomewhat brifker motion than they had or needed whilft in the form of a liquor. I might alfo demand both what new element or principle is added to a needle, when the bare approach of a vigorous loadftone endows it with thofe admirable qualities of refpecting the poles, and (in due circumftances) drawing to it other needles; and what ingredient the fteel lofes, when by a contrary motion of the loadftone, it is in a minute deprived of its magnetifm. And to thefe I might fubjoin divers like queftions; but of inftances and reflections proper to confirm this fecond confideration you may meet with fo many, partly in another treatife, and partly in the enfuing chapters, that it will be needlefs to multiply them here. Wherefore in the third place I fhall obferve, that when we are confidering how numerous and various phænomena may be exhibited by mixed bodies, we are not to look upon them precifely in themfelves; that is, as they are portions of matter, of fuch a determinate nature or texture, but as they are parts of a world fo conftituted as ours is, and confequently as portions of matter, which are placed among many other bodies. For being hereby fitted to receive impreffions from fome of thofe bodies, and to make impreffions upon others of them, they will upon this account be rendered capable of producing, either as principal or auxiliary caufes, a much greater number and variety of phænomena than they could exhibit, if each of them were placed in *vacuo* (or if a vacuum be a thing impoffible) in a medium, that could no way either contribute to, or hinder its operations.

This has been partly proved already in the difcourfe of the origin of forms, and will be farther manifefted ere long; and therefore it may fuffice, that of the particulars mentioned in thofe writings, thofe that are pertinent to this argument be mentally referred hither.

Wherefore having thus difpatched the third confideration, I now proceed to the fourth and laft, which is, that the four peripatetick elements and the three chemical principles are fo infufficient to give a good account of any thing near all the differing phænomena of nature, that we muft feek for fome more catholick principles; and that thofe of the corpufcularian philofophy have a great advantage of the other in being far more fertile and comprehenfive than they. I muft not here ftay to make full reprefentation of the deficiencies of the Ariftotelian hypothefis, having in other tracts faid much to that purpofe already; but yet our prefent argument invites me to intimate thefe two things; the firft, that fuch phænomena as the conftant and determinate fhape and figure of the mountains our telefcopes difcover (together with their fhadows) in the moon, and the ftrange generation and perifhing of the fpots of the fun, to omit the differing colour of the planets, and divers other qualities of celeftial bodies, cannot be afcribed to the four elements, or their mixtures, nor to thofe of the three chemical principles, which are allowed to be confined to the fublunary region. And the fecond, that there are very many phænomena in nature (divers of which I * elfewhere take notice of) feveral whereof neither the peripatetick nor the chemical doctrine about the elements, or the ingredients of bodies, will enable a man to give fo much as any probable account; fuch are the eclipfes of the fun, the moon, and alfo the fatellites of Jupiter, the proportion of the

* Principally in the Sceptical Chymift.

accele-

acceleration of defcent obfervable in heavy bodies, ebbing and flowing of the fea, a great number of magnetical, mufical, ftatical, dioptrical, catoptrical, and other forts of phænomena, which hafte makes me here leave unmentioned.

And having faid thus much about the firft part of our propofed confideration, and thereby fhewn that the vulgar doctrine about the ingredients of bodies falls very fhort of being able to folve feveral kinds of nature's phænomena, we may add, in favour of the fecond part, that it will follow in general, that it is fit to look out for fome more pregnant and univerfal principles; and that, in particular, thofe of the corpufculary hypothefis are, as to thofe two attributes, preferable by far to the vulgar ones, will I hope appear by our anfwers to the two objections that remain to be examined in the two following chapters, to which that I might the more haften, I thought fit to infift the lefs upon the objection hitherto examined, efpecially becaufe partly in this and the two next chapters, and partly elfewhere, I fuppofe there is contained a very fufficient reply to that objection. And I confefs I fhould think it ftrange that the confideration of the various motions and textures of bodies fhould not ferve to folve far more phænomena than the bare knowledge of the number (and even that of the proportions) of their quiefcent ingredients; for as local motion is that which enables natural bodies to act upon one another, fo the textures of bodies are the main things that both modify the motion of agents, and diverfify their effects according to the various natures of the patients.

CHAP. III.

I enter now upon the confideration of the fecond, and indeed the grand difficulty objected againft the (corpufcularian) doctrine propofed by me about the origin of qualities, viz. that it is incredible that fo great a variety of qualities as we actually find to be in bodies fhould fpring from principles fo few in number as two, and fo fimple as matter and local motion; whereof the latter is but one of the fix kinds of motion reckoned up by *Ariftotle* and his followers, who call it lation, and the former, being all of one uniform nature, is according to us diverfified only by the effects of local motion. Towards the folving this difficulty, I fhall endeavour to fhew, firft, that the other catholick affections of matter are manifeftly deducible from local motion; and next, that thefe principles being varioufly affociated, are fo fruitful, that a vaft number of qualities andother phænomena of nature may refult from them.

The firft of thefe will not take us up much time to make out; for fuppofing, what is evident, that the (1) local motion belonging to fome parts of the univerfal matter does not at all tend the fame way, but has various determinations in feveral parts of that matter, it will follow, that by local motion thus circumftanced, matter muft be divided into diftinct parts; each of which being finite, muft neceffarily be of fome (2) bignefs or fize, have fome determinate (3) fhape or other.

And fince all the parts of the univerfal matter are not always in motion, fome of them being arrefted by their mutual implication, or having transferred (as far as our fenfes inform us) all that they had to other bodies, the confequence will be, that fome of thefe portions of the common matter will be in a ftate of (4) reft (taking the word in the popular fenfe of it). And thefe are the moft primary and fimple affections of matter.

But becaufe there are fome others that flow naturally from thefe, and are, though not altogether univerfal, yet very general and pregnant; I fhall fubjoin thofe that are the moft fertile principles of the qualities of bodies and other phænomena of nature.

MOREOVER then not only the greater fragments of matter, but those lesser ones, which we therefore call corpuscles or particles, have certain local respects to other bodies, and to those situations which we denominate from the horizon; so that each of these minute fragments may have a particular (5) posture or position (as erect, inclining, horizontal, &c.) and as they respect us men that behold them, there may belong to them a certain (6) order or consecution, upon whose account we say one is before or behind another; and many of these fragments being associated into one mass or body, have a certain manner of existing together, which we call (7) texture, or by a word more comprehensive, modification. And because there are a very few bodies whose constituent parts can, because of the irregularity or difference of their figures, and for other reasons, touch one another every where so exquisitely, as to leave no intervals between them, therefore almost all consistent bodies, and those fluid ones that are made up of grosser parts, will have (8) pores in them, and very many bodies having particles, which by their smallness, or their loose adherence to the bigger, or more stable parts of the bodies they belong unto, are more easily agitated and separated from the rest by heat and other agents; therefore there will be great store of bodies that will emit those subtle emanations that are commonly called (9) effluviums. And as those conventions of the simple corpuscles that are so fitted to adhere to, or be complicated with one another, constitute those durable and uneasily dissoluble clusters of particles that may be called the primary concretions or elements of things, so these themselves may be mingled with one another, and so constitute compounded bodies; and even those resulting bodies may, by being mingled with other compounds, prove the ingredients of decompounded bodies, and so afford a way whereby nature varies matter, which we may call (10) mixture, or composition; not that the name is so proper as to the primary concretions of corpuscles; but because it belongs to a multitude of associations, and seems to differ from texture, with which it has so much affinity, as perhaps to be reducible to it, in this, that always in mixtures, but not still in textures, there is required a heterogeneity of the component parts. And every distinct portion of matter, whether it be a corpuscle or a primary concretion, or a body of the first, or of any other order of mixts, is to be considered, not as if it were placed in vacuo, nor as if it had relation only to the neighbouring bodies, but as being placed in the universe, constituted as it is, amongst an innumerable company of other bodies, whereof some are near it, and others very remote, and some are great and some small; some particular and some catholick agents; and all of them governed as well by (11) the universal fabrick of things, as by the laws of motion established by the author of nature in the world.

AND now, *Pyrophilus*, that we have enumerated 11 very general affections of matter, which with itself make up 12 principles of variation in bodies, let me on the behalf of the corpuscularians apply to the origin of qualities a comparison of the old atomists imployed by *Lucretius*, and others, to illustrate the production of an infinite number of bodies, from such simple fragments of matter as they thought their atoms to be. For since of the 24 letters of the alphabet associated several ways, as to the number and placing of the letters, all the words of the several languages in the world may be made; so, say these naturalists, by variously connecting such and such numbers of atoms, of such shapes, sizes, and motions, into masses or concretions, an innumerable multitude of different bodies may be formed. Wherefore, if to those four affections of matter, which I lately called the most primary and simple, we add the seven other ways, whereby, on whose account, it may be altered, that are, though not altogether, yet almost as catholick, we shall have eleven principles so fruitful, that from their various associations may result a much vaster multitude of phænomena, and among them of

qualities,

qualities, than one that does not confider the matter attentively would imagine. And to invite you to believe this, I fhall defire you to take notice of thefe three things.

The firft is, that fuppofing thefe ten principles were but fo many letters of the alphabet, that could be only put together in differing numbers, and in various orders, the combinations and other affociations that might be made of them, may be far more numerous than you yourfelf will expect, if you are not acquainted with the way of calculating the number of differing affociations that may be made between ten things propofed. The beft way I know of doing this is by algebra or fymbolical arithmetick, by which it appears, that of fo few things fo many (α) affociations may be made, each of which will differ from every one of the reft, either in the number of the things affociated, or in the order wherein they were placed.

But (which is the fecond thing to be taken notice of) each of thefe ten producers of phænomena admits of a fcarce credible variety. For not to defcend fo low as infenfible corpufcles (many thoufands of which may be requifite to conftitute a grain of muftard feed) what an innumerable company of different bigneffes may we conceive between the bulk of a mite, a crowd of which is requifite to weigh one grain, and a mountain, or the body of the fun, which aftronomers teach us to be above an hundred and threefcore times bigger than the whole terreftrial globe.

And fo though (β) figure be one of the moft fimple modes of matter, yet it is capable partly in regard of the furface, or furfaces of the figured corpufcles (which may confift of triangles, fquares, pentagones, &c.) and partly in regard of the fhape of the body itfelf, which may be either flat like a cheefe, or lozenge; or fphærical like a bullet; or elliptical, almoft like an egg; or cubical like a dye; or cylindrical like a rolling-ftone; or pointed like a pyramid, or fugar-loaf: figure, I fay, though but a fimple mode, is, upon thefe and other fcores, capable of fo great a multitude of differences, that it is concerning them, and their affections, that *Euclid*, *Apollonius*, *Archimedes*, *Theodofius*, *Clavius*, and later writers than he, have demonftrated fo many propofitions. And yet all the hitherto named figures are almoft nothing to thofe irregular fhapes, fuch as are to be met with among rubbifh, and among hooked and branched particles, &c. that are to be met with among corpufcles and bodies; moft of which have no particular appellations, their multitude and their variety having kept men from enumerating them, and much more from particular naming them.

To which let me add, that thefe varieties of figure and fhape do alfo ferve to modify the motion, and other affections of the corpufcle endowed with them, and of the compounded body, whereof it makes a part.

And that the (γ) fhape and alfo fize of bodies, whether fmall or great, may exceedingly diverfify their nature and operation, I fhall often have occafion to manifeft, and therefore I fhall now only give you a grofs example of it, by inviting you to confider how many differing forts of tools and inftruments, almoft each of them fit for many different operations and ufes, fmiths, and other not the nobleft fort of tradefmen, have been able to form out of pieces of iron, only by making them of differing fizes, and giving them differing fhapes. For when I have named bodkins, forks, blades, hooks, fciffars, anvils, hammers, files, rafps, chiffels, gravers, fcrews, vices, faws, borers, wires, drills, &c. when (I fay) I have named all thefe, I have left a far greater number unmentioned.

So likewife (δ) motion, which feems fo fimple a principle, efpecially in fimple bodies, may even in them be very much diverfified; for it may be more or lefs fwift, and that in an almoft infinite diverfity of degrees; it may be fimple or compounded, uniform or difform, and the greater celerity may precede or follow. The body may move in a straight line, or in a circular, or in some other curve line, as elliptical, hyperbolical, parabolical, &c. of which geometricians have described several, but of which there may be in all I know not how many more; or else the body's motion may be varied according to the situation or nature of the body it hits against, as that is capable of reflecting it, or refracting it, or both, and that after several manners: the body may also have an undulating motion, and that with smaller or greater waves; or may have a rotation about its own middle parts; or may have both a progressive motion and a rotation, and the one either equal to the other, or swifter than it, in almost infinite proportions. As to the determination of motion, the body may move directly upwards or downwards, decliningly or horizontally, east, west, north, or south, &c. according to the situation of the impellent body. And besides these and other modifications of the motion of a simple corpuscle or body, whose phænomena or effects will be also diversified, as I partly noted already, by its bulk and by its figure: besides all these, I say, there will happen a new and great variety of phænomena, when divers corpuscles, though primogenial, and much more if they be compounded, move at once, and so the motion is considered in several bodies; for there will arise new diversifications from the greater or lesser number of the moving corpuscles; from their following one another close, or more at distance; from the order wherein they follow each other; from the uniformity of their motion, or the confusedness of it; from the equality or inequality of their bulk, and the similitude or dissimilitude of their figures; from the narrowness or wideness, &c. of the channel or passage in which they move, and the thickness, thinness, pores, and the conditions of the medium through which they move; from the equal or unequal celerity of their motion, and force of their impulse: and the effects of all these are variable by the differing situation and structure of the sensories, or other bodies on which these corpuscles beat.

What we have elsewhere said to shew that local motion is, next the author of nature, the principal agent in the production of her phænomena, may I hope satisfy you that these diversities in the motion of bodies may produce a strange variety in their nature and qualities: and as I lately did, so I shall now adumbrate my meaning to you, by desiring you to apply to our present purpose what you may familiarly observe in musick; for according as the strings or other instruments of producing sounds do tremble more or less swiftly, they put the air into a vibrating motion more or less brisk, and produce those diversities of sounds, which musicians have distinguished into notes, which they have also subdivided, and whereto they have given distinct names. And though the bodies from whence these sounds proceed, may be of very differing (ε) natures; as metalline as wire, gut-strings, bells, human voices, wooden pipes, &c. yet, provided they put the air into the like waving motion, the sound and even the note will be the same; which shews how much that greater variety which may be taken notice of in sounds, is the effect of local motion. And if the sound come from an instrument, as a lute, where not only one string hath its proper sound, but many have among them several degrees of tension, and are touched, sometimes these, sometimes those, together; whereby more, or fewer, or none of their vibrations come to be coincident, they will so strike the air, as to produce, sometimes those pleasing sounds we call concords, and sometimes those harsh ones we call discords.

It would take up too much time to insist upon each of the ten remaining affections of matter that I lately enumerated and represented to you as exceeding fertile; and by what I elsewhere deliver about pores alone, and the many sorts of phænomena, in which they may have an interest, I could add no small confirmation to what has been hitherto discoursed; if the inserting of it here would not enormously encrease the bulk

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C H A P. IV.

The third and laſt difficulty that now remains to be conſidered, may be thus propoſed: that whereas, according to the corpuſcularian hypotheſis, not only one or two qualities, but all of them proceed from the bigneſs and ſhape, and contexture of the minute parts of matter, it is conſonant to their principles, that if two bodies agree in one quality, and ſo in the ſtructure on which that quality depends, they ought to agree in other qualities alſo; ſince thoſe do likewiſe depend upon the ſtructure wherein they do agree; and conſequently it will be ſcarce poſſible to conceive that two ſuch bodies ſhould be endowed with ſo many differing qualities, as experience ſhews they may.

To illuſtrate this objection by an example, it is pretended that the whiteneſs of froth proceeds from the multitude and hemiſpherical figure of the bubbles it is made up of. And if this or any other mechanical fabrick or contexture be the cauſe of whiteneſs, how comes it to paſs that ſome white bodies are inodorous and inſipid, as the calx of hartſhorn; others both ſtrongly ſcented and ſtrongly taſted, as the volatile ſalt of hartſhorn or of blood; ſome diſſoluble in water, as ſalt of tartar; others indiſſoluble in that liquor, as calcined hartſhorn, &c. ſome fixed in the fire, as the bodies laſt named; others fugitive, as powdered ſal armoniack; ſome incombuſtible, as ſalt of tartar; others very inflammable, as camphire. To which examples a greater variety of white bodies might be added, if it were neceſſary.

This I confeſs is a conſiderable difficulty may puzzle more than a novice in the corpuſcularian philoſophy; wherefore, to do ſomewhat in order to the clearing of it, I ſhall recommend to you the four following conſiderations:

1. And firſt I ſhall conſider, that in the pores of viſible and ſtable bodies there may be often lodged inviſible and heterogeneous corpuſcles, to which a particular quality that belongs to the body as ſuch, is to be referred. Thus we ſee in a perfumed glove, that in the pores of the leather odoriferous particles are harboured, which are of quite another nature than the leather itſelf, and wholly adventitious to it, and yet endue it with the fragrancy, for which it is prized. A like example is afforded us in raſberry wine made with claret; for the pleaſing ſmell is imparted to the wine, by the corpuſcles of the berries diſperſed *per minima* through the whole body of it.

2. The ſecond thing that I conſider is this, that oftentimes corpuſcles of very differing natures, if they be but fitted to convene, or to be put together after certain manners, which yet require no radical change to be made in their eſſential ſtructures, but only a certain

a straight line, or in a circular, or in some other curve line, as elliptical, hyperbolical, parabolical, &c. of which geometricians have described several, but of which there may be in all I know not how many more; or else the body's motion may be varied according to the situation or nature of the body it hits against, as that is capable of reflecting it, or refracting it, or both, and that after several manners: the body may also have an undulating motion, and that with smaller or greater waves; or may have a rotation about its own middle parts; or may have both a progressive motion and a rotation, and the one either equal to the other, or swifter than it, in almost infinite proportions. As to the determination of motion, the body may move directly upwards or downwards, decliningly or horizontally, east, west, north, or south, &c. according to the situation of the impellent body. And besides these and other modifications of the motion of a simple corpuscle or body, whose phænomena or effects will be also diversified, as I partly noted already, by its bulk and by its figure: besides all these, I say, there will happen a new and great variety of phænomena, when divers corpuscles, though primogenial, and much more if they be compounded, move at once, and so the motion is considered in several bodies; for there will arise new diversifications from the greater or lesser number of the moving corpuscles; from their following one another close, or more at distance; from the order wherein they follow each other; from the uniformity of their motion, or the confusedness of it; from the equality or inequality of their bulk, and the similitude or dissimilitude of their figures; from the narrowness or wideness, &c. of the channel or passage in which they move, and the thickness, thinness, pores, and the conditions of the medium through which they move; from the equal or unequal celerity of their motion, and force of their impulse: and the effects of all these are variable by the differing situation and structure of the sensories, or other bodies on which these corpuscles beat.

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CHAP. IV.

THE third and laſt difficulty that now remains to be conſidered, may be thus propoſed: that whereas, according to the corpuſcularian hypotheſis, not only one or two qualities, but all of them proceed from the bigneſs and ſhape, and contexture of the minute parts of matter, it is conſonant to their principles, that if two bodies agree in one quality, and ſo in the ſtructure on which that quality depends, they ought to agree in other qualities alſo; ſince thoſe do likewiſe depend upon the ſtructure wherein they do agree; and conſequently it will be ſcarce poſſible to conceive that two ſuch bodies ſhould be endowed with ſo many differing qualities, as experience ſhews they may.

To illuſtrate this objection by an example, it is pretended that the whiteneſs of froth proceeds from the multitude and hemiſpherical figure of the bubbles it is made up of. And if this or any other mechanical fabrick or contexture be the cauſe of whiteneſs, how comes it to paſs that ſome white bodies are inodorous and inſipid, as the calx of hartſhorn; others both ſtrongly ſcented and ſtrongly taſted, as the volatile ſalt of hartſhorn or of blood; ſome diſſoluble in water, as ſalt of tartar; others indiſſoluble in that liquor, as calcined hartſhorn, &c. ſome fixed in the fire, as the bodies laſt named; others fugitive, as powdered ſal armoniack; ſome incombuſtible, as ſalt of tartar; others very inflammable, as camphire. To which examples a greater variety of white bodies might be added, if it were neceſſary.

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1. And firſt I ſhall conſider, that in the pores of viſible and ſtable bodies there may be often lodged inviſible and heterogeneous corpuſcles, to which a particular quality that belongs to the body as ſuch, is to be referred. Thus we ſee in a perfumed glove, that in the pores of the leather odoriferous particles are harboured, which are of quite another nature than the leather itſelf, and wholly adventitious to it, and yet endue it with the fragrancy, for which it is prized. A like example is afforded us in raſberry wine made with claret; for the pleaſing ſmell is imparted to the wine, by the corpuſcles of the berries diſperſed *per minima* through the whole body of it.

2. THE ſecond thing that I conſider is this, that oftentimes corpuſcles of very differing natures, if they be but fitted to convene, or to be put together after certain manners, which yet require no radical change to be made in their eſſential ſtructures, but only a certain

certain juxta-position or peculiar kind of composition; such bodies, I say, may notwithstanding their essential differences exhibit the same quality; for invisible changes made in the minute, and perhaps undiscernible parts of a stable body may suffice to produce such alterations in its texture, as may give it new qualities, and consequently differing from those of other bodies of the same kind or denomination; and therefore though there remains as much of the former structure, as is necessary to make it retain its denomination, yet it may admit of alterations sufficient to produce new qualities. Thus when a bar of iron has been violently hammered, though it continues iron still, and is not visibly altered in its texture; yet the insensible parts may have been put into so vehement an agitation, as may make the bar too hot to be held in one's hand. And so if you hammer a long and thin piece of silver, though the change of texture will not be visible, it will acquire a springiness that it had not before; and if you leave this hammered piece of silver a little while upon the glowing coals, and after let it cool, though your eye will perchance as little perceive that the fire has altered its texture, as it did before, that the hammer had; yet you will find the elasticity destroyed.

If on the surface of a body there arise or be protuberant a multitude of sharp and stiff parts, placed thick or close together, let the body be iron, silver, or wood, or of what matter you please, these extant and rigid parts will suffice to make all these bodies to exhibit the same quality of asperity or roughness.

And if all the extant parts of a (physical) superficies be so depressed to a level with the rest, that there is a coœquation, if I may so speak, made of all the superficial parts of a body; this is sufficient to deprive it of former roughness, and give it that contrary quality we call smoothness; and if this smoothness be considerable exquisite, and happen to the surface of an opacous body of a close and solid contexture, and fit to reflect the incident rays of light and other bodies unperturbed, this is enough to make it specular, whether the body be steel, or silver, or brass, or marble, or flint, or quicksilver, &c.

And so, as I noted in the last chapter on another occasion, if a body be so framed and stretched, as being duly moved by another body to put the air into an undulating motion, brisk enough to be heard by us, we call that sonorous, whether it be a metalline bell, or gut strings, or wires, &c. Nay if waving motions whereinto the air is put by such differing bodies be alike, these bodies will not only in general give a sound, but will yield that particular degree of sound that men call the same note.

For here it is to be considered, that besides that peculiar and essential modification which constitutes a body, and distinguishes it from all others that are not of the same species, there may be certain other attributes that we call extra-essential; which may be common to that body with many others, and upon which may depend those more external affections of the matter, which may suffice to give it this or that relation to other bodies, divers of which relations we stile qualities.

Of this I shall give you an evident example in the production of heat: for provided there be a sufficient and confused agitation made in the insensible parts of a body, whether it be iron or brass, or silver, or wood, or stone, that vehement agitation, without destroying the nature of the body that admits it, will fit it for such an operation upon our sense of feeling, and upon bodies easy to be melted (as butter, wax, &c.) as we call heat.

And so in the instance named in the objection about whiteness; it is accidental to that quality, that the corpuscles it proceeds from should be little hemispheres; for though it happen to be so in water agitated into froth, yet in water frozen to ice, and beaten very small, the corpuscles may be of all manner of shapes; and yet the powder be be white. And it being sufficient to the producing of whiteness that the incident light be reflected copiously every way, and untroubled by the reflecting body, it matters not whether that body be water, or white wine, or some other clear liquor turned into froth, or ice, or glass, or crystal, or clarified resin, &c. beaten into powder; since without dissolving the essential texture of these formerly diaphanous bodies, it suffices that there be a comminution into grains numerous and small enough by the multitude of their surfaces, and those of the air, or other fluid that gets between them, to hinder the passage of the beams of light, and reflect them every way, as well copiously, as unperturbed.

PERHAPS it may not be impertinent to add to this, that there may be other catholick affections of corpuscles, besides the shape or structure of them, by virtue whereof aggregates even of such as are (as to sense) homogeneous, may exhibit differing qualities; as for instance, they may have some when they are in a brisk motion, and others, when they are but in a languid one, or at rest; as salt-petre, when its parts are sufficiently agitated by the fire in a crucible, is not only fluid but transparent almost like water; whereas when it cools again, it becomes a hard and white body; and butter, that is opacous in its most usual state, may be diaphanous when it is melted. So I shall hereafter have occasion to shew you that a great quantity of beaten alabaster, which usually retains the form of a moveless heap of white powder, by being after a due manner exposed to heat, obtains, and that without being brought to fusion, many of the principal qualities of a fluid body. And if with good spirit of nitre, or aqua fortis, you fill a glass half full, it will (unless it be extraordinarily deflegmed) exhibit no redness nor approaching colour in the vessel; but if you warm it a little, or cast into it a bit of iron or of silver, that it may put the liquor into a commotion, then the nitrous spirits devesting the form of a liquor, and ascending in that of fumes, will make all the upper part of the glass look of a deep yellow, or a red.

3. THE third thing I would recommend to your consideration is, to reflect on what I proposed in the last foregoing section, where I told you that in reference to the production of qualities, a body is not to be considered barely in itself, but as it is placed in, and is a portion of the universe. But of this subject I have said so much in the newly mentioned discourse, and in that which you are there referred to, that I shall now only put you in mind that divers of the particulars to be met with in those discourses are applicable to our present purpose.

4. To all this let me add in the last place, that, as to that part of the grand objection that we are clearing, which urges the difficulty of explicating upon the corpuscular principles, how, for example, the same body whose structure makes it shaped so as to be fit to exhibit whiteness, should likewise have divers other qualities that seemed to have no affinity with whiteness: this scruple, I say, we may, by what we have already discoursed, be assisted to remove; especially if we subjoin another consideration to it; for if corpuscles, without losing that texture which is essential to them, may (as we have shewed they may) have their shape, or their surfaces, or their situation changed, and may also admit of alterations (especially as these corpuscles make up an aggregate or congeries) as to motion or rest; as to these or those degrees, or other circumstances of motion; as to laxity and density of parts, and divers other affections; why should we not think it possible that a single, though not indivisible, corpuscle, and much more an aggregate of corpuscles, may by some of these, or the like changes, which, as I was saying, destroy not the essential texture, be fitted to produce divers other qualities, besides these that necessarily flow from it? Especially considering (which is that I have now to add) that the qualities commonly called sensible, and many others too, being ac-

cording to our opinion but relative attributes, one of these now mentioned alterations, though but mechanical, may endow the body it happens to with new relations both to the organs of sense, and also to some other bodies, and consequently may endow it with additional qualities.

If from good Venice or other turpentine you gently evaporate or abstract about a third part of its whole weight, you may obtain a fine transparent and almost reddish colophony; if you beat this very small, it will lose its colour and transparency, and will afford you an opacous and very white powder: if you expose this to a moderate heat, it will quickly and without violence both regain its colour and transparency, and acquire fluidity; and if, whilst it is thus melted, you put the end of a quill or reed a little beneath the surface, and blow skilfully into it, you may obtain bubbles adorned with very various and vivid colours. If when it has lost its fluidity, but whilst it is yet pretty warm, you take it into your hands, you will find that it has in that state a viscosity, by virtue of which you may draw it out into threads, as you may paste; but as soon as it grows quite cold, it becomes exceeding brittle; and if whilst it is yet warm, you give it the shape of a triangular prism, and make it of a convenient bulk, it will exhibit variety of colours almost like a triangular glass. Whilst this colophony is cold, and its parts are not put into a due motion, straws and other light bodies may be held unmoved close to it; but if by rubbing it a little you put the parts into a convenient agitation, though perhaps without sensibly warming the colophony, it displays an electrical quality, and readily draws to it the hairs, straws, &c. that it would not move before. All or most of these things you may also perform, if I mistake not, with clarified resin, though I am not sure it will do so well.

To this I shall add one instance more, which may let you see how the same body, which the chymists themselves will tell you is simple and homogeneous, may, by virtue of its shape, and other mechanical affections (for it is a factitious body, and that is made by the destruction of a natural one) have such differing respects to different sensories, and to the pores, &c. of divers other bodies, as to display several very differing qualities. The example I speak of, is afforded me by the distillation of putrefied urine; for though such urine have already lost its first texture before it come to be distilled, yet when it has undergone two or three distillations to deflegm it, the spirits of it swimming in a phlegmatick vehicle have a pungent saltness upon the tongue, and a very strong, and to most persons an offensive smell in the nostrils; and when they are freed from the water, they are wont to appear white to the eye; and to very tender parts, as to those that are excoriated, or to the conjunctiva, they feel exceeding sharp, and seem to burn almost like a caustick, not to say like fire; insomuch that I have seen them presently make blisters upon the tongue itself, that was not raw or sore before they touched it; the same saline particles invisibly flying up to the eyes prick them, and make them water; and invading the nose often cause that great commotion in the head and other parts of the body, that we call sneezing. The same corpuscles, if they are much smelt to by a woman in hysterical fits, do very often suddenly relieve her, and so may be reckoned among the specifick remedies of that odd and manifold disease, which is not the only one in which they are considerable medicines, as we have elsewhere declared. The same corpuscles taken into human bodies have the qualities, that in other medicines we call diaphoretick and diuretick; the same particles being put upon filings of brass produce a fine blue, whereas upon the blue or purple juices of many plants they presently produce a green; being put to work upon copper, whether crude or calcined, they do readily dissolve it, as corrosive menstruums are wont to do other metals; and yet the same corpuscles being blended in due a proportion with the acid salts of such menstruums,

ſtruums, have the virtue to deſtroy their corroſiveneſs; and if they be put into ſolutions made with ſuch menſtruums, they have a power, excepting in very few caſes, to precipitate the bodies therein diſſolved. I might here add, *Pyrophilus*, how the ſame particles applied to ſeveral other bodies, to which they have differing relations, have ſuch diſtinct operations on them, as may intitle theſe ſaline ſpirits to other qualities. But to enumerate them in this place were tedious, eſpecially having already named ſo many qualities reſiding in this ſpirituous ſalt; which I therefore the rather pitched upon, becauſe being a factitious body, and made out of a putrefied one, and ſo ſimple as to be a chemical ſalt (which, you know, ſpagyriſts make one of the three principles of compounded bodies) I ſuppoſe you will make the leſs ſcruple to admit, that it works by virtue of its mechanical affections; of which, to perſuade you the more, I ſhall add, that if you compound this urinous ſalt with the ſaline particles of common ſalt (which is alſo a factitious thing, and confeſſed by chemiſts to be a ſimple principle of the concrete that yields it) theſe two being mingled in a due proportion, and ſuffered leiſurely to combine, will aſſociate themſelves into corpuſcles, wherein the urinous ſalt loſes moſt of the qualities I have been aſcribing to it, and with the acid ſpirit compoſes, as I have often tried, a body little differing from ſal armoniac; which great change can be aſcribed to nothing ſo probably, as to that of the ſhape and motion (not here to add the ſize) of the urinous ſalt, which changes the one, and loſes a great part of the other by combining with the acid ſpirits; and to confirm that both theſe do happen, I have ſeveral times ſlowly exhaled the ſuperfluous, but not near the whole liquor from a mixture made in a due proportion of the ſpirit of urine and that of ſalt, and found, that anſwerable to my conjecture, there remained in the bottom a ſalt, not only far more ſluggiſh than the fugitive one of urine, but whoſe viſible ſhape was quite differing from that of the volatile cryſtals of urine, this compounded ſalt being generally figured, either like combs or like feathers.

If after all this we do either add or inculcate, that the extra-eſſential changes that may be made in the ſhape, contexture, and motion, &c. of bodies, that agree in their eſſential modifications, may not only qualify them to work themſelves immediately after a differing manner upon differing ſenſories, and upon other bodies alſo, whoſe pores, &c. are differently conſtituted, but may diſpoſe them to receive other impreſſions than before, or receive wonted ones after another manner from the more catholick agents of nature; if, I ſay, we recommend this alſo to your conſideration, what has been delivered in the whole diſcourſe will, I hope, let you ſee that the ſcruple propoſed at the beginning of it is not ſo perplexing a one to our philoſophy, as perhaps you then imagined it.

The three difficulties conſidered partly in this, and partly in the two foregoing ſections, I was the more inclined to take notice of in this place (for in divers other paſſages of my writings you will meet with things that are applicable to the paſt diſcourſe, and ſhould be referred thither) partly becauſe the ſcruples themſelves are of great moment, and for ought I know have been diſcuſſed by others; and partly becauſe theſe difficulties relating in ſome ſort to the corpuſcularian hypotheſis in the general, the clearing of them may both ſerve to confirm ſeveral of theſe things that have above been written about the origin of forms and qualities (to which it might therefore have been joined) and will be conducive to a clear underſtanding, and explicating divers of the particulars that I am about to deliver, and perhaps ſeveral other phænomena of nature.

OF THE

SYSTEMATICAL or COSMICAL

QUALITIES of THINGS.

CHAP. I.

I Expect, *Pyrophilus*, that being ſomewhat ſurprized at the title of this diſcourſe, you will preſently aſk what I underſtand by Coſmical or Syſtematical Qualities; that name being new enough to require that I ſhould tell you both what is meant by it, and why I make choice of it.

To anſwer ſo reaſonable a queſtion, I ſhall inform you that I conſider that the qualities of particular bodies (for I ſpeak not here of magnitude, ſhape, and motion, which are the primitive modes and catholick affections of matter itſelf) do for the moſt part conſiſt in relations, upon whoſe account one body is fitted to act upon others or diſpoſed to be acted on by them and receive impreſſions from them; as quickſilver has a quality or power (for I here take qualities in the larger ſenſe) to diſſolve gold and ſilver, and a capacity or diſpoſition to be diſſolved by aqua fortis, and (though leſs readily) by aqua regis: and this being premiſed, I obſerve farther, that, though in eſtimating the qualities of natural bodies we are wont to conſider but the power any particular one has of acting upon, or the capacity it has of ſuffering from ſuch and ſuch particular bodies, wherewith it is taken notice of to have manifeſt commerce in point of making or of receiving impreſſions, yet there may be ſome attributes which may belong to a particular body, and divers alterations to which it may be liable, not barely upon the ſcore of theſe qualities that are preſumed to be evidently inherent in it, nor of the reſpects it has to thoſe other particular bodies to which it ſeems to be manifeſtly related, but upon the account of a ſyſtem ſo conſtituted as our world is, whoſe fabrick is ſuch that there may be divers unheeded agents, which, by unperceived means, may have great operations upon the body we conſider, and work ſuch changes in it, and enable it to work ſuch changes on other bodies, as are rather to be aſcribed to ſome unheeded agents, than to thoſe other bodies with which the body propoſed is taken notice of to have to do: ſo that although if divers bodies that I could name were placed together *in vacuo*, or removed together into ſome of thoſe imaginary ſpaces which divers of the ſchoolmen fancy to be beyond the bounds of our univerſe, they would retain many of the qualities they are now endowed with, yet they would not have them all; but by being reſtored to their former places in this world, would regain a new ſet of faculties (or powers) and diſpoſitions, which, becauſe they depend upon ſome unheeded relations and impreſſions which theſe bodies owe to the determinate fabrick of the grand ſyſtem or world they are parts of, I have, until I can find a more proper appellation, thought fit to name their coſmical or their ſyſtematical qualities.

I have in the Origin of Forms touched upon this ſubject already, but otherwiſe than I am now about to do: for whereas that which I do principally (and yet but tranſiently) take notice of is, that one body being ſurrounded with other bodies, is manifeſtly wrought on by many of thoſe among whom it is placed; that which I chiefly in this diſcourſe conſider, is the impreſſions that a body may receive, or the power it may acquire from thoſe vulgarly unknown or at leaſt unheeded agents by which it is thus affected, not only upon the account of its own peculiar texture or diſpoſition, but by virtue of the general fabrick of the world.

C H A P. II.

Now though there be ſeveral of the grand mundane bodies, and divers laws and cuſtoms of nature, which may contribute (more or leſs) to the phænomena of the qualities we are treating of; yet, becauſe a diſtinct and particular inquiry into each of them would challenge a much longer diſcourſe than this ſhort eſſay is to be, and a much abler pen than his that writes it, I did not only think it fit to reſerve what occurs to me about the laws and cuſtoms of nature, as they concern this ſubject, to another diſcourſe, or an appendix to this; but to declare to you alſo, that whereas the three main bodies whoſe more unobſerved operations and changes have the moſt conſiderable influence on the qualities we are to treat of, are, the ſubterraneal parts of the globe we inhabit; the ſtars, whether fixed or wandering, with the æther that is about them; and the atmoſphere or air we live in; I foreſee that it will be requiſite for me to aſſign the experiments and obſervations I have collected about theſe three ſubjects to other tracts: ſo that in this eſſay my chief work will be to take notice to you of ſome conſiderations that may be introductory in a more general way to the clearer knowledge of the ſubject to be diſcourſed of, to which I may, as time and my occaſions may permit, ſubjoin ſome particulars, which, though perhaps they do not all of them ſo directly or properly belong to the ſolemnly propoſed heads of this diſcourſe, yet are not impertinent to the deſign of it, and on that ſcore may be allowed their places in it.

And leſt you ſhould think, that under the name of coſmical qualities I ſhould introduce chimæras into natural philoſophy, I muſt betimes advertiſe you that you will meet with divers particles in the following diſcourſe, fit to ſhew that theſe qualities are not merely fictitious qualities, but ſuch whoſe exiſtence I can manifeſt, not only by conſiderations not abſurd, but alſo by real experiments and phyſical phænomena: and to prevent miſtakes, I ſhall add, that under the name of catholick and unminded cauſes or agents, I comprehend not only divers inviſible portions of matter, but alſo the eſtabliſhed laws of the univerſe, or that which is commonly called the ordinary courſe of nature: and when I ſpeak of unobſerved agents or cauſes, I do not always mean that they are not known or taken notice of to be in *rerum natura*, but that they are not vulgarly conſidered or looked upon as the cauſes of ſome particular phænomena wherein I aſcribe to them an intereſt or efficiency.

But before I proceed any farther, it will not be amiſs to intimate in this place, that the things on which I founded the purpoſed notion of the coſmical attributes of bodies, were principally theſe three;

1. That there are many bodies that in divers caſes act not unleſs they be acted on; and ſome of them act either ſolely or chiefly, as they are acted on by the catholick and unheeded agents we have been ſpeaking of.

2. THAT there are certain subtle bodies in the world that are ready to insinuate themselves into the pores of any body disposed to admit their action, or by some other way affect it, especially if they have the concurrence of other unobserved causes and the established laws of the universe.

3. THAT a body by a mechanical change of texture may acquire or lose a fitness to be wrought upon by such unheeded agents, and also to diversify their operations on it upon the score of its varying texture.

THESE three propositions I shall endeavour to confirm distinctly by the ensuing experiments and phænomena; but because divers of these proofs may each of them serve to confirm more than one of these propositions, and because the making out of the two last, which are the most important (and the least probable) is the main design of this discourse, I shall say the less to the first, leaving it for the most part to you to refer to either of the three propositions what you shall meet with belonging to it in what is said upon either of the other two.

C H A P. III.

To begin then with the first proposition, namely, "That there are many bodies that "in divers cases act not unless they be acted on; and some of them act either solely "or chiefly, as they are acted on by the catholick and unheeded agents we have been "speaking of:" the former part of it will, I presume, be easily granted, it being evident by such gross instances as these, that a wedge will not cleave a block unless it be impelled against it by a hammer (or some equivalent instrument) nor a knife attract a needle, unless it be excited by a magnet: but as to the second, it will not in likelihood be so readily assented to, and therefore having *in transitu* illustrated it by observing to you that concave looking-glasses and convex burning-glasses kindle not other bodies unless they be enabled to do so by the reflected or trajected beams of the sun, I shall proceed to prove it by a couple of instances.

THE one is an iron bar that hath long stood in a window or some other fit place in a perpendicular posture; for though this bar was not when it was first erected, endowed with a magnetism any thing superior to that of other iron bars of the like shape and bigness, yet after it hath very long stood in that position, it will by the operations of invisible agents acquire a farther degree of magnetism than belonged to it as a bar of iron, and is enabled to produce some magnetical phænomena (elsewhere mentioned) that it could not before.

THE second instance is afforded us by what happens to a very flat and exquisitely polished piece of marble; for though of itself it hath no power to help to lift up any other dry body that it is laid upon, yet if it come to be skilfully laid upon another piece of marble as flat and smooth as it, and of a bulk not too unweildy, this upper stone, by virtue of the fabrick of the world, which gives the ambient air fluidity and weight, is enabled, without any other cement or fastening instrument than immediate contact, to raise with itself (in case a man lift it up) the lower marble, though perhaps an hundred times heavier than itself *: [whereas, if this laying one of these stones upon the other had been done *in vacuo*, I doubt not but no such power had thereby accrued to the uppermost of them].

* See this experimentally proved in the Continuation of the Author's New Experiments touching the air, Experiment the fiftieth.

CHAP. IV.

Proceed we now to our ſecond propoſition, which ſpeaks to this purpoſe; "That "there are certain ſubtle bodies in the world, that are ready either to inſinuate themſelves "into the pores of any body diſpoſed to admit their actions, or by ſome other way to "affect it, eſpecially if they have the concurrence of other unobſerved cauſes, and the "eſtabliſhed laws of the univerſe." I need not take notice on this occaſion that divers of the ancient philoſophers thought that there was a ſubtler body than the common air, and called æther; and that the Carteſians tell us that there is ſuch a ſubſtance diffuſed throughout the univerſe which they call, according to the differing ſizes of its parts, ſometimes *primum elementum* and ſometimes *materia cœleſtis*, to which they attribute the uſe of pervading all other bodies and adequately filling thoſe pores of theirs that are correſpondent in bigneſs and figure to the differing portions of this inſinuating matter: that there may be ſuch a ſubſtance in the univerſe, the aſſerters of it will probably bring for proofs ſeveral of the phænomena I am about to relate: but whether there be or be not in the world any matter that exactly anſwers to the deſcriptions they make of their firſt and ſecond elements, I ſhall not here diſcuſs, though divers experiments ſeem to argue that there is in the world an æthereal ſubſtance very ſubtle and not a little diffuſed. But though theſe things ſeem, as I was ſaying, probable enough; yet the inviſible agents I ſhall here chiefly, though not only, take notice of, will be the air (as it hath a weight and ſpring) and the magnetical effluvia of the terreſtrial globe.

If you take a bar of iron, or rather of ſteel, and another like it of ſilver, and having heated each of them red hot and put them to cool directly north and ſouth; though they be both acted upon by the ſame agent, the fire, and the ſteel, as to ſenſe, ſeems ſuch as it was before, yet the texture of theſe two metals being different, the ſilver acquires no new quality by what hath been done to it, whereas the ignition of the ſteel having opened its pores and made its parts more pliable (as may be argued from the ſwelling of iron heated red hot and its ſoftneſs under the hammer) it is eaſily, whilſt in this ſtate it lies north and ſouth, pervaded by the magnetical effluvia of the earth which glide perpetually through the air from one pole to another, and by the paſſage of theſe ſteams it becomes endowed with a magnetical property, which ſome call polarity, whereby, being freely ſuſpended and exactly poiſed, it will, as it were, ſpontaneouſly direct itſelf towards the north and ſouth, and exerciſe ſome operations peculiar to magnetical bodies: and that it may ſeem the leſs ſtrange that I ſhould aſcribe to ſo groſs and dull a body as the earth the power of inviſibly communicating to iron a magnetical virtue, which is thought to be of ſo ſpiritual a nature, I ſhall put you in mind of an experiment that I acquainted you with divers years ago, about the earth's power to impart, in ſome caſes, without the help of a loadſtone, a directive faculty to the loadſtone itſelf: for, having by ignition deprived an oblong magnet of its former attractive power by taking it red hot out of the fire, and ſuffering it to cool north and ſouth, I could at pleaſure, by placing either end northward or ſouthward, whilſt the ſtone was refrigerating, make what end I had a mind to, point to the north pole; and when it had done ſo, I could, by a new ignition and refrigerating of it in a contrary poſition, make the ſame end of the ſtone become its ſouthern pole.

If you take a capacious glaſs phial with a ſlender neck, ending in a ſharp angle, with only a pin-hole left open at the apex (inſtead of which veſſel, *Hero*'s egg, as ſome call it, though far ſmaller, and without ſuch a neck, may ſerve turn) and by ſuction or otherwiſe free it from as much of the included air as you can; and if then having ſtop-

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ped this hole with your finger you immerſe it ſomewhat deep under water, and, laſtly, withdraw your finger; the water will, contrary to its own nature (as is vulgarly conceived) ſpring up with violence and to a good height into the cavity of the phial; which motion of a heavy liquor upwards cannot be aſcribed to the motion of the finger; for that did but unſtop the orifice and not impel up the water, nor need be attributed to nature's abhorrence of a vacuum, which (whether there be ſuch a thing or not) it is altogether unneceſſary to have recourſe to in this caſe; the preſſure of the ambient air, proceeding from its weight upon the ſurface of the water, being ſufficient to force up that liquor into the phial, in which the remaining air, by being rarefied upon the ſcore of the abſence of that which was taken out, hath its ſpring too much weakened to be able to reſiſt the preſſure of the outward air, as it formerly could do, whereas if this experiment were tried *in vacuo*, the water would not be raiſed, there being no outward agent to impel it up.

C H A P. V.

I HAD ſometimes the curioſity to conſider beans and peaſe pulled up out of the ground by the ſtalks in order to an inquiry into their germination; and after having taken notice of their tumidneſs upon their having imbibed the moiſture of the ſoil and of their way through the ambient earth, not only upwards with their ſtems, but downwards with their tender roots; I thought fit to try with what ſtrength and force the cauſes of their intumeſcence endeavoured to dilate them, whereupon I filled with a quantity of ſuch dry beans as are in *England* wont to be given to horſes ſeveral phials and bottles, ſome of glaſs and ſome of earth, whereof two or three were of a conſiderable ſtrength; which done, the intervals between the beans were filled with water, and the veſſels were exactly ſtopped with corks ſtrongly tied down with ſtrings that nothing might get out; for I ſuppoſed that the water, ſoaking into the pores of the beans, would alter the figure of the pores and produce in them an endeavour to ſwell, which being checked by the ſides and ſtopples of the veſſels would diſcover, whether that endeavour were ſo forcible as I ſuſpected; the ſucceſs was, that moſt of theſe veſſels (for in one or two of them we found the ſtrings broke that withſtood the raiſing of the ſtopples) whether of glaſs or earth, were burſt in ſunder.

BUT being deſirous to make a nearer eſtimate how great this expanſive force of the ſwelling beans was, we put a convenient quantity of them into an hollow but ſtrong cylinder of braſs, which I had cauſed to be purpoſely made for ſuch kind of trials, whoſe cylindrical cavity was juſt ſix inches in length and two in diameter; then having put in water enough to reach the top of the beans, we put into the upper part of the cylinder, which was purpoſely left unfilled, a wooden plug, made fit for the orifice by being turned into a cylindrical form and a little narrower than the orifice, that it might move freely up and down, though the water ſhould make it ſomewhat ſwell; upon the top of this plug, on which leaned a broad and thick piece of wood ſhaped like a round trencher and made of the ſame piece with the plug, was placed a common half hundred weight of lead, which yet could not depreſs the plug too low, being hindered by the breadth of the trencher, made as well to prevent the too great depreſſion of the plug as to afford a convenient baſis to the weight. Laſtly, having kept the cylinder in a quiet place for a fit ſpace of time (which is in ſuch trials ſometimes two or three days, ſometimes more or leſs according to the temperature of the air and quantity of the included matter) we obſerved, as I expected, that the ſwelling beans had very manifeſtly heaved

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up the plug and the incumbent weight beyond the former ſtation: and I ſuſpected that if we had had ſmall weights (of a pound or two a piece) conveniently ſhaped, a heavier weight might have been raiſed by the ſame force.

It is not neceſſary in this place that I mention ſeveral particulars relating to the experiment, as how it ſucceeds in corn, ground and unground, how in dried fruits, as raiſins and currants, how in dried peaſe (which we found to dilate themſelves very ſtrongly) and what liquors will or will not cauſe an intumeſcence; nor ſhall I here ſpeak of divers circumſtances that may be taken notice of in ſuch trials; only I muſt not omit this particular, that I had a mind to make ſome trial whether the force of ſwelling beans to preſs or thruſt up the incumbent weight, would not in cylinders of different ſizes be increaſed in ſomewhat near a duplicate proportion to that of the diameters or the areas of the orifices of the differing cylinders (becauſe it is according to the greatneſs of thoſe areas that the force can be applied upwards) but having not weights enough ſo ſhaped as I needed, I could not make ſuch an experiment as I deſired; but thus much, however, I diſcovered in order to my purpoſe, that the preſſure upwards of the drenched beans was very much greater in wider cylindrical veſſels than in narrower ones; for having put a convenient quantity of dried beans into a metalline cylinder that wanted a pretty deal of being ſo deep as ſix inches, and was not quite four inches broad; when the included beans began to ſwell they manifeſtly lifted up ſuch a plug as was lately deſcribed (but broader) with weights upon it, amounting to an hundred pounds or better.

Whether this may paſs for a new phyſical *vis movens*, I freely leave to you to determine; as alſo to conſider, whether by mechanical contrivances ſo great a force as may be this way produced, and which ſlowly and ſilently proceeds till it hath attained its utmoſt energy, and may be conveyed into bodies without working any effect before the due time, may not in ſome caſes be made applicable to uſeful purpoſes.

I shall not now examine whether or how far the foregoing experiment may confirm the Carteſian hypotheſis about their *materia ſubtilis*; nor whether upon the notions which our experiments may ſuggeſt, we may be enabled to explicate the force wherewith fermenting liquors do often break the veſſels wherein they are too exactly ſhut up; about which phænomena, and of ſome others of kin to it, I elſewhere propoſe ſome conjectures.

I think it fitter in this place to take notice to you of ſomething that more directly belongs to our preſent ſubject; namely, that the air, within which name I here comprize the æther that may be harboured in its pores, may in ſome caſes by its conſtant preſence, and in others by its being always at hand, and its readineſs to inſinuate itſelf wherever it can get admittance, concur to the production of divers phænomena wherein its co-operation has not been ſuſpected even by philoſophers; for, not to mention what I have by experiments purpoſely deviſed, that the air's being preſent to preſs upon the ſuperficies of liquors is ſo requiſite in ſuction, that they will not thereby be made to aſcend without it; and beſides that to the putrefying of ſome bodies within the time (or even within ten times the time) that nature is wont to putrefy them in, they will not be brought to putrefaction if the air be all the while carefully ſecluded; beſides theſe things, I ſay, I found that the light, which appears in ſome rotten woods, and in ſome putrefied fiſhes, did ſo much depend upon the preſence of the air, that if that were quite withdrawn from them the light would diſappear, and when they were reſtored to the contact of the air, they would ſhine forth again as formerly. [But of this elſewhere.]

CHAP.

CHAP. VI.

I KNOW not whether it will be fit to add, that beſides what the air (with the ſubtler matter that may be mingled with it) may do as a ſubſtance, it may perform divers things upon other accounts, as its finer parts may be, though inſenſibly, moved in phyſical ſtraight lines; or as it is the ſubject of ſwarms of corpuſcles put into peculiar though inviſible motions. For inſtance, if I take a ſheet of paper and rub it over with oil, or even a fit kind of greaſe; that which the liquor apparently does is only to pierce or ſoak into the pores of the paper which before did by their crookedneſs, or upon ſome other mechanical account render the paper opacous: but this inſinuation of the unctuous body into the pores having altered them as to figure, or to ſize, or to both, and having by that alteration given the paper a texture diſpoſed to allow due paſſage to the corpuſcles of light, or to tranſmit their peculiar kind of impulſe (whence ſeveral naturaliſts derive light) the motions, as I was ſaying, or inviſible corpuſcles in the air, depending upon the conſtitution of the world do preſently act upon the paper, and produce beyond it both a ſenſation of light, and the repreſentations of a multitude of objects whence the light reflects, and which could not be ſeen through it before.

I NEED not perhaps tell you, that if a pretty large box be ſo contrived that there may be towards the one end of it a fine ſheet of paper ſtretched like the leather of a drum-head at a convenient diſtance from the remoter end, where there is to be left an hole covered with a lenticular glaſs fitted for the purpoſe, you may at a little hole left at the upper part of the box ſee upon the paper ſuch a lively repreſentation, not only of the motions but ſhapes and colours of outward objects, as did not a little delight me when I firſt cauſed this portable darkened room, if I may ſo call it, to be made: which inſtrument I ſhall not here more particularly deſcribe, partly becauſe I ſhewed it to you ſeveral years ago, ſince when divers ingenious men have tried to imitate mine (which you know was to be drawn out or ſhortened like a teleſcope, as occaſion required) or improve the practice; and partly becauſe that which I pretended in mentioning of it here, is to ſhew, that ſince that almoſt upon every turning of the inſtrument this way or that way, whether it be in the town or open fields, one may diſcover new objects and ſometimes new landſcapes upon the paper, there muſt be all day long in all parts of the air where this phænomenon can be exhibited, either certain effluvia emitted every way from the objects or certain motions of inſenſible corpuſcles, which rebounding firſt from the external object and then from the paper, produce in the eye the images of theſe objects, ſo that the air is every where full of viſible ſpecies which cannot be intelligibly explicated without the local motions of ſome minute corpuſcles, which, whilſt the air is enlightened, are always paſſing thorough it.

You may remember, *Pyrophilus*, that in the clauſe of the ſecond propoſition hitherto diſcourſed of, I take in the eſtabliſhed laws of the univerſe as a part of the preſent conſtitution of this our world; ſome of thoſe laws contributing much to the operation of thoſe unheeded cauſes we are treating of. Of theſe I may another time give you ſome inſtances, but for the preſent it may ſuffice to take notice of this one, that if you take a bar of iron, and holding it perpendicularly, apply the loweſt part of it to the northern point of a well-poiſed magnetical needle, the bar will preſently drive it away; but that magnetiſm by which the bar does it, as it is preſently acquired by the poſture which it had, ſo it is as ſuddenly changed if you invert that poſture, as appears by this, that though you hold the bar perpendicular, if it be held under the needle, ſo that the ſame part of the bar which before was placed directly over the north point of the needle, be held

held directly under the same point, the bar will not, as before, drive it away, but, as they commonly speak, attract it: but if this bar has been for a long time kept in an erected posture, as if it be taken from some old window; or if, having been heated and refrigerated, it have very long lain north and south, it will appear endowed with a stronger and more durable verticity, as we elsewhere more fully declare; which seems to proceed from this, that by lying north and south, it lay in the way, which, according to the established laws of nature, the magnetical effluvia of the earth must pass along in steams from pole to pole; whereby they have the opportunity by little and little to work upon the pores of the iron that lies in their way and fit them to give passage to the effluvia of magnetical bodies; in which fitness seems principally to consist the magnetism of iron; whereas, if this metal had all this while lain east and west, instead of north and south, it would have acquired little or no magnetical virtue. And the reason why an erected posture gives a rod or bar of iron a power to drive away the north point of the needle has been probably conceived to be this, that the lower end being nearer the earth does more plentifully participate of the magnetick steams which fly in a closer order there than further off, and by powerfully affecting that part of the iron turn it for a time into the iron's north pole, which, according to the laws magnetical, ought to drive away the north pole of the needle and attract the south; whereas if the bar being inverted, that end which was uppermost becoming the lower, must for the same reason have a contrary operation, unless by having long stood, its verticity be too well settled to be suddenly destroyed or altered by the effluvia of so languid a magnet as the earth: but whether or no this explication be the right one (for I would not contend for its being so) it appears by the requisiteness both of a determinate position of the iron and of its long continuance in that position, to make that metal acquire a durable verticity, that those unheeded magnetical steams which communicate such a magnetism to the iron, move and act according to laws established in nature; which is as much as my design in this discourse makes necessary to be made out.

CHAP. VII.

It remains now that we discourse of the last of our three grand propositions, namely, "That a body by a mechanical change of texture may acquire or lose a fitness to be "wrought upon by such unheeded agents, and also to diversify their operations on it "upon the score of its varying texture."

This proposition is of so much affinity with the foregoing, that there are divers cases wherein the same experiments and other arguments may serve for the confirmation of both.

But to illustrate a little what I mean by gross and sensible examples, it is a custom we often observe at sea, when we sail with too slack a wind, to take up water with certain instruments and throw it against the sails. At the first proposal this may seem a very improper way to promote the swiftness of the ship, since there is the weight of so much water added to that of the vessel itself; but yet I have seen the seamen make use of it as one of their best expedients when we were closely chaced by pirates, nor did I look upon it as irrational; for whereas, when the sails are dry a good part of the wind that blows upon them easily gets thorough those meshes or great pores that are left between the threads of which a sail consists; when it comes to be wetted the imbibed water makes the threads swell every way, and consequently very much streightens the pores or intervals that were formerly left between them; by which means the wind cannot permeate them as freely as formerly, but by finding a greater reſiſtance in the ſail comes to beat more forcibly upon it and conſequently drives it, and with it the ſhip, more ſtrongly on than elſe it would have done; not to mention the ſtiffneſs of the ſail acquired by the imbibed ſea-water, becauſe I would not ſtay to take notice of other particulars to which the ſucceſs of this practice may perhaps be in part aſcribed.

To add another inſtance to the ſame purpoſe with the former; ſuppoſe an high wind to blow againſt a chamber, wherein the windows and doors are all ſhut, the effect will be only to ſhake a little the room in general, but if one open the caſement, though he that does it do properly and immediately but diſplace ſome little piece of the iron or other thing that ſhuts the window, yet this being done in a place where there is a ſtrong current of air, which we call a wind, there will preſently follow a blowing up of curtains or hangings and blowing about of duſt, ſtraws, feathers, or other light bodies that are not firmly enough faſtened nor very ponderous, and yet are too heavy to be blown about.

But to proceed to inſtances that are not ſo groſs, I might take notice that though good common tartar does uſually of itſelf keep dry in the air, nay, and will not eaſily be diſſolved in cold water; yet if it be calcined, though but very moderately, the ſalt in the remaining coal, the texture being now altered, will readily enough in the moiſt air (as that of a cellar) run into that liquor that chemiſts have been pleaſed to call of tartar per deliquium: but in regard that to make the change the greater, part of the tartar muſt be driven away by the fire, I ſhall rather make uſe of an example eaſily drawn from an experiment I elſewhere mentioned to another purpoſe; for having taken a loadſtone, and according to the way there delivered, heated it and cooled it, though it had loſt ſo little by the fire that the eye took no notice of its being changed either as to ſhape or bulk, yet the operation of the fire by changing the inviſible texture did ſo diverſly alter the diſpoſition of it in reference to the magnetical effluvia of the earth, that I could preſently and at pleaſure change and re alter the poles of the ſtone, making the ſame end point ſometimes to the north and ſometimes to the ſouth. The like change of verticity I have, as I elſewhere declare, made mere iron capable of without the help of fire or any other magnet than the earth; and I have alſo found by trial that a certain heavy ſtone that is uſually thought to be not ſo much as of a metalline nature, may, by a ſlight and quick preparation that alters not the ſhape nor bigneſs, be enabled to attract and repel the poles of a magnetic needle.

C H A P. VIII.

To the inſtances already given in ſolid bodies it will not be amiſs to annex two or three in liquid bodies, becauſe it may be thought ſtrange by ſome that conſiderable changes of texture ſhould, without fire or any new ingredients, be produced in bodies, which, by reaſon of their fluidity, ſeem preſently to recover their texture if it be diſordered. If honey and water be each of them apart put into a convenient veſſel, they will both of them retain their nature; and though you mix them together in an undue proportion, ſo that by reaſon of overmuch honey the conſiſtence be too thick, or that by being diluted by too great a proportion of water, the ſolution of honey be too thin, they may continue honey and water; but if thoſe two liquors be duely proportioned (as if you put to one part of honey four or five of water) then their new texture ſo diſpoſes them to be acted on by the ſubtle permeating matter or whatever other common agent nature employs to produce fermentations, that the ingredients do no longer continue what they were, but

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begin to work like new muſt or beer-wort; and I have tried that ſo ſmall and ſhort a local motion (as carrying ſuch mixtures a while in a coach) has ſo excited the liquor as to make it violently force its way out of the veſſel or throw off the ſtopple, that I have wondered at it. And I remember that an eminent merchant of wines who ſpent divers years in the *Canaries*, being aſked by me about ſome things of this nature, aſſured me, that in thoſe fortunate iſlands (as the ancients ſtyle them) he had ſeveral times obſerved, that if a pipe of the beſt ſort of Canary were, when it was about a month old, rudely rolled, though but the length of an hall or moderate gallery, ſo tranſient and ſlight a diſcompoſure of the texture would quickly make ſo great a change in it, that oftentimes a good quantity of wine would be violently thrown out at the bung; or if the pipe were too cloſe ſtopped, that great veſſel itſelf would oftentimes have the bottom beaten out; by which means he had known ſeveral pipes of that rich liquor loſt.

WE have divers examples of the cracking of common glaſs when it is too ſoon, after it hath been removed from the fire, expoſed to the cold air, and the ſubtle bodies that are in it; which would not have cracked it if it had been cooled more ſlowly, ſo that its parts would have had leiſure to ſettle into a texture convenient for the paſſage of thoſe ſubtle bodies which in that caſe would harmleſly have permeated it. But I have ſometimes ſhewn the curious a more quick and manifeſt inſtance of the importance of the preſent texture of a body in reference to the catholick and inviſible cauſes that may work upon it: for having taken a plate of ſo ponderous and ſolid a body as copper, and heated it red hot, and then ſuffered it to cool a while upon ſome more moderately hot place in the fire, though it did not appear at all ignited, when I removed it to a plate or even to a ſheet of paper, yet upon its being expoſed to the atmoſphere, the ſuperficial part would not only crack as in over haſtily cooled glaſs, but would, and that preſently, fly off in flakes in good number, and not without noiſe; ſo that in a ſhort time I have had the neighbouring part of the paper, on which the braſs-plate reſted, almoſt quite covered with little ſcales, as it were, of that metal.

AND to give you, in favour of what I have been hitherto diſcourſing, an inſtance of a very ſubtle nature, I will not, though I juſtly might, take notice that in rotten fiſh and rotten wood the change of texture is oftentimes inviſible that will ſuffice to make the contact of the air, and the ſubtle corpuſcles whereto it gives harbour or paſſage, confer or loſe a power of ſhining; but I will rather chooſe to inſtance in the Bolonian ſtone, which by calcination acquires this admired property, that if it be but expoſed to the ſunbeams (to which I have found other ſtrong lights ſuccedaneous) it will not only in a few minutes acquire a luminouſneſs, but for ſome time after retain it in the dark.

COSMICAL SUSPICIONS

(SUBJOINED AS AN

APPENDIX

To the DISCOURSE of

The COSMICAL QUALITIES of THINGS).

IN the former eſſay, *Pyrophilus*, I propoſed to you ſome things about the ſubject there treated of that ſeemed to have in them ſuch a degree of probability as is wont to be thought ſufficient to phyſical diſcourſes, or at leaſt is uſually to be met with in them. But in regard the world, whether we take it in the larger ſenſe for the whole univerſe, or in the more narrow but not leſs common acceptation, for the globe we men inhabit, is a ſubject ſo vaſt, that not only all demonſtrable truths that may be diſcovered concerning it may be looked upon as important, but even conjectures and ſuſpicions themſelves that relate to it in general, if they be not very groundleſs or extravagant, may deſerve not to be altogether paſſed by in ſilence; I will adventure to entertain you a while with ſome thoughts of this nature, eſpecially becauſe they will give me opportunity to alledge in their favour ſome hiſtorical obſervations, which, whatever the doubts or conjectures be thought of, may appear to be more new than deſpicable.

IT may now, therefore, be not unſeaſonable to confeſs to you that I have had ſome faint ſuſpicion, that beſides thoſe more numerous and uniform ſorts of minute particles that are by ſome of the new philoſophers thought to compoſe the æther I lately diſcourſed of, there may poſſibly be ſome other kind of corpuſcles fitted to have conſiderable operations, when they find congruous bodies to be wrought on by them: but though it is poſſible, and perhaps probable that the effects we are conſidering may be plauſibly explicated by the æther, as it is really underſtood; yet I ſomewhat ſuſpect that thoſe effects may not be due ſolely to the cauſes they are aſcribed to, but that there may be, as I was beginning to ſay, peculiar ſorts of corpuſcles that have yet no diſtinct name, which may diſcover peculiar faculties and ways of working, when they meet with bodies of ſuch a texture as diſpoſes them to admit, or to concur with the efficacy of theſe unknown agents.

THIS ſuſpicion of mine will ſeem the leſs improbable if you conſider, that though in the æther of the ancients there was nothing taken notice of but a diffuſed and very ſubtle ſubſtance; yet we are at preſent content to allow that there is always in the air a ſwarm of ſteams moving in a determinate courſe betwixt the north pole and the ſouth; which ſubſtance we ſhould not probably have dreamed of, if our inquiſitive *Gilbert* had not hap-

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pily found out the magnetism of the terrestrial globe. And few, perhaps, would have imagined, that when an hunted and wounded deer has hastily passed over a little grass, he should leave upon it such determinate, though invisible effluviums, as should for many hours so impregnate the air, as to betray the individual flying and unseen deer, if there was no blood-hounds, upon whose peculiarly disposed organs of smelling these steams are fit to operate. And it is strange that there should be such effluvia for a long time (perhaps a year or two together) residing in the air, that though our senses discern them not, and though they have no operation upon other men, yet if they meet with persons of a peculiar temperament, who by that, and by their formerly having had the plague, have attained a peculiar disposition that fits them to be wrought on by pestilential steams, they may so operate upon them, that some of these persons may be able to discern those steams to be pestilential. To give some countenance to which paradox, I will here annex two or three testimonies, the first of which I find thus set down among my Adversaria. [Above three months before the late great plague began in *London*, in the year 1665, there came to Dr. *M.* a patient of his, to desire his advice for her husband; and the doctor having enquired what ailed him, she answered, that his chief distemper was a swelling in his groin, and upon that occasion added, that her husband assured her of his being confident, that the next summer the plague would be very rife in *London*; for which prediction he gave this reason, that in the last great plague he fell sick of that disease; and he then had a pestilential tumour.

So in two other plagues that since happened, though much inferior to that great one, each of them had a rising in his body to be its forerunner, and now having a great tumour in the forementioned place, he doubted not but it would be followed by a raging pestilence, which accordingly ensued. Having heard much talk of something of this nature, and being this morning casually visited by the doctor, a person of great veracity, I enquired of him how much of it was true, and I received for answer the foregoing narrative.]

The second is a very remarkable story, which I remember that famous and excellent chirurgeon *Fabricius Hildanus* records of himself; namely, that having had a pestilential tumour during a plague that happened in his youth, if, for many years after, he chanced to go to, or so much as to pass by an house infected with the plague, he was admonished of the particular disease that reigned there by a sensible pain in that part, where he had had a pestilential tumour so long before.

The third testimony is afforded me by that curious observer of the changes that happened as to the phænomena of diseases at the famous siege of *Breda*, where this diligent physician, practising much among patients afflicted with malignant and pestilential diseases, was at length infected himself; whereupon he informs his readers; *Annotandum hic merito naturæ facultatem ad pestis præservationem momenti esse maximi. Observavi in meipso contaminatos invisente statim inguen dolere vel axillas: afficiebatur aliquando caput, noctu inde sudor, & secessus tres quatuorve. Hoc & aliis accidit, qui fideliter mihi retulerunt.*

If these stories were related by ordinary persons of what happened to other men, the oddness of them might well tempt a wary man to suspend his judgment; but the judiciousness of the writers, and the profession they were of, and their relating these as things that did more than a few times happen to themselves, may well be permitted to bring credit to their assertions. And these instances, added to what has been already said, may, I hope, excuse me if I think it not time mispent to consider whether there may not be other, and even unobserved sorts of effluvia in the air; to excite your curiosity,

ſity and attention about which, rather than to declare a poſitive opinion, is that which is pretended to in what has been lately mentioned.

And whereas, *Pyrophilus*, I have in the former diſcourſe taken in the ſtructure and eſtabliſhed laws of the univerſe, as an help toward the giving an account of the coſmical attributes of things, I ſhall here alſo ingeniouſly confeſs to you, that I much fear, whether we have yet attentively enough taken notice either of the number, or the kinds of thoſe laws.

For as I am by ſome notions and obſervations inclined to think, that there may be a greater number even of the more general laws than have been yet diſtinctly enumerated, ſo I think that when we ſpeak of the eſtabliſhed laws of nature in the popular ſenſe of that phraſe, they may be juſtly and commodiouſly enough diſtinguiſhed; ſome of them being general rules that have a very great reach, and are of greater affinity to laws more properly ſo called, and others ſeeming not ſo much to be general rules or laws, as the cuſtoms of nature in this or that particular part of the world; of which there may be a greater number, and thoſe may have a greater influence on many phænomena of nature, than we are wont to imagine.

And firſt, whereas the ſtructure of the world is a main help in our preſent diſquiſition, I ſhall venture to tell you, that though I do not only commend, but in divers caſes admire the induſtry of aſtronomers and geographers, eſpecially of ſome later ones; yet they have not met with ſuch difficulties, that they have hitherto preſented us, rather a mathematical hypotheſis of the univerſe, than a phyſical, having been careful to ſhew us the magnitudes, ſituations, and motions of the great globes, ſuch as the fixed ſtars and the planets (under which one may comprize the earth) without being ſolicitous to declare what ſimpler bodies, and what compounded ones, the terreſtrial globe we inhabit does or may conſiſt of. And as of late years the diſcovery of the four planets about Jupiter, and the little moon (as ſome call it) that moves about Saturn, together with the phænomena of comets, have obliged the ſkilful to alter divers things in the theory of celeſtial bodies; ſo I know not but that future diſcoveries by improved teleſcopes and other philoſophical inſtruments may reduce us to make changes in the grand ſyſtem of the univerſe itſelf, and in that which we conſider as the moſt important of the mundane bodies to us, the terraqueous globe we live on.

What communication this may have with the other globes we call ſtars, and with the interſtellar parts of heaven, we have very little knowledge of, though I may elſewhere make it probable, that there may be ſome commerce or other; but without ſpeaking more particularly of that point, I confeſs I have ſome time ſuſpected that there may be in the terreſtrial globe itſelf, and the ambient atmoſphere, divers, whether laws or cuſtoms of nature, that belong to this orb, and may be denominated from it, and ſeemed to have been either unknown to, or overſeen by both ſcholaſtical and mathematical writers. And firſt, I have often ſuſpected, whether there may not be in the maſs of the earth ſome great, though ſlow internal change (whether originated there, or produced by the help of other mundane globes) by conſidering that almoſt in all countries, where obſervations have been made, there has been a plain and conſiderable alteration found in that which is commonly called the variation (for it is rather the declination) of the ſea compaſs or magnetick needle, which is the diſtance by which the needle declines eaſt or weſt from the true north pole. And whereas formerly, at or * near *London* the compaſs declined, as obſervations ſolemnly made, and upon record aſſure us, in the year 1580, above 11 degrees; in the year 1612, above 6 degrees;

* At *Limehouſe*.

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in the year 1633, no less than about 4 degrees; it has of late been found to have very little or no variation. And at a place within half a league of *London*, trying with a long and curious needle, purposely made and poised, I could scarce discern any declination at all, and if the needle declined sensibly any way from the pole, it seemed to do so a little towards the other side of heaven, than that towards which it did decline before. And having † afterwards by the help of a meridian line, much prized for having been accurately drawn by eminent artificers, made an observation in *London* itself; though I made it with two instruments, whereof one was a choice one, differing from the former, and from one another, I could not satisfy myself that I could discern the declination of the needle to exceed half a degree, if it amounted to so much. But since observations of this kind may prove more considerable than we yet know of, and since they ought to be made at distant places, I am contented to add here by way of confirmation, that the *Cape of Good Hope* being one of the eminentest parts of the terrestrial globe in reference to magnetisms, the acquaintance I had with one of the ancientest and most experienced navigators of this part of *Europe*, invited me to address myself to him, purposely to enquire of him, whether he had taken the variation of the compass at the *Cape of Good Hope*; and whether, if at all, he had taken it more than once, he answered, that he had often done it; whereupon asking him what he found the variation to be, and whether he had observed any change of it in his several voyages, he replied, that when he was a young seaman, he observed the variation to be about two degrees westward, and afterwards, during many years that he sailed to and fro betwixt *East-India* and *Europe*, he found the variation to encrease by degrees; and whereas he had learned from ancient writings and the tradition of old seamen, that before his time they had found no variation at all, he about 15 years ago (which was the last time he took it) found it by accurate instruments, to be 6 degrees and about 48 minutes. So that during the time that he practised the seas about the *Cape of Good Hope*, the variation still westward had increased near 5 degrees. Upon these grounds, which I may elsewhere have occasion to confirm by further observations, I cannot but think it probable that there may be agents that we know not of, that have a power to give the internal parts of the terrestrial globe itself a motion; of which we cannot yet certainly tell according to what laws it is regulated, or so much as whether it be constantly regulated by certain laws or no. And what other changes, agents that can produce a change in the terrestrial globle itself may make in this, or that part of it, who can inform us?

In the next place I consider the great uncertainty and irregularity that we have hitherto observed in the weight of the atmosphere by our new statical barometers, and much more sensibly by mercurial ones, without yet having discovered the causes of such considerable alterations in the air (save that in general they proceed for the most part from subterraneal steams) whose influences upon other things may be more considerable than we have yet had opportunity to detect.

It is very remarkable what a late and ingenious writer, that lived in some of the American islands, relates about the hurricanes in those parts; namely, that before the Europeans came thither, the inhabitants observed, that they had those fatal tempests once in seven years, and no oftener; afterwards they were troubled with them but once in six years; and in process of time, the unwelcome visits of those winds grew so frequent, that in my relator's time they came once a year; and, as a prodigy, they once observed two in one year; and afterwards three in another. I remember also, that

Monsieur de Rochefort.

† A. D. 1669.

meeting,

meeting with an inquisitive gentleman that had lived in *New England*, I desired to know of him, whether in that part of the country where he resided, there were not a great change made in the very temperature of the climate? whereto he answered me, that there was, for it was grown much milder than formerly; and because I doubted whether this change might not have been either accidental for a year or two, or apparently to the English, whose bodies by degrees might grow more accustomed to the coldness of the country, and less sensible of it; it was answered, that this change had been observed for many years after the English had planted a colony there, and that the change was manifestly perceived by the natives too, by the remisser operations of the cold upon running and standing waters, which were formerly wont to be frozen at such and such times. And I shall add for confirmation, that having one day the honour to be standing by his majesty when he received a solemn address from *New-England*, delivered by the governor of a colony there; that very inquisive monarch, amongst other questions, asking him about the temperature of the air, he told his majesty, in the presence of divers that came from *America* with him, "that the climate had much "altered and lost much of its former coldness for divers years, since the English set-"tled there."

WHETHER this decrement of the sharpness of the air will proceed, or how long it will continue, time will discover; but in the mean while, supposing with him the matter of fact to be true, and that the change depends not on any manifest cause; that which is happened already, seems to me very considerable, since I have lighted on a book * written by † one of the ancient planters of *New England*, by way of description of that country; where, among other things, I find those notable passages. The one in the seventh page: In former times, says he, the rain came seldom, but very violently, continuing its drops, which were great and many, sometimes 24 hours togethers, sometimes 48, which watered the ground for a long time after; but of late the seasons are much altered, the rain coming oftener, but more moderately, with less thunder and lightnings, and sudden gusts of wind. And the other in the 84th page; where speaking of the heathen natives, he says, They acknowledge the power of the Englishman's God, as they call him, because they could never yet have power by their conjurations to damnify the English either in body or goods; and besides they say, he is a good God that sends them so many good things, so much good corn, so many good cattle, temperate rains, fair seasons, which they likewise are the better for since the arrival of the English; the times and seasons being much altered in seven or eight years, free from lightning and thunder, long droughts, sudden and tempestuous dashes of rain, and lamentable cold winds.

So that by this it appears, that this grateful decrement of the coldness and rudeness of that climate was already taken notice of so ‖ many years ago.

To these relations may pertinently be subjoined a passage of the learned *Magnenus* in his ingenious little tract *de Mannâ* ‡; where he very solemnly delivers this notable observation, that in the country he calls *Cenotria*, there was no manna to be found a

* Intitled, *New England*'s prospect. † Mr. *W. Wood.*

‖ The book was published thirty-five years since.

‡ Sanctiorum naturæ interpretum nullus fraxinum inter arbores gummiferas aut resiniferas recensuit. Illud omnito, quo *Altomatus* sese jactare videtur, ignoravêre curiosissimi rerum indagatores, *Plinius*, *Galenus*, *Theophrastus*, & qui medium ætatem impleverunt viri doctrinâ diligentiâque celebres; quia scilicet illis temporibus multum pluebat in *Calabria* manna, quod à duobus tantummodo seculis legi cœptum. Dic amabo, *Altomate*, cur ante trecentos annos multum manna fuit in *Cenotriâ*: jam certe aderant pagi ibidem urbesque vicinæ, neque vero telelisset curiosam incolarum solertiam nihil plane video, quod pro te adduci possit ad hujus difficultatis evitandas angustias. *Magnenus de Manna*, P. M. 49.

little

little above three hundred years ago: and that in *Calabria* itself, a province so famous for manna, that the best is denominated thence, and that furnishes a great part of *Europe* with that odd drug, it is but since two ages, or thereabouts, that manna has fallen, or, as he expresses it, rained.

I know not whether it may be worth while to mention, after these more weighty observations, the œconomical traditions of house-wives, which I should not think worth taking notice of in this place, but that having purposely enquired after the truth of it, of two very sober persons (much versed in the art of making sweatmeats) that have, especially one of them, often tried it; they seriously affirmed to me that they find the spots made in linen by the juices of fruit (particularly of red currants) in straining bags, will best wash out (nay scarce otherwise) at that time of the year when those fruits are ripe the ensuing year.

To which may be for affinity's sake annexed, what is related by the ingenious French writer of the history *Des Isles Antilles* *, where he lived divers years; who, speaking of the fruit they there call *Acajou*, tells us, that the juice of some of the internal parts of it, though reputed an excellent remedy in fainting fits, is of such a nature, that if it chance to fall upon a piece of linen, it turns to a red spot, which lasts till the tree come to be again in flower; which phænomena, if the length of time, and the heat and temperature of the air usual in the seasons of producing blossoms and ripening of fruits, be found to have little or no interest in their causation, may prove of some use in our present enquiry.

Whatever be the true cause of the ebbing and flowing of the sea, yet at spring-tides the motions of such vast masses of matter as the great ocean, and most of the seas, are so constantly co-incident with the new and full moon; and the more stupendous spring-tides have been in most places so long observed to happen regularly enough about the æquinoxes, that it is worth an enquiry (though I cannot here afford it one) whether these conspicuous phænomena may not somewhat confirm the conjectures we are discoursing of.

And when I remember how many questions I have asked navigators about the luminousness of the sea; and how in some places the sea is wont to shine in the night as far as the eye can reach; at other times and places, only when the waves dash against the vessel, or the oars strike and cleave the water; how some seas shine often, and other have not been observed to shine; how in some places the sea has been taken notice of to shine when such and such winds blow, whereas in other seas, the observation holds not; and in the same tract of sea, within a narrow compass, one part of the water will be luminous, whilst the other shines not at all: when, I say, I remember how many of these odd phænomena belonging to those great masses of liquor I have been told of by very credible eye-witnesses (whose narratives to me you may elsewhere meet with) I am tempted to suspected that some cosmical law or custom of the terrestrial globe, or at least of the planetary vortex, may have a considerable agency in the production of these effects.

Nor am I sure that some subterraneal changes, or some yet unobserved commerce between the earth and other mundane globes, has not an interest in the origin, continuance, and expiring of those diseases that physicians call new, which invade whole countries (and sometimes greater portions of the earth) and last very many years, if not some ages, before they come to be extinct: of which sorts of diseases divers learned men have reckoned up divers, and whereof the venereal pox at least, as to its origin and

* Histoire Naturelle des Isles Antilles. *Liv.* 1. Chap. 6.

ſpreading, is but too manifeſt and unhappy an inſtance; whereto, according to ſome eminent doctors, we may add the rickets, a diſeaſe which, though ſcarce known in other countries, is here in *England* ſo fatal to children, which firſt (as is affirmed) diſcovered itſelf among us within the memories of multitudes of men yet alive: but of this perhaps more elſewhere.

If I ſhould now further deſcend to the peculiar phænomena of particular regions, I muſt launch out into a diſcourſe I could not have the leiſure to finiſh; and therefore I ſhall only advertiſe you of two ſuſpicions more, that I hold not unfit to intimate to you, about the eſtabliſhed laws and cuſtoms of nature.

The firſt of them is this, that I doubt thoſe that are thought the grand rules whereby things corporeal are tranſacted, and which ſuppoſe the conſtancy of the preſent fabrick of the world, and courſe of things, are not altogether ſo uniformly complied with as we are wont to preſume; at leaſt, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions; for what reaſon the wiſe author of nature pleaſed to permit that it ſhould be ſometimes, as it were, over ruled by the boiſterouſneſs (if I may ſo call it) and exorbitant motions of unruly portions of matter, I muſt not in this place (though I do it in another) inquire: but when I conſider the nature of brute matter, and the vaſtneſs of the bodies that make up the world, the ſtrange variety of thoſe bodies which the earth does comprize, and others of them may not abſurdly be preſumed to contain; and when I likewiſe conſider the fluidity of that vaſt interſtellar part of the world wherein theſe globes ſwim, I cannot but ſuſpect there may be leſs of accurateneſs, and of conſtant regularity, than we have been taught to believe, in the ſtructure of the univerſe, and a greater obnoxiouſneſs to deviations than the ſchools, who were taught by their maſter *Ariſtotle* to be great admirers of the imaginary perfections of the cœleſtial bodies, have allowed their diſciples to think. And in effect, to ſpeak only of the nobleſt of them, the ſun, and to paſs by about his motions the obſervation of the exacteſt aſtronomers, that natural days are not all of equal length (whatever the vulgar of philoſophers ſuppoſe to the contrary); and not to take notice of the great diſpute betwixt the eminenteſt aſtronomers, even of our times, about the anomaly attributed to the motion of the ſun's apogeum: to paſs over theſe things, I ſay, the ſun himſelf doth not only, from time to time, do what divers of our later aſtronomers ſtile to vomit out great quantities of opacous matter (which are called his ſpots) ſome of them bigger, perhaps, than *Europe* or *Aſia*, but has had almoſt his whole face ſo darkened with them (as about the end of *Cæſar*'s and the beginning of *Auguſtus*'s government) that for about a year together he was, as it were, under an eclipſe. To which if we add thoſe cœleſtial comets (for I diſpute not now about ſublunary ones) their number, vaſtneſs, duration, odd motions from orb to orb (as the ancients would have ſpoken) and other phænomena (whatever the cauſes of them be) it will appear, that even in the cœleſtial part of the world all is not ſo regular and unvariable as men have been made to believe.

I had ſome doubts whether this might not be much confirmed by what has been related by ſome navigators that have been in the ſouth-ſea, about certain black clouds ſaid to move as regularly in the antartick hemiſphere, as the neighbouring ſtars themſelves; to which ſome of our Engliſh ſeamen (whether firſt or no, I know not) have added, certain white clouds in the ſame hemiſphere move no leſs regularly. Of theſe relations, I ſay, I conſidered whether ſome uſe might not be made to my preſent purpoſe; but having made the beſt enquiry I could of thoſe few perſons of note I could meet with, that were likely to inform me, I do not yet ſee cauſe to alledge theſe phænomena by way of arguments. But yet, ſince I find that even pilots who have been frequently

frequently in ſome parts of the *Eaſt-Indies*, have not (whether becauſe they ſailed not far enough to the ſouthern pole, or upon ſome other ſcore) taken notice of them, I ſhall ſubjoin as a part of natural hiſtory, not obvious to be met with, the beſt account I could procure of them; which was from an obſerving captain of an Eaſt-India ſhip, with which he lately adventured to unfrequented parts of the ſouth-ſea.

THE ſubſtance of his anſwers to me about the forementioned phænomena was this, that he had divers times ſeen in the ſouthern hemiſphere, and in that part of the milky way, which is not to be ſeen upon our horizon (for he ſays, the galaxy is either completely, or almoſt a circle) two or three places that look like clouds, and move about the earth regularly with the white part of the circle in 24 hours. But by what he replied to ſome further queſtions that I aſked him, I gathered, that if theſe be the black clouds that navigators have ſpoken of, thoſe that gave them the name of clouds were probably much miſtaken; ſince, he anſwered me, that theſe are not black, but of a deep blue, which makes me ſuſpect them to be but perforations, if I may ſo ſpeak, of the milky way, by which I mean parts of the azure-ſky that are ſuffered to be ſeen by the diſcontinuations of the parts of the galaxy. And to this account of the dark clouds, his further anſwers gave me this of the white ones; which, he ſays, ſome call the Magellanick clouds, about which he related;

THAT he had divers times ſeen towards the ſouth-pole, the clouds that ſome few navigators mention to be there, and to move about the pole in 24 hours.

THAT he began to diſcover them plainly, when he was in about 18 degrees (as I remember) of ſouth latitude.

THAT they were white, in number three (though two of them be not very diſtant from each other) the greateſt being far from the ſouth-pole; the other not many degrees remoter than that ſtar, which of the * conſpicuous ones, they reckon to be neareſt to the pole; though it be about eleven degrees diſtant from it.

BUT from this account of his I dare not, as I was intimating, conclude theſe to be ſuch clouds as they are taken for, becauſe, for aught I know, if they were looked on through a good teleſcope, they would be found conſtellations of ſmall and ſingly inconſpicuous ſtars, like thoſe of the galaxy, the belt of Orion, &c. but to be reſolved about theſe matters, it is not amiſs to expect further obſervations, the propoſed conjectures being made but upon a ſuppoſition of the truth and ſufficiency of the relations.

AND thus much for the firſt of the two ſuſpicions that I above intimated I would propoſe to you: the other is very different from it, and might ſeem contradictory to it, but that they belong not to the ſame caſes; for though I lately told you I ſuſpected that in ſome things, eſpecially relating to the lines, according to which, and the limits within which ſome great maſſes of matter are ſuppoſed to perform their motions, there is more accurateneſs fancied than there really is; yet I ſhall now add, that there are caſes wherein I am not quite out of doubt, but that we may ſometimes take ſuch things for deviations and exorbitancies from the ſettled courſe of nature, as, if long and attentively enough obſerved, may be found to be but periodical phænomena, that have very long intervals between them; but becauſe men have not ſkill and curioſity enough to obſerve them, nor longævity enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, that ſpring not from any ſettled and durable cauſes; for the world, like a great animal, producing ſome effects but at determinate ſeaſons; as nature produces not beards in men till they have attained ſuch an age, and the menſes (as they call them) uſe not to happen to wo-

* This wary expreſſion keeps my relator from being contradicted by a curious modern aſtronomer, who tells of a ſtar not three degrees diſtant from the ſouthern pole; but then he ſays too, that it is a ſtar of but the fifth magnitude.

men before they come to such years, nor to last beyond such other years of their life; as may be also observed within a far shorter compass of time in the growth and falling of stags horns and bucks: if the first man had lived but one year in the world, he would perhaps have thought the blossoming of trees in spring, and their bearing fruit in summer, but an accidental thing, and would have looked upon the eclipse of the sun as a prodigy of nature; observing, that though every new moon the sun and she came very near together, yet neither before nor after was there any such terrible phænomena consequent thereupon. And we ourselves may easily remember what strange conjectures we had of the strangely varying appearances of Saturn, for divers years after our telescopes first discovered them to us.

But most remarkable is that cœlestial phænomenon afforded us by the emerging, disappearing, and re-appearing stars of this age; which have been observed in the girdle of *Andromeda*, and in or about the swan's breast (which is said to have been seen in the year 1600, and to have vanished in 1621) and especially that, which having about 25 years ago appeared for a while in the whale's neck among the fixed ones, and afterwards by degrees disappeared, was looked upon by those astronomers of that time, who did not out-live it, as a cœlestial comet: but afterwards an ingenious English gentleman of my acquaintance having observed here (as well as the vigilant curiosity of some few later astronomers hath taken notice of elsewhere) the return of the like phænomenon in the same part of heaven; it begat much wonder in all (which was increased by the slow disappearing of it) and in some curious men a resolution to have a watchful eye upon that part of the sky. Since when the justly famous *Bullialdus*, and besides some eminent foreign virtuosi (whose names I know not) divers excellent persons of our own nation having taken notice of it in the wonted place (where I had sometimes the satisfaction of seeing it); these observations, and especially the last disappearance of a star judged to have been placed among the fixed ones, and estimated to be of the fourth (if not the third) magnitude, have somewhat confirmed me in the suspicion I am now treating of. For if this and the other new stars do continue to return periodically to the same part of heaven, where they have been already long ago seen; as at least for as much as concerns this, its gradual increasing after it first begins to shew itself, and decreasing afterwards, seem to promise; then I may with somewhat more of probability than before, suspect that there may be vortices beyond the concave surface of what we call the firmament; which suspicion, if true, would much disfavour the hypothesis we now have about the system of the world, and will favour what I conjectured as possible about periodical phænomena. And however, if either the new star, without departing from its place, be only sometimes by degrees overspread and hid by spots, like those I formerly mentioned to have obscured the sun, which are afterwards by degrees dissipated, as I at first suspected; or if it have a dark hemisphere as well as a light one (or rather a greater part of its globe obscure than luminous, as *Bullialdus* ingeniously conjectures) and by turning slowly about its own center and axis, doth sometimes obvert to our eyes its luminous part, and sometimes its dark part (as Jupiter is said to do its belt-like spots, whence it must gradually both appear and disappear); according to either of these two hypotheses (though not so much as in that which preceded them) there will be reason to question the great uniformity imagined to be in the cœlestial bodies and motions; and to favour what has been proposed about periodical mutations in the mundane globes; especially since these phænomena argue, that even those stars we call fixed, and have looked upon as so invariable, are subject to mutations great enough to be taken notice of by our naked eyes at so immense a distance. I shall not here prosecute this discourse, because I would not anticipate what I foresee I shall have occasion to say about the

the terrestrial effluvia, with their causes and effects, in another discourse *, but I think myself obliged to mind you in this place, that doubts and suspicions are the only things promised by the title of this discourse; and therefore I shall not quarrel with you, if you conjecture, that though the last proposed suspicion may prove well grounded in some cases, yet in some others, the exorbitancies of the matter may, if they chance to be repeated, occasion a new custom, that may have the force of a law in this, or that part of the mundane globes, particularly in this terrestrial one we inhabit; as waters, by their frequent overflowings of the banks that cannot contain them, do sometimes make themselves new passages by their own deviations, and as it were, affect to run in the channel they once made. And as it happens also in animals, that noxious humours having once found a vent at an issue or an ulcer, do constantly take their course that way; which brings into my mind this odd observation, that having occasion to pass some years ago out of *England* into *Ireland*, traversing the maritime county of *Waterford*, the convoy that went with me shewed me once in my way, at a pretty distance off, a mountain, from whose higher parts there ran precipitously a river (which by my estimate was pretty broad) that within but two or three years before, at furthest, first broke out without any manifest cause from a great bog, that had been immemorially at the top of that mountain, and to the wonder of the inhabitants, after the first eruption of the water, had supplied the country with a river ever since: the circumstances of which new phænomenon I would gladly, at a nearer distance, have observed, but the convoy was not fond of a curiosity so dangerous in an enemy's country.

Other instances to the same purpose I cannot now conveniently stay to present you, having already made the conjectural part of this essay disproportionate to the other: and I hope there is already enough said in this latter part to answer my design, which was to excite your curiosity to seek after some certainty touching the things doubted of; and strive to enable yourself by watchful observations, somewhat to ease me of the troublesome suspicions I have confessed to you, by telling me whether they are altogether groundless or not.

* The reference here made is to a Tract about the Effects and Causes of some unheeded Changes in the Air.

OF THE TEMPERATURE OF THE SUBTERRANEAL REGIONS, As to HEAT and COLD.

ADVERTISEMENT.

THE two following tracts were designed to have been accompanied by three or four others, whereof the first treated about the temperature of the regions of the air, as to heat and cold, and had been premised to the two that now come forth, had it not been judged more proper to reserve them to accompany some other papers concerning the air. To the following tract about the submarine regions, it is thought fit to adjoin some relations about the bottom of the sea; to which was to have been added some observations concerning the saltness of the sea; but in that treatise, some blanks having been left for particulars, which the author could not seasonably find among his loose papers to fill them up with, these that now appear, having no dependance on them, it was not thought fit they should stay any longer for them.

BUT about these several tracts, this general advertisement is to be here given, that being historical pieces, consisting chiefly (though not only) of such particulars as the author must owe to the informations of others, he would not stake his reputation for the truth of every one of them; contenting himself to have performed what can be reasonably expected of him; which is, that he should carefully make his inquiries from credible persons, who, for the most part, deliver their answer upon their own knowledge; and that he should faithfully set down the accounts he procured from such relators.

Of the TEMPERATURE *of the* SUBTERRANEAL REGIONS, *as to* HEAT *and* COLD.

CHAPTER I.

IF when I used to visit mines, I had thought of writing on the subject I am now about to treat of, and had designed to satisfy myself about the temperature of the subterraneal air, as much as I did about the other subjects I was then concerned to be informed of, of, I think I ſhould have enabled myſelf to deliver much more upon my own obſervation than I ſhall now pretend to do. But though for the reaſon newly intimated, and becauſe of my being particularly ſubject to be offended by any thing that hinders a full freedom of reſpiration, I was not ſolicitous to go down into the deep mines; yet, after having diſcourſed of the temperature of the air above ground, I preſume it may not be improper or unwelcome to ſay ſomething of the temperature of the ſubterraneal regions and of the air reaching thither: for deep mines being places which very few have had the opportunity and fewer have had the curioſity to viſit, and of which I have ſcarce found any thing at all obſervable by claſſick authors, and by other writers but very little, eſpecially that I think probable enough to make uſe of; I preſume it will not be unacceptable to you, if, of regions ſo little frequented and leſs known, I report what I have been able to learn (by diligent enquiry purpoſely made) from the credible relations of ſeveral eye-witneſſes differing in nation, and for the moſt part unacquainted with each other.

Though I do not think it abſurd to ſuſpect, that in ſome places of the earth the peculiar conſtitution of the ſoil and other circumſtances may make it reaſonable to aſſign thoſe places fewer or more regions than three; yet, ſpeaking in the general, the ternary number ſeems not inconvenient to be aſſigned to the ſubterraneal regions; not ſo much upon the ſcore of the analogy that by this diviſion will be eſtabliſhed between the regions of the earth and of the air, as becauſe there ſeems to be a reaſon of the diviſion included in the diviſion itſelf. And indeed experience appears to favour it in the ſubterraneal cavity that I have hitherto been able to procure an account of from any ocular witneſs, and (very few excepted) one of the deepeſt that we yet know of in the world. And ſince it has been received for a rule among philoſophers, that which is perfecteſt or completeſt in its kind ought to be the ſtandard whereby the reſt are to be meaſured or eſtimated, I ſhall begin the remaining part of this eſſay by a relation that I obtained from a chemiſt that had purpoſely travelled into *Hungary* and other places, to viſit the mines thoſe parts are juſtly famous for; and who bringing me the honour of a compliment from a prince, to whom he belonged, gave me the opportunity of aſking him divers queſtions, his anſwers whereunto (which I preſently after put into writing) afforded me the enſuing account.

CHAP. II.

That very near the orifice of the groove he felt the air yet warm; but afterwards deſcending towards the lower parts of the groove he felt it cold until he came to ſuch a depth as he had ſcarce attained by a quarter of an hour's deſcent, and that the cold he felt during this time ſeemed to him conſiderable, eſpecially when in deſcending he had reached to a good depth.

That after he had paſſed that cold region he began by degrees to come into a warmer one, which increaſed in heat as he went deeper and deeper; ſo that in the deeper veins he found the workmen digging with only a ſlight garment over them; and the ſubterraneal heat was much greater than that of the free air on the top of the groove, though it were then ſummer.

[What is here mentioned of a cold region in the earth has been ſince confirmed to me by an ingenious phyſician upon an obſervation made in another Hungarian mine (near a town whoſe name I remember not) that was not of gold, but copper, and of much leſſer deepneſs than that newly ſpoken of; for this relator anſwered me, that in going

going down he felt a confiderable degree of cold; and when I afked whether he found the like in his return upwards, he told me he obferved it then too: and when I further inquired after the extent of this cold region, he replied, that not expecting to be afked about fuch circumftances, he had not taken particular notice of them; but thus much information my queftions procured me, that he began to feel the above-mentioned coldnefs, when he could receive no more light at all by the mouth of the groove; and that this cold region lafted till he came fomewhat near the bottom, which was eftimated to be about an hundred fathom or more diftant (in a ftraight line) from the top.

THIS relation agrees well enough for the main with that fhort but confiderable one of *Morinus*, which I elfewhere cite, who, above forty-five years ago, vifited the deep Hungarian mines in the month of July, and takes notice, that when he came down to the burrows, as he calls them, he did not find any heat as at the mouth of the well, but the beginning of a very cold as well as confiderably thick region; though I eafily believe him when he confeffes that he felt it much the colder, becaufe he had left off his own cloaths and put on the flight garments ufed there by the diggers. He further informs his reader, that when they had defcended about 80 fathoms beneath the furface of the earth, he began to feel a breath of an almoft lukewarm air; which warmth increafed upon him as he defcended lower, pleafing him not a little, becaufe it freed him from the troublefome fcents of his former coldnefs; adding, that the overfeer of the mine, who conducted him, affirmed to him, as alfo the officers of other Hungarian mines unanimoufly did, that in all their mines, at leaft all the deep ones, after a thick tract of cold earth there fucceeds a lower region that is always hot; and that after they arrived at fuch a depth they felt not any more cold, but always heat, how deep foever they dig: and to add upon the by, though this learned man lay much weight upon antiperiftafis, yet in the next page to thofe that contain what I have been juft now relating, he either very candidly or inconfiderately takes notice that they informed him, that in their mines, whether more or lefs deep, they obferved, that at fome times in the year a fomewhat intenfer heat was felt; and the two times that he exprefsly names are thofe oppofitely qualified feafons of fummer and winter.

HAVING laid down thefe general narratives, I now proceed to confider the earth's regions in particular; about which the fum of what I yet have to propound may be conveniently enough comprized in the four following propofitions.

CHAP. III.

PROPOSITION I.

"THE firft region of the earth is very variable both as to bounds and as to tempera-"ture."

THE former part of this obfervation will not be difficult to prove, fince it will be eafily granted that the manifeft operation of the fun-beams is *cæteris paribus* greater, and reaches further in hot climates than in cold ones; in the midft of fummer than in the depth of winter.

THE fecond part of the obfervation may be proved by the fame arguments as the firft; to which may be added, as to fome places, the folidity or poroufnefs of the earth; as alfo the nature of fome falts, marchafites, and other bodies contained in it, which by their natural temperature may difpofe the foil to coldnefs or heat, as I fhall have occafion to fhew when I come to fpeak of the fecond region.

IN

In the mean time I have this to obſerve further, that in this firſt region the air is uſually more temperate, as to cold and heat, than that above the ſurface of the earth; and that this region is not wont to be conſiderably deep, both parts of which obſervation are capable of being made good by the ſame reaſons, and therefore I ſhall endeavour to prove them jointly.

That in the uppermoſt region of the earth it ſhould be leſs cold than above the ſurface ſeems reaſonable to be allowed upon this conſideration, that the ſubterraneal cavities of the earth are ſheltered by the thickneſs of the ſides from the direct action of the ſunbeams, the winds, &c. and is alſo kept from an immediate, or at leaſt from ſo full a contact of the external air, when that is vehemently either heated or refrigerated.

And firſt as to the heat of the ſun, that that does much leſs powerfully affect ſuch places as are ſheltered from its action by ſolid bodies, may appear by the conſervatories of ice and ſnow, wherein frozen water is kept in that ſtate during all the heat of ſummer, and that oftentimes in cavities that are at no conſiderable depth beneath the ſuperficies of the earth: nay I remember, that having had occaſion (for the perfecting of ſome concluſions I was trying) to keep ice many weeks after the froſty weather was gone and a milder ſeaſon was come in, I was able to do it, contrary to the expectation of ſome curious men, without either digging to a notable depth in the ground or building any ſubſtantial ſtructure over the cavity. For wanting conveniencies, I contented myſelf, though it were in a champain place, with a pit ſomewhat broad at the bottom, of about four feet deep or leſs, whoſe mouth was ſheltered only by a little low thatched hovel that was wide open to the north, and only ſcreened the mouth or vent of the little pit from the direct beams of the ſun: and though I will not deny that in deep conſervatories of ſnow, the natural coldneſs of the earth, eſpecially in ſome places, may contribute to the effect; yet I remember, that diſcourſing once with a traveller and ſcholar that was born in hot countries, of a conjecture of mine, that in an arched building whoſe walls were ſufficiently thick and whoſe air were carefully kept from all avoidable intercourſe with the external air, one may, without digging ſo much as a man's depth into the ground, make a ſufficient conſervatory for ice in very open and unſheltered places, and even ſuch as *Saliſbury Plain* itſelf; diſcourſing, as I began to ſay, with this traveller about this conjecture, he told me, that at a place he named to me, in the ſouthern part of *France*, whoſe heat ſeemed to me to exceed that of divers parts of *Italy*, ſome curious perſons that were reſolved at any rate to have ice in ſummer, though the ſoil were ſuch that they could not dig four feet without meeting with water, were yet able to make uſe of conſervatories, by covering the brick building they made over their pits with clay and ſand to a very conſiderable thickneſs, and taking care that the only place that ſhould permit acceſs to the outward air ſhould be a ſmall northern door to go in and out at, fitted to ſhut exactly cloſe, and fenced with a little porch furniſhed with another door: and by this means he affirms theſe gentlemen to reſerve the included ice, not only all the ſummer long, but ſometimes for two or three years together, the heat of that region making many of their winters too mild to recruit them with ice.

To all theſe things I ſhall add, that even where the intercourſe is not quite debarred, but left free enough betwixt the ſubterraneal and the ſuperior air, the operation of the ſun-beams may be very much leſs in a cavity though but ſhallow beneath the ſurface of the ground than above it. For beſides that trials have informed me that liquors, that differ in little elſe than in conſiſtence, will not ſo eaſily pervade each other as a man would ſurmiſe, unleſs ſome external motion haſten their intimate mingling with one another; I remember, that one morning pretty late, having had the curioſity to deſcend into a pit where they were digging out iron ore, though this cavity had no very narrow origoing down he felt a conſiderable degree of cold; and when I aſked whether he found the like in his return upwards, he told me he obſerved it then too: and when I further inquired after the extent of this cold region, he replied, that not expecting to be aſked about ſuch circumſtances, he had not taken particular notice of them; but thus much information my queſtions procured me, that he began to feel the above-mentioned coldneſs, when he could receive no more light at all by the mouth of the groove; and that this cold region laſted till he came ſomewhat near the bottom, which was eſtimated to be about an hundred fathom or more diſtant (in a ſtraight line) from the top.

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THE ſecond part of the obſervation may be proved by the ſame arguments as the firſt; to which may be added, as to ſome places, the ſolidity or porouſneſs of the earth; as alſo the nature of ſome ſalts, marchaſites, and other bodies contained in it, which by their natural temperature may diſpoſe the ſoil to coldneſs or heat, as I ſhall have occaſion to ſhew when I come to ſpeak of the ſecond region.

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CHAP. IV.

AND as we have shewn that the subterraneal air, even in the first region, is usually much less heated than the superterrestrial air, so we may easily observe, that that inferior air is (*cæteris paribus*) wont to be much less refrigerated by the grand efficients of intense cold than the superior air.

I WILL not urge on this occasion what I have observed by a surer way than for aught I know has been before practised, about the smoaking of some springs in frosty weather; because I do not know but that those springs may have come from or passed a good way through some place very deep beneath the surface of the directly incumbent ground, and perhaps from a soil peculiarly fitted to warm them; whence the water may have derived a warmth considerable enough not to be quite lost, till it began to spring out of the ground, where it needed only not to be quite cold, to appear to smoke; the intense coldness of the air making those exhalations visible in frosty weather which would not be so in milder, as is evident in a man's breath, which appears like a smoke in such weather, though it be not visible in summer.

THAT therefore, which I shall propose in favour of our observation, is first taken from the nature of the thing, which may persuade us that the subterraneal air being, though comparatively cool, yet indeed moderately warm in summer, ought not to be affected with winter's cold so much as that contiguous to the surface of the earth, from whose immediate contact it is by a thick arch of earth, if I may so call it, defended; and that the cold reigns most in the free air and the superficial parts of the terrestrial globe, may appear by water's beginning to freeze at the top, not at the bottom; to which reason, from the nature of the thing, I shall add only this from experience, that we see that in cellars that are arched and carefully kept close from the communication of the outward air, beer and other liquors may be kept from freezing in frosty and snowy weather; as I have observed in a cellar that was but shallow but well arched, in a winter that was sharp to a wonder and froze stronger liquors than beer in another cellar very near it that differed not much from it in depth, but had not so thick and solid a roof: and that not only here in *England* where the cold is less violent, but even in *Russia* itself where it is wont to be so extreme, it reaches not near so deep as one would think, I learned by inquiry purposely made of an ingenious physician that lived at *Moscow*, who answered me that others, and he himself, did in that city keep all the winter long, not only their wine but their beer from freezing, in cellars that were not above twelve or fourteen feet deep, but well covered above and carefully lined with planks of fir, without any entrance but a small trap-door (commonly at the top) which was fitted so exactly to the orifice it was to close, as to exclude, as much as was possible, all communication between the internal and external air, that the latter might not affect the former with its coldness.

I HAVE indeed suspected, that in some cellars the comparative warmth we find there may be partly due to subterraneal exhalations that are pent up in them, and perhaps too in some measure from the steams of the fermenting or fermented liquors lodged in those places:

places: and I was somewhat confirmed in this suspicion by an information my inquiries obtained from the newly mentioned doctor, who told me upon his own observation, that in one of the cellars he made use of at *Moscow*, having occasion to open the above mentioned trap-door after the cellar had for a good while been kept very close shut, there came out at the vent that was thereby given a copious steam in the form of smoke, which to them who had their bodies affected with the external air, was very sensibly warm, and was almost unfit for respiration; which circumstance increased my suspicion that there might be among these steams some of the nature of those that have been observed to come from fermenting liquors, especially wine, and so abound in some cellars as almost to stifle those that ventured into those vaults, and to kill some of them outright; which effects the long abode of subterraneal steams in stagnating air, even in many places where no metalline ores at all nor other noxious minerals have been found, has enabled that air to produce, of which divers sad instances have been given within less than a mile of this place, upon men's first going down into pits or wells that had not in a long time been opened or made use of; but this is here mentioned only upon the by; nor have we any necessity to fly to subterraneal exhalations for the comparative warmth that good cellars in general afford in frosty weather, since that phænomenon may be accounted for by the reason formerly given, that the closeness of the cavity and the thickness of the sides and roof keep it from being vehemently affected with the cold of the ambient air.

I KNOW it is pretended that the warmth we speak of proceeds from an antiperistasis; but not now to engage in a controversy that would take up too much time, it may here suffice to represent, that in our case there appears no necessity of recurring to it, the phænomenon being solvable by the region newly cited, which may be confirmed by this experiment, that in the vaulted cellar above mentioned wherein beer was kept from freezing in an almost prodigiously sharp winter, the included air, though sensibly warm to those that came out of the free air, had not so intended its native heat as the asserters of antiperistasis would have expected, being colder than the free air commonly is in that place, not only in the heat of summer, but in other seasons, when the weather is temperate, as I was assured by comparing my own observations made at other times, with the account brought me by a skilful person whom I employed in that cellar at late hours in one or two of the sharpest nights of the forementioned cruel winter, with the same excellent sealed weather-glass that I had long kept suspended within a stone's cast of that place.

CHAP. V.

HAVING said thus much about the earth's uppermost region, I now proceed to that which lies next beneath it, whose temperature I cannot so conveniently give an account of in less than two propositions, whereof the first is this:

PROPOSITION II.

"THE second region of the earth seems to be for the most part cold in comparison of "the other two."

THIS proposition may be confirmed partly by reason and partly by experience.

And firft, it feems confonant to reafon, that fince the earth is naturally a body confifting of grofs and heavy parts that are ufually much lefs agitated than thofe of our organs of feeling, it fhould, as to fenfe, be cold; and that therefore that quality may be juftly afcribed to it in that region, where, by virtue of its fituation, it is kept from being confiderably affected either by the heat of the fuperior air, or by that of the deep parts of the earth; which upper and lower heat are the two agents that feem of all others the moft likely to put its parts into an unufual motion, and thereby change its natural temper.

That our propofition is alfo confirmable by experience, may be gathered from the relations fet down in the former part of this difcourfe.

And here it will be proper to take notice of the advertifement intimated in the clofe of our above delivered propofition, that this coldnefs afcribed to the fecond region of the earth is to be underftood comparatively to the other two. For otherwife that even this earth is not, as many naturalifts would have it, the *fummum frigidum*, I gather from this, that I could never hear of any ice met with there at any time of the year, though fnow or hail may be produced in the middle region at differing, and fometimes quite oppofite feafons of the year; nay, I have not found by the anfwers that were made me by thofe that have defcended far enough into this region, that they found the cold any where very great, or that in fome places they have found it at all confiderable; as we fhall fee in the explication of the next propofition. I know not whether it will much ftrengthen what has been faid, if I add, that I learned by enquiry of fuch perfons as I lately mentioned, that at the mouth of deep grooves, in mines, the fteams that afcend do often feel warm; though the outward air, where the obfervation is made, be affected with the heat of fummer: but though this probably argues, that if the middle region of the earth through which thefe fteams muft afcend were very intenfely cold, they would be fo refrigerated in their paffage as to feel rather cold than hot at their appearing above ground, efpecially in fummer; yet I fhall not lay much weight (for fome may perhaps be allowed it) upon this argument, becaufe I have not yet tried how far a warm fteam may be altered in its paffage through a cold conduit; not to mention, that in the earth, the paffage, by being directly upwards, may be much the nimblier traverfed.

CHAP. VI.

The fecond propofition relating to the temperature of the fecond region of the earth may be delivered in thefe terms:

Proposition III.

"In feveral places, which, by reafon of their diftance from the furface of the earth, "one would refer to the middle region of it, the temperature of the air is very differing "at the fame times of the year."

I chose to exprefs myfelf thus to prevent fome ambiguities and objections which I forefaw that fhorter, but lefs clear and full expreffions might give occafion to.

In the proof of our propofition both experience and reafon may diftinctly be employed. And to begin with experience,

Whereas in the above-recited defcent into the Hungarian mines there was obferved a notably cold region of a confiderable thicknefs, I have purpofely procured accounts from

from divers perfons that have here in *England* had occafion, fome of them frequently, to defcend into deep pits or grooves of differing minerals, without finding by the narratives they made me that they took notice of any notably cold part that they paffed thorough, unlefs I particularly afked a queftion about fuch a thing. But for aught I could gather from their fpontaneous relations, they felt in fummer-time a remiffion of the heat of the external air as foon as ever they began to defcend; which warmth did not fo far decreafe as to terminate in any notable coldnefs before they came into a deeper part of the earth, where they are never troubled with that quality: and fome of thefe relations I had from profeffed miners, and was curious that the relations I procured fhould be of fubterraneal parts feated in very differing parts of *England*, as well as of places not all, or moft of them having veins of one and the fame mineral. And I learned by particular inquiry from a practical mathematician that was often employed about lead mines, that at fuch depths, as (according to *Morinus*) the fecond region of the earth reaches to, he himfelf obferved it to be fenfibly warm at all feafons of the year (for about that circumftance I was peculiarly folicitous to be fatisfied).

Nor is it unconfonant to reafon that the middle region of the earth, in the fenfe meant in the propofition, fhould not be of the fame temperature in all places; not only becaufe of the differences which the climate may produce by reafon of its being very much hotter or very much colder in one place than in another; but from the peculiar conftitution of the foil, to the confideration whereof I fhall here confine myfelf.

Now this temperament of the foil itfelf may be diverfified not only by its greater or leffer compactnefs (upon which account fome foils are rocky or ftony and others light and fpongy) but from the nature of the fprings or fubterraneous liquors that may abound in it or ftrain through it into the groove or pit we fuppofe the obferver to be in; and that efpecially by the minerals, particularly falts and marchafites, that grow near the fides of the well, or are brought thither by the waters.

To illuftrate this, give me leave to confider that nature does not regulate herfelf under ground by our imaginary divifions; but, without taking notice of them, produces marchafites, falts, and other minerals, moft frequently perhaps in what we call the lower region of the earth; but yet fometimes too in our upper region, and oftentimes in our middlemoft region. Let us then fuppofe, that in fome places of this laft named region there be a mine of that earth that naturally abounds with embryonated nitre or with fome other falt that is apt, efpecially being diffolved or moiftened with water (a thing very familiarly to be met with in mines) to fend out a refrigerating effluvium, or by its contact to cool the air. Let us alfo fuppofe, that by the fides of another well of the fame depth there are ftore of unripe minerals that are in the procefs of generation, or rather a great quantity of marchafitical earth, if I may fo call it, that is fuch a fubftance as I have met with in more than one place copioufly impregnated, and, as it were, blended with minerals of a marchafitical nature, and yet of fo open and loofe a texture as not only water would in a few hours, but air alfo would not in very many, evidently work upon it. And, fince, during the time that marchafites are flowly diffolving, it has been obferved, according to what we have elfewhere delivered *, that many of them will conceive a very confiderable degree of heat, will it not be very probable that the temperature of the earth in the place that abounds with thefe marchafitical minerals will be very warm in comparifon of the temperature of the other place, where the foil does plentifully produce nitrous and other refrigerating bodies, though both the places be fuppofed to be

* The tract here pointed at is a Difcourfe of fubterraneal fires and heats.

at the ſame diſtance from the ſurface of the earth, and conſequently in the ſame ſubterraneous region.

Upon the like grounds it may alſo be ſuſpected, that in the ſame places the temperature may not be always the ſame, even upon the account of the ſoil; for I elſewhere ſhew, that ſome ſaline earths, eſpecially nitrous, and ſome minerals that partake of the nature of marchaſites, admit a kind of maturation, and perhaps other changes that ſeem to be ſpontaneous; and that ſuch changes happen the more notably in thoſe parts of ſuch bodies that are expoſed to the air, as thoſe are that chance to be placed at the ſides of the deep wells we are talking of; which things being pre-ſuppoſed, it will not be abſurd to conceive that the mineral, to which either heat or cold is to be referred, may be more copious, ripe, and operative at one time than at another; or, that at length all the earth capable of being, as it were, aſſimilated by the mineral rudiments harboured in it, may be conſumed, or the mineral itſelf may arrive at a perfection of maturity which will make its texture ſo cloſe as to be unfit to be penetrated and wrought upon, as before, by the water or other liquor that occaſioned its incaleſcence.

C H A P. VII.

I omit to ſpeak of the tranſient changes that may be occaſioned in the temperature of the ſecond region of the earth by ſeveral accidents, and eſpecially by the ſubterraneal exhalations that in ſome places and times copiouſly aſcend out of the lower regions of the earth; nor ſhall I inſiſt upon any of the other cauſes of a more durable difference of temper in ſome parts of the ſecond region, ſuch as may be the vicinity of ſubterraneal fires in the third region that heat the incumbent ſoil; becauſe I would haſten to the third and laſt part of this diſcourſe, which yet I muſt not do without premiſing this advertiſement, that I think myſelf obliged to ſpeak the more heſitantly and diffidently about the temperature of ſubterraneal air, becauſe mineraliſts have not had the curioſity to examine it by weather-glaſſes, which would give us much more truſty informations than our ſenſe of feeling powerfully pre-affected by the cold or heat of the external air. I did indeed ſend fit inſtruments to ſome days journey from this place, to examine the air at the bottom of ſome of our deep mines, but through ſome unlucky caſualties upon the place the attempt miſcarried. But when I ſhall (God aſſiſting) recover an opportunity that I have ſince wanted, I hope an accurate ſealed weather-glaſs, joined with a portable baroſcope, will give me better information than mineraliſts have yet done: I ſay a ſealed weather-glaſs, becauſe though common thermoſcopes had been employed by miners, I durſt not rely upon them; being perſuaded by trials purpoſely made, as well as by the reaſon of the thing, of the fallaciouſneſs of ſuch thermoſcopes; for in them the included air is liable to be wrought upon, not only by the heat and coldneſs, but by the weight or preſſure of the external air: ſo that if a thermoſcope be let down from a very conſiderable height, at the top of which the ſtation of the pendulous liquor be well marked, that liquor will be found to have riſen when the inſtrument reſts at the bottom, as if the included air were manifeſtly refrigerated, though the temper of the external air may be in both places alike, the cauſe of the pendulous liquor's riſing being indeed, that the aëreal pillar incumbent on the ſtagnant liquor is higher and heavier at the bottom where the inſtrument reſts than that which leaned upon it, at its firſt or upper ſtation nearer the top of the atmoſphere. From whence it will be eaſy to conclude, that at the bottom of a deep groove where the atmoſpherical pillar that preſſes the ſtagnant water will be much longer

longer and heavier than at the top, the air may appear by the inftrument to be colder in places where it is really much hotter, the increafed weight of the incumbent air being more forcible to impel up the pendulous liquor, than the endeavour of expanfion procured in the included air by the warmth of the place is to deprefs it.

CHAP. VIII.

THAT which challenges the third and laft part of my difcourfe is the lowermoft region of the earth, about whofe temperature I fhall comprize what I have to fay in the following propofition :

PROPOSITION IV.

"THE third region of the earth has been obferved to be conftantly and fenfibly warm, "but not uniformly fo, being in fome places confiderably hot."

I MENTION, that the recited temperature has been obferved in the lower region, becaufe I would intimate that I would have the propofition underftood with this limitation as far as has been yet (that I know of) obferved: for almoft all the deep grooves that mineralifts have given us accounts of, and wherein men have wrought long enough to take fufficient notice of the temperature of the air, have been made in foils furnifhed with metalline ores or other minerals, without which men would not be invited to be at fo great a charge as that of finking fo very deep pits and maintaining workmen in them; fo that experience has yet but flenderly, or at leaft not fufficiently informed us of the temperature of thofe parts of the third region of the earth that are not furnifhed with ponderous minerals, and confequently has not informed us of the temperature of the lowermoft region in general, as will better appear by what I fhall ere long reprefent.

HAVING premifed this advertifement about our propofition, we may proceed to the diftinct proof of the two parts or members it confifts of.

AND to begin with the firft, whatever the peripateticks teach of the innate coldnefs of the earth, efpecially where it is remoteft from the mixture of the other elements, yet having purpofely enquired of feveral perfons that vifited and alfo frequented the third region in differing countries, foils, and at differing depths under ground, and feafons of the year, I did not perceive that any of them had ever found it fenfibly and troublefomely cold in the third region of the earth; and on this occafion I remember I had fome light fufpicion, that at leaft in fome cafes the narrownefs of the cavities wherein the diggers were in divers places reduced to work, might make the warmth they felt proceed in great part from the fteams of their own bodies, and perhaps of the minerals, and from the difficulty of cooling or ventilating the blood in an air clogged with fteams: and I was the rather induced to think this poffible, becaufe I had (even in metalline mines that were but fhallow and very freely acceffible to the air) obferved a ftrong fmell of the metal abounding there.

I HAVE likewife found by feveral trials, that the exhalations that proceed from the bodies of animals do fo vitiate the air they abound in, as to make it much lefs fit for their refpiration, and to be apt to make them fick and faint; wherefore I thought it not altogether unfit to inquire whether the heat of the fubterraneal air in fuch places as have been newly mentioned might not be referred to thefe caufes, but I was anfwered in the nega-

tive;

tive; eſpecially by an inquiſitive perſon that had been in the deepeſt and hotteſt mines that have been viſited by any acquaintances of mine.

THIS way of accounting for the ſubterraneal warmth being laid aſide, it ſeemed, I confeſs, ſomewhat difficult to conceive how it ſhould be produced; yet two principal cauſes there are, to which I think we may probably refer the temperature of thoſe places where the air is but moderately warm, to which a third is to be added, when we come to give an account why ſome places are troubleſomely hot.

AND firſt, why the coldneſs of winter ſhould not be felt in the lowermoſt region of the earth may be, that the air there is too remote from the ſuperterreſtrial air to be much affected with thoſe adventitious cauſes of cold that make that quality intenſe in the air above ground; but becauſe this reaſon ſhews rather why it ſhould not be in the earth's lower region much colder in winter than in ſummer, but not why it ſhould be in all ſeaſons warm there, I ſhall add as a conjecture, that the poſitive cauſe of the actual warmth may proceed from thoſe deeper parts of the ſubterraneal region which lie beneath thoſe places which men have yet had occaſion and ability to dig. For it ſeems probable to me, that in theſe yet unpenetrated bowels of the earth there are great ſtore-houſes of either actual fires or places conſiderably hot, or (in ſome regions) of both, from which reconditories (if I may ſo call them) or magazines of hypogeal heat, that quality is communicated, eſpecially by ſubterraneal channels, clefts, fibres, or other conveyances, to the leſs deep parts of the earth, either by a propagation of heat through the ſubſtance of the interpoſed part of the ſoil (as when the upper part of an oven is remiſsly heated by the ſame agents that produce an intenſe heat in the cavity) or by a more eaſy diffuſion of the fire or heat through the above-mentioned conveyances (as may be exemplified by the pipes that convey heat in ſome chemical ſtructures) or elſe (which is perhaps the moſt uſual way) by ſending upwards hot exhalations and mineral ſteams, which by reaſon of the com a at vely heavy mi erals they conſiſt of, and by reaſon of their being leſs diſperſed nearer the places whence they proceed, are uſually more plentiful in the deeper parts of the earth, and ſomewhat affect them with the quality that they brought from the work-houſes where they were formed, and that they retain for ſome time after.

CHAP. IX.

THAT manifeſt ſteams oftentimes are found in grooves, eſpecially in deep ones, is evident by the damps that infeſt moſt of them, and that in diſtant regions, as in ſeveral provinces of *Germany*, *Bohemia*, *Hungary*, &c. as alſo in ſeveral parts of *England*, in grooves, ſome of which I have received relations of from the mine-men themſelves; by which it appears that ſeveral of theſe exhalations aſcending from the entrails of the earth are ſulphureous and bituminous in ſmell, and in ſome grooves (one whereof I elſewhere mention myſelf to have viſited) theſe ſteams are apt actually to take fire.

THE warmth of many ſubterraneal exhalations, I think, may be made further probable by ſome other obſervations. For though theſe newly mentioned are not to be rejected, and may be employed for want of better, yet I have ſeveral times queſtioned whether I ought to acquieſce in them alone; for I do not think the eaſy inflammableneſs of bodies to be always a ſure proof of the actual ſenſible warmth of the minute parts it conſiſts of, or may be reduced into. For though ſalt-petre be very inflammable, yet being by a ſolution in fair water reduced to inviſible corpuſcles, it highly refrigerates that liquor: nor have I obſerved its fumes (when far from the fire) to have any heat ſenſible to our touch.

And

And the like may be faid of the exhalations of highly rectified fpirit of wine; which yet we know is itfelf totally inflammable; nay I know not, whether (for a reafon elfewhere declared) copious exhalations may not afcend from the lower parts of the earth, and yet be rather cold than hot; for, in another paper, I mention a way by which I made a mixture that plentifully enough emitted fteams, of whofe being rather of a cold than a hot nature there was this probability, that the mixture whence they afcended, even whilft its component ingredients were brifkly acting upon one another, was not only fenfibly but confiderably cold.

One main thing, therefore, that induces me to affent to the opinion whereto the former inftances do but incline me, is, that having purpofely inquired of an obferving man that frequented deep mines (wherein he had a confiderable fhare) he anfwered me, that he plainly obferved the fumes that came out of the mouths of the deep pits to be actually and fenfibly warm, and that in a warm feafon of the year. And *Morinus* (above cited) fpeaking of the deep Hungarian mines, makes it the firft epithet of the copious exhalation that afcended from the bottom, that it was hot; and a few pages after he fays, that at the mouth of the well the afcending fumes were fenfibly hot in fummer itfelf: and the fame arguments that I have elfewhere given to fhew that there are very hot places, and, as it were, æftuary in the bowels of the earth, may ferve to make it probable that the fteams afcending thence may be actually warm.

That alfo in many places of the earth where no grooves are dug and no vifible exhalations are taken notice of, they may yet pervade the foil, and exercife fome operations of warmth, may be probable by this, that the experienced *Agricola* himfelf reckons it among the figns of a latent mineral vein, that the hoar-froft does not lie upon that tract of the furface of the earth under which a vein (though perhaps very deep) runs. The like directions I have known given by the fkilful in *England* for the difcovery of places that contain coal-mines. And I remember a near relation of mine fhewed me a great fcope of land of his, which (though in an outward appearance likely to be as cold as any place thereabouts) he affirmed would not fuffer fnow to lie upon it above a day or two in the midft of winter.

The probability of which relation was confirmed to me by the anfwer I received from a very ingenious gentleman who lives among mines, and is not a little concerned in fome of them. For having inquired of him what he had obferved about the lying or not lying of the fnow on the mineral foils near the place of his refidence, he replied, that in fome of them he did not take notice of any peculiar indifpofition to let the ice and fnow continue on them, which I conceive may proceed either from the want of fuch minerals in the fubjacent parts as were then in the ftate of incalefcence; or elfe from this, that (according to what we have elfewhere obferved about the fnow on *Ætna)* the direct afcenfion of the hot fteams was hindered by fome layers of rocks or ftone through which the fteams could not penetrate, or could do it but fo flowly as to lofe their actual warmth by the way. But this gentleman added, that in other places, near that of his abode, and fuch as he knew to have mineral veins beneath them, he obferved that the fnow (nor the ice) would fcarce continue at all upon the furface of the ground, even in an extraordinary cold winter.

It will be a confiderable inftance to our purpofe, if it be indeed true which fome learned men have written, that near the gold mines in *Hungary* the leaves of the trees (efpecially thofe that refpect the ground) are oftentimes found ennobled with a golden colour from the metalline exhalations of the gold mines; which, one would think, muft by reafon of their ponderoufnefs need a confiderable heat to elevate them, efpecially into the open air, but though doubting of this relation, as not made by mineralifts or accurate obfervers, I inquired about it of a perſon whoſe curioſity carried him purpoſely to viſit thoſe mines, I was anſwered that he could not be a witneſs to the truth of the obſervation; yet he told me an obſervation (which I elſewhere mention) that doth not diſcountenance that tradition.

If it be objected that what has hitherto been ſaid about latent fires and heats in the bowels of the earth will give an account of the warmth only of thoſe places that are within reach of the action of ſuch magazines of heat, which probably may be wanting in many places of the earth; I ſhall readily confeſs, that, as I firſt made this objection to myſelf, ſo I do not yet diſcern it to be unreaſonable; and that, for aught I know, if men had occaſion to dig as deep, and be as far converſant in many other low places of the earth where there are no ſigns of minerals, as they have done where the hopes of actual diſcovery of veins of metals and other minerals worth working have invited them, divers places in the third region of the earth would be met with that would be deſtitute of the warmth that has hitherto been generally found in places of the ſame region that either abound with minerals themſelves, or are near ſome of the deep and latent æſtuaries above mentioned.

And as for thoſe parts of the third region of the earth, which men feel not only warm but troubleſomely hot, that incommodious degree of heat ſeems not (at leaſt in ſome places) to be derivable from the two above-mentioned cauſes, which muſt (to produce ſo conſiderable an effect) be aſſiſted by a third cauſe more potent than themſelves; which ſeems to be the incaleſcence there is produced in many mines and other places by the mutual action of the component parts promoted by water of immature and more looſely contexed minerals, eſpecially ſuch as are of a marchaſitical nature: that ſuch an incaleſcence may by ſuch a way be produced in the bowels of the earth, I have elſewhere ſhewn (in my diſcourſe of ſubterraneal fires and heats) by the examples of ſuch incaleſcences producible in mineral bodies here above ground: that marchaſites, which for the moſt part abound in vitriol, are bodies very fit to procure this ſubterraneal heat, may be confirmed not only by the ſulphureous and ſaline parts they abound with, and by this, that many of them may be wrought on, as we have tried, both by ſimple water and even by moiſt air, which argues the reſolubleneſs of their conſtitution; but alſo by this, that having purpoſely inquired of a gentleman that went out of curioſity to viſit one of the deeper Hungarian mines, he confirmed to me what I had otherwiſe been informed of, by anſwering me, that in the lower parts of the mine he had gathered vitriol that appeared above ground to be of a golden nature; and that in a cave that is on one ſide of the groove in the deep gold mine near *Cremnitzo*, the corroſive ſmell is ſo ſtrong and noxious that men have not dared to dig out the native gold it richly abounds with, being deterred by the ill fate of divers that ventured to work in it. Adding, that though he paſſed by it in great haſte, yet he could not avoid the being offended by the noiſome exhalations. And on this occaſion, it will not be, I preſume, diſliked, if I illuſtrate what I was ſaying of immature minerals, by ſubjoining, that having aſked this chemiſt, whether the vitriol he found very deep under ground were all ſolid, or ſome of it ſoft? he affirmed, that as he gathered it he found ſome of it ſoft; and to ſatisfy my curioſity to know whether it continued that yielding conſiſtence; he farther told me that it was ſoft in the deeper part of the mine, but when he had brought it into the ſuperterreſtrial air, it hardened there, and appeared to have divers golden ſtreaks in it.

CHAP.

C H A P. X.

ONE thing there is, which muſt not be here omitted, though it will probably be great news to thoſe that philoſophize only in their ſtudies, and have not received information from any that viſited the deeper parts of the earth. The phænomenon is this, that the diggers in mines, having found by unwelcome experience, that in deep grooves, the air (unleſs ventilated and renewed) does in a ſhort time become unfit for reſpiration, have been put upon this expedient, to ſink, at ſome convenient diſtance from the groove where the miners work, another pit, by ſome called a vent pint, that uſually tends directly downwards (though ſometimes it make angles) to which our Engliſh mine-men in do ſeveral parts of this kingdom give differing names, whereof the moſt ſignificant ſeems to be that given it in the lead mines of *Derbyſhire*, where they call it an air-ſhaft, and are wont to make it 40, 50, and ſometimes 80 or 100 paces off; and, as one of the chief and ſkilful miners there informed me, as deep as the groove or well; (though I find that the beſt German and ſome Engliſh miners think a leſs depth will often ſuffice) from this air-ſhaft to the groove the men work in there paſſes a channel, or, if I may ſo call it, ventiduct, to convey the air from the former to the latter; which is that, that *Agricola* ſometimes (for he employs not the term always in the ſame ſenſe) denotes by his cunicu- Lib. V.
lus; and which, though differingly named by our miners in ſeveral parts of *England*, is & VI. de
in the above mentioned lead mines called a drift, becauſe the air does uſually in the form re metall.
of wind drive through it, and thereby enables the workmen to breathe freely and conveniently enough at the very bottom of the well. On this occaſion I remember that a very obſerving man, who much frequented theſe mines, told me, that at the depth of no leſs than about 200 yards, he found, that by the help of the air-ſhaft, the air was not only very commodious for reſpiration, but temperate as to heat and cold. And when I further aſked what time of the year it then was? he told me it was about the latter end of Auguſt, and the beginning of September.

Now that which ſeems to me to deſerve a farther and accurate obſervation about the motion and temperature of the air in theſe artificial under-ground cavities, is a relation of *Agricola*'s which (though he be the moſt claſſick author we have about mines) has not, that I know of, been taken notice of, in him. For this experienced writer, though in his treatiſe * *de ortu & cauſis ſubterraneorum*, he only ſays, indefinitely, that by means of the cuniculus or drift, which connects the air-ſhaft and the well, that air which comes in at one of thoſe two, paſſes out at the other; yet in his fifth book, *de re metallicâ*, he gives a more particular and odd account of the courſe of the air in theſe not over-clear terms, *aer autem exterior ſe ſuâ ſponte fundit in cava terræ, atque cum per ea penetrare poteſt, rurſus evolat foras. Sed diverſâ ratione hoc fieri ſolet; etenim vernis & æſtivis diebus in altiorem puteum influit, & per cuniculum vel foſſam latentem permeat, ac ex humiliori effluit; ſimiliter iiſdem diebus in altiorem cuniculum infunditur, & interjecto puteo defluit in humiliorem cuniculum, atque ex eo emanat. Autumnali verò & hyberno tempore contra in cuniculum vel puteum humiliorem intrat, & ex altiori exit: verum ea fluxionum aeris mutatio in temperatis regionibus fit in initio veris, & in fine autumni: in frigidis*

* Idcirco ſcrobes, putei cuniculi effoſſi complentur exteriore aere. Atque id ipſum in eos influere imprimis hyemali tempore evidens eſt in duobus puteis, ad quorum utrumque ex modico intervallo cuniculus aliquis pertinet. Nam aer in unum continuo influit, rectaque per cuniculum permeat & tranſit ad alterum; atque ex eo rurſus evolat foras.

autem,

autem, in fine veris & in initio autumni. To which he adds, † that which is more remarkable, that the air in both the mentioned times, before its wonted courſe come to be durably ſettled, uſes to be for the ſpace of a fortnight liable to frequent changes, ſometimes flowing into the upper or higher groove or drift, and ſometimes into the lower, and paſſing out at the other. If this obſervation conſtantly hold, though but in ſome deep mines, it may hint ſome odd inquiries about conſiderable and periodical changes in the ſubterraneal parts of the earth, or in the air, or in both; which, though they have not yet been conſidered, deſerve to be ſo. I have endeavoured to learn whether any ſuch thing has been obſerved in ſome deep lead mines, whence I have procured divers informations about other particulars. But a very obſerving perſon, that had the chief hand in contriving the ſubterraneal ſtructures there, aſſured me that both winter and ſummer, the current air went conſtantly the ſame way; the air entering in at the mouth of the air-ſhaft, and coming out at the perpendicular groove, which takes its denomination from a cave (or *caſa putealis)* uſually built over the orifice of it, to ſhelter the workmen from rain, and other inconveniencies.

AND ſince the writing of this, I found in *Morinus* (his relation already mentioned) a paſſage that may ſomewhat illuſtrate the darkly expreſſed obſervation of *Agricola*; for the lately mentioned author writes, that in the deep Hungarian mines he viſited, the outward air paſſed, firſt, through the boroughs, and ſo, through by-ways, if I may ſo call them, that tended not directly downwards, reached at length to the bottom of the well, or perpendicular groove, whence, together with the ſteams proceeding from the mine, it aſcended ſtraight upwards. But *Morinus* taking no notice at all of *Agricola*'s obſervation about the differing courſe of the ſubterraneal air at differing ſeaſons of the year, though, as I find by what he writes elſewhere, it was ſummer when he viſited the mines, and ſo what he reports, agrees well with one part of what *Agricola* ſeems to ſay; yet, as to the other and principal part of his obſervation, he ſays not any thing. And the ſenſible heat he aſcribes to the ſteams aſcending out of the perpendicular well, leaves it ſomewhat dubious, what intereſt the rarefaction of the air by the ſubterraneous heat may have in the phænomena we have been diſcourſing of.

BUT to return to what I was ſaying before I had occaſion to mention *Morinus*; which perhaps it will not be impertinent to add, that I learned by inquiry, that the air-ſhafts and the wells were in theſe mines much of a depth; but I hope before long to have accounts of what happens in other mines, in other parts of *England*, as to the courſe of the ſubterraneal air, eſpecially when its iſſuing out of the well or the air-ſhaft depend not on the changes of the winds that blow above ground; and I wiſh the curious would employ the like endeavours in other countries.

FOR indeed, what I have hitherto diſcourſed in this treatiſe, is accommodated but to the ſcant information I have hitherto received; and therefore ought to be rectified, or confirmed, by farther informations, if they can be procured.

IN the mean time, I think I may probably enough gather from the paſſed diſcourſe, that though in ſome mines, three ſubterraneal regions, and their diſtinguiſhing attributes, may be not inconveniently aſſigned; yet generally ſpeaking of the whole body of the terreſtrial globe, as far as we know it, both the bounds and the temperature of the regions of the earth, as well as thoſe of the air, are various and uncertain enough.

† Sed aër utroque tempore, anteaquam curſum ſuum illum conſuetum conſtanter teneat plerumque, quatuor decem dierum ſpatio crebas habet mutationes, modo in altiorem puteum vel cuniculum influens, modo in humiliorem.

AND

AND much leſs have we any certain knowledge of the temperature of the more inward, and, if I may ſo ſpeak, the more central parts of the earth; in which, whether there be not a continued ſolidity, or great tracts of fluid matter, and whether or no differing regions are to be diſtinguiſhed, and what their number, order, thickneſs, and qualifications may be, we are as yet ignorant, and ſhall, I fear, long continue ſo; for it is to be noted (with which obſervation I ſhall conclude) that what has been hitherto diſcourſed belongs only to the temper of thoſe ſubterraneal parts, to which men have been enabled to reach by digging. It is true, indeed, that ſome mines, eſpecially in *Germany* and *Hungary*, are of a ſtupendous depth, in compariſon of the generality of ours, and of the more obvious cavities of the earth; yet I find it boaſted in a diſcourſe, written purpoſely of the various mines in the world, that the rich mine at *Sueberg* is 400 yards deep: and they are ſcarce believed, that relate one Hungarian mine, which they viſited to be 400 fathom; which, though double the depth of the former, reaches not to half a mile. But the deepeſt of all the mines that I have as yet read or heard of from any credible relator, is that which the experienced *Agricola*, in the tract he calls *Bermannus*, cap. 12. mentions to be at *Cotteberg*. But this itſelf, though it reach to above 500 fathom, that is, 3000 feet, yet this prodigious depth does not much exceed half a mile, and falls ſhort of three quarters *; and how ſmall a part is that of the whole depth of the terreſtrial globe? whoſe ſemi diameter, if we admit the recent account of the learned *Gaſſendus*, is reckoned at 4177 Italian miles; in compariſon of which, as I was ſaying, how ſmall a thing is a depth, that falls very ſhort of a ſingle mile?

* Licet variæ de ambitu terræ opiniones ſint, nobis tamen propemodum conſtet, eſſet ipſam milliarium Italicorum 26255, quod in maximo ad terræ ſuperficiem circulo reſpondeant uni gradui milliaria proximè 73. &c. Gaſſend. Inſtit. Aſtronom. lib. 2, cap. 13.

OF

OF THE TEMPERATURE OF THE SUBMARINE REGIONS, As to HEAT and COLD.

CHAP. I.

THOUGH the Ariſtotelians, who believe water and air to be reciprocally tranſmutable, do thereby fancy an affinity between them, that I am not yet convinced of; yet I readily allow of ſo much affinity betwixt thoſe two fluid bodies, as invites me (after having treated of the temperature of the aerial regions) to ſay ſomething of that of the ſubmarine regions; which name of ſubmarine, though I know it may ſeem improper, I therefore ſcruple not to make uſe of, becauſe even among the generality of learned men, uſe has authorized the name of ſubterraneous places; for as theſe are not by this name, and indeed cannot in reaſon be ſuppoſed to be beneath the whole body of the earth, but only the ſuperficial parts of it; ſo by the appellation of ſubmarine regions it is not to be ſuppoſed that the places ſo called are below the bottom of the ſea, but only below the ſurface of it.

But to come from words to things, I preſume it will not be expected, that I, that never pretended to be a diver, ſhould give of the regions I am to treat of, an account built on my own obſervations; and I hope it may gratify a reaſonable curioſity about a ſubject, of which claſſick authors are ſo very ſilent, and about which philoſophers ſeem not ſo much as to have attempted any experiments (for want of opportunities and means to make them). I offer the beſt information I could ſupply myſelf with, by purpoſely converſing with perſons that have dived, ſome without, and ſome by the help of engines. To which I have added ſome reports that I judge fit to be allowed, made me by perſons that had converſed with the divers upon thoſe African and Indian coaſts, where the moſt famous and expert are thought to be found.

And I the rather report the anſwers and relations my enquiries procured, becauſe the informations they give us concern a ſubject conſiderable as well as vaſt, about which nevertheleſs I among many others am not in a condition to ſatisfy at all my curioſity by trials of my own making; and becauſe alſo, what I ſhall ſay will probably ſpoil the credit of the vulgar error, that in all deep water, of which the ſea is the chiefeſt, the lower-

lowermoſt are ſtill the warmeſt parts, unleſs in caſe that in ſome very hot climates, or ſeaſons, the ſuperficial ones happen to be a little warmed by the extraordinary or violent heat of the ſun.

CHAP. II.

THOUGH the air and the earth have been diſcriminated as to temperature, into three regions; yet the informations I have hitherto met with, invite me to aſſign to the ſea no more than two. The former of which may be ſuppoſed to reach from the ſuperficies of it, as far downwards, as the manifeſt operation of the variouſly reflected and refracted beams of the ſun, or other cauſes of warmth penetrate; from which to the bottom of the ſea, the other region may be ſuppoſed to extend.

ACCORDING to this diviſion, the limits of this upper region will not be always conſtant; for in the torrid zone, and other hotter climates, it will, *cæteris paribus*, be greater than in the frigid zone or in the temperate zones; and ſo it will be in ſummer than in winter; and in hot weather than in cold; ſuppoſing in theſe caſes the heat to come from the ſun and air, and not, as ſometimes it may do, from the ſubterraneal exhalations.

THE ſame cauſes are likewiſe proper, as it is manifeſt, to alter the temperature, as well as the bounds of this region; but this temperature may alſo be changed, in ſome few places, by at leaſt two other cauſes; the one is the differing conſtitution of the ſoil that compoſes the ſhore, which may affect the neighbouring water, if it do extraordinarily abound with nitre, looſely contexed marchaſites, or other ſubſtances capable conſiderably to encreaſe or leſſen the coldneſs of the water. Another, though unfrequent cauſe, may be the figure and ſituation of the leſs deep parts of the ſhore, which may in ſome ſort reverberate the heat that proceeds from the ſun; and upon ſuch an account may either add to the warmth, or allay the coldneſs, that would elſe be found in the neighbouring water. For whatever the ſchools are wont to teach about the intereſt of the attrition of air in the heat produced by the ſun beams, I have elſewhere ſhewn by experiments, that thoſe beams may conſiderably operate upon bodies placed quite under water.

BESIDES theſe two caſes, that may occaſion exceptions to the general obſervation; I intimated by the words, *at leaſt*, that there might be others; becauſe to mention now but one example, though it ſeem probable from what I have elſewhere delivered concerning the ſubterraneal fires and heats, that may in ſome places be met with, even beneath the bottom of the ſea, that the phænomenon I am going to recite may be reduced to the cauſes newly intimated; yet I am not abſolutely certain, but that in this caſe, whereto ſome others may perhaps be found reſembling, ſome other cauſe than thoſe hitherto mentioned may produce or concur to the effect. The relation here meant is afforded us by the following paſſage, taken out of the voyage of Monſieur *de Monts*, into *New France* (whereof he went to be governor) where the relator thus recites his obſervation: about the eighteenth day of *June* we found the ſea-water during three days ſpace very warm, and by the ſame warmth our wine alſo was warm in the bottom of our ſhip; yet the air was no hotter than before. And the 21ſt of the ſaid month, quite contrary, we were two or three days ſo much compaſſed with miſts and cold, that we thought ourſelves to be in the month of *January*, and the water of the ſea was extreme cold; which continued with us, until we came upon the bank, by reaſon of the ſaid miſts, which outwardly did procure this cold unto us. This effect he attributes to a kind a kind of antiperiſtaſis in the following part of his narrative; which I ſhall not now either tranſcribe or examine.

CHAP. III.

And thus much being briefly noted touching the upper region of the ſea, and the requiſite cautions (that may perhaps extend further than it) being premiſed, it remains, that I take notice of the temperature of the lower region, which, in one word, is cold; unleſs in ſome few places to be preſently mentioned. For water being in its natural or moſt ordinary ſtate a liquor whoſe parts are more ſlowly agitated than thoſe of men's organs of feeling, muſt be upon that account cold as to ſenſe; and conſequently it need not be ſtrange, that thoſe parts of the ſea which are too remote to be ſenſibly agitated by the ſun-beams, or wrought upon by the warmth which the air and upper parts of the earth may from other cauſes receive, ſhould be felt cold by thoſe that deſcend into it; unleſs in thoſe few places where the coldneſs may be either expelled or allayed by hot ſprings, or ſubterreſtial exhalations, flowing or aſcending from the ſubjacent earth, or the lower parts of the ſhore, into the incumbent or adjacent parts of the water.

To juſtify my aſcribing of this coldneſs to the ſecond, or lower region of the ſea, I ſhall now ſubjoin ſome relations I procured from perſons that had occaſion to go down into it, or otherwiſe take notice of its temperature in very differing regions of the world, and at very unequal depths.

And firſt as to the temperature of the lower region in the northern ſea, I had the opportunity to converſe often, and ſometimes to oblige a man bold and curious enough, who for ſome years got the beſt part of his ſubſiſtence by deſcending to the bottom of the ſea in an engine (whoſe ſtructure I elſewhere deſcribe) to ſeek for and recover goods loſt in ſhip-wrecked veſſels. This perſon I diligently examined about divers ſubmarine phænomena, about which his anſwers may be elſewhere met with; and as to the temperature of the lower parts of the ſea) the knowledge of which is that alone that concerns us in this place) he ſeveral times complained to me of the coldneſs of the deep water, which kept him from being able to ſtay in it ſo long as he might have been put into a condition of doing by the goodneſs of his engine; for I remember that he related to me, that he ſtaid once betwixt an hour or two, at a depth that was no greater than 14 feet and a half upon the coaſt of *Sweden*, in a place that was near the ſhore; and I afterwards learned that he ſtaid much longer in a deeper place (uſe having probably made the cold more ſupportable to him). He told me then, that about two years before, he was engaged by a good reward to go down with his engine to the bottom of the ſea to fetch up ſome goods of value out of a ſhip that had been caſt away there within about a mile's diſtance from a very little iſland, and, if I miſtake not, abcut ſix miles from the ſhore. He further anſwered me, that though he felt it not at all cold on the ſurface of the water (his attempt being made in *June*) yet about the depth of the ſhip, it was ſo very cold, that he felt it not ſo cold in *England*'s winter and froſty weather. And he told me, that an exceſſive cold was there felt, not only by him, but by very ſturdy men, who, invited by his example, would needs alſo go down themſelves to participate and promote the hoped-for diſcovery. He told me alſo, that the upper water did but cool and refreſh him; but the deeper he went, the colder he felt it, which is the more conſiderable, becauſe he had ſometimes occaſion to ſtay at 10 fathoms or even 80 feet under water. And I ſince found, that he informed divers virtuoſi that purpoſely conſulted him, that he found the coldneſs of the water encreaſe with its depth; and gave that for the

the reaſon why he could not ſtay ſo many hours as otherwiſe he might, at the bottom of the ſea; adding, that before his engine was well fitted, he was once ſo covered over with it, that he was forced to touch the ground with his hands and feet, and the neighbouring parts, to which he found a coldneſs communicated by the fundus he leaned upon; though the cloſeneſs of his diſordered engine made the other, and (whilſt he was in that poſture) upper parts of his body, of a very differing temper.

An inquiſitive perſon of my acquaintance, that made a long ſtay in the *Northern America* (at about two or three and forty degrees of latitude) and diverted himſelf often with ſwimming under water, anſwered me, that though he ſcarce remembered himſelf to have dived above two fathoms beneath the ſurface of the ſea, yet even at that ſmall depth, he obſerved the water to encreaſe in coldneſs, the lower he deſcended into it; which argues, that though the ſun-beams do often penetrate plentifully enough to carry light to a great depth under water, yet they do not always carry with them a ſenſible heat; and that, at leaſt, in ſome places, the upper region of the ſea reaches but a little way.

The coldneſs of the climate in theſe weſtern parts of *Europe*, and the want of conſiderable inducements to invite men to dive often to any great depth into our ſeas, has kept me from being able to procure many obſervations about the temperature of their lower region; but upon the hotter coaſts of *Africk* and the *Eaſt Indies*, the frequent invitations men have to dive for coral, pearls, and other ſubmarine productions, have made it poſſible for me to get more numerous obſervations; ſome of which I ſhall now annex.

C H A P. IV.

Meeting with a perſon of quality who had been preſent at the fiſhing of coral upon the ſhore of *Africa*, and who was himſelf practiſed in diving, I inquired of him, whether he found the ſea upon the African coaſt to be much colder at a good depth, than nearer the ſurface; whereto he anſwered me, that though he had ſeldom dived above three of four fathoms deep, yet, at that depth, he found it ſo much colder than nearer the top of the water, that he could not well endure the coldneſs of it.

And when I farther aſked him, whether, when he was let down to the bottom of the ſea, in a great diving bell (as he told me he had been) he felt it very cold, though the water could not come immediately to touch him; he replied, that when the bell came firſt to the ground, he found the air in it very cold, though after he had ſtaid awhile there, his breath and the ſteams of his body made him very hot.

That alſo at a greater depth in thoſe hotter climates, the ſea-water is ſenſibly cold, may be thus made probable: inquiring of a famous ſea-commander, who had been upon the African coaſt, to what depth he was wont to ſink his bottles to preſerve his wine any thing cool in that exceſſive hot climate, he anſwered me, that in the day time he kept it in a tolerable temper ſo as to be drinkable, by keeping it in the bottom of the ſhip, and in ſand; but in the morning he had it cool enough by ſinking his bottles over night into the ſea, and letting them hang all night at 20 or 30 fathom deep under water.

Inquiring alſo of an intelligent gentleman that was imployed to the river of *Gambra*, and ſailed up 700 miles in it, in a ſmall frigate, whether he had obſerved that in the ſea, even of thoſe hot climates, wine may be preſerved cool; he told me, that it might, and, that by the means I hinted to him, which was to let down, when the ſhip came to an anchor in the evening, ſeveral bottles full of wine (they uſed that of *Madeira*) exactly ſtopped to ten, twelve, or fourteen fathoms deep; whence being the next morning drawn up, they found the wine cool and freſh (as if the veſſels had been in theſe parts drawn up out of a well) provided it were preſently drank, for if that circumſtance were omitted, the heat of the air on the upper part of the water would quickly warm the liquor.

I REMEMBER too, that having met with a man of letters, that ſailed to the *Eaſt-Indies* in a Portugal-caraet, I learned by inquiry of him, that it was the practice in that great veſſel for the captain and other perſons of note, whilſt they paſs through the torrid zone, to keep their drink, whether wine or water, cool, by letting it down in bottles to the depth of 80, 90, and ſometimes 100 fathom or better, and letting it ſtay there a competent time; after which, he told me, he found it to be exceeding cool and refreſhing.

LASTLY, to ſatisfy myſelf as far as I could, to how great a depth the coldneſs of the ſea reached; meeting an obſerving traveller, whoſe affairs or curioſity had carried him to divers parts, both *Eaſt* and *Weſt Indies*, I inquired of him, whether he had taken notice of any extraordinary deep ſoundings in the vaſter ſeas, to which being anſwered, that ſome years ago, ſailing to the *Eaſt Indies*, in a very great ſhip, over a place on the other ſide the line, that was ſuſpected to be very deep, they had the curioſity to let down 400 fathom of line, and found they needed no leſs. Whereupon I inquired of him, whether he had taken notice of the temperature of the ſounding lead as ſoon as it was drawn up; to which he told me, that he and ſome others did; and that the lead, which was of the weight of about 30, or 35 lb. had received ſo intenſe a degree of coldneſs, as was very remarkable; inſomuch, that he thought that if it had been a maſs of ice, it could not have more vehemently refrigerated his hands; and when I aſked in what climate this obſervation was made, he told me it was in the antarctick hemiſphere, but at a great diſtance from the line. As indeed I concluded by ſome circumſtances he mentioned to me, that it was about the 35th degree of ſouthern latitude.

CHAP. V.

THESE are the chief relations I have hitherto been able to procure about the temperature of the ſea; which, if they be ſo confirmed by others, as that we may conclude they will generally hold, it will not be irrational to conceive, that in reference to temperature, thoſe two fluids, air and water, may have this in common, that where their ſurfaces are contiguous, and in the neighbouring parts, they happen to be ſometimes cold, ſometimes hot, as the particles they conſiſt of chance to be more or leſs agitated by the variouſly reflected ſun-beams, or more or leſs affected by other cauſes of heat. But that part of the air which they call the ſecond, and is ſuperior to the firſt, as alſo the lower region of the ſea, being more remote from the operation of thoſe cauſes, do retain their natural, or more undiſturbed temperature, which, as to us men, is a conſiderable degree of coldneſs, the agitation of their ſmall parts being uſually in thoſe regions much inferior to that of the ſpirits, blood, and other parts of our organs of feeling; ſo that the regions of the water and air ſeem to anſwer one another, but in an inverted order of ſituation; and the analogy might perhaps be carried further, if I had time and opportunity to do it in this place. And here I ſhall not diſſemble, that I was ſomewhat perplexed by meeting with a traveller that had viſited the Eaſt-Indian coaſt, near the famous Cape of *Comory*; for aſking him ſome queſtions touching the neighbouring ſea, I gathered from his diſcourſe, that he concluded from that of ſome divers, that the ſea near

near *Ceylon* was warmer at the bottom than at the top; and when I thereupon aſked him whether this happened not in their winter, he replied, that it was indeed winter, though not with us, yet with them. It occurred indeed to my thoughts on this occaſion, that perhaps in a part of the torrid zone ſo near the line as about 8 degrees, if the ſea were not of a conſiderable depth, the heat of the two not far diſtant ſhores of *Coromandel* and *Ceylon* might have no ſmall influence upon the temperature of the water. I conſidered alſo, which did not a little weigh with me, that in divers parts of the *Eaſt-Indies*, and even in a region bordering upon *Coromandel*, where an ingenious acquaintance of mine lived ſome years, it has been obſerved, that winter and ſummer are not ſo much diſcriminated by cold weather and hot, as by very rainy weather and very dry: nay, in ſome places the ſultry heat of the climate is more complained of in what they call their winter than their ſummer; ſo that there will be no neceſſity to recur to an antiperiſtaſis occaſioned by the coldneſs of the winter. I thought too, that it may perhaps be without abſurdity ſuſpected, that as the bottom of the ſea in this place had a peculiar conſtitution, that fitted it more than others for the copious production of pearls; ſo there might be ſome peculiarity in the nature of the ſubjacent ſoil, or there may be ſome ſubterraneal fire or heat beneath it, which may occaſion an unuſual warmth in that part of the ſea, by which cheriſhing warmth, perhaps, ſuch abundance of ſhell-fiſhes teeming with pearls may be invited to ſettle there, rather than in any of the neighbouring places. But with all theſe conjectures I ſhould not have been ſo well ſatisfied, as with the anſwer I afterwards obtained by a gentleman, whoſe curioſity had carried him to be an aſſiduous ſpectator of the famous pearl-fiſhing, near the iſland of *Manar*, between that and the coaſt of *Coromandel*, which reaches near, if not fully to the Cape of *Comory*. For this perſon having had much converſation with the divers for pearls, not only learned from them, that they found the water very ſenſibly cold at the bottom, which in ſome places he eſtimated to be 80 or 100 fathom deep; but obſerved divers of them at their return to the boats, to be ready to ſhake with cold, and haſten to the fires that were kept ready for them in little cabbins upon the ſhore; which relation being accompanied with divers circumſtances of credibility, and arguing, the perſon that made it to have been acquainted with the report above-mentioned, and had met with ſome that had dived in the place whereto it had relation, made me conclude, that as to that report, ſomething extraordinary had happened in that place; or that there was ſome miſtake of him to whom it was made; or that divers did not deſcend to a ſufficiently conſiderable depth.

If I had been furniſhed with opportunity, I would have engaged ſome ingenious navigators to examine the temperature of the ſubmarine regions, both of differing ſeaſons of the year, eſpecially the hotteſt part of ſummer, and coldneſs of winter, and with hermetically ſealed weather-glaſſes, in order to the diſcovery of ſuch particulars as theſe, whether there be in ſome ſeas any ſuch varying differences of temperature, as may invite us at leaſt in ſome places, to make more than two ſubmarine regions: whether the ſubmarine coldneſs do at the bottom of the ſea, or elſewhere, either equal or ſurpaſs that degree, which we here find ſufficient to freeze common water: whether the parts of the ſea-water are ſtill the colder, as they are the deeper; and whether or no this increaſe of coldneſs be regular enough to be reducible to any ſettled proportion. But for the reſolving of theſe and the like queſtions, I did not cauſeleſly intimate, that a ſealed weather-glaſs was to be employed; for I take a common one to be altogether unfit for ſuch purpoſes, not only becauſe the ſea-water would mingle with ſuch liquors as are wont to be employed in it, for that inconveniency I could eaſily remedy, by ſubſtituting,

 as

as I have feveral times done in other cafes, mercury inftead of ordinary liquors; but chiefly becaufe the incumbent fea-water would gravitate upon the reftagnant liquor of the weather-glafs, and thereby render its informations falfe or uncertain; according to what I have had occafion to obferve in another tract.

Whereto, that there may not in this place be any need to recur, I fhall add a flight experiment that I made for the fatisfaction of fome ingenious men not well acquainted with hydroftaticks, or not rightly principled in them: and this trial I fhall the rather mention, becaufe many will not allow water to prefs upon mercury immerfed therein, this being a far more ponderous liquor than that; and others will expect that the included air, having no place to efcape out at, fhould refift the afcenfion of the subjacent mercury, more than indeed it will. We made then a fmall weather-glafs differing from common ones, befides the bignefs, in that it was furnifhed with mercury inftead of water; and in that we employed to contain the ftagnant mercury a glafs phial with a narrow neck, wherein, by a piece of cork or two, the ftem of the glafs ball was well faftened, that this globular part of the inftrument might not be lifted up when it was under water. Then having by applying cold water to the outfide of the ball endeavoured to reduce the air to the fame temper with the water, or at leaft to an approaching degree of coldnefs; and having taken notice of the ftation of the mercury in the fhank or ftem above-mentioned, we did, by ftrings tied about the neck of the fmall phial, let the inftrument gently down into a large tall glafs body, filled with fair water, that the liquor and veffel being both tranfparent, we might eafily perceive the motions of the mercury in the flender pipe; by which means it appeared, that as the thermometer defcended deeper and deeper into the water, the mercury was preffed up higher and higher in the ftem; and that it may not be fufpected that this afcenfion proceeded only or chiefly from the refrigeration of the air by the water, I fhall add to what I have juft now noted, that though the coldnefs of the water may well be fuppofed uniform, as at leaft to fenfe, yet the whole inftrument being leifurely removed fometimes to the upper furface of the water, fometimes to the lower, the rifing and falling of the quickfilver in the flender pipe was fuitable to the depth of its furface, or its diftance beneath that of the water. (The like experiment we might have tried with a thermofcope furnifhed with water, and let into oil, or with deliquated falt of tartar and pure fpirit of wine, inftead of mercury and water; if we had been furnifhed with fufficient quantities of thofe liquors, and had judged it to be requifite.) But this circumftance I thought fit to admonifh the fpectators of, that it is not to be expected that the mercury fhould rife as much in proportion when it is (for example) a foot under water, as when it is but two or three inches; becaufe, according as the inftrument is let down deeper, and the air crouded into a lefs room, the fpring of that compreffed air becomes the ftronger, and makes the more refiftance; which advertifement agreed well with the experiment, whofe other phænomena I pafs over as not pertinent to this place, where I would only juftify what I faid of the unfitnefs of weather-glaffes made (though with other liquors) after the common ways for making the fubmarine trials I propofed.

But till fuch artificial obfervations can be obtained, we may from what has been above delivered probably gather, that though the lowermoft of the fubmarine regions be very fenfibly cold, yet water, at leaft that of the fea, does not by thefe phænomena appear to be the *fummum frigidum*. Though I have been feveral times able to produce ice in falt-water, yet I find not by any obfervation, that there has been ice met with, and generated at the bottom of the fea, under which the earth has been found unfrozen by our divers; and appears to be foft at depths exceedingly furpaffing the greateft they have reached; as as is evident by the mud, gravel, &c. fetched from the bottom of the ſea by ſounding plummets, let down to 80 or 100 fathom, or even a greater depth, whereof examples may be met with in the journals of navigators: nay, my curioſity procured me this account, from the ſober commander of a ſhip that came this year from the remoter parts of the great ocean, that at about 35 degrees of ſouthern latitude the tallow with which his ſounding lead was anointed, brought him up grey ſand from the immenſe depth of no leſs than two hundred and twenty fathom. But to this obſervation it is juſt to annex this caution, that we cannot ſafely conclude from men's finding no ice at the bottom of the ſea, that the cold there cannot be very intenſe; for, as I have found by more than one relation (* elſewhere recited) that, whatever the ſchools ſurmiſe, the ſea is at leaſt as ſalt at the bottom, as at the top; ſo I have more than once tried, that ſalt water will, without freezing, admit a much greater degree of cold, than is neceſſary to turn freſh water into ice.

RELATIONS

ABOUT THE

BOTTOM of the SEA.

SECTION I.

I DO not pretend to have viſited the bottom of the ſea; but ſince none of the naturaliſts whoſe writings I have yet met with, have been there any more than I; and it is great rarity in thoſe cold parts of *Europe* to meet with any men at all, that have had at once the boldneſs, the occaſion, the opportunity, and the ſkill to penetrate into thoſe concealed and dangerous receſſes of nature, much leſs to make any ſtay there; I preſume it will not be unpleaſant, if about a ſubject, of which, though none of thoſe very few naturaliſts, that write any thing at all, write otherwiſe than by hear-ſay, I recite in this place what I have learned by enquiry from thoſe perſons, that among the many navigators and travellers I have had opportunity to converſe with, were the likelieſt to give me good information about theſe matters.

It would be needleſs here to take notice that the ſea is uſually cold and ſalt at the bottom; nor to repeat thoſe other things that I have already delivered in other diſcourſes; I ſhall therefore begin what I have to ſay in this, by relating that one of the chief

* Notes about the ſaltneſs of the ſea.

things

things that I was folicitous to enquire after about the bottom of the fea, was, the inequality I fuppofed to be in the foil; for though the furface of the fea, when it is not agitated by the winds, appears very plain and level, and though it be indeed, at leaft in this, or that particular fea, fphærical and (phyfically fpeaking) concentrical to the earth; yet I could not think it probable, for reafons not neceffary to be here difcourfed of, that the bottom, the fuperficies of the ground, or of the veffel that contained it, fhould be either flat or level, or regularly concave.

To fatisfy myfelf about this matter, I enquired of a perfon that had vifited the famous pearl-fifhing at the little ifland of *Manar* (near the rich ifle of *Ceylon*) in the *Eaft-Indies*, and had by his ftay there much opportunity to fee divers at their work, and converfe with them. By the anfwers of this man, who was a fcholar, I learned that the divers had affured him, that they found the floor of the fea, if I may fo call it, in divers places, exceedingly unequal, in fome places being flat, in others afperated with crabby rocks a confiderable height, and elfewhere finking into precipitous depths, in which they found it very cold.

BESIDES the recited teftimonies of the divers, I enquired of feveral pilots and other navigators that had made long voyages, what gradual or abrupt inequality they had obferved at their foundings in very neighbouring places; it being eafy to be gathered from thence, whether the fea were there uniformly deep, or did at leaft, with fome regularity, alter its depth by degrees; or whether, as I fufpected, there were not at the bottom of the fea hilly places, and fteep precipices, and, perhaps, deep vallies or wells, as we obferve in the difcovered part of the terreftrial globe.

By thefe inquiries I obtained feveral obfervations, whereof the moft material are thofe that follow:

FIRST, an ancient fea-commander, that had many years frequented *Africa* and the *Indies*, told me, as others had done before, that when they failed in the ocean very far from fight of land, they did not often put themfelves to the trouble of founding; but that as far as they had founded, he had ufually found the depth of the fea to increafe or decreafe gradually, without very great irregularities, excepting fome places, inftancing particularly in the excavation that makes the bottom of the fea, within fight of the *Cape of Good Hope*, where though for the moft part, he found the water to deepen more and more, as he failed farther from fhore; yet in one place, he and others had met with a bank (as he conceived it to be) at a confiderable diftance from the furface of the water; fo that though when they were as they imagined near the edge of that bank, they found but a moderate number of fathoms, yet when failing a very little way farther they had gone beyond it, they found the fea of an immenfe depth. In fhort I gathered from his anfwers, that in the greater feas he had found, for the moft part, the ground at the bottom to fall away by degrees; but nearer the fhores, that is, within a moderate number of leagues, he obferved in divers places, that the fubmarine ground was very unequal, and had as it were, hills and precipices.

A MAN of letters that had failed both to the *Eaft* and *Weft Indies*, and in divers other regions befides, and had made fome of his voyages in fhips of fuch great burthen, as obliged the mariners to be very frequent and careful in founding, informed me, that fometimes at confiderable diftances from fhore, he had obferved the fea to be 20, 30, or perhaps 40 fathom deeper when they caft the founding lead from one fide of the fhip, than it had been juft before, when they had founded from the other; and from other things that he told me, I found myfelf much confirmed in the above propofed opinion.

HEARING of a fea-captain of extraordinary fkill in maritime affairs, that was come home this year from *Eaft India*, his reputation made me endeavour to have a little conference

ference with him about the ſubject of this diſcourſe ; but his occaſions haſtening him to another place before I could ſend to him, I procured from the chief perſons that employed him a ſight of ſome notes touching his laſt voyage, which he had left with them, hoping to find there ſomething at leaſt about the ſoundings of ſo accurate a ſeaman ; and accordingly I met with a paſſage very pertinent to my purpoſe, and worthy to be here tranſcribed.

FEBRUARY 12. After our obſervation, (he means a former one very agreeable to this) ſeeing the ground under us, we heaved the lead, and had but 19 fathom rocky ground, then haled by N. N. E. the wind at N. W. and found our water to ſhoal from 19 to 10 and 8 fathom hard coral ground, then ſuddenly deepened again from 8 to 20 and 22 fathom ſandy ground, and then ſuddenly ſaw rocks under us, where we had but 7 fathom, and the next caſt 14 fathom again: and ſo having run N. N. E. from 6 in the morning until 12 at noon about 19 mile, we deepened our water, from 16 to 25, and the next caſt, no ground with 35 fathom of line.

LASTLY, having opportunely met with an ancient navigator who paſſes for the moſt experienced pilot in our nation for an Eaſt-Indian voyage ; I aſked him about his own obſervations concerning theſe unequal ſoundings, I was anſwered, that he had not only met with them elſewhere, but, that not far from the mouth of our channel, he had ſometimes found the bottom of the ſea ſo abrupt, that in ſailing twice the length of the ſhip, he had found the water deepen from 30 fathom to a hundred, if not alſo much more.

SINCE I received theſe relations, having the honour to diſcourſe with a noble perſon who has divers times deſervedly had the command of Engliſh fleets, and is no leſs curious than intelligent in maritime affairs, I took the opportunity to inquire of his lordſhip whether he had not obſerved the bottom of the ſea to be very unequal in neighbouring places ? To which he replied, that he had found it exceedingly ſo. And to ſatisfy me that he ſpoke not upon mere conjecture, he told me, that ſailing once with his fleet, even in our channel, he perceived the water to make a ripling noiſe (as the ſeamen call it) as the *Thames* does under *London Bridge*, ſo that he was afraid they were falling upon ſome ſhoal, the water being 12 or 14 fathom deep, and going on a little farther, he caſt out the plummer again, and found it about 30 fathom. He added, that he made divers ſuch obſervations, but took notice of ſuch ripling waters only when the tide was ebbing ; and yet in a deep ſea meeting with the like appearance in the upper part of the water, and thinking it improbable that there ſhould be any ſhoal there, he ordered the depth to be ſounded, and found it to exceed 30 fathoms ; and after he had paſſed on a very little farther, he found the ſea ſo deep, that he could not fathom it with his ordinary line.

SECTION II.

ANOTHER thing obſerved at the bottom of the ſea is the great preſſure of the water there againſt any other bodies. For whatever men may philoſophize in their ſtudies, and may conclude from the principles that are generally received about the non-gravitation of water in its proper place, yet experience ſeems very little to favour that general doctrine.

FOR firſt, I remember, that having cauſed a pretty large cylinder of glaſs that was open only at one end, to be ſo depreſſed into a large glaſs-veſſel full of water, with a conveniently applied weight of lead, that none of the air could get out, I could eaſily diſcern difcern through the liquor and veffels, which were all tranfparent, that as the inverted cylinder defcended deeper and deeper, the external water compreffed the imprifoned air, and afcended higher and higher in the cavity of the cylinder, againft whofe fide we had beforehand placed a row of marks whereby to take notice of the gradual afcent and defcent of the internal water.

Secondly, having inquired of two feveral obferving perfons, whereof one had with a diving engine vifited the bottom of the fea in a cold northern region; and the other had done the like in an engine much of the fame fort upon the coaft of Africk; I found their relations to agree in this, that the deeper they defcended into the fea, the more the air they carried down with them was compreffed, and the higher the water afcended above the lip or brim of the engine into the cavity of it.

But I fhall now add a more confiderable experiment or two to the fame purpofe. For difcourfing one day with an engineer of my acquaintance that had been often at fea, and loved to try conclufions, of a way I had thought of, to make fome eftimate of the preffure of the water at a confiderable depth beneath the furface, and fhew that the preffure is great there; he told me, he could fave me the labour of fome trials by thofe he had made already, and affured me, that having divers times opportunity to fail near the ftreights mouth over a place where the fea was obferved to be of a notable depth, he had found, that if he had let down, with a weight into the fea, not a ftrong round glafs bottle, but a phial, fuch as the feamen ufe to carry their brandy and ftrong waters in; fuch a veffel, which might contain a pint or quart of water, would, when it come to be funk 40 fathom under water, if not fooner, be fo oppreffed by the preffure of the incumbent and lateral water, as to be thereby broken to pieces.

He alfo averred to me, that having exactly clofed an æolipile of metal, and with a competent weight funk it to a great depth in the fea, as to forty, fifty, or fixty fathom deep, when he pulled it up again he found to his wonder that the great preffure of the water had in divers places crufhed it inwards. And though I had fome fufpicion that the coldnefs of the fea at fuch a depth might, by weakening the fpring of the included air, fomething contribute to the effect, yet I did not admire the event, having divers years before had a thin æolipile of copper crufhed inwards by the preffure of a much lighter fluid than fea-water.

SECTION III.

Another thing obferved in the bottom of the fea is the tranquillity of the water there, if it be confiderably diftant from the furface. For though the winds have power to produce vaft waves in that upper part of the fea that is expofed to their violence; yet the vehement agitation diminifhes by degrees as the parts of the fea, by being deeper and deeper, lie more and more remote from the fuperficies of the water. So that the calm being lefs and lefs difturbed towards the bottom of the water, if that lie confiderably deep, the water is there either calm or fcarce fenfibly difturbed.

But that is for the moft part to be underftood of places at fome diftance from the fhore; for oftentimes, in thofe that are too near it, the progrefs of the waters being rudely checked, and other circumftances concurring, the commotion of the water is fo great, that it reaches to the very bottom, as may appear by the heaps of fand, the amber, and in fome places the ftones that are wont to be thrown up by the fea in and after ftorms.

The above-mentioned calmneſs of the ſea at the bottom will (I doubt not) appear ſtrange to many, who admiring the force of ſtormy winds and the vaſtneſs of the waves they raiſe, do not, at the ſame time, conſider the almoſt incomparably greater quantity and weight of water that muſt be moved to make any great commotion at the bottom of the ſea, upon which ſo great a maſs of ſalt-water, which is heavier than freſh, is conſtantly incumbent. Wherefore, for the proof of the propoſed paradox, I will here ſet down a memorable relation, which my inquiries got me from the diver, elſewhere mentioned, who by the help of an engine could ſtay ſome hours under water.

This perſon then being aſked, whether he obſerved any operation of the winds at the bottom of the ſea, where it was of any conſiderable depth? anſwered me to this purpoſe, that the wind being ſtiff, ſo that the waves were manifeſtly ſix or ſeven feet high above the ſurface of the water, he found no ſign of it at 15 fathom deep; but if the blaſts continued long, then it moved the mud at the bottom, and made the water thick and dark. And I remember he told me, which was the circumſtance I chiefly deſigned, that ſtaying once at the bottom of the ſea very long, where it was conſiderably deep, he was amazed at his return to the upper parts of the water to find a ſtorm there, which he dreamt not of, and which was raiſed in his abſence, having taken no notice of it below, and having left the ſea calm enough when he deſcended into it.

For farther confirmation, I ſhall add, that having inquired of a great traveller who had aſſiſted at a rich pearl-fiſhing in the *Eaſt-Indies*, whether he had not learned by his converſation with the divers, that ſtorms reach not to the bottom of the ſea, if it be of any conſiderable depth; he anſwered, that he had ſeen the divers take the water when the ſea was ſo very rough, that ſcarce any veſſels would hazard themſelves out of ports; that thoſe returning divers told him, that at the bottom they had found no diſturbance of the water at all; which is the more conſiderable, becauſe of the ſituation of that place where they dive for pearls; for this is near the ſhore of *Manar*, and that itſelf is ſeated between the great iſland of *Ceylon* and the vaſt cape of *Comori*; and though it may be much nearer the former, is not yet far diſtant from the latter; which ſituation and the neighbourhood of the vaſt Indian ocean, on the one ſide of *Ceylon* and the great gulph of *Bengala* (antiently *Sinus Gangeticus*) on the other, makes the place where the pearls are fiſhed for exceeding likely to be ſubject to very troubled ſeas.

It will perhaps be thought no ſlight addition to the foregoing arguments, if I here add, that meeting one day with an ancient and expert ſeaman, whom his merit had advanced to conſiderable employments in his profeſſion, I was confirmed by the enquiries I made of him, not only in the opinion I had about the calmneſs of the bottom of the ſea, but alſo, that the operation of good gales of wind does oftentimes not reach to near ſo conſiderable depths into the ſea, as hath been hitherto ſuppoſed, even by navigators themſelves. For he aſſured me, that having ſometimes ſailed in great ſhips that drew much water, as about 12 or 15 feet, he had dived to the keel of the ſhips when they were under ſail, and obſerved the agitations of the water to be exceedingly diminiſhed and grown very languid even at that ſmall diſtance from the upper part of the waves; and he farther anſwered, that when in *America* he learned to dive of the Indians, they taught him by their examples, to creep along by the rocks and great ſtones that lay near the ſhore, at the bottom of the water, to ſhelter themſelves from the ſtrokes and other ill effects of the billows, which near the ſhore, and where the ſea was ſo ſhallow as it was there, did often hurt and endanger ſwimmers and unſkilful divers. But when they were, by this means, got farther from ſhore and into deeper water, they would ſecurely leave the ſhelter they had till then made uſe of, and ſwim within a few yards of the ſurface of the ſea and commotions of the upper parts of the water.

But laſtly, for further ſatisfaction, I had the opportunity to make inquiry about this matter of a great ſea-commander, who has both an extraordinary curioſity to make marine obſervations and an unuſual care in making of them accurately, I found the opinion countenanced by his anſwer, which was in ſhort, that he had lately been at a place where the ſea was often tempeſtuous enough, and that they found by a ſure mark, that the ſtorm did not reach with any efficacy four fathom beneath the ſurface of the water.

ABOUT the tranquillity of the lower parts of very deep waters I had a ſuſpicion which, though I fear it might ſeem ſomewhat extravagant, becauſe I have not met with it in authors; yet I thought it worth examining for the uſe it might be of, if reſolved, in reference to the ebbing and flowing of the ſea.

I MADE, therefore, a ſolicitous inquiry whether the tides did reach to, or near the bottom of the deeper ſeas, but found it exceeding difficult, by reaſon of men's want of curioſity, to obtain any ſatisfaction about a problem that moſt navigators I have converſed with did not ſeem to have ſo much as dreamed of. But thus much I found, indeed, by inquiring of an engineer, who was curious of marine obſervations, that a famous ſea commander of his acquaintance, being alſo a great mathematician, had affirmed to this relator, that he had divers times obſerved, that when he let down his plummet to a great depth, but yet not to reach ground, it would be quickly carried by a motion quite contrary to that of the ſhallop, whence they ſounded, and very much quicker than it; but I had this only at ſecond hand. Alſo, if I miſ-remember not, I was informed by a ſkilful obſerver, that commanded many of our Engliſh men of war, that he had, near the Sound, obſerved the upper and lower parts of the water to move with a conſiderable ſwiftneſs quite different ways; but not having committed this relation to writing, I dare not build much upon it. And among the anſwers I had received and written down concerning thoſe matters, all that I can yet find among my adverſaria is a relation which, though ſingle, will not be unworthy to be tranſcribed in this place, becauſe the perſon who gave it me is one of the ancienteſt and moſt experienced pilots of our nation.

THIS perſon, therefore, aſſured me, that ſailing beyond the *Cape of Good Hope*, into the *South Seas*, he made trials of the motion of the upper part of the water above the lower, where ſometimes caſting out a large and heavy plummet, he let it down to ſeveral depths ſhort of 50 fathom, without any ſenſible operation upon the motion of the boat or ſhallop he ſtood in to make the trial; but when he let down the plummet lower, to about an hundred fathom or more, then he found, that though the plummet reached not to the bottom of the water, yet upon the ſcore of the ſtanding water beneath, the ſuperior water would make the boat turn towards the tide or current, as if it lay at anchor, and the water would run by the ſide of the boat at the rate of about three miles an hour; thus far this diligent obſerver: but how far the inequality of the ſoil at the bottom of the ſea, and how far various depth of the water, and ſome other circumſtances may alter the caſe, and make it hard to determine what ought to be aſcribed to tides, and what to currents, are things which I will by no means be poſitive in, till I can meet with further information.

[SINCE the writing of this, happening to meet with one that ſpent ſome time at a famous eaſtern pearl-fiſhing, and aſked him, whether he had inquired of the divers about the problem lately propoſed, and whether the ſea were there deep enough to make obſervations of that kind; to the latter part of which queſtion he replied, that in ſome places it was of a very conſiderable depth, and fit to make the obſervation in; and to the former he anſwered, that he had inquired of the divers, who affirmed to him, that ſometimes at the bottom of the deep waters there ſeemed to be a ſtagnation of the ſea for a great depth, ſo that till ſuch a height they could riſe directly upwards, but that at other heights they would be carried away by the leſs deep waters; ſo as to be found, when they came to emerge, a great way off from that point of the ſurface, which was perpendicular to that place at the bottom, whence they began to aſcend.]

NEW

NEW

PNEUMATICAL EXPERIMENTS

ABOUT

RESPIRATION.

Printed firſt in the *Philoſophical Tranſactions*, N° 62, for *Auguſt* the 8th, 1670.

TITLE I.

Obſervations made about the laſting of ducks included in the exhauſting receiver.

NATURE having, as zoologiſts teach us, furniſhed ducks and other water-fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they have occaſion to dive, to forbear for a pretty while reſpiring under water without prejudice; I thought it worth the trial, whether ſuch birds would, much better than other animals, endure the abſence of the air in our exhauſted receiver. The accounts of which trials were, when they were made, regiſtered as follows:

EXPERIMENT I.

WE put a full grown duck (being not then able to procure a fitter) into a receiver, whereof ſhe filled, by our gueſs, a third part, or ſomewhat more, but was not able to ſtand in any eaſy poſture in it; then pumping out the air, though ſhe ſeemed at firſt (which yet I am not too confident of, upon a ſingle trial) to continue well ſomewhat longer than a hen in her condition would have done; yet within the ſhort ſpace of one minute ſhe appeared much diſcompoſed, and between that and the ſecond minute, her ſtruggling and convulſive motions increaſed ſo much, that her head alſo hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which we preſently reſcued obſervations and an unuſual care in making of them accurately, I found the opinion countenanced by his anſwer, which was in ſhort, that he had lately been at a place where the ſea was often tempeſtuous enough, and that they found by a ſure mark, that the ſtorm did not reach with any efficacy four fathom beneath the ſurface of the water.

About the tranquillity of the lower parts of very deep waters I had a ſuſpicion which, though I fear it might ſeem ſomewhat extravagant, becauſe I have not met with it in authors; yet I thought it worth examining for the uſe it might be of, if reſolved, in reference to the ebbing and flowing of the ſea.

I made, therefore, a ſolicitous inquiry whether the tides did reach to, or near the bottom of the deeper ſeas, but found it exceeding difficult, by reaſon of men's want of curioſity, to obtain any ſatisfaction about a problem that moſt navigators I have converſed with did not ſeem to have ſo much as dreamed of. But thus much I found, indeed, by inquiring of an engineer, who was curious of marine obſervations, that a famous ſea commander of his acquaintance, being alſo a great mathematician, had affirmed to this relator, that he had divers times obſerved, that when he let down his plummet to a great depth, but yet not to reach ground, it would be quickly carried by a motion quite contrary to that of the ſhallop, whence they ſounded, and very much quicker than it; but I had this only at ſecond hand. Alſo, if I miſ-remember not, I was informed by a ſkilful obſerver, that commanded many of our Engliſh men of war, that he had, near the Sound, obſerved the upper and lower parts of the water to move with a conſiderable ſwiftneſs quite different ways; but not having committed this relation to writing, I dare not build much upon it. And among the anſwers I had received and written down concerning thoſe matters, all that I can yet find among my adverſaria is a relation which, though ſingle, will not be unworthy to be tranſcribed in this place, becauſe the perſon who gave it me is one of the ancienteſt and moſt experienced pilots of our nation.

This perſon, therefore, aſſured me, that ſailing beyond the *Cape of Good Hope*, into the *South Seas*, he made trials of the motion of the upper part of the water above the lower, where ſometimes caſting out a large and heavy plummet, he let it down to ſeveral depths ſhort of 50 fathom, without any ſenſible operation upon the motion of the boat or ſhallop he ſtood in to make the trial; but when he let down the plummet lower, to about an hundred fathom or more, then he found, that though the plummet reached not to the bottom of the water, yet upon the ſcore of the ſtanding water beneath, the ſuperior water would make the boat turn towards the tide or current, as if it lay at anchor, and the water would run by the ſide of the boat at the rate of about three miles an hour; thus far this diligent obſerver: but how far the inequality of the ſoil at the bottom of the ſea, and how far various depth of the water, and ſome other circumſtances may alter the caſe, and make it hard to determine what ought to be aſcribed to tides, and what to currents, are things which I will by no means be poſitive in, till I can meet with further information.

[Since the writing of this, happening to meet with one that ſpent ſome time at a famous eaſtern pearl-fiſhing, and aſked him, whether he had inquired of the divers about the problem lately propoſed, and whether the ſea were there deep enough to make obſervations of that kind; to the latter part of which queſtion he replied, that in ſome places it was of a very conſiderable depth, and fit to make the obſervation in; and to the former he anſwered, that he had inquired of the divers, who affirmed to him, that ſometimes at the bottom of the deep waters there ſeemed to be a ſtagnation of the ſea for a great depth, ſo that till ſuch a height they could riſe directly upwards, but that at other heights they would be carried away by the leſs deep waters; ſo as to be found, when they came to emerge, a great way off from that point of the ſurface, which was perpendicular to that place at the bottom, whence they began to aſcend.]

NEW

NEW

PNEUMATICAL EXPERIMENTS

ABOUT

RESPIRATION.

Printed firſt in the *Philoſophical Tranſactions*, N° 62, for *Auguſt* the 8th, 1670.

TITLE I.

Obſervations made about the laſting of ducks included in the exhauſting receiver.

NATURE having, as zoologiſts teach us, furniſhed ducks and other water-fowl with a peculiar ſtructure of ſome veſſels about the heart, to enable them, when they have occaſion to dive, to forbear for a pretty while reſpiring under water without prejudice; I thought it worth the trial, whether ſuch birds would, much better than other animals, endure the abſence of the air in our exhauſted receiver. The accounts of which trials were, when they were made, regiſtered as follows:

EXPERIMENT I.

WE put a full grown duck (being not then able to procure a fitter) into a receiver, whereof ſhe filled, by our gueſs, a third part, or ſomewhat more, but was not able to ſtand in any eaſy poſture in it; then pumping out the air, though ſhe ſeemed at firſt (which yet I am not too confident of, upon a ſingle trial) to continue well ſomewhat longer than a hen in her condition would have done; yet within the ſhort ſpace of one minute ſhe appeared much diſcompoſed, and between that and the ſecond minute, her ſtruggling and convulſive motions increaſed ſo much, that her head alſo hanging careleſly down, ſhe ſeemed to be juſt at the point of death; from which we preſently reſcued her by letting in the air upon her; ſo that this duck being reduced, in our receiver, to a gaſping condition, within leſs than two minutes, it did not appear, that, notwithſtanding the peculiar contrivance of nature, to enable theſe water-birds to continue without reſpiration for ſome time under water, this duck was able to hold out conſiderably longer than a hen or other bird, not aquatick, might have done; and to manifeſt that it was not cloſeneſs and narrowneſs of the veſſel in reference to ſo bulky an animal that produced in the ſubject of our trial the great and ſudden change above-recited, we ſoon after included the ſame bird in the ſame receiver, and having by a ſpecial way cemented it on very cloſe, we ſuffered her to ſtay thus ſhut up with the air for five times as long as formerly (by our gueſs, helped by a watch) without perceiving her to be diſcompoſed; and ſhe would probably have continued longer in the ſame condition, if my patience and leiſure would have held out ſo long as ſhe could have done in that priſon.

EXPERIMENT II.

Having at the ſeaſon of the year procured a duckling that was yet callow, we conveyed her into the ſame receiver wherein the former had been included, and obſerved, that, though for a while ſhe appeared not much diſquieted, whilſt the air was pumping out of the glaſs, yet before the firſt minute was quite ended, ſhe gave manifeſt tokens of being much diſordered; and the operation being continued a while longer, ſhe grew ſo much worſe, that ſeveral convulſive motions ſhe fell into before a ſecond minute was expired, obliged us to let in the air upon her, whereby ſhe quickly recovered.

N.B. I determine not whether it be proper in this place to add, that when the receiver was pretty well exhauſted, the included bird appeared to the ſpectators manifeſtly bigger than before the air was drawn, eſpecially about the crop, though that was very turgid before. And to manifeſt, that in this duck, as in the former, the convulſions that uſed to be immediately followed by death, proceeded from the withdrawing of the ambient air, and not from the clogging of it; we kept the ſame duckling in the ſame receiver very cloſe, to keep out all external air, and to keep in the excrementitious ſteams of her body for above 6 minutes, without perceiving her to grow ſick upon her impriſonment; which yet laſted above thrice the time that ſufficed to reduce her in the abſence of the air to a gaſping condition.

N.B. It not being intended that ducks and other water-fowl ſhould any more than other birds, live in an exceeding rarefied air, but only be able to continue upon occaſion a pretty while under water, it may ſuffice that the contrivance of thoſe parts which relate to reſpiration, be ſo far fitted for the purpoſe, as we ſhall ſee it is, when we come to the tenth title.

TITLE II.

Of the phænomena afforded by vipers included in an exhauſted receiver.

Considering that vipers are animals endowed with lungs (though of a different ſtructure from thoſe of men, dogs, cats, and birds, &c.) and that their blood is, as to ſenſe, actually cold; I thought it might, upon both theſe accounts, be very well worth trying; what effect the withdrawing and abſence of the air would have upon animals ſo conſtituted. I therefore made divers trials, ſome of which did not diſpleaſe me; but I know not by what misfortune the memorials of them were lost, except two or three, which were not perfect, that I shall here subjoin.

EXPERIMENT I.

We included a viper in a small receiver, and as we drew out the air, she began to swell, and afforded us these phænomena. Jan. 2. 166⅔.

1. It was a good while after we had left pumping before the viper began to swell so much as to be forced to gape, which afterwards she did.

2. That she continued, by our estimate, above two hours and half in the exhausted receiver without giving clear proof of her being killed.

3. That after she was once so swelled as to be compelled to open her jaws, she appeared slender and lank again; and yet very soon after appeared swelled again, and had her jaws disjoined as before.

EXPERIMENT II.

We took a viper, and including her in the greatest sort of small receivers, we emptied the glass very carefully, and the viper moved up and down within, as if it were to seek for air, and after a while foamed a little at the mouth, and left of that foam sticking to the inside of the glass, her body swelled not considerably, and her neck less, till a pretty while after we had left pumping; but afterwards the body and neck grew prodigiously tumid, and a blister appeared upon the back. An hour and an half after the exhaustion of the receiver, which we then by trial found to be pretty stanch, the distended viper did give by motion manifest signs of life; but we observed none afterwards. The tumor reached to the neck, but did not seem much to swell the under-chap, both the neck and a great part of the throat, being held betwixt the eye and the candle, were transparent enough where the scales did not darken them. The jaws remained mightily opened, and somewhat distorted; the epiglottis with the rimula laryngis, which remained gaping, was protruded almost to the farther end of the nether-chap. As it were from beneath this epiglottis came the black tongue and reached beyond it, but seemed by its posture not to have any life, and the mouth also was grown blackish within, but the air being re-admitted after 23 hours in all, the viper's mouth was presently closed, though soon after it was opened again, and continued long so; and scorching or pinching the tail made a motion in the whole body that argued some life.

EXPERIMENT III.

To these experiments upon vipers I shall add one made upon an ordinary harmless snake. April 25.

We included such an animal, together with a gage, in a pretty portable receiver, which, being exhausted and well secured against the ingress of the air, was laid aside in a quiet place, where it continued from 10 or 11 of the clock in the forenoon, till about nine the next morning; and then my occasions calling me abroad, I looked upon the snake, which, though he seemed to be dead, and gave no signs of life upon the shaking of the receiver; yet, upon holding the glass a convenient distance from a moderate fire, he did in a short time manifest himself to be alive by several tokens, and even by putting forth

forth his forked tongue. In that condition I left him, and, by reason of several avocations, came not to look upon him again till the next day early in the afternoon; at which time he was grown past recovery, and his jaws, which were formerly shut, gaped exceeding wide, as if they had been stretched open by some external violence.

TITLE III.

Of the phænomena afforded by frogs in an exhausted receiver.

Sept. 9, 1662. The same considerations that induced me to make several trials upon vipers, did also invite me to make several upon frogs; the success of some of which the following notes will declare.

EXPERIMENT I.

We took a large lusty frog, and having included her in a small receiver, we drew out the air, and left her not very much swelled, and able to move her throat from time to time, though not so fast, as when she freely breathed from the exsuction of the air. She continued alive two hours, that we took notice of, sometimes removing from the one side of the receiver to the other, but she swelled more than before, and did not appear by any motion of her throat or thorax to exercise respiration, but her head was not very much swelled, nor her mouth forced open. After she had remained there somewhat above three hours (for it was not $3\frac{1}{2}$ hours, *i. e.* 3 hours and a half) perceiving no sign of life in her, we let in the air upon her, with which the formerly tumid body shrunk very much, but seemed not to have any other change wrought in it; and though we took her out of the receiver, yet in the free air itself, she continued to appear stark dead. Nevertheless, to see the utmost of the experiment, having caused her to be laid upon the grass in a garden all night, the next morning we found her perfectly alive again.

EXPERIMENT II.

June 29, 1660. About 11 of the clock in the forenoon we put a frog into a small receiver, containing about $15\frac{1}{4}$ ounces troy weight of water, out of which we had tolerably well drawn the air (so that when we turned the cock under water, it sucked in about $13\frac{1}{4}$ ounces of water); the frog continued in it (the receiver all the while under water) lively enough until about 5 of the clock in the afternoon, when it expired. The frog at the first seemed not to be much altered by the exsuction of the air, but continued breathing both with her throat and lungs.

EXPERIMENT III.

Sept. 6, 1662. We included into a pretty large receiver a couple of frogs newly taken, the one not above an inch long, and proportionably slender; the other very large and lusty. Whilst the air was drawing out, the lesser frog skipped up and down very lively, and, somewhat to our wonder, clambered up several times to the sides of the receiver, insomuch, that he he fometimes refted himfelf againft the fide of the glafs. When his body feemed to be perpendicular to the horizon, if not in a reclining pofture, he continued to fkip up and down a while after the exfuction of the air; but within a quarter of an hour (meafured by a minute watch) we perceived him to lie ftark dead with his belly upwards. The other frog, that was large and ftrong, though he began to fwell much upon the withdrawing of the air, and feemed to be diftreffed by his frequently leaping up after the air was drawn out, which he did not before, yet being, as we faid, very lufty, he held out half an hour, at which time it was remarkable, that the receiver, though it had held out againft the preffure of the outward air during that fpace of time, notwithftanding that a piece of it had been cracked out, and was mended, with a cloth dipped in cement, yet, at the end of the half hour the weight of the outward air fuddenly beat it in, and thereby brought the imprifoned frog a reprieve, which hindered us from bringing the experiment to an iffue.

EXPERIMENT IV.

We took a fmall frog, and having conveyed her into a very fmall portable receiver, we began to pump out the air. At firft fhe was lively enough, but when the air began to be confiderably withdrawn, fhe appeared to be very much difquieted (leaping fometimes after an odd manner, as it were, to get out of the uneafy prifon, but yet not fo, but, that after the operation was ended, and the receiver taken off, the frog was perfectly alive, and continued to appear fo (if I am not miftaken) near an hour, though the abdomen was very much, and the throat fomewhat extended; this latter part having alfo left that wonted panting motion that is fuppofed to argue and accompany the refpiration of frogs. At the end of about three quarters of an hour, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which by that time was ftrangely fwelled, did not only fubfide, but feemed to have a great cavity in it, as the throat alfo proportionably had; which cavities continued, the frog being gone paft all recovery. Sept. 11.

EXPERIMENT V.

A large frog was conveyed into a plated receiver, and the air being withdrawn, her body by degrees was diftended; as appeared very notably, when by a cafual fpringing of a leak the air got in again, and made her look much more lank and hollow than ever. The receiver, with the gage, were kept under water near feven hours, becaufe I was obliged to ftay long abroad; at the end of which, coming home, I found the receiver ftanch, but the frog dead and exceedingly fwelled; upon the letting in of the air, fhe became more hollow and lank than ever. April 14.

N. B. I have purpofely, both under this title, and fome others, fubjoined fome trials, whofe events are not altogether fuch, as others recited under the fame head, would invite one to expect; but I purpofely do it, not only to be true to the impartiality I propofed to myfelf in writing thefe narratives, but to awaken the curious to confider and obferve what variety of phænomena in fuch trials may be attributed to the feafon of the year wherein they are made; and to ftrength, bulk, age, peculiar conftitutions, &c. that relate to the refpective animal on which the experiments are made; befides what things may on other account be fit to be alfo confidered.

TITLE

TITLE IV.

Of the phænomena afforded by a new kittened kitling in the exhaufted receiver.

BEING defirous to try whether animals that had lately been accuftomed to live, either without any, or without a full refpiration, would not be more difficult or flowly killed by the want of the air, than others, which had been longer ufed to a free refpiration; we took a kitling that had been kittened the day before, and put it into a very fmall receiver (that we gueffed to hold about a pint or lefs) that it might be the fooner exhaufted. As foon as the pump began to play, I took notice of the time, and found by a watch, that marks minutes and quarter minutes, within one minute, or little more, after the air firft began to be withdrawn, that the little animal, who, in the mean time had gafped for life, and had fome violent convulfions, lay as dead, with his head downwards, and his tongue out; but upon letting in of the air, he did in a trice fhew figns of life, and being taken out of the receiver, quickly recovered; and to allow him the benefit of his good fortune, we fent for a kitling of the fame age and litter, which being put into the fame receiver, quickly began, like the other, to have convulfions, after which he lay as dead; but obferving very narrowly, I perceived fome little motions, which made me conclude him alive; which I foon found I had caufe to do. For though we continued pumping, and could not perceive that the engine leaked more than in the former experiments; the kitling began to ftir again, and after a while had ftronger and more general convulfions than before; until at the end of full fix minutes after the exfuction of the air was begun, the animal feeming quite dead, the outward air was re-admitted into the receiver, which not reviving him as it had done the other, he was taken out of the veffel, and lay with his mouth open, and his tongue lolling out, without any fenfible breathing and pulfation; until having ordered him to be pinched, the pain, or fome internal motion, produced by the external violence done to him, made him immediately give manifeft figns of life, though there was yet no fenfible motion of the heart or the lungs; but afterwards gaping and fetching his breath in an odd manner, and with much ftraining, as I have feen fome fœtufes do, when cut out of the womb, he, little by little, within about a quarter of an hour, recovered; wherefore, thinking it fevere to make him undergo the fame meafure again, we fent for another, kittened at the fame time, and inclofing that alfo in the receiver, obferved that divers violent convulfions, as it were, gafping for breath, into which he began to fall at the fecond or third fuck, ended in a feeming death, within about a minute and a half; but, being made more diffident by the late experiments, I caufed the pump to be plied, and the rather, becaufe I had a mind to obferve, whether, when the air was from time to time drawn away, there would not, upon the opening of the ftop-cock to let it out, appear fome fudden fwelling, greater or lefs, of the body of the animal, by the fpring and expanfion of fome air (or aërial matter) included in the thorax or the abdomen. Such an inflation (though not great) we thought we obferved; but until farther trial, I dare not acquiefce in it. A while after, notwithftanding our continuing to pump, the kitling gave manifeft figns of life, which was not until it had endured divers convulfions as great as thofe of the firft fit, if not greater. When 7 minutes from the beginning of the exhauftion were completed, we let in the air; upon which the little creature that feemed ftark dead before, made us fufpect that he might recover; but though we took him out of the receiver, and put aqua vitæ into his mouth, yet he irrecoverably died in our hands.

THESE

THESE trials may deferve to be profecuted with farther ones, to be made not only with fuch kittens, but with other very young animals of different kinds; for by what has been related, it appears that thofe animals continued 3 times longer in the exhaufted receiver, than other animals of that bignefs would probably have done.

TITLE V.

Some trials about the air ufually harboured and concealed in the pores of water, &c.

IT might affift us to make the more rational conjectures about the phænomena of divers of our experiments, (if we knew (fomething near) what quantity of aërial fubftance is ufually found in the liquors we employ about them, efpecially in that moft common of them, water; and therefore, though it be very difficult (if at all poffible) to determine the proportion of the air that lurks in water, with any thing of certainty, many circumftances making it fubject to vary very much; yet, to make the eftimate, I eafily could, where none at all, that I know of, hath been hitherto made by any man, I confidered that it might afford us fome light, if we difcovered, at leaft, what proportion, as to bulk, the air latitant in a quantity of water would have to the liquor it came from, when the aërial particles fhould be gathered together into one place. For though, about this union, and the fpring that may be confequent to it, fome doubts may be fuggefted, which I have not now time to difcufs; yet I fuppofed, that, at leaft, fome difcoveries would by this way be made, though not of the true proportion between the air and the water, yet about two or three particulars, in due time to be taken notice of.

To find inftruments, which would any way accommodate our purpofe, proved a very difficult work; fo that among other things that we were fain to do, this was one, that to evince how little the air latitant in water, did appear to leffen the bulk of that water, if it were fuffered to fly away in an open tube; we fuffered it to efcape in an exhaufted receiver, without any artifice to catch it; by which trial the water did not part with any thing of its bulk that made a diminution fenfibly to the eye. Wherefore we endeavoured to make this lofs vifible by fome other trials, of which I can find but a few hafty memorials among my loofe entries.

A CHEMICAL pipe fealed at one end, and 36 inches, or fomewhat lefs, in length, was filled with water, and inverted into a glafs veffel, not two inches in diameter, but $\frac{1}{4}$ of an inch, or little more in depth. Thefe glaffes being conveyed into a fit receiver, and the air being leifurely pumped out, and fomewhat flowly re-admitted, the numerous bubbles that had afcended during the operation, conftituted at the top an aërial aggregate, mounting to $\frac{8}{10}$, wanting about an hundredth part of an inch.

PRESENTLY after the tube (by and by to be defcribed) was filled again with the fame water, and inverted; and the water being drawn down to the furface of the veffeled water, and the air let in again, the water was impelled up to the very top, within a tenth and half a tenth of an inch. These are two experiments.

THE tube for meafuring the air latitant in water was 43 inches and $\frac{1}{2}$ above the furface of the ftagnant water; the air collected out of the bubbles at the top of the water was the firft time $\frac{1}{4}$ of an inch, and fomewhat better; the fecond time we eftimated it but $\frac{1}{2}$ an $\frac{1}{16}$. The firft time the water in the pipe was made to fubfide full as low as the furface of the reftagnant water; the fecond time the loweft we made it fubfide feemed to be four or five inches above the furface of the water in the open veffel.

Matter of fact thus recited would afford divers difficulties worthy to be conſidered, which I have not leiſure to diſcuſs; eſpecially, the odd thing that happens to the aërial particles of water; for though, whilſt they lay concealed in the water, they took up ſo little room in it that it was inſenſible; and when they were permitted to eſcape out of the tube, the water was not manifeſtly diminiſhed by their receſs; yet, when they were aſſociated at the top of the tube, their aggregate did ſometimes maintain a place that was conſiderable enough in reference to the capacity of the whole tube, though I muſt here advertize that this aggregate did, at the top of the tube, poſſeſs more room than its bulk did abſolutely require; becauſe it was ſomewhat defended from the preſſure of the atmoſphere by the weight of the ſubjacent cylinder of water, which might be about three or four feet long.

Quere, Whether any conſiderable proportion of bubbles will be afforded by the ſame liquor, if it be ſuffered to continue in the glaſs for ſome competent time after it has been once, or oftener, freed from bubbles already?

Quere, How far it may be worthy our conſideration, whether, in common water, there may not be concealed air enough to be of uſe to ſuch cold animals as fiſhes; and whether it may be ſeparable from the water that ſtrains through their gills?

But though I was at firſt content to make uſe of this way of eſtimating the air concealed in water; yet, when I came where I could be a little better accommodated with glaſſes, I bethought myſelf of a ſmall inſtrument that would much better diſcloſe the wonderful plenty of the aërial particles I deſigned to diſcover. The ſtructure and uſe of this glaſs may be eaſily enough underſtood by the recital of the firſt experiment that was made with it, whereof take the following tranſcript.

We provided a clear round glaſs, furniſhed with a pipe or ſtem of about nine inches in length, the globulous part of the glaſs being on the outſide about three inches and half in diameter; the pipe of this glaſs was within an inch of the top, melted at the flame of a lamp, and drawn out for two or three inches as ſlender as a crow's quill, that the decrement of the water upon the receſs of the air harboured in its pores, might, if any ſhould happen, be the more eaſily obſerved and eſtimated. Above this ſlender part of the pipe, the glaſs, as was before intimated, was of the ſame largeneſs, or near it, with the reſt of the pipe, that the aërial bubbles, aſcending through the ſlender part, might there find room to break, and ſo prevent the overflowing or loſs of any part of the water.

This veſſel being not without difficulty and ſome induſtry filled, till the liquor reached to the top of the ſlender part, where not being uniformly enough drawn out, it was ſomewhat broader than elſewhere; we conveyed the glaſs, together with a pedeſtal for it to reſt upon, into a tall receiver, and pumping out the air, there diſcloſed themſelves numerous bubbles, aſcending nimbly to the upper part of the glaſs, where they made a kind of froth or foam, but, by reaſon of the above-mentioned figuration of the veſſel, they broke at the top of the ſlender part, and ſo never came to overflow.

This done, the pump was ſuffered to reſt a-while to give the aërial particles lodged in the water time to ſeparate themſelves and emerge, which when they had done a pretty while, the pump was plied again for fear ſome air ſhould have ſtolen into ſo large a receiver; theſe viciſſitudes of pumping and reſting laſted for a conſiderable time, till at length the bubbles began to be very rare and we weary of waiting any longer; ſoon after which the external air was let into the receiver, and it appeared ſomewhat ſtrange to the ſpectators, that notwithſtanding ſo great a multitude of bubbles as had eſcaped out of the water, I could not, by attentively comparing the place where the ſurface of the water reſted at firſt, to which a mark had been affixed, with that where it now ſtood; I could not, I ſay, diſcern the difference to amount to above, if ſo much as an hair's breadth; and

and the chief operator in the experiment professed, that, for his part, he could not perceive any difference at all.

Thus far for the narrative of the trial made by water; but that was not the only liquor into whose aërial particles I designed, by our little instrument, to inquire, and therefore filling a glass of the same shape, and much of the same bigness, with claret wine, and placing it upon a convenient pedestal, in a tall receiver, we caused some of the air to be pumped out; whereupon, in a short time there emerged, through the slender pipe, so very great a multitude of bubbles that were darted, as it were, upwards, as did not a little both please and surprize the beholders; but it forced us to go warily to work, for fear the glass should break, or the wine overflow. Wherefore we seasonably left off pumping before the receiver was any thing near exhausted, and suffered the bubbles to get away as they could, till the present danger was over-passed; and then from time to time we pumped a little more air out of the receiver, till we were weary, the withdrawing of a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles not only copiously, but very swiftly to ascend (by a minute watch) for above a quarter of an hour together.

The little instrument made use of about these trials, being designed to examine, among other things, the quantity of bubbles lurking in several liquors, is to be applied to spirit of wine and chemical oils, that are more subtle liquors than wine itself. And some circumstances of our trials made us think that it might be worth examining what kind of substance may be obtained by this way of handling aërial and spirituous corpuscles. But of the other uses of our instrument elsewhere.

TITLE VI.

Of some phænomena afforded by shell-fishes in an exhausted receiver.

EXPERIMENT I.

An oyster being put into a very small receiver, and kept in long enough to have successively killed three or four birds or beasts, &c. was not thereby killed, nor, for aught we could perceive, considerably disturbed; only at each suck we perceived that the air contained between the two shells broke out at their commissure; as we concluded from the foam which at those times came forth all round that commissure. About twenty-four hours after, coming to see in what condition this oyster was, I found that both this and another that had been put at the same time into the receiver, were alive; but how long afterwards they continued so, I did not observe.

EXPERIMENT II.

That same day we put a pretty large craw-fish into a pretty large receiver, and found, that though he had been injured by a fall before he was brought thither, yet he seemed not to be much incommoded by being included, till the air was in great measure pumped out; and then its former motion presently ceased, and he lay as dead, till upon the letting in a little air into the receiver, he began forthwith to move afresh; and upon the withdrawing the air again, he presently, as before, became moveless. Having

 repeated

repeated this trial two or three times, we took him out of the receiver, where he appeared not to have fuffered any harm.

EXPERIMENT III.

But I thought it not unlikely that there may be fome fuch inequality in the ftrength or vivacity of animals, as to fuch kind of experiments as ours, that it might be well worth while in feveral cafes to reiterate our trials. And on this occafion I fhall here add, that having put an oyfter into a phial full of water, before we included it in the receiver, that through the liquor the motion of the bubbles, expected from the fifh, might be the more pleafantly feen and confidered; this oyfter proved fo ftrong, as to keep itfelf clofe fhut, and repreffed the eruption of the bubbles, that in the other did force open the fhells from time to time, and kept in its own air as long as we had occafion to continue the trial.

EXPERIMENT IV.

Moreover a craw-fifh that was thought more vigorous, being fubftituted in the place of the former craw-fifh, though once he feemed to lofe his motion together with the air, yet afterwards he continued moving in the receiver, in fpite of our pumping, whether, becaufe there was fome unperceived leaking, that hindered a fufficient exhauftion of the air; or becaufe this particular animal was more ftrong or vivid than the other, we could not pofitively determine.

TITLE VII.

Of the phænomena of a fcale-fifh in an exhaufted receiver.

The following experiment is far from being the firft that was made on a fcale-fifh in our vacuum; but in regard that in the receivers wherein thofe trials were made, the external air could not be kept out near fo long, and fo well as in the veffel I am about to mention; I judged it well worth the pains to obferve, what would happen to a fifh in an exhaufted veffel, where it fhould be kept for fome hours together from all fupply of frefh air. And therefore I made feveral trials to that purpofe, whereof that which I think the moft confiderable was regiftered as follows:

We took a receiver fhaped almoft like a bolt-head, containing by eftimation near a pint, and the globulous part of it being almoft half full of water, we put into it, at the orifice (which was pretty large) a fmall gudgeon about three inches long, which, when it was in the water, fwam nimbly up and down therein. Then having drawn out the air fo well, that we gueffed by a gage, that about nineteen parts of twenty, or more might be exhaufted, we fecured ourfelves, that the regrefs of the air fhould not injure our experiment; about which we obferved thefe particulars.

First, The neck of the glafs being very long, though there appeared great ftore of bubbles all about the fifh, yet the reft of the water, notwithftanding the withdrawing of fo much air as has been mentioned, emitted no froth and but few bubbles.

Secondly,

Secondly, The fiſh both at his mouth and gills did for a great while diſcharge ſuch a quanity of bubbles as appeared ſtrange, and for about half an hour or more (for much longer I had not an opportunity to watch it) whenever he reſted awhile new bubbles would adhere to many parts of his body (as if they were generated there) eſpecially his fins and tail, ſo that he would appear almoſt beſet with bubbles; and if, being excited to ſwim, he was made to ſhake them off, he would quickly, upon a little reſt, be beſet with new ones as before.

Thirdly, Almoſt all the while he would gape and move his gills, as before he was included; though towards the end of the time that I watched, it often happened, that he neither took in, nor emitted any aërial particles, that I could perceive.

Fourthly, After a while he lay almoſt conſtantly with his belly upwards, and yet would in that poſture ſwim briſkly as before.

Fifthly, Nay, after a while he ſeemed to be more lively than at firſt putting in; whether by reaſon that by diſcharge of ſo many bubbles, which, by their diſtenſion, perhaps put him to pain, he found himſelf relieved, or for ſome other cauſe, I examine not.

Having occaſion to go abroad, I returned about an hour and a half after he had been ſealed up, and found him almoſt free from bubbles, and with his belly upwards, and ſeeming ſomewhat tumid, but yet lively as before. But an hour and a quarter after that, when riſing from dinner, I went to look upon him again, he ſeemed to be moveleſs, and ſomewhat ſtiff; yet, upon ſhaking the glaſs, obſerving ſome faint ſigns of life in him by ſome languid motions, he attempted to make when excited to them, I opened the receiver under water, to try if that liquor and air would recover him; and the external water, ruſhing in till it had filled the vacant part of the ball and the greateſt part of the ſtem too, the fiſh ſunk to the bottom of it, with a greater appearance than ever of being alive; in which ſtate, after he had continued a pretty while, I made a ſhift, by the help of the water he ſwam in, to get him through the pipe into a baſon of water, where he gave more manifeſt ſigns of life, but yet for ſome hours lay on one ſide or other without being able to ſwim or lie on his belly, which appeared very much ſhrunk in, as if ſomething during the time of its being ſealed up had been broken in his body, or his belly had been exceedingly diſtended, beyond reſtitution to its former tone.

All the while he continued in the baſon of water, though he moved his gills as before he had been ſealed up, yet I could not perceive that he did, even in his new water, emit, as formerly, any bubbles, though two or three times I held him by the tail in the air, and put him into the water again, where at length he grew able to lie conſtantly upon his belly, which yet retained much of its former lankneſs; and though it be now about or above twenty-four hours, ſince he was firſt included, he continues yet alive.

(Postscript. He lived in the baſon eight or ten days longer; though divers gudgeons ſince taken died there in much fewer days).

TITLE

TITLE VIII.

Of two animals included, with large wounds in the abdomen, in the pneumatical receiver.

EXPERIMENT I.

Sept. 12. A SMALL bird, having the abdomen opened almoft from flank to flank, without injuring the guts, was put into a fmall receiver, and the pump being fet to work, continued for fome little time without giving any figns of diftrefs; but at the end of about a minute and a half from the beginning of the exhauftion, fhe began to have convulfive motions in the wings; and though the convulfions were not univerfal, or did appear violent, as is ufual in other birds, from whom the air is withdrawn by the engine, yet at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable; notwithftanding which we did not find any notable alteration in the lungs, and found the heart, or at leaft the auricles of it, to be yet beating, and fo it continued for a while after.

EXPERIMENT II.

Sept. 12. WE took alfo a pretty large frog, and having, without violating the lungs or the guts, made two fuch incifions in the abdomen, that the two curled bladders or lobes of the lungs came out almoft totally at them, we fufpended the frog by the legs in a fmall receiver; and after we had pumped out a good part of the air, the animal ftruggled very much, and feemed to be much difordered; and when the receiver was well exhaufted fhe lay ftill for a while, as if fhe had been dead, the abdomen and thigh very much fwelled, as if fome rarefied air or vapour forcibly diftended them; but as, when the frog was put in, one of the lobes was almoft full, and the other almoft fhrunk up, fo they continued to appear after the receiver had been exhaufted; but upon letting in of the air, not only the body ceafed to be tumid, but the plump bladder appeared for a while fhrunk up as the other, and the receiver being removed, the frog prefently revived and quickly began to fill the lobe with air.

TITLE IX.

Of the motion of the feparated heart of a cold animal in the exhaufted receiver.

WITHOUT difcuffing the opinions of learned men about the connection and dependency of the motions of the blood and beating of the heart, I thought it might give me a fufficient inducement to make the following experiment, that feveral forts of animals would be prefently killed in our vacuum by the withdrawing of the air; and even the infects mentioned in the formerly publifhed digreffion about refpiration, though they alfo were not totally deprived of life by the abfence of the air, yet they were of vifible motion; wherefore fome good hint or other being to be hoped for from the difcovering whether or no a feparated heart, which is but a part of an animal, would continue its motion

motion in our vacuum; we made fome trials to that purpofe, whofe fuccefs I find thus fet down.

EXPERIMENT I.

The heart of an eel being taken out and laid upon a plate of tin in a fmall receiver, when we perceived it to beat there, as it had done in the open air, we exhaufted the veffel, and faw, that, though the heart grew very tumid, and here and there fent forth little bubbles, yet it continued to beat as manifeftly as before, and feemed to do fo more fwiftly, as we tried, by numbering the pulfations it made in a minute, whilft it was in the exhaufted receiver, and when we had re-admitted the air, and alfo when we took it out of the glafs, and fuffered it to continue its motion in the open air. The heart of another eel, being likewife taken out, continued to beat in the emptied receiver as the other had done.

EXPERIMENT II.

The heart of another eel, after having been included in a receiver, firft exhaufted, and then accurately fecured from leaking, though it appeared very tumid, continued to beat there an hour; after which, looking upon it, and finding its motion very languid, and almoft ceafed, by breathing a little upon that part of the glafs where the heart was, it quickly regained motion, which I obferved a while; and an hour after, finding it to feem almoft quite gone, I was able to renew it by the application of a little more warmth. At the end of the third hour, coming to look at it once more, a bubble, that appeared to be placed between the auricle and the heart, feemed to have now and then a little trembling motion; but I found it fo faint, that I could no more by warmth excite it, fo as plainly to perceive the heart to move, wherefore I fuffered the outward air to rufh in, but could not difcern, that thereby the heart regained any fenfible motion, though affifted with the warmth of my breath and hands.

TITLE X.

A comparifon of the times, wherein animals may be killed by drowning or withdrawing of the air.

To help myfelf and others to judge the better of fome difficulties concerning refpiration, I thought it might be ufeful that we compared together the times wherein animals may be killed by that want of refpiration, which, in thofe that are drowned, is caufed by the water that fuffocates them, and that other want, which proceeds from withdrawing the ambient air. Of the latter of thefe, a fufficient number of inftances is to be met with among our other experiments, and therefore I fhall now fubjoin about the former the more trials, becaufe this comparifon hath not, that I know of, been yet thought on by any.

EXPE-

EXPERIMENT I.

Sept. 10. A GREEN-FINCH, having his legs and wings tied to a weight, was gently let down into a glafs-body filled with water; the time of its total immerfion being marked; at the end of half a minute after that time, the ftrugglings of the bird feeming finifhed, he was nimbly drawn up again, but found quite dead.

EXPERIMENT II.

WHEREUPON a fparrow, that was very lufty and quarrelfome, was tied to the fame weight and let down after the fame manner; but though he feemed to be under water more vigorous than the other bird, and continued ftruggling almoft to the very end of half a minute from the time of his being totally immerfed (during which ftay under water there afcended, from time to time, pretty large bubbles from his mouth) yet notwithftanding that as foon as ever the half minute was completed, he was drawn up, we found him, to our wonder, irrecoverably gone.

EXPERIMENT III.

A SMALL moufe, being held under water by the tail, emitted from time to time divers aërial bubbles out of his mouth, and at laft, as one of the fpectators affirmed, he faw at one of his eyes; being taken out at the end of half a minute and fome feconds, he yet retained fome motions; but they proved but convulfions, which at laft ended in death.

" BY what is related under the firft title, it does not appear that water-fowl, at leaft, " that ducks could, in our receivers, endure the want of air much longer than other " birds; but now to fhew that the contrivance of nature is not infignificant, as to the " enabling them to continue much longer under water without frefh air than the land- " birds above mentioned, it will not be amifs to fubjoin the two following experiments."

EXPERIMENT IV.

WE took the duck mentioned in the firft title, and fo tied a confiderable weight of lead to her body, as it did not hinder her refpiration, and yet would be fure to keep her down under water; which we had found that a fmall weight would not do by reafon of her ftrength, nor yet a great weight, if tied only to her feet, in fuch a middle-fized tube as ours was, becaufe of the height of her neck and beak. With the above-mentioned clog, the duck was put into a tub full of clear water, under whofe furface fhe continued about a minute, by my watch, quietly enough, but afterwards began to appear for a while much difturbed; which fit being over, our not perceiving any motion in her made us, at the end of the fecond minute, take her out of the water to fee in what condition fhe was, and finding her in a good one, after we had allowed her fome breathing-time to recruit herfelf with frefh air, we let her down again into the tub, which in the mean time had been filled with frefh water, left the other, which had been troubled with

with the ſteams and foulneſs of the duck's body, might either haſten her death by its being infected with them, or hinder our diſcerning what ſhould happen by its being opacated by them.

The bird being thus under water, did, after a while, begin, and from time to time continue to emit divers bubbles at her beak. There alſo came out at her noſtrils divers real bubbles from time to time; and when the animal had continued about two minutes or better under water, ſhe began to ſtruggle very much, and to endeavour either to emerge or change poſtures; the latter of which ſhe had liberty to do, but not the former. After four minutes the bubbles came much more ſparingly from her; then alſo ſhe began to gape from time to time (which we had not obſerved her to do before) but without emitting bubbles; and ſo ſhe continued gaping until near the end of the ſixth minute, at which time all her motions, ſome of which were judged convulſive, and others, that had been excited by our rouzing her with a forceps, appeared to ceaſe, and her head to hang careleſly down as if ſhe was quite dead. Notwithſtanding which, we thought fit, for greater ſecurity, to continue her under water a full minute longer, and then finding no ſigns of life, we took her out, and being hung by the heels and gently preſſed in convenient places, ſhe was made to void a pretty quantity of water, of which, whether any had been received into the lungs themſelves, we had not time and opportunity to examine. But all the means that were to recover the bird to life, proving ineffectual, we concluded ſhe had been dead a full minute before we removed her out of the water; ſo that, to ſum up the event of our experiment, even this water bird was not able to live in cold water without taking in freſh air, above ſix minutes; which is but $\frac{1}{10}$ of an hour.

EXPERIMENT V.

The duckling mentioned in the firſt title, and ſecond experiment, having a competent weight tied to her legs, was let down into a tub of water which reached not above an inch or two higher than her beak: during the moſt part of her continuance there came out ſtore of bubbles at her noſtrils; but there ſeemed to come out more and greater from a certain place in her head, almoſt equidiſtant from her eyes, but ſomewhat leſs remote from her neck than they. Whilſt ſhe was kept in this condition, ſhe ſeemed frequently to endeavour to dive lower under the water, and after much ſtruggling and frequent gaping, ſhe had divers convulſive motions, and then let her head fall down backward, with her throat upwards. To which moveleſs poſture ſhe was reduced at the end of the third minute, if not a little ſooner; but a while after there appeared a manifeſt, but tremulous motion in the two parts of her bill, which continued for ſome time, but afforded no circumſtances whereby we could be ſure that they were not convulſive motions; but theſe alſo ceaſing upon the end of the fourth minute, the bird was taken out and found irrecoverable.

EXPERIMENT VI.

A viper that was kept ſo many hours in an exhauſted receiver till it was concluded to be ſtark dead, and to have been ſo for a good while, was nevertheleſs reſolutely hindered by me from being thrown away, till I had tried what could be done by keeping it all night in a glaſs-body upon a warm digeſtive furnace: whereupon this viper was found the next morning not only to be revived, but to be very lively, ſo as to invite me to make with her, without ſeeking for another, the following experiment.

We put her into a tall glaſs-body, fitted with a cork to the orifice of it, and depreſſed with weight, ſo that ſhe could come at no air. In this caſe we obſerved her from time to time; and after ſhe had been ducked awhile, ſhe lay with very little motion for a conſiderable ſpace of time. At an hour and a quarter ſhe often put out her black tongue, at near four hours ſhe appeared much alive, and, as I remember, about that time alſo put out her tongue, ſwimming all this while, as far as we obſerved, above the bottom of the water. At the end of about ſeven hours or more, ſhe ſeemed yet to have ſome life in her, her poſture being manifeſtly changed in the glaſs from what it was awhile before; unleſs that might proceed from ſome difference made in her body, as to gravity and levity. Not long after ſhe appeared quite dead, her head and tail hanging down moveleſly, and directly towards the bottom of the veſſel, whilſt the middle of the body floated as much as the above-mentioned cork would permit it.

Haste maketh me pretermit the mention of divers things ſuggeſted by what hath been delivered upon the preſent title. But this one thing would be taken notice of, that though ſome of the above-mentioned animals ſeem, by the relations we have given of them, to have been a little ſooner deſtroyed by drowning, than any we have mentioned were by our engine, that is no ſure proof that ſuffocation does kill animals faſter than the deprivation of air they are expoſed to in our engine. For in drowning, that which deſtroys is applied to its full vigour at the firſt, and all at once; whereas our receivers being made for ſeveral purpoſes, the deprivation of the air that they make cannot be made all at once, but the air muſt be pumped out by degrees; ſo that till the laſt the receiver will be but partly emptied. For confirmation of which, I have this to alledge, that, having in the preſence of ſome virtuoſi provided for the nonce a very ſmall receiver, wherein yet a mouſe could live ſome time, if the air were left in it, we were able to evacuate it at one ſuck, and by that advantage we were enabled, to the wonder of the beholders, to kill the animal in leſs than half a minute.

THE

THE CONTINUATION OF THE EXPERIMENTS CONCERNING RESPIRATION

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A PREFACE concerning theſe EXPERIMENTS.

THOUGH, to ſhun prolixity, the preface which the author had made to all he wrote about reſpiration, have been purpoſely omitted; yet there are ſome few points ſo neceſſary to be taken notice of, that it is thought unfit to leave them wholly untouched; for the following experiments being not at firſt written for the preſs, and thrown by for many years, till they were very haſtily gathered together, and in ſome places ſupplied with others, little leſs haſtily annexed, to make ſome neceſſary ſupplies, the reader muſt not expect in ſuch a caſual tract (which the author confeſſes to be one of the moſt imperfect and immethodical of all his compoſures) any thing but novelty and truth, and an earneſt deſire to be ſerviceable in an inquiry ſo important to mankind, to the curious in general, and eſpecially to phyſicians, who, by the encouraging mention they have made of his former endeavours in this kind, have invited him to add theſe many new experiments to thoſe few, they had hitherto exerciſed their wits upon; and to leave them the more freedom to do ſo, he purpoſely forbore to confirm or confute any hypotheſis, or ſo much as propoſe any of his own; declaring it to be his aim, not to eſpouſe or make a party, but to communicate to the curious fome matters of fact that are new, and in an hiftorical way impartially delivered. No more of preface is now to be added, but that it is thought fit, for prevention of ambiguity, to give this advertifement, touching the ground of the title of *Vacuum Boylianum*, to be met with in thefe experiments; that as learned men, both Englifh and foreigners, in their writings, have familiarly, for diftinction-fake, employed the titles of *Machina Boyliana* and *Experimenta Boyliana*; fo the author that writ thefe, for the moft part in hafte, and for his own memory, did for difpatch-fake, call the abfence of the air procured in his receivers, our vacuum; whence by analogy was framed the *Vacuum Boylianum*, which he therefore thinks the lefs improper, becaufe, to call it vacuum abfolutely, would be judged by many a declaring himfelf a vacuift, who does not yet own the being either of their opinion, or a downright plenift; or elfe he muft be troublefome to the reader and himfelf, by frequently explaining what fort of vacuum he underftands; whereas he declares once for all, that by the *Vacuum Boylianum*, he means fuch a vacuity or abfence of common air, as is wont to be effected or produced in the operations of the *Machina Boyliana*.

TITLE XI.

Of the accidents that happened to animals in air, brought to a confiderable degree, but not near the utmoft one, of rarefaction.

In the generality of our pneumatical experiments upon animals, it fuited with our purpofes, to rarefy the air as much, and for the moft part as faft as we could; but I had other trials in defign, wherein an extraordinary degree of rarefaction, but yet not near the higheft, to which the air might be brought by our engine, feemed likelieft to conduce to my inquiries, and particularly feemed hopeful to afford fome light, in reference to thofe difeafes and diftempers that are thought primarily to affect the refpiratory organs, or to depend upon fomething amifs in refpiration.

Wherefore, having gages, by the help of which fuch experiments might be much better performed than elfe they could, I attempted feveral of them; fome of whofe fucceffes I find in the following memorials.

EXPERIMENT I.

Aug. 16. A linnet being put into a receiver, capable to hold about four half pints of water, the glafs was well clofed with cement and a cover, but none of the air was drawn out with the engine or otherwife. And though no new air was let in, nor any change made in the imprifoned air, yet the bird continued there three hours without any apparent approach to death; and though it feemed fomewhat fick, yet being afterwards taken out, it recovered, and lived feveral hours.

EXPERIMENT II.

Aug. 13 From the above-mentioned receiver about half the air was drawn out, a linnet being then in the glafs, and in that rarefied air (which appeared by a gage to continue in that ftate) the bird lived an hour and near a quarter before it feemed in danger of death; after

after which, the air being let in without taking off the receiver, fhe manifeftly recovered, and leaped againft the fide of the glafs; being taken out into the open air, fhe flew out of my hand to a pretty diftance.

EXPERIMENT III.

We conveyed into a receiver, capable to hold four half pints of water, a lark, together with the gage, by the help whereof we pumped out of the receiver three quarters of the air that was in it before; then heedfully obferving the bird, we perceived it to pant very much, fo that a learned phyfician (from whom I yet diffented) judged thofe beatings to be convulfive; having continued thus for a little above a minute and a half, the bird fell into a true convulfive motion that caft it upon the back. And although we made great hafte to let in the air; yet, before the expiration of the fecond minute, and confequently, in lefs than half a minute from the time immediately preceding the convulfion, the lark was gone paft all recovery, though divers means were ufed to effect it. Sept. 9.

EXPERIMENT IV.

Presently after we put into the fame receiver a green-finch, and having withdrawn the air until it appeared by the gage there remained but half, we prefently began to obferve the bird, and took notice, that, within a minute after, fhe appeared to be very fick, and fhaking her head, threw againft the infide of the glafs a certain fubftance, which I took to be vomit, and which afterwards appeared fo; upon this evacuation the bird feemed to recover and continue pretty well (but not without panting) until about the end of the fourth minute, at which growing very fick, fhe vomited again (fhaking her head as at firft) but much more unqueftionable than before, and foon after, eat up again a little of her vomit; at which time (whether that contributed to her recovery or no) fhe very much recovered. And though fhe had in all three fits of vomiting, yet for the laft feven or eight minutes that we kept her in the receiver, fhe feemed to be much more lively than was expected, which may in part be attributed to a little air that by accident got in, though it were immediately pumped out again. At the end of a full quarter of an hour from the firft exhauftion of the receiver, the bird appearing not likely to die in a great while, and the engine being needed for other ufes, we took out the bird, and thereby put a period to the experiment. Sept. 9.

EXPERIMENT V.

I now thought it fit to try, whether, though a viper would not hold out very many hours in air, brought to as high a rarefaction, as we could bring it by our engine, yet to that cold and vivacious animal, a very fmall proportion of air, in comparifon of what was neceffary to hot animals, would not fuffice to keep it alive for a confiderable time; the narration of the experiment I find regiftered as follows:

A viper lately bought of the perfon, that at this feafon ufes to take new ones almoft from day to day, was included, together with a gage, in a portable receiver capable to hold about three pints and half of water. This veffel being exhaufted and fecured againft the regrefs of the air, the imprifoned animal was obferved from time to time; and obferved April 12.

ſerved not only to be alive, but nimbly to put out and to draw back its tongue about 36 hours after it was ſhut up; for which reaſon we continued the veſſel longer in the ſame ſhady place, where, at the end of 60 hours, looking upon her, as I was going to bed, ſhe appeared very dull and faint, and not likely to live much longer; and the next morning, being by ſome occaſions carried abroad, and coming to look upon the glaſs preſently after dinner, I found her ſtark dead, with her mouth opened to a ſtrange wideneſs; wherefore ſuffering water to be impelled by the outward air into the cavity of the receiver, to obſerve how far that veſſel was then emptied of air, we found by the water that was driven in and afterwards poured out again, and meaſured, that 4 parts of 5, or rather 5 of 6 of the veſſeled air (if I may ſo call that which was ſhut up in the receiver) had been pumped out; ſo that in an air ſo rarefied as to expand itſelf to 5 or 6 times its former and uſual dimenſions, our viper was able to live 60 hours that we are ſure of, and perhaps might a pretty while longer.

A digreſſive experiment concerning reſpiration upon very high mountains.

To illuſtrate what I have taken notice of in the printed experiments about the unfitneſs for reſpiration, obſerved by the learned *Acoſta* in the high mountains of *Pariacaca*, I ſhall here add, what I have had the curioſity and occaſion to learn from divers travellers, whom I purpoſely conſulted about theſe matters; whereof you will eaſily believe, that not many of them have had opportunity to give accounts. Meeting with an eccleſiaſtical perſon that had viſited thoſe high mountains of *Armenia* (on one of which, becauſe of their height, the tradition of the natives will needs have the ark to have reſted) I aſked him, whether thoſe mountains are as really ſo high as is given out, and whether at the top of that he viſited, he found any difficulty of breathing. To the firſt part of which queſtion he anſwered, that they were really exceeding high (which he might well judge of, having been upon ſome of the moſt famous both in *Europe*, *Aſia*, and *Africa*) and that he could not come to the top becauſe of the unpaſſable ſnows; and to the ſecond part he replied, that whilſt he was in the upper part of the mountain, he plainly perceived that he was reduced to fetch his breath much oftener than he was wont, and than he did before he aſcended the hill, and after he came down from it. And upon my inquiring whether or no that difficulty of breathing might not be accidental or peculiar to him, he told me, that he himſelf having expreſſed ſome wonder to find himſelf ſo ſhort-winded, the people told him, that it was no more than happened to them, when they were ſo high above the plain; it being a common obſervation among them. And I was the more inclined both to make inquiry about theſe matters, and to believe what he ſaid, becauſe what he related of their being covered with ſnow, and of an odd temperature of air, I had learned before from a traveller of another nation than this perſon, and a ſtranger to him.

The ſame churchman being aſked by me, whether he had not in ſome part of *Europe* made the like obſervation (of the difficulty of breathing) told me, that he had done it upon the top of a mountain in the country of *Cevennes*, in or near the province of *Languedoc*; which may ſerve to confirm what I am about to relate from the mouth of a learned traveller that was upon the top of one of the Pyreneans, that is not very remote from the mountains we ſpeak of.

This gentleman, who was a perſon curious and intelligent, being brother-in-law to one of the chief lords of thoſe parts, was, by him, invited, about the beginning of September, to viſit a neighbouring mountain that is at leaſt one of the higheſt of the Pyreneans, which

which is commonly called *Pic de Midi*, upon whose top, where a tent was spread for them, they staid many hours. His answers to the other questions I asked him are elsewhere related: all that concerns this place being, that I find this set down among my adversaria, viz. I also inquired of him, whether they found the air at the top as fit for respiration as common air, which he told me they did not, but were fain to breathe shorter and oftener than usual; and because I suspected that might come from their motion, I asked, whether they observed it to cease, when they came down to the bottom of the hill, which he told me they plainly did; besides that they staid many hours at the top, too long to continue out of breath.

But that I may not here conceal any thing that may conduce to the discovery of the truth in the matter under consideration, I shall here add, that I did sometimes think it worth further inquiry, whether the sickness, if not also the difficulty of breathing, that some have been obnoxious to, in the uppermost parts of Pariacaca, and perhaps some other high mountains, may not be imputed, not so precisely to the thinness and rarity of the air, in places so remote from the lowermost part of the atmosphere, as to include certain steams of a peculiar nature, which, in some places, the air may be imbued with? In favour of which suspicion, I remember, that inquiring once of an intelligent man who had lived several years in the Island of *Teneriff*, whether he had been at the top of the *Pic* of that name, and what he had there taken notice of about the air? he answered me, that he had attempted to go up to the top of the mountain, but that, though some of the company were able to do so, he and some others, before they had reached so high, grew so sick upon the operation they felt of the sharp air, and sulphurous exhalations which infected it, that they were fain to stay behind their companions, he having already found this effect of those piercing steams upon his face, which, when he made me this relation, was of a fair complexion, that the skin began to be of a pale yellow, and even his hair to be discoloured.

TITLE XII.

Of the observations produced in an animal, in changes as to rarity and density made in the self-same air.

In the experiments hitherto recited, the animals that were recovered from a gasping condition, have been so, by letting in fresh air upon them, and not the same that had been withdrawn from them. Wherefore I thought it very requisite to try whether the same portion of air, without being renewed, would, by being expanded much beyond its usual degree, and reduced to it, serve to bring an animal to death's door, and revive him again; since by the success of such a trial it would notably appear, that the bare change of the consistence of the air, as to rarity and density, may suffice to produce the above-mentioned effect.

But to devise a way to put this experiment in practice appeared no easy matter; since it required a receiver that should be transparent, and be capable of changing its bulk, without suffering any air to get in or out.

To surmount these difficulties, the first thing I thought on was, to take a fine limber and clear bladder of a sheep or hog, made more transparent by being anointed with oil, which was done on the outside, that the smell of it might less offend the animal to be included. Then we clipped off as much of the bladder at the neck as was judged absolutely necessary to make an orifice capable of letting in a mouse; that sort of animals being,

being, by reafon of their fmallnefs, the fitteft of thofe furnifhed with lungs and hot blood we could procure. And whereas it feemed very difficult, when the neck of the bladder was cut off, to make up fo large an orifice without wrinkles, at which the rarefied air may efcape; to obviate this inconvenience, we provided a round ftick fomewhat lefs than the orifice, that, the wood being laid over with a clofe and yielding cement (for pitch, or the like common ftuff will not always ferve the turn) we might be able to tie the bladder faft and clofe enough upon the thus fitted ftopple.

And now to reduce thefe things to practice, and by their help make our defigned experiment, we included a moufe into a receiver made according to this way, leaving in the bladder as much air as we thought might fuffice him for a long time, as the experiment was to laft. Then putting this limber or extenfible receiver, if I may fo call it, into an ordinary one of glafs, and placing this engine near a window, that we may fee through both of them; the air was by degrees pumped out of the external receiver (as for diftinction fake I fhall call it) and thereupon the air included in the bladder did proportionably expand itfelf, and fo diftend the external receiver, till being arrived at a degree of rarefaction, which rendered it unfit for the included moufe's refpiration, I perceived, though with fome difficulty, in this animal, the figns of his being in great danger of fudden death. Whereupon the outward air being haftily let into the external receiver, compreffed the fwelled bladder to its former dimenfions, and thereby the included air to its former denfity, by which means the fainting moufe was quickly revived. Having given him fome convenient time of refpite, the experiment was reiterated with the like fuccefs, and we doubted not, but the third trial we made would have ended as the two former did; but that, whilft we were confidering of the ficknefs of the moufe, which, by reafon of fome opacity that could fcarce be avoided in the wrinkled bladder, was not as to its degree fo eafily taken notice of, it grew irrecoverable by the fubfequent condenfation of the air.

N. B. The confirmation of this by further experiments will properly fall under another title.

TITLE XIII.

Of an unfuccefsful attempt to prevent the neceffity of refpiration by the production or growth of animals in our vacuum.

Having had frequent occafions to obferve how quickly thofe animals, whofe blood is actually warm, did expire in our vacuum; and that even thofe animals, with lungs, whofe blood was actually cold, were not able to live any confiderable time there; I thought it very well worth while, and yet extremely difficult, to try, whether there might not be fome ways yet unpractifed, either to make fuch animals as nature endows with lungs, live without refpiration, or at leaft, to bring fuch infects, and other animals, as can already live without air, to move alfo without it in our vacuum.

Therefore confidering with myfelf what happens to infants and other young animals in the womb, and even after they come from thence, if they continue to be wrapped up in the fecundines; though as foon as they are brought into the free air, they may be prefently killed by being kept from breathing; confidering alfo, what I elfewhere relate of the flow expiration of a very young kitling in our vacuum, together with the long want of refpiration, which cuftom enables fome divers to endure; confidering thefe things, I fay, though I know that fomewhat may be objected to fhew, that thefe inftances

ftances are not altogether full to my purpofe; yet they, among other things, invited me to think that the leaft unlikely projects that occurred to my barren invention, would be thefe that follow.

First, I thought fit to try whether the feeds of refpiring animals might be either hatched or otherwife brought to produce young ones, in our vacuum. For, if that could be compaffed, I fhould obtain my end.

Next, in cafe of my failing in the former attempt, and that, which is to be after a few lines propofed, I thought fit to try, whether, at leaft, I could not bring the eggs of infects to hatch or be animated; or aurelias, as they call them, that were already alive, turn, according to the courfe of nature, into winged infects, as flies, or butter-flies; of which trials, and thofe of the former fort, the account properly belongs to another place, where I relate the fuccefs of thefe and other attempts to produce plants and animals in our vacuum.

But thirdly, confidering that nature has fo ordered it, that frogs, though when they are grown big enough to deferve that name, they be amphibious animals endowed with lungs; yet before they attain to that pitch, they live wholly in the water like fifhes; I thought it the moft expeditious and leaft improbable attempt we could make, to try whether or no this animal, being as a fifh brought to live, either in our vacuum, or at leaft in highly rarefied air, would not continue to do fo after its lungs fhould be perfectly formed. Wherefore, though I forefaw, and foretold the difficulty that would be met with in the profecution of this experiment, namely, that the aërial bubbles that would be difclofed in fuch foft bodies upon the withdrawing of the preffure of the ambient air, would fo violate the flight texture of thofe tender animals, as to hinder them from living long, or moving freely; yet I thought it very fit to attempt the trial, whereof I find this account among my adverfaria.

EXPERIMENT I.

We took a good company of tadpoles, and put them with a convenient quantity of water into a portable receiver of a round figure, and obferved, that at the firft exfuction of the air they did rife to the top of the water, though moft of them fubfided again, till the next exfuction raifed them. They feemed by their active and wrigling motion to be very difcompofed. The receiver being exhaufted, they continued reftlefs, moving all of them in the top of the water; and though fome of them feemed to endeavour to go to the bottom, and dived fome part of the way, efpecially with their heads, yet they were immediately buoyed up again. Within an hour or little more, they were all movelefs, and lay floating on the water; wherefore I opened the receiver, upon which the air rufhed in, and almoft all of them (which were many) prefently funk to the bottom, but none of them recovered to life.

EXPERIMENT II.

A little after thefe, we included a leffer number of tapdoles in a fmaller glafs, which was alfo exhaufted with the like circumftances with the former. And when I found the other tadpoles to be dead, I hafted to thefe, which did not, except perhaps one, give any fign of life, but upon letting in the air, thefe having not been long kept from

from it, ſome few of them did recover, and ſwam up and down lively enough for ſome time; though after a while they alſo died.

EXPERIMENT III.

Some years after I repeated the ſame experiment in a portable receiver of a convenient kind; and though after the exhauſtion was perfected, the tadpoles did for a while move briſkly enough on the top of the water, none of them appearing able to dive or ſwim under water, yet coming to look on them at the end of an hour, they ſeemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the air upon them, yet all the effect of it was, that the moſt of them immediately ſunk to the bottom, as the reſt of them did a very little while after; none of them, that I could obſerve, recovering any vital motion.

EXPERIMENT IV.

There remains an experiment, which I often judged as well more hopeful as more noble, if I could procure an opportunity to bring my deſign to a trial, which I have found it very difficult to do; nevertheleſs I was able to do it once, though not fully as I deſired, yet not altogether without ſucceſs.

We procured then, and with much ado, ſome of thoſe odd inſects, which I elſewhere deſcribe, whereof gnats have by ſome ingenious men been obſerved to be generated about the end of Auguſt or beginning of September. Theſe, for ſome weeks, live altogether in the water, as tadpoles do, ſwimming up and down therein till they are ripe for a tranſmigration into flies; which itſelf is ſo great a rarity in nature, as makes theſe little creatures recompence to our curioſity the trouble they often give our faces and hands. Suppoſing then, that if I could get ſome of theſe, and include them, being of thoſe inſects they call aquatilia, and ſo minute as they are, they may live a great while in the receiver without air, and in the mean while attain the period, which, according to nature's courſe, is wont to turn them into flies, which might come forth winged creatures into a medium not furniſhed with common air, as others of their kind enjoy; ſuppoſing, I ſay, that theſe inſects would afford me ſome information about theſe particulars, having upon much watching met with four or five of them after a ſhower of rain, that dropped from a houſe into a veſſel laid on purpoſe for it, we included them with ſome of their water into a ſmall glaſs receiver, which being very exactly cloſed, we kept in a ſouth-window, where theſe little creatures continued to ſwim up and down for ſome few days, without ſeeming to be much incommodated by ſo unuſual an habitation; and at the end of that time, and much about the ſame day, they diveſted the habit they had, whilſt they lived as fiſhes, and appeared with their exuviæ or caſt coats under their feet, and ſhewing themſelves to be perfect gnats, that ſtood without ſinking upon the ſurface of the water, and diſcovered themſelves to be alive by their motion, when they were excited to it; but I could not perceive them to fly in that thin medium; to which inability whether the viſcoſity of the water might contribute, I know not, though they lived a pretty while, till hunger or cold deſtroyed them. Something in this experiment may deſerve ſerious reflections; which I cannot ſpare time to offer at.

A digreſſive experiment, concerning the expanſion of blood and other animal juices.

For ſome purpoſes, relating partly to reſpiration, and partly to other inquiries, I thought fit to endeavour to obtain what information could be procured, of the conſiſtence and diſpoſition to expand itſelf of blood and other animal liquors; in purſuance of which the enſuing trials, among others, were undertaken.

The warm blood of a lamb or a ſheep, being taken as it was haſtily brought from the butcher's, where the fibres had been broken, to hinder the coagulation, was in a wide-mouthed glaſs put into a receiver, made ready for it; and the pump being early ſet on work, the air was diligently drawn out; but the operation was not always, eſpecially at firſt, ſo early manifeſt, as the ſpirituouſneſs of the liquor made ſome expect; yet this hindered not, but, after a long expectation, the more ſubtle parts of the blood would begin to force their way through the more clammy ones, and ſeem to boil in large cluſters, ſome as big as great beans or nutmegs; and ſometimes, to the wonder of the by-ſtanding phyſicians, the blood was ſo volatile, and the expanſion ſo vehement, that it boiled over the containing glaſs; of which, when it was put in, it did not, by our eſtimate, fill above a quarter. Having alſo included ſome milk warm from the cow, in a cylindrical veſſel of about four or five inches high, though the operator were induced to pump a great while before any intumeſcence appeared in the milk, yet afterwards, when the external air was fully withdrawn, the white liquor began to boil in a way that was not ſo eaſy to deſcribe, as pleaſant to behold; and this it did for a pretty while with ſo much impetuoſity, that it threw up ſeveral parts of itſelf out of the wide-mouthed glaſs that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A yet greater diſpoſition to intumeſcence, we thought, we obſerved in the gall, which was but ſuitable to the viſcoſity of the texture.

Note, that the two foregoing experiments were made with an eye caſt upon the enquiry that I thought might be made; whether, and how far the deſtructive operation of our engine upon the included animal might be imputed to this, that upon the withdrawing of the air, beſides the removal of what the air's preſence contributes to life, the little bubbles generated upon the abſence of the air in the blood, juices, and ſoft parts of the body, may by their vaſt number, and their conſpiring diſtenſion, variouſly ſtrengthen in ſome places, and ſtretch in others, the veſſels, eſpecially the ſmaller ones, that convey the blood and nouriſhment; and ſo by choaking up ſome paſſages, and vitiating the figure of others, diſturb or hinder the due circulation of the blood; not to mention the pains that ſuch diſtenſions may cauſe in ſome nerves and membranous parts, which, by irritating ſome of them into convulſions, may haſten the death of animals and deſtroy them ſooner, by occaſion of that irritation, than they would be deſtroyed by the bare abſence or loſs of what the air is neceſſary to ſupply them with. And to ſhew, how this production of bubbles reaches, even to very minute parts of the body, I ſhall add on this occaſion, hoping, that I have not prevented myſelf or any other, what may ſeem ſomewhat ſtrange, what I once obſerved in a viper, furiouſly tortured in our exhauſted receiver, namely, that it had manifeſtly a conſpicuous bubble moving to and fro in the wateriſh humour of one of its eyes.

Another digreffive experiment belonging to the fame title.

To fhew, that not only the blood and liquors, but alfo the other foft parts, even in cold animals, have aërial particles latitant in them; we took the livers and heart of an eel, as alfo the head and body of another fifh of the fame kind, cut afunder crofs-ways fomewhat beneath the heart; and putting them into a receiver, upon the withdrawing of the air we perceived that the liver did manifeftly fwell every way, and that both the upper and lower parts did fo likewife; and at the place where the divifion had been made, there came out in each portion of the fifh divers bubbles, feveral of which feemed to come from the *medulla fpinalis*, or the cavity of the back-bone, or the adjoining parts; and the external air being let in both the portions of the eel prefently fhrunk, fome of the fkins feeming to be grown empty or flaccid in each of them.

TITLE XIV.

Of the power of affuefaction to enable animals to hold out in air, by rarefaction made unfit for refpiration.

"THE power of affuefaction in other cafes, made me think it very well worth trying "what it would do in refpiration; and the rather, becaufe I prefumed it might prove "an experiment of good ufe, if we fhould difcover, that by a gradual accuftomance an "animal may be brought to live, either in a much thinner air, or much longer in the "fame air, than at firft he could. But in regard, that to make fuch a trial perfpicuoufly "enough, the opacity of the bladder made ufe of in the former title was like to be an "impediment, I devifed another way to obviate that inconvenience, which may, I "hope, be competently underftood by the heedful perufal of the following trials."

EXPERIMENT I.

WE included in a round phial with a wide neck (the whole glafs being capable of containing about 8 ounces of water) a young and fmall moufe, and then tied ftrongly upon the upper part of the glafs's neck a fine thin bladder, out of which the air had been carefully expreffed, and then conveyed this phantaftical veffel into a middle-fized receiver, in which we alfo placed a mercurial gage (adjufted by our elfewhere mentioned ftandard) this done, the air was by degrees pumped out, until it appeared by the gage, that there remained but a fourth part in the external receiver (as for diftinction fake I call it) whereupon the air in the internal receiver expanding itfelf, appeared to have blown the bladder almoft half full, and the moufe feeming very ill at eafe by his leaping, and otherwife endeavouring to pafs out at the neck of his uneafy prifon; we did, for fear the over thin air would difpatch him, let the air flow into the external receiver, whereby the bladder being compreffed, and the air in the phial reduced to its former denfity, the little animal quickly recovered.

EXPE-

EXPERIMENT II.

A WHILE after, without removing the bladder, the experiment was repeated, and the air, by the help of the gage was reduced to its former degree of rarefaction, and the moufe, after fome fruitlefs endeavours to get out of the glafs, was kept in that thin air for full 4 minutes; at the end of which he appeared fo fick, that, to prevent his dying immediately, we removed the external, and took out the internal receiver; whereupon, though he recovered, yet it was not without much difficulty, being unable to ftand any longer upon his feet, and for a great while after continued manifeftly trembling.

EXPERIMENT III.

BUT having fuffered him to reft a reafonable fpace of time, prefuming, that affuefaction had accuftomed him to greater hardfhips; we conveyed him again into the external receiver, and having brought the air to the former degree of expanfion, we were able to keep him there for a full quarter of an hour; though the external receiver did not at all confiderably leak; as appeared, both by the mercurial gage, and by the continuing diftenfion of the bladder. And it is worth noting, that, till near the latter end of the quarter of an hour, not only the animal did fcarce at all appear diftreffed, remaining ftill very quiet; but, which is more, whereas when he was put in, the tremblings formerly mentioned were yet upon him, and continued fo for fome time; yet afterwards, in fpite of the expanfion of the air he was then in, they left him early enough. And when the internal receiver was taken out, he did not only recover from his fainting fit fooner than before; but efcaped thofe fubfequent tremblings we have mentioned.

EXPERIMENT IV.

ENCOURAGED by this fuccefs, after we allowed him fome time to recollect his ftrength, we reconveyed him and the odd veffel, wherein he was included, into the former receiver, and pumped out the air, till the mercury in the gage was not only drawn down as low as formerly, but near half an inch lower, that there the air might be yet further expanded than hitherto it had been. And though this did at firft feem to difcompofe our little beaft; yet after awhile he grew very quiet, and continued fo for a full quarter of an hour; when being defirous to try what operation a farther rarefaction of the air would have upon him, we caufed three exfuctions more to be made by the pump, before we difcovered him to be in manifeft danger (at which time the bladder appeared much fuller than before) but then we were obliged to let the air into the outward receiver; whereupon the moufe was more fpeedily revived than one would have fufpected.

AND thefe trials of the power of affuefaction feemed the more confiderable, becaufe the air, in which the moufe had all this while lived, had been clogged and infected with the excrementitious effluviums of his body; for it was the fame all along, we having purpofely forborn to take off the bladder, whofe regular intumefcencies and fhrinkings fufficiently manifefted, that the veffel, whereof it was a part, did not leak.

POST-

POSTSCRIPT.

" THOUGH the ſucceſs of the recited experiments is very promiſing; yet a ſubſequent " trial or two, whoſe particularities are ſlipped out of my memory, oblige me in point " of candour, to declare, that for further ſatisfaction, the trials of the power of accuſ- " tomance, in reference to air unfit for reſpiration, ought to be both reiterated, and " to be made in differing ſorts of animals."

TITLE XV.

Some experiments ſhewing, that air, become unfit for reſpiration, may retain its wonted preſſure.

EXPERIMENT I.

WE took a mouſe of an ordinary ſize, having (not without ſome difficulty) conveyed him into an oval glaſs, fitted with a ſomewhat long and conſiderably broad neck, which we had provided, that it might be wide enough to admit a mouſe in ſpite of his ſtruggling. We conveyed in after him a mercurial gage, in which we had diligently obſerved, and marked the ſtation of the mercury, and which was ſo faſtened to a wire, reaching to the bottom of the oval glaſs, that the gage, remaining in the neck, was not in danger to be broken by the motions of the mouſe in the oval part; the upper part of the long neck of the glaſs was, notwithſtanding the wideneſs of it, hermetically ſealed by the help of a lamp and a pair of bellows, that we might be ſure that the impriſoned animal ſhould breathe no other air than that which filled the receiver at the time when it was nipped up. This done, the mouſe was watched from time to time; and though by reaſon of the largeneſs of the veſſel, in compariſon of ſo ſmall an animal, he ſeemed to me rather drooping, than very near death at the end of the ſecond hour; yet coming to look upon him about half an hour after, he was judged by the ſpectators quite dead, notwithſtanding our ſhaking of the veſſel to rouze him up. This made me caſt my eyes upon the gage, wherein I could not perceive any ſenſible change of the mercury's ſtation. But being unwilling to give over the mouſe, without trying what freſh air would do to recover him, I cauſed the ſealed part of the glaſs to be broken off, and, notwithſtanding that his continuing to appear dead increaſed the confidence of thoſe that thought him ſo, I obtained after a while ſome faint tokens of life; though I am not ſure that they would have continued in a veſſel, where the air was ſo clogged and infected, if it had not been that freſh air was frequently blown in by a pair of bellows, whoſe noſe was inſerted into the neck of the glaſs. This freſh air ſeemed evidently, though but ſlowly, to revive the gaſping animal, whom I would not nor could not conveniently take out of the glaſs, till he had gained ſtrength enough to make uſe of its legs; after which, without breaking of the glaſs (which I was loath to loſe, having then no other of the kind) we took him out, and found him quickly able to go up and down. After which ſervice, and another trial we had with him, which belongs not to this place, we ſet him at liberty to ſhift for himſelf.

EXPE-

EXPERIMENT II.

Such an experiment as the former we made with like fuccefs upon a fmall bird included with a gage in a receiver holding about a quart of water. The bird in about half an hour appeared to be fick and drooping, and the faintnefs and difficulty of breathing increafed for about two hours and a half after that, at which time the animal died, the gage being not fenfibly altered, unlefs perhaps the mercury appeared to be impelled up a little thort higher than it was when put in; which yet might well enough proceed from fome accidental caufe.

EXPERIMENT III.

To fatisfy fome curious perfons, that it is not want of coldnefs, but fomething elfe in the included air that makes it deftroy the birds that are pent up in it, and by the hot exhalations that fteam from their bodies, may be fuppofed to overwarm it, we made the following experiment.

In a glafs-phial, capacious enough to hold about three quarts of water, we not only included, but for greater accuracy, hermetically fealed up a fmall bird, and found, that in a few minutes he began to be fick and pant; which fymptoms I fuffered to continue and increafe againft the mind of a learned by-ftander (who thought the animal would not hold out fo long) till they had lafted juft half an hour; at which time, having provided a veffel of water with fal armoniack, newly put into it, to refrigerate it (according to the way I elfewhere publifhed); and the liquor thus made exceeding cold, fomewhat to the wonder of thofe that felt it; the phial, with the fick bird, was immerfed in it, and kept there in that condition for fix minutes; and yet it did not appear, in the judgment of the by-ftanders, that the great refrigeration, that muft be this way procured to the imprifoned air, did fenfibly revive or refrefh the drooping animal, who manifeftly continued to pant exceedingly as before, and, as fome affirmed, more; fo that this remedy proving ineffectual, the phial was removed out of the water, and the bird fome time after did, as I foretold, make many ftrains to vomit (though fhe brought up little) followed by evacuations downward, before fhe quite expired, which fhe did within a minute or two of a juft hour, after the beginning of her imprifonment.

If I had been able (which I was not) to procure more birds, I would willingly have profecuted this experiment by feveral other, not unhopeful trials, which for want of fubjects I was fain to leave only defigned.

TITLE XVI.

Of the ufe of the air to elevate the fteams of bodies.

In the digreffion about refpiration annexed to the 41ft of our Phyfico-mechanical Experiments formerly publifhed, it is propofed as one of the confiderable ufes of the air in refpiration, that, being drawn into the lungs, it ferves to carry off with it, when it is breathed out again, the recrementitious fteams that are feparated from the mafs of blood in its paffage through the lungs; from which fuliginous excrements, if the blood were not continually freed by the help of the air, after nature had been accuftomed to that

way

way of discharging them, their stay in the body might have very great and destructive operations on it.

For the illustration of this use of the air, I shall now subjoin the following experiment.

We made by distillation a blood-red liquor, which chiefly consisted of such saline and spirituous particles as may be obtained from the mass of blood in human bodies; this liquor is of such a nature, that if a glass phial, about half filled with it, be kept well stopped, the red liquor will rest as quietly as any ordinary one, without sending up any smoke or visible exhalation; but if the phial be unstopped, so that the external air be permitted to come in and touch the surface of the liquor, within a quarter of a minute or less, there will, upon this contact, be elevated a copious white smoke, which will not only fill the upper part of the glass, but plentifully pass out into the open air, till the phial be again stopped.

My purpose in this tract to forbear sidings in controversies keeps me from taking notice of the speculations suggested by some of the phænomena of this liquor; which yet I thought I might lawfully mention, as far as I have done it, because it but adventures upon giving one of the uses rather of the air, than immediately of respiration itself; and is brought but to illustrate what I have not found denied by any, though considered by very few; namely, the office of the air to carry off in expiration the fuliginous steams of the lungs. For in our experiment we manifestly see, that the very contact of the air may give the corpuscles of moist bodies a peculiar volatility or facility to emerge in the form of steams. I know, there are some corrosive spirits, as in nitre and salt, simple, or compounded of them, that, when they are very strong, emit for a while manifest fumes, but the difference of those liquors, and their inferiority to our red spirit, in the capacity of smoking liquors, might easily enough be manifested, if it were judged proper in this place, where it may suffice to take notice of these two things. The one is, that when the phial has lain stopped and quiet a competent time, the upper half of it will appear destitute of fumes, of which the air, it seems, will imbibe, and constantly retain but a certain moderate quantity; which may give some light towards the reason, why the same air, which will be quite clogged with steams, will not long serve for respiration, which requires frequent supplies of fresh air. The other is, that if the unstopped phial were placed in our vacuum, it would not emit any visible steams at all, nor so much as to appear in the upper part of the glass itself that held the liquor; whereas, when the air was by degrees restored at the stop-cock, without moving the receiver itself, to avoid injuring its closeness, the returning air would presently raise the fumes, first into the vacant part of the phial, whence they would ascend into the capacity of the receiver; and likewise, when the air that was requisite to support them was pumped out, they also accompanied it, as their unpleasant smell evinced, and the red spirit, though it remained unstopped, emitted no more fumes till the new air was let in.

One may compare with this liquor another smoking one, mentioned in the 29th of the first published pneumatical experiments, where an experiment is related of it, that has something in common with this, and may so far serve to confirm what is now delivered, as this also has some things additional to that; besides that, that liquor being made with ingredients corrosive, and of a bad name among chemists themselves, the fumes that proceed from it may fright many from daring to meddle with it, whereas this our red spirit has been found potently medicinal for some distempers of the lungs, by a doctor of physick, whom I desired to try it. The other phænomena of this liquor I shall not stay to describe, as not belonging to this place; and the liquor itself, with very little variation, I have, in the History of Colours, communicated.

TITLE

TITLE XVII.

Of the long continuance of a flow-worm and a leach alive, in the vacuum made by our engine.

In the often cited digreffion about refpiration, there is mention made of the great vivacioufnefs of houfe-fnails, as they call them, and how little operation the withdrawing of the air had upon them in comparifon of what it is wont to have on other animals. I fhall now add by way of confirmation, that I made trial upon ordinary white fnails, without fhells, whereof two of differing fizes (the biggeft about an inch and a half, and the other about an inch in length) were included in a fmall portable receiver, which being carefully exhaufted, and fecured againft the return of the air, was attentively confidered by me, prefently after it was removed from the engine; whereby it was eafy to difcern, that both the fnails thruft out and retracted their horns (as they are commonly called) at pleafure, though their bodies had in the fofter places pretty ftore of newly generated bubbles fticking to them; but though they did not lofe their motion near fo foon, as other animals were in our vacuum wont to do; yet coming to look on them after fome hours, they appeared movelefs and very tumid, and at the end of twelve hours, the inward parts of their bodies feemed to be almoft vanifhed, and they feemed to be but a couple of fmall full-blown bladders; and on the letting in of the air they immediately fo fhrunk, as if the bladders having been pricked, the receding air had left behind it nothing but fkins; nor did either of the fnails afterwards, though kept many hours, give any figns of life.

Upon a fuppofition, that the cold and clammy conftitution of fnails might be a main caufe of their being able to endure the abfence of the air fo well, I thought it worth trial, whether effs and leaches might not yet be more able to continue in our vacuum than a fnail; and accordingly fome experiments were made purfuant to that curiofity; the moft fully regiftered whereof are thefe that follow.

EXPERIMENT I.

We included in a receiver, whofe globular part was about the bignefs of a large orange, one of that fort of animals, that they vulgarly call effs: having withdrawn, but not folicitoufly, the air, and fecured the veffel againft the unpermitted return of it, we kept him there about eight-and-forty hours, during all which time he continued alive, but appeared fomewhat fwelled in his belly; his under-chap moving the very firft night, but not the day and night following. By opening the receiver at length under water, we perceived, that about half the air had been drawn out. As foon as the water was impelled into the glafs, the animal, that was before dull and torpid, feemed, by very nimble and extravagant motions, to be ftrangely revived.

EXPERIMENT II.

We took a leach that was of a moderate bignefs, or fomewhat fhort of it; and having included it, together with fome water, in a portable receiver that was gueffed to be capable of holding about 10 or 12 ounces of that liquor, the air was pumped out after the ufual manner, and the receiver being removed to a lightfome place, we obferved, as we expected, that the leach keeping himfelf under water, there emerged, from divers parts of her body, ftore of bubbles, fome of them in a difperfed way, but others in rows or files, if I may fo fpeak, that feemed to come from determinate points. Though this production of bubbles lafted a pretty while, yet the leach did not feem to be very much difcompofed by her prefent condition. This done, we difpofed of the receiver, which was well fecured from the ingrefs of the outward air, into a quiet place, where we daily vifited it once at leaft, or oftener, as there was occafion; and found the leach fomewhat faftened by her tail to that part of the glafs that was under water, and fometimes wandering about that part which was quite above water; and ftill, when we endeavoured to excite her, fhe quickly manifefted herfelf to be alive; and indeed (which will be thought ftrange) appeared fo lively after the full expiration of five natural days, that expecting fomething might have happened to the receiver, and thereupon refolving to try how ftanch it had continued, I opened it under water, by which means the outward air impelled in fo much of that liquor, that I was fatisfied, the receiver was immediately before as well exhaufted, as others are wont to be in our pneumatical experiments.

TITLE XVIII.

Of what happened to fome creeping infects in our vacuum.

NOTWITHSTANDING the great variety of reptiles, that nature does almoft every where produce; yet the inconvenient time and place, wherein the following trials were made, fupplied me with fo few, that about thofe animals I find among my adverfaria, no more than the enfuing notes.

EXPERIMENT I.

WE took five or fix caterpillars of the fame fort; but I could not tell, to what ultimate fpecies the writers about infects referred them. Thefe being put into a feparable receiver of a moderate fize, had the air drawn away from them, and carefully kept from returning. But notwithftanding this deprivation of air, I found them about an hour after moving to and fro in the receiver; and even above two hours after that, I could, by fhaking the veffel, excite in them fome motions that I did not fufpect to be convulfive: but looking upon them again fome time before I was to go to bed (which may be was about 10 hours after they were firft included) they feemed to be quite dead; and though the air were forthwith reftored to them, they continued to appear fo until I went to bed; yet, for reafons elfewhere expreffed, I thought fit to try, whether time might not at length recover them, and leaving them all night in the receiver, I found the next day, that three, if not four of them, were perfectly alive.

EXPERIMENT II.

WE took from an hedge a branch that had a large cobweb of caterpillars in it; and having divided it into two parts, we put them into like receivers, and in one of them fhut

fhut up the caterpillars together with the air, which from the other was exhaufted. The event was, that in that, which had the air, the little and difficultly vifible infects, after a fmall time, appeared to move up and down as before, and fo continued to do for a day or two; after which, other occafions made the experiment to be neglected; whereas that glafs, whence the air had been drawn out, and continued kept out, fhewed after a very little while no motion, that we could perceive. But to try, whether caterpillars may continue fo far alive in our vacuum all the winter, as the next fpring or fummer, to proceed in the tranfmigration to a butterfly, is a trial, that we have but begun, and therefore muft not pretend to fay any thing about its event.

TITLE XIX.

Of the phænomena fuggefted by winged infects in our vacuum.

WHEN our Phyfico-mechanical Experiments were difpatched to the prefs, the inconvenient feafon of the year, and the difficulty of making the receivers I then employed to keep out the air for any long time, hindered me from then publifhing above a trial or two of what will happen to winged infects in our vacuum. But afterwards being provided of more commodious veffels, I thought fit at feveral times to repair that omiffion by various attempts, whereof the chief enfue.

EXPERIMENT I.

Nov. 12. about 8 a clock at night.

THERE were taken four middle-fized flefh-flies, which having their heads cut off, were inclofed in a portable receiver, furnifhed with a pretty large pipe, and a bubble at the end. As foon as the receivers were exhaufted, thofe flies loft their motion, which was not brifk before; an hour or two after, I approached them to the fire, which reftored not their motion to them (but as to one of them, I fufpect it had a languid motion for a while) wherefore I let in the air upon them, after which, in a very fhort time (though not immediately) they began one after another to move their legs, and one or two of them to walk; and having kept them all night in a warm place, when I fent one the next morning to try, if they would manifeft any motion, he told me, that for a while they did, though when I afterwards rofe myfelf, I could not perceive any motion in them.

EXPERIMENT II.

Sept. 11.

ABOUT noon we clofed up divers ordinary flies, and a bee or wafp; all which, when the air was fully withdrawn, lay as dead, fave that for a few minutes fome of them had convulfive motions in their legs. They continued in this ftate 48 hours, after which, the air was let in upon them, and that not producing any figns of life in them, they were laid in the meridian fun, but not any of them feemed in any degree to recover.

EXPERIMENT III.

Dec. 11. WE put a great flefh-fly into a very fmall portable receiver, where, at firft, it appeared to be very brifk and lively, but as foon as the air was drawn out, fell on his back, and feemed to have convulfive motions in her feet and probofcis; from whence fhe prefently recovered upon the letting in of the air, which being drawn out again, fhe lay as dead; but a while after (within a quarter or half an hour) I perceived, that upon fhaking the receiver, fhe ftirred up and down, but faintly. This was done pretty late yefternight, fince when I had not occafion to look on the glafs, till this night after fupper, when I found the fly not (whilft I ftaid to endeavour it) to be recovered either by warmth, or letting in the air. A while after this note was written, this fly recovered; and being next morning fealed up again in that glafs, and kept 48 hours, though over the chimney, died for good and all.

EXPERIMENT IV.

WE took a large grafhopper, whofe body, befides the horns and limbs, was about an inch in length, and of a great thicknefs in proportion to that length; this we conveyed into a portable receiver of an oval form, and capable of holding, by our guefs, about a pint of water and more; and having afterwards pumped out the air, till by the gage it appeared to have been pretty well drawn out, we took care, no air fhould re-enter to difturb the experiment. The fuccefs whereof was this; firft, though, before the exhauftion of the air was begun, the grafhopper was ftirring, and lively, and continued fo for a while after the beginning of the operation; yet, when the air began to be confiderably rarefied, he appeared to be very ill at eafe, and feemed to fweat out of the abdomen many little drops of liquor, which being united, trickled down the glafs like a little ftream, which made at the bottom a fmall pool of clear liquor, amounting to near a quarter of a fpoonful, and by that time the receiver was ready to be taken off, the grafhopper was fallen upon his back and lay as dead. Secondly, though having a little after laid the glafs in a fouth window, on which the fun then fhone, I perceived fome flow motions in the thorax, as if he ftrained to fetch breath; yet I was not fure they were not convulfive motions, and whatever they were, they lafted but a while, and then the animal appeared to be quite dead, and to continue fo for three hours from the removal of the receiver. Thirdly, that time being expired, the glafs was opened, and the air let in upon him; notwithftanding which there appeared no fign at all of life; but imagining there might be fome time requifite to recover him out of fo deep a fwoon, I let the glafs reft in a convenient pofture, that the water that came from him might not endanger him, for a quarter or half an hour; and though I then perceived no figns of life, yet being defirous to purfue the trial yet further, I caufed him to be carried into a fun-fhiny place, where the beams of a declining fun prefently began to make him ftir his limbs, and, in a fhort time, brought him perfectly to life again.

EXPERIMENT V.

April 15. WE took one of thofe fhining beetles they call rofe-flies, and having included it in a very fmall round receiver, which we exhaufted; and though he that attended the engine, affirmed,

affirmed, it ſtruggled much whilſt the air was withdrawing, yet preſently after I could perceive but little motion, and part of that ſeemed almoſt convulſive; and afterward going abroad, and not returning to look on the glaſs till about ſix hours after, the fly ſeemed quite dead, and diſcovered not any motion upon that of the glaſs. And within about an hour after, though I let the air ruſh in, yet no ſign of life enſued, neither immediately, nor for a pretty while after. So that ſuſpecting the fly to be really dead, and yet not reſolutely concluding it, though I would then wait no longer, yet three or four hours after (viz. about 10 of the clock at night) I returned to the receiver, and found the beetle lively enough. Whereupon I cauſed the glaſs to be again exhauſted, and ſecured from the ingreſs of the air, during which time the animal ſeemed to be much diſquieted by what was done to it, but did not loſe its motion before I went to bed, which was ſoon after.

EXPERIMENT VI.

About butterflies, I remember, I made ſeveral trials, moſt of which chanced to be loſt; but thus much I very well remember, that having obſerved them not only to live, but to move longer than was expected, I choſe to include divers of them in receivers ſomewhat large, eſpecially that I might ſee, whether in ſo thin a medium ſome or other of them, by the help of their large wings, would be able to fly. But though, whilſt the air continued in the glaſſes, they flew actively, as well as freely, up and down; and though after the exhauſtion of the air, they continued to live, and were not moveleſs; nay, though at the bottom of the receiver they would even move their wings, and a little flutter, yet I could not perceive any of them to fly, by which I mean, perform any progreſſive motion, ſupported by the medium only. And by frequently inverting the receiver (which I took care ſhould be pretty long, to let them fall from one extreme to the other) they would fall like dead animals without diſplaying their wings, though juſt as they came to touch the bottom, ſome of them would ſometimes ſeem to make ſome uſe of them, but not enough to ſuſtain themſelves, or to keep their falls from being rude enough.

TITLE XX.

Of the neceſſity of air to the motion of ſuch ſmall creatures, as ants, and even mites themſelves.

"In the experiments hitherto mentioned, the animals, on which the trials have been "made, were divers of them of a moderate bulk; and others of them, though ſmall, "yet not of the leaſt ſizes that nature afforded us. Wherefore I thought fit to annex the "following experiments, wherein I deſigned to examine. whether even thoſe minute "ſorts of animals, whoſe bulk is thought the moſt contemptible, have not, as well as "the greater, need of the air, if not to make them live, yet at leaſt to enable them to "move."

A pretty number of ants were included in a ſmall portable receiver exhauſted yeſterday about noon: between ſix and ſeven in the afternoon, they ſeemed to be all quite dead, and the rather, becauſe, though they were very lively juſt before they were ſealed up, running briſkly up and down the bubble they were in; yet they grew almoſt moveleſs as ſoon as the air was exhauſted; and a little while after appeared more ſo; though I then

I then ſuſpected more than I ſince did, that they were much inconvenienced by ſome ſmall glutinous ſubſtance that ſeemed to have got into the ſmall receiver from the vapours of the cement. When I looked on them at the time lately mentioned, I opened the glaſs, whereupon the air ruſhed in; but no ſign of life appeared for a great while in any of the ants; but looking upon them this morning about 9 a clock, I found many of them alive, and moving to and fro.

" It is ſaid by naturaliſts upon the authority of *Ariſtotle*, that the animal, the Greeks " call ἄκαρι, is the minuteſt of living creatures. But thoſe of this ſort being very hard, " if at all to be met with here, I thought fit to make ſome experiments upon the leaſt " of the terreſtrial animals I could procure, and try, whether or no mites themſelves, " which are reputed but living points, and not to be taken notice of by the naked eye " to be living, but by motions, which even an attentive one can ſcarce diſcover, ſtand " in need of the air; eſpecially, becauſe, in caſe they do, it may ſuggeſt to us ſome odd " reflections upon the ſtrange ſubtilty and minuteneſs of the aërial particles, which muſt " be capable of flowing in, and paſſing out, at the inviſible and almoſt in-imaginable " ſmall pores, and other cavities of the parts of an animal, whoſe entire body is reputed " but a phyſical point."

We conveyed then a pretty number of mites, together with the mouldy cheeſe they were bred in to nouriſh them, into three or four portable receivers (which were all of them very ſmall) not much differing in ſize. From all of theſe, ſave one, we withdrew the air; and then, making uſe of our peculiar contrivance to hinder its return, we took them one after another from the engine, and laid them by, for further obſervation. That one, which I took notice that we had reſerved, and, in which, to obſerve the difference, we thought fit to leave the air, was ſealed at a lamp-furnace, after the uſual manner of nipping up glaſſes there. This done, there remained nothing but to obſerve the event of our trials, which afforded us the enſuing phænomena.

1. Those mites that were incloſed in the ſmall glaſs, that never came near the engine, continued alive, and able to walk up and down for a full week after they had been put in; and poſſibly would have continued much longer, if the glaſs had not been accidentally broken.

2. As ſoon as ever one of the receivers was removed from the engine, I looked with great attention upon it, and though juſt before the withdrawing the air, the mites were ſeen to move up and down in it; yet, within a few minutes after, the receiver was applied to the engine, I could diſcern in them no life at all, nor was any perceived by ſome younger eyes than mine, whereunto I expoſed them. Nay, by the help of a double convex-glaſs (that was ſo ſet in a frame, as to ſerve me as a microſcope on ſuch occaſions) I was not able to ſee any of them ſtir up and down. Nor was any motion taken notice of in the other ſmall receiver of like bigneſs and ſhape with mine, by them that had exhauſted it of air. And my occaſions not permitting me to attend the obſervation any longer in the place where it was made, I took the receiver, I had ſo attentively conſidered myſelf, along with me in the coach; and having occaſion to make ſome ſtay, about an hour after I looked upon it attentively again, but could not perceive any of the mites to ſtir; and the like unſucceſsful obſervation I made, when I had a conveniency, two or three hours after that. And the place I did it in, being one, where I thought myſelf, as it were, at home, I firſt let in the air, to try if the mites were not quite dead; and though neither upon its ruſhing in, nor during my ſtay there, I could perceive any of them to ſtir; yet I left the receiver unſtopped, as it was in the window, upon a ſuſpicion, that the air might not be able to produce its operation upon them in a ſhort time.

3. And therefore paſſing by the ſame place about two or three days after, I called in to look upon my receiver, and found a number of my little animals revived, as an attentive eye might eaſily perceive by the motion of certain little white ſpecks, when it was helped to obſerve it by little marks I made on the outſide of the glaſs (which was purpoſely choſen thin and clear) near this or that mite, with a diamond; by the approach to, or receſs from which marks, the progreſſive motion became (perhaps within a minute) plainly diſcoverable; eſpecially, if we uſed the following expedient (which I found the beſt of thoſe I tried) namely, that when the eye perceived little white ſpecks that looked like mites, the receiver ſhould be ſo turned and returned, that the bellies and feet of thoſe little creatures were uppermoſt; notwithſtanding which, they would not eaſily drop down, but continue their motion; which ſpecks being made upon the concave ſurface of the thin glaſs itſelf (to which you may approach your eye as much as you pleaſe) are thereby rendered much more eaſily viſible. But, this being only intimated upon the by, I proceed to take notice, that in the newly mentioned receiver, the mites did, by ſtirring up and down, continue to appear alive for two or three days after, if not longer. I ſhould not, I confeſs, have thought it ridiculous to ſuſpect, that the mites, which at firſt loſt their motion, did at laſt really die; and that thoſe I after ſaw ſtirring up and down, were others newly generated in the included mouldy cheeſe; but I was not apt to think this ſuſpicion probable, not only becauſe of the extreme difficulty of making any living creature to be generated *in Vacuo Boyliano*, but becauſe it did not ſeem agreeable to what I elſewhere noted, about the way and time of the propagation of mites, whoſe eggs I have divers times obſerved with pleaſure, that at a ſeaſon of the year that was not favourable (for theſe things happened in a cold *March*) newly generated mites ſhould in two or three days grow up to their juſt bigneſs, which ſeveral of thoſe we obſerved, ſeemed to have attained.

4. But, becauſe it doth not by the third phænomena appear, whether or no, in caſe our mites had been kept in a moveleſs ſtate for a much conſiderabler time than three or four hours, they would have been recoverable by the admiſſion of the air; I ſhall add, to ſatisfy that doubt, that one of the portable receivers above-mentioned, being exhauſted, and carefully ſecured from the regreſs of the air, was kept from Monday morning to Thurſday morning: after all which time, our attentive eyes being unable to diſcover any ſigns of life among the included mites, the air was let in upon them, and after no long time, had ſuch an operation upon them, that both I and others could plainly ſee them creep up and down in the glaſſes again.

SOME

SOME

CONSIDERATIONS

TOUCHING THE

USEFULNESS

OF EXPERIMENTAL

NATURAL PHILOSOPHY.

Propofed in a familiar Difcourfe to a Friend, by way of Invitation to the Study of it.

The Second Tome, containing the latter Section of the Second Part.

The PUBLISHER to the READER.

WHEREAS the preface of the noble author to this fecond tome of the ufefulnefs of experimental philofophy, was written with defign it fhould come forth a year or two before the laft, it is fit, that fomething be now added about the prefent publication.

First, if enquiry be made, why the effays, that now come abroad, are not accompanied with thofe others, that, according to the forts of the titles, fhould precede fome of them; he reprefents, that it was not thought fit, that thofe, that are now publifhed, having no neceffary dependence on the reft, and being fufficiently intelligible without them, fhould ftay for difcourfes, that are not at prefent ready, and perhaps will not fuddenly be fo; partly, in regard they confift of no fmall number of loofe papers, which by by reaſon of ſome, yet inſuperable, obſtacles (of which want of health is none of the leaſt) he cannot conveniently ſeek out, range, and complete; and partly, becauſe he cannot, in the place where he is now detained, be maſter of divers uncommon minerals, and ſome chymical productions, whoſe deſcriptions through haſte he omitted, becauſe he had them at hand in the place, where thoſe eſſays were written, and preſumed, he could at leiſure fill up thoſe vacancies he left for ſuch deſcriptions.

Secondly, as to the eſſays themſelves, which, for the reaſons juſt now mentioned, come not abroad with the reſt, though the excellent author hath of late years conſtantly refuſed to promiſe any thing to the publick, yet, that the reader may the better judge of the ſcope and deſign of the whole treatiſe, he will not deny him an intimation of what ſubjects thoſe eſſays relate unto, by telling him that one of them treateth of the uſefulneſs of chymiſtry (not to phyſick, but) to the empire of man over the inferior works of nature: another, of the advantages, that a naturaliſt's country may derive from his curioſity: another, of the mutual aſſiſtance, that the ſpeculative and practical part of phyſiology may afford each other: after which comes a diſcourſe, containing inducements to hope for much greater things from experimental philoſophy, than men have hitherto obtained.

Lastly, as to what the author taketh notice of, about the coincidents of ſome experiments, that may be mentioned as well by others as by him; it is very poſſible, that the ſame things may, by the ſame, or other ways, come to the knowledge of different perſons. Beſides, that I have heard him mention with ſome complaint, that, when divers years ſince, he writ ſeveral diſcourſes (whereof ſome belonged to the uſefulneſs of experimental philoſophy) for the uſe of a private friend, not for the preſs, he was not ſo ſhy, as had been requiſite, of ſhewing divers experiments, and of imparting others in diſcourſe, to inquiſitive men, whether Engliſh or foreigners, that came to viſit him; divers of which things he afterwards found in print, ſometimes indeed with, but for the moſt part without, mention of his name. So, that ſometimes his unwillingneſs to diſoblige ſuch writers, and to contend about ſuch matters, made him either wholly omit ſome of the particulars he afterwards intended to publiſh, or even to croſs out ſeveral paſſages that he had already written, where he would without much inconvenience (for, that did not always happen) either quite leave them out, or ſubſtitute others (though leſs proper) in their ſtead. He added alſo, that ſometimes obſerving his notions and experiments to be aſcribed to other writers, and ſomewhat wondering at it, he found indeed ſuch writers to have mentioned ſuch things, but in editions that came abroad after the publication of our author's writings; from whence ſuch things might, with the greater likelihood be preſumed to have been borrowed, both becauſe ſome of the writers had converſed with him, and he could not find them in the firſt edition of ſuch books. But theſe unfair proceedings being the faults but of a few, he ſaid, he was far from imputing them to the generality of thoſe, that have mentioned (which divers of thoſe have very civilly done) his experiments, or writings in theirs.

The particulars being thus taken notice of, the curious reader ought not to be any longer detained from converſing with the author himſelf in this inſtructive treatiſe. Farewell.

The PREAMBLE.

I HAVE, in the preface, and body of the former, and already publiſhed part of this treatiſe, taken notice of ſo many of the things that concern the whole work in general, that I preſume it will not here be neceſſary to detain the reader with any other particulars, than thoſe, that will be offered by way of anſwers to ſome queſtions, that are like to be aſked about the publication of this preſent tome.

And in the firſt place, if it be demanded, why this latter part did not more cloſely follow the former, I have this to anſwer; that the papers it conſiſted of, chanced to be ſo unfortunately diſpoſed of, during the late publick confuſions, that for a great while I was not the maſter of them, and in the mean while was, ſometimes upon one occaſion, and ſometimes upon another, engaged to venture abroad the Hiſtory of Colours, the Hiſtory of Cold (with the preliminary and additional tracts) Hydroſtatical Paradoxes, and the Origin of Forms and Qualities; the publication of which treatiſes, beſides that of ſome anonymous papers, as it took up much of the time I had to ſpare for the preſs; ſo it may, I ſuppoſe, keep it from being thought ſtrange, that I did not trouble myſelf and others with this book alſo. And indeed, this having been (as the ſcope and divers paſſages of it ſufficiently intimate) one of the firſt I wrote to the gentleman I call *Pyrophilus*, I had occaſion, whilſt it was out of the way, to make uſe of ſo many of the experiments and obſervations, that belonged to it, that fearing I had thereby too much robbed, and disfigured it, to leave it any way fit for publick view, I had the greater temptation to neglect the looking after it.

But if it be further demanded, why then, ſince it was not ready to come out more early, I did not condemn it not to come out at all? I have two things to return by way of anſwer.

The firſt is, that ſome eminent virtuoſi, to whom I owe a peculiar reſpect, were pleaſed to challenge the edition of this tome, as if I had made myſelf a debtor to the publick for the ſecond part of this work, by having ſuffered what I wrote to a private friend to be divulged in the firſt. Eſpecially ſince the publick had given that ſo very favourable an entertainment; as, beſides other things, the early reprinting of it manifeſted.

The other part of my anſwer, and that, which made the former conſideration prevalent, is, that I was overcome, either by the reaſons, or by the authority, of thoſe ingenious perſons, that were pleaſed to think, that this work would not prove unſerviceable to mankind, to whoſe good, both as a man, and Chriſtian, I have been long ambitious to contribute, as well upon the account of the great Author and divine Redeemer of men, as of that common nature, whereof all men partake. What the utilities of this work were conceived to be, the reader will find diſcloſed at the end of this preface. To which I will therefore refer him for an account of them; and now only take notice, that as to one of the ſcruples I had againſt the publication, namely, that I had plundered this preſent treatiſe of divers particulars, wherewith I had accommodated ſome of my other writings; I could not well reject this anſwer, that in ſo many years as had paſſed ſince the writings of this book, I had not been ſo negligent a commercer with the works of nature and art, as not to be able to make ſome amends for

for what I had taken away, and eaſily ſubſtitute other experiments and obſervations, to ſupply the vacancies left by thoſe I had transferred to other diſcourſes.

And as to another of my ſcruples, about venturing abroad this tome, namely, that it muſt come forth ſo late, if it ſhould come forth at all, it was anſwered, that it could ſcarce come forth more ſeaſonably to recommend the whole deſign of the Royal Society, whoſe generous aims being to promote the knowledge of nature, and make it uſeful to human life. This treatiſe may procure them ſome number of aſſiſtants, in a work, whoſe vaſtneſs and difficulty will need very many, if men's curioſity and induſtry can by this treatiſe (or any to the like purpoſe) be well excited by a conviction of the real and wide diſparity betwixt true natural philoſophy, and that of the peripatetick ſchools; and that in cultivating the former, they will not meet with a field, that will afford them nothing, but (the wonted production of the latter) the thorns and thiſtles of acute indeed, but uſeleſs, and oftentimes troubleſome, ſubtilties; but, that they may expect a ſoil, that may by a due culture be brought to afford them both curious flowers to gratify curioſity, and delight their ſenſes, and excellent fruits, and other ſubſtantial productions, to anſwer the neceſſities, and furniſh the accommodations of human life.

And I will not deny, that I have had the fortune to be looked upon, as not the unfitteſt perſon in the world to offer ſomething in this kind; for thoſe, that are mere ſcholars, though never ſo learned and critical, are not wont to be acquainted enough with nature and trades, to be able to ſuggeſt thoſe inſtances, that are the moſt proper to manifeſt that, which men are to be convinced of. The mere chymiſts, beſides that their curioſity is wont to be too much confined to let them be fitteſt for ſuch a work, have the ill fortune to be diſtruſted by the generality of men, not credulous, which is a great unhappineſs in this caſe, becauſe, that though their experiments were never ſo true (as divers of them are) yet ſkill in their art being requiſite to make them, men's diffidence of the propoſers, joined with the difficulty of examining the things, will not allow them, either to believe what is propoſed, or to try it. And as for the new philoſophers (as they call them) though, if they were to write but for philoſophical readers, I know ſeveral of them, that would queſtionleſs do it rarely well; yet the generality of thoſe readers, to whom we would give good impreſſions of the ſtudy of nature, being ſuch as will probably be more wrought upon by the variety of examples, and eaſy experiments, than by the deepeſt notions, and the neateſt hypotheſes, ſuch a treatiſe for the kind, as that which follows, containing many practices of artifices and other particulars, that are either of eaſy trial, or immediate uſe, may perhaps by that variety gratify, and perſuade a greater number of differing ſorts of readers, than a far more learned and elaborate piece, and might be welcomer to more intelligent and philoſophical peruſers.

If it be aſked by ſome, that know me, whence it comes, that the ſecond part of the uſefulneſs of experimental philoſophy being written (as very credible perſons, that ſaw it can witneſs) about the year 1658, there may be met with in the following treatiſe ſome experiments of my own, that they know were ſince made, and ſome (though few) citations out of books publiſhed ſince that time? If, I ſay, this be aſked, the anſwer is intimated a little above; for having transferred to other tracts many paſſages, that belonged to thoſe I now publiſh, I was obliged to repair the injury I had done them, by ſupplying them with ſuch particulars, as offered themſelves to my memory, when I haſtily reviewed this tome, without ſcrupulouſly minding the times when the particulars inſerted did firſt occur. And if this advertiſement be applied to ſome other of my writings, that either the importunity of friends, or ſome unwelcome accidents, engaged me to publiſh out of their due time, and not in their intended order; it may keep men from thinking, that when I firſt wrote them, I had read over, or at leaſt ſeen which indeed I neither did nor could) every book of a recenter date, of which upon occasion I mention a paſſage or two, and thoſe perhaps, as they are cited by other authors, we being here in *England* but ſlenderly, and very ſlowly, furniſhed with modern foreign books.

All theſe inſerted paſſages the reader ſhould find included in parentheſes, as the printers call theſe marks ( ), by which he will yet be able to diſtinguiſh ſeveral of them, though I now find, that ſome others, by the negligence of the tranſcribers, or of the preſs, or of both, have been omitted; which advertiſement I fear may have need to be extended to ſome other printed tracts of mine, wherein parentheſes are to be met with.

Bating theſe few additional paſſages, the enſuing book comes forth, without taking notice of what changes or diſcoveries have happened in the commonwealth of letters, ſince the time it was written in. On which account, if ſome few of thoſe many particulars delivered there ſhould chance to be co-incident with what ſome other man hath written, I would neither on the one ſide be thought a plagiary myſelf, nor on the other ſide deny any man, to whom it may be due, the honour of the earlieſt publication; though, to ſhun needleſs controverſies, I am ſomewhat ſhy of naming this or that perſon, as the firſt propoſer or inventor of an experiment, which, eſpecially if the perſons or things be not conſiderable, is often difficult enough to diſcover: witneſs the conteſts, that have been, and yet continue, about the firſt inventors of common weather-glaſſes the aſcenſion of water in ſlender pipes, the glaſs drops that fly in pieces, the meaſuring of time by a pendulum, and, which is more ſtrange, the art of printing itſelf. If it be aſked, why I did not forbear to make uſe of ſome practices of tradeſmen, and other known, and perhaps ſeemingly trivial, experiments; theſe things may be replied.

I. That ſince on divers occaſions it was requiſite, that my diſcourſe ſhould tend rather to convince, than barely to inform my reader, it was proper, that I ſhould employ at leaſt ſome inſtances, whoſe truth was generally enough known, or eaſy to be known, by making enquiry among artificers, even by ſuch as out of lazineſs, or want of ſkill, or accommodation, cannot conveniently make themſelves the trials.

II. But yet I have taken care, that theſe ſhould not be the only, nor yet the moſt numerous inſtances, I make uſe of; it being in this tome, as well as in my other phyſiological writings, my main buſineſs, to take all juſt occaſions, to contribute as much, as without indiſcretion I can, to the hiſtory of nature and arts.

III. As to the practices and obſervations of tradeſmen, the two conſiderations already alledged may both of them be extended to the giving of an account of the mention I make of them. Of the truth of divers of the experiments I alledge of theirs, one may be eaſily ſatisfied, by inquiring of artificers about it; and the particular, or more circumſtantial accounts I give of ſome of their experiments, I was induced to ſet down by my deſire to contribute toward an experimental hiſtory. For I have found by long and unwelcome experience, that very few tradeſmen will, and can give a man a clear and full account of their own practices; partly out of envy, partly out of want of ſkill to deliver a relation intelligibly enough, and partly (to which I may add chiefly) becauſe they omit generally, to expreſs either at all, or at leaſt clearly, ſome important circumſtance, which becauſe long uſe hath made very familiar to them, they preſume alſo to be known to others: and yet the omiſſion of ſuch circumſtances doth often render the accounts they give of ſuch practices, ſo dark and ſo defective, that, if their experiments be any thing intricate or difficult (for if they be ſimple and eaſy, they are not ſo liable to produce miſtakes) I ſeldom think myſelf ſure of their truth, and that I ſufficiently comprehend them,

them, till I have either tried them at home, or caufed the artificers to make them in my prefence.

THEY that have given themfelves the trouble of endeavouring to make the experiments of tradefmen, to be met with in the writings of *Cardan*, *Weckar*, and *Baptifta Porta*, for inftance; and have thereby difcovered (what is not ufually obvious upon a tranfient reading) how lamely and darkly, not to add unintelligibly, feveral things are written, will probably afford me their affent, having found upon trial the inftructions of fuch learned and ingenious men to be often obfcure and infufficient for practice.

BUT here I muft give the reader notice, that as mechanical arts for the moft part advance from time to time towards perfection; fo the practices of artificers may vary in differing times, as well as in differing places, as I have often had occafion to obferve. And therefore I would neither have him condemn other writers or relators, for delivering accounts of the experiments of craftfmen differing from thofe I have given; nor condemn me, for having contented myfelf to fet down fuch practices faithfully, as I learned them from the beft artificers (efpecially thofe of *London)* I had opportunity to converfe with.

BUT here perhaps it will be demanded by way of objection, whether I do not injure tradefmen, by difcovering fo plainly thofe things, which our laws call the myfteries of their arts? To a queftion, that may perhaps by fome be clamoroufly preffed, not only upon me, but much more upon fome ingenious men of our nation, whofe pens have been more bold than mine in difclofing craftfmens fecrets, it will be requifite to return feveral things by way of anfwer; but that fuch readers, as are not troubled with the fcruple, may not be fo with the apology, they will find this printed in another character, fo that, if they pleafe, they may pafs it over unread.

[FIRST then, it may be reprefented, that I never divulge all the fecrets and practices neceffary to the exercife of any one trade, contenting myfelf to deliver here and there, upon occafion, fome few particular experiments, that make for my prefent purpofe: fo that, for much more than I allow myfelf to do, I can plead the example, not only of other writers that have publifhed books to teach the whole myftery of this, or that trade, as the prieft *Antonio Neri* hath diligently done in his Italian *Arte Vetraria*, and fome Englifh, as well as foreign virtuofi, have done on other fubjects; but alfo fome of the artificers themfelves, as the famous goldfmith and jeweller *Benvenuto Cellini* in his much efteemed Italian tracts of the lapidaries and goldfmiths trades. Thus alfo the famous mineralift *Georgius Agricola* publifhed in Latin a whole volume of the more practical part of mineralogy, wherein he largely and particularly defcribes experiments, tools, and other things, that belong to the callings of mine men. To which I might add divers other treatifes, fome of them French, others Italian (which, though I could not procure them, I have feen among curious collections of books) that have been publifhed about feveral arts by the artificers themfelves. And it is notorious, that in Englifh, as well as in divers foreign languages, we have books of the arts of gunnery, diftillation, painting, gardening, &c. divulged by perfons that profeffed thofe callings.

In the prefent edition, thefe paragraphs are included in crotchets.

SECONDLY, it is not the cuftom of tradefmen to buy books, efpecially fuch as are not intended for fuch readers, and treat (for the moft part) of things, either beyond their reach, or wherein they feem not likely to be concerned; and as for gentlemen and fcholars, though fome of them may, to fatisfy their curiofity, make a few trials, yet their doing fo will fcarce in the leaft be prejudicial to tradefmen. Since, to omit other arguments, it will not be worth while for a virtuofi to be at the charge and trouble of buying

buying tools, and procuring other neceſſary accommodations to ſell a few productions of his ſkill, though he ſhould not ſcruple to deſcend to ſuch a practice. For if he make but a ſmall number of experiments, their effects will coſt him more than the like may be bought for of thoſe that make them in great quantities, and whom their trade obligeth to be ſolicitous to buy their inſtruments and materials at the beſt hand, and ſell them to the beſt profit. Beſides that moſt of the works of artificers are chiefly recommended to the more curious ſort of buyers by a certain politeneſs, and other ornaments, comprized by many under the name of finiſhing; which require either an inſtructed and dexterous hand, or at leaſt ſome little peculiar directions, which I did not always think myſelf obliged to mention, in a treatiſe deſigned to aſſiſt my friend to become a philoſopher, not a tradeſman, and publiſhed to help the reader to gain knowledge, not to get money.

THIRDLY, to publiſh an experiment or two, or in ſome caſes, a much greater number belonging to a trade, is not ſufficient to rob a tradeſman of his profeſſion. For beſides that moſt trades conſiſt of ſeveral parts, and are each of them made up of divers practices, (that commonly are more than a few) thoſe numerous mechanical arts, that are called handicrafts, require (as their very name argueth) a manual dexterity, not to be learned from books, but to be obtained by imitation and uſe. And to theſe conſiderations I ſhall add this more important one, that mechanical profeſſions are wont to be, as it were, made up of two parts, which, for diſtinction ſake, I take leave to call the art and the craft; by the former whereof I mean the ſkill of making ſuch or ſuch things, which are the genuine productions of the art, (as when a taylor maketh a ſuit, or a cloak) and by the latter I mean the reſult of thoſe informations and experiments, by which the artificer learns to make the utmoſt profit, that he can, of the productions of his art. And this œconomical prudence is a thing very diſtinct from the art itſelf, and yet is often the moſt beneficial thing to the artificer, informing him how to chooſe his materials, and eſtimate their goodneſs and worth; in what places, and at what times, the beſt and cheapeſt are to be had; where, and when, and to what perſons the things may be moſt profitably vended. In ſhort, the craft is that, which teacheth him how both to buy his materials and tools, and to ſell what he makes with them, to the moſt advantage.

FOURTHLY, it may often prove more advantageous than prejudicial to tradeſmen themſelves, that many of their practices ſhould be known to experimental philoſophers. This I ſuppoſe, that I have ſufficiently proved in ſome, and eſpecially in * one of the following eſſays.

YET I ſhall now repreſent, that though ſome little inconvenience may happen to ſome tradeſmen by the diſcloſing ſome of their experiments to practical naturaliſts, yet, that may be more than compenſated, partly, by what may be contributed to the perfecting of ſuch experiments themſelves, and, partly by the diffuſed knowledge and ſagacity of philoſophers, and by thoſe new inventions, which may probably be expected from ſuch perſons, eſpecially if they be furniſhed with variety of hints from the practices already in uſe. For theſe inventions of ingenious heads do, when once grown into requeſt, ſet many mechanical hands a-work, and ſupply tradeſmen with new means of getting a livelihood, or even enriching themſelves. As to the diſcipline ſubordinated to the pure mathematicks, this is very evident; for thoſe ſpeculative ſciences have (though not immediately) produced their trades, that make quadrants, ſectors, aſtrolabes, globes,

* The eſſay here meant is that, which treats of the utility of the naturaliſts inſight into trades.

maps,

maps, lutes, phials, organs, and other geometrical, aſtronomical, geographical, and muſical inſtruments; and, not to inſtance thoſe many trades that ſubſiſt by making ſuch things as mechanicians, proceeding upon geometrical propoſitions, have been the authors of; we know, that whether the excellent *Galileo* was, or was not, the firſt finder out of teleſcopes, yet he improved them ſo much, and by his diſcoveries in the heavens did ſo recommend their uſefulneſs to the curious, that many artificers in divers parts of *Europe* have thought fit to take up the trade of making proſpective glaſſes. And ſince his death, ſeveral others have had profitable work laid out for them, by the newer directions of ſome Engliſh gentlemen, deeply ſkilled in dioptricks, and happy at mechanical contrivances; inſomuch, that now we have ſeveral ſhops, that furniſh not only our own virtuoſi, but thoſe of foreign countries, with excellent microſcopes and teleſcopes, of which latter ſort I lately bought one (but I confeſs the only one, that the maker of it, or any man, that I hear of, hath perfected of that bigneſs) which is of threeſcore foot in length, and which the ingenious artiſt that made it, Mr. *Reeves*, prized conſtantly at no leſs than an hundred pounds Engliſh money. I know not, whether or no I ſhould add, that poſſibly ſome particular experiments of mine have not been hitherto unprofitable to ſeveral tradeſmen. But this I may ſafely affirm, that a great deal of money hath been gained by tradeſmen, both in *England* and elſewhere, upon the account of the ſcarlet dye, invented in our time by *Cornelius Drebel*, who was not bred a dyer, nor other tradeſman. And, that we daily ſee the ſhops of clockmakers and watchmakers more and more furniſhed with thoſe uſeful inſtruments, pendulum-clocks, as they are now called, which, but very few years ago, were brought into requeſt by that moſt ingenious gentleman, who diſcovered the new planet about Saturn.]

I HAVE handled the ſubject of the foregoing arguments much more particularly, than I would have done, had not my pen been drawn on by a hope, that the things I have repreſented, may furniſh apologies to many inquiſitive men, who may be thereby emboldened to carry philoſophical materials from the ſhops to the ſchools, and divulge the experiments of artificers, both to the improvement of trades themſelves, and to the great inriching of the hiſtory of arts and nature.

IF it be further demanded, whether I have furniſhed theſe eſſays with the chiefeſt things I could have afforded them, I muſt confeſs, that I have not; for though I had, lying by me, ſeveral experiments and obſervations, leſs inconſiderable than many of thoſe I have made uſe of, which would have been pertinent enough to the ſubjects here treated of; yet I purpoſely forebore to imploy them in theſe tracts, becauſe I would not defraud thoſe others, to which they were more proper, and ſome of them neceſſary. For I freely declare, that my deſign in this preſent tome was not to furniſh it as well as I could, but to preſerve, as in a repoſitory, ſeveral ſcattered experiments and remarks, which I could beſt ſpare from the other treatiſes I had deſigned, which might otherwiſe probably be loſt: but yet I ſhall not deny, that I did not careleſsly draw up ſome of the following tracts, but, that I endeavoured to write them in ſuch methods, that they might contain ſeveral diſtinct heads, and thoſe as comprehenſive as I could eaſily make them, that both the young and hopeful gentleman, I call *Pyrophilus*, and I myſelf, might conveniently refer ſuch other practices and experiments (eſpecially thoſe of tradeſmen) as ſhould hereafter occur to us, and appear to belong to thoſe heads. And I did the leſs deſpair of his giving a kind reception to theſe diſcourſes, becauſe I could expect ſo little aſſiſtance in my undertaking, having never met with any book, great or ſmall, written upon the ſubject I was to treat of.

IF

If hereupon it be objected, that by my own confeſſion, divers of the particulars admitted into this book, are but ſlight, and ſome of them already known; I ſhall repreſent, that as ſome of the experiments ſpoken of are but ſlight, ſo there are others, that poſſibly diſcerning readers will not think to be altogether ſuch; and that it was fit (for reaſons already mentioned in this very preface) that I ſhould not forbear to employ, as proofs to convince others, things either known, or eaſy to be made ſo, eſpecially, ſince I commonly uſe them to ſome purpoſe, or other, whereto they have not been applied; and my deſign in the publication of theſe trifles being chiefly to invite the generality of readers, though of different inclinations, qualities, &c. to addict themſelves to the ſtudy of experimental philoſophy. The variety and eaſineſs I have aimed at in the experiments I have ſet down, may, for aught I know, be more proper, than if I had confined myſelf to the mention of a few choice and elaborate experiments, which ſome readers would think impertinent to their ſtudies, and others judge too difficult for them to put in practice. It appeared not unfit, that a book, whoſe title was like to procure it very different ſorts of readers, ſhould be for the moſt part written in a popular way; divers perſons, eſpecially thoſe of a higher quality, by a trifle, that hath the luck to gratify their curioſity, may be more ſucceſsfully invited to reliſh and eſteem experimental learning, than by a deep notion, or a weighty experiment. And there are others, that will eaſier be brought to value and try experiments, by meeting with ſome few, though but ſlight ones, that happened to ſuit with their humour or calling, or to accommodate them on ſome particular occaſions, than they would by many others, much more lucriferous, or otherwiſe important. And though it were to be wiſhed, that men's kindneſs to practical philoſophy were grounded on the beſt motives; yet this treatiſe will not altogether miſs the aim of its publication, if even upon the fore-mentioned ſlighter accounts, it engages readers to make, as well as reliſh, experiments; for the pleaſantneſs, variety, uſefulneſs, and other endearing qualities of ſuch an imployment will probably invite moſt of them to a further progreſs, whereby many uſeful phænomena and obſervations are like to accrue to what is already known of the hiſtory of nature and arts. And if this ſhall come to paſs, it will keep him from complaining of labour loſt, who in venturing upon ſuch a work, as now comes forth, was knowingly to poſtpone the appetite of fame to the deſire of doing ſome ſervice to mankind; to which end he takes one of the directeſt ways to be the contributing ſomewhat to the advancement of experimental philoſophy.

It remains, that I add ſomething more, which poſſibly may not a little befriend both theſe laſt mentioned anſwers, and ſeveral others contained in this preface; for, when all the former demands occurred to my thoughts, as likely to be made, ſome by one ſort of readers, and ſome by another, thoſe virtuoſi, that were ſolicitous for the publication of theſe papers, were not backward to urge the utilities, which they fancied would thence accrue to the publick. And I cannot very well deny, that, as meanly as I think of a treatiſe, to whoſe tome I did not, till the ſecond edition, (when I could conceal it no longer) let my name be prefixed; yet ſuch a work as this for kind, well performed, may be a very uſeful one. And even of this following book, ſuch as it is, it was ſuggeſted, that the uſes would not prove deſpicable, in regard, that beſide thoſe, that are common to it with the formerly publiſhed tome, ſuch as the improvement of the minds of men, and (eſpecially) the aſſiſting them to underſtand the works of God, and thereby engage them to admire, praiſe and thank him for them: beſides theſe (I ſay) there may be other uſes of the following tome, which, to avoid increaſing a prolixity that I fear is already too great, I ſhall rather name than diſcourſe of, contenting myſelf

briefly

briefly to intimate, that it was conceived, the peculiar uses of this present tome might be such as these.

I. It may afford materials for the history of nature, which, that it may the more plentifully do, I have purposely, on several occasions, added a greater number of instances, than were absolutely necessary, for the making out of what I intended to declare or prove.

II. It may afford some instructions, advices, and hints to promote the practical or operative part of natural philosophy in divers particulars, wherein men have been either not able, or not solicitous to assist the curious.

III. It may enable gentlemen and scholars to converse with tradesmen, and benefit themselves (and perhaps the tradesmen too) by that conversation; or, or least, it will qualify them to ask questions of men that converse with things; and sometimes to exchange experiments with them.

IV. It may serve to beget a confederacy, and an union between parts of learning, whose possessors have hitherto kept their respective skills strangers to one another; and by that means may bring great variety of observations and experiments of differing kinds into the notice of one man, or of the same persons; which how advantageous it may prove towards the increase of knowledge, our illustrious Verulam has somewhere taught us.

V. It may contribute to the rescuing natural philosophy from that unhappy imputation of barrenness, which it has so long lain under, and which has been, and still is, so prejudicial to it. And to effect this rescue, it will in some measure enable those, that desire it, to employ those practical arguments, that are proper to convince many, that are not to be convinced by any other sort of proofs.

VI. And which is the main of all, it may serve by positive considerations, and directions, to rouse up the generality of those, that are any thing inquisitive, and both loudly excite, and somewhat assist, the curiosity of mankind; from which alone may be expected a greater progress in useful learning, and consequently greater advantages to men, than in the present state of human affairs will be easily imagined.

OF THE USEFULNESS OF EXPERIMENTAL PHILOSOPHY.

The SECOND PART. The SECOND SECTION.

Of its USEFULNESS to the Empire of MAN over inferior Creatures.

ESSAY I.

Containing some general considerations about the means, whereby Experimental Philosophy may become useful to human life.

HITHERTO, my dear *Pyrophilus*, I have attempted to satisfy you of the usefulness of experimental natural philosophy to physick: it follows, that I proceed to endeavour to shew you, that it may be also very serviceable to husbandry, in all its subordinate parts, and to those other professions, that serve to provide men with food and rayment, or do otherwise minister to the necessities or accommodations of life; as the trades of brewing, baking, fishing, fowling, building, and the rest not needful here to be enumerated. For though the human body, in respect of the rational soul, (which is the inventress and seat of sciences) be one of the corporeal things, over which the empire of knowledge is to be established; yet, taking man as a creature made up of body and soul, the advancement of his empire seems to consist more properly in the inlargement of his power over the other creatures: physick seeming rather to defend him against revolts and insurrections at home, than to increase his power, and extend the limits of his empire abroad.

BUT, *Pyrophilus*, I hope, you do not expect, that I should now insist on each, or so much as on any of the above-mentioned trades, by whose intervention it is, that man exercises his dominion over external bodies. For such a work would require little less than an age, and much more than a volume; and besides (that it is vastly disproportionate,

tionate, both to my slender stock of mechanical skill, and to the little leisure I have to conclude this section in) I could not acquaint you with all that I could pertinently enough deliver about these matters, without too much defrauding some other treatises, that I design you: and therefore, I hope you will be content, if, in the remaining part of this tract, I do not only present you a not despicable number of considerations proper to manifest that, and to intimate, how experimental philosophy may be of great use to the promoting of mechanical arts and trades, but illustrate and confirm all, or most of those considerations by particular instances, derived from observations and experience.

This I shall, God assisting, endeavour to do in the following essays. But before I descend to particulars, it will be expedient in this place to premise some general considerations relating to the influence of experimental philosophy upon trades, and two or three advertisements, that concern the ensuing discourses.

SECTION I.

First then, to make it probable, that a true insight into natural philosophy may be capable of affording some reformation, or other kind of improvement to trades, I shall desire to consider, that being, for the generality of them, conversant about some few particular productions of nature, such men as are thoroughly skilled in her general laws, and acquainted with a vast number of her productions, and versed in the ways of applying nature and art jointly to several purposes, according to the several exigencies of things; such sagacious persons (I say) will, in all likelihood, be able, some way or other, to meliorate the inventions of illiterate tradesmen. As the husbandman's skill, for instance, consisting chiefly in the observations of the nature of a few plants and animals, their relation to such and such soils and kinds of culture, and the operations of stars and meteors upon them, which are subjects, that properly enough fall within the cognizance of the naturalist, it cannot seem improbable, that he, that has seriously and industriously inquired into the nature of generation, nutrition, and accretion, both in plants and animals, and knows how to vary an useful experiment, when once found out, so as to remedy the inconveniences, or supply the deficiencies, or improve the advantageousness, or translate and apply the use of it, and, in sum, he that can knowingly and dexterously manage, what his own or other men's observations have afforded him, will be able to cultivate the ordinary husbandman's skill with as much improvement, as that confused skill enables the husbandman to cultivate his ground.

SECTION II.

To carry on the foregoing consideration a little farther, I will add, that it may as well conduce much to the manifesting, how much trades are subordinate to natural philosophy, as to the improvement of trades themselves, that it be attentively considered, what things each particular trade is, as it were, made up of. As for example, the chief things in the refiner's trade are, to know the ways of making, and the operations of aqua fortis upon silver, gold, and copper; to know how to purge that menstruum, that it may dissolve no gold, nor precipitate any of the silver it dissolves; to know what proportion there ought to be dissolved in it; to know with what quantity of water to weaken the solution, and how long copper-plates need lie in it, to precipitate all the silver out of it; to know how lead is to be colliquated with them, and what proportion of it is necessary and sufficient to carry off with it (when it is blown off upon the test) the baser metals; to know how to make cupples of several sorts and sizes, and upon them to draw off the lead or antimony from the silver or gold, and discern when the metal is sufficiently refined; to know what proportion of gold and silver is requisite for the making of water-gold as they call it, (because it is separated from silver by aqua fortis, which dissolves this metal, and leaves the other in a fine powder;) these things, to which many others are subservient, belong to the refiner's trade, which, though understood by few, seems to be a very narrow and simple trade, in comparison of a hundred others, whose operations are far more numerous and complicated. Now if all trades were judiciously resolved (if I may so speak) into the several parts they consist of, it would, I question not, manifestly appear, that the most, if not all of them, are in many particulars but corollaries deduced from some particular physical observations, or but applications of them to the uses of human life.

And if this be so, you will not, I presume, think it unlikely, that by a farther discovery of the nature of those particular bodies wherewith the trade is conversant, and a solid knowledge of those laws of nature, and those operations of bodies upon one another, which it employs; some, if not most, of those parts, whereof the trade may be conceived to be made up, may be reformed or bettered; which is enough to make the philosopher an improver of the trade, which he may become upon such unobvious accounts, that perhaps it may not unreasonably be hoped, that even the chymist's charcoal may be made, by a good naturalist, equivalent to an excellent compost for land. For if it be true, as well as it is probable, not only that the food of those animals (as oxen, sheep, &c.) which the husbandman deals with, springs out of the ground; but that the plants, which affords them this food, are themselves nourished by a certain vegetative salt they find in the ground; and, that this salt being by frequent seminations exhausted, the soil grows barren, until either by the air, or the steams of the subterraneous parts, or the spontaneous maturation of the saline rudiments contained in the ground, or by adventitious manure, or by all or divers of these together, it be re-impregnated with a new vital saltness: if these things be true, I say, then those chymical experiments, that conduce to discover to us, what kind of salt that is, and to what other salts it is allied or opposite, as it is to several acid ones, may probably afford very useful directions to the husbandman, towards the meliorating of his land, both for corn, trees, grass, and consequently cattle. And having had the curiosity * to distil some earths, some dungs, and some seeds, and observe the salts abounding in the liquors yielding by them, (of which we have elsewhere occasion to speak) we found cause to wish, that experiments of that nature, in relation to the improvement of husbandry, might be industriously prosecuted by naturalists. He that has observed those many particulars in husbandry, which might invite that great naturalist Sir *F. Bacon* † (who yet mentions very few of them) to pronounce, that nitre is, as it were, the life of vegetables; he that observes how conducive that fertilizing dung of pigeons is, both to make earth fruitful to the husbandman, and to impregnate it with nitrous salt for the saltpetre-man; and he that knows, that most fat earths, so defended from the rain and sun, that the one may not draw up, nor the other wash down the embrionated saltness of them, will after a time abound in nitrous salt, if they are not permitted to spend any in producing of vegetables; such a

* *Verulam* hist. v. & mort. p. 237. Certissimum est quamcunque terram, licèt puram, neque nitrosis admixtam, ita accumulatam & lectam, ut immunis sit soli, neque emittat aliquid vegetabile, colligere etiam satis copiosè nitrum.

† Nat. Hist. cent. 5. exp. 444.

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one, I say, will perchance be apt to think, that enquiries into the nature of saltpetre, may be of great concernment to husbandry. And to give you, *Pyrophilus*, some inducements to expect, that chymistry may be very useful in such kind of enquiries, I shall here mention to you a couple of my experiments relating to nitre.

The first is that, whereby I endeavoured to give an inquisitive person hopes, that materials, which seemed unlikely, might, by due changes, and without much art, be turned into saltpetre. The experiment was this: I caused some earth to be digged up just underneath the clay-floor of a pigeon-house; such earths being believed to abound the most with nitre, that needs only to have its particles brought together and united to compose saltpetre: a pretty quantity of this earth being put into a retort, and distilled with a good fire *ex arena*, afforded me, though little or no oil, yet a pretty quantity of a reddish liquor, which, instead of being, as others would have expected, of an acid nature like spirit of nitre, was fit for my purpose, by strongly participating of the nature of volatile salts; as appeared, not only in that I could, without rectifying it, turn syrup of violets with it immediately green, and precipitate a solution of sublimate into a milky substance; but because there came over, with the spirit, into the lower part of the receiver, a salt in a dry form, which not only was in taste not unlike the other volatile salts, but was so far from being of an acid nature, that with an acid menstruum it readily fell to hiss, and made an ebullition. So that it seems (which in an enquiry about nitre is very considerable,) that a salt, very repugnant to acids, may, by the operation of the earth and air, be so altered, as afterwards by a slight management to afford saltpetre, whose spirit is highly acid. But of this experiment I may hereafter make farther mention.

The other, (which we elsewhere have occasion more particularly to take notice of with reflections on it) is briefly this. We took pot-ashes, which you know contain but the salt of burnt vegetables; and on those, first dissolved in a little fair water, we dropped aqua fortis (whose saline part consists indeed of little else than the spirits of nitre,) till all ebullition and hissing betwixt it and the resolved pot-ashes were perfectly ceased; and having filtrated this liquor, and set it in an open vessel in a gentle heat to evaporate, it did within two or three days after, (and sometimes, for we made it more than once, even in a few hours) being removed to a cold place, afford us very pure crystals of saltpetre, as both their shape, and flashing (on live coals) into a blue halituous flame, informed us. And since I have had occasion to mention the use of saltpetre in husbandry, I shall not forbear to add, that the knowledge, which the naturalist, as a discerning chymist, may give the husbandman of the natures and distinctions of saline bodies, may be of no mean use to him, by assisting him to discern and observe the considerablest differences of the various saltnesses to be found in soils, and what sort of saltness each particular seed or plant most affects. For by this means, not only many grounds might be made useful, which are thought barren, only by reason of our not knowing for what plants the saltness predominant in them may be proper; but the same ground may yield much frequenter crops than commonly it doth, when it is successively sowed only with one sort of seed, by the due alteration of plants delighting in the several sorts of salts to be met with in that ground; which oftentimes, by being impoverished, or rather freed from one sort of salt, doth but the more plentifully feed those plants, that delight in another: which, in some places we have observed, that husbandmen seem to have taken notice of already, by sowing (in fields too remote from their dwellings to have compost brought to them) turnips, to fit the ground for wheat, and serve for a manure; though in this method some other circumstances may possibly concur with the nature of turnip-seed, to the preparation of the ground for wheat. And I am

prone

prone to think, that there is scarce any ground or soil, except perhaps mere sand, that might not, even without much culture, be made fertile, or at least kept from being altogether barren, if we were on the one hand skilled in the ways of discerning the nature of the ground; and on the other hand acquainted with, and provided of, all the variety of seeds and plants that nature has, though not all in one country, afforded us. For there are divers soils, which here in *England*, or in other regions, are, as useless, left quite uncultivated; which seeds or plants, that abound in other countries, and would probably be made to grow in these, would make serviceable to the husbandman. Many steep and abrupt portions of ground (some of them very large) exposed to the southern sun are left altogether waste, not only in *England*, but in divers hot climates, where the planting of grapes for wine is not yet in use; though such pieces of land in *France* and *Italy*, and, as I have observed, even in the *Rhetian Alps*, nourish excellent vineyards.

I KNOW an ancient and landed gentleman, who communicated to me upon his own knowledge an experienced way of making wheat grow and prosper well on mere clay, where there was no grain at all did thrive; which, though I have not hitherto had opportunity to try, yet upon the credit of a person so sober and qualified, I scruple not to mention it here, because the art consisting mainly in the imbibition of the seed for a determinate time in a certain expressed oil, that is not dear; it may make it probable, that without altering the whole soil by manures, a slight, but convenient change made in the seed itself may serve to make them fit for one another. And (to add, that upon the by) to shew, that the particular dispositions of some sorts of seeds may enable them to make the ground they are sowed in, much more productive, than it would otherwise be, I shall relate to you, that being not long since in the company of a learned and curious traveller, I saw, among some rarities of a quite other nature, an ear or two of corn, not much unlike our common wheat; at which being somewhat surprized, I asked him, what peculiarity had procured that grain admission among such rarities? to which he replied, that in the warmer region, where he begged it of a virtuoso, one of those grains would afford so vast a multitude, as he was almost ashamed to name, and I am more than almost afraid to repeat: but before I went out of the house, an English gentleman, that had a more than usual curiosity for such kind of trials, assured me, that having obtained some grains of that corn, and carefully sowed it in some land of his own, not far from the place we were in, he had out of a single grain several hundreds; though not near so many of them, as the other traveller, who yet was a very sober and judicious man, related to have been produced in a better climate and soil. Of this strangely prolifick wheat, the gentleman readily granted me a promise of a sufficient quantity to make a trial; whereof, when I shall have received it from a servant of mine in the country, you may command the success. And this brought into my mind what I read in the learned Jesuit * *Acosta*, who affirms, that in divers parts of *America*, where it is known, that our European wheat prospers not, the Indian (or, as many English have stiled it, Virginian) wheat, they call *Maiz*, does so wonderfully thrive, that although the stalk bear often more than one cluster, and the grain be big; yet in some clusters he has reckoned seven hundred grains: to which he adds, that it is not strange in those countries to gather three hundred faneques, or measures, for one sown. Which passages, especially the former, speak of an increase that seems so little credible, that I should on that account forbear to mention it, were it not, that in *Europe*, and even in *England*, I myself have reckoned such a multitude of grains upon one of the very numerous ears produced by the same single grain, that I found myself very inclinable to

* Lib. IV. cap. 16. as he is published by *Purchas*.

absolve

absolve *Acosta*, and continue to look upon him as one of the best writers of the natural history of *America*.

We now proceed to take notice, that in some Eastern countries, a sort of rice (a grain that makes the chief and most usual food of the natives over almost all those parts) prospers very well upon land so drenched with waters, that seeds-men, to scatter the rice, do rather wade than walk. But this itself (which, for the main, was confirmed to me by eye-witnesses) is less strange, and does less illustriously confirm what I was proposing, than what the inquisitive Jesuit *Martinius* affirms to be the practice of some (as well great as small) countries in *China*, where, in divers places, that are all the year under water, and would by our European husbandmen be thought capable of no other use, than that of ponds or lakes, the Chinenses cast a certain seed so well appropriated to the place, that is to receive it, that though it falls not immediately on the land but on the water, so that one would think they were not about to sow a field, but bait a pond for fishes, yet this seed, being adapted to the soil it meets with at the bottom of the water, does so well prosper and shoot up to the top, that in its proper season the surface of the water looks as fresh and verdant, as a fruitful meadow, and yields as rich a crop. But for fear of digressing, I shall, *Pyrophilus*, proceed to tell you, that perhaps also chymistry, especially in conjunction with hydrostaticks, may prove serviceable to the ingenious husbandman, by assisting him to discover the kinds and degrees of saltnesses that are in several other bodies that he much deals with. I remember I have met with things surprizing enough, in examining some sorts of earths by distillation, and by several chymical instruments of discovery; but though I have likewise had the curiosity to distil dungs and grain, and fruits, and some other subjects, wherewith the husbandman is conversant, to observe what kinds of saline and other liquors, and in what proportion, and of what strength, they could afford me; yet not having any notes by me of the particular trials, I shall content myself to have given you this hint of a new sort of experiments in husbandry; and shall only add, as to salts, that since the fertilizing power of dungs seems to reside in the salino-sulphureous part of them, (and the like I have by chymical trials found in lime;) a practical insight into the differences and differing operations of salts (about which I elsewhere entertain you) may probably very much assist the husbandman to examine the several dungs, and other composts (the knowledge of which is of great moment in his art) and to multiply, compound, and apply them skilfully.

And as chymistry, that is conversant about fire, so even hydrostaticks and hydraulicks, that teach us to make engines and contrivances for the lifting up, and for the conveying of water, may in divers places be of no small use to the husbandman. For not to mention what is done in some more known parts of the East, of the like nature with what I am going to mention, *Martinius* informs us, that in one province of *China* (whose name I remember not) they are so curious to water their fields of rice, that they have upon the river excellent mills so made, as that great quantities of water are continually raised in buckets, or other convenient vessels, fastened to vast wheels driven by the stream; which watering-mills (to add that notable instance upon the by,) are not, as our European mills are wont to be, fixed to one place, but built upon vessels, with which they may remove the mills, how great soever, from place to place, as occasion requires. Nor is this eastern way of raising water by wheels, so as that it may be conveyed by convenient channels to places many feet higher than the river, or other receptacle of the water, that is to be distributed, the only way, whereby the hydraulist and mechanician may assist the husbandman; since he may considerably do it by the art of libellation, or conducting of water upon the ground. For the improvement, that

may

may be made of land by water, in soils fit for that way of culture, may be far more considerable than is yet wont to be taken notice of; as indeed this husbandry itself is in many countries both elsewhere, and in *England*, as yet unpractised. I have had some lands of my own much bettered by being skilfully overflowed; so that when I observed the difference, the tenant, though shy of acknowledging the utmost advantage, confessed to me, that he thought it yielded him double the former income. And a gentleman of quality of my acquaintance, whose improvements I went lately to view, shewed me a scope of ground, which at his first coming to that wild place, four or five years ago, was boggish, and which yet he had turned into a good dry soil, by only trenching it here and there with shallow trenches of not a foot deep, and overflowing it, by the means of those trenches, and conveniently placed dams, as evenly as he could, five, six, or seven times a year, betwixt the beginning of October, and about the middle of April, with the water of a neighbouring spring, which was no way enriched by land-floods, arising but in a very barren and uncultivated place, far from the neighbourhood of grounds capable of enriching it; and yet this spring drained away, if I may so speak, that ancient hydropical distemper of the land, and turned it, as I found by trial, into a good compact soil, on which store of mowers were (when I saw it) employed in making of hay, which this meadow yielded plentifully enough to be worth twenty times its former value. Nor is this the single considerable instance we have met with, of the improvement that may be made of divers kinds of land, only by skilfully overflowing them with common waters.

But, *Pyrophilus*, I may hereafter have so many occasions to mention particulars relating to agriculture, that I should presently dismiss them in this essay, were it not, that I am, by my having named husbandry to you, put in mind to employ it as an instance to confirm this observation, that the more comprehensive a trade is, the more likely it is, that it will be capable of being meliorated by natural philosophy. For such trades, as are of great extent, are obliged to deal with a considerable number of nature's productions, and to make use of divers of her operations; and consequently must comprehend the more particulars, wherein the manufacture or profession may be reformed, and otherwise advantaged by a knowing and dexterous naturalist. Thus the husbandman's corn makes it fit for him to have a competent skill in the whole art of tillage, the keeping of cattle great and small, the ordering of dairies, of wood, of flax and hemp, of hops, of the kitchen-garden, of an orchard, of bees, &c. besides that the particular productions of some of these, as honey, cyder, &c. require some skill, and are capable of much improvement; so that among so great a variety of things, wherewith the husbandman has to deal, it can scarce be otherwise, than that there will be several things, wherein the naturalist's higher and more reaching knowledge and experience will be serviceable to him. And whereas in the preservation both of cattle from diseases, and of the fruits of the earth from putrefaction, lieth one of the most beneficial and difficult parts of the husbandman's skill, he may therein be much assisted by an expert naturalist; who not only, by being able to accelerate putrefaction in divers bodies, may teach the husbandman to furnish himself with great variety of composts and manures, to relieve and enrich his ground with whatever peculiar sort of salt he observes to be deficient; but also may teach him how to preserve many of his seeds, and flowers, and fruits, beyond their wonted duration: as I know some persons, to whom I recommended methods of this kind, that use to preserve quinces, for instance, a great part of the year, by a strong liquor, or pickle, made of nothing but water, and what (for the most part refuse stuff) may be easily obtained from the quinces themselves. This way presented us fruit at almost the year's end; and a while since I could have shewn you (and, for aught I know,

know, can do ſo yet) cherries well ſhaped, and ſucculent enough, of above a year old, preſerved without ſalt or ſugar, by being kept in a ſpirit of wine fitted for that uſe and fully impregnated, before their immerſion, with the tincture of the ſkins of other cherries of the ſame kind. The vaſt benefit that the Hollanders derive from the beſt way of ſalting or pickling of herrings, and the advantageous uſe that is made by others, of ſo powdering beef, and ordering other fleſh, that it will laſt good to the *Indies*, and is ſometimes brought uncorrupted into theſe parts again, may perſuade us of the benefit that may accrue to the huſbandman by the diſcovery of the ways of keeping the productions of the earth from corruption; eſpecially if his ſkill be extended to weak wines, cyder, perry, and other liquors, which are wont to be made in great quantities, and yet apt to decay at home, and unfit to be tranſported far abroad. And the uſe of ſugar to ſtrengthen vinous liquors, and make them durable; and, without the help of ſalt or any ſharp thing, to preſerve great variety of fruits, and of the juices of herbs, may encourage us to think, that there may be very differing ways (and ſome of them ſeemingly oppoſite) to make many things outlaſt their natural periods of duration.

But my trials and obſervations (whether about the conſerving of fruits, flowers, and fleſh, or of other things of this ſort) belonging more properly to another diſcourſe (of the preſervation of bodies) I ſhall now mention no more of them, but paſs on to tell you, that very much prejudice, which often happens to the poor huſbandman (and ſometimes even to his utter ruin) by thoſe, either ſtubborn, or contagious diſeaſes (ſuch as the rot in ſheep, and the glanders in horſes) that make havock of his cattle, may in great meaſure be prevented by the inſtructions of a knowing naturaliſt, eſpecially if he be an expert phyſician too. For as many diſeaſes, ſo many cures are analogous in men and beaſts; and the remedies prove frequently more ſucceſsful in theſe than in them, as well for divers other reaſons, as becauſe the bodies of many brutes are more able to bear the operation of ſtrong remedies; and yet the unaccuſtomedneſs of almoſt all of them to phyſick makes them more relievable, than men by any (not improper) remedies. I will not now relate, that I have in ſome countries found medicines, that have been uſefully tried againſt diſeaſes in men, cried up for their efficacy againſt their analogous ones in horſes; nor with what difference in the doſe theſe may be purged by ſeveral of the ſame cathartics, eſpecially aloes, that are employed for the purgation of human bodies. I ſhall rather inform you, that as in theſe ſalt is, you know, reputed a great reſiſter of corruption, and an enemy to worms (with a ſort of which the livers and neighbouring veſſels of ſheep have been obſerved to be infeſted); ſo by the bare uſe of Spaniſh ſalt, which each ſheep, being firſt made to bleed a little under the eye, was made to take down a ſmall handful, two or three times, with ſome days of interval, without being ſuffered for ſome hours to drink any thing after it: by this remedy, I ſay, given at the time of the year when there is danger that the ſheep will begin to be blotched, many flocks have for divers years been preſerved by a rich intelligent gentleman of my acquaintance, that is a great ſheep-maſter, and has thereby (and that alſo lately) preſerved his flocks in a moiſt country, when moſt of his neighbours loſt theirs. I might here mention to you, *Pyrophilus*, the virtues of crude antimony, to cure the foulneſs of blood, and even the leproſy in ſwine; of quickſilver, to cure the worms in horſes; of *Palmarius*'s famous remedy, which he ſolemnly affirms to be a conſtant one againſt the bitings of a mad dog in cattle, and of a more parable one for men alſo, whoſe ſucceſs I almoſt admired in a near relation of yours and mine; of the uſe of the antimonial cup for ſeveral ſickneſſes in horſes and ſheep, which (if I miſremember not) was ſucceſsfully tried by one to whom I recommended it; and of another antimonial medicine, which (though much commended to me by a virtuoſo that took it himſelf) a gentleman of my acquaintance, reſident in the country, who prepares it, aſſures me, that he uſes it with ſtrange ſucceſs to fatten his horſes (made lean by occaſion of ſickneſs) with whom yet it works not, either as an emetick, or a purge. And I could here preſent you divers other receipts much prized for their having (as well as the newly mentioned remedies) frequently been found effectual againſt the ſame diſeaſes both in human bodies and in brutes, if I did not think it leſs proper to make in this place a veterinarian excurſion, than to tell you, that, if you have any curioſity for them, you may command them.

I MIGHT add, if I had leiſure, ſome reaſons, why I deſpair not, that in time the huſbandman may, by the aſſiſtance of the naturaliſt, be able to advance his profeſſion by a therapeutical part, which may extend not only to the animal productions of the ground, and to the vegetable ones; but (in a large acception of the term) to the diſtempers of the ground itſelf. For if the cauſes of the barrenneſs of ſoils in general, and of their indiſpoſition to cheriſh particular plants or animals, were by the philoſopher's ſagacity diſcovered, I ſee not why many of thoſe defects may not be removed by rational applications and proper ways of cure; as well as we ſee inconveniences remedied in many other inanimate bodies, without excepting the cloſe and ſtubborn metalline ones themſelves.

AND perhaps alſo, that by a way of management ſuggeſted by the knowledge of cauſes the barrenneſs of a ſoil may be cured, or its fertility much promoted by methods, that do nothing near ſo much require coſt as ſkill. Some ingenious huſbandmen have of late proclaimed themſelves much ſatisfied with a way of correcting two of the barreneſt ſorts of land, not by rich manures or other coſtly cultures, but by ſkilfully mixing the ſand and clay themſelves in a due proportion, according to the uſe the huſbandman deſigns to make of it. And whereas one of the beſt modern writers of agriculture reports, as he may, for a ſtrange thing, that he had ſeen ſeven or eight and thirty ears of barley, that ſprung from one grain; I remember, that an ingenious gentleman, to ſatisfy ſome curious perſons what might be done in that kind, ſowed corn upon a piece of land, very near the place of my abode, which proſpered ſo ſtrangely, that one root that I took particular notice of, though perhaps not the fruitfulleſt in the field, produced ſixty and odd ears of corn; and yet, which was the ſtrangeſt, this wonderful increaſe depended upon a philoſophical obſervation; nothing extraordinary having been done, either to the land, or ſo much as to the ſeed; as I had opportunity to know, both by the informations of obſerving men, and by the confeſſion of the gentleman himſelf, who was pleaſed to make choice of me to intruſt his ſecret with, that, in caſe he died before me, the publick might not loſe it. Upon which account he alſo confided to me another ſpecimen of his ſkill. He once preſented your excellent mother a company of ſeveral ſorts of choice apples, among which there was one ſort excellently taſted, but very ſmall; the following year he preſented her another baſket of the like fruit, but finding no ſmall ones among them, ſhe took occaſion to aſk him, what was become of the tree that produced thoſe delicious little apples, that made part of his former preſent? to which he replied, that he had brought ſeveral of its productions among the other fruits ſhe was looking on; and thereupon ſhewed her ſome that came from the ſame tree, and appeared by the peculiar reliſh to be of the ſame ſort, though exceedingly differing in bulk, that neither your mother, nor I, had any ſuſpicion that the ſame tree bore them. Upon which occaſion he readily gratified my curioſity by acquainting me with his way, which depended almoſt only upon a phyſical obſervation; all that he added being not any rich compoſt, but ſome deſpiſed leaves of a very cheap and common vegetable. But huſbandry is too large a ſubject for me to proſecute in this place, and therefore I ſhall here diſmiſs it.

SEC.

SECTION III.

THE next thing I shall observe to you, *Pyrophilus*, is, that it is not only to the trades that minister to the necessities of mankind, but to those also that serve for man's accommodation or delight, that experimental philosophy may bring improvements; for these arts also do, for the most part, consist in the knowledge and application of some of nature's productions and courses, whose being referred to the accommodation or delight of men, rather than to any other purpose, does produce nothing that is truly physical in the things so referred, which thereby acquire only such a kind of respect to man, as that which the metaphysicians call an extrinsical denomination; and we see that the same things, without varying their nature, are serviceable to men in very differing capacities: as wine serves one that is dry to quench his thirst, serves a fainting person to revive his spirits, and the drunkard to inebriate him; the same spirit of wine, that serves the physician to make tinctures and extracts for the recovery of health, may serve the ladies to dissolve benjamin into a tincted liquor, that diluted with fair water may be used as a cosmetick, which I have received many thanks for; and the same spirit skilfully employed upon ingredients, to be named to you ere long, is of excellent use for making divers fine varnishes made with rectified spirit of wine; nay, the newly mentioned solution of benjamin may itself be applied to all those differing uses; for of itself it is a prety and odoriferous varnish, and I have used it (though not often, for want of opportunity) with very good success against a sort of tetters, which I caused frequently to be bathed with it. What happy applications knowledge and skill may make even of unpromising things, to the furnishing men with delights, is methinks very evident in musical instruments, as lutes, viols, &c. For who would think (if experience did not assure us of it) that with a few pieces of wood joined together, and the guts of cats or lambs wreathed or twisted into strings, the skilful musician, by the help of mathematicks and exercise, should be able to charm the ear with the greatest, as well as most innocent delights, the sense belonging to the organ is capable of, and which sometimes does not only please, but ravish the transported hearers. But though, *Pyrophilus*, as I was lately saying, physicks may not only be very improving to those arts and professions that serve to provide man with the necessaries or accommodations of life, but also to those that serve chiefly to furnish him with pleasures and delights; as might be instanced in experiments of colouring, perfuming, making sweet-meats of all sorts, embellishing the face with cosmeticks, and divers others of the like voluptuous nature; and though I may elsewhere have occasion, when I come to treat of colours, odours, tastes, and other qualities, to acquaint you with some receipts and experiments of this kind; yet now I do not only want leisure to mention them, but am desirous, that natural philosophy should engage you to court her, rather by her gratifying and enamouring your reason, than by her bribing and inveigling your senses.

SECTION IV.

THOUGH what has been represented about the usefulness of experimental philosophy to trades does chiefly belong to those, wherein nature's productions are employed to human uses, by those operations, wherein nature herself, rather than the artificer, seems to have the chief hand, as the trades of brewing, baking, gardening, tanning, &c. yet I would not exclude those very trades, wherein the artificer seems to be the main agent, and in whose ultimate productions the chief thing that is wont to be considered, is the adventitious shape or form, which the artificer, as an intelligent and voluntary agent, does, by the help of his tools, give the matter he works on, as in the trades of the smith, the mason, the cutler (when distinct from that of the sword-maker) the watch-maker, and other handicrafts. For though these consist rather in the manual dexterity of men, than the skilful ordering of the productions of nature, by their material operations upon one another; yet to many, if not all, even of these, the naturalist may some way or other be a benefactor.

For there are divers of these manual trades, that, especially as they are exercised in cities and greater towns, consist of several parts, and have need of several other trades to prepare materials for them, and dispose them to receive the last form, which the artificer is to give them, to fit them for sale. And we may, in many cases, observe, that though this artificer, that gives the matter this last form, does it chiefly with his hands and his tools; yet those other tradesmen, to whom he is beholden for his materials, do some or other of them, to prepare and qualify them for his use, need some observations of the conditions of the body they deal with, or must employ some physical operations, wherein they may be much assisted by the knowing naturalist, who may also teach the manual operator himself how to make choice of his materials, and examine the goodness of those that subordinate workmen shall bring him. Thus, though stone-cutting be a trade that seems to consist almost wholly in giving, with proper tools, to marble, free-stone, and other materials, the shape, which the artificer designs; yet, if I had leisure, I could easily shew you, that even in this trade, there are many particulars, wherein experimental philosophy might be helpful to the artificer. For ways, hitherto unused, may be found out (as I have partly tried) to examine the nature and goodness of the marble, alabaster, and other stones, which the mechanicks deal with. A competent knowledge of the sap, that is to be found in stones employed for building, is of so much importance, that the experienced master workmen have confessed to me, that the same sort of stone, and taken out of the same quarry, if digged at one season, will moulder away in a very few winters; whereas digged at another season, it will brave the weather for very many years, not to say, ages (but of my observations of this kind, more elsewhere). The cements also, and stoppings (as they call them) which are of good use in this trade, may be easily bettered by the naturalist that is versed in such mixtures. And I remember, I had occasion to teach a fine cement for the rejoining of the broken limbs of statues to their bodies, to an inquisitive artificer, who, by such like helps, did in other cases so well counterfeit marble with a cement, that even where there was occasion to fill up great cavities with it, the work would pass for entire; the additaments being not distinguished from the natural marble. Want of curiosity also keeps our stone-cutters here in *England* unacquainted with the ways of working upon porphyry, which they will not undertake either to polish or to cut. Nor is *England* the only country, where the art of working upon porphyry (which appears to have been in great use amongst the *Romans*) is unknown, though at *Rome* there are some few, that do with great gain exercise it. And though I know not precisely, what it is they employ, yet I presume, it may be powder of emery; for with that, and water, and steel-saws, I have here in *England* caused a porphyry-stone to be cut. And the mention of porphyry puts me in mind of telling you, that by an art I have, white marble may be so stained, and that durably, with spots great or small, and red or brown, as it pleaseth the artificer, as I may hereafter have occasion more fully to relate. It would be too long to discourse to you here of artificial marble, and divers other things, that stone-cutters affirm to belong to their trade, wherein you will scarce doubt, but that it may be capable of improvement.

Where-

Wherefore I ſhall only add, that whereas this profeſſion does much require very good ſteel-tools, and they muſt have theſe from ſmiths, and others that deal in iron, if theſe men's trade were bettered by the naturaliſt, they might be able to afford the ſtone-cutter the better tempered tools; and that even the ſmith's craft, though it ſeem to be merely a manual art, is yet capable of much melioration by the knowledge of nature, were not difficult to manifeſt, if it were proper here to inſiſt on the proofs of it; yet thus much I ſhall here take notice of, to confirm this fourth obſervation, that not only the philoſopher may, as a mineraliſt, and a mechanician, improve the ways of making iron and ſteel, before they come to the ſmith's hand, but likewiſe may deviſe better expedients, than are among us in uſe, for the ordering of iron and ſteel, when it comes to be formed into weapons and tools. The ſword-blades, and other arms, that are made at *Damaſco*, are very famous every where, and (as far as ſome trials have informed us) juſtly for their excellency in cutting even iron. And yet it ſeems to be only the ſkill of the artificers in ordering it, that gives the ſwords and other inſtruments made at *Damaſco* ſo great a preheminence above others. For though the goodneſs of them have been preſumed to proceed from that of the iron-mines, and ſteel, peculiar to the region of that city; yet the judicious *Bellonius* *, having made particular inquiry at his being there, informs us otherwiſe, and tells us, that iron and ſteel, being brought thither from other parts (the country having no mines of it) receives there from the ſkill of the workmen its temper and perfection. And I ſee not, why I may not reaſonably ſuppoſe, that in the tempering of ſteel, it is not only the goodneſs of the metal, and the determinate degree of heat, though theſe be the only things artificers are wont to look after, that give the beſt temper; but that much may depend upon the nature of the liquors, or other bodies, wherein the hot ſteel is plunged, and upon other ways of ordering it, if thoſe be ſkilfully choſen and employed. I have had a graver ſo well tempered (but by whom I know not) that all the known ways uſed by me and others (who wondered, as well as I, at the unſucceſsfulneſs of our endeavours) could not deprive it of its temper, as they would have done any gravers that we make here; and it was afterwards affirmed to me, that it was made of ſteel tempered at *Damaſco*.

I MAY elſewhere tell you, *Pyrophilus*, both of a way I have tried of hardening gravers, without quenching them in any liquor or tallow, or any other unctuous body; and that having perſuaded an ingenious artificer to try an unpractiſed way of tempering gravers, he ſoon after brought me one to ſee the goodneſs of it, which, by being plunged in a certain cheap mixture (wherewith I may hereafter acquaint you) had been hardened and tempered at once; which though moſt artificers would think ſcarce poſſible, yet, upon the authority of trial, I ſhall venture to deliver, what ſome may think as ſtrange, namely, that though ignition and extinction in cold water be the common and known way to harden ſteel-gravers, yet by that way, only obſerving preciſely a nick of time, ſteel may be made ſtrangely ſoft. But of this more elſewhere. I ſhall now add, that having enquired of one of the curiouſeſt and moſt obſerving makers of ſteel tools, whether he did not find a difference in the employing of pump-water, or river-water, in giving them their temper, he ſatisfied me, that he did ſo; and obſerved the former to be fitter for ſome ſorts of tools, and the latter for others. There may be divers other particulars, wherein iron and ſteel may be improved by the naturaliſt. The firſt may be this; that the metal be rendered ſo ſoft, as to be, by the help of ſtrong moulds, put into ſhapes. This an eminent and credible artificer aſſured me, he had often ſeen his maſter do to iron, with conſiderable profit. Or elſe it may be made fuſible like an-

* P. Bellonius obſervat. Lib. II. cap 93.

other

other metal, as I remember I have (sometimes, with a certain flux-powder, which I composed, if I much forget not, of tartar, sulphur, and arsenick) made it run, even with a charcoal fire, into a mass exceeding hard, and very polishable. A third way may be this; that it be so ordered, as to be preserved very long from rust, which an ancient virtuoso, who had purchased the secret of a rare artist for a great prince, and used to shew his friend's steel so prepared, assured me, was done chiefly by tempering it in water well impregnated with the bark of a certain tree. In a word, there may be divers other ways, whereby iron or steel themselves, or their trades that employ them, may be meliorated; and to add, that on this occasion, there are many and very differing accounts, upon which a trade or profession may be benefited by the Experimental Philosopher; for he may either find out variety of materials, wherewith to perform the things desired by the tradesman, or he may render those materials that are already in use better conditioned; or he may discover and reform the unheeded errors and mistakes to be met with in the trade; or he may devise more easy and compendious ways of producing the effect that is required; or he may improve some of the auxiliary trades, of which the trade spoken of has need or use; or may instruct the artificer to choose, and examine, and preserve his materials and tools better than is usual, or can make the ultimate productions of his trade sooner, or cheaper, or easier, or better conditioned, or applicable to more uses, or more durable, than they are commonly made. Nor are these all the particulars that might here be enumerated to the same purpose, if this fourth consideration had not detained us too long already.

SECTION V.

THE naturalist may increase the power and goods of mankind upon the account of trades, not only by meliorating those that are already found out, but by introducing new ones, partly such as are in an absolute sense newly invented, and partly such as are unknown in those places, into which he brings them into request. For it were injurious both to nature and to man, to imagine, that the riches of the one, and the industry of the other, are so exhausted, but, that they be brought to afford new kinds of employments to the hands of tradesmen, if philosophical heads were studiously employed to make discoveries of them. And here I consider, that in many cases, a trade differs from an experiment, not so much in the nature of the thing, as in its having had the luck to be applied to human uses, or by a company of artificers made their business, in order to their profit; which are things extrinsical, and accidental to the experiment itself. To illustrate this by an example; the flashing explosion made by a mixture of nitre, brimstone, and charcoal, whilst it past not farther than the laboratory of the monk, to whom the invention is imputed, was but an experiment; but when once the great (though unhappy) use, that might be made of it, was taken notice of, and mechanical people resolved to make it their profession and business, to make improvements and applications of it; this single experiment gave birth to more than one trade; as namely, those of powder-makers, founders of ordnance, gunners (both for artillery and mortar-pieces) gunsmiths; under which name are comprized several sorts of artificers, as the makers of muskets, small pistols, common barrels, screwed barrels, and other varieties not here to be insisted on.

THE discovery of the magnetical needle's property to respect the poles has given occasion to the art of making sea-compasses, as they call them, which in *London* is grown to be a particular and distinct trade. And divers other examples may be given to the ſame purpoſe; eſpecially where mechanical tools and contrivances co-operate with the diſcovery of nature's production. So that oftentimes a very few mathematical ſpeculations, or as few phyſical obſervations, being promoted by the contrivance of inſtruments, and the practice of handicrafts men, are turned into trades; as we ſee, that a few dioptrical theories lighting into mechanical hands, have introduced into the world the manufactures of ſpectacle-makers, and of the makers of thoſe excellent engines, teleſcopes, and microſcopes.

The obſerving, that though quickſilver will amalgame with gold (and thereby ſeem to be deſtroyed, which made *Pliny* think it an enemy to metals) yet it may be ſeparated from the gold again, without diminution of that noble metal, has brought forth the trade of gilders, whoſe art conſiſts chiefly in mixing, by the help of a competent heat, good gold with five, ſix, or ſeven times its weight of quickſilver, until the mixture come of ſuch a conſiſtence, that they may ſpread it as they pleaſe upon the ſilver or copper to be gilt. For having by this means overlaid it evenly with gold, they can eaſily with fire force away the mercury; and, with a liquor impregnated with nitre, verdigris, ſal armoniack, and other ſaline bodies, which they call a colouriſh, reſtore its luſtre to the remaining gold, which they after make bright by poliſhing.

The almoſt obvious and trivial obſervation made by ſome ſagacious perſon (whoever it was) that a ſpring was a phyſical, continual, and durable power or force, and the corollary he thence deduced, " that this force, ſkilfully applied, might be equivalent " to the weights that were thought neceſſary to move the wheels of clocks:" theſe reflections, I ſay, joined with a mechanical contrivance, produced thoſe uſeful little engines, watches, that now afford a plentiful livelihood to ſo many dexterous artificers; which, though cuſtom has made familiar to us, yet were unknown to the ancients, and highly prized and admired in *China* itſelf, when firſt (in the laſt century) brought thither. The diſcovery of the virtue of aqua fortis, to diſſolve ſilver and copper without working upon gold, added to the obſervation, that lead melted with either of the two noble metals, and then forced from them by fire, will carry away with it any of the baſer metals that may have been mixed with them; theſe two particulars, I ſay, have begot in latter ages the art of the refiners we now have.

Men's having obſerved the operations of ſome lixiviums, clays, and a few other familiar things upon the juice of the ſugar-cane, has not only occaſioned the adding of the culture of thoſe reeds to the other parts of huſbandry left us by the antients; but has produced the ſeveral trades of ſugar-boilers, or makers of ſugar, refiners of ſugar, and confectioners; not to mention the great addition the concreted juice of the ſugar-cane brings to the apothecaries profeſſion, upon the ſcore of ſyrups, conſerves, electuaries, and other ſaccharine medicines. Nay, a very ſlight manual contrivance or operation, if it light fortunately, may ſupply men with a trade, as in the art of printing. To which I ſhall only add, that in *China*, and ſome other eaſtern parts, the lucky trial that ſome made to bore very ſmall holes through *Porcellane* or *China*-cups, and employ very ſlender wire inſtead of thread or ſilk, has given being to the vulgar trade of thoſe people, that go up and down in thoſe countries, as tinkers do with us, getting their livelihood by ſewing together the pieces of cracked or broken *Porcellane* veſſels; as I have been informed by more than one credible perſon that lived in the Eaſt, and had experience of the uſe of cups ſo mended, though filled with liquors as hot, as they are wont in the Eaſt to drink their coffee and tea.

The mention freſhly made of *China* brings into my mind, that whereas the knowledge of ſome gums and liquors in that country afforded thoſe uſeful as well as moſt beautiful varniſhes, which we call by the name of the kingdom that ſupplies us with them, and

which

which do both there, and in *Japan*, employ multitudes of tradesmen; I am credibly informed, that the art of making the like varnished wares is now begun to be a trade at *Paris*, and I doubt not but it will before long be so in *London* too. For though some accounts, that were given me by virtuosi, of that varnish, were such; that the trials of them did very ill answer expectation; yet having read in *Linschoten*'s voyages, that in *China* and *Japan* they make this excellent varnish of gum lacca, I found by some trials, that I was able to imitate one of the best sorts of it, by dissolving the gum in high rectified spirit of wine *, and then giving it a colour, and laying it on in such a manner, as I may have before long a fitter occasion to inform you.

AND without much impropriety, I might alledge the art of cultivating and gathering sugar-canes, and of ordering their juice, as a recent instance of the transplanting of arts and manufactures. For, as I am informed by very credible relations, there are not very many years effluxed, since, in our memory, a foreigner accidentally bringing some sugar-canes, as rarities, from *Brazil* into *Europe*, and happening to touch at the *Barbadoes*, an English planter, that was curious, obtained from him a few of them, together with some hints of the way of cultivating and using them. Which, by the curiosity and industry of the English colony there, were in a short time so well improved, that that small island became, and is still, the chief storehouse, that furnishes not only *England*, but *Europe*, with sugars. And this instance I the rather mention, because it is also a very notable one, to shew how many hands the introduction of one physico-mechanical art may set on work; since I have had particular opportunity to learn by enquiry, that the negroes, or, as they call them, blacks, living as slaves upon that spot of ground, and employed almost totally about the planting of sugar-canes, and making of sugar, amount at least to between five and twenty and thirty thousand persons. And, that you may see how lucriferous in that place this recent art of making sugar is, not only to private men, but to the publick; I shall add, that by divers intelligent and sober persons interested in the *Barbadoes* (and partly by other ways) I have been informed, that there is, one year with another, from that little island, which is reckoned to be short of thirty miles in length (and so I found it, by measuring it on one of the fairest and recentest maps) shipped off for *England*, especially, ten thousand ton of sugar, each ton being estimated at two thousand pounds weight, which amounts to twenty millions of pounds of that commodity; which, though it may seem scarce credible, yet one of the ancient magistrates of that island lately assured me, that some years it affords a much greater quantity.

I SHALL not fortify what I have hitherto discoursed with particulars that will elsewhere more properly fall in; it being sufficient for my present purpose, that the instances already mentioned may render it probable, that the experimental philosopher may not only improve trades, but multiply them, till I have occasion in the last essay of this book, to make it out more fully. Nor do I despair, that among other ways, whereby trades will be increased, one may be the retrieving some of those, that were anciently practised, and since lost; of which we have a catalogue in the learned *Pancirollus*. For as it is the skilful diver's work, not only to gather pearls and coral, that grew at the bottom of the sea, and still lay concealed there; but also to recover shipwrecked goods, that lay buried in the seas, that swallowed them up: so it is the work of the experimental philosopher, not only to dive into the deep recesses of nature, and thence fetch up her hidden riches; but to recover to the use of man those lost inventions, that have been swallowed up by the injuries of time, and lain buried in oblivion. This I do not say altogether ground-

* See the Appendix to Essay V.

lesly,

lefly, though for fome reafons I here decline mentioning the things, that induced me to fay it.

SECTION VI.

To what has been hitherto faid I fhall venture to add, not only, that the fagacious philofopher may better moft of the trades that are already in ufe, and add to the number of mechanical employments; but that I am apt to think it might, without much hyperbole, be affirmed, that there is not any one profeffion or condition of men (perhaps fcarce any fingle perfon of mankind) that may not be fome way or other advantaged or accommodated, if all the truths difcoverable by natural philofophy, and the applications that might be made of them, were known to the perfons concerned in them. So that befides thofe difcoveries that are compiled or formed into trades, there are, and may be found a multitude of loofe particulars, whereby the naturalift may much gratify and affift men, according to the exigency of particular occafions. The nature of the thing will fcarce permit me to illuftrate fo unlikely an affertion, without employing inftances in themfelves trifling, if not defpicable; of which I will therefore give you but a few, becaufe, if they were not pertinent to my prefent purpofe, they would be fitter to divert than inform you.

I HAD, not long fince, the honour to be known to a very great court-lady, who was much troubled, that having frequent occafion to write letters, fhe could fcarce handle a pen without blacking her fingers with ink. I fmilingly undertook to make her write without ink, which I myfelf was formerly wont to do, by firft preparing my paper with a powder made of copperas, flightly calcined upon a fire-fhovel, till it grow friable, and galls, and gum-arabick finely pulverized, and exquifitely incorporated with the vitriol in a certain proportion; which, though a few trials will better teach than rules (becaufe, according to the goodnefs and calcination of the vitriol, the proportion of the other ingredients muft fometimes be varied) yet to affift you in your firft gueffes, I fhall tell you, that, for the moft part, I ufed myfelf three parts of calcined vitriol, two parts of galls, and one part of gum-arabick, and mixed them not before I was ready to employ them; for this powder being with a hare's foot, or any other convenient thing, carefully rubbed into the paper, and the loofer duft ftruck off, doth, without difcolouring it, fo fill its pores with an inky mixture, that, as foon as it is written upon with a clean pen, dipped in water, beer, or fuch other liquors, the aqueous part of the liquor diffolving the vitriolate falt, and the adhering particles of the galls, makes a legible blacknefs immediately difcover itfelf on the paper. This mention of writing brings into my mind, that feveral times having had occafion to make a word or two, that was but lately written, look as if it had been written long before, I performed it, by lightly moiftening the words I would have to look old with oil of tartar per deliquium, allayed with more or lefs fair water, according as I defired the ink fhould appear lefs or more decayed; which experiments may be often ufeful in manufcripts, to keep the recent interlineations or other additions from betraying themfelves by their frefhnefs not to have been written at the fame time with the reft of the manufcript.

AND the defign I had in making ufe of the lately mentioned powder of galls and copperas puts me in mind of another way of writing without ink (and too without danger of blacking one's fingers or linen) which I remember I have practifed fometimes with one powder, and fometimes with another. For confidering, that common filver being rubbed upon bodies, whofe furfaces are a little rough, and even upon coloured cloth, the metal would leave a blacknefs on it, it was eafy to conclude, that if the furface of the white paper were afperated by a multitude of irregular grains of a powder as white as it, would retain a blacknefs, wherever a blunt filver bodkin fhould be drawn over the grating particles: and accordingly I found, that either exquifitely calcined hartf-horn, or clean tobacco-pipes, or (which is better than that) mutton-bones (taken between the knuckles, and) burnt to a perfect whitenefs, being finely powdered and fearfed, and well rubbed upon paper, would make it fit to be written upon with the point of a filver table-book pin, or bodkin of filver (which metal is not abfolutely neceffary in this cafe,) as well as that, which is called mathematical paper, (if the being prepared with one, or other of thefe powders, do not make it the fame.)

AND now I am upon the mention of fuch preparations of paper, I remember, that I was once in a place, where I could get no white leaves, to fupply a fine table-book that I had much ufe for; nor could I hear of any tradefman in the whole country, that knew the way of making fo much as ordinary table-books: wherefore I bethought myfelf of trying to make fomething by way of fuccedaneum, which fucceeded at the firft attempt. And though there may be better ways to make white table-books, yet perhaps you will find none more fimple and eafy; the two only ingredients we had in it, being to be had at every apothecary's fhop. I only take cerufs, rubbed to very fine powder, (which is done in a trice) and temper it up with fair water glutted with clear gum-ariback. With this mixture (being brought to the confiftence a fomewhat thick falve) I rub over the paper I prepare, putting on more or lefs, according as I would have it laft; and having fuffered it to dry (which it will quickly do) it may, if there be occafion, be prefently ufed with the point of a filver-pin, which will make the letters appear very confpicuous upon a mixture, that does not at all impair the whitenefs of the paper; and what was thus written I could, with fpittle or water, blot out three or four times fucceffively without fpoiling the paper. Which queftionlefs had been much better prepared, if divers couches of the mixture had been laid on, and fuffered each to dry, and if afterwards the paper had been fmoothed by being fcraped with a knife, and polifhed.

A VERY ingenious artificer, who had contrived an inftrument ufeful to others, and profitable to himfelf, whereof an abfolute neceffary part was a glafs filled with fair water, and exactly ftopped, complained to me, that though his inftrument did exceeding well in all but frofty weather, yet then it was apt to be fpoiled by the freezing of the included liquor, which too often broke the glafs. Whereupon I taught him to remedy it, by fubftituting, inftead of water, good fpirit of wine, which has not in our climate been obferved to freeze; or rather, (becaufe in his bigger glaffes, that liquor would be chargeable) either fea-water ftrengthened with a little falt, or elfe common fpring-water with a twentieth, or at moft a tenth part of falt diffolved in it. For though this brine look, if well made, as clear as common water, yet I have not obferved, that the fharpeft of our Englifh winters would make it freeze.

To a perfon of quality, that was very curious of the way of writing fecretly, I undertook to teach an eafy way (which after I knew it, I found alfo in an old printed book) of fending a written meffage, without putting it into the power of the bearer to betray it; which I could eafily have performed myfelf, if the meffage were to be delivered in a fhort time, and not too far off, by writing on his back, or other convenient part of his body, with a clean pen dipped in my own urine, (there being fome urines, with which I have found, to my wonder, that the experiment would not fucceed.) For if he, that receives the meffage, rubs but a little of the black fubftance remaining of paper after it is burnt, thofe fable parts adhering to thofe other of the liquor that lurk yet in the pores of the

the fkin (whence, if the meffenger went faft, and very faft, the fweat would probably diflodge them) do denigrate all that was written, and make it legible enough, fometimes, as I have tried, after many hours.

I REMEMBER too, that intending one fummer to make fome abode at a houfe I had in the country, I fent for from *London*, among other things, a quantity of damafk table-linen, with which he that fent it me, inconfiderately packed up a great pot of a certain confection, which, for fome purpofes, I had caufed to be made of the pulp of floes, which, by agitation of the horfe it was carried on, being brought to ferment, and run out of the broken pot, ftained all the new damafk from the top to the bottom. At which an old domeftick of mine (whom you remember very well) feeming much troubled, becaufe he had fent for it; to convince him, that experimental philofophy was not altogether ufelefs, I fteeped the ftained linen, for fome convenient hours, in new milk; and afterwards caufing it to be thoroughly and diligently wafhed in the like liquor, the damafk came forth unftained, and almoft as white as it. What urine, if duly and long enough employed, may do to take ftains, even of ink, out of linen, is but to be hinted in this place; where I might add, that with ftrong fpirit of falt, wherewith I moiftened, as often as was needful, the fpotted places (firft wetted with fair water) I have out of new linen taken fpots of ink (efpecially frefh ones) of very differing fizes, without leaving (after the linen was well wafhed out in fair water) any of thofe yellow ftains which many call iron-moles.

SOME ingenious perfons, that deal much in lixiviums and brines, complaining the other day, that befides that they could not fometimes eafily come at an egg, to try, by its finking or floating, the ftrength of the faline liquors they would examine, there needed a good quantity of liquor to make fuch a trial in, I allowed their complaint to be juft, and the rather, becaufe I obferve, for nicer eftimates of the ftrength of liquors, the trial by eggs is uncertain enough, in regard, that even the fame egg will, as I have found, by being kept, grow lighter, whence ftale eggs have ufually a great cavity (that feems filled only with air) at the bigger end: and I told them, to omit the more artificial, but more difficult, ways of examining fuch liquors, I fometimes ufed a way, whereby I could try the ftrength of the lixiviums made with chymical falts, though I had not above a thimbleful of the liquor, and this with a body, that will not eafily wafte like an egg, and therefore may be kept. For I fubftituted, inftead of the egg, a fmall piece of amber, about the bignefs of a pea, which in a very ftrong folution of lixiviate falt will, as I let them fee, fwim on the top, but fink in a weak one. And as you may take a piece of amber, lefs or bigger than a pea, as beft fits your occafions, and need not be at all fcrupulous about the figure, (provided the amber be once well ducked in the liquor; fo it is fome convenience, that two pieces of amber, whereof the one is far more reddifh, and the other paler, will be, as far as I have tried, of fomewhat differing fpecifick gravities, fo that the one will float in fome liquors, wherein the other will fink.

I REMEMBER I was once in a country, where I had a great mind to try fome things with Dantzick vitriol, or fome other blue copperas, but, by reafon of the wars, could not poffibly procure any, though there were in that country a place, where green vitriol was made by the help of iron: wherefore getting fome of that liquor, which the rain had wafhed from the copperas ftones, I did, by putting into it a convenient quantity of copper, reduced into fmall parts, make the newly mentioned liquor, ferve for a menftruum to work upon the metal, and by exhaling the folution to a due confiftence, I obtained the blue venereal vitriol I defired. And the like, I doubt not, may be done with fuch of those common green vitriols made of iron, wherein the saline part is not too much satiated with the martial.

An ingenious and well known person, that is a great dealer in cyder, coming to visit me, and expressing a great desire to be able to make some that would be stronger, and thereby likelier to keep longer than the ordinary way, I extempore directed him to an unusual course, for which he afterwards came to give me solemn thanks. The way was to take the strained juice of apples, and in ten or twelve gallons thereof to steep for 24 hours (more or less) about two bushels of the same kind of apples grosly bruised: the apples being lightly expressed, the infusion was (with fresh) repeated once more, (care being to be taken, that the infusion be not made too strong and thick, which may hinder the seasonable clarification of the liquor.)

It was not perhaps difficult to mend this prescription; but I give you the account of it, as I received it from him, because he assured me, that none of his many trials had furnished him with cyder so well bodied, and so much applauded. The cautions that belong to this practice, and the various applications that may be made of this way of making vinous liquors of fruits, without additions (so much as of water,) by infusion, and the varyings of the experiment according to particular cases, I must not here stay to mention.

It was not long since, that accidentally rummaging in a dark place, where I had not of a long time been, and where unknown to me some chymical glasses, negligently stopped, and not written on, had been put; one of them falling down made two or three great stains in the conspicuousest part of a new suit I had then on; and would have obliged me to leave it off, but that judging by the nature of the stain, that it was made with some acid spirit, I tried, by smelling to them, whether among the other bottles, one or other had not some urinous, or otherlike spirit; and lighting on a liquor, which, though I know not what it was, I guessed by the stink, to abound with volatile salt, I bathed the stained parts well with it, and in a trice restored them to their former colour. And, by a like way, I have presently remedied the discolorations made by some sharper and fretting liquors, of died garments of other sorts and materials, which those blemishes would else have rendered altogether unfit for wearing.

Another time, discoursing with a statesman of the ways whereby well-meaning persons may be injured and defamed, I undertook, that out of a parchment-writing, with his hand annexed, I would take out all that was written above his name, without spoiling or disfiguring the parchment, on which I would afterward write what I pleased, and whereby I make people believe, that he had acknowledged under his hand such things, as never came into his thoughts. And to satisfy him of the possibility of this, I did in a few minutes take off from the parchment all that was written on it, without defacing the parchment. Some attempt to free paper from what is written upon it with aqua fortis, but, that by discolouring the paper, make men apt to suspect some intended deceit. And for the true way of performing such an effect, and divers others of the like nature, which I have sometimes for curiosity prosperously experimented, I think it much fitter to be concealed than communicated; because if such secrets should fall into the hands of persons inclined to mis-apply them, they might very much disturb human society. And therefore it is better men should want the light afforded them by such experiments, than be brought into the danger of such mischiefs, as they may be made to suffer by the mis-imployment of such discoveries.

I remember, that not long since, a virtuoso happening to have made a solution of gold, wherewith he thought to make aurum fulminans, thought he had cause to suspect, that it had been enbased with copper, and therefore would not be so fit for his work; where-

whereupon I considered with myself, that a good urinous spirit being employed instead of the usual menstruum (oil of tartar,) as it would precipitate gold out of aqua regis, so it would readily dissolve copper, I conjectured, that by the affusion of such a liquor I might both discover, whether the solution (whose colour did not at all accuse it) contained any copper, and if it did free the gold in great part from the baser metal: and indeed I found, that after the urinous spirit had precipitated the gold into a fine calx, the supernatant liquor was highly tinged with blue, that betrayed the alloy of copper, that did not before appear.

I hope you think, *Pyrophilus*, that it is because these instances are more pertinent to my design, than many others (that might have been substituted) in themselves more valuable, that I have mentioned such inconsiderable ones; and I shall not repent the naming of such instances, if they have let you see, that even mean experiments are not to be despised, but, that the meanest may be sometimes, not only useful, but more proper to convince strangers to natural philosophy of the manifold uses of it, than experiments of a higher and abstruser nature. For as in a shipwreck, it may more advantage the distressed pilot to know the supporting nature of a bladder filled with wind, though otherwise but a despicable and airy thing, than to know the abstrusest properties of the magnetick needle; so, in some cases, the more obvious and slight experiments may be much more welcome and serviceable to us, than others at other times much more considerable. So true is that of the wise man, That every thing is beautiful in its season.

For my part, I am very apt to hope, that natural philosophy will prove more and more serviceable, both to single persons in their particular occasions, and to trades themselves in general; as by other ways, so especially by making a further search into, and thereby detecting new qualities, or discovering unheeded uses, of the productions of nature, and of art, that are already known.

I will not here take notice of what may be further hoped for in the detection of medical virtues of things, because I treat of that subject in a more proper place: and as for the mechanical uses (if I may so call them) and applications of the works and laws of nature, though he that gazes upon the seemingly great variety of productions to be met with among tradesmen, and in the shops of artificers, may be tempted to think, that art has curiously pryed into, and imployed, almost all the materials that nature could afford it; yet he that shall more narrowly and severely consider them, may easily discern, that tradesmen have really dealt with but very few of nature's productions, in comparison of those they have left unimployed; and, that for the most part, they have, in the things they daily converse with, scarce made use of any other, than the more obvious qualities of them; besides some few more lurking properties, which either chance, or a lucky sagacity, rather than inquisitiveness or skill, discovered to them. And indeed this great variety of productions we have mentioned, proceeds more from a manual dexterity of diversifying a small number of known things into differing shapes, than either from the plenty of natural or artificial productions they work upon, or any diligent or accurate search made into the qualities of these productions. But because, to a considering man, it cannot but be obvious enough, that the uses of the things they deal in, and much more those of other concretes, which they are not engaged to observe, have not been hitherto sufficiently enquired into; I shall content myself to add, that if men were but sensible enough of their own interest, and in order thereunto would keep their eyes heedfully open, partly upon the properties of things, and partly upon the applications that may be made of those properties, to this or that use in human life, they might not only discover new qualities in things, (some of which might occasion new trades,) but make such uses of them, as the discoveries themselves would never beforehand

hand have ſuſpected or imagined: whereof I may, God permitting, give you elſewhere divers inſtances.

SECTION VII.

After the foregoing general conſiderations (about the uſefulneſs of natural philoſophy to the empire of man over things corporeal,) which I thought fit to take notice of in this firſt eſſay, it remains, *Pyrophilus*, that I alſo add a word or two about thoſe that are to follow.

And firſt you muſt not expect, that I ſhould methodically enumerate, and particularly diſcourſe to you of all the grounds, and motives I may have of looking for great advantages to accrue to mankind by men's future progreſſes in the diſcovery of nature. To entertain you with conſiderations, which perchance you would judge but ſpeculative and remote conceits, would exceed my leiſure, and perhaps be unwelcome to you; and therefore I chooſe to confine myſelf to the inſiſting on thoſe grounds of expectation, which I can render probable by examples and inſtances of what is already actually attained to, or at leaſt very likely (in no long time) to be ſo. And this advertiſement I thought neceſſary to premiſe, partly indeed, that you may not think, that I have overlooked all the particulars pertinent to my ſubject, that I ſhall leave unmentioned, but much more, that you might not ſuſpect, that there are no other inducements to hope much from experimental philoſophy, than thoſe you will find treated of in the following eſſays. And this one thing in particular I dare not forbear to give you notice of, that for the freſhly intimated reaſon, you will there find omitted one of the principal grounds of hoping great matters from improved phyſiology; namely, that by the ſagacity and freedom of the lord *Verulam*, and other lights of this age, conſidering men are pretty well enabled both to make diſcoveries, and diſcern a poſſibility of removing all the impediments, and other cauſes of barrenneſs, that have hitherto kept phyſicks from being conſiderably uſeful to mankind; ſuch as many falſe and fruitleſs doctrines of the ſchools; the prejudices by which men have been hitherto impoſed on about ſubſtantial forms, the unpaſſable bounds of nature, the eſſential difference betwixt natural and artificial things, &c. a too plauſible deſpondency; a want of belief that phyſicks much concerned their intereſts; want of encouragement; want of natural hiſtory; want of curioſity; want of a method of enquiring; want of a method of experimenting; want of phyſical logick; want of mathematicks and mechanicks; want of aſſociated endeavours; to all which but too many other particulars might be added.

2. You will not think it ſtrange, that in the following tracts much of the uſefulneſs, for which I would recommend phyſicks, ſuppoſes future proficiency in them, if you conſider the nature of my deſign; which is not to make an elogium of natural philoſophy, imperfect as it yet is, but to ſhew, that as it may be, and probably will be, improved, it may afford conſiderable advantages to mankind. And ſince, as I long ago intimated to you, my purpoſe in this book is to invite you, and aſſiſt you to invite other ingenious men, to a farther ſtudy of nature, it is very agreeable to my deſign, to repreſent the greateſt benefits I make it promiſe you, as effects and recompenſes of your future attainments: and I ſhould allowably enough diſcharge my part in this treatiſe, if I ſhould not do any more (which yet I hope I ſhall do) than give you reaſonable inducements to entertain high expectations of the fruits, that may be gathered from natural philoſophy, if it be induſtriouſly and ſkilfully cultivated: and the very rendering ſuch an expecta-

expectation probable, I take to be a good step towards the attainment of the things expected; many of which would questionless be obtained, if men were thoroughly persuaded, that they are most worthy to be endeavoured, and very possible to be compassed. And therefore I wonder not, that so judicious a friend to philosophy and mankind, as Sir *Francis Bacon*, should in several places represent men's opinions of the impossibility of doing great matters of the nature of those things we are speaking of, as one of the chief obstacles to the advancement of real and useful learning: and I the rather insist on the things, that may heighten your expectations, not only because many prudent and learned men, who have been bred in the philosophy of the schools, are apt to judge of all philosophy by that, which for so many ages has been barren, as to useful productions, (though fruitful enough in controversies,) but because I have met with some morose authors, and others as despondent persons, who, because they have unsuccessfully attempted to perform things according to the prescriptions of some unfaithful writers of natural philosophy, fall presently to believe themselves, and to persuade others, that nothing considerable is now (at least without almost insuperable difficulties) to be performed by natural philosophy itself, especially, whilst men amuse themselves about speculations and trials, that seem not to tend directly to practice; our ancestors having had the luck to light upon all the profitable inventions, which skill in physiology is able to supply mankind with. But (to take notice first of what was last suggested) I make no doubt, but that many experiments, whereby men are not presently enabled to do what they could not before, may yet be very useful to men's interest, by discovering or illustrating the nature or causes of things. For though that famous distinction, introduced by the lord *Verulam*, whereby experiments are sorted into luciferous and fructiferous, may be (if rightly understood) of commendable use; yet it would much mislead those, that should so understand it, as if fructiferous experiments did so merely advantage our interests, as not to promote our knowledge; or, the experiments called luciferous, did so barely enrich our understandings, as to be no otherways useful. For though some experiments may be fitly enough called luciferous, and others fructiferous, because the more obvious and immediate effect of the one is to discover to us physiological truths, and of the other, to enable us to perform something of use to the professor; yet certainly there are few fructiferous experiments, which may not readily become luciferous to the attentive considerer of them. For by being able to produce unusual effects, they either hint to us the causes of them, or at least acquaint us with some of the properties or qualities of the things concurring to the production of such effects. And on the other side those experiments, whose more obvious use is to detect to us the nature or causes of things, may be, though less directly, and in somewhat a remoter way, exceeding fructiferous. For since, as I have formerly observed, man's power over the creatures consists in his knowledge of them; whatever does increase his knowledge, does proportionately increase his power. And perhaps I should not much hyperbolize, if I should venture to say, that there is scarce any considerable physical truth, which is not, as it were, teeming with profitable inventions, and may not by human skill and industry, be made the fruitful mother of divers things useful, either to mankind in general, or at least to the particular discoverer and dexterous applier of that truth. To countenance this opinion of mine, I have already given you some instances, and reserve more for the last essays of this treatise; especially having observed it to have been a fault, which, though prejudicial enough to the interest of mankind, is very incident to the more [illegible] and severe sort of philosophers, and perhaps more to them, than to others, to co[illegible] every thing to be impossible, or, at least, unfit to be attempted, that cannot

cannot be performed by the already known qualities of things and ways of applying them; without considering, that as many simples of excellent virtues grow in wildernesses, and not by the highway's side, so divers admirable properties of things may be found, out of the customary progress, or beaten roads (if I may so speak) of nature; and that philosophers are oftentimes deceived, when they think they may have made a true and perfect analysis of the possible ways, whereby such and such effects may be produced. For nature by her subtlety oftentimes transcends and illudes the greatest subtlety of human ratiocinations. And as she may have quite other ways of working than we are aware of, so the knowledge of some peculiar and concealed property of a thing may enable them, that are acquainted with it, to perform that with ease, which, by the known qualities of things, is either not at all to be performed, or not without great difficulty.

THIS seeming paradox you may find in due place confirmed; and in the mean while, to return to those learned men, who having attempted some things, and possibly performed a few in natural philosophy, would keep the world from expecting any great matters from it, I shall venture to say of them, that as the Jewish spies, though they
Numb. xiii. 14. brought their countrymen out of the land of *Canaan*, some few of the goodly fruits of that soil, yet bringing them withal a discouraging account of the difficulties they were like to meet with in conquering it, did the *Israelites* more harm by their despondency, than good by their fruits; so divers of the authors we are speaking of, though they may have presented us with some acceptable fruits of their enquiry into experimental learning, yet by bringing up an ill report concerning the study of it, and thereby deterring irresolute persons from addicting themselves seriously to it, they have more prejudiced them by their despondency, than advantaged them by their experiments. And though I dare not, a chymist would not, scruple to pursue the simile, and tell you, that as only those two of the spies, *Caleb* and *Joshua*, who made no doubt but that they should conquer the fertile (though never so well fortified) land of *Canaan*, did really
Numb. xiv. 28, 29, 3. possess it, all their disanimated brethren wandering and dying in the wilderness; so none but those generous attempters, that dare boldly venture upon the difficulties that surround the knowledge of nature, are like prosperously to overcome them, and possess what they contend for.

BUT I must leave this digression to proceed to the last advertisement I am to give you, which is, that I know you may possibly expect, that I should say something to you distinctly of the chief means, by which the naturalist may probably advance trades, and assist man, by the blessing of the author of nature, to recover part of his lost empire over the works of nature. And I confess, I have more than once had thoughts of a kind of project (if I may so call it) for the advance of experimental philosophy, consisting of such heads as these: a prospect of what probably may be attained to in physicks (both as to theory and practice.) A summary account of what is attained already. The imperfectness of our present attainments. What helps men now enjoy. The incompetency of our present helps. The hindrances and the causes of them. And the means and helps that may be employed. To which other heads might in case of need be added. But notwithstanding the expectations you may have, that I should handle such subjects, and the thoughts I have had about them; I purposely waved the treating of them by themselves in the ensuing essays, partly, because these unelaborate discourses are not designed for a just treatise on the subjects handled in them, containing but such loose experiments and observations, as could, without too much impoverishing other papers, be put together on this occasion; and partly, because I have in effect

effect been careful to mention several of those things, that you might expect to find separately treated of; but knowing, that a far less discerning eye than your's may easily, if there be occasion, distinguish them, I thought it more convenient to interweave them with the other parts of the following discourse, since every proposition of a probable way to improve philosophy is also a ground of expecting those advantages, that may be hoped for from philosophy improved.

OF THE USEFULNESS OF MATHEMATICKS TO NATURAL PHILOSOPHY.

OR,

That the Empire of MAN may be promoted by the Naturalist's Skill in MATHEMATICKS, (as well pure, as mixed.)

IF it were not allowable for any but those, that are thoroughly skilled in the abstruser mysteries of the mathematicks, to discourse of those disciplines, the title of this essay, would, I fear *(Pyrophilus)* make you think me guilty of presumption, since you may perchance remember, that when you were conversant about those studies, I confessed to you, that the great authority of some famous modern naturalists had, for a while, diverted me from making any great progress in those sciences, by their resolute denying them to be useful to physiology. But, as I do not pretend to have taken that pains, which else I might have done, to become a speculative geometrician; so I consider, that without understanding as much of the abstruser part of geometry, as *Archimedes*, or *Apollonius*,

Apollonius, one may understand enough to be assisted by it in the contemplation of nature; and that one needs not know the profoundest mysteries of it, to be able to discern its usefulness. And therefore I shall venture to propound something to you concerning this last-named subject, especially, since otherwise you may be influenced, as I once was, by the great authority of those modern philosophers, who would have the use of mathematicks, as disciplines, that consider only abstracted quantity and figure, to be rather hurtful than advantageous to a naturalist, the object of whose studies ought to be matter. But though these endeavour to keep men from thinking the mathematicks to be of any great use toward making a man a good naturalist, by alledging the extravagant opinions that *Kepler* himself, who was mathematician to three emperors, and some other modern astronomers, have broached or maintained concerning matters physiological; yet I confess, that after I began, by reflecting upon divers of my experiments, especially mechanical, to discern how useful mathematicks may be made to physicks; I have often wished, that I had employed about the speculative part of geometry, and the cultivating of the specious Algebra I had been taught very young, a good part of that time and industry, that I spent about surveying and fortification (of which I remember I once wrote an entire treatise) and other practick parts of mathematicks. And indeed, I think, that a competent knowledge in mathematicks (for a profound one is not always necessary) may be so serviceable to those that would become philosophers, that I shall not scruple to mention it as another thing, which may increase your expectation from physiology, that those, who pass for naturalists, have, for the most part, been very little, or not at all, versed in the mathematicks, if not also jealous of them. And I the less scruple to write to you on this subject, because I do not know that others have prevented me; for though the learned *Clavius*, and some other expositors of *Euclid*, have said much of the usefulness of geometry to other mathematical disciplines; and though not a little has been said in the praise of mathematicks in general; yet it is left free for me to discourse to you of (what is the subject of this essay) the utility of mathematicks, in reference to modern physicks, and therein not only to the notions of the corpuscular philosophy, but even to practical and experimental knowledge.

Now there are several scores, upon which skill in mathematicks may be useful to the experimental philosopher. For there are some general advantages, which mathematicks may bring to the minds of men, to whatever study they apply themselves, and consequently to the students of natural philosophy; namely, that these disciplines are wont to make men accurate, and very attentive to the employment they are about, keeping their thoughts from wandering, and inuring them to patience of going through with tedious and intricate demonstrations; besides, that they much improve reason, by accustoming the mind to deduce successive consequences, and judge of them without easily acquiescing in any thing but demonstration.

And indeed the operations of symbolical arithmetick (or the modern Algebra) seem to me to afford men one of the clearest exercises of reason that I ever yet met with, nothing being there to be performed without strict and watchful ratiocination, and the whole method and progress of that appearing at once upon the paper, when the operation is finished, and affording the analyst a lasting, and, as it were, visible ratiocination.

But, *Pyrophilus*, I may not insist on these, or the like general uses of pure mathematicks, since there are divers others, which more immediately respect natural philosophy.

And to shew this the better, give me leave to premise to the following particulars, a couple of observations.

The

The first is, that the phænomena which the mathematician concurs to exhibit, do really belong to the cognizance of the naturalist. For when matter comes once to be endowed with qualities, the consideration how it came by them, is a question rather about the agent or efficient, than the nature of the body itself. So the image or picture that a man sees of his face in a looking-glass, though that be an artificial body, falls as well under the speculation of the naturalist, as when the like picture is presented him by calm and clear water. And the rain-bows, that are often artificially made in grottos, by dispersing the water of fountains into drops and showers, have a just title to his contemplation, as well as the rain-bow that is formed in the clouds. And the echoes, that are admired in some of those grottos, purposely and artificially contrived to afford rare ones, do as well belong to his cognizance, as those that nature makes in ruder dens, and other cavities of hills and mountains. And indeed most of those phænomena require (for the main) the same solutions, whether the skill of man do or do not intervene to exhibit them.

The second consideration, which I am often obliged to repeat, is this; that since man's power over the creatures depends chiefly upon his knowledge of them, whatever serves to increase considerably his knowledge, is likely, either directly, or in its consequences, to add to his power; which two advertisements being thus given you, *Pyrophilus*, I now advance to the particulars, whose mention they made me suspend.

1. And first, these disciplines teach men the nature and properties of figures, both upon surfaces and solids, and the relations (for they can scarce be properly called proportions) betwixt the surface and solidity of the same body. It is true, that matter, or body, is the subject of the naturalist's speculations; but if it be also true, that most, if not all the operations of the parcels of that matter (that is, of natural bodies) one upon another, depend upon those modifications, which their local motion receives from their magnitude and their figure, as the chief mechanical affections of the parts of matter; it can scarce be denied, that the knowledge of what figures are, for instance, more or less capacious, and advantaged or disadvantaged, for motion or for rest, or for penetrating or resisting penetration, or for the being fastened to another, &c. must be of considerable use in explicating many of the phænomena of nature; and it is sufficiently known, how much of the doctrine of figures may be learned from geometricians, who treating expresly and copiously of triangles, circles, surfaces, elliptical, parabolical, hyperbolical, and other plain figures; as also of spheres, cones, cylinders, and especially prisms, pyramids, cubes, and regular bodies, intimate also the methods of judging of the figures of other bodies, that are either composed of them, or may, by reason of some analogy, be referred to them.

There are divers properties, as well of planes and solid figures, and their habitudes to each other, as of such lines as are described by motions, or wherein motions may be made; the knowledge whereof may be of good use not only to the speculative naturalist, but the practical.

To know the proportion that *Archimedes* has demonstrated to be between a sphere and a cylinder, and either of those to a cone so and so qualified; or to know that a triangular pyramid is the third part of a prism, having the same base and height; and, in a word, to know the proportions between geometrical bodies, may sometimes be of good use, in cases where we can procure the one and not the other, or at least not so well as the other. Of this an instance is given us by the ingenious *Marinus Ghetaldus* (as I find him cited by a late mathematician) who tells us, that *Ghetaldus*, finding it very difficult to procure an exact metalline sphere, wherewith to examine the proportion, in point of weight, between heavy bodies of the same bulk, found, that yet he could get a cylinder of tin to be turned true; and having therewith made his experiments or obfervations, it was eafy for him, knowing out of his *Archimedes*, that the proportion of a cylinder, whofe bafis is equal to one of the great circles of a fphere, and whofe height is equal to the diameter of that fphere, is to that fphere *in ratione fefquialterâ*, as they fpeak, i. e. has the fame proportion that three has to two; it was, I fay, eafy for him, who had often had occafion to weigh his cylinder exactly, by fubtracting a third part of the whole weight, to find in the remainder the defired weight of a fphere of tin, whofe diameter was equal to that of the bafis, or to the height of the cylinder *: which weight of a fphere of a known diameter being once obtained, he deduced from them the weights of the other fpheres he had occafion to employ about the conftruction of thofe tables, which have been much made ufe of by divers fucceeding mathematicians. And what applications I have made of the fame Archimedean theorem, I may elfewhere inform you.

It being alfo taken for granted by divers modern geometricians and engineers, that the excellent *Galileo*, and his not degenerate difciple *Torricellius*, had demonftrated the line, which a heavy body, projected, and even the bullet, fhot out of a cannon, defcribes, to be parabolical; it may be of moment in the practice of gunnery, and in reference to divers experiments to be made with other projected bodies, to be well verfed in the nature of the parabola and parabolical lines, which are alfo thought to be capable of doing wonders in burning-glaffes, in cafe thefe metalline fpecula can be brought to a parabolical figure; one of whofe remarkable properties is, that all the beams that, being parallel to the axis, fall upon the internal fuperficies, are reflected to one point or focus; where confequently, if the burning-glafs be any thing large, the heat muft be very intenfe, efpecially in comparifon of a fpherical burning-glafs of the fame bignefs.

And as for delightful and recreative experiments, you will eafily allow me, that there are abundance of catoptrical ones of that fort, which depend upon the figure of fpherical, cylindrical, and other forts of reflecting glaffes.

2. I might here tell you, *Pyrophilus*, that pure mathematicks themfelves, fetting afide the affiftance they are wont to give to mixed mathematicks, may be of ufe to human life, and to the experimental naturalift; of which I fhall give you, as a fpecimen, this notable example.

The properties of arithmetical and geometrical progreffions in numbers feem to have very little to do with the practice of weighing out things in fhops and warehoufes. And yet by the knowledge of the double progreffion, beginning from an unit (as arithmeticians call that, wherein the confequent is ftill double to the antecedent) as 1, 2, 4, 8, a great deal of cumber, and fometimes of charge, may be faved. For with three weights you may weigh all the pounds that are from one to feven inclufively; with four weights, all thofe that exceed not fifteen pounds; upon which obfervation is grounded the divifion of fome boxes or fets of weights ufed by our goldfmiths. And if you would, as is very ufual, put weights (when there is occafion) in both fcales, to help the thing to be weighed to bring the balance to an æquilibrium, then the triple progreffion (i. e. where the numbers increafe in a triple proportion, as 1, 3, 9.) has a much more notable property for our purpofe; by confidering which, the induftrious *Stifelius* concluded, that by three weights you may weigh any number of pounds from one to thirteen inclufively; with four weights, any number of pounds from one to forty inclufively; with five weights, any number of pounds not exceeding fixfcore and one; and with but fix weights, any number of pounds from one to three hundred and fixty-

* Archimed. propofit. 32. lib. 10. de fphæra & cylindro.

four,

four. But the method of ordering ſo few weights to ſerve ſo many purpoſes is beſt found out by ſymbolical arithmetick or algebra, by which I have taken pleaſure to work ſo fine a problem; which, becauſe it is applicable, not only to pounds, but to the parts of pounds, and thoſe of differing denominations, it may be of ſo great uſe to you, if ever you buſy yourſelf about ſtatical experiments, that I ſhall to the end of this eſſay annex a table, to ſhew what weights are to be taken in every poſſible caſe, which I found ready calculated to my hand by the ingenious *Franciſcus a Schooten*, profeſſor of mathematicks at *Leyden*.

To the former inſtance, of the uſe that an experimenter may make of pure mathematicks, I might, if it could be ſufficiently delivered in a few words, add the method of computing the combinations that may be made of any number of things propoſed, which ſome mathematicians call *Regula combinatoria*. For though I remember not to have found this method fully handled in any one author, even among the modern algebricians; yet, as it is delivered by ſome arithmeticians, it is by no means to be deſpiſed, but, as it may be managed by ſymbolical arithmetick, it will, if I miſtake not, want nothing, but the being ſkilfully applied by the naturaliſt, to be on certain occaſions very ſerviceable to him.

3. We may take notice in the next place, that mathematicks may much help the naturaliſts, both to frame hypotheſes, and judge of thoſe that are propoſed to him, eſpecially ſuch as relate to mathematical ſubjects in conjunction with others.

What wretched theories the ignorance of mathematicks has made naturaliſts, otherwiſe very conſiderable in their way, frame and propoſe, may be evidently ſhewn in the accounts that *Epicurus*, and his paraphraſt *Lucretius*, give of the ſun, and other celeſtial bodies. And indeed what ſatisfactory account can be given of the varying lengths and viciſſitudes of days and nights, and the eclipſes of the ſun and moon, the ſtations and retrogradations obſerved in planets, and other familiar celeſtial phænomena, without ſuppoſing theſe great mundane bodies to have ſuch ſituations in reſpect to one another, and to move in ſuch lines, or at leaſt to be made to appear to move in them by the motion of the earth in ſuch a poſition, and in ſuch lines? Nay, how without the knowledge of the doctrine of the ſphere will the naturaliſt be able to make any ſober and well grounded judgment in that grand and noble problem, which is the true ſyſtem of the world? which is endeavoured to be ſolved after ſuch differing manners by the Ptolomæans and Peripateticks, by the Tychonians and by the Copernicans, both leſs and more modern.

That then the knowledge of celeſtial bodies is not well to be attained, nor conſequently the theories propoſed of them, to be intelligently judged of, without arithmetick and geometry (thoſe wings on which the aſtronomer ſoars as high as heaven) he muſt be very little acquainted with aſtronomy, and particularly with the various and too often intricate theories of planets, that can doubt. And truly, when I conſider the aſtoniſhing diſtance and immenſity of the celeſtial bodies, and thoſe almoſt numberleſs fixed ſtars (each of them perhaps much vaſter than the whole earth) which in a clear night I take pleaſure to gaze at through the better ſort of teleſcopes, both in the milky way, and in other parts of the ſky, that ſeem not ſo much as whitiſh to our eyes; I cannot but highly prize a ſcience that acquaints us, that what we know of ſo much of the univerſe as the globe we inhabit and call the world, is but a point to it, taking up a little more room in it, than a phyſical center in the ſphere.

The uſefulneſs alſo of pure mathematicks to geography is likewiſe evident: and ſure inquiſitive men ought not to deſpiſe this and the former part of learning, without which, as I was lately ſaying, they cannot know ſo much, as whether the earth we live upon moves or ſtands ſtill.

There are alſo divers phænomena of nature, that are neither aſtronomical nor geographical, where the uſefulneſs of mathematicks is manifeſt enough. For as to the phænomena of that ſenſe, to which the naturaliſt is moſt beholding, ſight, what a pitiful account is given of them by thoſe Ariſtotelians, phyſicians, and other writers, without excepting many good anatomiſts, that have been ſtrangers to mathematicks, in compariſon of what has been done (not to mention *Euclid*, *Alhazen*, and *Vitellius*) by *Kepler*, *Scheiner*, *Herrigon*, and ſome other modern mathematicians.

And it is evident to thoſe that are acquainted with dioptricks, that without ſome knowledge, not only of the properties of convex bodies, and of the laws of refraction from and towards the perpendicular (as the maſters of opticks ſpeak) but alſo of the properties of lines, as circular, parabolical, hyperbolical, &c. and figures, as ellipſes, circles, parabolas, hyperbolas, &c. it is almoſt impoſſible, either well to explicate moſt of the phænomena of that nobleſt of our ſenſes, ſight itſelf, or to make a well grounded judgment of others explications of them. He, that is altogether a ſtranger to this part of mathematicks, will ſcarce be able to conceive the reaſon of the admirable fabrick of the eye, and how the cryſtalline humour does by its convex figure (like a lenticular glaſs) refract and converge the beams (or at leaſt the pencils) that proceed from the viſible object, that they may paint the more lively picture of it upon the retina at the bottom of the eye; nor will he underſtand why, by reaſon of the decuſſation of the beams within the eye, this picture muſt be made inverted, though we apprehend the objects themſelves in a right poſture; nor why ſmall objects, placed near the eye, where they are ſeen under a wide angle, appear as big, as very much greater, that are ſeen at a greater diſtance from it. And much leſs will he be able to underſtand the reaſon of thoſe many deluſive apparitions exhibited by concave, convex, conical, and cylindrical glaſſes, the catoptricks, or doctrine of reflex viſion, belonging yet more to the mathematicks than dioptricks do.

4. And ſince that from the magnitudes of divers bodies, or of ſeveral parts of the ſame body, and ſo likewiſe from their degrees of celerity in their motion, there will ariſe a certain reſpect, which if they be but two, geometricians call a ratio, and if more than two, a proportion (though theſe terms are oftentimes confounded, and promiſcuouſly employed by authors; and ſince proportion is ſo frequently to be met with in the works of him, who by an eminent, though apocryphal writer, is truly ſaid to have made all things in number, weight, and meaſure; and ſince the doctrine of proportion, as ſuch, belongs to the mathematician, as the nobleſt parts of thoſe ſciences he treats of; I think it may ſafely enough be affirmed, that he, that is not ſo much as indifferently ſkilled in mathematicks, can hardly be more than indifferently ſkilled in the fundamental principles of phyſiology. Nor perhaps would it be raſh to ſay, that the fifth book of *Euclid's* elements, where the doctrine of proportions is chiefly delivered, may prove more inſtructive to the naturaliſt than the fifth book of *Ariſtotle's* phyſicks. And therefore I do not ſo much wonder, that *Plato* ſhould, over the gate of his ſchool place an inſcription (ἐδεὶς ἀγεωμέτρητος εἰσίτω) forbidding the entrance to perſons unacquainted with geometry, as unfit to judge of what was there taught.

Nay this, though you may think it ſtrange, is very true, that there are ſome conſiderable phænomena of nature, which are ſo far from being explicable by their cauſes, that men cannot ſo much as underſtand what is meant by them, without ſome knowledge of the doctrine of proportions. As, for inſtance, when the teacher of opticks tells us, that the

the increments of light are *in duplicatâ ratione distantiarum, secundùm quas à corporibus recedunt, à quibus primum efficiuntur*; he, that knows nothing of proportions, cannot tell so much as what they mean by this theorem, much less whether or no it be true. And so, when the same proposition is by the diligent *Mersennus* * applied also to sounds, a common reader would not at all understand him, if he did not add by way of explanation, that if, for instance, the noise of a piece of ordnance be heard a league off, that noise will be four times stronger, if it be heard but at the distance of half a league. Nor will this example itself give such a reader, as we speak of, a clear understanding of the proposed theorem. But a considerabler instance in this kind may be afforded us by the noble discovery of the moderns, especially *Galilæo*, who observe, that when a heavy body descends through the air, the spaces past through, from the beginning to the end of the motion, are among themselves in a (not double, but) duplicate ratio of the moments or equal divisions of time spent in the full; which requires the knowledge of what a duplicate proportion is, to be well understood; but it may in some sort be explained, and so noble a phænomenon must not be here omitted, by saying, that *Galilæo* affirms himself to have observed, that a brass bullet of 100 pounds will, in the space of one minute of an hour, descend an hundred Florentine cubits (which some reckon to be 180 feet of ours, and consequently, saith *Mersennus*, four cubits in one second, or sixtieth part of a minute; and by adding, that the bullet falls in such a ratio, that the acceleration of the motion is made according to the progression of odd numbers, beginning from an unit, or one; so that if in the first moment of time the weight fall down one fathom, in the second moment it must descend three fathoms; in the third, five fathoms; in the fourth, seven; in the fifth, nine; in the sixth, eleven; and so onward. Whence *Mersennus* gives this rule, to know how far the weight will descend in a determinate time assigned; and by knowing how far it has descended, to calculate how long it was in falling. † *Regula generalis*, says he, *hæc est. Si dentur tempora, & quærantur spatia, quadrentur tempora, & habebuntur rationes spatiorum. Si dentur spatia, & quærantur tempora, investigetur latus spatiorum, & dabitur ratio temporum.*

Divers other instances might be produced, to manifest the requisiteness and advantageousness of some knowledge in mathematicks to a speculative naturalist; but I shall content myself to name one more, viz. that the grand theorem or rule of the staticks, that in the balance, or resembling instruments, the proportion betwixt the equivalent weights, and their distances from the fulcimentum or prop, is reciprocal (so that it is usual with butchers and other tradesmen, to weigh in the statera, commonly called the stiliards, 10 or 20 pounds weight, for instance, hung near the fulciment, with one pound weight placed on the other side of the beam, at 10 or 20 times distance from it) and many other theorems that serve to explicate the properties of the grand instrument of nature, motion (especially as produced or modified by weight, or equivalent force variously adapted, and applied) cannot well be understood without an insight into geometry, and especially the doctrine of proportions; and how much the knowledge of the principles and theorems of the mechanicks may assist the naturalist, both to explicate many of nature's phænomena, and to try experiments, and work great changes on her productions, men will then more readily confess, when they shall better discern how many of her works are but engines, and do operate accordingly.

5. And give me leave, *Pyrophilus*, to add in this place, that the doctrine of proportions, as it is the soul of the mathematicks themselves, so it may be of vast, though

* Harmonic. lib. I. prop. 12.
† Mersen. Harmon. lib. I. Propos. 24. Corollar. 1.

perhaps

perhaps yet unheeded, use in physiology too; not only as it helps the naturalist (as we have newly seen it does) to understand divers phænomena of nature, but as it may enable him to perform divers things, which he could not perform without it; of which though I may have occasion to give you hereafter in other papers several examples, yet I shall now mention two or three for illustration sake.

THAT the pendulum is the accuratest instrument that we yet have of measuring short spaces of time, I presume, you do not doubt; and I need not tell you, that he, who would know what length a pendulum must be of, to measure by its swing some determinate space of time, as, for instance, a half second (or half the sixtieth part of a minute) must find it out by trial and observation, if he be not acquainted with the doctrine of proportions; but in case he is versed in that, as well as in the phænomena of pendulums, he may from the length of one pendulum, that exactly measures a known part of time, without making particular trials and observations, deduce the length of pendulums that will serve to measure other divisions of time. For instance, that diligent observer *Mersennus* assures us, that he found by frequent trials, that a slender string with a pistol or musket bullet at the end of it, whose length comprehending the bullet, was three feet and a half (elsewhere he mentions three feet and a 27th) vibrates second (minutes); this now being taken for granted, and it being a received theorem concerning pendulums alike in all things but length, that the lengths are in duplicate proportion to the times in which their vibrations are respectively performed, or are as the squares of the vibrations they perform in the same time, and consequently, the times are in subduplicate proportion to the lengths of the pendulums; if a man would, as I was saying, have a pendulum that shall vibrate half-seconds, he must not take, as one unacquainted with these things would be apt to do, a pendulum of a foot and three quarters, which is one half the length of that which vibrates a whole second, for such a pendulum would prove much too long for his purpose; nor need he by multiplied observations laboriously find out how much it is too long (which oftentimes for want of a standard he cannot do) but since the proportion between a second and half a second is double, and the proportion betwixt the length of the strings, that are to vibrate these two differing spaces of time, must be duplicate of the proportion of the times themselves, it follows, that the length of the strings must be as four to one, which is the duplicate of the proportion of two to one, and so the length of the shorter string must be but a fourth of that of the longer.

THIS, if it were needful, might be confirmed by a problem of the learned *Ricciolo's*, whereof I shall here give you an example, because I may hereafter have occasion to shew you the farther use of it. Let us then suppose, to avoid fractions, that a pendulum that vibrates seconds, is three entire feet long (as indeed some modern mathematicians tell us it is, and as it may well be according to the measures used in some places). If then you multiply 3600, the square of the vibrations, which are 60, that your three feet pendulum makes in a second, by the length of the pendulum, which is 36 inches, and divide the product, viz. 129600, by 9 inches, the fourth part of the length of the former pendulum; and if lastly, of the quotient (14400) you extract the square root, you shall find it to be 120, that gives you the number of vibrations that will be made in a second by a pendulum of nine inches long, and this root being twenty, which is the double of sixty, you may see, that to make a pendulum that shall vibrate half-seconds, it must be but one quarter so long as that, which vibrates whole seconds. And if I thought you were like to think these rules as strange, as a person wholly unacquainted with the nature of pendulums, and the doctrine of proportions may do; I would invite you to consult experience, as I have purposely done in differing pendulums, that divide a minute into seconds, half-seconds, and quarter-seconds; since though your trials should

not

not be very nicely made, they may suffice to persuade you, that the above-mentioned rules are either accurately true, or at least true for the main, and therefore true enough to be very useful in many occurrences.

To the above-mentioned instances afforded by pendulums I shall here add but one more, that comprehends many thousands; for the art of composing of that great variety of harmonious tunes, that makes musick so delightful to us, depends upon the doctrine of proportions. And he, that being well skilled in that, knows how to apply it to the notes or words proposed, according to the observations which experience has afforded, of the gratefulness of such and such consonancies, &c. may out of his own head compose a strange variety of new and pleasing tunes, which are so many exercises, that man makes of the power his skill gives him over the bodies, of which his musical instruments consist, and over those which they affect.

6. I KNOW not, *Pyrophilus*, whether I may not reckon amongst the advantages that mathematicks may afford the naturalist, that they will in many cases suggest to him divers new experiments, whereby to vary those, wherein the figures of bodies, the lin s of motion, as also numbers, proportions, and the like affections, which the mathematician is wont to treat of, may come into consideration. For it is very likely that those suggested experiments, which either would not be thought on, or could not be skilfully proposed, by a person not versed in mathematicks, may, either immediately, or upon the score of the applications that may be made of them, prove serviceable to men; of which I hope in one of the following essays to give you some instances. See Essay IX.

I CARE not to mention to you, how great a variety of trials and observations, about the best way of levelling great guns, and the differing distances to which they will carry at such and such elevations, and the lines described by the motion of the bullet, and other particulars belonging to the art of gunnery, have been proposed and tried, upon the hints suggested by geometry's mathematical disciples (especially) and others, because many good men wish these fatal arts had been less understood. And therefore I shall rather put you in mind of the great variety of phænomena, which pure mathematicks have helped men to discover and derive from these familiar observations; that a beam of light, passing through differing mediums, is not continued in a straight line, but broken or refracted; and, that in such and such conjunctures of circumstances, the sun or moon will suffer an eclipse that will obscure such a part of the body, and last from such a time to such a time; from which observations of eclipses divers very considerable things have been deduced by mathematicians, not only as to astronomy, but also geography, navigation, and chronology. And he that considers what the doctrine of proportions, and of concords (or, as our musicians call them, cords) and discords, has contributed to the great number of musical instruments that have been actually made, and delightfully practised, and that it may afford the naturalist divers hints applicable to other purposes (which I shall hereafter have occasion to intimate) he, I say, that considers these things, especially if he be also acquainted with ingenious, pleasant, and some of them useful experiments, that have been or may be derived from the observations, that when a beam of light falls upon a body, and rebounds from it, the angle of incidence is equal to that of reflection; that if the superficies of the body be curve, the angle is to be estimated as if it fell upon a tangent to that superficies; that if the beam penetrate the body, and come to it through a thinner medium, it is refracted towards the perpendicular, if through a thicker medium, from the perpendicular; he, as I was saying, that shall consider these things, and withal, what a great variety of propositions, as well problems as theorems, have been deduced by mathematicians by the help of these few observations, and of as few propositions touching the place of the object seen by the help of specular and dioptrical glasses, will easily grant, what by so many instances I have been endeavouring to prove.

7. I COME now to the consideration, wherewith I shall conclude this essay, viz. that divers disciplines that are reckoned amongst the mixed mathematicks, are chiefly practical, and may assist the naturalist in making experiments and observations, which he either could not make, or could not make so accurately without them; as may appear, partly by the art of dialling, which teaches how to measure time, and tends chiefly to practice; partly by the art of perspective, which is of great use to represent solids and distances upon a small and plain superficies, and is very serviceable to the limner's art; wherein if scholars and travellers were more generally conversant, the history of nature would be far better adorned with lively representations of plants, animals, meteors, &c. and also by several parts of the art of navigation, and particularly that which they call histriodromia, or the doctrine of the lines, by which pilots make their ships to sail. Now if in these and divers other instances that may be given, it must be acknowledged, that mixed mathematicks may be serviceable to the naturalist, and assist him to promote the empire of man; it ought not to be denied that pure mathematicks themselves, as vulgar arithmetick, geometry, and algebra, may be of use to the naturalist, since it is from those speculative parts of the mathematicks, that not only these other more practical disciplines are derived, but a greater number of those disciplines that are called mixed mathematicks, may, according to what I elsewhere observe, be hoped for. For as sounds and pure mathematicks make up musick, and water with the same sciences make hydrostaticks; so, as I elsewhere note, by a further application of the same parts of knowledge to other subjects (and in some cases even to the same) those disciplines that are called mixed mathematicks, may be advanced probably as to number, as well as certainly as to usefulnesses and variety of experiments. Nor is it only in those parts of learning that I have now particularly named, that useful applications may be made of the theorems and problems of pure mathematicks, since upon these sublime sciences do also in great part depend those other mathematical disciplines, which are wont (by a synecdoche) to be called mechanical, and which it is now time that I pass on to consider.

OF THE

USEFULNESS

OF

MECHANICAL DISCIPLINES

TO

NATURAL PHILOSOPHY.

SHEWING

That the Power of Man may be much promoted by the Naturalist's Skill in MECHANICKS.

TO prevent the danger of ſtumbling (as they ſpeak) at the threſhold, I ſhall begin this diſcourſe with advertizing you, that I do not here take the term mechanicks in that ſtricter and more proper ſenſe wherein it is wont to be taken, when it is uſed only to ſignify the doctrine about the moving powers (as the beam, the lever, the ſcrews, and the wedge) and of framing engines to multiply force; but I here underſtand the word mechanicks in a larger ſenſe, for thoſe diſciplines that conſiſt of the applications of pure mathematicks to produce or modify motion in inferior bodies; ſo that in this ſenſe they comprize not only the vulgar ſtaticks, but divers other diſciplines, ſuch as the centrobaricks, hydraulicks, pneumaticks, hydroſtaticks, baliſticks, &c. the etymology of whoſe names may inform you about what ſubjects they are converſant.

Now that theſe arts (if you will allow them that name) may be of great uſe to the experimental philoſopher, and aſſiſt him to enlarge the empire of man, may be made probable by this general conſideration, that divers of thoſe things, which in the former eſſay have been evinced to make the mathematicks uſeful to the naturaliſt, may be applied *mutatis mutandis* to the mechanicks alſo. Beſides, that theſe diſciplines have ſome advantages peculiar to themſelves. But the truth of what is thus repreſented in general terms will poſſibly be better diſcerned, and more perſuaſive, if we deſcend to ſome particulars.

I. First then, the phænomena afforded us by these arts ought to be looked upon as really belonging to the history of nature in its full and due extent. And, therefore, as they fall under the cognizance of the naturalist, and challenge his speculation; so it may well be supposed, that being thoroughly understood, they cannot but much contribute to the advancement of his knowledge, and consequently of his power, which we have often observed to be grounded upon his knowledge, and proportionate to it. When, for instance, we see a piece of wood, ducked under water, emerge again and float, even vulgar naturalists think that it belongs to them to consider the reason of this emersion and floating, which they endeavour to render from the positive levity, which they fancy to be (upon the account of the air and fire) inherent in the wood, though some woods that will swim in water, being put into oil, or high rectified spirit of wine, may sink.

But I see not, why it should not belong to philosophers to consider and investigate the reason, why one part of floating wood appears above the water, whilst the other keeps beneath it; and why the extant part is equal to the immersed, or either greater or lesser than it, in such a determinate proportion; and why the same wood will sink deeper in some waters than in others (as in a river than in the sea) as on the other side some woods will sink lower than others in the same water. For if these things be duly examined, as they may by the help of hydrostaticks, not only the cause of these and the like phænomena will be discovered; but by the applications of that discovery an easy way may be devised to measure and estimate the differing strength of several salt springs, and also of divers kinds of lixiviums, and brines; to which may be added divers other practical corollaries from the same discoveries, which I shall hereafter have occasion to particularize.

II. The mechanical disciplines help me to devise and judge of such hypotheses as relate to those subjects, wherein the notions and theorems of mechanicks either ought necessarily to be considered, or may usefully be so.

Of this we have instances, not only in those engines that are artificial, and are looked upon as purely mechanical, as the screw, the crane, the balance, &c. but in many familiar phænomena, in which the theorems of mechanicks are not wont to be taken notice of to have an interest; as in the carrying a pike or musket on one's shoulder, in the force of strokes with a longer or shorter sword or other instrument, the taking up and the holding a pike or sword at arm's length, and the power that a rudder has to steer a ship; in rowing with boats, in breaking of sticks against one's knee, and in a multitude of other familiar instances, of which the naturalist's skill in mechanicks will enable him to give a far more clear and solid account than the ancient schoolmen, or the learnedest physicians that are unacquainted with the nature and properties of the centre of gravity, and the several kinds of levers, the wedge, &c.

III. Nay, there are several doctrines about physical things, that cannot be well explicated, and some of them not perhaps so much as understood, without mechanicks.

That which emboldens me to propose a thing that seems so paradoxical, is, that there are many phænomena of nature, whereof though the physical causes belong to the consideration of the naturalist, and may be rendered by him; yet he cannot rightly and skilfully give them without taking in the causes statical, hydrostatical, &c. (if I may so name them) of those phænomena, i. e. such instances as depend upon the knowledge of mechanical principles and disciplines.

Of this we have an obvious example in that familiar observation, that we partly touched upon just now, about the swimming and sinking of wood in water. For if it be demanded, why wood does rather swim upon water than sink to the bottom of it, a school-philosopher would answer, that wood abounds with air, which being an element very much

much lighter than water, keeps it aloft upon the furface of that liquor. But this anfwer will fcarce fatisfy a naturalift verfed in hydroftaticks. For not now to queftion what is taken for granted, that there is a pofitive levity, and that the air is endowed with that quality, experience fhews us, that though when wood is not heavier than fo much water, as is equal to it in bulk, it will fwim; yet in cafe it be heavier than fo much water, it will fink. As we fee in divers woods, and particularly in guaiacum, which I therefore the rather name, becaufe chymifts obferve, that if it be burnt, it leaves far lefs afhes (and fuch are fuppofed to contain the terreftrial and heavy parts) behind it, than many woods that we know will float in water. And though ftones and iron be, upon the fcore of their weight, believed to be bodies that have little air in them, yet if the liquor into which they are put, be heavier, bulk for bulk, than they, they will not fink, but float, and if forcibly depreffed, they will emerge; as you may try, when you pleafe, by putting ftones or iron, or the like ponderous body, upon quickfilver or melted lead; fo that we need not here confider, whether air be, or be not predominant in a propofed body, when we would know whether it will or will not fink in an affigned liquor.

And though we fhould admit the air, whether included in the pores, or looked upon as an elementary principle, to be the caufe of its being lighter than an equal bulk of liquor, yet the air would be but the remote caufe of its fwimming, its immediate caufe being, that the floating body is lighter than an equal bulk of the liquor, and therefore the fame body, without acquiring or lofing air, may fwim in one kind of water, and fink in another. As in the cafe of heavy bodies, as loaden fhips, that having profperoufly failed over the fea, are recorded to have funk as foon as they come into harbour, i.e. into a more frefh water; and an egg, that will fink in common water, will fwim in a ftrong brine. Nay a body may (as I, and others have tried) be fo poifed in water, that if the liquor be a little warmer, than when the body was poifed in it, the body will fink; as it will emerge again upon the refrigeration of it.

And if this general anfwer of the lightnefs of the air will not give fo good an account as hydroftatical principles, why a piece of wood will float or fink, it will much lefs give fo fatisfactory an account, why differing woods in the fame water, or the fame piece of wood in differing waters, will fink juft fo far, and no farther; whereas, by hydroftatical principles, the phænomenon is eafy to be accounted for, according to that theorem of *Archimedes* * περὶ τῶν ὀχυμένων, that folids lighter than the liquor they are put into, will fink in it fo far, as that as much of the liquor as is equal in bulk to the demerfed part, be equal in weight to the whole floating body; whence thefe corollaries are derived, that a floating body has the fame proportion in weight to as much liquor as is equal to it in bulk, as the immerfed part of the body has to the whole body. And likewife, that as much liquor, as is equal in bulk to the whole body, has the fame proportion in weight to the faid body, as the whole body has to that part of itfelf which is beneath the furface of the liquor. And as thefe corollaries determine the proportion between the immerfed and extant part of the floating body; fo (to fhew you that thefe theories lead to practice) they fuggeft the way of making a fmall and light inftrument, elfewhere defcribed, to meafure by a floating body the differing gravities of feveral liquors in reference to one another, as well as to the body itfelf. And upon the fame grounds, the learned *Stevinus* fhews, that if you know what part of a floating body is immerfed in a liquor, whofe fpecifick gravity is alfo known, as it eafily may be, you may prefently find the weight of the whole folid body, let it be never fo much too great to be weighed

* Lib. I. Prop. 5.

in balances or ſtateras, yea, though it were a vaſt ſhip itſelf; as ſuppoſing that that part of ſuch a veſſel that lies under water, ſhould be 100,000 cubick feet, and that a cubick foot of water weighs 70 lb. (which though it be not the weight we have obſerved a foot of water Engliſh meaſure to amount to, yet that alters not the general rule) by multiplying 100,000 by 70, the product will be 7,000,000 lb. for the weight of the whole ſhip, with all that is contained in it, as ballaſt, ordnance, &c. or reſts or leans upon it. If I ſhould aſk a mere ſchool-philoſopher, why ſucking-pumps will not raiſe water higher than 40 feet (though it be commonly preſumed they will raiſe it to any height) or why in an inverted ſiphon of glaſs, if you pour water and quickſilver in a ſufficient quantity, the ſurface of the water in one leg of the ſiphon will not be in a level with the ſurface of the quickſilver in the other, but 13 or 14 times as high above the bottom of the ſiphon; or why, if a piece of iron, and a piece of marble or a flint, &c. be equiponderant in the air, if the ſcales be let down into the water, the metal will appear far heavier than the ſtone; if, I ſay, I ſhould aſk a mere naturaliſt both theſe or the like queſtions, I doubt I ſhould much more perplex him, than he would ſatisfy me. And it were eaſy to add a multitude of examples, whereof a good account will ſcarce be given by a naturaliſt that is unacquainted with mechanicks, and may eaſily be aſſigned by one that is ſkilled in them. But referring the ſchoolmen to *Ariſtotle*'s mechanical queſtions, to ſhew them the neceſſity and uſefulneſs of mechanical knowledge, to give the ſolution of ſundry phænomena that frequently occur, I will only add an example or two to make good the moſt paradoxical part of what I was ſaying; namely, that there are divers phyſico-mechanical phænomena, which are not to be, I ſay not explicated, but ſo much as well underſtood, without the knowledge of mechanical diſciplines.

There is a conſiderable theorem in hydroſtaticks, which is thought to have been firſt taken notice of by *Merſennus*, and in a late writer, is thus expreſſed: *Velocitates motus aquæ deſcendentis & effluentis per tubos æqualium foraminum, ſed inæqualium altitudinum, habent ſubduplicatam rationem altitudinum.* Of which the corollary is, that the tubes are in a duplicate ratio to that of the velocities of the water that ſubſides in, and runs out of them; ſo that to make one tube at a circular hole of the ſame diameter run out in the ſame time twice as much water as another, the greater ought to be not only twice, but four times as long as the ſhorter. And of the ſame proportion (my trials about which I may elſewhere acquaint you with) divers other practical applications may be made, which muſt not be here inſiſted on.

IV. As I formerly ſaid of the mathematicks, ſo I now ſay of the mechanicks, that they may aſſiſt the naturaliſt to multiply experiments by thoſe enquiries, that they will ſuggeſt, and thoſe inferences and applications, whereto they may lead us.

Of this we have a noble inſtance in the great variety of trials, which enquiries, verſed in hydroſtaticks, and other mechanical diſciplines, have, upon the ſcore of their being ſo qualified, been either prompted, or at leaſt aſſiſted to make, about the famous quickſilver experiment deviſed by *Torricellius*; about which, though ſo much has been done already, yet almoſt every year brings forth new phænomena.

Another example to our preſent purpoſe we may take from the great number of new propoſitions, that the diligent *Merſennus* has given us in his baliſticks, about the force and effects of bows, and the like ſpringy bodies. But a yet more noble inſtance is given us by the moſt ingenious *Galilæo*, who, as we may learn from the already mentioned French writer, that has given us an account of *Galilæo's* new thoughts in that language, has publiſhed ſo many propoſitions (of which he ſets down 19 or 20, with the demonſtrations) about the reſiſtance of bodies to be broken, and the weights requiſite

to

to break them, and the lengths at which they may be broken by their own weight, that he has reduced them into the form, and given them the title of a new art.

To all which I ſhall need to add no more, than that he, who knows and conſiders what a variety of uſeful propoſitions have been, or may be mechanically deduced from the obſervation of *Archimedes*, that a ſolid body weighs leſs in water than in the air, by the weight of water equal in bulk to that body, will eaſily diſpenſe with me for not adding any farther inſtances on this occaſion.

And the mention of this hydroſtatical propoſition of *Archimedes* falls in the more properly in this place, becauſe it will warrant me to tell you, that divers mechanical theorems are not only fertile in other theorems, but in uſeful applications too, of which I may hereafter have occaſion to give you ſome examples, by acquainting you with the uſes I have made of the lately mentioned propoſition of *Archimedes*, and ſome corollaries, that partly by others, and partly by us, have been inferred from it.

V. Besides the utilities that may be aſcribed to the mechanicks in common, with the more ſpeculative mathematical diſciplines, they have ſome, as I formerly intimated, that are more peculiarly their own, ſince they may be of great uſe to the naturaliſt in making of ſuch inſtruments and tools, as for many of his obſervations, trials, and other purpoſes, he may either abſolutely need, or advantageouſly employ.

Of this we have an example in the mariner's compaſs, as it is called; which is ſo neceſſary to thoſe remote navigations, whereto natural philoſophy and mankind owes ſo much. For though *Baptiſta Porta* * does, as well as other authors, aſcribe the invention of the directive faculty of the magnetick needle to one of his countrymen (*Amalphi*, in the kingdom of *Naples*) yet he confeſſes, that for want of the knowledge of making ſuch ſea-compaſſes as we now uſe, this lucky inventor was fain to make uſe of a piece of wood or ſtraw, to keep the needle a-float, and then imbue it with a magnetick virtue; which was a ſhift ſubject to great and manifeſt inconveniencies. And indeed, notwithſtanding the knowledge of the verticity of magnetical needles, if by that of the properties of the center of gravity, or ſome practices derived thence, ſome men, verſed in mechanicks, had not deviſed a way ſo to poiſe the needle, that notwithſtanding the rolling and toſſing of the ſhip, it will continue horizontal enough to direct the pilot; what would become of him in thoſe ſtorms, when he has moſt need of a faithful guide?

By the help of the centrobarical doctrine, mechanicks have been enabled to make thoſe dipping needles, whoſe phænomena are very odd; and though, as far as I have tried, they yet ſeem uncertain enough; yet it may very poſſibly happen, that farther obſervations may reduce them to ſome theory, whence practical inferences may be deduced.

And you will the more eaſily believe, that the mechanical applications of centrobarical notions may be of immediate uſe, if we conſider, that by virtue of them, divers writers, and others of unſuſpected credit, aſſure us, that they have made a kind of lamp ſo poiſed, that one may roll it up and down like a bowl, without overturning the veſſel that contains the oil, or extinguiſhing the flame.

From the knowledge that compreſſed air has a ſpring, whereby it reſiſts farther compreſſion, and a ſlight contrivance to make uſe of this pneumatical principle, an acquaintance of mine made a ſlight engine, which afterwards I found mentioned in a printed book, by which he was a great gainer, going, when he was well ſatisfied for his pains and hazard, to the bottom of the ſea, and by the help of this engine ſtaying there ſome-

* Mag. Nat. lib. VII. cap. 7.

times

times for divers hours, till he had fetched up valuable things out of sunk ships, and tied cables about their guns, that they might afterwards be buoyed up.

But there might be given so many examples of instruments and tools that are useful to the naturalist, and for which, yet, he ought to thank the mechanicks, that it were tedious to enumerate them, especially since the shops of mathematical instrument-makers, and other tradesmen, may supply you with enough of them, to verify what this paragraph would persuade.

VI. I shall conclude the considerations I designed for this essay by this, that as the knowledge of the theorems of mechanicks, and the practices which have been thence derived, may very much assist the naturalist to make good mechanical contrivances, according to the exigencies of his several purposes; so one good mechanical contrivance may be equivalent to, and may perhaps actually produce many good experiments.

The former part of this proposition will not, I think, require much proof. For a man must be but a dull naturalist, that shall know the properties of the center of gravity, of levers, balances, screws, wedges, and other instruments for increasing force, and by frequenting the shops and work-houses of mechanicians, shall have seen variety of engines and instruments to compass different things, if he do not, from the survey and consideration of all these, grow more able, by compounding, varying, and otherwise improving them, to devise such means and expedients, as he would not else have thought on, to make some trials, that he could not make before, and to make others more accurately, or more easily, or some way or other better.

And as to the second part of our proposition, namely, that one good mechanical contrivance may be as considerable as many particular experiments, by enabling the naturalist to produce either numerous, or noble ones, or both, it may be manifested by several examples.

And I shall begin with so familiar a one, as that afforded by valves, or trap-doors. For as slight and obvious as the invention of them seems, yet not only we owe to them a great variety of pumps and bellows for œconomical uses, but they make very considerable parts of several other engines, and may, as some trials have informed us, be applied about several new experiments, especially if they be made of brass, and yet so small, that like some of those I have had made by skilful workmen (who, when I first directed them, told me, that they could not be made) they may be used, not only in small glass-pipes, but in syringes themselves.

By the help of small valves, and the knowledge of the spring of compressed air, have been made those wind-guns, which may be employed, not only to weigh the air (whose weight we found them to evince, but not determine) but to kill deer, and other game, without making a great noise that would fright away the rest.

If I did not, *Pyrophilus*, foresee, that in the following essays of this treatise, I shall have occasion to mention some other instances of the service, that mathematical and mechanical disciplines may do the naturalist, I should here add divers particulars, which I had rather you should, when you meet with them, refer hither; and therefore I shall conclude what I intended now to say about these disciplines, by two or three short instances that relate to what I have already said concerning them.

The first is, that it was not my design to treat of the utility of the mathematicks and mechanicks in an absolute way; for then I must have said much to their advantage, which I have omitted, because it would have too much swelled these essays, and not have been pertinent enough to them. And therefore I thought it sufficient for me to touch upon those things, on whose account these disciplines may be made useful to the naturalist, by assisting him either to frame theories, or to make observations and experiments,

ments, ſome (at leaſt) of which, directly, or in their applications, either are already, or are like to prove, practical and uſeful. And it ſeems to me very probable, that the notions and practices of theſe diſciplines that have been too much hitherto reſtrained by mere mathematicians and mechanicians to the ſtars, the earth, the water, and ſome few other conſpicuous parts of nature, may be very well extended by a philoſopher to ſundry other productions, as well of nature, as of art. As *Archimedes* deduced hydroſtaticks from the application he made of vulgar ſtaticks to bodies weighed in air and water, or in water only; and the ingenious *Torricellius*, and others, have of late applied the principles of hydroſtaticks to that ponderous body (which the chymiſts reckon among metals) mercury.

My next advertiſement is, that mentioning mechanical inſtances, not ſo much to acquaint you fully with the things themſelves, as to make the mediums to infer what I would prove, I have taken the mechanical propoſitions, that I employed, as they are delivered by the artiſts themſelves, without warranting that their proportions will hold true in mathematical ſtrictneſs. For though I have made trials myſelf of ſeveral things of this nature, yet having often obſerved, how difficult it is to find a mathematical preciſeneſs in phyſical and mechanical things, I think it not amiſs to intimate thus much to you, though I may elſewhere have a fitter opportunity to make it out, that ſo great an exactneſs is in many caſes not neceſſary to make the rules, that want it, uſeful in practice.

The concluding intimation I mean to give you, is, that I have not hitherto mentioned a ſervice, that mathematicks and mechanicks may often do the naturaliſt, which is not fit to be ſilently pretermitted; and it is, that by lineal ſchemes, pictures, and inſtruments, they may much aſſiſt the imagination to conceive many things, and thereby the underſtanding to judge of them, and deduce new contrivances from them.

That I do not groundleſly ſay this, you will grant, if you conſider, how difficult (not to ſay impoſſible) it were to go through with a long geometrical demonſtration, without the help of a viſible ſcheme, to aſſiſt both the fancy and the memory; and how difficult it is to give beginners an idea of the grounds of coſmography and geography, without material ſchemes and globes, your own very recent experience, as well as that of others, will, I preſume, inform you. As it alſo may, how uſeful, not to ſay how neceſſary, pictures, and in ſome caſes, models, are wont to be, when engines, houſes, ſhips, and other ſtructures are to be judged of, that they may be approved or improved; but I ſhall rather take notice, that not only mechanical, mathematical, and anatomical things, need ſchemes and pictures to repreſent them clearly to our conceptions; but many things that are looked upon as more purely phyſical, may, in my opinion, be much illuſtrated the ſame way. Of which, if *Des Cartes* has, as ſome ſay, been the intruder, I think he deſerves our thanks for it. For, as *Plato* ſaid, God does always geometrize; ſo in many caſes it may be as truly ſaid, that nature does play the mechanician, not only in animals, but in plants and their parts, and divers other bodies; in the explication of which, curious, and oftentimes inviſible contrivances of her's, pictures, that repreſent them well to the eye, and, if it were needful, in dimenſions much greater than natural, may very much further the framing of right ideas of them in the mind.

THAT THE

GOODS of MANKIND

May be much increaſed by the

NATURALIST'S INSIGHT

INTO

TRADES.

TO make out what is propoſed in the title of this diſcourſe, I ſhall endeavour to ſhew two things. The one, that an inſight into trades may improve the naturaliſt's knowledge. And the other, that the naturaliſt, as well by the ſkill thus obtained, as by the other parts of his knowledge, may be enabled to improve trades.

SECTION I.

AND firſt, it ſeems to me to be none of the leaſt prejudices, that either the haughtineſs and negligence, which moſt men naturally are prone to, or, that wherewith they have been infected by the ſuperciliouſneſs and lazineſs, too frequent in ſchools, have done to the progreſs of natural philoſophy, and the true intereſt of mankind, that learned and ingenious men have been kept ſuch ſtrangers to the ſhops and practices of tradeſmen. For there are divers conſiderations that perſuade me, that an inſpection into theſe may not a little conduce, both to the increaſe of the naturaliſt's knowledge, and to the melioration of thoſe mechanical arts.

I. AND I conſider, in the firſt place, that the phænomena afforded by trades, are (moſt of them) a part of the hiſtory of nature, and therefore may both challenge the naturaliſt's curioſity, and add to his knowledge. Nor will it ſuffice to juſtify learned men in the neglect and contempt of this part of natural hiſtory, that the men, from whom it muſt be learned, are illiterate mechanicks, and the things that are exhibited are works of art, and not of nature. For the firſt part of the apology is indeed childiſh, and too unworthy of a philoſopher, to be worthy of a ſolemn anſwer. And as for the latter part, I deſire that you would conſider, what we elſewhere expreſly diſcourſe againſt, the unreaſonable difference that the generality of learned men have ſeemed to fancy betwixt

betwixt all natural things and factitious ones. For besides, that many of those productions that are called artificial, do differ from those that are confessedly natural, not in essence, but in efficients; there are very many things made by tradesmen, wherein nature appears manifestly to do the main parts of the work: as in malting, brewing, baking, making of raisins, currans, and other dried fruits; as also hydromel, vinegar, lime, &c. and the tradesman does but bring visible bodies together after a gross manner, and then leaves them to act one upon another, according to their respective natures; as in making of green or coarse glass, the artificer puts together sand and ashes, and the colliquation and union is performed by the action of the fire upon each body, and by as natural a way, as the same fire, when it resolves wood into ashes, and smoak unites volatile salt, oil, earth, and phlegm into soot; and scarce any man will think, that when a pear is grafted upon a white thorn, the fruit it bears is not a natural one, though it be produced by a coalition of two bodies of distant natures put together by the industry of man, and would not have been produced without the manual and artificial operation of the gardener.

II. But many of the phænomena of trades are not only parts of the history of nature, but some of them may be reckoned among its more noble and useful parts. For they shew us nature in motion, and that too, when she is (as it were) put out of her course, by the strength or skill of man, which I have formerly noted to be the most instructive condition, wherein we can behold her. And as it is manifest that these observations tend directly to practice, so, if I mistake not, they may afford a great deal of light to divers theories, especially by affording instances, wherein we see by what means things may be effected by art, and consequently by nature, that work mechanically.

III. The phænomena afforded by trades are therefore the fitter to be translated into the history of nature by philosophers, because they, whose profession it is to manage those things, being generally but shopkeepers, and their servants being for the most part but apprentices and boys, they neither of them know themselves how to describe in writing their own practices, and record the accidents they meet with; so that either learned men must observe and register these things, or we must, to the no small prejudice of philosophy, suffer the history of nature to want so considerable an accession, as the shops and workhouses of craftsmen might afford it; which accession would be much the more copious, if the experiment of trades were made by a naturalist, who would doubtless so manage them, as to make them far more instructive, and better fitted for the design of a natural history, than the same experiment would be, if they were related but by an illiterate tradesman, though never so honest.

And, *Pyrophilus*, to invite you, as you design a further progress in natural philosophy, to disdain as little as I do, to converse with tradesmen in their workhouses and shops; give me leave to tell you, that as he deserves not the knowledge of nature, that scorns to converse even with mean persons, that have the opportunity to be very conversant with her; so oftentimes from those that have neither fine language nor fine cloaths to amuse him with, the naturalist may obtain informations, that may be very useful to his design, and that upon several scores.

For first, tradesmen are usually more diligent about the particular things they handle, than other experimenters are wont to be; because these, if they want diligence, lose nothing but what that very want of it keeps them from taking notice of, or at most, the satisfaction of an unnecessary curiosity; whereas tradesmen have anotherguise concern in the management of what they employ themselves about, for their livelihood depends upon it. And as, if they be careless, others more diligent will get away their custom; so, if they do any thing extraordinary well, the chiefest, and, for some time, the whole benefit will accrue to themfelves, and by improving their profeffion they better their income.

Secondly, As it is proverbially faid, that neceffity is the mother of inventions, fo experience daily fhews, that the want of fubfiftence, or of tools and accommodations, makes craftfmen very induftrious and inventive, and puts them upon employing fuch things to ferve their prefent turns, as nothing but neceffity would have made even a knowing man to have thought on. By which means they difcover new ufes and applications of things, and confequently new attributes of them; which are not wont to be taken notice of by others, and fome of which, I confefs, I have not looked upon without wonder.

Thirdly, I have feveral times obferved trades deal with things unknown to claffical writers, and unufed, fave in their fhops. And thefe are not only factitious, but divers of them natural; as manganefe (by fome called magnefca); and zafora (if at leaft it be what many repute it) emery, tripoli, &c. and of both forts there are fome that are exceeding ufeful; as of thofe formerly mentioned, the two firft are to glafs-men and potters; and the two latter to a number of other tradefmen; and as among artificial concretes, foaders are of neceffary ufe to gold-fmiths, lock-fmiths, copper-fmiths, brafiers, pewterers, tin-men, glafiers, &c. amels to gold-fmiths, glafs-men, &c. lakes of feveral forts to painters, heralds, &c. and putty to amel-founders, potters, ftone-cutters, gold-fmiths, glafs-grinders, and divers other profeffions. I fhall add, that even of thofe natural things, of which fome mention is made in famous books, one may learn many things in fhops, not to be met with there, both as to the differing kinds of things, and as to the marks of their goodnefs, and as to other particulars conducive to the knowledge of thofe fubjects. And I freely confefs to you, *Pyrophilus*, that I learned more of the kinds, diftinctions, properties, and confequently of the nature of ftones, by converfing with two or three mafons, and ftone-cutters, than ever I did from *Pliny*, or *Ariftotle* and his commentators.

Fourthly, You fhall often find, that tradefmen, being unacquainted with books, and with the theories and opinions of the fchools, examine the goodnefs and other qualities of the things they deal with, by mechanical ways, which their own fagacity or cafual experiments made them light upon. And though thefe, having little or no affinity with thofe, that a bookman would have taught them, will appear to him extravagant; yet being fuch, as, if they really ferve the craftfman's turn, muft be true and ufeful, their being extravagant will but make them the more new and inftructive, and confequently the more fit to be admitted into the hiftory of nature.

Fifthly, The obfervations that tradefmen can fupply us with, though they are not probably at any one time fo accurately made by them, as they would be by a learned man; yet that defect is recompenfed by their being more frequently repeated, and more affiduoufly made, than moft of the experiments, wherein men of letters have furnifhed natural hiftory; fo that thofe circumftances, which are not heeded by the artificer at one time, may obtrude upon his obfervation at another, and, by reiterating the fame proceffes fo often, it can fcarce be doubted but that divers phænomena will offer themfelves, even to an unattentive eye, that would not have been all of them taken notice of by a more heedful experimenter that had performed the operation but once or twice. But this will be further confirmed in the next paragraph.

Sixthly, There are tradefmen that do often obferve in the things they deal about, divers circumftances unobferved by others, both relating to the nature of the things they manage, and to the operations performable upon them.

Of the particulars, wherein the obfervations of tradefmen (for the utility of many of their practices is not queftioned) may help us to inveftigate the nature of bodies, I could name

name more than my present haste allows me to mention; and I shall, as a specimen, take a little notice, first, of some of the remarks they have to distinguish and estimate what they call the goodness and badness of the things they deal with; and then of some few of their observations that depend upon the influence that time and season have on the things they handle, and upon the artificers operations on them. For, to begin with the first, although they commonly mean by such terms (of goodness and badness) no more than the fitness or unfitness of such things to yield a good price, and in order thereunto for the purposes they are to be employed about in their particular trades; yet this fitness or unfitness is wont to consist in, or to suppose qualities that may relate to divers other things, and be applied to many other purposes. For some of the tradesmen's criteria discover to us a variety and a difference of kinds in bodies of the same denomination; as from the potters, the tobacco-pipe-makers, and the glass-men, we may learn a considerable variety of clays; and from stone-cutters and masons no less variety of stones untaken notice of by classick authors. So from carpenters, joiners, and turners we may learn, that some woods, as oak, are fit to endure both wet and dry weather; others will endure well within doors, but not exposed to the weather; others will hold out well above ground, but not under water; and others on the contrary will last better under water, than in the air.

And as the distinguishing marks we were speaking of may inform us of the differences and kinds of bodies; so they may likewise on other accounts give us notice of divers of their qualities. Thus we find by the glass-men and soap-boilers, that some ashes, as those of kaly, bean-stalks, &c. do much more abound in salt, than other some; and yet some of those sorts of ashes make clearer, or otherwise better glass, than the rest do. We may likewise learn of the malsters the differing impressions that the barley receives according to the fewel, whether straw, wood, furze, &c. that makes the fire wherewith it is dried. And I remember, I have known an ingenious malster much advantaged by a way he had of so preparing malt, as if it had not been dried with wood (usually the cheapest, but not the best fewel for that purpose) whereas indeed it was a secret consisting only in the choice and seasoning of such a kind of wood, that even the solid parts of it cleft burnt almost like straw with a clear flame, so strangely free from smoke, that I could not behold it without some wonder.

The other sort of instructive observations to be learned of tradesmen consists of those that are made about the operation, that continuance of time, or change of season and weather may have upon certain bodies, and ways of handling them. For naturalists, usually contenting themselves to make their experiments but once or twice, when their leisure best serves, or their occasions most require, have not the same opportunity to discern what influence the temper, which the air then is put into, either by the season or the weather, or both, may have on the event of the trial; whereas tradesmen, by long and sometimes unwelcome experience, are taught such and such things will be best done at such seasons of the year, or in such kind of weather; which if they be not in some cases observed, either the thing will not succeed, or the tradesman will be damnified by his trial.

Thus we see, that tanners make choice of that part of the spring, when the bark abounds with the rising sap, to take it off from the trees; because at all seasons it will not be so good nor come off so easily. Thus joiners think not wainscot sufficiently seasoned, till it be so many years old. And in several countries, butchers observe, that though a young bullock may be very good meat, if spent soon after it is killed; yet if powdered, to be long kept, before the beast be four or five years old, the salt will too much fret it, and make it little worth. And I look upon it as one of the advantages

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the naturaliſt may derive from tradeſmen's obſervations, that the ſame things being ſucceſſively dealt with by the father and the ſon, the maſter and the apprentice, they ſometimes make far more long winded obſervations than the philoſopher has opportunity to do. As for inſtance, thoſe that make mortars of lignum vitæ, and will make them good, will keep it in the houſe twenty years, or perhaps more, to ſeaſon, as they call it, before they will employ it. And experienced maſons tell us, and as far as I have obſerved truly enough, that as there are ſome ſorts of lime and ſtone, that will decay in few years; ſo there are others, that will not attain their full hardneſs in thirty or forty, or a much longer time. Of which I may elſewhere give you ſome inſtances.

To the ſix foregoing particulars one more may be added to the ſame purpoſe with the reſt, and it is, that by frequenting the workhouſes and ſhops of craftſmen, a naturaliſt may often learn other things, beſides the truth and falſity of what they relate, concerning the hiſtory of the arts they make profeſſion of. For though a tradeſman, being for the moſt unlearned, and aiming only at making or performing thoſe particular things, which, when done, are to bring profit, uſually overlook thoſe phænomena that make not to his purpoſe; yet nature, (who minds as little his deſign, as he does thoſe works of her's, that conduce not to it) is by ſome agents and operations, that he employs to compaſs his ends, engaged to do ſeveral things, that have a connection with thoſe the artificer proſecutes, or elſe do depend upon them: ſo that the naturaliſt may oftentimes obſerve in ſhops divers conſiderable phænomena, that the tradeſman regards not; becauſe they neither further, nor hinder him in his work, and will be looked upon by him as impertinent to the hiſtory of his profeſſion, in caſe he ſhould be put upon delivering it. And yet ſome of theſe occurring phænomena being produced by nature, when ſhe is as it were vexed by art, and roughly handled by ways unuſual, and ſometimes extravagant enough, may diſcover to a heedful and rational man divers luciferous things not to be met with in books, or probably not ſo much as dreamed of by the authors of them. Sundry examples of this I ſhall have occaſion to diſperſe in the following Eſſay, and other tracts that are deſigned you in this ſecond volume of our preſent treatiſe.

SECTION II.

I WILL now therefore proceed to ſhew, that as the naturaliſt may, as we have ſeen, derive much knowledge from an inſpection into trades; ſo by virtue of the knowledge thus acquired, as well as by that which he has upon other accounts, he may be as able to contribute to the improvement of trades.

THIS he may do by ſeveral ways, and eſpecially by theſe three. The firſt, by encreaſing the number of trades, by the addition of new ones. The ſecond by uniting the obſervations and practices of differing trades into one body of collections. And the third, by ſuggeſting improvements in ſome kind or other of the particular trades.

THE firſt of theſe I ſhall here lightly paſs over, having elſwhere occaſion to diſcourſe of it more fully; only I ſhall here take notice, that, for the experimental philoſopher to increaſe the number of trades now in uſe among us, it will not be abſolutely neceſſary, that he ſhould invent new ones, ſince he may do it by reviving the trades formerly known to the ancients, but loſt to us; ſuch as the making incombuſtible cloth of *lapis amiantus*, the Tyrian purple, the making of Moſaick work, and thoſe many other inventions, which you may find mentioned in *Pancirollus*, and his learned commentator *Salmuth*. Of which it were not amiſs, that a catalogue were made publick; for ſuch things, having been once actually done by men, are not impoſſible to be done again; and

and therefore I see no reason to despair, that in so ingenious an age as this, some, if not most, of them may be retrieved.

The second advantage that trades may derive from an inquisitive naturalist, is, that by this means the several observations and different practices of trades, whose managers want the curiosity, the skill, or the opportunity, to make a general inspection into trades, which they would find the more difficult to do, because craftsmen will often be more shy of one another, and more backward to disclose the mysteries of their art to one that may make a gain of it, and thereby lessen theirs, than to a philosopher, that inquires to satisfy his curiosity, or enable himself to be helpful to them. And certainly, if so much as the known hints, that may be given by the experiments already dispersed among men of several professions, were known to any one man, though otherways but of common abilities; as my own experience has in some measure informed me; those united beams, which scattered are scarce considerable, would afford him light enough to better most of the particular trades, that are retainers to philosophy. And perhaps, it were not amiss, if there were some knowing and experimental persons appointed by the publick to take an exact survey of the trades in use amongst us, and inform themselves particularly of all the secrets and practices belonging to them, that thus discerning the errors and deficiencies of each, they may rectify the one, and supply the other, partly by the hints afforded by the analogous experiments of some other trades, and partly by their own notions and trials.

Thus a few of the more ingenious French gardeners have of late usefully applied to the watering of young and tender plants that way of filtration, which is used by apothecaries with moistened cotton wicks or rolls, or else with lists of either linen or woollen cloth, so ordered, that one end being immersed in the liquor to be strained, the other may hang over the brim, and out of the vessel somewhat lower than the bottom, or at least the surface, of the liquor. For if this lower end of the list be placed over the root of any seed or tender plant, it will, by constantly and leisurely dropping on it, water it much more temperately and uniformly, than can be done by common watering-pots. And even this way of irrigation may by a cheap and easy mechanical contrivance be very much improved. There is another practice among stone-cutters, that cast or mold things with plaister of Paris, to obtain finer powders than searces are wont to give them, by stirring the powder well in water, and after it has rested a little while, pouring off the upper part of the troubled liquor into another clean vessel; at the bottom of which there will in time settle an impalpable powder. I will not here tell you what use I make of this in chymistry, to obtain much finer powders than are usually to be met with of the same denomination. And I shall but intimate to you, that by letting the first water stand but so much the longer before you pour off the upper part of it, till not only the grosser and heavier, but the less fine particles be subsided, you may get a powder, yet much more subtle, than those artificers that imploy the former way, without this circumstance, are wont to obtain. This, I say, it shall suffice me to have pointed at, because it is more proper to take notice, that the way of obtaining subtle powders by the help of water is useful, not only to the above-mentioned craftsmen, but likewise to glass-men, potters, makers of telescopes, and microscopes, those that cast metals in spaud, and other tradesmen too. Besides, that I may hereafter have occasion to tell you, that it is of great use in *China* for the makers of porcelain.

But it is not only by acquainting artificers of different professions with one another's practices, that the naturalist may further trades, but by making materials employed by one sort of craftsmen serviceable to another. That philosopher, who has surveyed a great number of trades, and compared them together, may do this with advantage, you will

will eaſily grant, when I ſhall have advertiſed you, that without any ſuch aſſiſtance as that of a philoſopher, in whom their diſtinct knowledge may concenter, and who has ſkill to enlarge the applications of them, we may obſerve, that ſometimes tradeſmen themſelves can make uſe of one anothers productions. Of which I ſhall give you a couple of examples, the one furniſhed me by litharge, the other by aquafortis.

THE former of theſe, which is but lead powdered and almoſt vitrified, by being blown off (or melted into) the refiner's teſt, as it ſerves the chymiſt to take his ſugar of lead (which it has been obſerved to do better, than minium) and other ſaturnine medicines; ſo it ſerves divers comb-makers to die horns (as we have tried by the mixture of litharge, quick-lime, and ſharp vinegar. It ſerves alſo ſome painters and others to accelerate the preparations of their fat oils, as they call them. And ſome varniſhers to make their varniſhes dry quickly. It likewiſe ſerves ſome artiſts to make counterfeit gems; and we have tried, that by melting it with about a third part of pure white ſand, or calcined cryſtals, and then putting in a ſmall quantity of mineral concretes, according to the colour intended to be introduced, one may make ſapphires, emeralds, &c. coloured like the natural ones; though this way makes theſe productions too ponderous, ſoft, and dim, and is far inferior to another we may elſewhere have occaſion to diſcloſe.

OTHER mechanical uſes of litharge I omit, to come to the ſecond inſtance I was mentioning, which is taken from aqua fortis. For not only refiners uſe it to part ſilver from gold and copper (whence the French call it *Eau de depart*) but divers makers of curious wooden works uſe it for the diſcolouring and ſtaining of their woods. Dyers make great uſe of it about their colours, and even about ſcarlet itſelf. Other artificers employ it to colour bone or ivory, ſteeped for a convenient ſpace of time therein, having firſt made it of the colour they deſire, by diſſolving in it copper (inſtead of which I have ſometimes uſed verdigris) or other bodies, fit for their preſent turn; and ſome too by diſſolving in it the fourth part of its weight of ſal armoniac, turn it into aqua regia, and in that make a ſolution of gold, wherewith may be ſtained (as we have tried and taught ſome artificers) the ivory haſts of knives, and boxes of the ſame matter, with a fine kind of purple colour, which yet will not ſuddenly diſcloſe itſelf on them. Some book-binders alſo employ aſperſions of aqua fortis to ſtain the leather, that makes thoſe fine covers of books that, for their reſemblance to ſpeckled marble, are wont to be called marbled. It is alſo employed (as themſelves have acknowledged to me) by ſome of the diamond cutters, to free the duſt of diamonds from metalline powders, as I ſhall hereafter declare. It is likewiſe of great (and as they imagine of neceſſary) uſe to thoſe that etch plates of copper or braſs. To which may be added, that we have cauſed canes to be ſtained into the likeneſs almoſt of tortoiſeſhell by a mixture of aqua fortis, not too well rectified, which is unexpedient in this work, and oil of vitriol laid on at ſeveral times and places, upon canes held over a large chafing-diſh of coals, that by the heat the ſtaining liquor may be the better ſucked in by the canes, which muſt afterwards have a gloſs given them, by being diligently rubbed with a little ſoft wax and a dry cloth. Nor are theſe all the uſes made of aqua fortis, as you will find hereafter by inſtances, that I reſerve for other places. But I thought fit to mention this liquor in this place, rather than any of thoſe many factitious bodies I might have taken notice of, for theſe two particular reaſons. The one, that the uſes hitherto enumerated of this menſtruum, may ſerve to confirm what I told you in the ſecond eſſay, of the great utility of menſtruums; and the other, that though aqua fortis be a liquor of exceeding common uſe, and wont to be diſtilled by men of ſeveral profeſſions, as chymiſts, refiners, gold-ſmiths, &c. yet they have had hitherto ſo little curioſity to enquire into the nature of it, or vary the ways of making it, that not only the ways, that a skilful naturalist might direct for improving it, have not been taken notice of, but no small oversights may be observed to be generally and daily made about it. And an ingenious gentleman of my acquaintance, by making some trials to improve it, has been so far successful in his attempts, that he makes it by great odds better than that which the refiners are wont to employ, or, as far as my trials have informed me, than any I have used; and affords it for not much above half the price that is commonly given for it. Nor have his experiments this way alone promoted the refiner's trade, but have also disclosed to him a way of clearly recovering most of his aqua fortis, after he has used it in the separation of metals, not only in its former strength, but somewhat encreased in virtue; which you will the more easily think possible, if I tell you, that aqua fortis may be made and received in other vessels, than those that are usual. As also, that without dreaming of this chymist's way, I have re-obtained that menstruum exceeding strong, after having employed it upon certain minerals, for from others I know not whether it may be so regained. And lastly, that there are some bodies, besides glass and earth, that are not brittle like these, and yet serve for the second distillation of aqua fortis, though made very strong at the first.

And since I am mentioning of this liquor, I shall intimate (and only intimate here) that, by adding to saltpetre, instead of the usual additament of three times its weight of brick, or clay, or the like, about an eighth or tenth part only of its weight of another substance, we have, even in ordinary sand furnaces, obtained, though slowly, a nitrous spirit, or aqua fortis much stronger at the first distillation, than that which is wont to be sold by our refiners, for double or rectified aqua fortis.

You, *Pyrophilus*, and divers other virtuosi, have much more opportunity to make an inspection into particular trades, than my other studies and occasions will allow me, and yet I have been more than once able to suggest to eminent artificers such things, concerning their own profession, as they tried and thanked me for. And therefore I have often wished, that some ingenious friends to experimental philosophy would take the pains to enquire into the mysteries, and other practices of trades, and give us an account, some of one trade, and some of another, though the more are handled by the same person it will be, *cæteris paribus*, the better, not only delivering historically what is practised, but also adding their own reflections, and any other thing they think fit to propose, towards the melioration of the professions they write of.

And to give you, for a specimen of this (not perhaps the best that I could, but) such an one, as will be sure not to make you despair of out-doing it, I will add at the close of this essay, what came into my mind, and cost me about an hour to set down, about the trade of those that sell varnished wares.

Some Italian writers (who indeed are to be commended for it) have given us accounts of some particular professions, as besides others, that I have heard of, but could not procure: *Antonio Neri* has written *Dell' Arte Vetraria*, and *Benvenuto Cellini* of sculpture, and the statuary's art, and of some other professions, worthy, with the art of glass-making, to be made English.

And indeed, I would willingly invite both you and other virtuosi of our own country, as well as of others, not to disdain to contribute their observations to the history of trades. And if you pitch upon any, you may command my thoughts of the method, wherein an account of it may be the most conveniently given. For I look upon a good history of trades, as one of the best means to give experimental learning both growth and fertility, and like to prove to natural philosophy what a rich compost is to trees, which it mightily helps, both to grow fair and strong, and to bear much fruit.

 And

And this I was so persuaded of, that I once designed, if the publick calamities of my country had not hindered, to bind several ingenious lads apprentices to several trades, that I might the better, by their means, both have such observations made as I should direct, and receive the better historical accounts of their professions, when they should be masters of them.

III. But it is not only by making the practices and productions of some trades serviceable to others, that the experimental philosophy may be a benefactor to those professions. For he may do it by the third of the formerly mentioned ways (which in some cases is coincident with the second) namely, first by surveying the rules and observations already received, and the practices already in use of each particular trade he would improve, and then by taking notice of two things concerning it, viz. the deficiences and inconveniencies, that blemish it, and the optatives that may be made about it; that he may also in the last place propose rational (if not certain) methods or expedients to supply or remedy the first; and either accomplish the second, or make approximations to it, as far as it is feasible, or as his skill reaches.

By deficiencies and inconveniencies I do not here mean those things, which are wanting to the absolute perfection, which a philosopher might wish to find in the trade he considers; (for these belong to the optatives) but those, which are wont to be complained of, and not irremediable, or that are wanting to a more easily obtainable degree of perfection. I shall not pretend to enumerate these in particular trades, but only observe in general, that the chiefest of them seem to be such as these.

First, that the artificer may be too much confined to certain materials, some of which may be scarce, or dear, or ill-conditioned, in comparison of others, that the naturalist might propose. As I remember, that being in a place, where we could not procure good vitriol to make aqua fortis with, after the manner of our English refiners, by a substitution of burnt alum for vitriol, but in a far less proportion, we made solvents for silver, as good as theirs, if not much better.

And especially in such cases as these it is, that the naturalist may be very much assistant to tradesmen. For there are many things, which he who is acquainted with variety of bodies, and the accounts on which they work on one another, will either quickly discern to be performable by other materials, than those that tradesmen confine themselves to, or probably guessed to be performable by other agents more in the tradesmens power; and by making trials of his conjectures, it is like he will within a few trials discover what he seeks. I know an ingenious person, that upon the general complaint made by tanners, of the scarcity and dearness of the bark of oak, found a way to prepare leather without that or any other bark, as well, if not much better, than it is wont to be done the ordinary way; at least, as far as I, and divers others more skilful than I, could guess by some variety of it, which he shewed me. And this variety of materials, which may be suggested by the naturalist, is therefore the more considerable, because, that though the suggested materials be dearer than that in common use, yet it may be so much better conditioned in other regards, as to be preferable to it. And though diamond dust be very many times dearer than the powder of emery, yet I sometimes cause work to be done for me in a shop, where, to cut some gems, and even loadstones themselves, the craftsmen I made use of did, by my encouragement, employ the precious powder of diamonds, instead of that of emery, because the former makes so great a dispatch, and obliges them so much the seldomer to change their tools they apply it with, as makes an advantageous amends for the dearness. And so, though common spelter-soder be much cheaper, than that which is made with silver instead of spelter, yet in divers cases, this last is preferable, even by artificers themselves. For trials informs us, that

that this will run with so moderate a heat, as often needs not endanger the melting of thin and delicate pieces of work, that are to be sodered; and if this silver-soder be so well made, as some I can shew, you may with it, soder even upon soder itself, made the ordinary way with brass and spelter, and so fill up those little holes or crannies that may have been left or made in the first sodering, and are not safely to be mended, but by a soder more easily fusible than the first.

Secondly, that the tradesman may be confined to certain ways of working, when perhaps it would be much more advantageous to him, if he had others proposed him by the experimental philosopher, who may perhaps discern, that what is mechanically done by the artificer, may be better done physically; and on the contrary. Whereas goldsmiths, first directed probably by some chymist, by boiling silver-spurs, hilts, &c. of curious workmanship in salt, alum, and argol, give it that whiteness and clearness, which it would scarcely be securely brought to by brushing, or pumice stone, or putty. And the like clearness, experience has informed us, that old sullied pieces of good gold may be brought to in a trice, by the help of warm aqua fortis. And as there are divers other things (some of which you will find mentioned in a following essay) that, though wont to be done mechanically, may be done better by physical means; so of those things that ought to be done mechanically, many things, that are wont to be done by the labour of the hand, may with far more ease and expedition (the quantity considered) be performed by engines; by which, if they be skilfully devised, our observations make us bold to think, that many more of those, that are wont to require a laborious or skilful application of the hands, may be effected, than either shop-men or book-men seem to have imagined. For not to mention those several instruments, on which I have *extempore* played divers tunes that I had never learned, when we see, that timber is sawed by wind-mills, and files cut by slight instruments, and even silk-stockings woven by an engine, besides divers other artificial inventions left not named, because they cannot intelligibly be so in few words, we may be tempted to ask, what handy work it is, that mechanical contrivances may not enable men to perform by engines?

Thirdly, there may be deficiencies also in this, that what the artificer undertakes is either long in doing (as in the ordinary way of tanning, brick-making, seasoning of wood, &c) or takes up more pains, or requires a greater apparatus of instruments, or else in some other way more chargeable, or troublesome, or laborious to be effected, than it needs be. And these kinds of deficiencies may in very many cases be supplied by the experimental philosopher. As I know an inquisitive person, that has upon a solemn trial, tanned as well as the masters of the profession, in far less time, (and if I much forget not, in less by above half) than they; so in some places they have a quick way of seasoning some kinds of wood, for the use of sea-timber, by baking it in ovens, (which way I have also known used here in *England*, to season some sorts of wood for other uses in a few hours;) so, whereas our grinders of dioptrical glasses have hitherto believed, that they must make use of Venice glass, which is very dear, and oftentimes very scarce to be come by, some virtuosi, considering that the great clearness of an object-glass is rather an inconvenience, than a very desirable qualification, have newly taught some of the artificers to employ that coarser and cheaper sort of glass, they call green-grass, which is made here in *England*, instead of the other, which now begins to be thought by the skilful (with whom my observations disagree not) to be inferior to it. And several dyers employ our woad, which is not far fetched and much cheaper, instead of the eastern indigo, for dying of some (if not all) sorts of blues, and those other colours, which that grand tincture prepares the cloth to receive.

Fourthly, another sort of deficiencies or inconveniencies may be the want of durableness, either as to the very being of the thing produced by the artificer, or as to the beauty or the goodness of it.

Of the former sort may be (not to mention the decay and souring of cyder, perry, &c.) the cracking of glass of its own accord, and particularly that which is complained of by divers who deal in telescopes, that the object-glasses, which are wont to be made, as I was saying, of fine Venice glass, will sometimes, especially in water, flaw of themselves, and so grow useless, to prevent which, some, that are very curious, carry them in their pockets.

Of the latter sort is the fading of the bow-dye of water colours in limning, and the rust of shining arms, and other polished steel. Divers of these inconveniencies also the naturalist may obviate or remedy; as some of the virtuosi above-mentioned, by teaching the glass-grinders to make the object-glasses of their telescopes of green glass, have taught them a way to make them durable in spight of the vicissitudes of weather. And I have had pieces of artificial crystal, whereof some, though in no long time, cracked in so many places, that they changed their transparency for whiteness; yet another, though much larger, did, as I conjectured it would, hold sound during some winters, nor was ever broken but by accident: and I remember, I told the artificer, in whose furnace the crystal, that lasted not, had been made, that I took, as I do still, the reason of the difference to be, that the durable crystal had but a due, and the other an over-great proportion of fixed salt. The reasons of which conjecture I shall have occasion to give you in another place.

And, as to the scarlet dye (whereof I lately made mention) that it may be much advanced, as to point of fixedness and lastingness, beyond the common bow-dye, I was persuaded by an honest merchant of *Amsterdam*, who had got a great estate by colouring of cloth, and was particularly curious about the scarlet dye: for he presented me with a piece of scarlet (of which he said he could make enough at a reasonable rate, wherein he almost defied me to find either any part undyed, or to stain it with vinegar, lixivium, and other liquors that he named; and indeed by cutting it I found, that though it were a thick piece of cloth, the middle of it was not (as is usual in scarlets) white or pale, but it was dyed quite thorough; and though of scarlet I shall elsewhere have occasion to speak farther, yet I the rather mention it in this place, because it affords me a notable instance, that trades may be considerably improved by those that do not profess them. For the most famous *Cornelius Drebel*, who was the inventor of the true scarlet dye, was a mechanician, and a chymist, not a dyer; and as an ingenious man (that married his daughter) related to me, was so far from having been versed in that profession, when some merchants put him upon the advancement of a certain way of dying a fine red, or rather crimson, that had been a while before casually lighted on in *Holland*, and proved very gainful to the finders, that he did not know so much as the common way of dying the ordinary reds, though the merchants having once taught him, that, by the help of a sagacious conjecture (to be told you in one of the following essays) he soon invented the true scarlet dye, which has since been so much esteemed.

It now remains, that I mention in a few words the optatives, that may be proposed by the naturalist about the particular trades he would improve. By which name of optatives I mean all those perfections, that being desirable, are rather very difficult, than absolutely impossible, to be obtained. Of which optatives there may sometimes belong several to one craft or profession.

Of this sort, in the blacksmith's profession, may be the making iron to be fusible, with a gentle heat (as the flame of a candle) and yet hard enough for many ordinary uses,

uſes. In the glaſs-men's trade and the looking-glaſs-makers, may be the making of glaſs malleable or flexible. In the clock-maker's trade, the making the newly deviſed pendulum clocks, uſeful in coaches, boats, ſhips, and in other caſes where they are put into irregular motions.

In the braſier and copperſmith's trade, the making of malleable ſoder. In the ſhipwright's art, the making of boats and other veſſels to go under water. In the diver's profeſſion, ſome ſmall and manageable inſtruments, to procure conſtantly, at the bottom of the ſea, freſh air not only for reſpiration, as long as one pleaſes, but alſo for the burning of lights.

In the aſſay-maſter's trade, the quick melting down of ores, and cupelling of them, or at leaſt of metals, in a trice, without bellows or furnace.

In the carver's and joiner's trades, the way of giving a ſhape to wood in molds, as we do to plaiſter of Paris and burnt alabaſter.

I know, *Pyrophilus*, that ſuch optatives may be thought but a civil name for chimerical projects; but I ſhall hereafter more fully declare to you, why I think it not altogether unuſeful, that ſuch optatives ſhould be propoſed, provided, as I hinted above, that they be very difficult, and not impoſſible; that is, that they be ſuch as are not repugnant to the nature of the things, nor the general principles of reaſon and philoſophy, and ſeem no otherwiſe to be chymically or mechanically impoſſible, than becauſe we want tools, or other inſtruments, and ways to perform ſome things neceſſary to the compaſſing of the propoſed end, or to remove ſome difficulties, or remedy ſome inconveniencies, that are incident to us in the proſecution of ſuch difficult deſigns.

And let me here tell you, *Pyrophilus*, that this advantage may be derived from the deviſing of ſuch optatives to bold and ſagacious men, and if they deſpair of attaining to the perfection they are invited to aim at, they may at leaſt endeavour to reach ſome approximation to it. Thus unſuſpected eye-witneſſes have informed us, that in ſome countries they are wont to ſhoe horſes without the help of a forge, bringing their iron to ſuch a temper, that, having a company of ſhoes ready made, they can eaſily hammer them cold, ſo as to fit them to the ſize of any horſe's foot, which the heat of the climate, where this is uſed, makes the greater conveniency. Nor do I much doubt, but, that by various tempers, iron may be made very ſoft and afterward hardened; and the rather becauſe, as I elſewhere tell you, we have, without antimony or ſulphur, melted it in a crucible, ſo as to pour it out like lead, and yet afterwards it grew harder than it was at firſt. So, that flexible looking-glaſſes may be made with the help of ſelenitis, you will elſewhere be ſhewn; as alſo to foliate with eaſe all kinds of hollow glaſſes, and ſo turn them into ſpecula. That malleable ſoder may be made, though we have not yet performed it, we do not much deſpair; and by good ſilver-ſoder ſome approximation to it has been already made.

Submarine navigation, at leaſt for a ſhort ſpace, has been ſucceſsfully attempted by the excellent *Cornelius Drebel*, as *Merſennus* aſſures us; and as I have been informed, both by *Drebel*'s ſon-in-law, and by other judicious perſons, that have had the account of trials from the very men that went in the veſſel under water for a good while together; who affirmed, that though there were many in the boat, yet they breathed very freely, and complained not of any inconvenience for want of freſh air. And here alſo give me leave to take notice, that this inventive *Drebel* was no profeſſed ſhipwright, nor ſo much as bred a ſeaman.

As for the optative propoſed for the divers, I know one of them, who by a ſlight inſtrument, that is all under water, and has not, as others, any chimney open to the air

above

above the furface of the water, has been able to ftay divers hours at the bottom of the fea, and remove his refpiratory engine (if I may fo call it) with him; and *Merfennus* affures us, that a much better way, and in my opinion an admirable one, (if the thing be certain) was found out and practifed in his country, by one *Barieus*, who was able to ftay fix hours under water, by the help of an almoft incredibly fcant proportion of air, and even to preferve, at the bottom of the fea, the flame of a lamp or candle, in a veffel not much bigger than an ordinary lanthorn.

As to the optative propofed in the affay-mafter's trade, I fhall in the next effay teach you a way of cupelling in fmall quantities, without a furnace, or coals, or ordinary cupel, or other veffel.

And I remember, that by way of approximation, I made a certain powder, with which, without a furnace, I have, in a trice, melted lead-ore (which very often holds filver) into metal, and perhaps confumed fome of the bafer metal too.

And laftly, as for the making of emboffed works of wood in molds, I am credibly informed by a learned man, that it was actually performed lately at the *Hague* by the fecretary of a foreign ambaffador; but of the way I could not procure the leaft hint, though fuppofing the truth of the relation, I fufpect it was done either by fome menftruum, that much foftened the wood, which may afterwards be eafily hardened again, by which way tortoife-fhell may be molded; or elfe, by reducing the wood into powder, and afterwards uniting the parts into one body with fome very binding and thin kind of glue, whofe fuperfluous parts may afterwards be preffed out. And I remember, I began (but was accidentally hindered to proceed) a trial to make an approximation to this by the help of a rare glue, of which I had the hint, without being much beholding to him for it, from the practice of an ingenious tradefman, which, as I now prepare it, is made by foaking the fineft ichthyo-colla (i. e. ifinglafs) for twenty-four, or at leaft for twelve hours, in fpirit of wine (or even common brandy, for the menftruum need not be very good, unlefs for fome particular ufes). When by this infufion the liquor has opened and foftened the body (which will much fwell) both the ingredients are very gently to be boiled together (and kept ftirring, that the ichthyo-colla burn not, till all be reduced to a liquor, fave perhaps fome ftrings, that are not perchance very diffoluble) when it is boiled enough, a drop, fuffered to cool, will foon turn to a very firm jelly, and whilft it is hot, it fhould be ftrained thorough a piece of clean linen into a glafs or other veffel that may be kept well ftopped; a gentle heat fuffices to melt this glue into a tranfparent liquor with little or no colour, and yet this fine thin glue holds fo ftrongly, and binds fo very faft, that having fometimes taken two ordinary fquare trenchers (for the round ones are wont to be too thick) and laid the one a pretty way over the other, a little of this liquor put between them, and fuffered to dry of itfelf, united the trenchers fo faft, that when force was employed to break them, it did it elfewhere, not where they were joined together: fo that it feems, the gluten that faftened the trenchers together, was ftronger than that which joined the parts of the fame trenchers to one another. The other ufes of this jelly (which by reafon of the fpirit of wine will not eafily corrupt like other jellies) belong not to this place. Only I fhall add to our prefent purpofe, that having taken fome common faw-duft, and after having imbibed it with melted glue, ftrained out flightly what was fuperfluous through a piece of linen, and fhaped the reft with my hand into a ball, this negligent trial (which was only made to fee whether a more accurate might be hopeful) made the ball, after it had been leifurely dried, fo hard, that being thrown feveral times againft the floor, it rebounded up without breaking; but as I was faying, an accident hindered me from profecuting the experiment, which therefore I recommend to you.

I will

I will not now ſtay to tell you, *Pyrophilus*, how it may aſſiſt you toward the making ſuch approximations, as we have been ſpeaking of a little above, to take each of the difficulties, you would ſurmount, into the ſeveral parts it may be conceived to conſiſt of, and make an enumeration of the poſſible ways of maſtering each of theſe, according to ſome methods that might be propoſed; becauſe to diſcourſe of this ſubject would take up too much of the time allotted to the following eſſays, and therefore I ſhall conclude this, by obſerving to you, that as you are, I hope, ſatisfied, that experimental philoſophy may not only itſelf be advanced by an inſpection into trades, but may advance them too; ſo the happy influence it may have on them is none of the leaſt ways, by which the naturaliſt may make it uſeful to promote the empire of man. For that the due management of divers trades is manifeſtly of concern to the publick, may appear by thoſe many of our Engliſh ſtatute-laws yet in force, for the regulating of the trades of tanners, brick-burners, and divers other mechanical profeſſions, in which the law-givers have not ſcorned to deſcend to ſet down very particular rules and inſtructions.

APPENDIX.

I. Having in the foregoing eſſay mentioned a way of making ſpherical, and other hollow looking-glaſſes, with an intimation that it ſhould not be a ſecret to you, I ſhall no longer delay to acquaint you with it, partly, becauſe, though it may ſeem but a curioſity, yet it may not prove uſeleſs to you in making, very eaſily, divers catoptrical experiments, that are otherwiſe difficult enough; and partly, becauſe trial hath informed me, that ſome ways preſcribed, of thus foliating glaſſes, were much inferior to what was pretended. And even in a recent and famous writer, I lately found a proceſs of performing this, which when I had read over, I foretold it would not ſucceed, which prediction was ſoon verified by experience; and indeed they that know the way and difficulty of foliating much more tractable glaſſes, than hollow ones, will ſcarce wonder, that it ſhould not be found a very eaſy matter to foil, eſpecially without heat, ſpherical, cylindrical, and other concave glaſſes on the inſide, to which the figure of the glaſs prohibits ordinary foils to be faſtened: yet a mixture, that by the ſucceſs appeared to be fit enough for ſuch a purpoſe, I chanced to ſee employed by an illiterate wandering fellow I met with in the country, the conſideration of whoſe practice did, I confeſs, ſuggeſt to me another mixture, that I afterwards ſeveral times tried, and found it to foliate not only ſpherical glaſſes (to which he confined himſelf) but other concave glaſſes, at leaſt as well as his, if not better, which he held for a great ſecret, and which indeed excelled any I have met with in print. To give you then the way I have practiſed myſelf, take tin and lead, of each one part (by weight) melt them together, and forthwith add of a good tinned-glaſs (or biſmuth) two parts, carefully ſkim off the droſs, and afterwards, taking the crucible off the fire, before the mixture grow cold, put to it ten parts of clean quickſilver, and having ſtirred all well together, keep this foliating liquor in a clean new glaſs for uſe. When you would employ it, ſtrain it through a clean linen cloth, to ſever it from droſs, and then by the hole of the ſpherical or cylindrical glaſs, put in a long and narrow funnel of paper, reaching almoſt to the bottom of the glaſs, that the falling liquor may not ſputter to the ſides. By this funnel you muſt ſoftly pour in ſome ounces of the mixture, and then

then dexterously and leisurely inclining the glass every way, endeavour to make it fasten on all the inward cavity thereof: this being done for the first time, and the vessel being laid aside for some hours, that the foil may the better stick to it, it is best to take it in hand again, and after the former manner frequently, but slowly, pass the liquor over those parts of the glass, which by holding it against the light you shall discern not to have been sufficiently foliated the first time; afterwards the glass being again laid aside for some hours more, the former operation is to be reiterated once (or if it be needful twice) more, until you find the glass equally and sufficiently foiled, which, when you perceive it is, you may gently pour out the superfluous liquor, to be reserved for the same use in other glasses. Lastly, with a cloth well sprinkled with putty or scraped tripoli, or for need, powdered chalk, the outside of the glass must be carefully rubbed, to take off the foulness it may have contracted by being handled, and to make it look clean and polished.

This way I have made use of in glasses of several sizes and figures, and preferred before that, which I remember, I once saw tried, and was ascribed to a learned Italian, one *Caneparius*, as being more easy than it, and more safe in regard ours need no arsenick. I found it also much better than another, which is kept as a secret and highly esteemed, because though the ingredients, abating the tin, be the same in both, yet in the way already delivered, the liquor or amalgam being used cold, there is no danger of breaking the glass to be poleated, or mistaking the degree of heat to be given it, to both which inconveniencies trial taught me, that the other way is obnoxious.

II. And on this occasion it will not be amiss to acquaint you, that I made this improvement of our way; that having made the outside of glasses so foliated very clean, I have (by laying on very thinly such a kind of varnish, as that yellow one elsewhere described, as fit to make gilt-leathern hangings) made them appear richly gilt, and yet so bright and polished, that they would, notwithstanding this gilding, serve very well for looking-glasses.

III. What other improvements I made of this experiment, I must not here insist on, especially that I may comply with the haste, which obliges me to omit what I had thoughts of annexing here about varnishes; so that though I have made many trials (whereof another time you may command an account) about several sorts of them, some, that emulate gilding upon metals as well as leather, others, that imitate and diversify (if not also excel) the China varnish, and others designed for differing purposes, yet I can at present only tell you in general, that they are an useful, as well as ornamental, sort of productions, and capable (if I mistake not) of much improvement.

OF

OF DOING BY

PHYSICAL KNOWLEDGE

What is wont to require

MANUAL SKILL.

OR,

That the Knowledge of Peculiar Qualities, or Uſes of Phyſical Things, may enable a Man to perform thoſe Things Phyſically, that ſeem to require Tools and Dexterity of Hand, proper to Artificers.

THE particulars to be mentioned in this eighth eſſay might have been ranged partly under the preceding diſcourſe, and partly under the eleventh eſſay, (which will be the laſt of this treatiſe,) whoſe titles are comprehenſive enough to take in the inſtances, that make up this preſent diſcourſe; which yet I have rather choſe to deliver apart, not only becauſe they ſeem ſomewhat differing from the examples alledged in the two mentioned eſſays, but chiefly becauſe the uſes, that may be made of ſuch inſtances, may make them deſerve a diſtinct and peculiar mention. For it is both a notable argument of the induſtry of mankind, and may prove a great encouragement to it, that the help of philoſophy may ſupply the office of manual dexterity, ſtrength, or art; and a knowing head may do what is thought not performable but by a ſkilful hand, or an arm aſſiſted by ſome inſtrument or engine. And of theſe inſtances (which may be juſtly looked upon as ſo many trophies of human knowledge, and ſo many incitements to human induſtry) it will be needleſs to make any diviſion; and therefore I ſhall barely ſet them down as they come into my mind, no other order being neceſſary for particulars that are brought but as proofs, and have not a dependency upon one another.

THE aſſertion, that makes the title of this diſcourſe, the king of *Spain* finds true ſo much to his advantage, that, if I miſtake not, it amounted for a good while to divers millions

millions yearly. For whereas formerly in the silver-mines of *Potosi* in *Peru*, (accounted the richest in the world) it was wont to be a very tedious, laborious, and consequently chargeable work, to sever the silver particles of the ore from the ignobler parts of it, by many slow and costly, both manual and metalurgical fusions, and other ways of segregation, much of that labour is now saved by *Pero Fernandes de Valesco*, who, as *Acosta* informs us, first made use at *Potosi* of the property of quicksilver to amalgamate with the nobler metals. For now, by accurately grinding the powdered and searsed ore with quicksilver (strained through a cloth) and salt, and decocting them for five or six days, in pots and furnaces fitted for the purpose, the greedy mercury licks up the silver and gold (which it sometimes meets with) without meddling with the ignobler parts of the ore; and being enriched with as much of them as it can imbibe, and diligently washed from the adhering sordes, the amalgam is, by distillation with a strong fire, freed from the mercury; which coming over revived into the receiver, leaves behind it the fixed metals, viz. gold and silver, which may be afterwards (if need be) easily reduced into bodies, and parted by the common way. And by a not unlike way some of our goldsmiths and refiners are wont (as themselves inform me) to regain out of the dust and sweepings of their shops, the filings and other small particles of gold and silver which fall to the ground in their operations, and in process of time may amount to a considerable value.

To make an head, exactly representing the size, shape, and lineaments of the face of any living man, seems to require an exquisite skill in the statuary's art; and yet at my desire, and in my presence, that was lately performed by a tradesman after the following manner: the party, whose face was to be cast off, was laid flat upon his back, having round about the edges of his forehead, his cheeks, and his chin, something placed to hinder the liquid plaister from running over on his hair: then into each of his nostrils was put a hollow piece of stiff paper, of about a quarter of a foot long, and of the figure of a sugar-loaf, and open at both ends, that the affusion of the plaister might not hinder him to take breath. And of these pipes, which were carefully oiled over, the acuminated extremes rested upon his nostrils, and the other were supported by one of the assistant's hands. Then his face being lightly oiled over, to hinder the plaister from sticking to it, with oil olive, and his eyes being shut, alabaster newly calcined in a copper-kettle, till it was as white as before, was tempered up with fair water to the consistence of batter, and by spoonfuls nimbly put all over his face, till the matter lay every where near an inch thick. Almost as soon as it was all laid on, it began to grow sensibly hot, and in about a quarter of an hour hardened into a kind of lapideous concretion; which being gently and easily taken off, shewed us, in its concave surface, the exact impressions made there by the parts of the face, and even by the single hairs of the eye-brows. In this mould they cast a head of good clay, by working it in, and on that head they open the eyes, which in the prototype and mould were shut, and, if need be, heighten the forehead, and make what other amendments they think fit; and anointing this new face with oil, they after the former manner make a second mould (of two parts, contiguous all along the ridge of the nose) with calcined alabaster, and in this second mould (lightly oiled on the inside) they cast with the same matter the fore-part of an head, more like the original, than ever I saw made by the most skilful statuary, and yet with so much ease, that the very first trial I made myself to cast a face thus, succeeded.

To take the impression of a leaf, or other flattish part of a plant, it may seem requisite that a man be a good painter; and yet I found, that the thing may be performed, only by holding a whole leaf (or sprig of rosemary, &c.) in the smoke of a piece of common gum sandarack, resin, camphire, or some such body, that emits a copious and fuliginous ſteam (for which purpoſe I have made uſe of a common link, when that was moſt at hand); for the leaf being well blacked by theſe fumes, and placed betwixt the leaves of a ſheet of white paper, if you carefully preſs the paper upon the leaf with the haft of a knife or ſome other ſmooth thing, you may thereby print on the paper in a few moments the exact ſize and figure, but not colour, of both ſides, but eſpecially the back-ſide of the leaf, with the very ramifications of the fibres that are diſſeminated through it. And this may be performed, though not ſo lively, by blacking the plant, whoſe picture is required, with the fumes of a candle or taper (eſpecially if it be of wax) inſtead of thoſe of the aforementioned reſinous concretes, and afterwards proceeding as in the former experiment; which ſometimes may be of good uſe to you, when you turn botaniſt, and in your travels meet with plants, whoſe pictures you think worth having, but have not time or conveniency to draw them.

Another inſtance, of the ſame import with the foregoing ones, may be afforded us by the art of etching, whereby copper and ſilver-plates may be enriched with figures, which may ſeem to have been made by the tool of ſome excellent graver; and yet thoſe engravings do not require the preſumed manual ſkill, and are made without ſuch tools, by having a peculiar ſort of varniſh (for on the goodneſs of that depends much of the ſucceſs of the operation) on the plates, and drawing on it the figures to be engraved. For all thoſe lines, where the plate is freed from the varniſh, by ſkilfully tempered aqua-fortis (from whoſe corroſive violence the remaining varniſh ſecures the reſt of the plate) may be ſo curiouſly wrought on by thoſe few artiſts that are ſkilful in it, that I have very ſeldom ſeen lovelier cuts made by the help of the beſt tempered and beſt handled gravers, than I have ſeen made on plates etched, ſome by a French, and others by an Engliſh artificer.

But the knowledge of the phyſical properties of things may ſometimes enable a man to perform, not only things to which mechanical tools and manual dexterity ſeem to be neceſſary, but ſome things alſo, whereto even mathematical inſtruments, and ſkill in mathematicks are thought requiſite; of which I ſhall at preſent propoſe a couple of inſtances.

In the elſewhere mentioned French abridgment of *Galilæo*'s Italian book *, I find a paſſage very pertinent to our preſent deſign, which agreeing very well with our obſervation of that kind, we ſhall propoſe it a little more clearly, as follows:

Suppose in a high church (the book exemplifies *Noſtre Dame)* the great candleſtick that hangs from the top of the church being made to ſwing, a philoſopher that has obſerved that the vibrations of a pendulum, though the arches it deſcribes be unequal, are in the ſenſe formerly declared equitemporaneous; and that, when the ſtrings at which ſuch pendulums hang, are very unequal, their lengths will have the ſame proportion as is between the ſquares of the numbers of their ſingle vibrations performed in the ſame time; ſuppoſe, I ſay, that ſuch a perſon have a pendulum with him, whoſe ſtring (which may be of any length, ſo it be determinate) is, for example, a yard long, it will not be difficult for him, without any quadrant or geometrical inſtrument, to find out the length of the ſtring that ſupports the candleſtick, and conſequently the height of the church. For the candleſtick and the ſhort pendulum being made to ſwing, beginning both at the ſame time, let us ſuppoſe, that when the candleſtick has made nine vibrations, the pendulum of a yard long has made 54, the ſquares of theſe two numbers will be 81 and 2916; and becauſe, as we lately ſaid, the length of the pendulums will have the ſame proportion with the ſquares of the number of their vibrations, dividing 2916 by 81, the

* Nouvelles penſées de Galiléo liv. I.

product will be 36; which shews, that the string at which the lamp hangs is 36 times as long as that of the shorter pendulum, and consequently a yard, containing three feet, amounts to 36 yards, or 108 feet.

Upon the knowledge of another physical property of heavy bodies, I remember I have grounded a way to measure vast heights and depths without any geometrical instruments, and in such cases where such an instrument cannot be employed, by the help of a pendulum; which, because in this case it must be very short, will require an attentive and expert observer. For it being known, that a stone, or a piece of lead, or the like solid weight, falling from a height, does so accelerate its descent, that the differing spaces it has transmitted at any differing times assigned, will have betwixt the same proportion with the squares of the times, wherein the respective spaces were transmitted; if it be once known by diligent observations, how far a stone, or such a solid body (whose greater or lesser bulk is not here considerable) does fall at the end of the first second-minute of its motion downwards, it will be easy enough for a naturalist versed in the doctrine of proportions, to collect, from the time that the stone employs in descending perpendicularly from the top of a high tower or steeple, how high that building is. This way of measuring, provided attention and accuracy be not wanting, we found agreeable enough to divers observations of our own and our friends; and by this way, one may measure the depth of a well (to the surface of the water) how deep soever, though the bottom, as it is usual, by reason of the darkness, cannot be seen, which makes the depth unfit to be measured by quadrants and such like geometrical instruments. For if at the same time that you let fall a stone or other weight, you also let go a pendulum that vibrates quarter-seconds, that is, makes two excursions, and as many returns in the sixtieth part of a minute, and reckon its vibrations, till you hear the noise made by the stone dashing against the water in the bottom of the well, you may easily enough collect the depth. For let it be supposed, that it be found by experience, that a falling stone, or other like weight, do in the first second-minute of its descent dispatch (as the diligent *Mersennus* affirms himself to have often found) 12 feet (which I understand of French, not having found it hold in English measure) and let us also suppose the pendulum to have perfected six single vibrations before the dashing of the stone against the water was heard; if we proceed according to the rule formerly given, we shall find, that if the time wherein the falling stone transmitted those spaces, that are to direct our calculation, be one and six, the square of those two numbers being 1 and 36, the stone must have fallen at the end of the sixth second 36 times as far as at the end of the first. And since by observation (about whose accurateness we need not be solicitous here, where we design only the giving an explanatory example) a falling stone in the first second descends 12 feet, we need but multiply 36 by 12, to obtain in the product 432, the perpendicular depth of the well to the surface of the water. And the same number may be collected, and perhaps you will think more easy, by supposing, as *Galileo*'s experiments seem to prove, that a falling body accelerates its descent according to a progression of odd numbers, beginning from an unit; so that, if in the first second minute, or any other determinate part of time, it falls one space, whatever that be, in the next second it will fall three spaces, and in the third five spaces, and so onwards; according to which reckoning, if the falling body be supposed to descend 12 feet during the first second, it will descend 36 (besides the former 12 in the next second) in the third 60, in the fourth 84, in the fifth 108, in the sixth 132, which, summed up together, amount to 432. And, by the same way, one may measure the height of divers precipices how great soever, as far as one can reach downward in a perpendicular line. And one may also give some guess at the depth of some volcanos, which are not accessible to those that know but the common ways

ways of menfuration, or which have burned the ropes, and even melted down the chains and weights, by which fome curious perfons have attempted to fathom their depth. It is true, that in mathematical rigour, fome abatement ought to be made, becaufe the ftone ftrikes the furface of the water, or the bottom of the precipice, fome little while before the found, produced by that ftroke, can arrive at our ears. But unlefs the height or depth to be meafured be very extraordinary, this allowance, for the delay of the noife, either may be neglected without much inconvenience, or in probability will fcarce exceed a quarter (or at moft half) of a fecond; fince, as has been elfewhere noted, it has been found by obfervation, that a found in the air moves above twelve or thirteen hundred feet in one fecond. And in what I have here delivered concerning the way of meafuring depths and heights by the falling of a heavy body, I have been much confirmed by an obfervation I chanced to meet with in an outlandifh book, which I have not now by me to look out the place, where the mathematician that writes it, who feems to have been a diligent obferver, affirms, that he found a weight let fall from the top of a church, or fteeple (for I remember not which, nor is it material) fo high as to amount to 300 feet, to reach the ground in about five feconds; which agrees very well to what we have been delivering. For fuppofing the weight to fall 12 feet the firft fecond, at the end of the fifth fecond it muft have fallen 25 times as far, (1 and 25 being the fquares of the numbers of the feconds of time) and confequently 300 feet.

To flit (or divide tranfverfly into flakes or leaves) fo thin a piece of metal as an old groat, which feems not to exceed, if it fo much as equal the thicknefs of a leaf of white paper, may be thought, if it be feafible, to require fome very fubtle dividing inftrument, with an edge finer than that of a razor; and yet the way of performing this by phyfical means is but an almoft ludicrous experiment, which (if you know it not already) is eafily thus made. Take three pins, and ftick them in the form of a triangle, at fuch a diftance from each other, that the groat may reft upon the heads of them; put upon this thin piece of metal almoft as much flour of brimftone, or, at leaft, finely powdered fulphur, as will conveniently lie on it; then kindling the fulphur, let it burn out of itfelf; which done, take off the groat, and throwing it hard againft the floor, the upper part, with the adhering remains of the fulphur, will be parted from the lower; which (lower) if the coin were not very thin, will retain its former fhape. I have obferved in this experiment, a pretty circumftance or two, the knowledge of which is very apt to be mifemployed, and need not here be mentioned; though I would not filently pafs by the experiment itfelf, becaufe as ludicrous as you may think it, it may fuggeft uncommon fpeculation to a confidering naturalift, and alfo intimate a way of preparing filver, of which I may elfewhere tell you the practical ufe.

He that takes notice of fo pretty a variety of colours and fhapes as may be difcerned on a fkilfully made fheet of marble-paper, will be apt to conclude, either, that the differing colours were laid on one by one with a pencil, which would require a great deal of time and pains; or that the fheet was marbled by being printed off from fome plate, on which the differing fhapes were cut or engraven, and the differing colours fingly placed, which would require yet more labour, and a greater apparatus; whereas the whole fheet is painted thus varioufly and delightfully at once, and in a trice, by the contact of the furface of a veffel full of water, on which the colours (firft blended a little, by a quick and eafy motion of the artift's hand) are fo ordered as to fwim without being confounded. This artifice hath, as I am informed, been delivered by the curious *Kirckerus*. But if you have a mind to know the particulars of it more fully, you may command me to acquaint you with what I have learned from experience, by which the practice is fuppofed to have been of late improved.

If it were propofed to free weak fpirit of wine, or aqua vitæ, from a great part of its phlegm, the generality of diftillers would think it not to be effected but by the help of fire and a furnace, an alembick, or fome other diftillatory veffels; and yet, without the help of any of all thefe inftruments, I have fometimes taken pleafure to dephlegm brandy (as they call weak fpirits of wine of the firft diftillation) only by putting it into falt of tartar. For confidering the faculty this alkalizate body has to attract (as men commonly fpeak) or imbibe the aqueous particles that fwim in the air, and refolve itfelf, with them into that liquor that the chymifts call oil of tartar *per deliquium*, there feemed fufficient reafon to expect, that the fame falt, being put very dry into phlegmatick fpirit of wine, would embody with the phlegmatick parts, with which, if it were not overcharged, it would probably keep them feparate from the more fpirituous liquor; fince fuch oil of tartar as I have juft now mentioned, and dephlegmed fpirit of wine, will fwim upon one another without mixing; and accordingly, I have fometimes taken pleafure, by putting a fufficient proportion of dry falt of tartar into brandy, and leaving it there for fome time (for the experiment will, to be completed, require fome while) to make fome feparation of a great part of the phlegm, which by degrees diffolving the falt, will reduce again part of it into a liquor that will keep its furface diftinct from that of its fupernatant fpirit, and if confounded therewith, by the fhaking of the glafs, would fpeedily part from it, and regain its own ftation; and if you would have a feparation of the phlegm begin to appear quickly, you may compafs what you intend, by tying up a convenient quantity of dry falt of tartar in a dry rag of linen cloth, and immerfing it a little while in the brandy, and then lifting it up a little above the liquor; for the phlegmatick parts being copioufly imbibed in the falt, which will be thereby refolved into a ponderous liquor, will in drops (whofe defcent will be diftinguifhable enough, if the glafs be held againft the light) fall to the bottom of the fpirit of wine. And left you fhould fufpect, that this defcent comes not from their weight, but from the force they acquire in falling through the air, you may keep the rag immerfed beneath the furface of the liquor, and yet may perceive the efflux and fubfidence of the lixivium we have been fpeaking of.

There are fome cafes, wherein bodies that are to be held very foftly, are either fo brittle that it would be hard to hold them faft enough without danger of breaking them; or elfe fo fmall, and fo inconveniently fhaped, that it would be very difficult to procure inftruments to lay hold on them, and keep them movelefs in the inftrument; and in feveral fuch cafes, the ufe of tools, to hold faft fuch bodies, may be advantageoufly fupplied by artificial cements. As I remember I have known the glafs-grinders, inftead of more mechanical tools, employ pitch, melted and made up with afhes, very well ftirred and incorporated with it, into a ftiff pafte. For this mixture, being by a fit heat brought to a convenient foftnefs, the glafs to be ground or polifhed is bedded in it, in what pofture and as far as the artificer pleafeth; and by the fame mixture the glafs being faftened at the end of a ftick or fome proper inftrument of wood, the glafs, upon the cooling of the cement, remains firmly faftened, until the artificer have done with it what he defigned; after which, by foftening the cement with heat, he can readily take it off again.

And even the diamond-cutters, who, to grind thofe ftones into fhapes are wont to employ a very vehement attrition, make ufe, for holding their diamonds, efpecially when they would polifh them, of a cement, the like to which I remember I have fometimes made to other purpofes; for themfelves have confeffed to me, that they made theirs chiefly of refin, melted and brought to a ftiff pafte with fine brick-duft, to which one

one of the eminenteſt of them for ſkill adds a proportion of ſealing-wax (I told him I preferred plaiſter of Paris before brick-duſt, and he told me he did the like).

And indeed by variety of cements we may be aſſiſted to make divers experiments that we could not otherwiſe make ſo well, if at all; for which reaſon I have been ſomewhat curious about making a pretty number of ſuch mixtures, whoſe compoſitions you may command of me.

There are divers artificers, eſpecially thoſe that ſlit and poliſh cryſtal, agates, and other hard ſtones, and cut ſeals in gems, who have need of powders of emery, of differing degrees of fineneſs, and ſome of them extremely ſubtle; to obtain theſe, one would think it neceſſary to have variety of ſearces, and ſome of them as fine as it is poſſible. But the ſkilfulleſt artificers judge they can obtain their deſire much better by fair water, than by the beſt ſearces. For having in a mortar beaten the hard body of emery as long as they think neceſſary, they put the powder into a pail or other fit veſſel full of water, and then with a ſtick, or ſome ſuch thing, they ſtir very well all that is at the bottom, that it may be raiſed and thoroughly mingled with the liquor; then pouring it out into another veſſel, the groſſeſt and the moſt ponderous grains of the diſperſed powder will firſt fall to the bottom, and give a powder leſs groſs than that which remained in the firſt veſſel, which may be again beaten ſmall in the mortar. Afterwards they pour the troubled water of the ſecond veſſel into a third, and there ſuffer the duſt to ſubſide, and then decanting the liquor, if this duſt be not yet fine enough, they trouble the water again, and after a little while pour it off either into one veſſel, or two, or more, ſucceſſively, according to the exigency of their uſes; and then ſuffering the tranſvaſated water to ſettle for ſome hours (more or fewer) as the diſperſed duſt is more or leſs light, they decant the liquor, or ſuffer it to exhale, and take the remaining powders, of which that which ſettles ſloweſt, will oftentimes be ſtrangely ſubtle. And by this way, if a man will have patience to pour ſucceſſively the troubled liquor into veſſels enough, and give the diſperſed powder a competent time to let fall the leſs light parts, before the upper part of the water be poured off into the veſſel it is finally to ſettle in, he may obtain ſeveral degrees of powders, leſs and leſs groſs, and ſome ſo fine, as one would admire how it was made ſo. And this (*Pyrophilus*) I the rather mention to you, becauſe it is not only from emery, but from divers other bodies, that one may obtain extremely minute, and (as they ſpeak) impalpable powders, of great uſe in ſome of the moſt curious trades, and perhaps in phyſick too. For I may elſewhere tell you how I apply this way to magiſteries of cryſtal, and of gems, and even to *Crocus Martis*; the naming of which laſt puts me in mind to add, that a chymiſt, much prized for finer *Crocus Martis*, than others of his profeſſion, and thereby enabled to ſell it at an extraordinary rate, confeſſed to me, that it was to the artifice I had been commending that the *Crocus* he ſold owed all its advantages.

It has long been, and ſtill is in many places, a matter of much trouble and expence, as well of time as money, to cut out of rocks of alabaſter and marble, great pieces, to be afterwards ſquared or cut into other ſhapes; but what by the help of divers tools and inſtruments cannot in ſome quarries be effected without much time and toil, is in other places eaſily and readily performed, by making, with a fit inſtrument, a ſmall perforation into the rock, which may reach a pretty way into the body of it, and have ſuch a thickneſs of the rock over it, as is thought convenient to be blown up at one time; for at the farther end of this perforation (which tends upwards) there is placed a convenient quantity of gunpowder, and then all the reſt of the cavity being filled with ſtones and rubbiſh ſtrongly rammed in (except a little place that is left for a train) the powder, by the help of that train being fired, and the impetuous flame being hindered from expand-

ing,

ing itſelf downwards, by reaſon of the newly mentioned obſtacle, concurring with its own tending another way, diſplays its force againſt the upper parts of the rock, which, in making itſelf a paſſage, it cracks into ſeveral parts, moſt of them not too unwieldy to be manageable by the workmen.

And by this way of blowing up rocks a little varied and improved, ſome ingenious acquaintances of ours, employed by the publick to make vaſt piles, have lately (as I received the account of themſelves) blown up or ſcattered, with a few barrels of powder, many hundred, not to ſay thouſand tuns of common rock.

To give ſmall glaſſes the ſhape that is requiſite to fit them to ſerve for covers to the dial plates of watches, and for other purpoſes to which artificers ſometimes employ them, one would think it neceſſary that they ſhould be ground or otherwiſe wrought with tools, by a ſkilful hand, to give the glaſſes the concave, as well as the convex figure they ought to have. And yet I have learned by trial, that a flat plate of glaſs of a competent thickneſs that has its two ſurfaces ſmooth and parallel to each other, being carefully laid upon a deep ring of iron, or a ſhallow and hollow cylinder of the ſame metal, and of the diameter required, ſo that the edge of the glaſs (which is to be reduced to roundneſs) may every where reſt upon that of the cylindrical piece of metal; the heat of the fire warily and ſkilfully adminiſtered will ſo ſoften this plate of glaſs, that its own weight will ſo depreſs the middle parts, that the glaſs will thereby obtain the figure required. And though ſuch glaſſes do not conſtantly fall juſt into the deſired figure, yet when they are ſkilfully ordered, they fall into it ſo often, that I am told that ſome ingenious artificers have quitted the ordinary way of making covers for watches, for that we have been deſcribing; which, though not free from caſualties, is yet ſo much more cheap and eaſy.

We have in ſome parts of *England* various kinds of talk, or *lapis ſpecularis* (ſeveral of which I have been poſſeſſor of) and of ſome of them there is ſo great plenty, that one may procure good ſtore for little or no charge; but the reducing of a great lump of this talk to fine powder, if it muſt be done the common way, by beating it in mortars, and ſearcing it often, will require much time and pains; but as I have ſeveral times tried, the ſmaller pieces may, by the help of an actual flame, be quickly reduced to a ſnow-white calx; ſo by the experiment of a ſagacious acquaintance of mine, even great lumps of it may, almoſt in a trice, be brought to fine powder, by heating them red-hot, and caſting them, while they are ſo, into cold water, whereby there will preſently be made a comminution of them into a fine, and, as it were, mealy calx.

The ground of this operation is much the ſame with that, whereby ſome chymiſts granulate maſſes of gold and ſilver, when they pour the ſtrongly melted metal from a competent height into cold water, whereupon there happens a diſſilition of the parts of the metal; many of which fall to the bottom in little fragments. But the more eaſily fuſible metals, tin and lead, may be quickly and better granulated by the mechanical way, freſhly mentioned, as to talk. I remember I was wont (eſpecially if the ignition and extinction were repeated two or three times) to reduce cryſtal flints, almoſt in a trice, to a fitneſs to be eaſily brought to a very ſubtle powder, proper to make amaules (or counterfeit gems) of.

The mention I have already made in this eſſay, of what may be performed by the faculty, that burnt alabaſter, made liquid with water, has to grow hard again, puts me in mind of another inſtance, very properly referrible to the ſubject of this eſſay. For one that beholds how curiouſly oranges and lemons, and other fruits are counterfeited in wax, would imagine that ſo lively a repreſentation of them could not be effected but by a hand as ſkilful at leaſt as that of a painter; ſince by this plaſtick art, not oranges, and lemons,

lemons, &c. in general, but this or that particular orange or lemon may be moſt lively repreſented; and yet you may learn this art within one hour or two, the thing being performable eaſily and quickly; for having the orange, &c. we would imitate, we bury it half way in a coffin of clay, whoſe brim, together with the extant part of the fruit, being oiled over to keep the mixture from ſticking, the tempered alabaſter (or plaiſter of Paris) is nimbly laid on to a good thickneſs, and, upon its concretion, removed, whereby you obtain an half mold for that part of the orange; then the formerly latent part of the fruit being likewiſe placed uppermoſt in the half-mold, which ſhould have ſome pretty deep notches cut in the rim of it, which, with the protuberant part of the fruit, ought to be oiled, the tempered mixture is likewiſe put upon that, and thereby an exact mold is completed, at any convenient part of which a hole being made, to pour in a little tempered and coloured wax, when it is brought by fuſion to a due heat (for every degree of that quality is not convenient) ſhaking the mold nimbly and every way, the wax comes to be ſo applied to the internal ſurface, that when the mold is cold, and the parts taken aſunder, you have an orange of wax very lively repreſenting the original.

There are ſome circumſtances belonging to this eaſy and delightful art of molding and caſting in the wax (which is pleaſant enough to be practiſed even by ladies) that I purpoſely omit; what has been mentioned being ſufficient to ſhew you as much as is neceſſary for my preſent purpoſe. And I the rather pitched on this experiment, becauſe it may afford us another inſtance not impertinent to the deſign of this tract. For one that ſhould ſee how great a cavity is left within the counterfeit orange, would think that there were ſome great and rare artifice requiſite to caſt it thus hollow, and make ſo ſmall a quantity of wax reach to the counterfeiting of ſuch a fruit; whereas the bare ſhaking of the mold, when the melted wax is in it, together with the expanſive endeavour of the included air, applies the wax to every part of the inſide of the mold, and thereby turns it into one great film, which one would think it very difficult to ſeparate, without injuring it, from the mold, to which it is applied ſo cloſe; and indeed it might be ſo, if nature did not again aſſiſt the artiſt, by making the mixture, when it cools, ſhrink a little, and thereby part eaſily from the mold it ſtuck to.

But one of the prettieſt and the ſtrangeſt artifices that belong to this eſſay, is that, whereby the knowledge of a few unheeded phyſical properties of two or three bodies may enable a man to perform that, which ſeems to require not only good tools and great dexterity in the art of graving, but likewiſe an exquiſite ſkill in caligraphy or the art of writing fair; for I know a graver, famous for ſkill in his profeſſion, who writes, as I have had good opportunity to obſerve, but a bad hand; and yet this man with his tool writes rarely well, and will imitate and emulate the fineſt copies of the choiceſt writing-maſters, ſo that even virtuoſi have much admired how a man, with a ſtiff iron tool upon a tough copper-plate, can write incomparably fairer than the ſame perſon can with a good pen upon paper. But to eaſe you ſomewhat of your wonder, I ſhall add, that though this artifice be kept for a choice ſecret, and though I could not learn a conſiderable particular or two, which belong to the delicacy of it; yet (partly by putting queſtions, and partly by ſome trials of my own) I attained to the ſubſtance of this myſtery, as they call it, which ſeems to be this.

A writing-master, or ſome other that writes a very fair hand, is deſired to write a copy, or what elſe is to be engraven, with a peculiar kind of ink, which differs not in ſhew from common ink, being fully as black as it: then they take a very clean and well-ſmoothed copper-plate, which being moderately warmed, is to be ſo rubbed over with a certain white varniſh, or ſomething equivalent (to be mentioned a little beneath) that when the plate grows cold again, it may be thinly and evenly caſt over with a kind of ſkin or film (if I may ſo call it) of varniſh; then lightly moiſtening the paper, that it may part with its ink the more readily, the written ſide is to be laid on the prepared ſide of the plate, and that, together with the paper, being paſſed through a rolling-preſs, enough of the ink will ſtick (but in an inverted poſture) to the varniſh, whoſe whiteneſs renders the black letters very conſpicuous; ſo that it is eaſy with a needle fitted with a wooden handle, to draw over the very ſame lines and ſtrokes through the yielding varniſh upon the metalline plate, whence they may, after the plate is by heat, or otherwiſe, freed from the varniſh, be completed with a graver; and laſtly, when the whole engraving is finiſhed up, may be printed off in a rolling-preſs like ordinary cuts. And even without a rolling-preſs I have ſometimes taken off written characters, only by laying the moiſtened paper very ſmooth upon the varniſhed copper, and rubbing it hard thereon with a convex piece of glaſs or ſome ſuch ſmooth and hard body, whoſe preſſure makes the ink ſtick to the varniſh, for which I have uſed the purer ſort of virgin wax, if the ink be good, and have been laid on plentifully enough by the pen. That ink, which I moſt uſed, I made only of fine Franckfort black, as the painters that ſell it are wont to call it; by grinding it little by little, but very diligently, with water, till it had attained the conſiſtence of a ſomewhat thick ink; in which this only circumſtance is carefully to be obſerved, that no gum be added, as is uſual in other inks, leſt that hinder its coming off.

And here it will not be impertinent to the argument in hand to add another artifice, whereby a printed cut may be ſo far taken off, that at leaſt the out-lines and the principal ſtrokes may be ready copied for the graver's hand; by which way, beſides other uſes that may be made of it, copies of rare and choice pieces may be procured, and the periſhing or want of the originals ſupplied; if then the print to be taken off be recent enough (as it is wont to be, if it exceed not a year, or perhaps two) then the paper needs only be well moiſtened, as if it were to be printed off at a rolling-preſs (with the ink proper to which it is ſuppoſed that the cut was, as uſually cuts are printed off): but if the picture or ſcheme be more ancient, it muſt be laid all night to ſoak in water, and then hung in the air till it have but ſuch a degree of moiſture as makes it fit for the rolling-preſs. The paper being thus prepared, either by bare wetting, or by ſteeping, the printed ſide is to be laid upon a copper-plate, thinly caſed over, as was formerly directed, with virgin-wax; for the plate and paper being put into a rolling-preſs, the compreſſion of that will make the moiſtened ink ſtick to the pure wax, which conſequently will take the impreſſion of the cut, or at leaſt of the outlines and chief ſtrokes of it.

There is another thing which ſeems above all theſe to require the expreſs and immediate operation of the hand, and it is a phyſical way, if I may ſo ſpeak, of tranſcribing a whole page of a letter, or other writing, all at once. Whether this can be performed cheaply and eaſily enough for common uſe, is hereafter to be conſidered. But that, abſtracting from theſe circumſtances, it is poſſible to be done (by an artificial application of phyſical things) I have been perſuaded by ſome experience; of which I may in one of the following papers give you a more particular account than I now conveniently can.

In the former part of this eſſay, *Pyrophilus*, I have preſented you ſome inſtances wherein phyſiological knowledge may be ſubſtituted for manual dexterity, mechanical tools, and even mathematical inſtruments; but now to ſhut up this diſcourſe, I ſhall ſubjoin a relation that will manifeſt, that even a mathematician and an engineer may ſometimes perform that by the knowledge of a ſlight phyſical quality of obvious bodies, which, without that knowledge, all his ſkill in mathematical diſciplines, and his vaſt and artificial engines, will not have enabled him to accompliſh. For who would think, that by a comparatively few pounds of water (perhaps the moiſture of the air in wet weather might have ſufficed) a maſſy body of peradventure ſome hundred thouſand pounds in weight

weight ſhould be raiſed; and yet, that this was performed at *Conſtantinople*, is one of the remarkableſt things I remember I met in the ingenious account of his voyage that is given by the learned *Buſbequius*, ambaſſador from the king of the Romans to the Turkiſh emperor. His words are theſe. * *De obeliſco, cujus ſuprà memini, qui eſt in hypodromo, ſic Græci commemorant; à baſi convulſum multis ſeculis jacuiſſe humi: tempore poſteriorum imperatorum repertum architectum, qui operam ſuam in eo ſuæ baſi reſtituendo deferret; illumque, poſtquam de pretio conventum eſſet, ingentem apparatum organorum ex trochleis & funibus præſertim inſtituiſſe, quibus lapidem illum ingentem erexerit, ſublimemque eo evexerit, ut uno tantùm digito abeſſet à dorſo aſtragalorum, quibus imponi debebat, tum indicâſſe populum ſpectatorem oleum illi operam tanti apparatus periiſſe, magniſque denuo laboribus & impenſis opus inſtaurandum: at illum minimè diffiſum perito à rerum naturalium ſcientia ſubſidio juſſiſſe afferri immenſam aquæ vim, qua multis horis in machinam illam injecta, funes, quibus obeliſcus librabatur, ſenſim madefactos rigenteſq; (ut eorum eſt naturæ) ſe contraxiſſe, ſic ut obeliſcum altiùs ſublatum in aſtragalis ſtatuerunt, magna cum admiratione & plauſu vulgi.* And for confirmation of this narrative, it may be added, that the ſame thing is mentioned by good authors, as having been practiſed elſewhere; and a like ſtory is allowed, and ſomewhere made an argument of, to another purpoſe, by that great maſter of mechanicks *Galileo* himſelf.

To catch any ſtore of fiſh the ordinary way, you know it is cuſtomary, that even in rivers, either ſtore of angles, and ſome ſkill in uſing them, or nets, or ſome other artificial inſtruments be made uſe of; and if it be in the ſea that men are to fiſh, large nets or ſome peculiar contrivances are employed as neceſſary; and one would not expect from ſuch people as the Americans, eaſier ways of fiſhing than theſe, and yet theſe illiterate barbarians, by having found out (probably by chance) the phyſical property of a wood, make that ſerve them to catch fiſh in great plenty, and with as much eaſe. For our late Engliſh navigators have obſerved, as their voyages witneſs, that in ſome parts of the *Weſt-Indies*, the natives, by impregnating the water with this wood, do ſo ſtupify the fiſh, that rolling up and down upon the ſurface of the water, as if they were foxed, they are eaſily taken up in great numbers in their hands; which relation of our ſeamen I therefore, notwithſtanding its ſtrangeneſs, ſcruple not to alledge, partly becauſe, that though we do not uſe a ſimple drug, much leſs a wood, for the ſame purpoſe, yet our foxing-ſtuff, as they call it, which is but a ſlight compoſition, produces effects not much inferior; and partly, becauſe having purpoſely inquired of a learned phyſician that came not long ſince out of a part of *America*, where this practice is in requeſt, he aſſured me, that he ſaw the Engliſh themſelves uſe this way of fiſhing, only by tying a log of this wood, to which, for what reaſon I know not, they have given the name of dogwood, to the ſtern of their boats; ſo eaſily does the odd property of this wood enable them that make uſe of it, to catch fiſh.

To take off the hair is generally ſuppoſed to require both a razor and other implements, and the manual ſkill and operation of a barber, eſpecially if the hair be grown under the arm-pits, and in other places, which an inconvenient ſituation or figure makes to be of difficult acceſs; and yet by the knowledge of a property of that natural production, formerly mentioned in the ſixth eſſay, under the name of *Ruſma*, the hair may be, without inſtruments, taken off from any part of the body, and that not only in much ſhorter time than is required to ſhaving, but, as far as the eye is wont to diſcern, by the roots, which makes it much longer before the part be again covered with hair of the former dimenſions. The way uſed in the eaſt to affect this the forecited *Bellonius*

* Aug. Buſbequii, Epiſt. I. pag. 69.

annexes, inſtead of which I ſhall tell you what I tried with a parcel of it, brought into *England* before I met with his obſervations about it. We mixed the fine powder of it with an equal weight of ſtrong powdered quick-lime (*Bellonius*, probably not without reaſon, preſcribes but half as much quick-lime) and having ſuffered them to ſoak together a ſhort while in a little fair water, we thinly ſpread the ſoft paſte or ſlime, made by the water and ingredients, upon that part of the body which we deſigned to free from hair; and having ſuffered this mixture to ſtay on about three minutes, or ſixtieth part of an hour, meaſured by a minute-watch (our author preſcribes as long time as is requiſite to the boiling of an egg) we wiped it off with a linen-cloth dipped in warm water, and found the hair taken off by the roots, without any inconvenience to the part that we could diſcern, though I ſeveral times ſhewed the experiment to others, and the trial of it was more than once made upon myſelf.

It may ſeem ſcarce poſſible, without the help of water, or any engine made with ſprings or wheels, to meaſure time, though but for a little while, as exactly as our beſt clepſydras, clocks, or watches are wont to do. And yet (which is now a known and almoſt vulgar thing) ſuch an account of time may be kept by him, that has obſerved that the vibrations or diadroms of a pendulum are made in ſenſibly equal ſpaces of time, though the arches continually decreaſe, that are made by the ſwinging pendulum (as you know they now call a bullet, or the like weight hanging at the end of a ſtring from a nail or other fixed ſupporter). For by ſo ſlight a thing as I have been mentioning, if you watchfully obſerve and reckon the returns that the ſwinging weight makes towards you in a minute or other determinate ſpace of time, doubling the number of thoſe returns, and adding thereto an unit, if you left off counting, when the weight was at the further end of the arch deſcribed by its motion, you may obtain a more accurate diviſion of time, than by any of the formerly known ways of meaſuring it. For if you make your pendulum of the length of very little (perhaps a tenth of an inch) leſs than ten inches (or twelve parts of our Engliſh foot *) accounted from the nail, or other thing, whence it is ſuſpended to the center of the piſtol-bullet (or the like ſmall round weight); and, removing this a pretty way from the perpendicular it naturally reſts in, ſuffer it to fall gently out of your hand, each of its two ſwinging motions (the one whereby it is carried from you, and the other whereby it returns to you) will be (eſpecially whilſt the arches are of a moderate length) phyſically *æqui-temporaneous*; and theſe motions will very diſtinctly enough, to an attentive eye, divide a minute or ſixtieth part of an hour into an hundred and twenty parts (called half-ſeconds) and will conſequently divide an hour into ſeven thouſand two hundred parts, if not perfectly equal, yet leſs unequal, as to ſenſe, than the diviſions of time made even by good watches are wont to be. And therefore this way may be of very great uſe, in making aſtronomical and other obſervations that laſt not long, but require exact meaſures of time. And by the help of a pendulum, a ſkilful muſician of my acquaintance teaches his unpractiſed ſcholars to keep time when they ſing in his abſence. But when we meaſure experiments by the excurſions and returns of a weight, the beſt way is to make the duration of the pendulum's whole motion (before it come to reſt) as long as the place where the experiment is made will permit, renewing now and then, if need be, the impulſe given to the weight, when the arches begin to grow too ſhort; that being increaſed, the vibrations may be the better reckoned.

* N. B. The author has elſewhere ſhewn, that the Engliſh foot differs very little, if at all, from the ancient Romans.

The mention I have been making of the uſes of pendulums, joined to that I lately made of æquivelocity of ſounds, brings into my mind another inſtance pertinent to this part of our diſcourſe. For it is not impoſſible, by the knowledge of the velocity of a ſound's motion in the air, and the æquivelocity (as to ſenſe) of great and ſmall ſounds, to meaſure without geometrical inſtruments, in ſome caſes, the breadth of a river, though exceeding wide, or the diſtance of the place one ſtands in, from the top of a high tower or hill on the other ſide of a river, or ſituated in ſome inacceſſible place, and this, in caſes where the difference of ſtations uſually in geometrical menſurations is not allowed. The way is evident by what is elſewhere delivered. For it having been found by *Merſennus*'s trials, that ſounds (as well ſmall as great) do move in a ſecond (as they call the ſixtieth part of a minute) 230 fathoms, or thirteen hundred and eighty feet; if I ſee my correſpondent fire a gun on the other ſide of the river, or if I ſee muſkets or other guns caſually fired on ſome tower or baſtion, though never ſo far diſtant, and never ſo inacceſſible to me, it is eaſy for me, by letting fall a ſhort pendulum as ſoon as I ſee the flaſh of light produced by the kindled powder, and by reckoning the vibrations (made by that ſhort pendulum, which diſtinguiſhes ſeconds into halves or quarters) that ſhall happen to be made before the noiſe arrive at my ear, to know how far off the place where the gun was diſcharged is from that I am in. As if a correſpondent, ſtanding over againſt me on the other ſide of a river, or ſome ſoldiers being there exerciſing, I ſee the flaſh or ſmoak of a muſket, or other gun, two ſeconds ſooner than I can hear the report of it, I may conclude the river to be 2760 feet broad; and if a piece of ordnance being fired upon the tower of a beſieged place, the noiſe arrive at my ear in half a ſecond, I may collect 690 feet to be the diſtance betwixt that gun and my ſtation. And by this means may that problem be performed, that we elſewhere mention as a thing, which, when nakedly propoſed, may ſeem impoſſible. For if I ſee a ſhip at ſea be ſhooting, whether in earneſt, or for ſalutation, or for joy, it is very poſſible for me to meaſure, without geometrical inſtruments, how far it is off, though the ſhip itſelf be under ſail. For veſſels that fire guns, uſually firing more than one, whether to offend their enemies, or to ſalute their friends, it is eaſy to take warning, by the firſt gun, to be in readineſs with a ſhort pendulum againſt another to be fired, and in this way of meaſuring (though not in any other yet known) one may take diſtances in the darkeſt night. For it matters not, whether I ſee the ſhip or place, whoſe remoteneſs from me I would know, provided, by ſome candle or taper I ſee the pendulum before the flaſh of the fired gun, which will ſufficiently diſcover itſelf by its own light. And (to add, that upon the by) I have had ſometimes thoughts, that if the velocity of echoes, which are but reflected ſounds, be ſo well determined as that of direct ſounds, navigators might ſometimes make uſeful eſtimates in dark nights, whether they be near coaſts, or conſiderably great rocks. For though upon diſcharging a gun, they cannot conclude how near the ſhore they are, becauſe there may be parts of it leſs remote than thoſe that ſend the echo; yet if they follow very quick upon the diſcharge of the gun, they have reaſon to ſuſpect, that the ſhore, whoſe approach the ſeamen do ſo juſtly fear in the night, is at leaſt as near as the vibrations of the pendulum inform them that the echoing place is.

annexes, inftead of which I fhall tell you what I tried with a parcel of it, brought into *England* before I met with his obfervations about it. We mixed the fine powder of it with an equal weight of ftrong powdered quick-lime (*Bellonius*, probably not without reafon, prefcribes but half as much quick-lime) and having fuffered them to foak together a fhort while in a little fair water, we thinly fpread the foft pafte or flime, made by the water and ingredients, upon that part of the body which we defigned to free from hair; and having fuffered this mixture to ftay on about three minutes, or fixtieth part of an hour, meafured by a minute-watch (our author prefcribes as long time as is requifite to the boiling of an egg) we wiped it off with a linen cloth dipped in warm water, and found the hair taken off by the roots, without any inconvenience to the part that we could difcern, though I feveral times fhewed the experiment to others, and the trial of it was more than once made upon myfelf.

It may feem fcarce poffible, without the help of water, or any engine made with fprings or wheels, to meafure time, though but for a little while, as exactly as our beft clepfydras, clocks, or watches are wont to do. And yet (which is now a known and almoft vulgar thing) fuch an account of time may be kept by him, that has obferved that the vibrations or diadroms of a pendulum are made in fenfibly equal fpaces of time, though the arches continually decreafe, that are made by the fwinging pendulum (as you know they now call a bullet, or the like weight hanging at the end of a ftring from a nail or other fixed fupporter). For by fo flight a thing as I have been mentioning, if you watchfully obferve and reckon the returns that the fwinging weight makes towards you in a minute or other determinate fpace of time, doubling the number of thofe returns, and adding thereto an unit, if you left off counting, when the weight was at the further end of the arch defcribed by its motion, you may obtain a more accurate divifion of time, than by any of the formerly known ways of meafuring it. For if you make your pendulum of the length of very little (perhaps a tenth of an inch) lefs than ten inches (or twelve parts of our Englifh foot *) accounted from the nail, or other thing, whence it is fufpended to the center of the piftol-bullet (or the like fmall round weight); and, removing this a pretty way from the perpendicular it naturally refts in, fuffer it to fall gently out of your hand, each of its two fwinging motions (the one whereby it is carried from you, and the other whereby it returns to you) will be (efpecially whilft the arches are of a moderate length) phyfically *æqui-temporaneous*; and thefe motions will very diftinctly enough, to an attentive eye, divide a minute or fixtieth part of an hour into an hundred and twenty parts (called half-feconds) and will confequently divide an hour into feven thoufand two hundred parts, if not perfectly equal, yet lefs unequal, as to fenfe, than the divifions of time made even by good watches are wont to be. And therefore this way may be of very great ufe, in making aftronomical and other obfervations that laft not long, but require exact meafures of time. And by the help of a pendulum, a fkilful mufician of my acquaintance teaches his unpractifed fcholars to keep time when they fing in his abfence. But when we meafure experiments by the excurfions and returns of a weight, the beft way is to make the duration of the pendulum's whole motion (before it come to reft) as long as the place where the experiment is made will permit, renewing now and then, if need be, the impulfe given to the weight, when the arches begin to grow too fhort; that being increafed, the vibrations may be the better reckoned.

* N. B. The author has elfewhere fhewn, that the Englifh foot differs very little, if at all, from the ancient Romans.

THE

The mention I have been making of the uses of pendulums, joined to that I lately made of æquivelocity of sounds, brings into my mind another instance pertinent to this part of our discourse. For it is not impossible, by the knowledge of the velocity of a sound's motion in the air, and the æquivelocity (as to sense) of great and small sounds, to measure without geometrical instruments, in some cases, the breadth of a river, though exceeding wide, or the distance of the place one stands in, from the top of a high tower or hill on the other side of a river, or situated in some inaccessible place, and this, in cases where the difference of stations usually in geometrical mensurations is not allowed. The way is evident by what is elsewhere delivered. For it having been found by *Mersennus*'s trials, that sounds (as well small as great) do move in a second (as they call the sixtieth part of a minute) 230 fathoms, or thirteen hundred and eighty feet; if I see my correspondent fire a gun on the other side of the river, or if I see muskets or other guns casually fired on some tower or bastion, though never so far distant, and never so inaccessible to me, it is easy for me, by letting fall a short pendulum as soon as I see the flash of light produced by the kindled powder, and by reckoning the vibrations (made by that short pendulum, which distinguishes seconds into halves or quarters) that shall happen to be made before the noise arrive at my ear, to know how far off the place where the gun was discharged is from that I am in. As if a correspondent, standing over against me on the other side of a river, or some soldiers being there exercising, I see the flash or smoak of a musket, or other gun, two seconds sooner than I can hear the report of it, I may conclude the river to be 2760 feet broad; and if a piece of ordnance being fired upon the tower of a besieged place, the noise arrive at my ear in half a second, I may collect 690 feet to be the distance betwixt that gun and my station. And by this means may that problem be performed, that we elsewhere mention as a thing, which, when nakedly proposed, may seem impossible. For if I see a ship at sea be shooting, whether in earnest, or for salutation, or for joy, it is very possible for me to measure, without geometrical instruments, how far it is off, though the ship itself be under sail. For vessels that fire guns, usually firing more than one, whether to offend their enemies, or to salute their friends, it is easy to take warning, by the first gun, to be in readiness with a short pendulum against another to be fired, and in this way of measuring (though not in any other yet known) one may take distances in the darkest night. For it matters not, whether I see the ship or place, whose remoteness from me I would know, provided, by some candle or taper I see the pendulum before the flash of the fired gun, which will sufficiently discover itself by its own light. And (to add, that upon the by) I have had sometimes thoughts, that if the velocity of echoes, which are but reflected sounds, be so well determined as that of direct sounds, navigators might sometimes make useful estimates in dark nights, whether they be near coasts, or considerably great rocks. For though upon discharging a gun, they cannot conclude how near the shore they are, because there may be parts of it less remote than those that send the echo; yet if they follow very quick upon the discharge of the gun, they have reason to suspect, that the shore, whose approach the seamen do so justly fear in the night, is at least as near as the vibrations of the pendulum inform them that the echoing place is.

ESSAY

ESSAY X.

OF
MEN's GREAT IGNORANCE
Of the USES of
NATURAL THINGS:
OR,
That there is ſcarce any one Thing in Nature, whereof the Uſes to human Life are yet thoroughly underſtood.

THIS being an entire propoſition, and clear enough of itſelf, will not need to be explicated, but evinced.

AND evinced ſomewhat ſolemnly it will require to be, not only becauſe it is a paradox, but ſuch an one as will meet with a peculiar indiſpoſition to be entertained; ſince men cannot allow this paradox to be a truth, without ſuch a confeſſion of their ignorance, as muſt implicitly accuſe them of lazineſs too. But however, I think, we may juſtly enough apply, with a little variation, to our preſent purpoſe, that true ſaying of *Seneca*, *Multi ad ſapientiam perveniſſent*, *niſi*, *&c.* and affirm, that many had attained to a greater knowledge and command of nature, if they had not preſumed, that what is arrived at already, is much greater, and more conſiderable than indeed it is; eſpecially in compariſon of what is ſtill behind, and yet attainable; and therefore, I think it not fit to ſuppreſs the conſiderations I was about to mention, ſince the diſplaying them may perhaps do you and others ſervice, if they rouze up your curioſity, by ſhewing how much it has been defective, and if (which they ought to do) they encourage it alſo, by ſhewing you how much of nature undiſcovered there yet remains, to recompence, as well as exerciſe your induſtry.

BUT becauſe that of the particulars whereby our paradox may be confirmed, there are divers, that properly belong to the next enſuing eſſay, the proofs that we ſhall mention in this diſcourſe, though I hope they will appear ſufficient alone, will yet be much ſtrength-

ſtrengthened, both as to number and weight, if you pleaſe to add to them thoſe inſtances to be mentioned in the next diſcourſe, that may be conveniently referred to this. In which I ſhall therefore inſiſt but upon five general conſiderations; in all which I hope you will not forget, that I have already taken it for a ſuppoſition, which I doubt not of your granting me, that the uſefulneſs of the works of nature to us depends chiefly upon the knowledge we have of their properties and other attributes; and conſequently, that the more we know of theſe, the greater uſe we are like to be able to make of thoſe phyſical things (and on the contrary); and therefore that ought to be looked on as an uſe of a phyſical thing, even though not immediately practical, that helps us to make diſcoveries of things that properly may prove ſo.

SECTION I.

AND I conſider in the firſt place, "That there are very few of the works of nature "that have been ſufficiently conſidered, and are thoroughly known," even as to thoſe qualities and other attributes of this and that body (or other phyſical thing) which belong properly to it, and are not thought to be ſo relative to other bodies. It is not only in the terreſtrial globe, but in almoſt every body to be met with in it, that there may be a kind of *terra incognita*, or undetected part, whoſe diſcovery is reſerved for our further induſtry.

THIS will appear the leſs improbable, if we conſider theſe two things; whereof the one is, that there are divers ways of inveſting the attributes of bodies, as chymical, optical, ſtatical, &c. which being artificial, and requiring ſkill, and induſtry, and inſtruments, there are very few men that have had the curioſity and ability to examine them after theſe ſeveral ways; without which, nevertheleſs, divers other attributes, ſome of which probably are capable of uſeful application, are not like to be diſcovered. To the proof of which, if it were needful, a multitude of paſſages in theſe preſent eſſays, as well as in our other writings, might be eaſily referred.

I SHALL therefore rather inſiſt a little on the ſecond of the two particulars lately mentioned. For it will eaſily appear not unlikely, that there ſhould be many things undiſcovered in the other works of nature, when there are ſo even in thoſe obvious and familiar objects, that men are frequently converſant with, and have occaſion to take notice of; nay, even in thoſe nobleſt of mere corporeal things, our own bodies, whoſe ſtructure does ſo much merit our curioſity, and of which it ſo highly concerns no leſs than our healths and lives, that we have an accurate knowledge. How many new diſcoveries have been made in the preſent age, beyond what the induſtry of the phyſicians and philoſophers for above two thouſand years has been able to take notice of? Witneſs the circulation of the blood, the Aſellian, Pecquetian, and Bartholinian veſſels; to which may be added, the Ductus Pancreaticus, and to which I doubt not will be added divers other diſcoveries, to recompenſe the induſtry of the anatomiſts of this inquiſitive age.

IN ſo familiar bodies as eggs and chickens are, which ſo many thouſand perſons do daily ſee and handle, and perhaps eat, though many ages ſince, even *Ariſtotle* was ſolicitous about the hiſtory of them, concerning which he has delivered divers not inconſiderable particulars; yet there has been little within theſe few years ſo much undiſcovered, that whilſt men were hotly diſputing whether the chick was firſt formed of the yolk or the white, our excellent *Harvey* made it evident (which our own obſervations have confirmed to us) that it is made of neither, nor yet of the treadle (as ſome modern obſervers have taught) but of the cicatricula, or ſpeck that appears on the coat of the yolk.

WHO

Who would imagine, that in a body ſo familiar, and ſo often treated of by philoſophers, as ſnow, mankind ſhould, for ſo many ages, take no notice of a thing ſo obvious as the figure of it frequently is? and yet *Kepler* is, by a very learned writer, acknowledged to have been the firſt that acquainted the world with the ſexangular figure (as it is wont to be called) of ſnow, in a diſcourſe by him publiſhed on that ſubject. And though I find mention made of it in *Olaus Magnus*, and have obſerved it ſo often (but not conſtantly in the ſame ſhape) eſpecially about the beginning of the ſeaſon of ſnow, that I cannot but admire men ſhould not have very early heeded ſo obvious a phænomenon; yet I find not the diſcovery of it had been made ſo much as an age ago.

As many ages as vinegar has been one of the commoneſt liquors in the world, yet, that it oftentimes abounds with ſhoals of living creatures, that move, and in the microſcope look like little eels, was looked upon but few years ſince as ſo new a diſcovery, that when, as I formerly noted, I firſt propoſed it here in *England* to divers very learned men and virtuoſi, as a thing to be ſeen even without the help of a magnifying glaſs, they took it to be a deception of my eyes, till their own aſſured them of the contrary.

That the milky way, though conſiſting of innumerable ſtars, ſhould for two thouſand years paſs for a meteor, the inconſpicuouſneſs of thoſe ſtars keeps me from much admiring. And, for the ſame reaſon, I wonder not that the men that lived before *Galileo* reckoned no more than ſeven planets, or ſuſpected not that *Venus* herſelf is ſometimes horned, and has her full and wane as the moon. Though theſe inſtances may ſerve to confirm what I lately told you, that many of the attributes of bodies are not like to have been diſcovered by thoſe that employed not artificial helps But what may we not expect that mankind may overlook, when the ſun himſelf, which is not only the moſt conſpicuous body in the world, but that, by whoſe light we ſee all the others, may have vaſt and dark bodies (perhaps bigger than *Europe* or *Aſia)* frequently enough generated and deſtroyed upon him, or about him; and men, without excepting aſtronomers, never took notice of it, till of late years the excellent *Galileo*, or the induſtrious Jeſuit *Scheiner* informed the world of them. And though I grant, that they diſcovered them by the help of teleſcopes (inſtruments unknown to the ancients) yet if men had been as watchful as the nobleneſs and conſpicuouſneſs of the object would make one expect, they might have diſcovered ſome ſpots at leaſt without thoſe helps. For I find by an Italian letter of *Galileo*'s, that ſome curious perſons of his acquaintance, after his diſcoveries had awakened them, deſcried and diſcovered ſome of thoſe ſolar ſpots with their naked eyes, unaſſiſted by his tubes.

It may belong to this firſt ſection of our preſent eſſay to take notice, that one account, on which we may reaſonably ſuppoſe men to be ignorant of the uſes even of thoſe things wherewith they think themſelves well acquainted, may be, that the bare difference of climates and of places, may even in ſuch bodies, as we familiarly converſe with, beget ſuch new relations betwixt them, as may endow them with qualities, and fit them for uſes we dreamed not of.

I will not here mention the differing qualities, that bodies vulgarly referred to the ſame ſpecies of plants, animals, and other bodies, in almoſt all countries, are endowed with in ſome countries (as, that ſpiders are not venomous in *Ireland*, and Iriſh wood in general, if the received tradition be true, has an hoſtile faculty againſt venomous creatures) becauſe the inſiſting on this ſubject would take up too much room in this place, and is reſerved for another; and therefore I'll only add a couple of inſtances, the one to manifeſt what difference of climates may do, and the other to ſhew the unexpected influence of difference of places, though perhaps in the ſame climate.

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THE firſt of theſe examples is afforded us by water and ice; for thoſe that live in thoſe warmer regions where it never freezes, and who have divers of them derided the relations of what happens in gelid climates as ridiculous, in probability would never dream that it could be a familiar uſe of a liquor they were ſo well acquainted with as water, to be broken or beaten in mortars like a dry body, and carried in carts or wheel-barrows from place to place, and kept all the year in that form, to make other water intenſely cold in the greateſt heats of ſummer. And even amongſt us, thoſe that have not been very inquiſitive, can ſcarce imagine that one of the uſes of water ſhould be to ſerve for highways, whereon armies may march for divers days together with all their carriages and artillery, and whereon they encamp and fight battles with as much aſſurance as on the firm land; and yet thoſe that have been in *Ruſſia*, and the neighbouring northern countries aſſure us, that during the winter, when the rivers are frozen over, they uſually take great journies on them, and oftentimes rather than in ſummer, and chooſe that rigorous ſeaſon, which allows them to march every where without ſinking into the ground, to proſecute their wars in.

THE ſecond of the forementioned inſtances we are ſupplied with by the declination of the magnetick needle from the true north and ſouth points, and the variation of that declination. For though the loadſtone were highly admired as well by philoſophers and mathematicians as the vulgar; and though, ſince the great and happy uſe of it to navigation has been generally known, men have been upon ſeveral accounts invited to conſider it with a peculiar attention and regard, yet that in ſome places the magnetick needle does not point directly, perhaps not by a great many degrees, at the pole, as in others it does, is no ancient obſervation, ſince it is aſcribed to *Sebaſtian Cabot*; and it appears by the writings of our famous countryman *Gilbert* himſelf *, that it muſt be ſomebody that lived ſince he wrote that muſt have the honour of being allowed the firſt obſerver of that ſtrange and unexpected phænomenon, that oftentimes in the ſelf ſame place, the declination of the needle towards the eaſt or weſt does in proceſs of time conſiderably alter. Which diſcovery I could confirm, by comparing ſome obſervations I have had opportunity to make, with thoſe recorded by ſome modern authors.

AND as the ſame kind of bodies may have differing qualities, and conſequently uſes in differing places; ſo they may have, if examined or employed at differing times, comprizing under that name, together with the four ſeaſons of the year, thoſe peculiar ſeaſons or periods of time, to which ſome ſignal change of qualities or ſtate in particular bodies do belong.

THE mutations, upon the account of time, which I am here ſpeaking of, are not thoſe that are obvious to every eye, ſuch as the differing qualities of fruit green and ripe, or the degeneration of wine into vinegar; but ſuch as are not vulgarly taken notice of, and require either ſkill or curioſity, or both, in the obſerver; and of theſe a few inſtances will ſuffice for a taſte.

WHEN common urine either is freſhly made, or has not long been kept, the volatile and pungent ſalt is ſo clogged with other particles wherewith it is aſſociated, that uſually to obtain it one muſt evaporate or diſtil away near eight or nine parts of ten of the liquor, and then at length give a not inconſiderable heat to force up the laſt; but though the tradeſmen that deal in urine do commonly overlook the difference, yet if the crude liquor be kept ſix or ſeven weeks, though not near the fire, the ſaline and noble parts will have ſo extricated themſelves, that a very gentle heat will make them aſcend, and leave behind them that phlegm that formerly would have preceded them.

* *Gilbert* de magnete, lib. I. c. 1. See the ſame *Gilbert*, lib. IV. c. 3.

That the Thames water, which our navigators are wont to take with them in long voyages, after a while, if they fail into hot climates, ftinks very often too offenfively to be potable, that, which happens ufually to water, which is vulgarly obferved to putrefy by long ftanding, will eafily perfuade us; and yet it is found, that this water, by being kept long enough in the fame veffels, though it be in the fame, or even in an hotter climate, will at length lofe its ftink, and grow potable again; as I have, upon enquiry purpofely made, been affured, not only by the vulgar tradition, but by two very inquifitive perfons upon their own knowledge; the one having particularly obferved it, failing betwixt *Europe* and *Africa*, and the other in a voyage to and from *America*. And I the rather mention this, becaufe I am very credibly informed that there are divers other waters that have this faculty of recovering after putrefaction, which is fuppofed to be peculiar to the water of the Thames.

And, if I much miftake not, one or both of thefe very perfons named another river to me, with an affirmation of its having the fame power of felf-recovery. And having held fome curiofity to try experiments, how pump-water, or the like rough water, as they call them, that would not bear foap, may be helped; an induftrious perfon I employed affured me, that he met with pump-waters, which after having ftood a few days, without having any thing done to them, would bear foap, which before they would not do.

Coriander feeds being frefhly gathered have been obferved to have fo much acrimony, that divers of the ancient phyficians reckon them among venemous plants; and in difpenfatories they are ufually prefcribed to be prepared with vinegar, or fome other corrective; whereas the more accurate obfervers take notice, that within a competent time after the feed is gathered, it lofes of itfelf that exceffive acrimony that at firft blemifhed it. And the like I find obferved, by good apothecaries, of the roots of aron, which are mitigated by keeping (and which fome noted phyficians of my acquaintance do little lefs magnify to me than does *Quercetan* himfelf).

[That vegetables, what known way foever they are wont to be laid up and ordered, do not afford, unlefs firft reduced to foot, any dry volatile falt, like that of animal fubftances, I elfewhere more particularly declare, and thofe that have had the curiofity to try it, will confirm; but yet by fome difcourfe I lately had with a very ingenious perfon, and fome fubfequent trials made after a way I devifed to examine diftilled liquors, I was fatisfied that there are divers vegetables, and thofe very commonly growing here in *England*, which being gathered and laid together at a certain feafon, and diftilled alfo at a certain nick of time, will yield, inftead of the vinegar-like, and other liquors, wont to be afforded by fuch plants diftilled the common way, a volatile fpirit; which in fmell, tafte, and divers operations, as turning fyrup of violets green, hiffing with acid fpirits, &c. refembles the volatile fpirits and falts of animal fubftances; and which I doubt not but you will wonder at, this great change, whofe fecret I wifh I durft teach you, is effected without the help of any additament.]

And, that you may not think that it is only in vegetable and animal fubftances, that are commonly of a more loofe or alterable texture, that the trying things at one time rather than another may be very confiderable, I will add a couple of inftances even in mineral bodies.

It is a chymical complaint, even of the curious and experienced, that though authors teach us to make the falt of violently diftilled or calcined vitriol, by forthwith taking the caput mortuum (from which all the oil has been by the violence of fire forced out) and extracting the faline part by effufions of water; yet thofe that make exact trials of it find, that when the dark red mafs of powder is newly taken out of the veffels, it is fo so totally robbed of its saline particles, that no affusion of water will at all obtain from it the expected salt. Notwithstanding which, having purposely enquired of some that distil great quantities of oil of vitriol, whether or no, when they had made an end of one distillation, if they lay by the *caput mortuum* for a pretty while in the air, they could not find it impregnated enough with new saline particles, to be fit to yield more menstruum, and be worth another distillation? I was answered in the affirmative, provided, that (as I mentioned in the state of the case) there were a competent time interposed between the former and the latter distillations. (The reason of which, according to my trials and conjectures, may be assigned of this odd phænomenon, belongs not to this place, but you will hereafter meet with it in another).

THE second instance I promised you, is afforded me by stones; for there are, and not far from this place, quarries of solid and useful stone, which is employed about some stately buildings I have seen, and which yet is of such a nature, wherein divers other sorts of stone are said to resemble it, that though, being digged at a certain season of the year, it proves good and durable, as in those structures newly mentioned; yet employed at a wrong time it makes but ruinous buildings, as even the chief of those persons whose profession makes him more conversant with it, has himself acknowledged to me to have been found by sad experience. But concerning this observation, you may expect to meet elsewhere with a farther account.

AND though time and place be two of the principal, yet they are not the only circumstances whose variations may make some such attributes discovered in natural things, as are not usually heeded; of which I shall mention but a couple of instances, because they may serve to shew you that such circumstances as are thought the slightest, may afford new uses even of solid and lasting bodies. Skilful artificers that grind optical glasses for tubes, have complained to me, that oftentimes the convex glasses they fashion will prove veiny, and consequently, after all their labour, of little value; and yet they are not able to discover these unwelcome veins in the glass, by the most careful viewing it against the light, till they have spent a pretty deal of time about working of it; and even then they are unable to descry these blemishes, if they hold the glass at an ordinary distance from the eye; but they are obliged to remove it a great way (perhaps six or seven feet) farther, so much may an increase of distance become serviceable even where one would expect the quite contrary.

BUT probably you will look upon posture as a slighter circumstance than distance itself, and yet Dr. *Gilbert* has observed, and I have found it true by many trials, that long irons, as the bars of windows, that have stood upright for a great while, do, by that perpendicular posture, acquire a verticity or magnetick virtue, as having acquired magnetick poles. So that if you apply the needle of a dial, which I mention as the readiest way of trial, to the lower part of the bar, it will draw the south end of the needle; whereas the upper extreme of the bar will seem to drive away that end, and will draw the northern.

BUT here I must not forget to take notice, that I can scarce think men will be able to know all the properties and uses, even of familiar bodies and other things, till they have mathematically considered them; there being several attributes belonging even to such things, which a naturalist, though curious, will probably never find out, unless he be both acquainted with mathematical disciplines, and have the curiosity to apply them to physical subjects. And though in other essays of this book, divers things are delivered that do directly enough tend to manifest what I have now said; yet it is of such importance, that naturalists should be thoroughly persuaded of a truth, that may be so much more useful than it is yet generally admitted, that I am content to inculcate it, by setting down here a few inſtances of ſomewhat a differing ſort from thoſe elſewhere delivered, and more appropriated to the preſent ſubject of our diſcourſe.

You will not doubt, but that ever ſince the firſt ages of the world, the majority of men have had ſome occaſion or other to ſee bodies ſwing; and yet, till *Galileo* (for he is generally believed the diſcoverer) took notice of the vibrations with a mathematical eye, men knew not this property of ſwinging bodies, that the greater and ſmaller arches were, as to ſenſe, equitemporaneous; from which diſcovery have been derived ſeveral practices of good uſe, ſome of which have been already mentioned in theſe eſſays.

That water, running out at a hole made in the ſides near the bottom of the veſſel, makes a parabolical line, or one that near reſembles it, and that in ſuch effluxions of water, there is a determinate proportion aſſignable betwixt the perpendicular height of the liquor, and the diameter of the hole, whereby the velocity and quantity of water that would run out may be computed, has not been, that I know of, taken notice of, till the obſervations of the above-named *Galileo*, and the diligent *Merſennus* (to which we may elſewhere add ſome of our own) have endeavoured to define thoſe matters.

As conſtantly as we have occaſion to take notice of the air, and water, and glaſs, yet the curioſity of our modern maſters of opticks has obſerved many things touching the refraction of the beams of light made in thoſe mediums in different quantities, and to and from the perpendicular, not to ſay any thing of the equality of the angles of incidence, and of reflection made on the ſurface of ſtill water, unheeded by thoſe that are not verſed in opticks: the drops of dew that hang in numberleſs multitudes upon the graſs and leaves, are things that every eye has been invited to take notice of by the orient colours the ſun is wont to make them afford us; but till the excellent *Des Cartes*, contemplating them with a more critical eye, found, that in ſuch a determinate angle made at the ſpectator's eye, between the ray of light coming from a certain part of the drop, and the imaginary ſtraight line reaching from the eye to the ſun's center, the drop appeared red, and in another determinate angle exhibited yellow, blue, and other colours, and at other angles, ſhewed no colour at all; the world ignored a conſiderable property of ſpherical diaphanums irradiated by the ſun, and ſeems not to have dreamt of a neat hypotheſis, with which ſome ingenious mens minds are no leſs taken, than their eyes are with thoſe vivid colours of the rainbow, which it pretends to give a clear account of. And though we daily ſee pieces of wood and timber broken by the weight of over heavy bodies, yet till the often named, and ſtill to be commended *Galileo* applied geometry, and the doctrine of proportions to matters of this kind, the reſiſtance of ſolid bodies to be broken by weight (whether their own or that of other bodies) ſeems not to have been ſo much as ſuſpected to be reducible to ſuch an eſtimate, as he and others have brought it to. And a virtuoſo of my acquaintance (for whom *Merſennus* laid the way) in a muſical inſtrument that I have with pleaſure heard him play on, can obſerve a property of metals that chymiſts thought not of, namely, that equal wire-ſtrings made of differing metals, and having a due tenſion, will yield ſounds differing as to ſharpneſs by determinate muſical notes, or the diviſions of them. And to theſe I might add divers other remarks of *Merſennus* and *Galileo* about the force of guns (which were found to increaſe with their length but till ſuch a number of feet, beyond which the length did but leſſen it) and the parabolical line, in which bullets (that are thought of all other bodies to move the ſtraighteſt) are ſaid to move; and I know not how many other mathematical attributes, if I may ſo call them, of natural things, that geometricians, aſtronomers, engineers, &c. have already obſerved, might be here added, but that I think it ſufficient to ſubjoin one inſtance more that may well ſerve to keep us from imagining, that even the moſt familiar objects in the world, and that ſeem likely to afford the leaſt diſcoveries, have been ſufficiently conſidered,

Meteorum, cap. VIII.

dered. For how few phænomena in nature are there that occur to us more frequently than the falling of heavy bodies? And yet, though the ancients and *Ariſtotle* himſelf took notice that there was an acceleration of deſcent in falling bodies, we find not that any ſo much as fairly attempted to determine that acquired velocity, till *Galileo*'s obſervations reduced it to the proportion mentioned in ſome of the former eſſays, wherein moſt of the following mathematicians (for I have ſcarce met with two diſſenters) have acquieſced; and whereby in the eighth eſſay we endeavoured to meaſure heights and depths without geometrical inſtruments. In a word, till geometry, mechanicks opticks, and the like diſciplines be more generally and ſkilfully applied to phyſical things, I cannot think otherwiſe, than that many of the attributes and applications of them will remain unknown; there being doubtleſs many properties and uſes of natural things that are not like to be obſerved by thoſe men, though otherwiſe never ſo learned, that are ſtrangers to the mathematicks.

And as I have hitherto obſerved of bodies, ſo I ſhall venture to add of qualities and divers other natural things, that even thoſe that are very familiar may have attributes and uſes, which the generality of men, without excepting thoſe that are otherwiſe learned, are not wont to take any notice of.

That black bodies, for inſtance, as ſuch, are much more ſtrongly and eaſily warmed by the ſun-beams than white ones, nay, though the diſparity be not ſo great, than bodies of any light colour, *cæteris paribus*, is perhaps more than even you have taken notice of; and yet I ſhall hereafter have occaſion to prove it by divers inſtances, and you may eaſily try it, either by expoſing for ſome time to the ſummer-ſun a white glove and a black, or a couple of eggs, whereof one is inked, or otherwiſe blacked all over.

Cold is one of the moſt familiar qualities men have to deal with; and though they otherwiſe are not wont to expect much from it, yet leaſt of all would they expect that it ſhould, contrary to the received definition of it, which is, *congregare tam heterogenea quàm homogenea*, that it ſhould, I ſay, perform the office of heat in ſpirit of wine, nay, and in preſenting us ardent ſpirits from beer and other liquors inferior to wine; and yet, not to mention *Paracelſus*'s proceſs of making the eſſence of wine by freezing all the phlegm, we have the repeated experiments of navigators into the frigid zone, who aſſure us, that not only from wine, but from beer, by the congelation of the aqueous parts, there may be ſeparated or obtained a liquor, ſtrong, hot, and ſpirituous, almoſt like aqua vitæ.

And even in our temperate climate ſome odd ſeparations may be made by cold; for, not to anticipate thoſe trials of mine that belong to other papers, there may, by ſuch cold as we have here, be made a ſeparation in oil, of a liquor much finer and more ſpirituous than the reſt; for I know an eminent artificer who kept it as a choice ſecret to reſort (as himſelf confeſſed to me he did) in hard froſts to the great jars of oil, where he often found greater or leſſer cracks or chinks in the congealed part of the oil, in which crannies was contained an unfrozen liquor that appeared thinner and finer than common oil, and was much better than it to preſerve things from ruſting, as perhaps having left many of its ſaline parts in the concreted oil; and for that purpoſe was much prized, not only by him, but by ſome watchmakers that were made acquainted with the virtue of it.

But it were tedious to inſiſt on all the inſtances that may be brought of the applications that may be made of colour, ſound, levity, ſpringineſs, fermentation, and even putrefaction; and it would be not only tedious, but almoſt endleſs to proſecute thoſe inſtances that might be afforded by other more general and operative ſtates and faculties of bodies. For not only motion and reſt, fluidity and firmneſs, gravity, and the like, have-

have a more univerſal influence of natural things, than even philoſophers are wont to take notice of; but thoſe leſs catholick affections of matter, that are reckoned among but particular qualities, ſuch as gravity and heat, may have ſo diffuſed an influence, and be applicable to ſo many differing purpoſes, that I doubt, whether all the uſes of that particular degree or pitch of heat that reigns in fire, will have all its uſes diſcovered, before the laſt great fire ſhall diſſolve the frame of nature.

Nor muſt I pretermit one conſideration more, that belongs to my preſent ſubject, which is, that probably many more qualities or other attributes would be taken notice of, even in thoſe natural things, that are reckoned among the moſt known, if men did not want a meaſure of curioſity that might juſtly be expected. For I ſpeak not here of curioſity in general (which I doubt not would make far more numerous diſcoveries than were neceſſary to juſtify my preſent diſcourſe) but I only ſpeak of ſuch a curioſity about the things of nature we familiarly converſe with, as we could ſcarce want, if it were not out of lazineſs, or a prejudicate opinion, that makes us take that for granted, that we ſhould find to be quite otherwiſe, if we did not chooſe rather to preſume than to try.

Thus, that falling bodies, the heavier they are, the faſter in proportion they fall, has been a received opinion in the ſchools ſince *Ariſtotle*'s time, and has kept the equivelocity, as to ſenſe at leaſt, of bodies of very differing bulks and weights falling from moderate heights, ſuch as ſurpaſs not ordinary towers and ſteeples, from being taken notice of, till of late inquiſitive men by experiments found it out.

That water by glaciation is reduced into a leſſer room, has been and is ſtill the opinion, not only of the vulgar, but of the generality of learned men; and yet, that water is not condenſed, but expanded by freezing, he that will congeal that liquor in veſſels ſtrong enough, may eaſily find by trial. And the floating of ice upon water, and the bubbles that are uſually to be obſerved in it, may alone ſuffice to make a conſidering man diſtruſt the vulgar opinion.

That the common air we breathe and live in, is a body endowed with poſitive levity, has been for many ages, and continues to be almoſt univerſally believed; and yet if men had the curioſity to examine this ſuppoſition by one or other of thoſe ſeveral ways, by which the gravity or levity of the air may be diſcovered, they would quickly find that is not devoid of weight. And even ſo ſlight a way as the condenſing the air in a blown bladder, by tying a ſtring ſomething ſtrong about the middle of it, may bear witneſs to what we ſay. For though we ſhould oppoſe, as ſome have lately done, that in ſuch caſes the air is not in its natural ſtate, but condenſed; beſides, that is an objection to which all the expedients of weighing air are no way liable, it makes rather againſt the objectors, than the concluſion, againſt which they urge it; ſince, if the particles of the air be really light, the filling the bladder the fuller of them ought to make it rather lighter than heavier.

That greater and leſſer ſounds do, as to ſenſe, move with an equal ſwiftneſs, is that whoſe contrary is taken for granted; and the more excuſably, becauſe it is evident and confeſſed that great and ſmall ſounds do not move equally far; and yet, that this equivelocity of ſounds has been made out by the late obſervations of the diligent *Merſennus*, and others, you may remember to have been delivered in a foregoing eſſay, where I alſo endeavoured to ſhew, that this property of ſounds is not unappliable to human uſes. Eſſay VIII.

That the loadſtone, which by immediate contact will take up iron, ſhould have ſo ſtrange a property, as to take up far more when a cap or conveniently ſhaped piece of ſteel is interpoſed betwixt it and the body to be raiſed, is a thing ſo unlikely, that though the ancients knew and much admired the attractive virtue of the loadſtone, yet they ſeemed not to have ſuſpected it enough to vouchſafe it a trial: and yet ſince *Gilbert*'s writings came abroad, he muſt be a great novice in magnetical affairs, that either ignores or doubts it. But I muſt not do any more than touch upon magnetical experiments, ſince they alone would afford me ſo many truths (which the generality of men would not have thought likely enough to be worth trying) that to enumerate them, though it might convince your underſtanding, would, I fear, exerciſe your patience.

That it is the property of unſlaked lime to grow hot by antiperiſtaſis, upon the pouring on of cold water, and other cold liquors, and conſequently not to grow hot upon the effuſion of liquors that are not cold, is not only generally believed, both by learned and unlearned; but this property of lime has been employed as an argument to prove other matters, as well by divers of the new philoſophers, as by many of them, that embrace the old Ariſtotelian principles: whereas I doubt not but a little trial might eaſily diſabuſe them: for by the affuſion of divers liquors actually warm, I have made lime ſlake with its wonted violence, if not with a greater. And in other liquors actually cold, like unheated water, and one or two of them far more thin or ſubtle than it, I have kept lime long without ſlaking, and without imparting to the ambient liquor any ſenſible heat. The quality of theſe inſtances makes me think it needleſs to increaſe their number, ſince we can ſcarce wiſh a greater inducement to expect, that many new attributes may be diſcovered in the works of nature, if men's curioſity were duly ſet on work to make trials, than that divers have been found out, that ſeemed ſo unlikely, that men thought it would be in vain to try them.

To theſe ſeveral ſorts of inſtances, that have hitherto been reduced to our firſt conſideration, might well be added, that bodies, which have the ſame denomination, and from whence men are therefore wont to expect the ſame, and but the ſame, operations and uſes, may yet have peculiar ones, and ſome of them very differing from thoſe of the generality of other bodies, that bear the ſame name. But examples of this kind will more conveniently be mentioned in the laſt eſſay: and leſt this ſhould ſwell too much, diſmiſſing this preſent conſideration, we will advance to the next.

SECTION II.

I consider in the ſecond place, that the faculties and qualities of things being (for the moſt part) but certain relations, either to another, as between a lock and a key; or to men, as the qualities of external things referred to our bodies, and eſpecially to the organs of ſenſe, when other things, whereto theſe may be related, are better known, many of theſe, with which we are now more acquainted, may appear to have uſeful qualities not yet taken notice of.

I shall elſewhere, *Pyrophilus*, have occaſion to ſhew you more fully on what grounds, as well as in what ſenſe it is, that I take the moſt of the qualities of natural bodies to be but relative things. To our preſent purpoſe it may ſuffice to adumbrate my meaning by the newly hinted example of a lock and a key, where, as that, which we conſider in a key, as the power or faculty of opening or ſhutting, ſuppoſes, and depends upon the lock, whereto it correſponds; ſo moſt of thoſe powers and other attributes, that we call qualities in bodies, depends ſo much upon the ſtructure or conſtitutions of other bodies, that are diſpoſed or indiſpoſed to be acted on by them, that if there were no ſuch objects in the world, thoſe qualities in the bodies, that are ſaid to be endowed with them, would be but aptitudes to work ſuch effects, in caſe convenient objects were not wanting. As if there were no lock in the world, a key would be but

but a piece of iron of ſuch a determinate ſize and ſhape. And this compariſon I the rather imploy, becauſe it may be further applied to our preſent diſcourſe. For as if ſome barbarous American ſhould, among other pieces of ſhipwreck thrown by the ſea upon the ſhore, light upon a key of a cabinet, he would probably look on it as a piece of iron, fit only for the inconſiderable uſes of any other piece of iron made much broader at each end than in the middle; but, having never ſeen a lock, would never dream, that this piece of iron had a faculty to ſecure, or give acceſs to, all that is contained in ſome well furniſhed cheſt or rich cabinet: ſo there is many a thing, that ſeems to us uſeleſs, whilſt we look upon it only in itſelf, which will perhaps hereafter prove highly uſeful, when we ſhall light upon ſome other bodies peculiarly fitted to act upon it, or receive impreſſions from it. But this will be better apprehended by the following inſtances.

Though iron be ſo common a body as it is, and its uſes are very many, and have been known as long as ſince *Adam*'s time, yet all thoſe differing bodies, on which men of all ſorts imployed it to work, and all thoſe various ways, whereby chymiſts, phyſicians, and mineraliſts have wrought on it, during ſome thouſands of years, did never diſcover to man one of its nobleſt and uſefulleſt properties, which, for aught we know, was never found out till within theſe three or four ages: for a ſteel needle, being applied to a loadſtone, manifeſted itſelf to be capable of conſtantly ſhewing the north and ſouth in all ſeas, in all weathers, and in all times of the day and night to navigators, who, by this property, which depends upon the relation that iron has to one only ſtone, have been enabled to diſcover the new world, and enrich the old with the drugs and treaſures of it.

After all the vain attempts that even ſubtle chymiſts have made to arreſt the fluidity of quickſilver, the knowingeſt perſons that have meddled with that mineral, and eſpecially if they have obſerved, that the keeneſt froſts, that are capable of freezing even aqua vitæ, are unable to congeal it, have been very much indiſpoſed to reckon an eaſy coagulableneſs amongſt its qualities; and yet we ſee, that though the mixture of no other known body will diſcloſe its having any ſuch affection, yet the vapour of melted lead will ſometimes (for that experiment will not always ſucceed) reduce quickſilver, even in its maſs, into a conſiſtent and ſomewhat tough and hard body.

Vinegar being a liquor that has been generally known and uſed for ſome thouſands of years, men have imployed it upon great variety of bodies, and to very many uſes, but eſpecially to communicate a ſourneſs to the things wherewith it was mingled; but when it came (probably by chance) to be applied to the diſſolving of lead calcined or crude, it manifeſted, that it had a faculty to exhibit a more than ſaccharine ſweetneſs, which, for aught I know, it exhibits with that metal only; for I have not yet known crude vinegar diſſolve tin, though calcined: and though by a ſlight artifice, elſewhere mentioned, we have been able to make ſtrong vinegar diſſolve the calx of Jupiter, yet was the ſolution far differing from, and inferior to, the taſte of the ſolution of lead newly mentioned.

Spirit of urine is a liquor, that has been long known to chymiſts, and might reaſonably be looked upon as likely to be a good menſtruum for ſeveral bodies: but it is not probable, that after it had been imployed to diſſolve divers compact bodies, it ſhould be ſuſpected, that it would coagulate ſo thin, light, and fugitive a body, as ſpirit of wine itſelf; and yet we have often (as there will be hereafter divers occaſions to relate) tried, that if both liquors be ſufficiently pure and dephlegmed, they will afford that ſtrange ſnow-white concretion, that *Helmont* calls his *Offa alba*; which, however by his followers aſcribed to him as the inventor, I find mentioned in ancienter books than his: and

and I remember, that even *Raymond Lully* relates, with what wonder he first saw this experiment (which indeed is considerable) performed.

And as the spirit of urine has such an odd property, when it meets with ardent spirits dephlegmed; so the spirituous parts of urine, without being separated from the rest, have a faculty, that one may yet less expect, if they be duly imployed, to operate upon musk: as I have had the opportunity to inform myself by inquiry of a scholar, who lived in *China*, and affirmed himself to have divers times seen musk made. For this person answered me, that he had observed it to be the practice of others, and had made trial of it himself in those eastern parts, that the musk being made up, and put into cods or bags made of the skin of the same animal, (in which form I have received presents of musk sent me from the *Indies*) they do, either before or after, hang it in a house of office, so as it may, without touching the grosser bodies, receive the fœtid exhalations of that nasty place; by which urinous steams, which it is exposed to for some days, the less active, or more immersed scent is, as it were, called out, and excited or heightened. And I found, by farther enquiry of the same person, that having carried musk from those eastern regions, where it is made, to other and remote parts of the same *Indies*, he found, that, by the length of the voyage by sea, his musk had very much lost its strength, which he afterwards restored to it, by following the advice of some skilful persons, according to which he tied the musk close in a bladder, wherein, having pricked many little holes with a needle, he hung it up for some days in such stinking place as has been newly mentioned. Whereto agrees very well what I have read in a late eminent physician of *Rome*, (where the art of perfuming is very much cultivated) who communicates it as the chief secret practised by the perfumers there, for recovering the scent of decayed musk, that it be kept for a competent time in linen well moistened with rank urine.

The uses of gesso (as the Spaniards and Italians call it,) or gypsum, are numerous enough in the shops of stone-cutters, moulders in plaister or wax, and divers other artificers; but one would scarce suspect, that, besides the various uses these tradesmen put it to, it should have one so very differing from them, as to be an excellent medicine, if I may so call it, for wine: and yet, that they use great store of it about those choice ones, that come to us from the Canaries, is a noted tradition among those that deal in that sort of liquor, and has been confirmed to me by an eminent wine-merchant, that lived several years in those islands. And, that about *Malaga* they put up a good proportion of it into the juice of their grapes, when they tun it up, is affirmed to me by a curious eye-witness, who was there in vintage-time, and of whom I purposely enquired about it.

Though silver be so noble a metal, and so much known and used, that it was the price of things as early as *Abraham*'s time, yet one very fine use of it has been known but since the art of annealing upon glass came to be practised. For among other experiments of this art we find, that prepared silver (and I have sometimes done it pretty well with the crude metal) being as it were burned upon a plate of glass, will tinge it with a fine yellow or golden colour: there are also divers mineral earths, and other coarse fossils, of use in this art, which, by the help of the fire, makes them impart colours to glass, both transparent, and sometimes very differing from those of the bodies themselves, as I may elsewhere have occasion to specify. In the mean time, give me leave to name this reflection upon the art of painting, that it is very hard for us to be sure, that we know so much, as all the several sorts of uses that may be made of the particular bodies we converse with, since upon the invention of a new art or trade, of which we know not how many remain yet to be found out, divers uſes and applications of bodies come to be diſcloſed, that were never ſuſpected before.

THE uſe of lyes made with common aſhes to waſh linen has rendered them for theſe many ages very familiar: but though their effects on the other bodies, upon which they have been employed, ſeemed not to have any affinity with what I am going to mention; yet when a ſtrong lixivium is applied to ſyrup of violets, (which is alſo a very known liquor) to which it has a peculiar relation, it will then immediately change the colour of that ſyrup from a blue to a perfect green, and ſo it will the violet leaves cruſhed on a piece of white paper, without the help of ſugar, or any preparation.

REDNESS, though a colour as obvious as moſt others, and to the generality of men very pleaſing, however it hath no offenſive property, in reference to other animals, familiarly known amongſt us, (at leaſt, that we have taken notice of;) yet being preſented to the eyes of turkey-cocks, it has ſuch an incongruity with them, that oftentimes it is obſerved to make them very angry, as far as can be judged by the tokens of being diſpleaſed it produces in them.

THE leaves of oaks, that are ſuch common things, and are not obſerved to have, in reference to any other body, which chance or induſtry applies them to, any ſuch property as that I am about to name; theſe leaves, I ſay, if when freſh, they be immerſed in the water of mineral ſprings, impregnated with the ſubtle corpuſcles of iron, I have ſeveral times found to turn the liquor blue or black, according to the proportion and vigor of the two ingredients.

ONE would not expect, that ſo dark and black a body as charcoal ſhould be the main thing employed, not only to cleanſe and brighten ſome metals, but to procure a clearneſs, and give a gloſs to ſome tranſparent bodies. And yet I learned from the makers of mathematical inſtruments, gravers, and other artificers, that the beſt way they have, and which I have ſeen them employ, to poliſh their plates of braſs and copper, (after they have been rubbed clean with powdered pumice-ſtone) is with charcoal, (which ſome of the more curious burn a ſecond time, and quench in appropriated liquors,) as that, which both ſerves to fetch out the ſcratches of the pumice-ſtone, and itſelf ſcours without ſcratching, and thereby poliſhes very ſmoothly. And by the ſame way they may cleanſe and poliſh the plates of horn, of which they make lanthorns, drinking-cups, &c. To which, as to the metalline plates, a gloſs may be afterwards given with tripoli.

PERHAPS it will not be improper to take notice to you, *Pyrophilus*, in this place, that not only the nature of the body to be wrought upon, but ſome peculiar circumſtances relating to it, may contribute to the effects of ſuch experiments, as thoſe treated of in this ſection. As for example, one would not expect that water, which is ſo apt to run out at the chinks of wooden veſſels, ſhould, without addition, become the fitteſt inſtrument for cloſing them. And yet I have more than once found by trial, as I preſume many tradeſmen have done, that when wooden barrels or firkins, and the like veſſels, by having been long kept too dry, come to have clefts and commiſſures, this inconvenience may be remedied by pouring water into them. For though at firſt the liquor quickly runs out again, yet by frequent affuſions of it, the wood, eſpecially thoſe edges between which the water runs out, becomes ſo ſoftened and plumped up, that the little intervals or chinks are, by the ſwelling of the neighbouring parts, cloſed up, and the veſſel becomes ſtanch.

AND upon a like reaſon ſeems to depend that odd experiment, much talked of by ſome of our eminent Engliſh ſeamen, who, for the haſty ſtopping of a leak that is not too great, much commend the thruſting into it a piece of powered beef; for this being much

much more falt than the fea-water, that liquor pierces into the compact and (in great part) dry body, and by opening the falts, and foaking into the flefh, makes the fwelling beef expand itfelf, fo as to bear ftrongly againft the edges of the broken plan s, and thereby hinders the water from flowing into the fhip as it did before.

SECTION III.

I CONSIDER in the next place, that a body in affociation with others may be made fit for new ufes, and fome of them quite differing from thofe, that were proper to it before.

THIS third confideration is, in fome regards, of affinity with the firft, but yet is not the fame, fince in the former we confider the power, that one body has to act upon another, or the difpofition it hath to be acted upon by it; whereas now we confider the two bodies or more, as being by conjunction qualified to act on a third body, or fuffer from it, as one entire concrete, upon the account of new and emergent properties, accruing to the compound by the affociation of the more fimple bodies, that compofe it.

YOU will meet with ftore of inftances, both in thefe effays, and other of my writings, eafily applicable to the illuftration of what is here delivered, and therefore it will fuffice to name in this place the fewer.

HE that takes notice, how flexible a metal tin is, and how dead a noife it yields, will fcarce dream, that one of its ufes, and that none of the defpicableft, fhould be, to make another metal, which is lefs yielding, and has a lefs dead found than itfelf, not only hard, but fonorous: and yet we fee, that bell-metal, which, when caft into bells, makes a hard mixture, that founds fo loudly, is made principally, as has been already noted, by the addition of a certain proportion of tin to copper.

IN the common experiment of making ink, the infufion or decoction of galls is yellowifh, or reddifh, and the folution of vitriol will, as the concrete participates more of iron or of copper, be either green or blueifh; but from the mixture of thefe two liquors there will emerge an inky blacknefs.

THAT oil, that is a body fo mollifying and flippery, and whofe unctuofity make its moifture fo much more difficult to be wafted or deftroyed, than that of water, wine, or other not tenacious liquors, fhould be one of the two or three main ingredients, and the only moift one of a hard and durable cement, is that, which probably you would very little expect from it: and yet, not to mention what trials of that nature I have made, becaufe I had not time to obferve the full event, a very ingenious man, much employed about coftly water-works and dams, affures me, that the beft way he has to join together, and, if need be, piece and mend with a clofe and lafting cement the pipes that are ufed for fubterraneal aqueducts, that are long to hold running water, is to take good clay (fuch as tobacco-pipes are made of,) and having dried it, and reduced it to a very fine powder, and mixed good ftore of fhort flocks with it, beat it up very diligently with as much linfeed-oil, as will ferve to bring it to a ftiff pafte, almoft like well kneaded dough. This pafte he fafhions into pipes of the length and bignefs required, which, though they will be long a drying in the air, yet, when once throroughly dry, are very ftanch and lafting. And I remember, that before I learned this, having occafion to try divers experiments about cements, I chanced to meet with an ancient artificer, employed to keep in repair the conduits that brought water to *London*, and in exchange of a lute or cement that I taught him, he was forward to satisfy the curiosity I had to know what cement he employed about so impo-tant a work, and he assured me, that oil was one of the main ingredients (and the only liquid one) he employed.

He that considers, that lead is one of the most opacous and flexible bodies, that the world affords, will not easily imagine, that one of its uses should be to make up about three parts of four of a mixture transparent, and exceeding brittle; and yet this is easily performed by divers chymists (and I elsewhere mention my having often done it) in making of calcined lead, and powdered flints or sand, a brittle and diaphanous composition, called by Spagyrists Vitrum Saturni.

And this mention of glass suggests to me another instance, fit for my present purpose: for who would imagine, that such a body as the fixed salt of kaly, which, as other alkalies, that take their denomination from it, has a strong and fiery taste, and is not only readily dissoluble in water, wine, or any such liquor, but will in a short time, being but left in the air, be reduced into a liquor; who would expect, I say, that it should be of any use, much less the main of this caustick, and easily dissoluble body, to be one of the two main ingredients of a substance both perfectly insipid, and indissoluble, not only in water, wine, &c. but even in aqua fortis, aqua regia, spirit of wine, quicksilver, spirit of urine, and other menstruums, some of them highly corrosive, and others extremely subtle and piercing; and yet such a mixture is usually afforded us in glass (especially the more durable sort of it) wherein that there is actually a great proportion of alkalizate salt, I confess, I doubted, till having purposely enquired of an ingenious master of a glass-house, how much glass he usually obtained, when he put in such a quantity of sand, I found by his answer, that the glass obtained was many pounds in the hundred more than the sand that was employed to make it: whence I gathered, (what he also affirmed) that the alkaly did not only seem (as one might suspect) to promote the fusion of the sand, but does materially and plentifully concur with it to compose the glass.

And whereas I intimated at the very beginning of this third section of this essay, that bodies when associated, may be applied, not only to new uses, but perhaps to some that are quite differing from those, that belong to some of the respective ingredients; this observation may be made good by several instances, and even by some that are very obvious, as well as by others that are not so familiar. For we may take notice, that though oil, and tallow, and other such unctuous bodies, be those, that do grease and spot linen and woollen clothes; yet those very bodies, being skilfully associated with others, though with but a lixiviate salt and fair water, do plentifully concur to the making up of soap, by the solution of which grease is readily washed out of linen cloths, and others, besides those, are also freed from the spots of it. But divers others instances applicable to this purpose belonging more properly to the following part of this essay, till we come thither, it may suffice, that I illustrate and confirm what hath been proposed by the single, but noble instance of Aurum fulminans. For though salt of tartar be a fixed body, and of a fixing quality, yet being skilfully associated with gold, dissolved in aqua regia, though that be thought the fixedest, not only of metals, but of bodies; yet the gold precipitated by this fixed and incombustible salt becomes so exceeding fugitive, that by a gentler heat than would kindle any known body in the world, it is made to fulminate like gun-powder, (but many degrees more violent than it;) and, which you will also think strange, though sulphur be a body of so quick accension as is obviously known, yet by an easy way, elsewhere to be taught you, of mixing those two only, you may, as trial hath informed us, make it (which you will easily allow to be one of the unlikeliest uses of sulphur) even by its being set on fire, to hinder

hinder the accenſion of this ſo eaſily kindled gold, which I have known thereby readily turned into a medicine, that ſome cry up for excellently diaphoretick, (though I doubt whether Aurum fulminans work not rather another way,) and which I remember I have, in a crucible, kept long in the fire without loſs.

I SHALL only add to this third conſideration this one particular, that is of too great moment to be pretermitted here, though it have been already in part taken notice of on another occaſion; namely, that the effects and uſes of mixtures do not only depend upon the nature of the ingredients, but may be oftentimes much varied by their proportion. And of this the mineral, which at the glaſs-houſes they are well acquainted with, under the name of manganez, will afford us a pertinent and conſiderable inſtance. For though it be a coarſe and dark mineral itſelf, and though being added to the materials of glaſs in a fuller proportion, it make the black glaſſes, that are ſold in ſhops; yet not only a moderate proportion of it is uſed to make glaſs red, but, which is more remarkable, a ſmall and due proportion of it is commonly employed to make glaſs the more clear and diaphanous.

SECTION IV.

IN the fourth place I conſider, that a body, by a differing preparation of management, may be fit for new, and perhaps unthought of purpoſes. For the qualities of bodies depending for the moſt part upon the texture of the ſmall parts they are made up of, thoſe ways of ordering greater bodies, which do either by addition, detraction, or tranſpoſition of their component corpuſcles, or by any two, or all of thoſe ways, make any notable change of the former texture of the body, may introduce new qualities, and thereby make it fit for divers uſes, for which it was not proper before.

WE ſee to how many ſeveral uſes men, that were neither philoſophers nor chymiſts, but for the moſt part illiterate tradeſmen, have been able to put iron, by but varying the viſible ſhape of certain portions of it, and connecting ſome of them after a peculiar manner; as is obvious in the ſhops of blackſmiths, lockſmiths, gunſmiths, cutlers, clockmakers, ironmongers, and others. But to give you a more phyſical inſtance in the ſame metal, be pleaſed to take notice, how much a change, made by a natural agent, the fire, in the inviſible texture of iron, does ſpeedily alter it; when of the ſame bar of iron, by the help of fire and water, the artificer makes hardened iron, and iron of a temper fit for drills, and knives, and ſprings, and I know not how many other inſtruments, which require diſtinct tempers in the metal they are made of; that temper, which renders them fit for one uſe, leaving them unfit for another.

BUT we need not confine ourſelves to inſtances, wherein no new ingredient is added to, or taken from the body to be altered; it being ſufficient, that the additament upon its own account do not bear ſo great a ſtroke in the change produced, but that it be principally aſcribed to the way of ordering the body wrought upon; and ſpeaking of the management of a body in this ſenſe, (which is uſual and proper enough,) I ſhall ſubjoin a few inſtances, of the many I might add, to make good our propoſition.

THOUGH paper be one of the commoneſt bodies that we uſe, yet there are very few, that imagine it is fit to be employed otherways than about writing, or printing, or wrapping up of other things, or about ſome ſuch obvious piece of ſervice, without dreaming, that frames for pictures, and divers fine pieces of emboſſed work, with other curious moveables, may, as trial has informed us, be made of it, after this or the like manner. Firſt, ſoak a convenient quantity of whitiſh paper, that is not fine, about two or three days in water, till it be very soft; then mash it in hot water, and beat or work it in large mortars or troughs, (much after the manner used in some places to churn butter) till it be brought to a kind of thin pap, which must be laid on a sieve, without pressure, to drain away the superfluous moistness, and afterwards put into warm water, wherein a good quantity of fish, glew, or common size, has been dissolved. Being thence taken out by parcels with a spunge, it must therewith (for the spunge will dry up the superfluous moisture) be pressed into moulds of iron, or of such plaister as statuaries use, wherein having acquired the figure, which is intended to be given it, it is thence to be taken out, and permitted to dry, and is to be strengthened, where need requires, with plaister, or grated chalk, made into pap with water, or some other convenient matter; and afterwards, having first been leisurely dried, it is to be either painted or overlaid with foliated silver or gold, as the artist pleases. I may elsewhere have occasion to mention another unlikely use of paper, namely, to stop the clefts and commissures of wooden instruments and vessels that are to hold water. For paper being thrust into these narrow places, the first water, that comes to it, being soaked up, occasions a forcible dilatation, which makes the swelling paper fill the chinks it is lodged in, according to what was lately delivered at the close of the second section.

The sugar-cane has been a plant well enough known to many countries and ages, who were not unacquainted with the sweetness of its juice, and yet seem never to have made sugar of it, for want of knowing the way of so ordering it, as to coagulate into a durable, as well as delicious substance.

Tobacco was likewise a noted plant in the *West-Indies*, which was yet suffered yearly to rot and perish like other herbs, till the industry of the moderns finding the way of curing it, as they call the method of ordering it, made it, by the help of mere skill, last in an improved condition for divers years, and fit to be transported, as it plentifully is, over all the world.

The leaves likewise of indigo, which would uselesly perish like those of other shrubs, by the mere way of ordering them, which too is rather by subtraction than addition, have been long made a lasting pigment or dying stuff, and one of the most staple merchandises that even the *East-Indies* send us.

I might add, the great use that we are enabled to make of madder, woad, and divers other perishable plants, by the way of ordering them; but there is one instance of this kind so considerable, that though I have formerly named it to another purpose, and though I am willing to mention but one example more of this sort, I cannot but pitch upon this; since it excellently manifests, what may be expected from a skilful ordering of nature's productions, by shewing us, what even the savages of *America* have been able to perform in this kind. For though their mandioca be confessedly a poisonous plant, yet without addition they make of it their cassavi-meal, whereof not only the Indians, but also many Europeans make their bread, which I also have made some use of without dislike. And with no addition, unless it be perhaps that of spittle, they make of the poisonous juice of the same root a not unpleasant nor strengthless drink, which divers, even of the English, compare with our beer. And of the bread made of that cassavi-root, they brew, in some of our American colonies, a liquor by the planters called Perino, which I have known, even by persons of quality, equalled, if not preferred to wine itself.

The shreds of leather pared away and thrown aside by the glovers, by so slight a way of ordering them, as only the boiling them long in fair water, dissolves them in that liquor, and reduces them with it, the decoction being strained and cooled, into a kind of jelly, that they call size (which may be also made the same way of cuttings of parchment,

ment, and better yet with thofe of vellom) which is of great ufe towards the production of very differing trades: fome of which productions are already touched upon in this book, to which I fhall here only add, for the eafinefs of the experiment, that the fine red ftands, and hanging-fhelves, are made with ground vermilion, being only tempered up with it, and laid upon wood, which being thus coloured, is, when it is dry, laid over with common varnifh, which preferves it from wet, and gives it a glofs.

It would fcarce be fufpected, that fo white a body as ivory fhould, among other ufes, be proper, without the addition of any black, or fo much as dark-coloured body, to yield one of the deepeft blacks that has been hitherto known; and yet many of our eminent painters count that black, which they call ivory-black, the perfecteft that has been hitherto employed in their art. And this fable may be made of ivory, without addition, only by burning it a-while in a clofe pot; and we have made it by keeping it a-while among coals and afhes, only wrapped in ftore of wet paper to keep it from fpending its denigrating fulphur in an actual flame; (to prevent which, the pots it is burnt in, are wont to be clofed with lute, or otherwife fufficiently ftopped) as if artificers were acquainted with the old rule, *aduſta nigra, peruſta alba.*

And on this occafion I fhall add, that this black made of ivory is fo excellent in its kind, that I fcarce know any thing fo proper to make foils of, for that nobleft fort of gems, diamonds. And I remember, that a very fkilful jeweller, of whom I bought fome of thofe ftones, and whom I employed to fet others for me, confeffed to me, that burnt ivory was the thing he made ufe of for foils to the diamonds he had a mind to fet well.

Another inftance there is, which I muft by no means pretermit, now that I am endeavouring to fhew what the preparation or management of a body, even by illiterate tradefmen, may do to make it fit for unlikely ufes. For one would fcarce imagine, that from fo grofs and foul a body, as the *inteftinum rectum* of an ox or cow, there fhould be obtained a tranfparent fubftance, more thin by far than paper; and yet of fo great a firmnefs and toughnefs, as is fcarce at all credible to thofe, that have not been, as I have, convinced of it by experience. But it is certain, that fome of our gold-beaters in *London*, and perhaps not there only, do, by cleanfing and otherwife preparing the abovementioned nafty gut of an ox, obtain exceeding fine membranes, fome of which I keep by me, that though clear and ftrangely thin, are yet of fuch tenacity, that when the thin plates of gold are put between them, or in their folds, the force of a man frequently ftriking them, with a vaft hammer made of purpofe, almoft as heavy as he can well lift up, does ufually, as I have feen with fome wonder, attenuate and dilate the included gold, without being able to break thefe fo fine fkins.

These inftances, *Pyrophilus*, we have hitherto produced, are almoft all of them fuch, as either nature herfelf, or nature affifted but by tradefmen, and other illiterate perfons, has prefented us. And therefore queftionlefs, the power that a fkilful management may have to produce great changes in bodies, and thereby fit them for new ufes, will be much advanced, when they fhall be ordered by fuch, as are either good chymifts, or dexterous at mechanical and mathematico-mechanical contrivances, efpecially, if in the fame perfons a fkill in thefe two forts of knowledge fhould concur.

That fkill in mathematicks may teach a man fo to manage natural things, as to enable him to make other ufes of them, than thofe that want it will dream of, we may be perfuaded by feveral particulars. For we fee, that from a bare giving to a piece of ordinary glafs a prifmatical fhape, that diaphanous and colourlefs body may be made to exhibit in a moment all thofe delightful and vivid colours, for which we admire the rainbow; and though merely by giving a piece of foliated glafs or metalline fpeculum

a concave

a concave figure, it may be made to burn ſtrongly by reflection, yet by giving a piece of glaſs a convex figure, you may qualify it to burn by refraction, and even with water fitly figured, you may readily kindle fire. For though a round and hollow ſpherical phial of pure glaſs will tranſmit the ſun-beams without making them burn, and conſequently has not of itſelf the faculty I am going to name, but ſerves chiefly to terminate the water, that is to be poured into it, and give it its due figuration; yet by filling a ſpherical phial, I have taken pleaſure ſo to unite the ſun-beams, as, when froſt and ſnow was about me, to make them burn; (and perhaps ice itſelf, if choſen free from bubbles, and conveniently ſhaped, may, as ſome incomplete trials make me hope, be made fit enough for that purpoſe.) And much more vigorous the accenſion would be, if two bare concave glaſſes of like ſhape, equal bigneſs, and truly ground, had their edges ſo joined by a cloſe frame, that the cavity contained between the inſide of the glaſſes and the frame may be filled with fair water; for by this means (the convex ſide of each glaſs being outermoſt) the whole inſtrument (one or two of which I have ſeen in a virtuoſo's hands) will ſerve for a double convex glaſs, which may by this means be made far larger, and more efficacious, than other burning glaſſes of that figure, which conſiſting each of them of a ſingle piece of ſolid glaſs, are wont to be far inferior in bigneſs to ſuch hollow ones, as may be eaſily enough attained.

And now I have named ſolid glaſs, give me leave to take hence a riſe to add, that though glaſs ſtopples are made only by giving them an almoſt conical figure, and a ſuperficies fitted by grinding, for an exquiſite contact with the inſide of the neck of a glaſs-bottle; yet this way of ordering glaſſes, which is aſcribed not to mere philoſophers, but men verſed in optical and mechanical trades, produces ſtopples much ſurpaſſing all known before; not only in this, that neither aqua fortis, nor other corroſive liquors, work upon them, but alſo in their being able to keep in even the ſubtileſt ſpirits ſo ſtrictly, that I remember having once forgot ſome ſpirit of ſal armoniack in a large bottle, which it did not near a quarter fill, when I long after (as I remember about ſeven years) came to that part of *England* again, I chanced to find this bottle in a place, where, being without an inſcription, I knew not what the contained liquor was; and taking off the glaſs-ſtopple, to diſcover by the ſcent what it might be, upon ſmelling to that ſolid body, the adherent ſpirits operated ſtrongly enough upon my noſe and eyes to make me almoſt ſtagger, and wiſh my curioſity had been more cautious.

What I have further obſerved about the way of making, and the applications of this kind of glaſſes, belongs not to this place, where it would be fit to proſecute my former diſcourſe, by ſhewing you, how much the chymical management of things may alter and improve them, were it not, that it would be improper to venture upon ſo copious a ſubject in one of the ſections of an eſſay, where I ſhall therefore but point at it, without pretending to treat of it.

We ſee that chymiſts can out of ſome fruits, that grow wild in the hedges, and are not edible, as alſo out of the lees of ale and beer, draw an inflammable ſpirit which, for many purpoſes (not medicinal) may be made uſe of for that of wine. We ſee that out of the dry body of hartſhorn, as likewiſe out of the ſkull and bones of dead men, and other animals, which have been wont to be looked upon to be ſo devoid of moiſture, that men proverbially ſay, as dry as a bone, chymiſts do ordinarily, to the wonder of the ignorant, draw ſtore of ſpirit, and oil, and phlegm, as they likewiſe do from the drieſt woods. Some of them alſo, of the opacous body of lead mixed with ſand, and a few grains perhaps of metalline pigment, can make in a few hours variety of amauſes, or metalline ſtones, which, by their tranſparency and lovely colours, do pleaſingly

pleasingly emulate rubies, emeralds, and other native gems; about the emitation of which, I may elsewhere acquaint you with some of my trials.

How unlikely effects may be sometimes produced by a slight spagyrical preparation of things, may sufficiently appear by the Bolonian stone, from which (though one would not, upon the sight of it, expect any such matter, yet) being duly prepared by chymical calcination, it acquires that strange property of shining in the dark a while after it has been exposed to the sun, for which it is so justly admired by us, that have seen it, that it is judged unfit to be believed by many criticks, that have not.

And here let me take notice to you, *Pyrophilus*, that very slight circumstances in the management of a body may sometimes produce considerable and unlikely effects.

That salt, dissolved in water, is a powerful hinderer of the congelation of that liquor, is a matter of common observation; neither the sea-water, nor brine, being usually frozen with us by such frosts, as turn common water, and some liquors more indisposed than that is, into ice. And yet sea-salt, which being dissolved in water, keeps it from freezing, being outwardly applied to water, does so powerfully concur with snow or ice to make it freeze in artificial glaciations, and is so necessary to the effect, that the snow or ice, without the salt, would not ordinarily, here in our climate, produce in a seasonable time any ice at all, as I more than once purposely tried.

There is a certain powder, which by the proportion and mixture of nitre (whereof it chiefly consists) with other ingredients, obtains so odd a texture, that if putting it into a crucible, you should place that upon the coals, as is usually done in other fluxes, the powder would blow up, or take fire with violence enough, and perhaps not without danger; and yet, if instead of kindling this powder from the bottom upwards, you kindle it from the top downwards, there will be no danger in it, but it will make a powerful flux for the reduction of metalline powders mixed with it into a body.

SECTION V.

In the fifth and last place I consider, that the generality of effects to be performed, being not produced by one single and unassisted production, either of nature or of art, but requiring the concurrence of more; he, that knows not the nature or properties of all the other bodies, wherewith that, on which the experiment proposed is actually, or may be usefully associated, or otherwise imployed, can hardly discern all the effects the experiment may possibly concur to produce. For, whereas many inventions or operations consist, as it were, of distinct actions; a body, that seems useless to the main and ultimate effect, may usefully concur to the performance of some intermediate or subordinate part of the operation (by being requisite to which, it may be of no use to the experiment considered in the gross, though not to each distinct part of it.)

Though spirit of wine will scarce (if at all) even in a very long time draw a red tincture out of the flowers of sulphur, yet, when they have been opened, by having been fluxed together with an equal weight of salt of tartar, we have found, that they will in a few minutes, and in a gentle heat, give, in thoroughly deflegmed spirit of wine, a tincture or solution as red as blood; which being freed from the superfluous menstruum, will afford us a balsam much finer than that vulgar one, which is wont to be made of the same flowers dissolved in oil of turpentine.

That such amalgams of gold and mercury, as goldsmiths are wont to gild silver with, cannot by ordinary ways be made to adhere either to iron or steel, is a thing so well known among gunsmiths, and such artificers as work upon iron, that when I inquired of ſeveral of them (as well Dutch as Engliſh) whether they could gild iron with water-gold (as they call that way of gilding) by the help of quickſilver, they judged it a thing not to be done: and yet I know a very ingenious tradeſman, who was able to perform it, but not (that we may apply this experiment to our preſent purpoſe) without the aſſiſtance of another body, which was to perform one part before the amalgam could perform the other. The artificer's way was to coat (if I may ſo ſpeak) the iron or ſteel to be gilt, with a coat of copper, to which purpoſe he uſed diſtilled liquors tempered with other ingredients, wherein the iron was to be immerſed with great warineſs and dexterity; for otherwiſe, not only the trial would not ſucceed, but oftentimes the iron would be ſpoiled. To obviate which inconveniencies, there occurred another way of caſing the iron with copper, namely, by diſſolving very good vitriol, that has copper in it (for it is not every vitriol, that is fit for the purpoſe) in warm water, till the liquor be ſatiated with vitriol, and immerſing ſeveral times into this ſolution the iron, firſt ſcoured till it be bright, and ſuffering it each time to dry of itſelf; for this immerſion being repeated often enough, there will precipitate upon the iron enough of the cupreous parts of the diſſolved vitriol, to fill all its ſuperficial pores with particles of copper. So that by this ſafe, cheap, and eaſy way, having, as it were, overlaid your iron with copper, you may afterwards gild it as copper with the above-mentioned amalgam, which will adhere to copper, not to iron.

But here we muſt not omit an obſervation very conſiderable to our preſent ſcope, namely, that though the ſeveral parts of an experiment or a proceſs may in moſt caſes, each of them be purely phyſical, or chymical, &c. yet in divers other caſes, it may far more uſefully be ſo ordered, that one part of it may be phyſical, (taking here that term in contradiſtinction to ſubordinate parts of learning) and ſeveral, or each of the reſt may belong to other arts, as one may be chymical, and another ſtatical, another mechanical, another hydroſtatical, &c. and by ſuch a concurrence of differing parts of knowledge to the ſame operation or production, I doubt not that many things may be performed, that have not yet been attempted, nor ſo much as thought of. For he that has ſkill but in one of theſe ſingle parts of learning, muſt needs have his attempts as well as his knowledge much ſtraitened, by confining himſelf to operate by ſuch means and inſtruments as are within the compaſs of his own art; which, aſſiſted by others, may bear a good part in the performance of divers conſiderable things, which it is by its ſelf very inſufficient to accompliſh.

Of this we may take notice of ſome inſtances in the productions, that art and nature have preſented us with already; for not only handicraft trades, as we have formerly noted, do many of them aſſiſt each other in their operations, but even thoſe arts, that are counted ingenious, have ſometimes need or uſe both of the ſervice of the more mechanical trades, and of mutual aſſiſtance among themſelves. The maſters of captoptricks know very well what would be the properties of ſpherical, cylindrical, and other ſpecula; but to procure ſuch ſpecula, you muſt have recourſe to the chymiſt, or the founder, whoſe part it is by artificial mixtures of metals and minerals, and by mechanical contrivances, to caſt bodies, that give a more ſincere and vivid reflection, than the ſingle metals would do, and to give them withal that curious poliſh, for which the metalliſts and chymiſts are beholden to ſmiths, ſtone-cutters, watch-makers, or other handicrafts men.

Another eminent example to the ſame purpoſe may be taken from the conſideration of organs uſed in churches. For to deviſe the rules of making them well, there is firſt requiſite no ſmall ſkill in the ſpeculative part or theory of muſick: next he that would make the inſtrument well, muſt know how to chooſe wood proper for that purpoſe,

(moſt

(most woods being unfit for it) how to season it, and how to discern, whether it be duly seasoned, and otherwise well conditioned. To excavate and fashion the pipes, and other parts of the instrument, that are made of this wood, there is use of the turner's and joiner's crafts. It is often needful also, that the organ-maker be skilled in the effects of metals, and perhaps their mixtures; and the ways of casting them, in order to the making of his pipes of a sonorous matter, and to the giving them a due shape, and other desirable qualifications. I might here borrow further instances from bells, lutes, harps, and other musical instruments; but I hasten to examples of another kind.

He that has never so attentively considered the nature of salt-petre or of brimstone apart, shall never be able to make the considerablest uses of either of them, till he skilfully associate them to one another, and incorporate them into that wonderful body, called gun-powder, which will afford us an instance fit enough to explicate what we have been saying: for consisting of three differing ingredients, nitre, brimstone, and charcoal, though neither of these be sufficient, *in omni genere* (as they speak in the schools) yet each of them is very useful by being sufficient *in suo genere*, and really concurs to the effect produced by them all, as you may elsewhere find more particularly declared.

He must remain ignorant of another considerable use of sulphur, that is unacquainted with some properties of common oil and calcined alabaster. For artists have a way of making moulds, wherein to cast off the impression of medals, and other works embossed on metals, which, though the effects of it seem strange to those, that know not how they are produced, they easily thus perform. They make about the embossed work, whose impressions they desire to have, a little border or ledge of clay, to hinder the melted sulphur to be poured on it from running over; then they lightly (but very carefully) with a pencil or feather anoint the metalline work with oil, to hinder the sulphur from adhering to it; then they melt good brimstone in any convenient pot (which they cover well to prevent its taking fire) and whilst it is hot, they pour it gently upon the embossed metal, all whose extances will make perfect impressions on the lower surface of the thus melted brimstone, which ought to be poured on in a considerable quantity, that the moulds thus made may prove the stronger. About the edge of this mould they make a little rim or border of clay as before; and lightly anointing both all the surface of the mould, and the inside of the clay with oil (which if it be too copious, is, as we have tried, apt to prejudice the accurateness of the impression) they pour in by degrees to the thickness of about a fourth of an inch of that mixture I formerly mentioned (in the eighth essay) to be made of recently calcined alabaster, stirred and incorporated with such a quantity of fair water, as may suffice to bring it to the consistence of the thicker sort of honey. And this mixture in about a quarter of an hour growing hard, and then being taken out of the mould (to which the oil hinders it from sticking) will, if the work have been dexterously done, and the mixture before affusion carefully freed from bubbles, perfectly exhibit the shape and dimensions of the work embossed upon the metalline pattern. And by this way in a few minutes have we sometimes cast off a coin, a medal, and sometimes too a whole landscape, without any trouble, and not without some delight.

And here, *Pyrophilus*, let me perform what I lately intimated an intention of, by now taking notice to you in this fifth section of this essay (of what I had not long since occasion to observe in a former part of it) that you may oftentimes find such particular bodies conducive to the main effect of an [illegible] or experiment, by performing some subordinate part or office in it, as yet may [illegible] at all of kin to the ultimate effect promoted by the [illegible]

THAT aqua fortis, that ſo greedily corrodes and devours ſilver and braſs, ſhould eminently conduce to the real ſilvering over of the latter metal by the former, is that, which few goldſmiths, or even chymiſts would judge probable. And yet this fretting liquor performs a principal part in that ingenious way of ſilvering over braſs and copper, which is more applauded than known. For firſt, aqua fortis ſerves very well to make clean ſuch emboſſed or otherwiſe uneven pieces of metal, whoſe inequality hinders us from being able to cleanſe their little cavities with tripoli, or thoſe other powders commonly uſed to ſcour braſs: whereas if ſuch bodies be lightly waſhed over with aqua fortis, and immediately thrown into fair water, the foulneſs may be fretted off, and the work not disfigured. And this is eſteemed the beſt way of ſcouring ſuch metalline pieces of work by the beſt maker of mathematical inſtruments, that I have met with. And I the rather mention it to you, *Pyrophilus*, becauſe that though it be not always requiſite to our experiment of ſilvering (for many pieces of brazen work may well enough be made clean after the ordinary manner) yet divers trials have aſſured us, that the ſcouring of the braſs and copper is neceſſary to the ſucceſs of this experiment; probably, becauſe any greaſe or filth remaining upon the ſurface of the metal is ſufficient to keep out thoſe little parts of diſſolved ſilver, which ought to lodge themſelves ſo thick in the pores of the metal, as to ſeem one continued ſilvered body.

THE remaining part of this operation may be thus performed. The metal to be wrought upon being made very clean, you muſt diſſolve good ſilver (the finer the better) in aqua fortis in a broad bottomed veſſel of glaſs, or at leaſt of glazed earth; and having, over a chafing-diſh of coals, or with ſome ſuch heat, evaporated away all the aqua fortis, you muſt upon the remaining dry calx pour of water five or ſix times its quantity, or as much as will be needful perfectly to diſſolve it. This water with the like heat muſt be forced away as the former menſtruum, and the like quantity of freſh water muſt be poured on, and evaporated quite away the ſecond time, and, if need be, the third time, toward the latter end making the fire ſo ſtrong, as to leave a perfectly dry calx; which, if your ſilver has been good, will be of a good white, and will by theſe operations be competently freed from the ſtinking and fretting ſpirits of the aqua fortis. Of this calx you muſt take one part, and about as much (in quantity, not in weight) of common ſalt, and as much of cryſtals of tartar (or at leaſt powder of good white tartar) as of either of the former ingredients; which, like this, ought to be finely beaten; and theſe three powders being exquiſitely mixed, you muſt plunge the ſcoured braſs, to be ſilvered over, into fair water; and then taking up as often as need requires, with your wet fingers, ſome of the newly mentioned mixture, you muſt rub it on well, till you find every little cavity of the metal ſufficienly ſilvered over; remembering, that if you would have it richly done, you muſt rub in more of the powder. And laſt of all, you muſt waſh well your ſilvered metal in fair water, and rub it very well and hard with a dry cloth, that it may appear ſmooth and bright. And this way of ſilvering, though it be preſently and cheaply performed without quickſilver, yet may be made to laſt ſome years, as experience has partly informed me, and may be eaſily renewed, when the ſilvering begins to decay or wear off.

AND here, *Pyrophilus*, it will not be improper to give you this advertiſement, that we ought not to conclude, as we are very prone to do, that ſuch an uſe is not to be expected, or endeavoured to be obtained from ſuch a thing, becauſe we ſee the like uſe to be made of things, that are thought to be of a quite differing nature from that we conſider, or perhaps quite contrary to it: for in many caſes, as there are more ways than one, or even than a few, to bring to paſs a thing propoſed; ſo among the various inſtruments, that may be employed the ſame purpoſe, ſome may exceedingly differ between

tween themselves as to other qualities, and yet agree in that, which is requisite and sufficient for the performance of the thing designed. As though, for instance, resin and sal armoniack be differing in colour, smell, taste, weight, hardness, &c. though the one be a vegetable concrete juice, the other an aggregate of urinous, fuliginous, and marine salts; the one readily dissoluble in water, the other not dissoluble in that liquor, but in oil; and though there be I know not how many other differences between them; yet either of them single may be, and is, usefully employed for the tinning of brass and copper-vessels, each of them being endowed with a fitness to make tin stick to those metals, as I elsewhere more particularly declare. Thus, though water, sand, and tin, are bodies in other respects very unlike, yet the two latter are found fit to make hour-glasses, as well as the first; though that alone, as is presumed, were for many ages employed by the ancients for that purpose.

To the foregoing advertisement I shall annex another, that may seem very differing from it, but yet is no less true; namely, that we are not always to suppose, that because a natural body has such an use on some occasions, the same body cannot on other occasions be employed to uses, that seem of a quite differing, and perhaps of an opposite nature.

This I conceive may be done principally by these two ways. First, by the differing constitutions of the several bodies the same agent works upon; as when the heat of the sun melts wax and hardens clay; and the same spirit of vinegar, which on filings of copper will by digestion obtain an abominable taste, will upon filings of lead acquire, by the same way, a very great sweetness: and spirit of salt, that will dissolve copper and iron, as aqua fortis also does, will yet precipitate silver dissolved in that menstruum. And to this first way I shall subjoin the second, which is, that such a parcel of matter, as is wont to be considered as one and the same body, may contain in it parts of very differing natures, upon whose account its operations may be diversified. Thus when we calcine some unripe minerals with nitre, the inflammable parts of the nitre do burn up and dissipate into smoke the volatile and combustible parts of the mineral; but by virtue of the remaining alkali of the nitre, several other parts of the mineral are made far more fixed and capable of enduring the fire, than they were before. So sulphur has in it some parts, that make it more readily inflammable than even nitre or oil; and yet it abounds with acid and vitriolate particles, that are not inflammable themselves, and much resist the accension of flame in divers other bodies. And accordingly, though in matches used in tinder-boxes to take fire readily, the kindled brimstone acts upon the shivers of wood, whose ends were crusted over with it, as an ordinary flame; yet the same burning body, by virtue of its acid parts, works in another capacity, than that of a common flame upon some metals, especially iron, and likewise upon the leaves of red roses, which its fumes turn white.

I could, if it were needful, propose in this place sundry other instances of the differing actions of the differing parts of a body, and could likewise subjoin other cases, than I have yet mentioned, wherein bodies may be applied to uses, that many would be unapt to expect from them: but judging it more convenient to reserve those for other places, especially in the last essay, I shall conclude this with the two following advertisements.

The first is, that I have in all this discourse purposely forborn to treat of the medicinal uses of things, because my scope in the volume, whereof this essay is a part, obliged me so to do. But yet I am sensible, and would have you so too, that hereby I have forborn to employ a multitude of particulars, that would have much enriched this treatise. For there is a great number of bodies, both natural and factitious, that being employed as medicines for human bodies, have there very various and ſometimes ſeemingly repugnant operations, many of which would ſerve to illuſtrate and confirm ſundry paſſages of this eſſay. Thus rhubarb, whether taken in ſubſtance or infuſion, does by virtue of its differing parts, firſt purge, and then bind. Spirit of wine taken inwardly exceedingly heats the body; whereas outwardly it is employed to appeaſe the heat cauſed by ſome hot humours and inflammations. Mercury taken inwardly, crude as it is, has often, though not always, proved an effectual and harmleſs medicine in worms, and ſome other diſtempers, even to children and women in labour: but the ſame mercury rarefied into fumes (which yet may be condenſed again into running mercury) and in that form taken into the body, does too often cauſe vehement and dangerous commotions in the juices of the body, as exceſſive ſalivations, fluxes, &c. declare. And he that ſhall attentively conſider the various operations of that one mineral antimony, and the not only differing, but oftentimes contrary effects, that it produces, according to the complexions and diſpoſitions of the taker's body, and the preparation of the mineral itſelf, will not, I preſume, ſtick to allow me, that the medicinal uſes of things, if I had not thought fit to decline them in this eſſay, might have much increaſed the number of inſtances it contains; the effects of others bodies upon thoſe of men being no leſs proper inſtances of nature's ways of working, than the changes they produce, when they work only upon one another.

THE ſecond advertiſement, wherewith I ſhall conclude this eſſay, is, that though what I have hitherto diſcourſed, hath almoſt ſolely related to the neglected uſes of particular natural bodies; yet I would not have you thence take occaſion to imagine, that there are not other natural things, whereof divers uſes may be made, that men have hitherto either ignored, or overſeen. By other natural things I mean the differing ſtates of matter, or of bodies (ſuch as rarity and denſity, fluidity and firmneſs, putrefaction and fermentation, may ſeem to be) as alſo the more operative qualities, ſuch as heat, cold, gravity, &c. the laws of local motion among the parts of matter, and the preſent fabrick of the univerſe, and eſpecially that of our terreſtrial globe and its effluviums; to which might be added other things in nature, that are not properly bodies in the uſual ſenſe of that word, but may be called things corporeal, as they belong to bodies, and entirely depend on them. In favour of this advertiſement it were eaſy for me to ſuggeſt to you ſuch a multitude of particulars, that reſerving ſome few for the laſt eſſay, I here purpoſely forbear to mention any at all, to avoid being enticed or engaged to enter upon a ſubject that could not be otherwiſe than very lamely handled, without enormouſly ſwelling an eſſay, that does already exceed its juſt dimenſions.

TRACTS.

T R A C T S.

O F

A Diſcovery of the ADMIRABLE RAREFACTION of the AIR.

New Obſervations about the DURATION of the SPRING of the AIR.

New Experiments touching the CONDENSATION of the AIR by mere COLD; and its COMPRESSION without mechanical Engines.

The admirably DIFFERING EXTENSION of the ſame Quantity of AIR rarefied and compreſſed.

A D V E R T I S E M E N T.

THE Author of the following papers ſuppoſeth his readers to have learned, either from the books he hath publiſhed, or from what hath been borrowed thence by other writers, the ſtructure and more familiar uſes of a pneumatical engine of his, mentioned by ſeveral authors under the name of *Machina Boyliana*; with whoſe deſcription therefore thoſe are deſired to acquaint themſelves, that ſhall think it worth the while to underſtand, as well as read, the following papers; about which it might be further taken notice of, that the firſt of them was indeed written to a learned friend, though his name be not now annexed (for certain reaſons); preſently after which the three others were thought fit to be ſubjoined. As for for the omitting of the compliments and forms, uſual at the cloſe of epiſtles, the author did it, as well to ſpare the reader, as himſelf; who hopes he may be excuſed, if the tranſitions from one diſcourſe to another, and even the ſtile and method of them, be not ſo ſmooth and regular, in regard the enſuing writings were traced, when he was afflicted with a great fit of ſickneſs, that kept him from ſo much as once reading over himſelf, what he had indited.

A DIS-

A DISCOVERY OF THE ADMIRABLE RAREFACTION OF AIR, (EVEN WITHOUT HEAT) IMPARTED In a LETTER to a FRIEND.

DO not imagine, Sir, that I did at all wonder to ſee you yeſternight ſo much admire, to hear me talk with ſo much ſeeming extravagancy about the rarefaction and condenſation of the air; for I confeſs, that I did deliver ſomething on that occaſion, that might eaſily, at firſt ſight, appear ſo near impoſſible, as to be utterly improbable.

AND though you were pleaſed, even on ſuch an occaſion, to expreſs a very favourable opinion of my veracity, yet thinking it fit, that ſuch an obligation ſhould not divert, but engage me, to endeavour to juſtify you to yourſelf, by confirming what I ſaid to you; I have already ſought and found among papers, many years ſince laid aſide, ſome that will enable me to make good more, than what the diffidence of my memory allowed me to ſay in the very boldeſt part of my yeſternight's diſcourſe. For now that I luckily find not only the originals of the relations, whereof this paper contains copies, but that my engine is in good order; I am ſo far qualified to countenance a diſcourſe, wherein I kept ſomewhat within compaſs, that though it will perhaps coſt me much pains and trouble, to make *extempore* experiments fully equal to the incloſed; yet if any juſt doubt ſhould require it, I preſume, I can make ocular proof of, at leaſt, as much as I laſt night told you.

AND now it is time, after having contrary to my cuſtom, raiſed in you a high expectation, that I endeavour in ſome meaſure to anſwer it, which I hope I ſhall the more eaſily do, becauſe the agreement you have often had occaſion to obſerve between the relations regiſtered in my adverſaria, and the phænomena of the experiments they de-

ſcribe,

scribe, will, I presume, make it needless to persuade you, that the ensuing trials, being transcribed thence, may be safely credited. Wherefore I shall proceed to annex them, as soon as I have presumed a few historical lines, by way of manuduction to them.

It is now many years since, that having a desire to reduce the air to a degree of rarefaction, that appeared to be considerable, upon surer grounds than slight conjectures, I attempted to do it by the help of heat, and particularly by that of an œolipile, which I have mentioned in another tract *: but finding, that the diligent *Mersennus* had, if there be no mistake in his account, been able to rarefy air that way, full as much, or more than I could, I betook me to try, whether I could not, by the spring of the air (without heat) procure a greater expansion of it? I found (as I have long since elsewhere † related) that in the pneumatical engine, which has been since called Machina Boyliana, I could encrease the expansion of air, till the body attained to about one hundred fifty-two times its former and usual dimensions. But this expansion, though it were above twice as great as the utmost procured by *Mersennus*, did not yet satisfy me, but put me (according to what I there intimate) upon another contrivance, which though put in practice eight or nine years ago (as the date of one of the trials may inform you) had the relation of its successes laid aside among those of others, made in the same engine, which yet lie by me unpublished. So that I may now proceed to give you the transcripts of the trials themselves, as they were hastily and inelegantly, but very faithfully, set down among my Pneumatical Collections. And this I am ready to do, as soon as I have intimated to you, that in that noble collection of experiments, that has about two years since appeared in publick, as the first-fruits of the justly famous Florentine Academy, I find, that those virtuosi had, according to their sagacity, so advanced the extent of the air, as without the help of heat to bring the dilatation to exceed one hundred seventy-three times its former dimensions; and that, which made their improvement the more considerable, and consequently the more worthy of them, is, that they procured this great rarefaction, as well as I had done mine, by the air's own spring; and had surpassed, without the help of my engine, what I was then at first able to do by the conveniencies that it afforded me. Whereupon, remembering what I had performed in that kind several years before, I sought among my papers for the trials I had then made, and found those notes, whereof, I now, at length, think it high time to give you the promised copies in the following terms.

EXPERIMENT I.

We took a round glass-egg (as they call them) of clear metal furnished with a pipe, or shank, of some inches in length; this we filled with water, and conveyed both it and a phial with water in it, into a receiver of a convenient size, and by pumping the air out of it, we made the bubbles both in the egg and the phial to disclose themselves in great numbers; so as to make the liquor in the glass-egg seem to boil, and to make all that was in the shank really to run over. When we thought the water was sufficiently freed from air, which it was not quickly brought to be, we took out the glasses and filled up the pipe of the egg with water taken out of the phial, and inverted it into more of the same water, in such manner, that the egg was quite full, shank and all, excepting a small bubble of air, that we purposely left to gain the top of the egg; where, the glass being transparent, with a pair of compasses we measured as accurately as we could, and found it to be a tenth, and less than two centesms of an inch. Then putting the glasses

* New Physico-Mechanical Experiments, Exper. VI. † Ibid.

again into a receiver, we set the pump at work, and the little bubble, after a while, began to expand itself, which when it had once done, it did at each suck strangely increase, till at length it drove all the water out of the round part of the glass. And lest it might be objected, that it was only the subsiding of the water upon the withdrawing of the outward air, that before kept it up to the top of the glass, we caused the pumping to be so continued, till the expanded air had several times driven the water in the pipe of the egg, a pretty way beneath the level of the external and surrounding water in the other glass. This done, we let in the air by degrees, with a design to observe, what bubble we should find at the top of the egg, when the water should be again driven up into its cavity. But the expanded air had forced over so much water, that there remained not enough to fill the globulous part of the egg: wherefore we tried the experiment again, and when we had proceeded thus far, we compared the above-mentioned diameter of the small bubble, with that of the spherical part of the glass, which we took with a pair of calliper compasses: and though we found it to be somewhat more than 20 times as great, yet being willing rather to disfavour than flatter the experiment, we supposed the two diameters to be as 1 to 20, and consequently, since, as *Euclid* demonstrates, the proportion between spheres is triplicate to that of the diameters, and in our case, the cube of the lesser diameter being one, is also but one, the cube of 20, the greater diameter, must be 8000; and so the air appears to have, by expanding itself, acquired a place 8000 times as big as it possessed before. Nor was it overseen by us, that the globulous part of such glasses as we used is scarce ever made spherical. But not only I, but Dr. *Wallis*, who was pleased to assist at the experiment, concluded, that the cavity of the shank, which the expanded air drove the water from, but which we did not compute, would make abundant compensation for the two above-mentioned particulars. After this, for further satisfaction, we took water, laboriously freed from air, and putting it into the same glass-egg, we inverted it as before, but left not any bubble in it. This we did, that in case we could make the water subside, the experiment might prevent a suspicion, that some air latitant in the water might increase the bubble that was formerly left in it; having then exhausted the receiver as much as before, and if we mistook not, more, the water in the egg did not all subside; but at length, with obstinate pumping, a bubble disclosed itself, and drove all the water clear out of the round part of the glass; and though by reason of some small leaks, that we could not find or stop, we were not able, as before, to make the expanded air depress the water in the shank, beneath the surface of the external water, yet we wanted very little of it; and then out of weariness giving over, we found, that when the water was impelled up again into the egg, there was at the top of it a bubble, whose diameter we measured as faithfully as we could, and found it to be the diameter of the globular part of the glass, as 1 to 14; so that, though the little bubble had been a perfect sphere, yet spheres being, as was lately noted, in triplicate proportions to their diameters, the bubble when expanded, must have been 2744 times as big as the bubble unexpanded. But Dr. *Wallis*, who will be allowed to be a very competent judge in these matters, observing (what we all took notice of) the great thinness of the bubble, positively and constantly affirmed, that he could not estimate it to be at most any bigger than the third part of a perfect sphere of that diameter; by which estimate the expansion of the bubble must have reached to 8232 times its natural dimensions.

El. 12. Propos. ult.

N. B. By letting in water into the receiver, as much as it would admit, we found, that by reason of some secret leak, we had not been able so to exhaust it, but that there remained some air.

EXPE-

EXPERIMENT II.

A SMALL and almost inconspicuous bubble expanded itself, when the ambient air was pretty well exhausted, to more than 10,000 times its former extent. The manner thus: we took a small bolt-head, blown by a lamp, which contained in all about 80 grains of water, and inverting the small neck into a jar of water, it was included in the receiver, and the ambient air being exhausted, store of bubbles rose out of the water, and expanding itself, quickly drove all the water out of the bolt-head. Then re-admitting the outward air, the bolt-head was presently almost filled, and all the expanded air shrunk into a bubble, little bigger than a small pin's head; then taking the bolt-head out of the water, and inverting it, that the bubble might get out at the neck, we carefully filled it up with the water that had been freed from air, and then inverting it as before into the jar with water, we again included it, and after some exsuctions found, that there was gotten out of the water into the neck a very conspicuous bubble, which, upon the admitting of the air, shrunk almost into an invisible one, and ascended into the head of the glass. Then again exhausting the receiver very well, we found it expand itself, so as to fill all the capacity of the bolt-head, and to drive out almost all the water. And upon the re-admitting of the air, it again shrunk into a bubble, whose diameter (according to our best estimate) was not bigger than one two-and-twentieth part of the diameter of the head of the above-mentioned glass; so that to fill the whole cavity of the head only, it expanded itself 10648 times: but because it filled likewise the greatest part of the neck, we found by weighing the water that filled that part, and the water that filled the head, that the capacity of that part of the neck was almost a third of the capacity of the head, being as 141 to 481: if therefore 481, the capacity of the head, contained it 10648 times; 141, the capacity of the neck, must contain it 3121 $\frac{167}{481}$ times; so that in all, the small bubble of air was expanded to above 13769 times its former bulk. June 2, 1662.

THE diameter of the small bubble retracted was $\frac{1}{27}$ of an inch.

THE diameter of the outside of the head of the glass was $\frac{29}{36}$ of an inch.

THE water that filled the head only weighed 60 $\frac{1}{2}$ grains.

THE water that filled the head, and as much of the neck as the air had before expanded itself into, weighed 78 $\frac{1}{8}$ grains; so that that part of the neck weighed 17 $\frac{5}{8}$ grains.

THE bolt-head itself weighed 15 grains.

I MIGHT have set down this second experiment unaccompanied either with the first, or with that I am going to subjoin; because the expansion produced by neither of them was, at least by measure, so vast, as that produced by the trial newly mentioned: but this was so stupendous, that I thought it not so fit to present it to you by itself alone, but rather accompanied with other experiments, the least prosperous of which produced a dilatation of air sufficient for my present purpose, and such as may not a little confirm, that what is recited in the second experiment, was neither a lucky chance, or mistake. And that may be enough for my present purpose; for as for the little abatements, that some will perhaps think fit to be made upon the score of the unequal thickness of glass or some such circumstances, they are not considerable enough to deserve to be now solicitously debated; nor to hinder the expansion, that must be granted from proving what they are alledged for: wherefore I will proceed to what follows.

EXPERIMENT III.

WE tried this experiment again, and found a small bubble, much about $\frac{1}{12}$ of an inch in diameter, filled not only the ball at the end of the bolt-head (which was 1 $\frac{1}{2}$ of an inch

inch in diameter) but the whole neck, which contained near as much water as the head, and beat down the ſurface of the water within the pipe, much below that of the water without the pipe.

These experiments already found among my old papers will, I hope, without ſeeking for more, ſuffice to manifeſt, that the expanſion, which the air may be reduced to without heat, is indeed admirable; for if we make an eſtimate of it but according to the experiment, which had the moſt moderate ſucceſs, it appeared, that one ſpace poſſeſſed, though not adequately filled, by a portion of air, may have its air extended to at leaſt 2744 ſpaces equal to it; I ſay, at leaſt, becauſe very probably it was above twice as great: and if we make our eſtimate according to the moſt proſperous of our trials, we muſt allow the air to be rarefiable at leaſt 13000 times; I ſay again at leaſt, becauſe I am not ſure, that in that trial it was reduced (not fully, though perhaps very near) to the uttermoſt degree of rarefaction attainable in our engine: ſo that I preſume you will now grant, that I ſpoke warily and much within compaſs, when I mentioned but an expanſion from one to a thouſand.

And now having performed the promiſe I made you, it remains only, that I take notice of the requeſt that you made me, about communicating theſe experiments to the curious. But this deſire of yours is oppoſed by no ſmall inconveniencies, that would reſiſt my compliance with it. For it would oblige me, by tearing out theſe papers, to diſmember a collection long ago in making, and wherein they were placed to be much otherwiſe diſpoſed of, and not only make a great gap in it, but ſtrip or deprive it of ſome things, that were the likelieſt to recommend it. Beſides that theſe appearing before the reſt are odd enough to make theſe ſeem far leſs uncommon, than perhaps otherwiſe they would. Yet all this notwithſtanding, I find it uneaſy to refuſe what you, and thoſe friends that concur with you on this occaſion, deſire, that if after having once more peruſed theſe papers, you perſiſt in the ſame earneſtneſs you expreſſed yeſterday, when you had not yet ſeen them, I ſhall not refuſe you the diſpoſal of them, both for the reaſon now given, and becauſe I have been informed as well by you as by other means, that the rarefaction of the air is at preſent the ſubject, that buſies the diſquiſitions of ſeveral eminent virtuoſi, both domeſtick and foreign, to whom I pay ſo much reſpect, that I ſhall think it a happineſs, if it may be acceptable to them, not only becauſe it will be ſeaſonable, but becauſe, that though the engine, that moſt of the attempts were made in, has not been thought altogether barren, yet theſe trials will probably paſs for one of the leaſt inconſiderable productions of it: and theſe two ſervices I hope this ſhort writing may do ſeveral ingenious readers; the one, that it will invite and accuſtom them to take notice of, and conſider the great ſubtlety of nature, and the ſcarce imaginable ſmallneſs of thoſe aerial inſtruments that ſhe employs even about viſible operations: the other, that theſe relations will excite the more curious and piercing wits to debate, and I hope help them to ſolve the two problems here propoſed to them; what figures and motions may be aſſigned to the particles of the air, to explicate it's ſo wonderful rarefiableneſs, and that perhaps without quite loſing its durable ſpring, and how the air comes to be rarefiable ſo many times more without heat, than hitherto we have found it to be by heat. To which might be added, as a third, what might be reaſonably conjectured about that part of the cavity of an exactly cloſed glaſs, where, though the eye diſcovers no viſible ſubſtance harboured in it, it appears not, that the common air does adequately fill ſo much as the ten thouſandth part?

NEW

NEW OBSERVATIONS ABOUT THE DURATION OF THE SPRING OF EXPANDED AIR,

(Subjoined by way of APPENDIX to the foregoing EPISTLE.)

FORASMUCH as reviewing the former paper about the Rarefaction of the air, I took notice in the close of it of an expression (viz. and that perhaps without quite losing its durable spring) which, I fear, may, to some readers, seem to need explication; it will not be improper on this occasion to subjoin something by way of appendix about it.

FIRST then, the reason why, in this short intimation, I thought fit to employ the diffident term *perhaps*, was, because I had not (nor yet have) been taught by trial, whether and how far the utmost expansion of the air actually produced in my engine, or otherwise procurable, and its retaining a sensible spring, are consistent. I express myself thus, to insinuate, that I thought of other instruments and methods, whereby the dilatation of the air may not improbably be measured and promoted; as by making the Torricellian experiment in a glass with a very capacious head or globulous part, and applying the aërial particles, that will ascend out of the subsiding mercury together with a bubble of other air, if it be needful, to the use we have been speaking. Something also may be done to some purpose, with very fine and large fish-bladders; but I shall not insist on these or the other expedients that came into my thoughts, contenting myself to have intimated, and thereby acknowledged, that there may be other means besides the Machina Boyliana, to bring air to a very great expansion. But whether any of them will surpass what has been actually attained in that engine, time must declare; till when, we shall be content to make use of the experiments it has already actually furnished us with.

WHEREFORE to come to the second or other remaining part of it; whereas in the mentioning of the spring of the expanded air, I employed the attribute of durable, you may easily gather the reason from what I am now going to annex.

I HAD

I HAD obſerved, not without ſome wonder, in the enquirers into the nature of the air, that they have not, that I know of, ſo much as attempted to diſcover, whether the air, either in the utmoſt or in the intermediate degrees we can bring it to, does retain a conſtant and durable elaſticity?

FOR, firſt, men have not determined whether a portion of our common air being exactly ſhut up in an hermetically ſealed glaſs or ſome other exactly cloſed veſſel, will conſtantly and uniformly, for a moderate time at leaſt, retain the degree of elaſticity it had when it was ſhut up; and whether it will not ſometimes vary its preſſure, as we ſee, that the atmoſpherical (though I think upon peculiar grounds) is, by the help of our baroſcopes, obſerved to do? Next, it does not appear, whether included air, in caſe it retain an uniform elaſticity for a moderate time, will retain it for a very long one. Nay, whether it would not at length come not to have a weaker ſpring, but perhaps to have no ſenſible ſpring at all, as we ſee it happen in ſword-blades and divers other ſpringy bodies, which, after having ſtood too long bent, will continue ſo, and loſe their former power of ſelf-reſtitution, as they call it.

THIRDLY, men have not yet determined any thing about the degrees of the air's elaſticity, whether the durableneſs and uniformity, or varying of its ſtrength, may not depend upon the differing degree it had when it was firſt ſhut up.

FOURTHLY, much leſs have we yet attempted to diſcover, whether the ſpring of an incloſed portion of air may be ſometimes weakened, and ſometimes ſtrengthened by the changes, as to gravity, of the outward atmoſpherical air, the new and full moon? To which I might add divers other external accidents, which, as yet, we ſcarce ſuſpect. And to theſe I might add ſome other doubts and enquiries that may not be impertinently ſuggeſted, but here would, I fear, paſs for a digreſſion.

WHEREFORE I ſhall proceed to tell you, that having taken notice of it, as an omiſſion among the inquirers into the nature of the air, in whoſe negligence I was too long a ſharer, that we have not, that I know of, ſo much as attempted to diſcover this itſelf; whether the air, either in the utmoſt or in the intermediate degrees of rarefaction, we might bring it to, would for a conſiderably long time retain its elaſticity, or at leaſt ſome determinate degree of it, or loſe it by determinate and regular decrements, I thought fit to make ſome trials about this matter, but cannot brag of the ſucceſs of my intentions, having been hindered either by want of inſtruments, or by removes, or by ſickneſs, or by unlucky accidents, or by one unwelcome thing or other, from accompliſhing what I had chiefly deſigned, and partly alſo made ſome progreſs in it; but yet to give you ſome hints, as well as ſome occaſion to more proſperous experiments, I ſhall not ſtick to annex, what I readily call to mind about my attempts on that occaſion.

I REMEMBER then, that when I firſt began to try ſomething in order to my deſign, being deſtitute of fit accommodations, I was fain to content myſelf, by cauſing a good bubble of glaſs, with a ſtem to be ſo blown at the flame of a lamp, that whilſt the ball was yet exceeding hot, and conſequently contained none but highly rarefied air, the ſtem was very nimbly clapped into the flame of a candle that was purpoſely kept ready at hand; ſo that being ſlender, it was in a trice ſealed up, and the air within remained as much expanded as the great heat it had been expoſed to had brought it to be. This bubble many months after I inverted into a baſon of water, and having broken off the ſeal under the ſurface of it, the liquor was violently impelled into the cavity, but yet was not able to fill it, a conſiderable part being defended from the further aſcenſion of the water by the ſpring of the remaining air, which, for all the long ſtretch it had been put

to,

to, had not loſt any thing of its ſpring that we could take notice of. But this was a trial in which I could by no means acquieſce; and therefore when I was a little more befriended by opportunity, I tried another way, partly to give a ſomewhat pleaſing ſurprize to unaccuſtomed beholders, and partly, becauſe though it could not ſhew all that I deſired, yet it might plainly ſhew, that the air, even at a very conſiderable extenſion, would hold out for a conſiderable time. Wherefore leaving a very ſmall proportion of air in the folds of a fine limber bladder, whoſe neck was very cloſely tied, I cauſed it to be, by the help of the Machina Boyliana, ſo expanded, that at length it ſo dilated itſelf, as to ſeem to fill the whole bladder, and reduce it to the extent it had juſt before it was emptied; and the bladder, by a peculiar contrivance, was ſo included in another veſſel, that being protected from all intruſion of the outward air, it maintained its plump and tumid figure, and in that unwrinkled ſtate I ſhewed it many months ſince to ſome virtuoſi, now here in *London*, after it had continued ſo, if I miſtake not, near two years. Since the writing of this, I did, at length, find the newly-mentioned veſſel, and ſhewed it to ſome curious ſpectators, who, with me, took notice, that the included bladder, inſtead of being wrinkled or ſhrunk, appeared to be plump and full, as well blown bladders are wont to be. So that many months, perhaps a dozen, may be added to the freſhly mentioned duration of the expanded air.

But this way ſatisfying me neither as to ſome of the particulars I deſired my attempts ſhould diſcover, I deviſed a little inſtrument, whoſe contrivance, though it ſeemed very ſimple, promiſed, and for ſome time gave me a far more accurate account of what I expected. The inſtrument, if you deſire it, I can eaſily ſhew you, having lately been forced to make a new one, which is now by me; but it may ſuffice to tell you, that it is ſo framed that it is fit to diſcover, beſides divers other things, whether, and how long air brought to the greateſt expanſion I could conveniently reduce it to in my engine, will retain its ſpring; and by what degrees or ſtages and periods of time, the decrement, if any be, is made? But of the iſſue of the trial made in it, I can give you but a very imperfect account, in regard, that, though I made it about three years ago, yet having left the inſtrument in a place, where it is ſo lodged, that I cannot have it without returning thither, till I ſee it again myſelf, I dare not venture to judge of the ſucceſs of the experiment; only this I remember, that I took no notice of any obſervable diminution in the air's elaſticity, though it were preſſed, and, as it were, clogged with a weight, that one would wonder how it could, when it was ſo highly rarefied, ſupport for one minute *.

* See the Poſtſcript.

There is alſo another way that I contrived, wherein the air in a little portable inſtrument, which I can ſhew you, being expanded, as one may gueſs, to five or ſix hundred times (perhaps a thouſand times) its wonted extent, has not only for a long time preſerved its ſpring, but ſatisfies me alſo about one of my chief queries, which was, whether the air, very much dilated without heat, would be conſiderably ſenſible of external heat? which it plainly appears to be in this inſtrument, where, notwithſtanding the great rarity it has already attained and ſeems likely to preſerve, the heat of one's hand applied to the outſide of the veſſel has a quick and very manifeſt operation; and upon the withdrawing of it, the ſenſible air quickly returns to its former dimenſions, as well as temper; ſo that one may employ it as a kind of weather-glaſs, and perhaps make ſome diſcoveries by long comparing it therewith.

But hitherto I have been doing what I do not love to do, and very rarely have done, when I mention my own experiments, that is, I have not punctually ſpecified any determinate quantities and proportions of the things ſpoken of; but one of my former trials I have newly found out regiſtered in a looſe note, and therefore the quantities being annexed, ed, I hope it may both give some countenance to what I have been saying, and give some, though not an entire satisfaction about the thing itself.

March 3 A GLASS, as cylindrical as we could get it blown at our lamp, and having a long stem coming out at the unsealed end, was quite filled with water and inverted into water placed at the bottom of a large pipe sealed at one end, and of three or four feet in length. This external pipe, so called for distinction sake, was exhausted, till the air that disclosed itself in the water of the internal pipe, had drawn out the water in the cylindrical pipe as low as the upper part of the stem; at which great expansion of the air, the external pipe being speedily and securely closed by a certain contrivance, the air, thus rarefied, was kept sometimes in my own chamber, that was warmer, sometimes in an under room; and after it had been kept from first to last about eleven weeks or three months, if I mis-remember not, without any other remarkable variation than that in the cold room the water ascended, as I guessed, about an eighth, or near a fourth at that part of the internal pipe; where the lower end of the cylinder gradually lessened itself into the slender stem. Yesterday I invited Dr. *Wallis* to be present at the breaking of the glass, and to favour me with his assistance for the better estimating the expansion of the air upon the breaking of the closed apex. The water was but leisurely (because of the slenderness of the orifice that was made for the air to get into it) impelled up into the formerly deserted cavity of the cylinder, which it filled all, save a little bubble, which was exceeding shallow. We made use of our eyes at a fit distance, and of compasses both ordinary and calliper, to obtain these measures. The cylindrical part of the internal pipe was three inches in length, and three fifths of an inch, or less, in diameter on the outside. The bubble was two tenths in diameter, and about two centesms in depth; from all which, by the doctor's calculation, the bubble, to the space it possessed unexpanded, was as one to one thousand three hundred and fifty.

NEW

NEW EXPERIMENTS

TOUCHING THE

CONDENSATION OF THE AIR BY MERE COLD,

AND

Its COMPRESSION without MECHANICAL ENGINES.

BECAUSE it is as truly as commonly ſaid, that *contraria juxta ſe poſita magis eluceſcunt*, and becauſe what I am now going to interpoſe is little leſs than neceſſary to be premiſed, to clear the way to what follows, and to connect the paſt writing to that which is to enſue; it will not be improper to add ſomething in this place touching the condenſation and compreſſion of the air.

AND here I cannot but a little wonder, that among ſo many that have had occaſion to conſider the nature of cold, and the condenſation of the air by it, I have not yet met with any that have had the curioſity to meaſure that condenſation; wherefore I long ſince attempted to do it, as I have related in another diſcourſe; but not having that by me at preſent, and remembering in general that I did it in winter, when it may be objected that the air, being already præ-affected with the coldneſs of the ſeaſon, was not capable of being ſo conſiderably contracted by an additional cold, as it would be at a time of year when it is wont to be in a ſtate of greater laxity; I thought fit to make the experiment about the beginning of Autumn, without tying myſelf to make it with the ſame circumſtances that I had done before: the event of this trial I find regiſtered as follows.

AFTER the midſt of *September*, on a ſun-ſhiny day, and about noon (which circumſtances we made choice of, that the air might be the more rare and expanded) we took a bolt-head or round phial furniſhed with a long ſtem, and placed in a frame purpoſely provided, ſo that the ſtem was perpendicular to the horizon, and the globulous part was ſupported by ſuch a veſſel, that thorough a hole, purpoſely made at its middle, the ſhank reached downwards till the orifice of it was a little immerſed beneath the ſurface of a glaſs full of water, that was placed at the bottom of the frame. This done, we took a good proportion of ice, and having beaten it in a mortar, and mixed with it a due quantity of bay-ſalt, we not only laid it round about the lower part of the ball, but the veſſel contiguous to that part, being purpoſely made with turned-up brims, we were enabled to heap up the frigorifick mixture, ſo as to bury the whole globulous part of the glaſs in it, and cover the very top of it therewith to a conſiderable thickneſs; upon which occaſion, the air within being exceedingly refrigerated, the water, into which the ſhank terminated, was made to aſcend ſomewhat faſt along the cavity of the ſhank, till we perceived it would reach no higher, but after a while began to ſubſide again; which nick of time being carefully watched, we made a mark at the higheſt ſtation of the water, and then taking out the bolt-head we filled it with water, making allowance for that ſmall part of the ſtem, which was immerſed at the beginning of the operation. This water we weighed, and found it amount to nineteen ounces and ſix drachms, then weighing as much water, as ſufficed to fill the ſhank up to the mark newly mentioned, we found that to be one ounce and three drachms, by which number the former being divided, the quotient was fourteen drachms four elevenths, ſo that the proportion of the two quantities of water being as eleven to one hundred fifty-eight, the ſpace, into which the air was condenſed by refrigeration, was, to the ſpace it poſſeſſed in its former ſtate of laxity, as one hundred forty-ſeven to one hundred fifty-eight, and conſequently the greateſt condenſation that ſuch a time of the year and in ſuch weather, ſo high a refrigeration could bring the air to, made it loſe but $\frac{11}{158}$ of its former extent.

N.B. FIRST, the ſtem of the glaſs ought to be of a conſiderable length, leſt by the great contraction made of the air in the ball by its high refrigeration, the water ſhould aſcend into the cavity of the ball itſelf, and thereby become exceeding difficult to be meaſured.

SECONDLY, if one would be nice, one may take notice, that the height to which the water aſcended in the ſtem was about two feet; which cylinder of water, by its weight or tendency downward, might ſomewhat hinder the liquor from aſcending quite ſo high as it would, and conſequently keep the condenſation of the air from appearing fully ſo great as it was, but ſo light a cylinder as that of the ſuſpended water would ſcarce be very conſiderable.

THIRDLY, when the water was aſcended near as high in the ſhank as it would riſe, there was obſerved in it an odd kind of ſubſultus, or riſing and falling alternatively, almoſt like the mercury in the Torricellian experiment, before the mercury comes to ſettle after its firſt ſubſidence. [But the conſideration of this phænomenon belongs not to this place; for which reaſon I inſiſt not on this, and forbear mentioning ſome others.]

FOURTHLY, that though it appears not by this experiment, whether the cold thus produced is equal to that of froſty weather in winter, and conſequently capable of contracting the air as much as that ſeaſon is wont to do; yet by preceding trials made with fit inſtruments, I had found, that by ſuch an application of ice and ſalt as we had made in the late experiment, a greater degree of cold, and that in a warmer ſeaſon, might be produced than had been found neceſſary to make froſty weather in winter. The way of experimenting, for brevity ſake, I omit, but if you pleaſe you may command it.

BUT it is not chiefly to acquaint you with the condenſation that nature uſes to make of air, that I have been entertaining you with theſe memorials; for that which makes it very pertinent to my preſent purpoſe, is, that it will ſhew you, that as to the condenſation or compreſſion of air that I am to recite, though cold were employed about it, yet it was not really produced by cold, which could not contract the air to ſo much as half that degree you will find it was reduced to by our operation, preſently to be mentioned; wherein the frigorifick mixture did not primarily or immediately compreſs the included air, but only ſo affected the water that was ſhut up within the ſame veſſel, as to make it ſwell, and conſequently crowd the aërial particles into leſs room; wherefore it now remains that we proceed to the experiment itſelf, a ſhort account of which be pleaſed to take in the enſuing tranſcript.

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[To convince fome ftrangers, we took a new glafs bolt-head, with a neck not long, and filled it fo far with common water, that being hermetically fealed, the liquor reached within three inches of the top, as near as we could guefs by meafuring it with a ruler, and making an eftimate of the fharp end, made fo for the conveniency of fealing up the glafs, which fharp end we gueffed to be about a quarter of an inch in length, then applying fnow and falt to the lower part of the bolt head, we readily drove out the water further and further into the neck, till at length it was got up to the bafis of the fharp and conical end, where the glafs was fealed, and then juft as I was looking upon it, the glafs flew with a noife about my ears, being broke into many pieces, which argued the compreffion of the air to have been very great. And Doctor *Wallis*, who was prefent, and meafured it from time to time, defired me to regifter the experiment, with his eftimate of the compreffion, which was, that the air was reduced into the fortieth part of its former extenfion.]

I KNOW fo great a condenfation of air will feem ftrange to thofe that have taken notice, that fome of the beft mathematicians of our age that have made ufe of wind-guns, and other forcible engines to crowd the air into as narrow room as poffibly they could, confefs themfelves not to have been able, with all their ftrength and induftry, to force the air into lefs than the fifteenth part of its ufual extent; and befides, that this was done in countries, where the air may * well be fuppofed more lax and rare than in *England*. I confefs I faw no trials made with wind-guns, that convinced me that the condenfation was fo great as that newly fpecified (about which *Merfennus* himfelf fomewhat hefitates, feeming to doubt, whether the air were indeed reftrained into a fifteenth, or but into one eighth part of its former room). And he that hath obferved and confidered, as I have done, that in wind-fountains, as they call them, of glafs, the air will feem to be notably compreffed, whilft, indeed, we could not find it compreffed into much lefs than its third part, will be the lefs unapt to be diffident of the great things that are faid of the compreffion of the air; but becaufe experience has informed us, that our Englifh air may in peculiar inftruments be forcibly crowded into a tenth, twelfth, or perhaps a fifteenth part of its former extent, I am content to take it for granted, what is related about the compreffion of the air, into the fifteenth part of its ufual dimenfions; and yet our experiment will be a confiderabler inftance of the great compreffibility, if I may fo fpeak of the air; for, according to the eftimate delivered in the foregoing narrative, our compreffion, which was without mechanical inftruments or engines, reduced the air into the fortieth part of the fpace it had lately poffeffed; and how great a force is requifite, when the air is once confiderably condenfed, to furmount, though but a little, its great refiftance to further condenfation, may be gathered from the obfervations about the gradual renitency of the air to compreffion, which we many years fince made with mercury, and afterwards *Defence* publifhed in another treatife; but though upon the recited grounds that great compreffion of the air produced by our experiment may, as I was faying, feem very ftrange, yet it would not feem incredible, if I fhould here borrow thofe experiments and obfervations from my already publifhed hiftory, and fome unpublifhed papers about cold, that would countenance what I have been delivering, and efpecially if I fhould ftay to communicate to you the way I not unfuccefsfully made ufe of, to eftimate by weight the great force of the expanfion of water upon its freezing. But fince an account of this contrivance is not here neceffary, and would require more leifure than I can fpare at this time, it remains only, that by way of corollaries from what has been hitherto delivered in this and the two precedent writings, we rather point at than difcourfe of fome obfervations that it fuggefted to us in the enfuing paper.

* See *Merfen.* Phæn. Pneum. Prop. 32.

OF THE ADMIRABLY

DIFFERING EXTENSION

OF THE

SAME QUANTITY OF AIR,

RAREFIED and COMPRESSED.

HAVING already declared, that what I pretend to in the cloſe is but to ſet down ſome obſervations that reſult from or are ſuggeſted by what hath been already delivered, I preſume I need not trouble you or myſelf, with any other preface to what follows:

THAT then, which ſeems firſt worth taking notice of, is the differing alterations that the air is ſubjected to by cold and heat; for whereas we could not find in this our climate, that cold would reduce the air into near the twentieth part of its former extenſion by condenſation, heat would advance it to near ſeventy times its uſual laxity by rarefaction.

NEXT, we may obſerve, that by engines and other artificial inſtruments, the air may be two or three times as much compreſſed as nature is wont to condenſe it by cold, even in froſty weather; and ſo on the other ſide, the air may by the intervention of art and inſtruments be much more rarefied and expanded, than it has been yet found to be by the bare application of external heat, though it were that of an intenſe fire itſelf.

See as to the utmoſt expanſion by heat *M*[illegible]. in Cog. Phy. Mathem.

FURTHERMORE, it may ſeem worth while to obſerve how much the utmoſt degree of rarefaction by heat, that experiment hath ſhewn us of the air falls ſhort of the degree of expanſion to which it has been advanced in our pneumatical engine, the proportion betwixt theſe two expanſions being that of one to ſeventy or thereabout.

BUT, perhaps, it will not be neceſſary to conclude that the air is ſo much more rarefiable than compreſſible, as moſt readers will be prone to infer, by comparing the greateſt compreſſion and expanſion of it that are mentioned in theſe experiments; ſince, if I miſtake not, it ought to be conſidered that the air we made our trials with, upon the ſurface of the earth, was not (no more than is the air we commonly breathe) properly in a true natural conſiſtence, as they ſpeak; or, if you pleaſe, in a free and indifferent ſtate, in reference to rarefaction and condenſation, but was already highly compreſſed by the weight of the atmoſpherical pillar that leaned upon it, ſo that it had already a very ſtrong renitency to further compreſſion; whereas the air that was to be rarefied, had, by virtue of its ſpring (ſtrongly bent by the weight of the incumbent air) a ſtrong propenſion or tendency to dilate itſelf; which difference I muſt content myſelf to have intimated, and leave you to conſider whether and how much it may alter the caſe.

FOURTHLY, to ſome perhaps it will ſeem more fit to conſider than eaſy to reſolve, how, ſince the corpuſcles of the air are acknowledged to be heavy, and thoſe that remain

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muſt be ſo wonderful thinly diſperſed in the cavity of the receiver, they come to be ſupported and kept, as it were, ſwimming therein, and do not appear to ſubſide by their own weight, the *Materia ſubtilis* (though the preſence of that ſhould be admitted) not appearing to have gravity wherewith to ſuſtain them; and the vacuum (if that be ſuppoſed wherever the aërial particles are not) being too near a-kin to nothing to be able to oppoſe their deſcent; but though ſomething may be ſuggeſted about the ſolution of this doubt, my haſte obliges me to leave it as ſuch.

FIFTHLY, I will not make it my buſineſs to make mention in this place of the wonder that may be juſtly excited in thoſe, that when they look on one of our well exhauſted receivers, attentively conſider how ſmall a proportion the common aërial corpuſcles, which are very ſparingly diſperſed there, bear to the whole cavity of the veſſel, which, before it was exhauſted, was thought to be repleniſhed with air alone. This, I ſay, I ſhall not ſolicitouſly obſerve, becauſe I think I need not; for I little doubt the thing will be obſerved and laid hold of, both by the *Carteſians* and *Epicureans*, the former of which will endeavour thereby to eſtabliſh the neceſſity of their *Materia ſubtilis*, to maintain the plenitude of the world, and the circle they attribute to moving bodies; and the latter will here triumphantly pretend to have a more illuſtrious inſtance than ever of their *vacuum coacervatum* within the world, ſince there is an impenetrable veſſel out of which it is manifeſt, that an almoſt incredible proportion of aërial ſubſtance hath been manifeſtly made to iſſue; whereas it is no ways manifeſt to any of our ſenſes, that any other body has got in to ſucceed in its room; wherefore leaving them to debate what it is that is contained in that far greateſt part of the veſſel, that the air pumped out of our receiver has deſerted, I take notice,

SIXTHLY, that to conclude with what was the main drift of this and the foregoing papers, we are here invited to obſerve, with wonder, the ſtupendous mutability of the air, as to rarity and denſity, whereby the ſame quantity of air, being ſometimes compreſſed, ſometimes dilated, may change its dimenſions to a degree that ſeems almoſt to tranſcend the power of nature and art, and by conſequence might probably be rejected as incredible, if it were abruptly and nakedly propoſed; and therefore it will be convenient to do, though very briefly, theſe two things.

FIRST, to conſider, what we have upon experience delivered in our defence againſt the learned *Linus*, touching the condenſation and rarefaction of the air, as it is expoſed to a greater or ſmaller preſſure, without the intervention of either external heat or elaborate engines. For from theſe experiments (that may be found in the lately mentioned defence *) eminent mathematicians have inferred, that one can ſcarce ſafely put determinate limits to the ſtupendous rarity, which the upper part of the atmoſphere, being almoſt totally uncompreſſed by incumbent particles of air, may be ſuppoſed to have by nature un-aſſiſted by art.

AND this is the firſt of the two things I above deſired to have taken notice of. But the other (which, though it be but the ſecond, is much the more conſiderable) is to confer together the ſmalleſt extent; to which we have reduced it by condenſation, and the greateſt, to which we have advanced it by rarefaction, after having taken notice, that according to the leaſt eſtimate of any recited in the foregoing experiments, the extenſion of the ſame air, is as 1 to 2744, or thereabouts; and if, inſtead of the moderateſt, we take the greateſt expanſion of the air, being (leaving out the odd hundreds to make the rounder number) as 13000 to 1, when the uncompreſſed air was highly rarefied, that

* Chap. V. Whoſe Title is, Two New Experiments touching the Meaſure of the Force of the Spring of the Air compreſſed and dilated.

number being multiplied by 40, becauſe of the fore-mentioned compreſſion of the air, will amount to 520000, for the number of times, by which the air at one time exceeds the ſame portion of air at another time; which is a difference of expanſion ſo great, that I hope it will keep you from thinking the title of the foregoing epiſtle, where the expanſion of the air is called admirable, immodeſt, eſpecially ſince I have forborn to mention what probable arguments might be offered to prove it at leaſt poſſible that the induſtry of men, and perhaps our own, may find means to make both the condenſation and rarefaction of the air to exceed the uttermoſt whereto we have yet been able to bring them.

P O S T S C R I P T.

Touching an Obſervation to be inſerted above, (Page 503) *immediately after the Mark* *.

SINCE the writing of this, the author chanced to find one of the lately mentioned inſtruments of a conſiderable bigneſs, which was preſumed to have miſcarried; and comparing it with a memorial made, when it was firſt completed, to keep in memory the heights, dimenſions, &c. of the incloſed mercury and air; we found, that in about ten weeks, there was not any conſiderable variation of them; and the little ſhrinking of the air, which was diſcoverable by an attentive eye, was not ſuch but that it might be probably aſcribed to the change of the weather to a far greater coldneſs, which might be ſuppoſed a little (and it did but very little) to weaken the ſpring of the included air, and conſequently abate of its full reſiſtance to the preſſure of the mercury in the longer leg of the ſiphon.

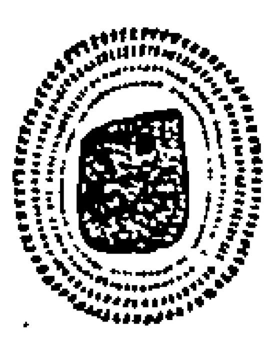
AN

AN OBSERVATION OF A SPOT IN THE SUN.

Firſt Printed in the *Philoſophical Tranſactions*, N° 74, p. 2216, for *April* the 27th, 1671.

Friday, April 27, 1660.

" ABOUT eight of the clock in the morning there appeared a ſpot in the lower
" limb of the ſun, a little towards the ſouth of its æquator, which was entered
" about $\frac{1}{40}$ of the diameter of the ſun, itſelf being about $\frac{1}{160}$, in its ſhorteſt diameter,
" of that of the ſun; its longeſt about $\frac{1}{40}$ of the ſame. It diſappeared upon Wedneſday
" morning, May 9th, though we ſaw it the day before, about ten in the morning, to
" be near about the ſame diſtance from the weſtward limb a little ſouth of its æquator,
" that it firſt appeared to be from the eaſtward limb, a little ſouth alſo of its æquator.
" It ſeemed to move faſter in the middle of the ſun than towards the limb. It
" was a very dark ſpot almoſt of a quadrangular form, and was incloſed round
" with a kind of duſkiſh cloud, much in this form, and in this proportion to
" the ſpot.



" WE firſt obſerved this very ſame ſpot, both for figure, colour, and bulk, to be
" re-entered the ſun, May 25th, when it ſeemed to be in a part of the ſame line it had
" formerly traced; and was entered about $\frac{4}{33}$ of its diameter about ſeven of the clock in
" the afternoon. At the ſame time there appeared another ſpot, which was juſt entered,
" and appeared to be entered not above $\frac{1}{132}$ part of the ſun's diameter. It appeared to
" be longeſt towards the north and ſouth, and ſhorteſt towards the eaſt and weſt.
" There ſeemed to be diſperſed about it divers ſmall clouds here and there."

[THESE obſervations were made, as the noble obſerver told us, with an excellent teleſcope, in the preſence of divers curious and ingenious perſons, one of whom was Mr. *Hook*. And diſcourſing of thoughts he had entertained touching the effects of ſuch ſpots, he ſuggeſted this inquiry, whether they might not cauſe a conſiderable alteration both in the body of the ſun itſelf, and in our air, and the bodies in it, upon their diſſipation?]

AN

AN

ESSAY

ABOUT THE

ORIGIN AND VIRTUES

OF

GEMS.

Wherein are propofed and hiftorically illuftrated fome Conjectures about the Confiftence of the Matter of PRECIOUS STONES, and the Subjects wherein their chiefeft Virtue refides.

The PUBLISHER to the READER.

THE philofophy and origin of gems, as well as their ufefulnefs and virtues, will, I am perfuaded, be found, upon the attentive perufal of this effay itfelf, fo rationally and warily delivered therein, that there will need nothing to be faid in the praife of the compofure thereof. I dare venture, notwithftanding the noble author's modefty, to prefent it to the moft critical tafte without hanging out a bufh to it.

ALL I have to fay in the publifhing thereof fhall be the fame that was alledged by the Englifh interpreter of the learned *Steno*'s Prodromus to an intended differtation of his concerning folids naturally contained within folids, printed the laft year by *Mofes Pitt* in *Little Britain*; where, in the Englifh preface, occur paffages to this effect, *viz.*

" THAT

"THAT the honourable author of this essay, before he would see or hear any thing "of that Prodromus of *Steno*, did, upon occasion, solemnly declare to the author of "that English version (who there protests that he speaks it *bona fide*) the sum and sub"stance of what is deduced at large in this tract; the manuscript whereof the said inter"preter then saw, and received it into his custody for publication; which sum was "this; first, that the generality of transparent gems have been once liquid substances, "and many of them, whilst they were either fluid, or at least soft, have been imbued "with mineral tinctures that con-coagulated with them; whence he conceives, that "divers of the real qualities and virtues of gems may be probably derived.

"SECONDLY, as for the opacous gems and other medical stones, as blood-stones, "jaspers, magnets, emery, &c. he esteems them to have, for the most part, been "earth (perhaps in some cases very much diluted and soft) impregnated with the more "copious proportion of fine metalline or other mineral juices or particles, all which were "afterwards reduced into the form of stone by the supervenience (or the exalted action) "of some already inexistent petrescent liquor, or petrific spirit, which he supposeth "may sometimes ascend in the form of steams; from whence may be probably "deduced, not only divers of the medical virtues of such stones, but some of their other "qualities, as colour, weight, &c. and also explained how it may happen, what he "hath (which he doubts not but others have done also) observed of stones of another "kind, or marchasites, or even vegetable and animal substances that have been found "inclosed in solid stones; forasmuch as these substances may easily be conceived to have "been lodged in the earth, whilst it was but mineral earth or mud, and afterwards "to have been, as it were, cased up by the supervenient petrific agents that pervaded "it.

"NOR are these petrescent liquors the only ones, to which he supposes that many "fossils may owe their origin, since he thinks there may be both metalescent and minera"lescent juices in the bowels of the earth, and that sometimes they may there exist and "operate under the same spirits and steams."

So far the preface to that translation; which is here repeated to do right to this noble author, in the matter of the theory relating to the origin both of precious and other stones. Which done, I shall keep the curious reader no longer from the contentment which he will doubtless find in the perusal of this essay.

The PREFACE.

THAT the ſcarcity, the luſtre, and precioufneſs of gems have made them in all ages to be reckoned among the fineſt and choiceſt of nature's productions, is generally granted. But whether the books that have been divulged of them be anſwerable to the nobleneſs of the ſubject, ſeems not to me ſo unqueſtionable. For, as for the origin of gems; to ſay with *Ariſtotle*, towards the cloſe of his third book of *Meteors*, that a dry exhalation, ξηρὰ ἀναθυμίασις (whether) fiery or firing (ἐκπυρῦσα) makes, among other foſſils, the ſeveral kinds of unfuſible ſtones; or to tell us, according to the more received doctrine, that gems are made of earth and water finely incorporated and hardened by cold; this, I ſay, is to put us off with too remote and indefinite generalities, and to found an explication upon principles, which are partly precarious, and partly inſufficient, and perhaps alſo untrue. And as to the hiſtory of gems, that has been ſo fabulouſly delivered, that, eſpecially among the moderns, many learned men, philoſophers and phyſicians, have, for the ſake of ſo many improbable, and ſometimes impoſſible virtues that have been aſcribed to gems, been induced to deny them any virtues at all. It is true, that I am not altogether ſo ſevere, and that the eſteem that I find made by learned men of the inquiſitive Emperor *Rudolfus*'s phyſician *Boetius De Boot*, makes me diſcriminate him and two or three modern authors, that in books profeſſedly made on other ſubjects, have written incidentally of ſome gems, from ſuch notoriouſly fabulous writers as *Mizaldus*, *Albertus Magnus* (if his name be not injured by the imputation of a ſpurious book) *Baptiſta Porta*, *Kirannides* (and ſome others that I forbear to name) from whoſe learning one would expect more warineſs and judgment. But though, for reaſons elſewhere mentioned, I do not unreſervedly think that precious ſtones, eſpecially opacous ones, can have no medical virtues at all; yet when I conſidered how difficult it was to aſſign any thing that is poſſible and intelligible (which I do not take a ſubſtantial form to be) whence their virtues may probably be derived, without giving ſome ſuch account of the origin of gems themſelves, as was not to be expected from the followers of the peripatetick, that is, the received philoſophy; I could not but wiſh that ſomething were attempted on that ſubject according to the principles of the Corpuſcularian.

THESE things made me the leſs backward to comply with the curioſity of my friends, which put me upon the following diſcourſe, wherein I was content to try, what, without ranſacking the authors that had profeſſedly written *de Gemmis*, the conſideration of the ſubject to be treated of, my natural propenſity to take notice of nature's productions, and the trial whereto theſe conſiderations and obſervations lead me, would ſuggeſt to my pen.

WHETHER my conjectures and ratiocinations be as new to others as to thoſe I chiefly wrote for, it is not my part to determine; only I deſigned to ſuit my diſcourſe to the phænomena of nature, without being ſolicitous with whom I diſagreed or complied. And therefore, though it ſhould happen that ſome conjectures of mine ſhould, unknown to me, be coincident with the opinion of ſome claſſic writer about gems; yet, I preſume, the whole ſubſequent hypotheſis, and the arguments it is founded upon, will appear to have been ſuggeſted to me by the nature of the thing itſelf, and my way of conſidering it; not to mention, that ſometimes one may meet with a good particular conjecture in an author that underſtands not the importance of it himſelf, and knows not how

how to make ufe of it, but builds it on fome fuch fabulous relation or erroneous principle, as is apt to difcredit it with wary readers, unlefs they be fuch to whom its compliance with the opinions they have on better grounds already entertained, happen to recommend it. I know it may be thought ftrange that I have been fo very fparing in the citation of thofe authors that have writ whole books about gems; but I have this to fay for myfelf, that I had neither them, nor fo much as my own papers about the origin of minerals at hand, when I writ the following effay. Which I was the lefs troubled at, upon two diftinct accounts; the firft, becaufe I remembered that feveral paffages that I had met with about the virtues of gems, cited out of divers of thofe authors, were fuch, as I fhould have much fcrupled to vouch; fome of them being fuch as I knew to be falfe; others that I fhrewdly fufpected not to be true; and others that appeared to me altogether incredible; and the fecond, becaufe, to forbear tranfcribing what my friends might probably have met with in authors already, would beft comply, both with their defires, which was to know my particular thoughts; and with my defign, which was partly to fee how far I could make out thofe thoughts, by my own arguments and obfervations, affifted only by fome very few hiftorical paffages that I lighted on in writers not claffic; and partly, to take this occafion to profecute divers matters of fact relating to the fubject I was treating of, which probably would otherwife have been quite loft. And I doubted not, but if this firft draught of my conceptions were by my friends thought worthy of being enlarged, it would not be difficult for me, when I fhould come at my books and papers again, to enrich this tract with many hiftories borrowed from famous writers; if that fhould be thought neceffary by perfons that were poffibly lefs diffident of me than of them. In fhort, I propofed this difcourfe but as a conjectural hypothefis, wherein I attempted to derive the origin of gems, and one of the main caufes (I do not fay the only caufe) of their qualities and virtues, from principles lefs remote and more intelligible than thofe of the peripateticks; and having delivered divers obfervations and experiments of my own about the phænomena of gems, to explicate fome of them by intelligible principles, and illuftrate others by refembling things, that may be really obferved in nature, or eafily performed by art. Which way of handling my fubject permitted me to hope, that, whether or no I fhould be thought a lucky conjecturer about the fubject I attempted, I fhould, at leaft, in fome meafure, prove a benefactor to what is, perhaps, preferable even to lucky conjectures themfelves, the natural and experimental hiftory of fuch noble fubjects as gems.

AN ESSAY ABOUT THE ORIGIN AND VIRTUES OF GEMS.

SECTION I.

THOUGH it will not perchance prove very difficult to propofe to you my conjecture about the caufes of the virtues of precious ftones; yet I fear it will not be eafy for me to acquaint you fully with the grounds of it. For unlefs I fhould tranfcribe for you my whole difcourfe of the origin of minerals in general (of which you know ftones make a part) I cannot well lay before you all the confiderations by which I have been induced to take up the conjecture or hypothefis I am about to propound; and confequently, I cannot well comply with your curiofity about gems, without either omitting feveral things which might much countenance the following difcourfe, or propofing (without amply proving them) fome things, that I confefs feem not clear, nor fome of them fo much as probable, by their own light. But fince you will have it fo; I will, rather than difobey you, prefent you in one difcourfe feveral things concerning gems, whereof fome belong to others of my little tracts about the origin of minerals from fluid, or at leaft foft bodies; though fome indeed were more directly written concerning gems; notwithftanding that they were delivered not as an entire tract about that fubject, but as corollaries that might be drawn from, and applications that might be made of, what had been in a more general way difcourfed about the origination of ftones and other minerals. And therefore prefuming that you will fuppofe with me in this difcourfe fome few particulars that, I think, I have elfewhere made probable, and might perhaps

perhaps do so from some of the phænomena mentioned in this writing itself, I would immediately address myself to the subject of it, if I did not think a previous admonition very requisite.

For, I must, at the very entrance of this discourse, desire you to take notice, that when I propose my conjectures about the virtues of gems, I do not suppose the truth of all, or so much as the tenth part of those wonderful properties that men have been pleased to ascribe to them. For not only some of the writers of natural magick, but men of note, who should be more cautious and sober, have delivered in their writings many things concerning gems, which are so unfit to be credited, and some of them perhaps so impossible to be true, that I hope the believers of them will, among the votaries to philosophy, be as great rarities, as gems themselves are among stones. And those that can admit such unlikely fables will be as much despised by the judicious as jewels can be prized by the rich.

For my part I never saw any great feats performed by those hard and costly stones (as diamonds, rubies, sapphires) that are wont to be worn in rings. But yet, because physicians have for so many ages thought fit to receive the fragments of precious stones into some of their most celebrated cordial compositions; because also divers eminent men of that profession, some of them famous writers, and some virtuosi of my own acquaintance have, by their writings, or by word of mouth, informed me of very considerable effects of some gems (especially crystal) upon their own particular observations; and lastly, because, that (as I shall shew anon) I find no impossibility, that at least some costly, and less hard (though indeed more valuable) gems, may have considerable operations upon human bodies, some few of which I have had opportunity to be convinced of; I will not indiscriminately reject all the medicinal virtues that tradition and the writers about precious stones have ascribed to those noble minerals; contenting myself to declare in a word, that suspecting most of them to be fabulous, my conjectures aim only at giving one of the causes of those virtues ascribed to gems, which experience warrants to be real and true.

Having thus explained in what sense my conjecture about the virtues of precious stones is to be understood; it follows, that I propose the conjecture or hypothesis itself; the substance of which may be comprized in these two particulars; first, that many of these gems and medical stones either were once fluid bodies, as the transparent ones; or in part made up of such substances as were once fluid; and secondly, that many of the real virtues of such stones may be probably derived from the mixture of metalline and other mineral substances, which (though unsuspectedly) are usually incorporated with them; and the greatness of the variety and efficacy of those virtues may be attributed to some happy concurrent circumstances of that commixture. The first of these heads relates properly to the origin of gems. The second, partly to that, and partly to the kinds and degrees of their virtues.

But that any gems, especially the hardest sorts of them, should have a later beginning than that of the earth itself, will probably be thought to relish of a paradox; and I doubt not, it will pass with many for a great one, that some of these hardest of solid bodies should have been once fluid ones or liquors; wherefore I shall endeavour to countenance this hypothesis by the following considerations.

1. And first the diaphaneity of diamonds, rubies, sapphires, and many other gems, agrees very well with this conjecture, and thereby seems to favour it. For it is not so likely that bodies that were never fluid should have that arrangement of their constituent parts, that is requisite to transparency, as those that were once in a liquid form, during which it was easy for the beams of light to make themselves passages every way, and dispose

pose the solid corpuscles after the manner requisite to the constitution of a transparent body. Therefore we see that silver in aqua fortis, or lead in spirit of vinegar, having by that solution had their particles reduced into a fluid form, those particles, though before opacous, are so disposed of as to make, not only a diaphanous solution, but, if one pleases, transparent crystals. And what chymists usually try with those metals, I have had the curiosity to try with several stones, which I may hereafter have occasion to name to you. But this argument I bring rather to confirm than evince my conjecture.

SECONDLY, the origin assigned to gems may be also countenanced by the external figuration of divers of them. For we plainly see that the corpuscles of nitre, allum, vitriol, and even common salt, being suffered to coagulate in the liquors they swam in before, will convene into crystals of curious and determinate shapes. And the like I have tried in several metalline bodies dissolved in several menstruums. But unless a concreting stone or other like body be either surrounded with, or in good part contiguous to a fluid, it is not easy to conceive how it should acquire a curious angular and determinate shape. For concrescent bodies, as I may so speak, if they have not room enough in an ambient fluid for the most congruous ranging of their parts, cannot cast themselves into fine and regular shapes, such as I shall presently show, that divers gems seem to affect; but the matter they consist of must conform to the figures of the cavity that contains it, and which in this case has not so much the nature of a womb as of a mold. And so we see, that saltpetre, and divers other salts, if the water they were dissolved in be much too far boiled away before they are suffered to shoot, will, if the liquor fill the glass, sometimes coagulate into a mass fashioned like the inside of the containing vessel, or if a pretty quantity of liquor remains after the coagulation, that part of the nitrous mass that was reduced to be concreted next the glass, will have the shape of the internal surface of it, whatever that be; but those crystals that are contiguous to the remaining liquor, having a fluid ambient to shoot in, will have those parts of their bodies that are contiguous to the liquor curiously formed into such prismatical shapes as are proper to nitre.

To apply this now to gems; that divers kinds of them have geometrical and determinate shapes, though it be not vulgarly observed, because we are wont to see them when they are cut, if not also set in rings and jewels; yet I have often had the opportunity to take notice of it, by having had the curiosity to look upon many of them rough as nature has produced them, and the good fortune to take divers of them out of their wombs. For I remember I have taken a good number of Indian granates out of a lump of heterogeneous matter, whose distinct cavities, like so many cells, contained stones, on some of whose surfaces you might see triangles, parallelograms, &c. And being once near the rock whence those stones are chiefly fetched, that are commonly called Bristol-stones, I remember I rid thither and procured a workman or two to dig me up a number of them, divers of which I found to be curiously and determinately shaped, much like some crystals of nitre that I have taken pleasure to compare with them. And the like figuration I have also observed in divers Cornish diamonds, and in a fair and large one, which one, that knew not what it was, found growing, with many lesser, in *Ireland*, and presented me. And to let you see that it is not only in these softer gems that this curious figuration is to be met with, I shall add, that I found among many stones I had, and took to be rubies (and those the jewellers will tell you are exceeding hard) a considerable number, whose shapes, though not the same with those of the Cornish and Irish stones, were yet fine and geometrical. And the like I have observed even in those hardest of bodies, diamonds themselves; of which remembering, that in my collection of minerals, I had a pretty large one that was rough, I perceived that the surface

face of it conſiſted of ſeveral triangular planes which were not exactly flat, but had, as it were, ſmaller triangles within them, that for the moſt part met at a point, and did ſeem to conſtitute, as it were, a very obtuſe ſolid angle; encouraged by this, I examined ſeveral other rough diamonds, and found the moſt of them to have angular and determinate ſhapes, not unlike that newly mentioned. And having thereupon conſulted an expert jeweller that was alſo a traveller, though he could not name to me the ſhapes of the uncut diamonds he had met with; yet he told me he generally found them to be ſhaped like that I ſhewed him; inſomuch, that ſuch a ſhape was a mark, by which he uſually judged a ſtone to be a right diamond, if he had not the opportunity to examine it by the hardneſs.

AND this I ſhall add in favour of the compariſon I lately intimated betwixt the coagulation of petre and that of gems, that having once made an odd menſtruum, wherein I was able to diſſolve ſome precious ſtones, there ſhot in the liquor cryſtals pretty large, and ſo tranſparent and well-ſhaped, that they might well have paſſed for cryſtals of nitre; and yet, if I much miſremember not, they were inſipid. And I have divers times taken notice in ſuch ſtones, as the Briſtol diamonds, that though that part which may be looked upon as the upper part of the ſtone, were curiouſly ſhaped, having ſix ſmooth ſides, which at the top were, as it were, cut off ſloping, ſo as to make ſix triangles, that terminated like thoſe of a pyramid in a vertex; yet that which may be looked upon as the root or lower part of the ſtone, was much leſs tranſparent (if not opacous) and devoid of any figuration; of which the reaſon ſeems to be, that this being the part whereby the ſtone adhered to its womb, it was ſullied by the muddineſs of it, and reduced to conform itſelf to whatever ſhape the contiguous part of the cavity chanced to be of; whereas the upper part of the ſtone was not only formed of the clearer part of the lapideſcent juice before the wateriſh vehicle was exhaled, but had room and opportunity to ſhoot into the curious figure belonging to its nature. And this is much more conſpicuous where many of theſe cryſtals grow, as it were, in cluſters out of one mineral cake or lump; as I have ſeen not only in thoſe ſoft but yet tranſparent concretions, which ſome of the later mineraliſts (for the ancients ſeem ſcarce to have known them) call fluores, and particularly in a very fine mineral lump, that I had once the honour to have ſhewed me by a great prince, and no leſs great a virtuoſo, to whom it was then newly preſented. For this maſs conſiſted of two flat parallel cakes, that ſeem compoſed of a dirty kind of cryſtalline ſubſtance, and out of each cake there grew, towards the other, a great number of ſtones, ſome of which, by their coheſion, kept the two cakes together; and moſt of theſe ſtones, having each of them a little void ſpace about it, wherein it had room to ſhoot regularly, were geometrically ſhaped, and, which looked very prettily, were coloured like a German amethiſt. And I have myſelf a pretty large ſtone taken up here in *England* by a gentleman of my acquaintance, which conſiſts, as it were, of four parts; the lowermoſt is a thin and broad flake of coarſe ſtone, only adorned here and there with very minute gliſtering particles, as if they were (as probably they may be) of a metalline nature; over this is ſpread another thin white, but opacous bed, which is ſo incloſed between the firſt named bed and the two others, that, without defacing the ſtone, I cannot well examine it: the third conſiſts of a congeries of minute cryſtals exceedingly thick ſet, which therefore look whitiſh, having little or no tincture of their own; and this part, no more than either of the former, is not much thicker than a barley-corn. The fourth and uppermoſt part, which yet ſeems in great part to be the ſame cryſtals, which, as they grow higher and ſpread, acquire a deeper colour, is made up of a great number of amethiſts, ſome paler, and ſome highly tincted, which are of very differing figures and bigneſſes, according (as one may gueſs) as they had conveniency to ſhoot; theſe at one end of the ſtone

ſtone lying in a flat bed, as it were, and ſcarce exceeding a barley-corn in length; whereas thoſe at the other end ſhoot up to a good height into figured cryſtals, ſome of them as big as the top of my little finger, and thoſe are the moſt deeply coloured, being alſo of a good hardneſs, ſince I found that they would eaſily grave lines upon glaſs.

I REMEMBER alſo, that going to viſit a famous quarry that was not very far from a ſpring, which had ſomewhat of a petreſcent faculty in it, I cauſed divers ſolid pieces of rough and opacous ſtones to be broken, out of hope I had to find in them ſome finer juice coagulated into ſome finer ſubſtances; and accordingly I found, that in divers places, the ſolid and maſſy ſtone had cavities in it, within which, all about the ſides, there grew concretions, which, by being tranſparent, like cryſtals, and very curiouſly ſhaped, ſeemed to have been ſome finer lapideſcent juice, that by a kind of percolation through the ſubſtance that groſſer ſtone was made of, had at length arrived at thoſe cavities, and upon the evaporation of the ſuperfluous and aqueous parts, or by their being ſoaked up by the neighbouring ſtone, had opportunity to ſhoot into theſe fine cryſtals, which were ſo numerous, as quite to overlay the ſides of the cavities, as I can ſhew you in ſome large cluſters of them that I brought from thence. And enquiring of an ancient digger, whether he had not ſometimes met with greater quantity of them? he told me that he had, and preſented me a great lump or maſs made up of a numerous congeries of ſoft cryſtals (but nothing ſo colourleſs as theſe other newly mentioned) ſticking to one another, but not any of them to any part of the rock; ſo that they ſeemed to have been haſtily coagulated in ſome cleft or cavity, as it were, in a mold, where meeting and mingling before concretion with ſome looſe particles of clay, the maſs may thereby be diſcoloured.

OUR argument, drawn from the figuration of tranſparent ſtones, may be much ſtrengthened by the coalition I have ſometimes obſerved of two or more of ſuch ſtones, and the congruity in the ſhape of ſome of them to the figures of thoſe parts of the others that were contiguous to them, and ſeemed to have been formed after them. But though this phænomenon be conſiderable to the ſcope of my diſcourſe, yet perceiving that I ſhall have occaſion to inſiſt on it hereafter, I ſhall not do it now.

THIRDLY, nor is it only the external figuration of theſe gems, but the internal texture that favours our hypotheſis, ſome of them ſeeming much to imitate in their coagulation ſeveral of thoſe ſubſtances, which I have obſerved to have once been fluid. That common ſalt may be made up of ſmall ſaline particles, that by a convenient juxta-poſition may be aſſociated into great lumps, divers of which are cubically ſhaped, is an obſervation eaſy enough to be made. And that ſuch coalitions of particles may conſtitute ſolid and conſiderably hard bodies, I have tried by breaking ſome of the larger cubes of ſal gem, and the lumps of the iſle of *Mayo* ſalt, whereof the firſt is foſſil, the other marine, and both natural. I have likewiſe found by trial, that, though ſilver diſſolved in aqua fortis appears uſually to ſhoot, if it be taken notice of, into flat and exceeding thin flakes; yet it is very poſſible ſo to order the coagulation, that many of theſe thin plates ſhall, in their convention, have their flat ſides ſo placed over one another, as to make up pretty large and thick cryſtals, whoſe very outſides will be finely ſhaped, as being ſome peculiar kind of vitriol. Nor are theſe the only fluid bodies, which I have reduced to coagulate into conventions, of ſuch a flaky texture; wherefore I began to ſuſpect that divers tranſparent minerals may have the like; and in ſome diaphanous kinds of talk, whoſe outſides were mathematically figured, I found encouragement to try, whether even ſome gems themſelves, notwithſtanding their hardneſs, might not have ſuch an internal figuration. Nor was I deterred by conſidering that it is taken for granted, that gems are of an uniform texture, and that there muſt be a ſtrange thinneſs in the plates, that make up tranſparent ſtones, ſince no ſuch thing has been noted by the moſt curious eye,

but

but men have taken it for granted, that the texture of all gems is uniform, without any grain or fibres, no more than there is in gold. But as to the thinness of the plates, I remember, I have several times taken pleasure to hold a piece of good Muscovia glass against the light, when it was of such a thinness, that the spectators, though provoked to look with curious eyes, could scarce see the plate itself, and would by no means be brought to think that it was possible to split it, till I did actually do it; and sometimes I then subdivided it beyond even my own expectation. But to examine this conjecture, I took some stones that had geometrical figures on part of their surfaces, and which I had other grounds to think to have been once fluid substances, and having diligently surveyed some of them which seemed likeliest to give me satisfaction, I manifestly enough perceived, not only with my assisted, but with my naked eyes, divers parallel commissures, which seemed plainly to be made by the contiguous edges of little thin plates of stone, that appeared to lie one over another, almost like the leaves of a book that is a little opened.

I REMEMBER, that holding a large and rough grizolette (as artificers call hard gems of a blueish colour, brought them from *East-India*) against the light, and curiously observing it, I have sometimes discerned a grain, as they call it, in the stone, and was answered by a skilful artist that used to make seals of them, that such stones would usually split, according to the ductus of their grain. I will not urge, that in some other precious stones that were cut and polished, as particularly the hyacinth, and even the sapphire, by obverting them several ways to the light, I have been able to observe, as it were, commissures, which were so fine as not to hinder, or call in question the intireness of the stone, for the lapidary's purpose. This, I say, I forbear insisting on, because the phænomenon is far less considerable than what I have several times observed in New-English granates, wherein, especially when they are broken, the edges and commissures of the thin plates or flakes whereof they consisted, were very easily discernible. And, to try whether this observation would hold even in the hardest stones, I had recourse to a pretty big diamond unwrought, which being placed in a microscope, shewed me the commissures of the flakes I looked for, whose edges were not so exactly disposed into a plane, but that some of them were sensibly extant like little ridges, but broad at the top above the level of the rest. And these parallel flakes, together with their commissures, I could in a somewhat large diamond plainly enough discern, even with my unassisted eyes. And for further satisfaction, I went to a couple of persons, whereof the one was an eminent jeweller, and the other an artificer, whose trade was to cut and polish diamonds, and they both assured me upon their repeated and constant experience, and as a known thing in their art, that it was almost impossible (though not to break, yet) to split diamonds, or cleave them smoothly cross the grain, if I may so speak, but not very difficult to do it at one stroke with a steeled tool, when once they had found out from what part of the stone, and towards what part the splitting instrument was to be impelled; by which it is evident, that diamonds themselves have a grain, or a flaky contexture, not unlike the fissility, as the schools call it, in wood; which you will easily grant to consist of assimilated water or juices; which having been once fluid bodies, were fit to have their particles so ranged or disposed, as to constitute a body far more easy to be cleft according to the ductus of the fibres or planes, than otherwise. And I remember, that having, as I thought, observed in a rough diamond, which I purposely examined, that the flakes, whose edges were terminated in one plane, were far enough from being parallel to those, whose edges composed another plane (I speak of physical planes of the same stone) I imagined, that if this diamond were to be cleft, it would not be smoothly split into two pieces, because the commissures did probably make angles in the body of the ſtone; and accordingly I learned of the ancienteſt of theſe diamond-cutters, that ſometimes he met with ſtones that eluded all his ſkill, and would by no means be ſplit like others into two parts, but, before they were cleft quite through, would break in pieces; which was a defect in the ſtone he could not certainly foreſee, but was fain to learn from the unwelcome event.

Fourthly, it ſeems not unprobable, that the colours of divers gems (for I do not ſay of all) are adventitious, and were imparted to them, either by ſome coloured mineral juice, or ſome tinging mineral exhalation, whilſt the gem or medical ſtone was either in *ſolutis principiis*, or of a texture open enough to be penetrable by mineral fumes. Which argument's conſiderableneſs makes me hold it unfit to be lightly touched in this place; though I cannot diſcourſe any thing fully of it in few words, becauſe it not only ſuggeſts divers obſervations and other particulars, but requires alſo the mention of ſome of the chief of them; which therefore I ſhall now ſubjoin.

1. And the firſt ſhall be, that many gems, not to ſay almoſt all of them, have been obſerved to be deprived of their colour, if having fallen, or been put into the fire they have lain too long there; inſomuch, that I have found it affirmed upon the teſtimony of the learned and experienced *Boetius de Boot*, that all gems will loſe their colour in the fire, except Bohemian granates. How far this may be true, I have not had opportunity thoroughly to examine. But I well remember, that having purpoſely expoſed divers gems to the fire, though that were but moderate, and had a crucible interpoſed between it and them, ſome of them ſeemed to have their tincture much impaired, and others quite deſtroyed. But I muſt be ſo free as to admoniſh you, that if theſe trials be not warily made, they may eaſily impoſe upon us; eſpecially if we do not conſider the nature and cauſe of whiteneſs. For any diaphanous body, as far as I have yet obſerved, being divided into a multitude of very minute parts, and conſequently acquiring a multitude of diſtinct ſuperficies, which do briſkly reflect the light every way outwards, will appear to have a white colour that will be more or leſs vivid, as the particles are more or leſs numerous, minute, and otherwiſe fitted to ſcatter the incident beams of light; as you may ſee by reducing to powder fine Venice-glaſs, which will be white; and even red ink, if ſo ſhaken or beaten as to be brought to a froth, conſiſting of many minute bubbles, will ſeem to have put on a whiteneſs. So that if by too haſty an ignition, or too haſty a cooling of the fired gems, they come to be flawed with innumerable little cracks, they may be thought to be made white, by having their tincture driven away, when their whiteneſs really proceeds from the multitude of thoſe little flaws which are ſingly unperceived; and the rather, becauſe the body may ſtill retain its former ſhape or ſeeming intireneſs. To illuſtrate which, I have ſometimes taken pleaſure to heat a piece of cryſtal red hot in a crucible, and then quench it in cold water; for even when the parts did not fly or fall aſunder, but the body retained its former ſhape, the multitude of little cracks that were by this operation produced in it, made it quite loſe its tranſparency, and appear a white body. In making which experiment, the multitude of produced flaws may be pretty well diſcovered to the incredulous, if, as I have ſometimes done, the ignited cryſtal be warily and dexterouſly quenched, not in water, but in a very deep ſolution of cochineal made with ſpirit of wine, in which operation, if it be well performed, but not otherwiſe, enough of the red particles of the ſolution will get into the cracks of the cryſtal to give it a pleaſing colour.

Tincture of coral. The other trials that I have made about the reducing of whiteneſs or paleneſs in bodies, either tranſparent, or even ſemi-diaphanous only, belonging to another paper, I ſhall here forbear to mention them, having already ſaid enough for my preſent purpoſe, which is not ſo much to affirm poſitively, that no proof at all can be drawn from the operation

operation of fire upon the colour of gems, as to make you cautious, what proofs drawn from thence you admit.

2. Wherefore declining to say any thing more about the first, I shall now proceed to the next circumstance, that belongs to our argument (which you may think to be more considerable than the former) namely, that the colours of several gems, when they are not destroyed by fire, will be altered thereby; which being a thing that happens to divers fossil pigments (of which some I employ to tinge glass) and other bodies confessedly mineral, argues a commixture of mineral substances in those stones, whose colour receives some of the alterations I speak of: which last words I add, because I would not impose upon you, by concealing, that there may be a great change of colour produced by the fire, without any alteration of the tinging parts as such. For by flawing the heated gem in very many parts, a degree of whiteness or paleness emerging thereupon may somewhat change the former colour. But this alteration being but a kind of dilution is not that, which I here mean. For I remember, I have taken Indian granates, and having in a crucible exposed them to the fire, I found they had exchanged their reddish colour for a dark and dirty one, like that of iron that has been long kept in the air. And having taken some pieces of agate prettily enough adorned with waves of differing colours, and kept them a competent time (for they should not be kept too long) in the fire, I found, as I conjectured, that the greatest part of the agate seemed to be deprived of its tincture, being reduced to a pleasant whiteness. But in some places, where there were stains of a different kind from the rest, and where there ran little veins, that I guess to be of a metalline nature, there, I say, the colour was not destroyed, but changed, and the veins of pigment, thus coloured, acquired a deep redness, which they will retain, if let alone; though I was induced to think by some trials made on other pieces of Indian agate, that even these metalline tinctures were not so fixed, but, that a lastinger fire would drive them away, and leave the stones purely white. Such a change of colour, as I lately mentioned in the veins of agate, is likewise found in those of some other stones, as also in some pebbles, amongst divers of which, that lost only their transparency by ignition and extinction in water, one or two acquired so much deeper a colour than it had before, that I thought it remarkable.

3. Another circumstance, that seems to favour our conjecture, may be this, that it has been observed not unfrequently, that near many of the places, where coloured gems are found, some mines or veins of metals are to be met with. And I think it not unlikely, that if search were skilfully made, many more discoveries would be made of veins either of metalline ore, or some other mineral, liquid or concreted, whence, by way of juices or fumes, the gems may be presumed to have received tinctures. But usually, where precious stones are found, men's industry and curiosity is too much confined to those rich minerals, and does not make them solicitous to look after inferior ones. Besides that in *East India*, whose countries are best for the most gems, they are wonderfully unskilful at digging mines; as I have gathered from the answers of some, who purposely went to visit the diamond mines, as they call them. To this may be also referred, that gems are several times found in the metalline veins themselves, or very near them; as I can shew you divers amethysts, that an ingenious gentleman of my acquaintance took himself out of a piece of ground abounding with the ores of iron and tin, the latter of which was there plentifully dug up. And in those colder countries, such as *Germany* and *England*, where hard gems are more unfrequent, those soft ones, that mineralists call fluores, are often to be found in, or near metalline veins, so finely tincted by mineral juices, that, were it not for their softness, they might pass, at least among most men, for emeralds, rubies, sapphires, &c. as I have been informed, not only by

ſome mineral writers of good credit, but alſo by eye-witneſſes, and partly by my own obſervation.

4. The fourth circumſtance, which may be alledged to the ſame purpoſe with the three foregoing, is, that it ſeems poſſible, from ſome gems, by menſtruums, to obtain tinctures, that ſeem rather extractions, than diſſolutions, ſtrictly ſo called. I will not urge the chymical proceſſes, that may be met with in ſome authors to this effect; becauſe ſome circumſtances in the things, and in the writers, made me ſo far ſuſpect thoſe I could try (and thoſe that required undiſcovered menſtruums, as they may be true, ſo, for aught I know, they may not) as to keep me from meddling with them. But I remember, I once made a menſtruum (I ſay once, becauſe its preparation is ſo ſubject to caſualty, that I have often failed in it) which being poured upon well coloured granates, not only not calcined, but entire, was in no long time beautified with a high and lovely tincture, which was admired by very ſkilful perſons, to whom I ſhewed it, becauſe the menſtruum was not more corroſive than white-wine; and which yet I therefore took to be a genuine tincture, partly becauſe it was drawn in the cold, partly becauſe the liquor would not tinge itſelf by ſtanding, if no body were put in it; and partly becauſe it drew a tincture from antimony of a very differing colour from this we ſpeak of. Nor are granates the only gems, which I have made the liquor work on, in the cold.

5. To theſe four circumſtances I ſhall add this fifth, that ſome gems, which jewellers affirm, without ſcruple, to be rubies, ſapphires, &c. either are colourleſs, or have other colours, than thoſe that are wont to belong to them. That famous goldſmith *Benvenuto Cellini*, in his little Italian tract of his own profeſſion, admoniſhes his reader, that there are one kind of rubies, that are naturally white (and not made ſo by art) which he proves by the degrees of hardneſs peculiar to rubies. And the ſame author elſewhere tells us of beryls, topazes, and amethyſts, that are white. And it ſeems, by what he ſays not far from that place, that the Italian jewellers did not look upon the tinctures of gems, as any thing near ſo eſſential to them, as they are commonly reputed, ſince they reckon topazes and ſapphires, whereof one is blue and the other yellow, but both extremely hard in compariſon of other gems than diamonds (and perhaps rubies) to be of the ſame ſpecies. The degree of hardneſs of rubies and ſapphires is oftentimes ſo equal, that I knew an expert Engliſh jeweller, who for that only reaſon (for he knew not whence the difference of colour might proceed) took rubies and ſapphires to be of the ſame kind of ſtone.

And that gems, referred by lapidaries to the ſame kind, may be very differingly tinged, is a truth, whereof I have ſeen notable inſtances in diamonds themſelves; which I therefore prefer to other inſtances, becauſe the extreme hardneſs of diamonds is ſuch, as keeps jewellers from miſtaking any other ſtone for a true diamond, if they are permitted to put them on their rapidly moved wheels employed to cut them. Now, of true diamonds I have ſeen ſome, that were yellowiſh; others, that were more yellow; and, among the reſt, one that was ſo perfectly yellow, that I firſt took it for a fair topaz, though it were a diamond valued at near three pounds weight of gold. I have alſo ſeen diamonds, and thoſe rough, as they came directly out of the *Indies*, and were ſoon after bought by traders in diamonds for ſuch, which were either blueiſh or greeniſh. And I particularly contemplated one ſtone, which, if its ſhape and other things had not convinced me of the contrary, was ſo green, that I ſhould have taken it for an emerald.

I remember I had once occaſion to buy a conſiderable number of ſmall rubies, divers of which were very curiouſly ſhaped; and coming to look upon the whole parcel more leiſurely,

leisurely, than my haste would permit me when I bought it, I found in a great number of other stones, one, and but one, that was devoid of any colour; but in any other respects was so like the rest, as invited me to conclude, that it would have encreased their number, but that it was coagulated and hardened before the mineral pigment had tinged it of the same colour with the rest. In which guess I was confirmed, when having met with a gentleman, who had been in the chief places of the *East-Indies*, where rubies are found, and particularly at the river of *Siam*, or *Pegu*, near which he lived a good while, and where he frequently saw rubies taken out of the bottom of the water, and sometimes took them out himself; I learned of him by enquiry, that he had there seen several stones, each of which was partly a ruby, and partly colourless: and sometimes in the same stone, there would be two portions of one sort, and the third, though lying betwixt them, of another; which has frequently obliged the jewellers considerably to lessen the bulk of such stones, by cutting off the untincted part. And, if my memory do not much deceive me, I saw, in a great and curious prince's cabinet, among other rarities, a ring, in which was set a stone of a moderate bigness, whereof only one half, or thereabouts, was well tincted, the other being colourless. In gems, that are less precious, and not so transparent, especially in agates, and in opacous gems, I could easily give a multitude of instances of the differingly tincted parts of the same entire stone. And I usually wear in a ring a small sardonyx, that was once a great prince's, wherein there are three portions, one within another, the uppermost black, the middlemost of a kind of chesnut colour, the other of a blue, almost like a turquois; each of which portions is exactly of a fine oval figure, and each of the two uttermost is throughout of a very uniform breadth as well as colour, and exactly parallel to the other. But it would not be here so proper, as it will be hereafter, to multiply instances of opacous gems: wherefore (having mentioned only the sardonyx, because it is not always opacous) I shall add concerning transparent ones, that jewellers reckon among sapphires not only that sort of azure gems, which usually pass for such, but also another sort of stones, because of their sapphirine degree of hardness; though for their want of tincture, they call them white sapphires.

6. The sixth and last circumstance belonging to the foregoing argument or consideration is this, that sometimes one may find gems, that are partly tincted and partly not; as if the tinging pigment mixing with one part of the matter, whereof the stone consisted, whilst it was liquid or soft, were not copious enough to diffuse itself to the whole, nor to give an equal intense colour to all that portion that it tinges. It is true, that in some cases, the diffusion may be stopped by the petrescent juices coagulating first in another part than that with which the tincture was mixed. And perhaps, in some other cases, the different colours may have belonged to differing portions of matter, coagulating upon or against each other, at different times, yet so as to seem one intire stone; as I may have hereafter occasion to declare. Yet since, which soever of these explications be admitted, it will, if it belong not to this place, at least, confirm our main hypothesis (of the origin of gems from fluid or soft materials) I shall return to what I was saying about gems, partly tincted, and partly colourless. And having only intimated upon the by, that in some hard semidiaphanous stones, European and East-Indian, I have observed a very unequal and irregular diffusion of the tincture; I shall add to the things, that may be gathered in favour of the proposed conjecture from some of the things before (as also since) related, these two particulars:

The one, that I have (as I think I elsewhere mentioned) seen in *Italy*, among rarities, a large piece of crystal about the bigness of my two fists, whereof the pyramidal part was of a transparent green, the vertex being richly tinged like an emerald; but the further

further the colour spread from the vertex, the fainter and paler it grew; so that, before it came near the base, it was quite spent, if I may so speak, leaving the bigger part of the stone transparent, but colourless, like ordinary crystal. And by this, perhaps, we may explain an expression of *Josephus Acosta*, where he says, that emeralds grow in stones like unto crystals; and that he had seen them in the same stone fashioned like a vein; and they seem, adds he, by little and little to thicken and refine. And in the same place, this learned author has a memorable observation, that may confirm both what I have just now related, and what we mentioned a little above, about colourless gems: I have seen, says he, some that were half white and half green; others all white, and some green and very perfect. And this is the first particular I was to mention.

The other is afforded me by the way I have used, and elsewhere described, of giving to pieces of rock crystal passably good tinctures by mineral fumes. And supposing the thus coloured pieces to be as intire stones, as the beholders have generally believed them, the instance will be pertinent to our purpose in spite of an objection. For though the colours thus given are not wont to pervade them very deep, and have their penetration assisted by no faint degree of heat; yet it is to be considered on the other side, that these pieces of crystal had attained their full hardness, and after their colouration, are cut and polished like other crystals: whereas the gems, that our conjecture means, are supposed to have been tinged under ground, when they were yet fluid, or at least soft. That there are sometimes generated in the bowels of the earth, mineral exhalations capable of applying themselves to the stones they meet with there, I have, in another discourse, sufficiently declared. That also some hard and stony substances have been actually tinged with such mineral steams, I shall, in the subsequent part of this discourse, have occasion to take notice. And I remember too, that even in so hard a gem as a sapphire, I have observed the efficacy of these subterraneal fumes; having divers times seen one of those stones, wherein a fine seal was cut, which continued so oddly tinged, notwithstanding what had been taken off to reduce it to an exquisite shape, that having inquired of a skilful person of my acquaintance, by whom it had been engraven, he both assured me, that he had found it of the full hardness of a sapphire, and confessed to me, that the mineral fumes had so oddly tinged it, that in his opinion, it might, by the looks, pass rather for a Chalcedonian.

Of subterraneal fires, &c.

And now, Sir, I fear I may need your pardon, for having been so prolix in discoursing of one of the particulars belonging to our argument; to excuse which, I have no other apology to make, but that I hope what hath been delivered will scarce seem impertinent, and that I might easily have made it more tedious, if, to decline doing so, I had not purposely made some omissions.

Having then said thus much about our fourth consideration, I proceed now to add, in the fifth place, on the behalf of the hypothesis hitherto favoured, an argument, which I presume you will not think inconsiderable; namely, that solid gems may include heterogeneous matter in them. Several instances of this sort, in opacous stones, I elsewhere recite upon my own observation; but in transparent ones they are very great rarities; and therefore it will not, I presume, be thought strange, if I mention but a few.

First then, on this occasion I remember, that a very ingenious and qualified lady, who had accompanied her husband in an embassy to a great monarch, assured me, that she brought thence, among several rich presents and other rarities (some whreof she shewed me) a piece of crystal, in the midst of which there was a drop of water, which by its motion might be very easily observed, especially when the crystal was made to change its posture. And, if my memory deceive me not, I have, in some pieces of rock-

rock-cryſtal, taken notice of things, that ſeem to argue, that ſomewhat or other was intercepted within the body of the ſtone.

A curious perſon, that traded much, and was very ſkilful, in Indian gems, particularly grizolettes, which he got from the Indies, and whereof he ſhewed me the largeſt I have yet ſeen, being aſked by me, whether he had ever found in them any heterogeneous ſubſtance, which ſomething, I had obſerved, made me ſuſpect, that ſome of them might harbour, notwithſtanding their hardneſs; he averred to me, that among divers rough ones, that were brought from the *Indies*, he had with wonder ſeen one, that was about the bigneſs of a philbert, in the ſolid ſubſtance whereof there was a cavity with a certain liquor in it; which, by changing the poſture of the ſtone, might be made to move to and fro in the cavity: and when the drop was ſettled, it was of the bigneſs of a round pearl, that he ſhewed me, which wanted ſomewhat of a moderate ſize for a neck-lace. And when he had anſwered the queſtions I propoſed him, to clear my doubts, he added, that this rarity made the ſtone, which was otherwiſe of a ſmall value, prized at an hundred pounds. And I have myſelf ſeen a monſtrous gem, if I may ſo call it, and little leſs a rarity than the former, that an acquaintance of mine had bought (as I afterwards learnt) from this relator; whoſe narrative about the grizolette I think the more credible, becauſe that having had the curioſity to break a ſtone, that was brought as a rarity from the *Eaſt-Indies*, where gems are often harboured in ſuch ſtones, I found in the ſolid ſubſtance of it (which was ſo hard as to ſtrike fire, like a flint, and in its little flakes was at leaſt ſemi-diaphanous) a cavity, wherein were coagulated very minute but poliſhed and cryſtalline ſtones, which ſeemed to have their points inwards; which argued, that there had been ſome liquor, in which theſe gliſtering particles had ſhot, though in proceſs of time the remaining and incoagulable part of it may have been imbibed by the ambient matter, if not have eſcaped thorough it, by virtue of ſome peculiar congruity of it with the pores of the ſtone. Which need not be thought impoſſible, ſince experience has aſſured us, that ſome ſolid ſtones, and even gems, may be, though ſlowly, penetrated, or have their texture altered by common water. Nor are theſe the only heterogeneous ſubſtances I found included in this ſtone.

And if, as amber is reckoned among gems, and is ſometimes of a greater hardneſs, than one would expect, ſo I could reckon it among true ſtones, it were eaſy for me to borrow thence a great confirmation of what I have been ſaying; and, however, it will afford me an illuſtration of it. For, not to mention many things, of what I elſewhere recite myſelf to have ſeen in amber, I have now by me a fine piece of clear and ſolid amber (preſented me by a perſon no leſs extraordinary than it) in which is included a large intire fly, in ſhape and ſize much like a graſs-hopper, but variouſly and curiouſly coloured, with his wings diſplayed.

To theſe obſervations I ſhall add only this; that I have had myſelf, and ſhewn to others, one of that ſort of pale amethyſts, that ſome call white amethyſts; which had been cut, to be ſet in a ring, or turned into a ſeal, and was, like that ſort of gems, ſo hard, that I could readily cut glaſs with it, and yet in the body of this ſtone there appeared to be a conſiderable number of things, that looked juſt as if they had been hairs, ſome of them lying parallel, and others inclining to one another; and having contemplated them, as well by day-light as candle-light, and in divers poſitions in reference to the light and the eye, ſome of them ſeemed at times to be of a lovely reddiſh colour, but reflecting the light, as if they were well filled either with air or water: but for the moſt part they did, as I was ſaying, ſeem to be hairs of a browniſh colour, which made the ſtone not a little wondered at, even by curious and ſkilful men. I leave you to judge, whether it will be fit here to add, that I have ſometimes ſuſpected, that even

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in diamonds themſelves there may poſſibly be found intercepted, or mingled with a pure lapideſcent ſubſtance, ſome particles of heterogeneous matter. And that in this ſuſpicion I was ſomewhat confirmed, as by the odd clouds I had obſerved in an extraordinary diamond, and by ſome hydroſtatical, and other obſervations I made about thoſe ſtones (ſome of which I found heavier than either cryſtal or white marble); ſo by my having purpoſely demanded of an ancient cutter of diamonds of great practice and experience, whether he obſerved not a ſenſible difference of weight among diamonds of the ſame place: for to this he replied, that he had, eſpecially in thoſe that were cloudy or foul; inſomuch, that ſhewing me a diamond, that ſeemed to me to be about the bigneſs of two ordinary peas, or leſs, he affirmed, that he ſometimes found in diamonds of that bigneſs about a carrat (which is by common eſtimation four grains) difference in point of weight.

SIXTHLY, the laſt argument I ſhall employ to ſhew, that the matter of divers gems may have once been fluid, may be taken from the proofs you will meet with (in the following part of this tract) of the ſecond member of our hypotheſis. For if it ſhall appear, that ſeveral even of the tranſparent gems have metalline or other extraneous mineral bodies mingled with them, *per minima*, it will be very agreeable to reaſon to ſuppoſe, that ſuch a mixture was made, when the mingled bodies were in a fluid form; ſince, beſide that one may well aſk, how elſe the metalline corpuſcles came to be conveyed into ſuch compact and hard bodies as gems, it is very eaſy to conceive, if our hypotheſis be admitted, and very hard otherwiſe to apprehend, how among bodies, that differ *toto genere*, as metals and ſtones, there ſhould be made mixtures ſo exquiſite, as many of theſe appear to be, partly by the uniform coloration of the gem, and partly by the diaphaneity retained notwithſtanding this diſperſion of mineral pigments through the whole maſs; and in many inſtances alſo by the curious figuration, that we have lately been diſcourſing of.

POSTSCRIPT.

To all the foregoing circumſtances, I can now add ſomething, that I met with, ſince I thought to conclude with the laſt of them, and that tends highly to the confirmation of our hypotheſis. In a tract, that makes part of a ſmall book freſhly publiſhed in French, principally to acquaint men with the ways of eſtimating gems, according to the rates of modern jewellers, the anonymous, but curious author, takes occaſion, to give us, from the mouth, as he affirms, of the famous late travellers he converſed with in divers places, and whoſe relations are indeed the recenteſt I have ſeen in print, an account of the number, and names of the places, where diamonds and rubies are found in the *Indies*; adding ſome circumſtances and particularities about the qualities of the ſoil in thoſe places, that I have not elſewhere met with. This author, then, ſpeaking of the firſt of thoſe three diamond-mines, which he makes to be the only ones in the *Eaſt-Indies*, having told us, that the ſtones are there found, ſome in the ground, and ſome in the rock, ſubjoins, that thoſe that are drawn from the rock, or the neighbouring parts, have ordinarily a good water; but for thoſe, which are drawn out of the ground, their * water partakes of the colour or ſoil, wherein they are found. So that if the earth be clean and ſomewhat ſandy, the diamonds will be of a good water; but if it be fat or black, or of another colour, they will have ſome tincture of it. Nay, he

* Que s'il y a quelque ſable noir ou rouge parmi la terre, le Diamant auſſi en aura quelqu'un, p. 9.

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immediately annexes, that if there be ſome black or red ſand among the earth, the diamond will alſo have ſome grain of it. And elſewhere mentioning the ſecond mine of diamonds, which the natives call gems, he admoniſhes his reader, that in this, as in the mine of *Viſapour*, which is that formerly mentioned, the ſtones partake of the quality of the ſoil where they are found; ſo that if that be boggy or moiſt, the ſtone will incline to blackneſs, and if it be reddiſh, it will have an eye of that colour. Elſewhere he tells us, that of late years there were found in the kingdom of *Golconda* ſtore of diamonds, which were brought to the Nababe, or firſt miniſter of ſtate, who forbad the Page 18, making any further ſearch after them, finding not one in the whole number to have a 19. good water, all of them being black or yellow. But by the way, whereas this author Page 37. affirms it as a clear truth, that as gold is the heavieſt and moſt precious of metals, ſo diamonds are the hardeſt and heavieſt of all ſtones, he muſt excuſe me, if I declare, that what he aſſerts agrees not with my experience, who have tried the weight of an uncut diamond hydroſtatically, having taken ſuch a courſe to eſtimate its ſpecifick gravity, as I find not to have been yet taken by any other, and which you will eaſily grant to be more exact than any other of the known ways can be.

The argument that hath detained us all this while, comprized ſo great a variety of matter, and may, I hope, perform ſo great a part of my taſk in this diſcourſe, that though I ſhall not apologize for having dwelt ſo long upon it, yet I ſhall think myſelf obliged to make ſome amends for my paſt prolixity, by being ſuccinct in the remaining part of this treatiſe; and therefore, having left off with an intimated promiſe to ſhew more fully, that divers gems contain metalline, or other mineral ſubſtance, in them, I ſhould immediately connect thoſe arguments to what hath been lately ſaid, but that I think it altogether requiſite, to make way for what is to follow, by firſt taking notice of a main objection that may be urged againſt the doctrine we have been propoſing.

This is taken from the figuration of ſome gems, and eſpecially the priſmatical one of cryſtal, and ſeems the more fit to be urged againſt us, becauſe we ourſelves have, in the ſecond of the above-recited arguments, given ſeveral inſtances of it. For it ſeems ſcarce poſſible, that ſo curious a ſhape ſhould be ſo uniformly produced in ſuch a multitude of cryſtals, great and ſmall, unleſs there were ſome ſeminal and plaſtick power to faſhion the matter after ſo regular and geometrical a manner.

But he that ſhall attentively conſider, what I elſewhere ſay concerning the figuration of ſalts, and of metalline and other magiſteries diſſolved by, and concoagulated with ſalts, may be very much aſſiſted to diſcover the invalidity of this objection. But yet, becauſe I confeſs it is very ſpecious, if not important, I am content here to conſider it a little more particularly.

To this plauſible objection then I have two or three things to anſwer; firſt, that there is no abſurdity to conceive, that, if there be a ſeminal and plaſtick power in mineral bodies, it may be harboured in liquid principles, as well as otherwiſe. For we ſee, that the ſeed of animals, which oftentimes, as in elephants, rhinoceroſes, &c. produces hard and ſolid bones, teeth, and horns, is at firſt but a liquid ſubſtance; and the formative power in ſome trees and their fruits does convert the alimental juice into woods, ſhells, and other bodies very ſolid and ponderous.

But ſecondly, I elſewhere * ſhew, that even in the figures of alum, vitriol, and other ſalts, that are ſo curiouſly and geometrically ſhaped, there is no neceſſity to fly to a diſtinct architectonick principle; but that thoſe bodies themſelves may receive their ſhapes from

* See the Origin of Forms and Qualities, now publiſhed by the Author.

the coalition of ſuch ſingly inviſible corpuſcles, as by the motion of the fluid, wherein they did ſwim, and by divers aſſiſtant circumſtances, are determined to ſtick together rather in that manner, than in another. That this may be applied alſo to other bodies, I ſhall need to ſhew in this place, by no other inſtance than that of the ſalt, that (in this or ſome other paper) I formerly told you I made of common ſalt, only by the help of oil of ſulphur, or of vitriol and water. For though it be manifeſtly a factitious body compounded of ſalt and ſulphur, and ſuch a body, that therein the ſea-ſalt, whereof it was chiefly made, has had its own nature deſtroyed; yet, by reaſon of the figure of the reſultant corpuſcles, and their fitneſs to convene, when diſſolved in water, into curiouſly ſhaped bodies, this factitious ſalt, when I have rightly prepared it, did ſundry times ſhoot into long cryſtals with points like diamonds, that did emulate native cryſtal as well in the regularneſs of the ſhape, as in the tranſparency of the ſubſtance. And to make it the more evident, that it was partly the figure, that happened to reſult from the operation of the oil of vitriol upon the ſea-ſalt, and partly other circumſtances, that determined the ſhape of the cryſtals; I ſhall add, that uſually, when the quality or proportion of the oil of vitriol was other than it ſhould have been, or an error was commited in ſome important circumſtance or other of the operation, the ſaline concretions, though they did not ſhoot at all like cubes, as the ſea-ſalt, which they were made of, would alone have done; yet they did not ſhoot any thing at all like rock-cryſtal, as did thoſe formerly mentioned; and for all this did, by reaſon of the curious ſhapes of the corpuſcles they conſiſted of, ſhoot into cryſtals, for the moſt part, finely figured; though ſometimes of one ſhape, and ſometimes of another. And that you may not have any ſuſpicion, as if the regular figure, which ſea-ſalt is naturally of, is any way neceſſary to ſuch figurations, I will add an experiment, that I deviſed to ſhew, that even out of a petreſcent juice ſuch curiouſly figured bodies may be made. I took then ſome ſtony ſtiriæ, elſewhere mentioned to have been found in caves or grottos, where petreſcent liquors coagulated before they have time to fall down; and having diſſolved them in ſpirit of verdigris, I put the clear ſolution to evaporate in a digeſtive furnace, after the ordinary manner; by which means, though I made the experiment more than once, I had rather a coagulated maſs, than any thing like cryſtals. Whereby you may learn the truth of what I was ſaying, that a concourſe of divers circumſtances may be requiſite to determine the figuration of conſiſtent bodies made out of fluid ones; ſince here, for want of time for making occurſions enough for the particles to concrete in after the moſt convenient manner, the experiment ſucceeded not: wherefore it being agreeable to my notions, that ſome ſorts of bodies may require a longer time to make ſuch a convention in, than others, I allowed many days to another ſolution of ſtiriæ made in the ſame menſtruum; after which there ſhot, as I deſired, about the ſides and bottom of the glaſs a number of diſtinct cryſtals, long, tranſparent, and curiouſly ſhaped, moſt of which, I think, I can yet ſhew you.

Perhaps it will be ſaid, that the petreſcent juice, when broken, does oftentimes appear to abound, within, with ſtiriæ, or narrow ſtreaks like thoſe of antimony; and that I myſelf obſerve ſome gems to be made up of thin flakes or plates; which internal figuration ſeems to be much more difficult to be accounted for, without a plaſtick form, than the external.

I will not reply to this, that, for aught I know, divers known ſalts would, when broken, appear to be geometrically figured, even in the leſſer corpuſcles, as well as they are evidently ſo in their entire bulk, if we had eyes quick enough to diſcern the ſhapes of the minuter, as well as of the bigger bodies. And we have great inducements to think, that whether or no *Carteſius* do rightly make the inviſible particles, of which the

the ſmalleſt viſible grains of ſea-ſalt are made up, to be long and rigid like ſticks; the minute viſible concretions, of which the bigger grains of ſalt conſiſt, are, as well as themſelves, of a cubical figure; I will not, I ſay, inſiſt on this reply, but proceed to alledge, that there are divers bodies ſo luckily ſhaped, that upon a ſlow coalition, they will convene into a multitude of manifeſt concretions; ſome of which will conſiſt of ſtreaks, and other be made up of flakes; as in the ſal armoniack commonly ſold in the ſhops (for I ſpeak not of the native, that is ſaid to come from *Armenia*,) though it be avowedly a factitious body, you may often obſerve, upon breaking the bigger maſſes, great multitudes of ſtreaks, like thoſe we may uſually obſerve in the broken ſtiriæ of petrifying water. And I have more than once ſeen, and alſo made, artificial concretions (of whoſe preparation I elſewhere ſpeak) ſome of which conſiſted of ſalts alone, and others of ſalts and minerals, as ſtones or antimony, which look very like talk, being white bodies made up of a multitude of very ſlender ſtreaky particles lying longways one upon another, as in that mineral. And as I have taken out of earth many concretions, which, as they were for the moſt part outwardly ſhaped like rhombuſſes or lozenges, were compoſed of a multitude of flat and extremely thin plates; ſo I have ſometimes taken pleaſure to imitate ſuch concretions by art. And though a ſolution of ſilver in putrified aqua fortis does uſually afford only a great company of ſmall, thin, and ſeemingly ſimple flakes, like ſcales of fiſh, becauſe men have not any deſign like ours in procuring the concretion; yet having diſſolved a good quantity of the metal together, and ſuffered it to ſhoot leiſurely, and with due circumſtances, I have obtained ſundry cryſtals, which both were geometrically figured without, and conſiſted of a multitude of exceeding thin flakes orderly ſticking to one another. And I remember, that whilſt the objection, I am anſwering, was in my thoughts, I pitched upon a yet more pregnant experiment for the clearing of it. For conſidering how tin-glaſs, though a compact and ponderous body, does naturally conſiſt of a multitude of ſhining poliſhed flakes (which may be eaſily perceived and diſtinguiſhed by breaking a lump of it into three or four pieces); I found by trial what I expected, that though a maſs of this mineral were beaten to powder, yet if it were melted and ſuffered to cool of itſelf, the diſpoſition of the component particles would determine them to ſtick to one another in broad and ſhining flakes, whereof many will be incumbent one upon another, and ſome croſs to one another at various angles, according as the matter happened in its ſeveral portions to be diverſely refrigerated. And ſome factitious bodies may afford us the like inſtances, as I have obſerved in ſome mixtures of copper, iron, and other minerals; and very conſpicuouſly in good regulus martis ſtellatus, whoſe internal configuration may be found by breaking it; by which way I have obſerved with pleaſure, that the regulus abounded with flat and ſhining flakes of an almoſt ſpecular poliſh.

If it be urged, to confirm the former objection, that ſome lapideſcent juices, even of thoſe we mentioned in theſe diſcourſes, do concrete, even whilſt men are looking on; and yet our ſtony ſtiriæ, often mentioned (which probably may be alſo haſtily coagulated) have in ſome places a ſtreaky, and in other places an angular configuration of parts; I anſwer firſt, that I have ſeen divers of that kind of concretions, which, as far as the eye took notice of, were made up of parts confuſedly jumbled together. And next, that (to conſider now thoſe whoſe texture is more uniform) I have found by trials, that, if there be a due diſpoſition in the component corpuſcles of bodies to ſuch configurations, they may be brought to concrete accordingly in a far ſhorter time, than almoſt any, that have not tried, would expect, not to ſay believe. Having ſometimes, for curioſity's ſake, warmed ſix or ſeven ounces of aqua fortis, glutted with fine ſilver, till the mixture was all brought into a tranſparent liquor; and having then put the clear, but ſtrong glaſs, that contained it, into cold water, that the menſtruum might be the more haſtily refrigerated, I obſerved, that when once the diſſolved metal began to ſhoot, the coagulation into figured cryſtals proceeded ſo faſt, that a naked eye could ſee the progreſs of it. And having ſometimes put a quantity of ſalt and ſnow, or of ſome other ſtrongly refrigerating mixture, into a convenient glaſs, and wetted the outſide with a ſtrong ſolution of ſal armoniack, or ſome urinous ſpirit, though in leſs than a minute of an hour it would be coagulated; yet the ſalt, into which it ſhot, had uſually a curious and determinate figure, according to the nature of the liquor that afforded it; as I have often ſhewn the curious.

Perhaps you will ſay, that theſe inſtances are taken from ſaline bodies, which are, for the moſt part, diſpoſed to convene in ſmooth ſurfaces, and angular ſhapes, and eaſy enough to be wrought on by the external cold; and it may yet ſeem ſtrange to philoſophers themſelves, what in ſome caſes muſt have happened, if our hypotheſis be admitted, namely, that external circumſtances and accidents, ſuch as the figure of a mould or womb, the coldneſs of the ambient, &c. ſhould viſibly, and ſometimes not a little, diverſify even the internal figuration of cloſe and ſolid minerals and gems, without excluding all thoſe that are ſuppoſed to be of a quicker concretion.

Wherefore, to clear this difficulty, it may not be amiſs to ſubjoin an experiment that I deviſed to ſhew, that if the corpuſcles of a body be ſo ſhaped, as to be fitted by their coalition, to conſtitute ſmooth (and if I may ſo ſpeak) gloſſy planes, though they be variouſly ſhuffled and diſcompoſed, as to their priſtine order, yet, if they be but a little kept in a ſtate of fluidity, that they may the fitlier place themſelves, or be placed by other agents, they will preſently be brought to convene into ſmooth and ſhining planes, and the ſituation of thoſe planes, in reference to one another, will be more uniform and regular, than almoſt any one would expect in a concretion ſo haſtily made; notwithſtanding which, their internal contexture will be much diverſified by circumſtances, as particularly the figure of the veſſel or mould, wherein the fluid matter concretes.

Considering then, that, according to what I noted already, if we break tin-glaſs (taken for the biſmuth of the ancient mineraliſts) as it is wont to be ſold in lumps in the ſhops, it will diſcover a great many ſmooth and bright planes, larger, or leſſer, according to the bigneſs of the lump; which ſometimes meet, and ſometimes croſs one another at very differing angles: conſidering this, I ſay, I thought it probable, that a body, that had already been melted, and was apt to convene into ſuch planes, not only would do ſo upon another fuſion, but might have the order and bigneſs of thoſe planes diverſified by the figure and capacity of the veſſel I ſhould think fit for my purpoſe. Wherefore, having beaten a ſufficient quantity of it to powder, and, when it was well melted, caſt it into a good pair of iron moulds, whoſe cavity was an inch in diameter, we had a bullet, which being warily broken, did, as we expected, ſeem to be, as it were, made up of a multitude of little ſhining planes, ſo ſhaped and placed, that they ſeemed orderly to decreaſe more and more, as they were further and further removed from the ſuperficies of the globe; and they were ſo ranked, that they ſeemed to conſiſt of a multitude of theſe rows of planes reaching every way, almoſt like ſo many radiuſes of a ſphere from the center or middle part to the circumference: whereas, if we melt tin-glaſs in a crucible, and let it cool there, the matter being taken out and broken, will appear indeed full of ſmooth planes, but (as was lately intimated) very irregularly and confuſedly aſſociated or placed.

I will not now ſtay to enquire, whether the orderly compoſition of the planes in our bullet (which ſome curious perſons, that I ſhewed it to, looked on, as a not unpleaſant ſight)

sight) may be derived from this, that the matter was cooled first on the outside, by the contact of the cold iron mould, and the neighbourhood of the ambient air; and that the coagulation being once thus begun, the parts of the remaining fluid, as they happened to pass by this already cooled matter, with a motion which, by reason of their removal from the fire, was now slackened, they were easily fastened against the already stable parts (as may be illustrated by the concretion of dissolved nitre and alum, both about the injected sticks, and the grains, that first concrete against the sides of the vessel) and the refrigeration still reaching further inwards, till it came last of all to the middle of the globe, that being the remotest part from the refrigerating agents; the apposition was successively and orderly made, till the whole matter was concreted. But (as I was saying) I must not now stay to inquire, whether the figuration of our bullet may be explained after this or some such way; or whether we are not to take in some subtle or pervading matter, or some other catholick agent? For though such points may be well worth discussing, and we may possibly elsewhere say something of them; yet here it may suffice to say, that we have varied the foregoing trial, by casting bullets of some other bodies (and particularly the simple regulus of antimony) wherein it succeeded well enough, though the produced contexture were not so uniform as in tin-glass. And I also tried, that having cast melted sulphur itself into a globulous body of about five or six inches in diameter, and warily broken it, though one would think it an unlikely mineral to make any other, than a confused concretion, it presented me great fibres, almost like little straws, whose number, and, in great part, orderly situation, afforded me a much less unfit instance for my present purpose, than one would have lightly expected. But what I came from saying, may serve to make out what I propounded to myself; which having named already, I need not here repeat.

But one thing more there is, that may be pertinent on this occasion, namely, that I have broken divers marchasites of a peculiar sort, that were either of a roundish, or of an almost cylindrical figure, to observe their internal structure and qualifications; whereupon, I found in more than one of them (for I remember not, that I did in all) a great many rows of little planes or glistering corpuscles, reaching from the innermost to the external surface, and in those, that were somewhat cylindrically shaped on the outside, these ranks of gold-coloured particles in the several planes of the broken mineral seemed like semi-diameters issuing out from a row of physical points, conceived to be placed on an imaginary line, lying almost like the axis of a cylinder between the opposite ends (though I do not well remember, how near it reached to them); as if the cavities of the chalk or clay, wherein these marchasites were found, had made the soil like a mould, wherein the matter of the marchasite being detained, whilst it was in a fluid form, did afterwards concrete much after the manner, that the bullets of tin-glass, regulus, &c. did in our moulds. But the prosecution of this conjecture belongs to another discourse.

I shall therefore now proceed to a further answer to the formerly raised objection: wherefore, as to the exquisite uniformity of shape, which is so admired in gems, and is thought to demonstrate their being formed by a seminal and geometrizing principle: though I have, in the second of the above-mentioned arguments, ascribed to them such curious figures, as argue their having been generated after the way proposed in our hypothesis; and though also I willingly allow their shapes to deserve from us a delightful wonder at the curiousness of nature's, or rather her author's, workmanship, yet, upon a more attentive surveying of them, I do not find the uniformity to be near so great as is wont to be imagined; but have rather met with such diversities, as agree well with our hypothesis about their figuration.

In

In several transparent gems, it seemed manifest enough to me (as I lately also noted) that the shape was, in great part, due to the figure of the womb, or mould, wherein the matter, whilst liquid or soft, happened to settle. In some other transparent and well figured gems of the same kind or denomination, and sometimes growing very near one another, by a diligent inspection I found a manifest and sometimes very considerable difference in their shapes, either as to the number, or the figures, or the bigness of the sides or planes, that made up the respective gems; or as to two, or all, of these; comparing these deviating particulars with what would have been in a stone of that kind or denomination, that were perfectly figured. This I had opportunity to take notice of, particularly in two sorts of stones; the first granates, of which I had a considerable number brought me out of *America* growing in one lump of matter; but in distinct parts of it, and without touching one another: among which I took notice of a manifest disparity of shape, and so I did in some African ones, that were presented me; as also in others, that were European, one of which, that was of an extraordinarily large size for a figured gem of a transparent kind (for it weighed above eleven drachms and a half) I considered with a particular attention, and found, that though it seemed to have been coagulated in a fluid medium, and to consist of twelve planes, at the concourse of two or three of which it seemed to have been broken off from the womb or root; yet it was very far from the dodecahedron of geometricians. For, whereas that consists of twelve æquilateral and æquiangled pentagons, almost all the planes, that made up our granate, were quadrilateral and very different from what regularly they should have been, not only in magnitude, but in shape: for one of them seemed to have five sides, and of the rest, some were most of kin to a rhombus, others to a rhomboides; but the most were but little better figured, than those that the geometricians call the trapezia. And thus much for the first sort of gems, whose shapes I observed to be not regular. The second consists of those crystalline stones, which they call Cornish diamonds, and which are some of them much harder than the Bristol diamonds, or perhaps, than rock-crystal itself; it being easy to write upon glass with them. Of these stones having procured a good number (many of which I have yet by me) I took notice, by comparing them heedfully together, that though some of them were geometrically and curiously shaped like rock-crystal, having each six sides, whereof every two, that were opposite, were thoroughly like and equal enough to one another; and though the stone had a pyramidal termination, made up of several resembling and curiously figured planes, that terminated in a solid angle or apex; yet the greatest number, by much, of these titular diamonds was made up of stones, far from being so exactly and uniformly shaped, as those newly described. For though most of them had six long planes; yet oftentimes the opposite ones (besides that they were not so parallel to one another, as they should have been) were unlike and exceeding unequal; and those planes, that were to make up the apex, though apart, they were usually angular; yet being compared to one another, or to the regular patterns abovementioned, their figures, their bignesses, and their manner of concurring (which was sometimes not in a point or apex, but in a line) was so remote from being uniform, that this great diversity and irregularity agreed far better with our hypothesis, than with its rival. And yet in these stones, the want of room to coagulate freely in, could not with probability be pretended; for they seemed to have been formed separately in a fluid ambient, save at the bottom, where they were fastened to the rock, as appeared by an opacous root, if I may so call it, which still adhered to most of them. And, if I much misremember not, I have more than once in diamonds, newly brought from the *Indies*, and some of them very fair ones, observed a great want of uniformity in the areas of the superficial planes, or in their figures, or both;

both; and ſometimes too in the very number, as well as ſituation, of their ſolid angles or corners: about which I hope to recover ſome notes. And ſo I have done with the firſt part of my anſwer to the abovementioned objection; whereby it may appear, that there is no ſuch regular and conſtant uniformity in the ſhapes of gems, but that their real likeneſs may be reconciled to our hypotheſis.

But now in the ſecond part of my anſwer, I ſhall endeavour to ſhew, that the figuration of gems may not only conſiſt with our conjectures, but confirm them. For I have, more than once taken notice in the Corniſh diamonds I have been mentioning, that ſometimes a ſmall ſtone of the ſame kind has made up, as it were, one body with a greater; ſo as that the leſſer ſtone did not only adhere cloſely to the other, but was, if I may ſo ſpeak, ſet or bedded in it. So that when the ſeparation was made, there remained in the greater ſtone a cavity, whoſe figure did curiouſly anſwer that of as much of the ſmaller ſtone, as chanced to be harboured there. And, as ſometimes I obſerved, that there was ſuch an adnaſcency (if you will pardon the word) of a leſſer ſtone to a much greater; ſo at other times, I met with the like of a greater to a much leſſer, with a cavity in the leſſer, anſwerable to that part of the greater that had been lodged in it. Which, for aught I know, allows us with high probability to conjecture, that the ſtone, to which the other grew, was firſt formed and hardened; ſince it retained its own ſhape, and that, whilſt this remained adherent to the rock or ſoil, ſome more liquor, either, that came afterwards by chance into the ſame cavity, or (in caſe it were there before) that was leſs diſpoſed to an early concretion, began to be coagulated by faſtening itſelf againſt the ſolid body, that was already concreted: upon which account theſe two diamonds muſt ſtick cloſe together, and yet be but contiguous, and a cavity, ſuch as I freſhly mentioned, muſt be left in the laſt concreted gem. Which may be illuſtrated by putting into a ſtrong ſolution of pure nitre, or rock-alum, ſome little ſticks of wood or any ſolid body, that may be kept ſteadily in the poſture; for you will ſee many coagulations begin to be made againſt them, and the cryſtals thus concreted will neceſſarily have their figures incomplete, and have in them cavities correſpondent to thoſe parts of the ſtick, whereto the ſaline corpuſcles faſtened themſelves. To which I ſhall only add, that though I have given inſtances of the adnaſcency of figured ſtones only in Corniſh diamonds, yet they are not the only tranſparent minerals, wherein I have been able to obſerve it. And particularly I remember, that I obſerved among ſome minerals, left by a goldſmith to his widow, a fine tranſparent and neatly figured ſtone, which ſeemed to be pure cryſtal, but was coagulated about a kind of branching wire, whereof a good part was incloſed by the ſtone, that ſeemed to grow out of a piece of ore, that looked like ſilver-ore, and which the woman, that was a curious perſon, upon the ſtrict enquiry that I made, affirmed to be, together with the above-mentioned branch, good ſilver, produced by nature in that form (which I thought the more credible, becauſe of the odd and almoſt hair-like ſhape, wherein I have ſeen ſilver-ore to have, as it were, grown); which will excellently agree with the reſemblance, I was juſt now propoſing, betwixt the coagulation of diſſolved ſalts and the liquid matter of gems, about ſtable bodies partly immerſed in thoſe fluids.

The very many circumſtances belonging to our firſt argument, and the laſt anſwered objection, have ſo long detained us, that I doubt, you now think it more than time I ſhould advance to, and diſpatch the ſecond of thoſe grand conſiderations, whereon I at firſt intimated our hypotheſis was founded; and this is built upon the weight of ſome gems, which being greater than that which ſeems to belong to them, as hard and tranſparent ſtones, I think we may probably derive it from metalline or mineral mixtures.

I QUES-

I QUESTION not, but as you will think this allegation new, so you will be apt to question, how I come to know the truth of what I here deliver; since, though gems are wont to be estimated by lapidaries, as they weigh such or such a number of carrats, or of grains, yet they compare only the weight of this and that stone of the same kind in reference to one another, as the greater and lesser weight argues the greater or lesser bulk, without looking after, or knowing how to discover the specifick gravity of several gems, which depends not on the greater or lesser bulk; as (if you know it not already) you will gather from what I am now going to relate.

CONSIDERING then with myself, that for my purpose, it was requisite to have a gem as free as I could get from the metalline mixtures, that I suspected many precious stones to have; and remembering, that rock-crystal, as it is by mineralists reckoned among gems, so it is hard enough, as I tried, both to cut glass, and to strike fire; and that its having so great a transparency, and its being devoid of colour, makes it exceeding likely to be free from adventitious mixtures; I pitched upon it as the standard, whereby to make a probable estimate of the weight of gems; and having hydrostatically, and with a tender balance examined the weight of it, first in the air, and then in water, I found its weight to be to that of water, of equal bulk, as two and almost two thirds to one: which, by the way, shews us, how groundlesly many learned men, as well ancient as modern, make crystal to be but ice extraordinarily hardened by a long and vehement cold; whereas ice is bulk for bulk lighter than water (and therefore swims upon it) and (to add that objection against the vulgar error) *Madagascar* and other countries in the torrid zone abound with crystal.

HAVING thus found the ponderousness of crystal in reference to water; when I met with a coloured gem, whose specifick gravity I guessed to be sensibly greater, I sometimes gave myself the trouble (for a trouble it is) to weigh them in the air and in the water, and so discover, whether I conjectured aright. And if its specifick gravity did much exceed that of crystal, I thought it a probable argument, that there might be some metalline or mineral corpuscles mingled with the stony ones of the gems, and that also it may probably derive its tincture thence. I will not tell you, that I then found many sorts of transparent stones much heavier than crystal: for, besides that the trials were troublesome enough to make, I chanced to fall upon them in a place, where I had not any store and variety of gems to examine. But one instance among those that occurred to me, I shall here set down, because, being so notable, it may suffice to shew, that, as to some gems at least, my opinion of their having an adventitious gravity, and consequently ingredient, is very probable. I had some American granates, which I had a great and peculiar reason to believe had been once liquid bodies, and therefore thought them the more worthy to be examined; and finding their colour to be so deep, that they were almost opacous, and judging by my hand, that they were much heavier than pieces of crystal of the same bulk would be, I weighed them in a pair of nice scales in the air, and in the water, and found them, as I expected, to be almost four times as heavy as water of the same bulk, and consequently heavier by about a third part than pieces of crystal, equalling them in bigness, would be. Whence so great an accession of ponderousness proceeded, I shall tell you, when I come to my next argument; to which I shall advance, as soon as I have noted, that though, when coloured gems have a greater gravity than crystal, it is a probable argument, that they have some metalline pigment, or other mineral substance, mingled with them; yet, if such gems have no such surplusage of weight, it will not follow, that their colour cannot proceed from any mineral tincture; since it is not unreasonable to conceive, that a mineral substance may be present in a liquor (such as the lapidescent juice) that we suppose gems

to

to be made of, even when it adds no manifest weight to the body that harbours it; since I have observed (what is odd) that a mineral water, which by its taste, its effects, and the colour it would strike, appeared to be richly impregnated with iron, being carefully by me examined hydrostatically, did appear very little, if at all, sensibly heavier than common water.

The third and last argument, I shall now make use of, is taken from hence; that out of divers medicinal stones, and even out of some fine gems, real and corporeal metals, or other mineral substances, may be extracted.

Of this argument I shall at present say the less, because the further prosecution of it will be more proper in the second part of this discourse, where I shall be obliged to handle it, with reference to opacous gems, in which its force will best appear. And therefore I shall desire you to take notice, when you arrive at that part of the subsequent discourse, of those particulars, that may serve to strengthen the newly proposed argument: and if it be objected, that the bodies, there treated of, are opacous stones, not gems, I have these things to answer.

First, that divers stones, that are reckoned amongst precious ones, are opacous too; as the turquoise, the onyx, the sardonyx, &c. not to mention divers others, as cats-eyes, opales, &c. which are as it were semi-opacous. Besides I much question, whether diaphaneity be absolutely necessary to the essence, though it be to the beauty, of those precious stones wherein it is usually found. And I might here make it probable by discourse, that transparency and opacity oftentimes depend but upon the manner of the pigment's dispersion thorough the stony matter of the gem, and the convenient or inconvenient situation of the pores, in reference to the beams of light. But waving this speculative argument, I shall rather take notice, that several precious stones, and even diamonds themselves, have sometimes great clouds, which make them in those parts almost (if not quite) opacous, without being thereby hindered from being true diamonds or gems, of this or that kind, to which their hardness, colour, &c. makes them appertain: and not to mention cornelians, agates, and some other stones, that we may observe to be (as the tinging corpuscles happen to be, in a due or an over great proportion, mixed with the petrescent matter, and to be uniformly or inconveniently mingled with it) some of them transparent, and some of them semi-diaphanous; I have seen worn in a ring a sardonyx itself, that was transparent, as unlikely a gem as that is to be so. And as for granates, though you know, that both of them are diaphanous, yet I have had some figured ones, that seemed quite opacous; and I have others by me of several countries (whereof one very remarkable for its large size and geometrical shape) that are in some places diaphanous, but, as to the main bulk of their bodies, appear at least almost as dark as ordinary stones.

I further add, that I little doubt, but that experiments, not unlike those I shall hereafter tell you, I tried to obtain mineral or metalline substances from load-stones, native cinnabar, blood-stones, &c. might succeed in several other of the more ponderous gems, if it were not, that the glassy nature, or exceeding compactness of many of them, makes the mineral corpuscles, that are harboured in the stony and insoluble parts, to be inaccessible to our common menstruums. And when the metalline and mineral ingredient is very abundant, and the tincture of the stony parts not so very close, I question not, but even from transparent gems the adventitious ingredient may, in part at least, be dissolved. And to satisfy you about this matter, I shall now inform you, that having by the ponderousness of the lately mentioned kind of granates been induced to conclude them impregnated with somewhat metalline, and for that reason to think it fit to try, whether I could separate it from them, or otherwise discover it in them; I kept ſome of them (in a crucible) for a competent time in the fire, and found, that they had exchanged their colour, or one not unlike that of unbrightened iron; and having reduced them to very fine powder, and digeſted ſome acid menſtruums, and particularly rectified ſpirit of ſalt upon them, they afforded me a rich tincture: encouraged by which, I hoped, that, without their being previouſly burnt, they would in aqua regis afford a tincture, and acordingly I obtained from crude granates (only reduced to very fine powder) a rich ſolution, which, though in colour it ſomewhat emulated a ſolution of gold, yet partly by the colour of the burned granates, and partly by the taſte of this ſolution, I ſuppoſed, that another metal was likelier than gold to be the predominant mineral; and having gently evaporated part of that menſtruum, I obtained from ſome of the reſt certain cryſtals, whoſe ſhape, by reaſon of their ſmallneſs and diſorderly coagulation, I could not well determine; and touching with the tip of my little finger the uncoagulated portion of the liquor, this part of a drop being put to a great many drops of the infuſion of gall, did ſo immediately turn it into a ſubſtance, that ſeemed full as black, if not blacker than ink, as you would, I think, have been ſomewhat ſurprized to behold.

Which trial I made to examine the conjectures I had, that one mineral (for perhaps it was not the only, that helped to conſtitute theſe granates, was of a martial nature; which, if it were, I ſuppoſed it would, like other bodies that participate of iron, afford with galls an inky colour. I tried alſo with a parcel of ſmall and red tranſparent ſtones, which ſome gueſſed to be granates, others, more probably, rubies, that being finely powdered, they would in an appropriated menſtruum (made extraordinary ſtrong) give a colour like that of diſſolved gold. And that there were really ſome parts of the gem diſſolved in the menſtruum, appeared not only by the above-mentioned colour, but by theſe two indications; the one, that having put ſome of this liquor to ſome of the ſame ſolution of galls I juſt now ſpoke of, it produced indeed, at the very firſt, a dark colour, but not near ſo black as that of the granates, and in a trice let fall a copious precipitate, that was almoſt white: the other, that I was able to precipitate from it, by an urinous ſpirit, a reddiſh ſubſtance, which being ſuffered to dry in air, ſeemed to grow into bodies, in ſhape not unlike moſs, and here and there ſmall muſhrons, all of them prettily coloured. And from certain granates, that were in ſome places opacous, as well as in others diaphanous, I obtained a ſolution, from whence the ſuperfluous liquor being abſtracted, the reſidue, which was deeply coloured, did in the cold afford me a kind of ſaline concretions, which yet were not large enough to enable one to determine their figure.

And on this occaſion, I hold it not unfit to intimate, that perhaps, if men had curioſity enough to make trials, there would be other tranſparent minerals found capable of being wrought on by appropriated menſtruums. For I do not think, that every ſeemingly glaſſy contexture of a mineral makes it unfit to be wrought on: for though the clear ſpar, which in moſt of our weſtern lead mines in *England* is found next to the metalline veins, be at leaſt ſemi-diaphanous, and be of ſo glaſſy a contexture, that it uſually breaks into ſmooth and gloſſy ſuperficies, and looks like a talk, and alſo, for the moſt part, is made up of, and preſently reducible into geometrically figured bodies, ſhaped like rhombuſſes or rhomboides; yet ſome other trials, that I have made with this ſpar, inducing me to ſuſpect, that it was not indeed a talk, but a body of a much more open texture, I found I could diſſolve it in ſeveral liquors, and particularly in good ſpirit of ſalt, which would preſently work upon it, even whilſt it was in lumps, and that without the aſſiſtance of heat; which obſervation may perhaps give ſome encouragement to ſuch a curioſity as yours.

But

But by what I have said of the usefulness of menstruums, I would not have you think, that they are the only instruments, wherewith something metalline may be obtained from some gems: for in another paper of mine (to which such trials more properly belong) you may find an account of some attempts of that kind of fusions and appropriated additaments. And however such trials may succeed with you, that aim at separating from a gem a metalline or mineral body of a determinate species; I can teach you an easy way, whereby I have (by the help of fusion) more than once manifested in the general, that there may be substances, partaking of a metalline nature, in some kinds even of transparent gems. And partly by the same way, and partly by some others, I have been able to determine probably enough, in some cases, that the mineral substance is predominant in it.

And here, before I dismiss the first part of our essay, I think, I may possibly somewhat illustrate our hypothesis, if I briefly mention to you an experiment, I remember I once made to that purpose. And it was this: I reduced to powder some of those stiriæ, that I have often spoken of, of water petrified, as it were, spontaneously. I also considered with myself, that I had found spirit of verdigris (which I make without the tedious preparations, that *Basilius* and others prescribe, by barely distilling, without additaments, good French verdigris, and rectifying the obtained liquor) I had, I say, found this menstruum to be, not only, as I elsewhere observe, a good solvent for many bodies, but also to be distillable from many of them, without leaving near so much of itself behind, as other saline solvents are wont to do: considering this, I say, I dissolved the stony stiriæ in this liquor; and having suffered some of it to evaporate away, and put the rest into a cool place, I obtained, as I expected, store of small, but finely figured and transparent crystals, that shot much after the fashion of those of the purer sort of nitre. With some part also of the stony solution I mixed, in a convenient proportion, a high coloured solution of copper, made likewise in spirit of verdigris; and these two solutions being made with the same menstruum, and warily enough put together, did not precipitate one another, but afforded me, upon the evaporation of the superfluous moisture, among divers crystals, that were transparent and colourless, some that were richly adorned with a greenish blue tincture of the dissolved metal. What trials I made by this way, little varied, to imitate nature, by associating into transparent bodies stony and metalline substances, I cannot now give you a full account of; since I neither have by me the notes I set down about those trials, nor think it fit to make this first part of our discourse more prolix, than I now perceive it to be already.

SECTION II.

Containing a conjecture about the causes of the virtues of gems.

What has been hitherto delivered in the first part of our discourse, will, I suppose, make it allowable for me to be more succinct in the second. I shall now, therefore, proceed to those other considerations, which, being assisted by what has been already said, may, I hope, suffice, to keep our conjecture about the cause of the virtues of gems from seeming unreasonable.

And my first observation shall be, that not only there is in the earth a great number and variety of minerals, already known by particular names; but probably there are very many others, that are not yet known to us.

The former part of this proposition will not be doubted by those, that consider, how great a multitude of metalline ores, marchasites of several sorts, antimonies, tinned glass, fluores, talks of various kinds, spars, sulphurs, salts, bitumens, &c. are mentioned partly by chymists, and other mineralists, and partly by those that have given us accounts of museums and other collections of natural rarities; insomuch, that of only one kind of fossils, the diligence of some modern writers hath reckoned up between two hundred and two hundred and fifty; besides animals stones, as lapis bezoar, lapis manati, oculus crancri, lapis porcinus, &c.

And as for the second part of our proposition or observation, you will scarce deny it, though you consider with me but these two things.

The first is the small and inconsiderable proportion, that the perpendicular depth, that the generality of mines bears to the semi-diameter of the earth, reckoned to be above 3500 miles; so that, though our globe were inhabited by some hundreds of millions of men more than now it is, and they had curiosity enough to dig mines every where, and consequently there were millions of inquisitive and laborious men, more than really there are, their spades and pickaxes would, except here and there, penetrate so little a way into the earth, that a vast multitude of fossils might, by lying deeper in the bowels of it, continue undiscovered.

And to this first observation I shall subjoin this second; that, as far as I have observed, almost every region affords minerals of its own, differing from those, that are taken notice of in other regions. And in particular countries, as in some shires of *England*, a curious and heedful eye may, I doubt not, observe several, that are not taken notice of by the inhabitants themselves; especially, if well-made borers were diligently and skilfully employed to pierce the ground, and bring up samples of divers fossils, that lie hidden under it. But having elsewhere discoursed of this matter, I shall here only tell you, in general, that in some parts of *England*, where I had more opportunity than in others, to exercise some curiosity about minerals, I met sometimes, in a small compass of ground, with a much greater variety than I expected, and several of them undescribed, that I know of, by any writer; of which sort I have received divers others from several parts, both of the old world and the new.

In the next place, I consider, that nature has furnished the earth with menstruums, and other liquors of several sorts, and endowed it with divers qualities. This I have already manifested in the discourse of subterraneal menstruums, whereto I shall therefore refer you; only taking notice in this place, that whereas water is abundantly to be met with under ground, and, for the most part, very copiously in mines, by which it is capable to be variously impregnated; this liquor itself, especially being thus altered, may, in some cases, act the part of no despicable menstruum, and, on some occasions, otherwise concur to the production of mineral bodies.

I further observe, that the subterraneal liquors, upon one account or other (for we need not now particularly determine it) are qualified to work, either as corrosive menstruums, or as other solvents, upon many of the medicinal earths, and other minerals they meet with under ground: which minerals, having never been exposed to our fires, have their texture more open, and their parts more soluble, than those that have been melted by the violent heats of our furnaces.

And that even common water will suffice to dissolve and impregnate itself, both with the saline, and, oftentimes, with metalline parts that it meets with in its passage, is obvious enough in the differing tastes, and other qualities of liquors, that all pass for common water, whereof some is found better, and some worse than others, to brew, some

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to wash linen, some to dye scarlet, or other determinate colours; some to temper steel, and some for other uses.

But others, unquestionably more eminent instances, are given us by the mineral springs, whether thermæ or acidulæ, as authors distinguish those that are actually hot (as at *Bath*) and those that are saline, and, for the most part, sourish (like those at *Tunbridge*, and the *Yorkshire Spa*); of which two sorts, good store are enumerated by physicians and geographers; and of which a far greater number would be discovered, if men wanted neither skill nor diligence. And here I shall desire you to take notice, that, though common water do the most readily dissolve the salts, more properly so called, though not altogether pure, it meets with in the bowels of the earth, as we see it happens in those salt-springs that come not from the sea; yet there are also many other subterraneal bodies, which, upon the score of their abounding with saline particles, will be dissolved by water, though they be of a compounded nature, and contain very differing substances; as it is plain in those waters of *Hungary*, and other regions, which, by the evaporation of their superfluous moisture, will yield vitriol, a mineral not only compounded, but decompounded, as containing in it a saline, a sulphureous, a metalline, and an earthly part (which, itself, I have found to be none of the simplest bodies); every one of which may be made distinctly to appear.

Lastly, I consider, that the petrific juice of spirit coming to be in a sufficient proportion mingled with these impregnated waters, so as to coagulate them, and concoagulate with them; from their coalition may result those precious stones, that we call transparent gems. For it is certain, that bodies, that were a while before in the form of waters, may coagulate into stony stiriæ, of whose odorousness and reducibleness into lime, I have already given an account in my discourses of lapidescent juices; of which you may command a sight. And that even diamonds themselves, the hardest of gems, were once fluid substances, the first part of this discourse has, I hope, evinced.

To which I shall now add, that procuring some petrified bodies to be brought me from a place in *England*, which I could not be admitted to, I found, that the petrific juice or spirit, that abounded in the earth of that spot of ground, was so penetrating, and so operative, that it made some of the vegetable substances, that were found in it, in their pristine shape, and, for aught I could perceive, bigness, hard enough to cut glass, as well as grave on iron. And it was among these rarities (if I much mis-remember not) that I picked up a (moderately) transparent body (which, I think, I have yet by me) that, by the shape, and other circumstances, I judged to have been a diaphanous gum, belonging to one of the pieces of petrified wood that had been brought me, and was hardened to a degree, that made it capable of scratching glass.

And now to bring home these things to my present subject, I conceive, that some, at least, of the real virtues of diver gems may be derived from this, that whilst they were in a fluid form, or, at least, not yet hardened, the petrescent substance was mingled with some mineral solution or tincture, or with some other impregnated liquor; and that these were afterwards concoagulated, or united and hardened into one gem, as a diamond, a sapphire, a granate, an onyx, a blood-stone, &c. And as divers of the virtues of gems may be, in a general way, deduced from the commixture of these mineral corpuscles; so the greatness of those virtues, and the variety of those properties, in particular, may be ascribed to the peculiar nature of the impregnating liquors, to the diversity of them, and to the greater and lesser proportions wherein they are mixed with the petrescent juice.

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To render this conjecture (for I propose it as no other) thus summarily and briefly expressed, the more probable, it will be fit to recall to mind the arguments, whereby we have already shewn, both that gems were once fluid or soft bodies, and that divers of them were not simple concretions of a petrescent liquor, but consisted also of other mineral adventitious corpuscles: which may appear, partly by the separableness of such substances from some gems (as we exemplified in granates) partly by the specific gravity of others, and partly by the differing tinctures (whereof one, at least, may well be supposed adventitious) to be met with in gems of the same species, as rubies, sapphires, granates, and even the hardest stones, that we yet know of, diamonds themselves; of which (as is before noted) I have seen some yellow, and that to a great degree, some of other colours, but not so vivid; and some green, almost like emeralds.

Now since there may be in gems, and, in some of them, abundantly such adventitious corpuscles; and since there is cause to think, that some may be endowed with divers properties, and medicinal virtues; since also there is a great difference among these impregnating particles, and, probably, of a greater variety of them than is known to us; since, lastly, divers gems are not sparingly, but richly impregnated with these ennobling corpuscles, I see no sufficient reason, why some of the virtues of divers gems are not more likely to proceed thence, than from those unintelligible and precarious substantial forms to which they are wont to be referred.

But because there are some difficulties, that the objections of others, or my own thoughts, have suggested against our hypothesis; though I neither have time, nor do think it very necessary, to discourse amply of them; yet to clear the way for what I am afterwards to represent, I shall (though I can but briefly do it) say something to each, that may, perhaps, appear no insufficient answer; especially after I have declared, as I here do once for all, that I speak of the true and medical virtues that belong to gems; and that, as to those magical, and other extravagant properties, that either notoriously fabulous, or other credulous writers have made bold to deliver, I am so far from pretending to afford them an explication, that I do not allow them the least degree of assent.

This premised, let us consider the chief difficulties themselves; among which, I doubt not, but it will be objected, that it is not credible, that the mineral substances, wherewith our hypothesis would have gems to be impregnated, should have any medical operation at all on the human body, in regard that they are so locked up, that they can communicate nothing to it, especially being indigestible and unconquerable by so small a heat, as that of the stomach and other parts of the body.

But to this specious objection I have several things to return by way of answer. And first of all, had there yet never been any actual trial made, whereby to know, whether a gem be capable of having any medical virtues, I confess I should find probability enough in the objection to suspend my judgment, till experience should determine the question. But since upon the very credible testimony of eminent physicians and patients themselves of my own acquaintance, I find much less cause to disbelieve, than to assent to some matters of fact about the operations of gems; and since such matters of fact do strongly argue, in the general, that a precious stone may have medical virtues; I think the objection, as it is proposed in general, is sufficiently enervated by such particular instances, and ought not to keep us from believing upon experience the possibility of the thing denied; especially since there are other things besides, that may be alledged in favour of our hypothesis.

For it may be considered in the next place, that vigorous load-stones emit copious and very plentifully effluvia; and yet, besides that ordinary magnets are usually a very hard

sort

sort of stones, I have met with some load-stones much harder than ordinary ones, and possibly than divers gems. And it is farther considerable, that there are load-stones (some of which I can shew you) which do not only work upon iron, and other magnetical bodies, but have a manifest and inconvenient operation upon human bodies, by being worn in men's pockets, or long held in their hands; as those, that have resented such operations themselves, and observed them in others, have complained to me; which I might confirm by some analogous observations, if I had time to relate them.

But now I proceed to observe, that among transparent pebbles, some of which, you know, are, by being barely well cut and set, made to counterfeit diamonds, I have found several, that may be brought, in a trice, to emit copious, and even strongly-scented steams. And if you allow the opinion of the generality of modern philosophers, who ascribe electrical attractions to the effluvia of bodies excited by rubbing, you will, I presume, allow me to infer, that very light alterations may suffice to procure expirations, even from transparent gems: many of which are electrical, and so are the hardest of them, diamonds themselves; one of which I keep by me, that, upon a little friction, attracts vigorously enough to be wondered at by the spectators.

And as to that part of the objection I am answering, which contends, that gems are not to be digested or conquered by the heat of the stomach; I will not stay to examine, whether, and how far, the digestion of things in the stomach be to be ascribed to heat, contenting myself to say at present, that, to make the objection valid, it should be first proved, that such bodies cannot have any operation upon the human body as pass thorough it, without any sensible change of bulk, figure, &c. as gems, that are swallowed down, are supposed to do. For we know, that some chymists make bullets of the regulus of antimony (which we also have made, and observed something odd about them) pilulæ perpetuæ, because, when they have performed their operation in the body, and have been ejected with the excrements, they are by some more thrifty, than cleanly persons, washed, and employed again and again to the former purposes. Nor do we know, what analogy there may be between some juices in the body, and some of the mineral substances that impregnate gems with their virtues.

For, though the oculus mundi be reckoned, by classic authors, among the rare gems (as indeed good ones may be justly accounted rarities) yet, if one of the best sort be but a while kept in common water, it will, as experience assures me, receive an alteration obvious to the eye. I might here alledge the concurrent authority of many, and the common practice of most physicians, who, in their publick dispensatories, as well as private prescriptions, ordain the fragments of precious stones to be taken inwardly, upon the score of the cordial, and other virtues they ascribe to them. But I shall rather make use of less questioned arguments, and, without insisting on the manifest operation, that the juices of the body have not only on the chalybeate preparations, where the metal is presumed to be opened, but upon crude steel itself; or urging the examples of Lazarus vitri-vorax, or the devourers of stones, as being rare ἰδιοσυγκρασίαι; I shall proceed to acquaint you, that with a faint liquor, distilled from a vegetable substance, as temperately qualified, and as plentifully eaten as bread, I have obtained, and that without heat, from divers hard bodies, and amongst them, from a transparent sort of gems, a manifest tincture. And whether some juices of the body, assisted by the natural heat of it, may not, in reference to some gems, serve for extracting menstruums, though it may well be more than either I, or the objectors, certainly know, yet the instance, I come from alledging, favours our hypothesis more than theirs.

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But to this specious objection I have several things to return by way of answer. And first of all, had there yet never been any actual trial made, whereby to know, whether a gem be capable of having any medical virtues, I confess I should find probability enough in the objection to suspend my judgment, till experience should determine the question. But since upon the very credible testimony of eminent physicians and patients themselves of my own acquaintance, I find much less cause to disbelieve, than to assent to some matters of fact about the operations of gems; and since such matters of fact do strongly argue, in the general, that a precious stone may have medical virtues; I think the objection, as it is proposed in general, is sufficiently enervated by such particular instances, and ought not to keep us from believing upon experience the possibility of the thing denied; especially since there are other things besides, that may be alledged in favour of our hypothesis.

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But now I proceed to obſerve, that among tranſparent pebbles, ſome of which, you know, are, by being barely well cut and ſet, made to counterfeit diamonds, I have found ſeveral, that may be brought, in a trice, to emit copious, and even ſtrongly-ſcented ſteams. And if you allow the opinion of the generality of modern philoſophers, who aſcribe electrical attractions to the effluvia of bodies excited by rubbing, you will, I preſume, allow me to infer, that very light alterations may ſuffice to procure expirations, even from tranſparent gems: many of which are electrical, and ſo are the hardeſt of them, diamonds themſelves; one of which I keep by me, that, upon a little friction, attracts vigorouſly enough to be wondered at by the ſpectators.

And as to that part of the objection I am anſwering, which contends, that gems are not to be digeſted or conquered by the heat of the ſtomach; I will not ſtay to examine, whether, and how far, the digeſtion of things in the ſtomach be to be aſcribed to heat, contenting myſelf to ſay at preſent, that, to make the objection valid, it ſhould be firſt proved, that ſuch bodies cannot have any operation upon the human body as paſs thorough it, without any ſenſible change of bulk, figure, &c. as gems, that are ſwallowed down, are ſuppoſed to do. For we know, that ſome chymiſts make bullets of the regulus of antimony (which we alſo have made, and obſerved ſomething odd about them) pilulæ perpetuæ, becauſe, when they have performed their operation in the body, and have been ejected with the excrements, they are by ſome more thrifty, than cleanly perſons, waſhed, and employed again and again to the former purpoſes. Nor do we know, what analogy there may be between ſome juices in the body, and ſome of the mineral ſubſtances that impregnate gems with their virtues.

For, though the oculus mundi be reckoned, by claſſic authors, among the rare gems (as indeed good ones may be juſtly accounted rarities) yet, if one of the beſt ſort be but a while kept in common water, it will, as experience aſſures me, receive an alteration obvious to the eye. I might here alledge the concurrent authority of many, and the common practice of moſt phyſicians, who, in their publick diſpenſatories, as well as private preſcriptions, ordain the fragments of precious ſtones to be taken inwardly, upon the ſcore of the cordial, and other virtues they aſcribe to them. But I ſhall rather make uſe of leſs queſtioned arguments, and, without inſiſting on the manifeſt operation, that the juices of the body have not only on the chalybeate preparations, where the metal is preſumed to be opened, but upon crude ſteel itſelf; or urging the examples of Lazarus vitri-vorax, or the devourers of ſtones, as being rare ἰδιοσυγκρασίαι; I ſhall proceed to acquaint you, that with a faint liquor, diſtilled from a vegetable ſubſtance, as temperately qualified, and as plentifully eaten as bread, I have obtained, and that without heat, from divers hard bodies, and amongſt them, from a tranſparent ſort of gems, a manifeſt tincture. And whether ſome juices of the body, aſſiſted by the natural heat of it, may not, in reference to ſome gems, ſerve for extracting menſtruums, though it may well be more than either I, or the objectors, certainly know, yet the inſtance, I come from alledging, favours our hypotheſis more than theirs.

And even the natural heat of a human ſtomach, nay, perhaps, the outward parts of the body, may be able, though not to digeſt precious ſtones, yet to ſolicitout ſome of their

their virtues; since I am sure, it makes a sensible alteration in the hardest sort of them. For I have a diamond, whose electrical faculty may be excited not only by rubbing, but, without it, by a languid degree of adventitious heat. And I have had in my keeping a diamond, which, by water made a little more than lukewarm, I could bring to shine in the dark.

Object. If it be further alledged, that, though some virtues may be conceded to gems upon the account of the minerals that impregnate them, yet it will be no way likely, that their virtues should be so various and great, as even the modester sort of authors pretend: if this, I say, be alledged, I shall readily acknowledge, that I do not think others, or myself, obliged to believe all the strange things, that even some learned writers do sometimes ascribe to gems: and if any man will think, that some of them are fabulous, and more of them hyperbolical, he may sooner find me his associate, than his adversary in that point. For the rarity of transparent gems, their lustre, and the great value, which their scarceness, and men's folly sets upon them, imboldens some to say, and inclines others to believe, that such rare and noble productions of nature must be endowed with proportionable, and consequently, with extraordinary qualities.

But this being freely granted, I answer to the objection; first, that it is not improbable, that there may be in the earth a much greater variety of minerals dissoluble by the subterraneal menstruums, and capable of concoagulation with petrescent juices, than authors have yet taken notice of: to which conjecture divers subterraneal productions, that I have met with, do strongly incline me. And from the number and various mixtures of these may proceed, not only a great variety of operative particles in precious stones, but a high degree of energy in some of them.

And next I consider, that the efficacy of those mineral tinctures or solutions that are already known to us, and may be concoagulated with the petrescent juice, may be reasonably presumed to be much greater in some gems, whereof they became ingredients, whilst they were (as chymists speak) *in solutis principiis*, than may be expected in our shops, or laboratories, from the vulgar solutions of the same metals or minerals, after they have, by vehement fires, been reduced into gold, or silver, or lead, or antimony, &c. For whereas, in these vehement fusions, requisite to bring metalline or other ores into such substances, the volatile and spirituous parts are wont to be driven away, and the remaining body becomes more hard and compact, and has his virtues, as it were, locked up; in the state of fluidity those subtle and efficacious parts are preserved, and united to the other ingredients of the gems, whence some emanations of them may be easily enough drawn out; as in the instance I not long since mentioned, of the easy eduction of strongly-scented streams from pebbles so hard, that I found them more disposed to strike fire, than flints themselves, that are used in guns. And from the greater or less plenty, and natural activity of the impregnating particles in this or that gem, may, probably, be deduced the difference in colour of some, and in virtue of other stones of the same denomination: of which we have, in a learned writer or two, eminent examples given us of the great virtue of some, and the inefficacy of other, that experience has discovered, among those stones that go under the title of lapis nephriticus. For, though they be not properly transparent gems, yet the analogy betwixt them, and those that are, seems sufficient to warrant the mentioning of them on this occasion.

See *Unzerus* de nephritide.

And here we may subjoin two things, in favour of both the foregoing answers: the first, that for aught we know, the petrescent juices themselves may have all that is requisite to make them such, and yet have distinct natures, and be endowed with peculiar qualities, abstracting from those which they acquire upon the score of their coalitions with

with adventitious liquors. This I cannot stay to make probable by the differences I have observed in petrescent fluids, and therefore I hasten to the second.

The next thing which I would represent, is, that having observed petrific liquors, or spirits, to pervade and give a high degree of hardness to bodies that chanced to lie within their reach, though one would have thought them sufficiently indisposed to receive such an induration; I see no absurdity in supposing, that, sometimes, such a liquor may invade, permeate, and subdue transparent minerals, abounding in saline, sulphureous, and bituminous particles; which consequently being duly excited, may be made to emit their more subtle and more active parts. And as I have cause to think, that subterraneal fires and menstruums do divers times make various compositions and decompositions in the earth (as it were not hard for me to shew, if I had leisure); so it is not impossible but that the spirit we have been speaking of, supervening, may mingle itself with such bodies, and petrify them, together with itself, into gems. On which occasion, I remember, that I have had salt, made by nature in the bowels of the earth, just like that which chymists compound by art on the surface of it. And I have sometimes made, by an easy operation, and a moderate degree of fire, a certain composition of volatile particles of salt and sulphurs (some of which I have yet by me) which, after distillation, did, in a fluid medium, shoot into crystals transparent, and more curiously figured, than I have seen divers natural gems to be. So that, if either beneath, or upon the surface of the earth, such kind of substance happen to be pervaded and subdued, by a clear petrifying liquor; we may well presume, that the resulting concretions may be indued with qualities, as well uncommon for the kind, as considerable for the degree.

Object. If it be yet objected, that it is very unlikely that gems should part with any effluvia or portions of themselves, since they lose not of their weight, and some of them are very little heavier than crystal itself, and consequently are not like to have much adventitious substance to part with; I might leave the answering of one part of the objection to physicians and chymists, who teach, that the antimonial glass and cup imbue wine and other liquors with a strong emetic quality, without any sensible loss of weight. But having elsewhere spoken of those things, I shall rather here demand, whether the objectors have tried the truth of what their argument supposes by any way sufficiently accurate? For I much doubt, that that has neither been attempted, nor would be found easy to be performed. And till due trial be made, let me represent, that though they will not allow common water to be a menstruum fit to draw any thing with, from such a body as mercury, which is wont to mock the chymists aqua fortis and aqua regis; yet both *Helmont* and others inform us, that mercury kept for a day or two in common water, or boiled a while in it, though it be taken out without any sensible diminution of weight, or bulk, will have imbued a considerable quantity of water with a virtue of killing worms; for which purpose it is much used, and often with good success, in a great hospital in *London*, as the chief physician of it (a very judicious and experienced man) has more than once informed me.

And as for the lightness that is objected against some gems, besides that it may safely be granted, that, *cæteris paribus*, such may have fewer or more languid virtues than others of the same kind; it may also be answered, that the adventitious substance that impregnates the petrescent juice, may be of so small specifick gravity, as not to make the gem at all heavier in specie than crystal itself. For this (as we have formerly observed) being about two times and a half heavier than common water of the same bulk, I have hydrostatically found, that divers salts and some other mineral substances are of less specifick gravity; and consequently, if they were concoagulated with the petrescent juice, that hardens into crystal, need not increase the ponderousness of it, and yet may imbue it with considerable virtues; nor is it necessary (to add that *in transitu* on this occasion) that, not to alter even the colourlesness of crystal, or the colour of another gem, the adventitious substance should be purely saline; for I have divers times made bodies, which though transparent and colourless like crystal, and sometimes curiously and regularly figured, were yet of a compounded nature, and particularly abounded with an easily separable and strongly-scented sulphur. But to give yet a farther and more direct answer to the objection; I shall add, that though when a gem has much more specifick gravity than crystal, or will suffer an adventitious mineral to be separated from it, it is a very probable argument that the petrescent juice is that body compounded with an adventitious substance; yet it will not necessarily follow, that, when neither of these signs appear, the gem is quite devoid of any such substance. For (according to what I elsewhere declare) the petrescent liquor it mainly consists of, may be impregnated, not with the grosser substance, but with the finer and more spirituous part of the mineral, without having the specifick gravity sensibly encreased. Of which I remember I shewed a notable instance to some curious persons, at a mineral spring, which many were then drinking of by the advice of learned physicians for several diseases. For, though this water, both by its inky taste, by its blacking the excrements of those that drank it, and by other signs appeared to participate richly enough of iron; yet the ferruginous particles it abounded with, were so light and spirituous, that not only they would, as I tried, be easily lost, if the liquor were kept too negligently stopped; but when I came, whilst the spirits were yet there (it being but newly taken from the spring itself) to examine it hydrostatically with very good scales and much diligence, I convinced the virtuosi that assisted, that this ferruginous water was very little, if at all heavier in specie than other water, which was brought as common water to be compared with it, and examined with the same scales, and after the same manner.

And now, if you recall to mind what I have elsewhere said, partly of the atmospheres of solid bodies, and partly of the great efficacy of effluviums; I hope you will not think it absurd to conjecture, both, that some precious stones may have medical virtues, and, that divers of these may be ascribed to the mineral substances, whereof they participate or consist; and especially to those which are best fitted to exert their powers, by the copious effluxions of their more agile and subtle parts.

And by this time it may be seasonable to tell you, that though what I have hitherto discoursed do chiefly belong to transparent gems; yet divers of the things already delivered may, with no great alteration, be applied to opacous gems; of which I shall speak much more briefly, not only for the reason just now given, but because, if we have shewn, as I hope we have, that even diaphanous gems may be endowed with virtues by the mineral substances they contain, or are in part made up of; the arguments will hold more strongly as to opacous gems; both because these are, for the most part, much less hard than the others, and because it is far more easy to shew, by their specifick gravity, and the compoundedness of divers of them, that the dark ones, than it is that the clear ones may partly, and sometimes plentifully consist of mineral substances embodied with and hardened by petrescent juices or petrific spirits.

In favour of this doctrine, I shall endeavour, in the first place, to shew that what has been delivered is possible; and afterwards set down some particulars to make it very probable.

The first part of my task might be easily performed, or, perhaps, would be needless, if I were sure you had no need to be told of any thing I have written about lapidescent juices. But for greater security I shall, in this place, briefly intimate, that among the kinds of those liquors I have observed a sort that is of so fine a substance, and yet of so

petrifying

petrifying a virtue, that it will penetrate and petrify bodies of very differing kinds, and yet ſcarce, if at all, viſibly encreaſe their bulk, or change their ſhape or colour. To which purpoſe, I remember, that I have ſeen divers animal and vegetable ſubſtances ſo petrified, as ſcarce at all to be taken notice of, by their appearance to have been altered by the operation of the petreſcent liquor. I have, with pleaſure, ſeen a thin cream-cheeſe turned into ſtone, where the ſize, ſhape, and colour, even of the wrinkles, and the blueiſh mold (which, it ſeems, it began to have, when the liquor invaded it) were ſo well preſerved, that an hungry man would not have ſcrupled to have fallen upon it for a good bit. And as for the hardneſs that this petreſcent juice can give to the body that it penetrates, I ſhall now only remind you of what I lately told you; that I have had, and, I think, yet have, in another place, a pretty quantity of wood petrified in *England*, which retaining its former figure, and grain, and ſcarce at all viſibly increaſed in bulk, was ſo very hard, that I could make impreſſions with it upon iron and glaſs itſelf, and make it ſtrike fire like an excellent flint. To which I ſhall here add, that the ſtony parts did not ſuffer the wood which they had penetrated to be reduced in the fire, either to aſhes or charcoal. And I have by me a lump of mineral ſubſtances, wherein a petreſcent liquor that fills the large intervals between them, is tranſparent enough and harder than moſt ſtones, as far as we could gueſs by ſome trial of it made by a ſkilful engraver of gems.

AND to theſe inſtances might be added many others, if it did not by theſe few ſufficiently appear, that petrifick agents may inſinuate themſelves into the pores of various bodies, and turn them into ſtone, without otherwiſe deſtroying their priſtine nature, or ſo much as their former figure.

WHEREFORE, having in general ſhewn our hypotheſis to be poſſible, we may now deſcend to four or five particular arguments, that it is hoped may help to render it very probable. And theſe I ſhall fetch, partly from the great ſpecifick gravity of divers opacous and medicinal ſtones; partly from the fitneſs of our hypotheſis to render a reaſon of divers phænomena relating thereunto, ſome of them ſcarce at all, and others much leſs probably to be accounted for without it; partly from the metalline ſubſtances to be manifeſtly ſeparated or obtained from the ſtones we are treating of; and partly from the nature of the bodies whereof medicinal ſtones ſeem to be compounded.

Arg. I. THAT the ſpecifick gravity of divers opacous ſtones, whereunto medicinal properties are aſcribed, is very conſiderable, is a truth, which if thoſe that have written of ſuch concretions had been verſed in hydroſtaticks, and had had the curioſity to examine them that way, they might have eaſily diſcovered, as will quickly appear by particular examples; before the mention whereof, it will be fit for me to take notice to you, that conſidering with myſelf, that white marble is generally allowed to be a pure and ſolid ſtone, and, upon the ſcore of its whiteneſs, is likelier than moſt others to be free from mineral mixtures; I thought I might at leaſt as well pitch upon that as on any other, for the ſtandard of the ſpecifick gravity of opacous ſtones, as they are merely ſuch. And accordingly having weighed a piece of white marble in air and water, I found it to be in weight to an equal bulk of that liquor very near $2 \frac{72}{100}$ to 1, or, that the proportion with very little error may be the better remembered, as two and ſeven tenths to one. And to make trial in a ſtone uncoloured, but, becauſe harder, ſuppoſed to be of a cloſer texture, we examined a fine white pebble, which we found to be to an equal magnitude of water, as two and above ſix tenths to one. This being determined, it was not difficult for me to think, both that divers bodies that commonly paſſed for mere ſtones, are more ponderous than white marble of the ſame bulk; and that if there were any ſuch great ſurpluſage of ſpecifick weight, as I gueſſed, many will be found to have above that of marble, it might proceed from ſome metalline body, though not viſibly, yet really, and perhaps plentifully mingled with the petreſcent matter of theſe ſtones. The latter part of this conjecture will hereafter be confirmed in the third argument; which makes it unneceſſary for me to give you now of the former more than a few inſtances: which I ſhall ſoon diſpatch, by telling you, that I quickly found by weighing the following minerals, firſt in the air, and then in the water, that a blood-ſtone (bought at the druggiſt's) was in weight to water of the ſame bulk at $5\frac{7}{10}$ to 1; the load-ſtone I then tried (for all are not equally heavy in ſpecie) as 4 and $\frac{6}{10}$, to 1; lapis calaminaris, uſed for rheums in the eyes, and to turn copper into braſs, as $4\frac{7}{10}$ to 1; lapis tutiæ, as they call it, which is alſo much employed in rheumatick eyes, as very near 5 to 1.

But here I muſt advertiſe you, that I have not found the proportion of each of theſe bodies and water to be any thing near conſtantly the ſame, but ſometimes to differ very much in particular ſtones of the ſame kind; which agrees very well with our hypotheſis. For, according to that, thoſe particular ſtones that happen to partake more plentifully of mineral ſubſtances, heavier in ſpecie than ſtone, as ſuch needs to be, ought to be more ponderous than others of the ſame kind, that are not ſo qualified; I ſaid, heavier in ſpecie than a ſtone, as ſuch need to be, becauſe there are ſubſtances that are reckoned among minerals, and are capable of endowing the ſtony matter wherewith they are coagulated with medical virtues, and yet thoſe ſubſtances may make the ſtone, or aggregate whereof they are made, not to be heavier, but lighter in ſpecie. From jet, which in ſome parts of *Europe* being found in quarries of mines, is indeed a foſſil, which is wont to be reckoned among ſtones, and by many worn as a gem, I obtained no inconſiderable proportion of oil; and having weighed choice jet itſelf in water, I found it to be, bulk for bulk, to that liquor, but as $1\frac{22}{100}$ to 1. And there are ſome other foſſils, hard as ſtone and poliſhable as marble, from which I have, by diſtillation, obtained two kinds of oil, whereof one was lighter than common water; which ſhews, that even bituminous and light ſubſtances may be ingredients of a ſtone; and that ſalts, which are moſt of them leſs heavy in ſpecie than white marble, may plentifully concur to the making up of ſtones, I ſhall have occaſion to manifeſt at the cloſe of this diſcourſe, by thoſe ſtones whereof we in *England* uſe to make vitriol. The foregoing reflection I have here touched upon, becauſe I would intimate to you, that ſtones that are lighter in ſpecie than white marble, may be compounded of foſſils, whence they may derive peculiar qualities, at the ſame time, when I tell you, that in my opinion ſuch ſtones as are conſiderably more heavy in ſpecie than marble, may afford us a ſtrong preſumption of their owing their gravity to the mixture of metalline or mineral ſubſtances. And this may ſuffice for our firſt argument.

Arg. II. The next ſhall be taken from the conſideration of ſome phænomena (relating to medicinal ſtones) which agree very well with our hypotheſis, and will ſcarcely be very well explicated without it.

And, firſt, as to tranſparent gems themſelves, I have learned by inquiry of travellers that have viſited thoſe parts of the *Eaſt-Indies*, where they grow, that ſometimes one ſort of gems, ſometimes another, and ſometimes alſo diamonds themſelves are found included, in the rocks where they are digged for, or in the midſt of hard looſe ſtones, which muſt be broken in pieces, to take out the diamond or other incloſed gem; which phænomenon will be hard to be accounted for, unleſs by our hypotheſis; according to which it may rationally be ſuppoſed, that the gem was firſt formed either in earth, or ſome other ſoft and eaſily permeable ſubſtance, which being afterwards pervaded by ſome petrifick juice or ſpirit, was turned into rock or looſe ſtones, according as the earth and other ambient

bient matter chanced to be an entire and coherent mass, or divided into clods and other portions. And I remember, that the governor of an American colony, having sent me, among other rarities digged up in his country, an odd kind of mineral that seemed more ponderous than at first sight it promised, I had the curiosity to break it, and found in it, here and there, several gems, which by their figuration and some other circumstances, were concluded to have been formed there, before the ambient mineral had obtained the nature it then appeared to be of. And in opacous stones, it may hence happen, that a great lump of medicinal earth may be invaded and petrified after the newly mentioned manner; so that it may not be thought incredible that some of these medicinal stones should be very large in comparison of others: as I remember that an ingenious physician told me of a spleen-stone, as they call them, in the hands of an acquaintance of his (where I might have seen it, if my occasions had permitted) amounting to about fourscore pounds weight. And on this occasion, I also remember, that even in a medicinal stone much harder and heavier than marble, and whereof I have seen lumps far greater than I could lift; I remember, I say, that having had the curiosity to cause a pretty big piece, violently broken off from the mass whereto it belonged, to be sawn asunder, that I might consider the internal textures, as far as it was visible; I found several empty cavities of diff'ring sizes and figures in the solid substance of the stone (which I think I have not yet lost); which seems to argue, that this compact and ponderous body was made of a stony nature, by the supervening of some petrescent liquor or spirit upon porous earth, or some other consistent substance. For if it had been a mere liquor wherein those cavities must have been so many aërial bubbles, it is not like that some of them should have such irregular shapes, and that all should have continued, without emerging to the top.

Secondly, our hypothesis will also help to render a reason of what seems exceeding difficult to be explicated; namely, how some gems, that seem to be entire stones, are in part of one colour, and in that, which is contiguous to it, of a quite differing; of which sort we have the sardonyx, and some other opacous gems. And I have observed the like, though very rarely, in diaphanous ones. For, according to our hypothesis, it may be said, that a portion of matter, imbued with one of the tinctures of the particoloured gem, was first formed, and afterwards, some petrescent juice, endowed with another colour, came to settle contiguously to it, and so by accretion made up one stone with it. I might illustrate this by telling you, that though fire do make a far greater agitation of bodies melted by it, than need be supposed in cold petrescent liquors, yet I have found in making artificial gems, that by some mischance or error in the operation, the mineral pigment has richly tinged one part of the transparent mass, without at all imparting that colour to the very next part to it; so that if I should shew one of those I have yet by me, you would judge it to consist of two differing gems subtilely glewed or fastened together, unless you should in vain try, as others have done, to discover by the eye or otherwise, some naked commissure, which may keep those so differingly coloured bodies from making up one entire mass.

But let us leave these artificial gems, and add to what I was saying about our natural ones, that the union of parts in these resulting stones (if I may so call them) I was speaking of before, might be the more perfect, if the supervening matter found not the first formed stone to have attained to its full induration; though, for aught I know, even in this case, the apposition may be so close, and the two matters so near of kin, that both may pass for one stone, and be polished both together, without any blemishing discontinuity of surface at those parts, where one would expect commissures. For I have by me a lump, wherein there plainly appear stones of colours very different from each other, that

that were once distinct and incoherent; but by some petrescent liquor have had all their intervals so exquisitely filled up, that neither the touch, nor the artificers tool, the lump being now sawn asunder, discovered any commissures; but the whole mass bears an uniform polish, and is harder than divers gems that are worn in rings, readily enough striking fire with a steel. And to confirm this the more, I shall add, that in a place where a prying person of my acquaintance lighted on this portion of petrified matter, he found not only other lumps, but divers loose stones, that seemed altogether of the same nature with those, that by the supervention of the petrescent liquor were united into stony masses. I have also had a curious agate so formed, that it seemed highly probable that the opacous parts of its matter had been some thin, but not altogether contiguous beds of fine clay, or earth, lying almost parallel to each other (but not to the horizon) which by some petrescent liquor that chanced to settle there, was reduced to coagulate with it into a partly opacous and partly diaphanous stone. And of such clays or mineral earths, I have sometimes with pleasure observed more than one or two, which, though distinct, and perhaps of differing colours, were so very thin, that the thickness of them all did scarce exceed an inch, nor did they always lie flat or horizontally, but in differing postures, both in reference to the horizon, and one another, and now and then the exterior ones did successively almost surround the interior; and of these thin couches or layers of earth, I remember, I have observed a considerable number within a very small compass of ground. I must not in this place stay to shew how probable it is, that much after the same way may be explicated the production of divers other gems besides agates, as chalcedonians and jaspers, which are for the most part opacous, but oftentimes have some parts that are not so. But I am content, before I go further, to mind you, on this occasion, of what I elsewhere deliver, that by purposely calcining, without breaking, some of these stones, whose greater part was diaphanous, I found, that the transparent parts turned white; and that some of the thin layers or couches of mineral earth had retained their colour, as well as position, and had it much heightened; so that one of these layers, after calcination, was of a very rich and permanent red. And this difference of colours I observed not only in layers, but in the specks and irregularly shaped clouds, if I may so call them, of other colours, as greenish, blueish, &c. I might here add, that I have found shining marchasites, not only in other solid stones, but in marbles, as also flints themselves, inclosed in great masses of marble, and likewise wood; in strong stones employed to build a wall, and shells, at least as was judged by their shapes and sizes; in a great mass of stone that I met with almost on the top of a hill remote from the sea, together with divers other such phænomena, which I think may probably be accounted for by our hypothesis, and scarce without it. But being willing to dispatch this discourse, and unwilling to intrench upon the discourse of the effects of the petrescent juice, to which the consideration of these and divers other phænomena, to be met with about the generation of stones and petrified bodies, especially in wombs or molds, more properly belongs: I shall in this place only point back to one observation, and answer one objection; because both of them are pertinent to our present discourse.

The observation is this: that even in transparent gems, and which is more, of the self-same species, I have sometimes taken notice of such an aggeneration or accretion of stones, to one another, as argues their having been produced at several times. For proof of this, I need no more than refer you to what I have not long since related about those Cornish diamonds, wherein, sometimes, a lesser stone, though geometrically shaped, was found in good part inclosed in a greater, as well as in part also extant above it. Whence I argued, that the production of this aggregate of two crystalline bodies was not

not made all at once, but successively, and that the lesser was first formed, which I shall now confirm by this consideration. That if the greater stone had been first hardened, the matter of the lesser must only have exteriorly stuck to it, and been, as it were, imbossed upon it; but could not have made itself, in the substance of the greater, a bed or mold, especially of such a geometrical figure as itself had not yet received.

AND though this successive generation of the parts of seemingly entire gems, may appear to you somewhat new and strange; yet, that its fitness and requisiteness to explain the foregoing phænomena, and others to be hereafter mentioned, may the more recommend it to you; I shall add, that, perhaps, you may be assisted to conceive, if not invited to admit it, by a mechanical illustration. For we see, in divers chymical solutions, as of salts, and other bodies, that there are certain stages, or periods, of coagulation; so that, when such a quantity of the superfluous moisture is exhaled, especially upon any considerable refrigeration or other favourable circumstance, those particles that are most disposed to coagulation, will convene, and shoot into crystals; after which, no more will do so, till a farther and more considerable evaporation of the water or other menstruum, be made; upon which will ensue a new crystallization of the parts. And I can shew you the productions of a metalline but uncommon solution, that I so made in an appropriated liquor; that the first shooting afforded me a layer, or bed of curiously figured crystals, and the following another layer of fine crystalline bodies, that have fastened themselves to the former, but differ notably from them, both in shape and posture. And in this experiment, the dissolved body was but one, as the menstruum but one; but if there be a diversity of nature in the liquors that make up a menstruum, or in the bodies that are dissolved in it, some of the corpuscles may convene either apart with those of the same nature, or mingle with those of a differing nature, but yet at the same time; and so make up crystals of a compounded nature; and some of them may convene with homogeneous particles, but at differing times; and so miss of such uniformity as might else appear in their concretions. Which may be illustrated by what I have elsewhere related concerning the crystallizations of salt-petre and sea-salt, dissolved together in ordinary water; where, most commonly, grains of salt of resulting figures are produced; and also a considerable part of the sea-salt coagulates in the form of imperfect cubes, about the bottom, before the nitrous corpuscles shoot into crystals of their own almost prismatical shape. And I might further add, that it matters not, whether the superfluous water be wasted by exhalation, or by being drained by a body fit to soak it up; as we have had occasion to observe in accelerating the crystallization of some bodies, where I was not willing to employ the heat of the fire, by placing underneath the solution, dried earth, or some other porous and soaking body.

WITH some analogy to such instances as these, we may conceive, that where there are petrescent liquors, mingled with common water, there may, by divers accidents, and particularly an hot summer, a sufficient discharge be made of the superfluous moisture, to make the more disposed parts of the petrescent liquor to coagulate; and afterwards the coagulation may be suspended, either by the supervening of a colder season, as winter; or even in summer itself, by a plentiful rain, or the effect of it, a land-flood, which might check the progress of coalitions by over much diluting the liquor, that might else have turned into stone. Not to mention, that trial hath assured me, that there are bodies, and those of very differing kinds, which will, in tract of time, especially if their coalition be furthered by cold weather, coagulate, after they have long remained in a fluid form, though the water, or other menstruum, by being inclosed in stopped glasses, be kept from wasting. And since the earth harbours differing kinds of these liquors, as I have elsewhere shewn, and divers of them may be copiously impregnated,

nated, ſome of them with one ſort of mineral, and ſome with another; we may conceive, that they may have diſtinct periods for their reſpective coalitions, and yet may ſtick cloſe to one another; in regard that, though in our chymical cryſtallizations the artiſts are wont to take out of the veſſel what ſhoots the firſt time, before they make a freſh exhalation of the water for a new cryſtallization, and by this means have the coagulated bodies that they obtain at one time more uniformly ſhaped; yet in the hollow receptacles that the earth affords to petreſcent liquors, the veſſels continuing the ſame from firſt to laſt, the uniformity of the bodies produced by coalitions, made at ſeveral times, muſt be leſs regular; and the manifeſt accretions or aggregates of coaleſcent bodies muſt, in all likelihood, be more frequent. And accordingly having ſuffered the exhaling of ſome liquors to be continued in the ſame veſſel, I had coalitions of very differing bodies at the bottom.

What I was not long ſince ſaying, makes me remember, that, in order to a ſatisfaction, which the event gave me, of the conjectures I had about the ſucceſſive concretions of ſome ſolid fire-ſtones, that were not ſuſpected to be other than entire and uniform maſſes, I cauſed two or three, that I thought likely, and of very different ſizes and ſhapes, and brought from diſtant places, to be warily broken; which trial gave me the pleaſure of obſerving, that the internal texture of the leaſt of theſe minerals, which was almoſt ſpherical, was very differing from that of the more internal part of the ſubſtance of the ſtone; and that in the other and greateſt mineral, there was a little globulous ſtone that manifeſtly was not of the ſame piece with the invironing maſs, differing from it not only in texture, but here and there by a diſcernible commiſſure; though in moſt places, their adheſion was ſo ſtrict, that we could not make any ſeparation of the two minerals by the help of this commiſſure. The greateſt part of this double fire-ſtone I keep by me, and ſhall ſay nothing of what I further obſerved in it, having mentioned what I ſaid already but upon the by.

I might add, that in ſome circumſtances, even in cloſe veſſels, and therefore without any manifeſt exhalation of the water, or other menſtruum, and, ſometimes, where the diſſolved body was homogeneous, I have, in proceſs of time, had coagulations, where the laſt formed cryſtals ſeemed plainly to have been generated by way of accretion to the firſt.

Difficulty. Having now done with my obſervation, I ſhall endeavour to clear a grand difficulty, which, I foreſee, may be objected againſt our hypotheſis; namely, that theſe aggenerations, if I may ſo call them, of medicinal and other ſtones, are ſometimes found in places where there are no petrifying ſprings, and perhaps no ſprings or other waters at all; nay, little or nothing but quarries or other maſſes of ſtone.

But to this I anſwer, firſt, that if we admit of the relations that I elſewhere mention out of approved authors, concerning men and beaſts turned into ſtone by a petrifying ſpirit that ſuddenly invaded them, it will not be abſolutely neceſſary that there ſhould be any petreſcent ſprings, or other-like water, to produce ſuch minerals as we are now diſcourſing of.

Secondly, for aught has yet been ſhewn to the contrary, we may ſuppoſe, that rain-water does ſometimes bring along with it ſuch petrifying particles as may ſerve our turn. In confirmation whereof, I ſhall add, that having, of a learned and judicious perſon, enquired after divers particulars relating to a famous bath, by him viſited in *Hungary*, whoſe water abounds very much with petreſcent particles, over which there is very high building erected, I learned by his anſwers, among other remarkable things, that to the roof, or upper part of this tall ſtructure, there were faſtened many long ſtony concretions (like thoſe wont to be employed to adorn grottoes) which he affirmed to be, from time

time to time generated there; not, as I at first suspected, by the dashing up of any drops of water (which he averred could not reach any thing near so nigh) but by the copious petrific steams that being there checked in their ascent, did, according to their natural propensity, coagulate into stone. Whether this relation may warrant me to guess, that, in some places, stones may be generated without the help either of rain, or springs, by the ascent of petrific particles, in the form of exhalations, from some lower parts of the earth; which exhalations, suffering the lighter steams that accompanied them to exhale, may operate upon some disposed materials that they find in their way, and turn them into stone; whether, I say, this narrative may well suggest this conjecture, I shall not now stay to examine, though the earthy and sometimes sulphureous sediments that have been observed at the bottom of rain-waters, suffered to settle in clean vessels, may seem to favour it; and though also I might illustrate it by what I observed in a bottle of distilled liquor, whereof no part would naturally ascend in a dry form; for having kept this phial well stopped in a safe and quiet place for a year or two, I observed, that the ascending steams had quite pervaded the cork, and had formed, at the top of it, numerous whitish stiriæ, slender, but of a length that surprized me.

Thirdly, there is no necessity that in all soils where petrific waters are to be met with, there should be petrifying springs, at least above ground. For I have caused to be digged store of figured and transparent stones in a certain earth that lay upon the upper part of a rock, and seemed to be a very dry soil; perhaps you will allow me to tell you, that I have, by pouring a solution of stony stiriæ, made with spirit of verdigris, on a convenient quantity of bolus Armenus, and suffering the soft mixture to remain in a glass in the open air, till the superfluous moisture was exhaled; I have, I say, by this means, imitated in a little, what I have been now relating, and found small, but untinged and figured crystals dispersed through the little cavities of the red earth. But it will be more considerable to our present purpose to add, that the fairest and hardest petrifying wood that I ever had, or tried, was taken up by an ingenious person I employed in a plot of sandy ground, where he could not find any petrifying, or so much as any other spring. To which I know not whether I should add, that supposing the ground to have been once moistened with a lapidescent liquor, whether brought thither by springs or any other way; one may, in our hypothesis, well enough account for this difficult phænomenon, that now and then, not only in the surface of the ground, and perhaps upon rocks themselves, there are found aggregates of figured stones that seem to grow upwards, as it were, from a root; which much puzzle men to know how they came there, and may incline them to their opinion, who ascribe vegetations to stones. But to this may be answered, that many of the concretions we are speaking of may have been formed in wombs, that lay, though not deep, yet under ground, or in shallow cavities in the surface of it; and that after their formation, the looser earth that surrounded them, may have been washed off, by rains blown off by winds, or otherwise removed, leaving behind them these stones that adhered firmly to a solid body. Besides, if I had time, I think it were possible for me to shew, that stony concretions might be produced by the mechanical action of the air, upon the stony particles that successively apply themselves to the matter, that first begins to coagulate, when they are ready to be forsaken by the moisture that accompanied those particles, and was necessary to their due application to the casual rudiments (which pass for roots) in imitation whereof, I have, more than once, obtained both from saline and stony solutions dry tufts of prettily figured, and diaphanous or white, but very slender stiriæ, if I may so call them, that seemed to grow out of the solid glass, and made men wonder how they came thither, no water, or other liquor appearing near them.

 Fourthly,

Fourthly, It may very well happen that the petrescent liquor may be so mingled and diluted with ordinary water, as not to be distinguished from it by the generality of men, nor to be capable of disclosing itself by its effects, till either by the copious exhalation of the common water, or by some peculiar advantages it has to operate upon bodies, it has opportunity to discover itself. On which occasion I shall add, that there is a lake in the north of *Ireland*, wherein I could never hear but that fishes lived as well as in other lakes, and yet there are some rocks near the bottom of it, to which there fasten themselves divers masses and other pieces of a finely figured substance, and transparent as crystal; of which an eminent person, the chief owner of the lake, presented me with some, and promised me more. Now, if we suppose that either by springs of petrescent water, or by rains, or by subterraneal steams, or otherwise, waters resting in any hollow place, though upon the top of rocks and mountains, shall be sufficiently impregnated with petrifick particles; and that afterwards, in process of time, the merely aqueous parts shall be, by degrees, by the heat of the sun, the soaking of the grounds, the winds, or the continual action of the air, brought to exhale away in the form of vapours, the petrifick particles, which are not so volatile, will turn the soil beneath them, and on the sides of them, as far as the sphere of their activity reaches, into stone harder or softer, of this or that kind, according to the particular nature of the petrescent liquors, and the structure and other dispositions of the soil they invade; in which soil, if there chance to be lodged bodies heterogeneous to it, whether vegetable substances, as roots, pieces of wood, gums, &c. of the whole bodies of animals, as toads, frogs, serpents, fishes, &c. or their parts, as shells, bones, &c. or minerals of an open texture, as boles, unripe ores; or else gems or stones of another kind already formed; any of these things, or any other that shall chance to be lodged there, must be found either petrified, or inclosed in stone, when this changed and hardened soil shall come to be broken up. Nor is at all necessary, that this petrifaction of the extraneous bodies, and of the soil or bed, be made at once; for it may well be made successively at several times, according as some parts of the petrescent juice happen to be more copious and penetrant, and consequently more fit to be soaked in further than other. For, as the porousness happens to be greater in one part of the soil, than in another; or, as the texture and disposition of particular bodies, lodged in the earth, gives advantage to the petrifick particles to work on some of them, sooner, or in a differing manner, than in others; so the induration of the pervaded matters may be very unequally made in point of time, as well as in other circumstances. So that (to omit many other things explicable by it) we may, from what hath been already delivered, conceive how it may happen that medical stones of very differing colours, consistencies, and operations (of which I have several by me, that I had from the same mineral mass) may be generated, and seem entire bodies, though (as in some that I found) the difference is great, that so one part of the medical stone is dark, heavy, and opacous, and the other much lighter, transparent, and quite otherwise coloured. And upon the same principle may be explained, what I lately mentioned to you about the finding of diamonds enclosed in loose stones and even in rocks; of which we have credible testimony, which seems not more strange to me than a stone, which I have by me, which being a kind of pebble, contains in it a perfectly shaped serpent, coiled up, but without a head, which appears to have been formed before the stone, in regard, that in the upper and lower parts of the solid stone there are cavities left, which together make up one cavity, just of the size and shape of the contained body; to which, as it was easy for the matter of the stone, whilst it was yet a soft body, to accommodate itself exactly; so it is scarce conceivable, how, if the pebble had been first formed, the inclosed animal, if it were one, or the matter whereof the seeming animal afterwards

afterwards was formed, ſhould not only get in, but find a cavity ſo curiouſly ſhaped, and ſo fitted to its bulk. And that this variety was produced at ſeveral times, might be further argued from this, that the ſeeming ſerpent is plainly of another and clearer kind of ſtone, than that of the mould that encompaſſes it; and of the mould itſelf, one part, contiguous to the included body, is whitiſh, and abounds in ſhining grains or flakes; in both which, it differs from the other and far greater part. And now it will be time to haſten to the

FIFTH conſideration, which is, that, for aught we know, in thoſe very places, where now there is nothing to be ſeen but looſe ſtones, and perhaps beds of ſtones themſelves, that in thoſe very places, I ſay, there may in times paſt have been petreſcent liquors, whether ſtagnant or running. For, I elſewhere * ſhew (to another purpoſe) that earthquakes, inundations of ſeas and rivers, ſinkings of ground, encroachments of the land on the water, fiery eruptions, and other ſuch accidents (ſome related by authentick authors, and others happening in our own times, in places, ſome of which I had the curioſity to ſee) have, among other odd effects, been able to dry or choak up pools and lakes, and to ſtop and quite divert the courſe, not only of ſprings, but of rivers, ſo as to leave no footſteps of them, where they plentifully flowed before. Upon the ſcore of which tranſpoſitions of notable quantities of terreſtrial matter, and other great changes of the ſtructure and diſpoſition of the ſoil in divers places, it may well be ſuſpected, that the ſtony wombs or moulds, wherein the above-mentioned bodies were found, were heretofore, at ſome time or other, of a muddy or earthy nature, and were receptacles of petreſcent liquors, which, at ſeveral times, turned the whole maſs of the ſoil into ſtone, before the ſprings, or other waters, containing the petrific liquors or ſpirits, were quite conſumed, or had their courſe altogether diverted. But though I could ſay much more to confirm and apply this, and the preceding conſiderations, yet having ſpent ſo much of my time already, I ſhall not only leave all that unſaid, but, to make ſome amends for having ſtaid ſo long in clearing this difficulty, I ſhall do little more than name the two remaining arguments.

Arg. III. It agrees very well with what we were formerly ſaying (in the firſt argument) about the great ſpecifick gravity of ſuch, as the newly-mentioned ſtones, in compariſon of that of white marble, or tranſparent pebbles, that it ſhould be poſſible, out of thoſe minerals, to extract ſome of that ſubſtance, whether metalline, or of kin to it; upon whoſe account, I told you, I ſuppoſed them to be ſo ponderous. And accordingly we have, by appropriated menſtruums, obtained from the fore-mentioned bodies, and not from thoſe only, ſolutions or tinctures, which, beſides that, by their colour or taſte, they diſcover themſelves, did, upon their being dropped upon a ſolution of galls, or ſome other convenient liquor, or upon their being examined by other proper ways, produce ſuch changes of colour, or ſuch determinate phænomena, as argued them to abound with metalline, or mineral particles, which, for the moſt part of them, I obſerved to be of a vitriolate nature; ſo I found, that the ſolution of a blood-ſtone, which taſted very rough upon the tongue, would, with the infuſion of galls, make an inky mixture; and the like would alſo be made with load-ſtone, emery, marchaſites, &c. opened with corroſive menſtruums. But the ſolution of lapis calaminaris, which was of a golden colour, did not operate like the reſt on the infuſion of galls; but yet by its taſte, as well as colour, ſufficiently diſcovered itſelf to have copiouſly impregnated the menſtruum. And now the mention of lapis calaminaris minds me, to take thence an inſtance of what I lately intimated, that there may be other ways, beſides that of diſ-

* In an examen of an experiment urged for the magnetiſm of the earth.

ſolutions in proper menſtruums, to ſhew, that ſome medicinal ſtones participate of metalline and mineral ſubſtances. For it is by melting lapis calaminaris with copper, and keeping them together for a competent while in fuſion, that braſs is made; wherein the red colour of the copper is changed into a golden one, and the abſolute weight (for I ſpeak not of the ſpecifick gravity) conſiderably increaſed. Nor is this the only mineral ſtone, from which I have, by a way quite differing from thoſe I have yet mentioned, namely, with running mercury, obtained a metalline ſubſtance. And though native cinnabar, uſed by eminent phyſicians both inwardly and outwardly, be looked upon by the vulgar as only a red ſtone; yet it is known, in the quickſilver mines of *Friuli*, and ſome other places where it abounds, that it is a mercurial ore, whence, by vehement fires, they diſtill running mercury, which we, by moderate ones, have ſometimes done.

But here perhaps it may not be improper to tell you, that though before any admonition given men of the expediency of examining ſtones hydroſtatically, I could not receive from others, yet I made againſt myſelf the following objection, that there are ſome ſtones, to which uſeful qualities are aſcribed, which are either not at all heavier in ſpecie, than is requiſite for a ſtone, as ſuch, to be; or ſo little heavier, that it is no way likely, that metals, or any ſuch ponderous minerals, ſhould contribute either to their productions, or their virtues.

In anſwer whereunto, I thought it may be ſaid in the firſt place, that our hypotheſis does no way oblige us to deny, that there may be ſuch ſtones. For though it aſcribes the virtues of moſt gems, and metalline ſtones, to the metalline and ponderous mineral ſubſtances they partake of, yet the conceſſion agrees very well with our doctrine; which (as will in the fourth argument be more manifeſted) ſpeaks in general, when it teaches, that the virtues of ſtones may, in many caſes, depend upon their conſiſting not of a pure petreſcent ſubſtance, but a ſubſtance impregnated with other minerals, which, though moſt commonly they prove ſpecifically heavier than the petreſcent matter, as ſuch, without being the leſs, but rather, in ſome caſes, the more operative and communicative of their virtues; yet, in divers ſtony concretions, the adventitious ingredients may be ſpecifically lighter than the genuine matter of the ſtone; as may be eaſily gathered from ſome paſſages of the foregoing diſcourſe. For, not here to urge, that divers bodies, that paſs for ſtones, do abound in particles of ſalt, which may be much leſs heavy than pure ſtone of the like bulk, I have obſerved, that ſome other hard foſſils abound with a kind of bitumen, which, when by diſtillation brought to an oil, is much leſs heavy than a ſtone of the ſame bulk. And, as I remember, I have had ſome portions of ſuch oil, that would ſwim even upon common water; and, leſt this ſhould be aſcribed to the ſubtilization, the bitumen received from the fire, I will add, that having hydroſtatically weighed a piece of good aſphaltum, we found it to be to water of the ſame bulk, but as 1, and ſomewhat leſs than $\frac{4}{10}$ to 1. Which was within a tenth of the proportion to water of a ſtony, though a bituminous foſſil, commonly called in *England* Scotscoal. And becauſe ſulphur, as well as bitumen, is very apt (and indeed, more apt, than before trial I expected) by even a moderate heat, or attrition to diffuſe its ſteams (uſually rank-ſcented enough) I ſhall add, that there are variety of hard ſtones, which abound in ſulphur: (witneſs, that in ſome places they obtain their common brimſtone by ſublimation thence) and yet having weighed a roll of brimſtone in air and water, I found it to be but a fraction ſcarce worth mentioning above double its weight to the liquor; which ſhews it to be much lighter in ſpecie than cryſtal itſelf.

An improvement of this firſt anſwer may furniſh me with the ſecond. For hence we may argue, that it is not impoſſible, that the principal virtue of a light medical ſtone ſhould fhould be due to fome mixture of a metalline, or the like ponderous fubftance; fince, if fome of the ingredients, that are plentifully mixed with the true ftony matter, be of the lighter fort, though there be alfo fome metalline, or other heavy mineral particles mingled with the fame matter, yet the fpecific levity of the one, in comparifon of this matter, may compenfate the fpecifick gravity of the other; and they may all compofe a ftone, either lefs, or not more ponderous than white marble. On which occafion, I remember, not only that I found a blackifh Eaft-Indian flint, and likewife a black Englifh one, to have to water not full the proportion of $2\,\frac{6}{10}$ to one, but that one of the firft pieces of black marble, that I examined hydroftatically, was found, notwithftanding the darknefs of its colour, to be to water of the fame bulk, fcarce any thing more than $2\,\frac{7}{10}$ to 1, which, you may remember, was the proportion I found between white marble and water, unlefs we fhould fay, that this blacknefs of colour proceeded, not fo much from any grofs bituminous matter, imbodied with that of the ftone, but from fome mineral fmoke that had pervaded it. And this puts me in mind of fpeaking fomething in this place about what might properly enough have been difcourfed of long ago.

Wherefore I fhall fubjoin, in the third place, that it feems not impoffible, that the matter, which medical ftones are made of, may, before it comes to be hardened, derive various colours, and be imbued with virtues by fubterraneal exhalations, and other fteams. This, I fear, you will think fomewhat ftrange, and therefore I fhall briefly endeavour to confirm it by the mention of two or three particulars.

That then many places of the lower part of the earth emit copious exhalations into the upper, and even into the air itfelf, I prefume you will grant, and I have elfewhere proved it. That alfo fuch fubterraneal fteams will eafily mingle with liquors, and imbue them with their own qualities, may be inferred from the experiment of mixing the gas (as the Helmontians call it) or the fcarce coagulable fumes of kindled and extinguifhed brimftone, with wine, which is thereby long preferved. And I have elfewhere mentioned, how I have incorporated this fmoke with other liquors, wherein I obferved its operations to be notable.

That beneath the furface of the earth there may be fulphureous, and other fteams, that may be plentifully mixed with water, and there, in likelihood, with lapidefcent liquors, I have alfo manifefted in another * difcourfe.

That quickfilver may be in part refolved into fumes by lefs fires than many of thofe that burn under ground, will be readily acknowledged by chymifts and gilders, and is obvious in the fumigations employed in the cure of the lues venerea. And that mercury may in the bowels of the earth be fo difguifed, and well mixed with ftony matter, as to fuffer the whole concretion to pafs for ftone, may be obferved in fome kind of native cinnabar.

That fal armoniac, of which, in fome places, there is to be dug up ftore, will with a moderate fire be made to afcend in form of exhalations, is vulgarly known, as to the factitious falt of that name; and I have found it to hold in the native. That common fal armoniac, fulphur, mercury, and tin, will be fublimed into a gold-like fubftance, that participates of moft, if not of all the ingredients, may appear by the account I have elfewhere given of the way, I ufed, in making aurum muficum: and that even gold itfelf, the heavieft and fixedeft of the bodies we know, may, by no great proportion of additament, and that with but a moderate fire, be made to afcend in the form of fumes, or even of flame, I have feveral times tried, by ways elfewhere delivered. And that

* Of fubterraneous fteams.

mineral

mineral exhalations may be met with in the bowels of the earth, is witnessed by the relations of divers credible persons, conversant about minerals, that affirm themselves to testify what they write upon their own observation, to which some things, that I had seen myself, did the more incline me to give credit. And this copious ascension of mineral fumes, and even of metalline ones, may be much confirmed, not only by what is written by professed chymists, but by the learned and curious *Johannes Kentmannus*, who, in the useful catalogue of the *Misnian* fossils he had collected, amongst the pyritæ or fire-stones, reckons one, whose title is, *Pumicosus, & ab exhalatione ardenti nigro colore tinctus*; and another whose inscription is, *Coloris argenti, qui ab exhalatione virosa colore cinereo est tinctus*. The same may be further confirmed by what I have somewhere met with, as related *in terminis* by the learned *Cabæus*, that he found in the territory of *Modena*.

To bring this home to our purpose, since there are mineral exhalations of very differing kinds, dispersed in divers places under ground, and since there are several volatile minerals, as arsenic, orpiment, sandarach, &c. that are very actively hurtful; there may be others endowed with medical qualities, and the exhalations of such minerals, either alone, or mixed with petrescent liquors, pervading duly-disposed earths, and bolusses, and other fluid, soft, or open substances, before their induration, may endow them with medicinal and other qualities.

Nay, when I recall to mind the old phænomena, that I have partly observed, and partly received from credible testimony, about the coalitions, mixtures, tinctures, and the emanations, as it were, of those tinctures in metalline, stony, and other fossile concretions; I dare not peremptorily deny, but that even after subterraneal bodies have obtained a considerable degree of induration, and perhaps great enough to make them pass for stony ones, there may be subterraneal steams subtle enough to penetrate, tinge, and otherwise impregnate them. Which you would think the less impossible, if you reflect upon what I just now related out of *Kentman*; and especially, if I had time to add here, what I remember I elsewhere delivered about my trials to tinge native crystal with differing colours, by the fumes of volatile minerals. And that a very small proportion of a metalline substance, resolved into minute particles, may suffice to impart a tincture to a greater quantity of other matter duly disposed, may appear, by those factitious gems, wherein, with three or four grains of a skilfully calcined metal, or some such mineral pigment, we may give the colour of a natural gem to a whole ounce, or more, of vitrified matter. And I remember, that in subtiler fluids, I have made the instance by vast odds more conspicuous, having tinged with one grain, or less, of a prepared metal, as gold or copper, as much successively generated phlegm, as, if it could have been all preserved, would have amounted to a bulky lump of deeply-coloured matter.

But your allowing the hesitancy I have expressed in this last paragraph is not necessary to my present purpose; wherefore I shall not borrow any thing to countenance it from another paper, but pass on to what remains.

Arg. IV. The last thing that I shall represent, to shew that the virtues of opacous gems, and medicinal stones, may be more easily, than those of transparent ones, accounted for in our hypothesis, is this, that the main ingredients, whereof many such opacous stones consist, were complete mineral bodies before they became stones; some of them having been medicinal bolusses, or the like earths; some earths abounding with metalline or mineral juices; some, ores of metals, or minerals of kin to metals; and some, in fine bodies of other sorts, or natures, differing from these and one another. For all these several kinds of fossils may, by the supervening and pervasion of petrific spirits, be turned into stone; and consequently retain many of the virtues, they were indowed with by the mineral corpuſcles, that had copiouſly, either under the form of liquors, or exhalations, impregnated them, whilſt they were yet earths, or other bodies of a more open or penetrable texture.

I MIGHT illuſtrate this by the way I elſewhere mention, whereby I made ſuch mixtures, even of ſtony and metalline ingredients, that, notwithſtanding their coalition, were tranſparent; though you will grant that to be more difficult, than to compound ſuch concretions when one is allowed to make them opacous.

BUT here I muſt obviate an objection, which I foreſee may be made againſt our preſent fourth argument, unto which, even what I have been now ſaying, may afford a riſe. For ſince it ſeems by our doctrine, that gems may be but magiſteries, and conſequently but ſuch compoſitions, as, though made in the bowels of the earth, might be made or imitated by human ſkill, it may ſeem very improbable to many, that bodies ſo near of kin to artificial ones ſhould be endowed with ſuch peculiar, and, ſome of them, with ſuch ſtrange virtues, as are aſcribed to divers gems, and are thought to be capable of flowing only from certain ſubſtantial forms, and thoſe very noble ones too.

To this I might reply, that I admit not any ſuch imaginary beings as the Peripatetic forms, which, I fear, they will never be able to demonſtrate. But to avoid unneceſſary diſputes, I will rather anſwer in ſhort, that ſuch compoſitions, as are called artificial, may, for all that, be endowed with great virtues, and ſuch as are called ſpecifick; witneſs the virtues of many chymical preparations, even of thoſe that are uſed by phyſicians of all ſorts. And leſt you ſhould think, I need to fly to chymiſtry, of which ſome learned men are pleaſed to have a great diſtaſte, I will name a couple of inſtances out of *Galen* himſelf; the one is the aſhes of cray-fiſh, to which, notwithſtanding the deſtruction that has been made of the priſtine body by fire, he gives a greater commendation againſt the as ſtrange, as fatal poiſon infuſed by the biting of a mad dog, than he does either to the fiſh itſelf unburned, or to any medicine of nature's own providing; and I hope, you will grant a virtue of that kind and degree to be ſpecifick enough. My other inſtance ſhall be taken from treacle, which, though allowedly a factitious body, and conſiſting of I know not how many ingredients ſhuffled together, was yet, in the days of *Galen*, to whom a book is attributed about it, and ever ſince has been, the famouſeſt antidote, in theſe parts of the world, and has been celebrated, not only for its alexipharmical virtues, which alone are ſufficient to intitle it to ſpecifick ones, but for divers others, which are generally aſcribed to it, ſome indeed upon the ſcore of manifeſt, but others alſo upon that of occult qualities.

THE objection being thus diſpatched, we may return to our medicinal ſtones, about which I ſhall venture to add, that, according to our way of explicating the production of them, a not impoſſible ſolution may be offered of this difficult phænomenon; that ſometimes ſtones, that are thought, without ſcruple, to be of the ſame kind (as hath been particularly obſerved by learned men of the lapis nephriticus) are of ſuch different qualifications, that ſome of them prove very conſiderable remedies in caſes, where others prove almoſt utterly ineffectual. And I have obſerved alſo, though very rarely, that a medical ſtone may have virtues, that are taught to be the properties of ſtones of another kind. For, according to our hypotheſis, when the ſtony matter is impregnated, as it ought to be, with thoſe minerals, that in the ordinary courſe of nature belong to that ſpecies, its virtue will be ſuch, as it ſhould be for kind, but for degree may be very various, anſwerable to the plenty, purity, ſubtlety, &c. of the mineral that impregnates it. But if the ſtony matter chance to be imbued with ſome other ſubſtance of a contrary nature, though, perhaps, the proportion of it may be ſo ſmall, and the colour of it ſuch, as not to make an alteration in the ſtone obvious to ſenſe, and great enough to make it judged

See *Unzerus* de nephritide.

judged to be of another ſpecies; yet it may ſo vitiate the matter, wherein its expected quality reſides, or check and infringe its operations, as not to leave the ſtone any conſiderable degree of virtue. And on the other ſide, if it happen, that the mineral corpuſcles, that are wont to impart a certain virtue to the ſtony matter of one gem, ſhould, by ſome lucky hit, be ſo united with that of another ſort of gems (of which caſe I formerly gave an inſtance in green diamonds) though the quantity of this unuſual ingredient may be but very ſmall, yet, if its efficacy be great, it may ennoble the ſtone with a notable degree of ſome ſuch virtue, as is ſuppoſed not to belong to that ſpecies, but to another.

And on this occaſion I ſhall add, that I know a gentleman, a profeſſed ſcholar, who to the eye ſeems to be of a complexion extraordinary ſanguine: this perſon was for a long time ſo troubled with exceſſive bleedings at the noſe, that notwithſtanding all the remedies he could procure in an academy of phyſick, where he lived, he was divers times brought to death's door; till at length his caſe growing very famous, there was ſent him by an ancient gentlewoman a blood-ſtone, about the bigneſs of a pigeon's egg, with an aſſurance, that it had done ſcarce credible cures in his diſeaſe, by being worn about the patient's neck. Upon the uſe of this ſtone he quickly recovered his health, and had long enjoyed it, when I converſed with him, but yet ſo, that when he left it off any conſiderable time, his diſtemper would return. And when I ſeemed to ſuſpect, that imagination might have an intereſt in the efficacy of his remedy, he anſwered, that he was very well ſatisfied of the negative; and particularly upon this trial, that he had, by the hands of a third perſon, that lived not far off, and whom he named to me, ſtopped a hæmorrhage in a neighbouring gentlewoman, whom the violence of the diſtemper kept from knowing, that any thing had been applied to her, till a pretty while after the blood was ſtanched. I ſhall not here mention other inſtances, though very remarkable, of the efficacy of this ſtone, which I had, both from the gentleman itſelf, and an intimate friend of his, who is a very learned man and a phyſician; becauſe I have ſaid enough to make it ſeaſonable for me to tell you, that notwithſtanding all the odd operations of this ſtone, when I came to look upon it, it was ſo differing in colour and texture from what I expected, that I ſhould have taken it much rather for a gem of ſome other ſpecies, than a blood-ſtone.

To confirm ſome of the particulars comprized in this our fourth argument, and ſhew the variety, and ſometimes great plenty of mineral and other ſubterraneal matters, that may concur to the compoſition of bodies, that paſs for ſtones; I ſhall obſerve, that the ſubtility and penetrancy of ſome liquors, if duly conſidered, may evince it to be poſſible, that ſuch bodies ſhould be petrified by them, and with them, as may in part conſiſt of animal and vegetable ſubſtances, as in petrified ſkulls, bones, and pieces of wood: and we ſee, that ſoft ſtone, which is plentifully found near *Naples*, and commonly called the lapis lyncurius, being rubbed a little and moiſtened with water, and then expoſed to the ſun in a due ſeaſon of the year, will, in a very ſhort time (as eye-witneſſes have aſſured me) produce muſhrooms fit to be eaten; as if even the ſeminal principles and rudiments of vegetables may be ſo preſerved in a petrified earth, as to be able to diſcloſe themſelves, when they find an opportunity. To which agrees well what an eminent perſon, maſter of ſome of theſe ſtones informs me, that they now and then find them of a vaſt bigneſs, as if whole maſſes of earth, pregnant with the prolifick principles of muſhrooms, were, by ſome ſupervening, but not very potently hardening petreſcent liquor, turned into ſtone.

And not only there may be boluſſes, ſealed earths, and ſuch like foſſils, that are commonly known to be medicinal, hardened into ſtone, by petrifying agents; but alſo other earths,

earths, subject to be petrified, may have medicinal and subtle particles of such a kind in them, as scarce any body would expect. But to omit instances, belonging to another paper, I have visited a certain clay-pit in a waste piece of ground, in which, at a considerable depth from the surface of the earth there lay a bed of clay, which by distillation yielded some acquaintances of mine a salt so volatile and strong, and so differing from other subterraneal salts, that my examens did not discover the manifest qualities of it without some wonder; and the owners of it (persons curious and rich) did themselves use it as well as give it in physick, and cried it up for an excellent cordial, and a great opening and diaphoretic medicine.

THAT sublimable salts, sulphurs, bitumen (bodies, that communicate enough of their virtues) may be met with in the bowels of the earth, I have elsewhere shewn: and that such substances may be found in bodies that pass for stones, I have been induced to think by the chymical examen, that I purposely made of some such concretions, particularly of that solid and heavy one, that is commonly called Scotch-coal, from whence I obtained by distillation (wherein I somewhat wondered, other mens curiosity did not, as far as I knew, prevent me) a good proportion of oil or liquid bitumen, and no small number of saline particles, that seemed to be of an uncommon nature.

THAT metalline particles may concur to make up a body, that passes for a medicinal stone, may appear by native sulphur, which is itself a compounded body, besides a good proportion of mineral earth.

I HAD thoughts not to make an end of this discourse, without mentioning to you some attempts that I partly designed, and partly made, to illustrate some passages of it by purposely contrived experiments, whereof some were unprosperously, and others not altogether unsuccessfully tried. But not having the minutes of them by me, and not daring to trust my single memory in experiments so nice, and so long since made as those were, I shall here put an end to your trouble; especially since at length I perceive, that the forgetfulness of my first intended brevity has misled me so far beyond the bounds of it into excursions, whereinto the unforeseen connexion of things unawares engaged me, that I stand in need both of your pardon and my own; of yours, for having exercised your patience with a prolix discourse; and of my own, for having receded from my custom, by contributing to that prolixity, and by expatiating upon conjectures; to which the more I conform to my own practice, the less I am indulgent: though these may be the more pardonable, because I have proposed them but as guesses, not peremptory assertions, much less physical demonstrations. And if *Aristotle* himself, where he gives an account of phænomena appearing above the surface of the earth, scrupled not to think he had done enough, if he had shewn how such things may be produced; I hope it may be tolerable in me, who treat of things, that nature does privately in her dark and subterraneal recesses, to have offered accounts, that are possible, if not probable. And yet I should have spent much less of my discourse upon conjectures, if I had not seen that they gave me rises to bring in more of natural history, than I could else decently do. But after all this, I confess to you (though you may think it a paradox) that one of the main causes of the prolixity of these papers was my haste, and that experience hath taught me on this occasion (as well as on some others) that there may be more truth, than there is likelihood, in the genteel conceit of a French secretary, that said, he had written his friend a long letter, because he had not leisure to write him a short one.

TRACTS.

CONTAINING

NEW EXPERIMENTS, touching the Relation betwixt FLAME and AIR. And about EXPLOSIONS.

An HYDROSTATICAL DISCOURSE, occasioned by some Objections of Dr. HENRY MORE against some Explications of NEW EXPERIMENTS made by the Author of these TRACTS.

TO WHICH IS ANNEXED

An HYDROSTATICAL LETTER, dilucidating an Experiment about a way of weighing Water in Water.

NEW EXPERIMENTS,

Of the POSITIVE OR RELATIVE LEVITY of Bodies under Water,

Of the AIR'S SPRING on Bodies under Water,

About the DIFFERING PRESSURE of heavy SOLIDS and FLUIDS.

The PUBLISHER to the READER.

IT will, it is presumed, be altogether needless to preface any thing by way of commendation to the following Tracts; they will certainly commend themselves by their own worth to the intelligent and attentive reader, who might have seen them sooner, if the press had not detained them longer than was expected; since, to the publisher's knowledge, they were ready in the year 1671, except the hydrostatical discourse, and the explication of the author's experiment of weighing water in water, the former of which was finished in the beginning of this year 1672; though the latter could not be so till near the end of the same year, viz. the month of February, English stile, because the book of Mr. *George Sinclair*'s Hydrostaticks, in which it is excepted against, came not, I think, before that time to *London*; I am sure, not to the view of the honourable Author. Farewel.

NEW

NEW EXPERIMENTS

TOUCHING THE RELATION BETWIXT

FLAME AND AIR,

SENT IN A LETTER

To the Learned Publiſher of the PHILOSOPHICAL TRANSACTIONS.

SIR,

YOU may have obſerved, as well as I, that ſince the publiſhing of the experiments I ſent you touching reſpiration, divers of our learned men have ſpent both thoughts and diſcourſes in inquiring and diſputing, whether there reſides in the heart of animals ſuch a fine and kindled, but mild ſubſtance, as they call a vital flame, to whoſe preſervation, as to that of other flames, the air (eſpecially as it is taken in, and expelled again by reſpiration) is neceſſary. This, among other conſiderations, makes me think it ſeaſonable (though many avocations make it inconvenient) to complete the performance of the promiſe I made you, by adding to the experiments about reſpiration, which your commands have already obtained of me, thoſe ſcattered notes, that I have been able to pick up about the relation betwixt flame and air. And though, I confeſs, they are very much inferior in number to the trials about reſpiration; and, that in making them it was not ſo much my deſign to complete an entire and diſtinct tract, though but a ſmall one, of ſuch experiments, as to gratify my own curioſity in the examining of a paradox or two I had been writing about flame; yet the nobleneſs of the queſtion now under debate, and their pertinency to it, will poſſibly keep them, as few as they are, from being uſeleſs. And that alſo they may be the better kept from being unwelcome, I have choſen to make myſelf a relator of matters of fact, without engaging with either of the litigant parties in a controverſy, wherein I am the leſs tempted to be partial, becauſe I have not formerly declared my opinion about it, and at preſent, I ſee, on either ſide, perſons, for whom I have no ſmall reſpect and kindneſs.

AND now, Sir, that you may not expect in the following papers ſuch a number and variety of experiments, as I might perhaps be able to preſent you with, on ſome more tractable ſubject, I ſhall briefly mention to you ſome of the chief difficulties I met with in the making of theſe; which I do the rather, that if you and your ingenious friends have a mind to proſecute ſuch trials, you may not be ſurprized with the difficulties I have met with; but provide at leaſt againſt thoſe foreſeen ones, by which you will ſcarce fail to be encountered.

I shall then inform you, that the ensuing experiments were rendered uneasy and troublesome to me by this; that some of them could not be conveniently done at all seasons of the year, nor in any season in all weathers, but must be made not only in the day-time, but in sun-shine days. You will easy guess, that I speak of those experiments, that are to be made by the help of a burning-glass, casting the reflected or refracted beams of the sun upon the combustible matter placed in the exhausted receiver: for, by reason of the interposition of so thick a glass, whereby many of the incident beams of light are reflected, and others inconveniently refracted, there is ordinarily requisite a clear day, and a competent height of the sun above the horizon, and sometimes also a convenient time of the year, to bring such experiments, as we were speaking of, to a fair trial. Not to take notice, that in such attempts there usually intervene circumstantial difficulties, not so easy to be foreseen: and it not being summer, when I had occasion to make the following experiments, I could make but very few with the sun-beams; besides that there are divers others, which are not that way to be made so conveniently, if at all, as by the help of the fire.

But though the trials of this second sort had their conveniencies, in regard they might be made in any weather, and as well by night as day; yet they were not unattended with peculiar inconveniencies; some of which you will easily discern by the mention of them, that was necessary to be made in some of the relations themselves. And, besides more particular and emergent difficulties, there was this in general, that rendered these experiments troublesome; that, whether I made them in larger receivers, or in small, or in middle-sized ones, each of these cases had its inconveniencies: for very large receivers, besides that it was very toilsome and tedious to empty them of air, required so much time for the exhaustion, that too frequently, by that time the operator had done pumping, the included, or other heated body was grown too cold to perform the desired effect: and if the receiver were not considerably large, than the red-hot iron, or other included body, that was to burn the combustible matter, would much endanger the breaking of the over-heated glass, and not afford room enough for some phænomena to be fairly exhibited in; and, besides, create another difficulty, to which we found middle-sized receivers also obnoxious: for, several times, when the experiment required an intense heat within the receiver, then (especially if some casual obstacle hindered the quick exhaustion) the heat of the ignited iron, or some such other included body, would so melt or soften the cement that fastened the receiver to the engine, that, when the glass was brought to be well exhausted, and sometimes also before, the external air would, by its pressure and fluidity, squeeze or thrust in somewhere or other the yielding cement, and thereby cause in the instrument a leak, that would much incommodate us, if not reduce us to begin the experiment again, insomuch, that, for some trials, we were fain to provide a cement on purpose; the least fusible, that we used on other occasions, being yet found too fusible on these.

Nor were those I have already mentioned, the only difficulties and impediments I met with in making experiments about flame and air; but I shall not here trouble you with them in this place, where it may suffice for me to have mentioned those, that are of a more general nature, and are like the most frequently to occur.

But though I declined to name any other to you, than the foregoing difficulties in making the following experiments; yet I must not omit to take notice of one, that may occur to you about judging of them. For, in those trials that require to have an ignited iron or any such thing included in the receiver, it would usually happen, that so much heat would rarefy the air shut up in the mercurial gage, and consequently inable it to depress the mercury, that lies under it, far beneath the mark it would have staid

at,

at, upon the mere account of ſo much ambient air pumped out: this would happen, I ſay, before the heated receiver was well exhauſted; ſo that, if one be not aware of this, it will be obvious, by looking on the gage, to conclude the receiver to be well emptied, before it really is ſo. And therefore the ſafeſt way in theſe caſes is, to continue to pump (without truſting to the ordinary marks) till you ſee, that the mercury will be no further depreſſed in the ſealed leg of the gage; though otherwiſe, by concurring ſigns, one that is verſed in thoſe trials, may well enough judge, when he needs to pump no longer.

But perhaps you will here demand, whether, by our engine, we can competently withdraw the air out of a receiver; or whether, at leaſt, that may not be much better done by the help of quickſilver, after the manner of the Torricellian experiment, in regard that ponderous liquor frees the glaſs, it deſerts, from all the air at once, and exactly hinders the regreſs of it.

In anſwer whereunto, I hope you do not expect, that I ſhould contend for a favourabler judgment of the engine I employ, than the virtuoſi (as well foreign as Engliſh) have been pleaſed to paſs on it already: and therefore, to tell you freely my thoughts about the main part of the propoſed queſtion, I ſhall readily avow to you, that I think, there may be experiments (ſuch as ſome of thoſe, where the included body need be but ſmall, and where the being ſuddenly produced is chiefly deſired in the effect) wherein, by the help of the quickſilver, the exhauſtion of the air may be diſpatched with greater celerity, and conſequently make the effect be more conſpicuous, than, by our ordinary way of trying, it would be in our engine; ſince the fall of the mercury does, as the objection intimates, produce a vacuum (in our ſenſe of that word) very nimbly, whereby the expanſion of the air is preſently effected, and the aërial particles, harboured in the pores of any body placed in this deſerted cavity, will thereby have opportunity more ſuddenly to expand themſelves. But, on the other ſide, I might anſwer in general, that when I have particular occaſions to diſpatch the exhauſtion of the air, I can very much haſten it by barely leſſening, as I have ſeveral times done, the capacity of the receiver; inſomuch, that I have ſometimes employed ſo ſmall an one, that in half a minute, or much leſs, after it was fitted on, we could conſiderably exhauſt it, and thereby produce phænomena exceeding conſpicuous. And as to the experiments of this little tract in particular, it may be ſaid, that, not to mention the troubleſomeneſs, and other inconveniencies of needing to employ ſuch an unweildy weight of mercury, you will eaſily find, by the phænomena of divers of the enſuing trials, that moſt of them cannot be with any conveniency, and ſome of them not at all, made in the Torricellian tubes. As for the ground of the objection, that the air cannot be ſo well drawn out by our way, as by the ſubſiding of the mercury; though you may think that very clear, yet one that were very jealous of the reputation of the inſtrument I employ, may perhaps reaſonably enough queſtion it. For the vacuum that is produced in the Torricellian experiment, as it is made all at once, ſo it is made once for all; and therefore, if there were any aërial particles lurking in the mercury, as there will be pretty ſtore, if the quantity of that liquor be great enough to make a conſiderable vacuum, which if it be not, it will be too ſmall for very many of our trials; they will remain in the deſerted cavity at the top of the glaſs, and, by their expanſion there, much hinder the full operation of an ambient vacuum upon the bodies placed in it. Beſides that almoſt all ſuch bodies, if they be dry, will be ſo incongruous to mercury (which ſcarce ſticks to any conſiſtent bodies but metals) that probably there will be no ſmall number of aërial corpuſcles intercepted between the mercury and thoſe ſurfaces, to which it does not cloſely adhere;

which

which aëry corpuſcles, when the ſubſiding mercury deſerts them, will be left to encreaſe the number of thoſe, that, as we were ſaying, will emerge from the mercury; from which, as alſo from the pores of the included bodies, will perhaps ariſe divers new ones, from time to time, for a pretty while after. And in caſe the vacuum be made by a cylinder of two or three and thirty feet of water, as for ſome experiments that have been tried in *France* and *Italy*, hath been done, the emerſion of bubbles may laſt a long time, as may be gathered from ſome obſervations of mine, elſewhere related.

On the contrary, in our engine, though when the receivers are not very ſmall, they are more ſlowly emptied; yet in recompence, we may continue the pumping out of the air as long, and renew it in the ſame experiment as often as we think fit: ſo that, if we perceive, that after the firſt exhauſtion of the glaſs, there happen any aërial particles to extricate themſelves ſucceſſively out of the included body, we can, by reſuming the pump from time to time, whenever need requires, free the vacuum from theſe alſo; which, in ſome caſes, I have found to be longer and more copiouſly emitted by the included bodies, than any thing but jealous trials could have convinced me of. And to confirm what I have been ſaying by ſomething hiſtorical, I ſhall add, that though the excellent Florentine academians are thought to have proſecuted the experiments about the vacuum made with mercury the furtheſt of any, yet ſome eminent members of that illuſtrious ſociety were pleaſed to confeſs to me, that they never were able, by the help of mercury, to bring a glaſs bubble, ſealed up with air in it, to burſt of itſelf by the withdrawing of the external air; which yet I have often done with the engine I employ, and convinced them, that I could do ſo, by doing it in their preſence.

You will, perhaps, think it ſomewhat ſtrange, to find, that I ſet down ſome of the following narratives in ſuch a way, as does not expreſs me ſollicitous to aſcribe and vindicate to the air ſo abſolute and equal a neceſſity to the production and conſervation of all flames, as divers learned men have concluded from my former experiments. But I, that am content to be kind to the air, but not partial, ſhall not ſcruple to declare to you, that, as much as ſome may think me beholden to the air for any diſcoveries of itſelf, it may have vouchſafed me; yet, I think, a natural, as well as a civil hiſtorian, does, in his accounts of matters of fact, owe more to truth, than to gratitude itſelf. And though, wherever the air can challenge a clear, or, at leaſt, a probable intereſt in a phænomenon, I am not only diſpoſed, but glad to do it right; yet I would not eaſily aſſert to it a larger juriſdiction than I find nature to have aſſigned it; eſpecially ſince, without partiality, that, I preſume, may be ſhewn to be very large and conſiderable, and perhaps to reach to many things, wherewith men ſeem not to have yet taken notice, that it hath any thing to do at all.

What hath been hitherto ſaid, will not, I hope, ſeem impertinent or uſeleſs, whenever you ſhall fall upon the actual making of ſuch experiments as you are about to read. But I fear, that to add any thing more (which were not difficult for me to do to the preliminary part of this ſmall tract) would make it too diſproportionate to the hiſtorical; from which I ſhall therefore no longer detain you.

THE

THE FIRST TITLE.

Of the difficulty of producing FLAME without AIR.

EXPERIMENT I.

A way of kindling brimſtone in vacuo Boyliano, *unſucceſsfully tried.*

WE took a ſmall earthen melting pot, of an almoſt cylindrical figure, and well glazed (when it was firſt baked, by the heat; and into this we put a ſmall cylinder of iron, of about an inch in thickneſs, and half as much more in diameter, made red hot in the fire; and having haſtily pumped out the air, to prevent the breaking of the glaſs; when this veſſel ſeemed to be well emptied, we let down, by a turning key, a piece of paper, wherein was put a convenient quantity of flower of brimſtone, under which the iron had been carefully placed; ſo that being let down, it might fall upon the heated metal; which as ſoon as it came to do, that vehement heat did, as we expected, preſently deſtroy the contiguous paper: whence the included ſulphur fell immediately upon the iron, whoſe upper part was a little concave, that it might contain the flowers when melted. But all the heat of the iron, though it made the paper and ſulphur ſmoke, would not actually kindle either of them, that we could perceive.

EXPERIMENT II.

An ineffectual attempt to kindle ſulphur in our vacuum another way.

ANOTHER way I thought of to examine the inflammability of ſulphur without air; which, though it may prove ſomewhat hazardous to put it in practice, I reſolved to try, and did ſo after the following manner:

INTO a glaſs-bubble of a convenient ſize, and furniſhed with a neck fit for our purpoſe, we put a little flower of brimſtone (as likely to be more pure and inflammable than common ſulphur); and having exhauſted the glaſs, and ſecured it againſt the return of the air, we laid it upon burning coals, where it did not take fire, but riſe all to the oppoſite part of the glaſs, in the form of a fine powder; and that part being turned downward and laid on coals, the brimſtone, without kindling, roſe again in the form of an expanded ſubſtance, which (being removed from the fire) was, for the moſt part, tranſparent, not unlike a yellow varniſh.

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THOUGH theſe unſucceſsful attempts to kindle ſulphur in our exhauſted receivers were made more diſcouraging by ſome more, that were made another way; yet judging that laſt

last way to be rational enough, we persisted somewhat obstinately in our endeavours, and conjecturing, that there might be some unperceived difference between minerals, that do all of them pass, and are sold for common sulphur, I made trial, according to the way hereafter to be mentioned, with another parcel of brimstone, which differed not so much from the former, as to make it worth while to set down a description of it, that probably would not be useful.

But in this place, it may suffice to have given a general intimation of the possibility of the thing. The proof of it you will meet with under the third Title, when I come to tell you what use I endeavoured to make of our sulphureous flames.

EXPERIMENT III.

Shewing the efficacy of air in the production of flame, without any actually flaming or burning body.

Having hitherto examined by the presence of the air, what interest it has in kindling of flame; it will not be impertinent to add an experiment or two, that we tried to shew the same interest of the air, by the effects of its admission into our vacuum. For I thought it might reasonably be supposed, that if such dispositions were introduced into a body, as that there should not appear any thing wanting to turn it into flame but the presence of the air, an actual accension of that body might be produced by the admitted air, without the intervention of any actual flame, or fire, or even heated substance; the warrantableness of which supposition may be judged by the two following experiments.

When we had made the experiment, ere long to be related in its due place, (viz. Title II. Experiment the 2d) to examine the presumption we had, that even when the iron was not hot enough to keep the melted brimstone in such a heat, as was requisite to make it burn without air, or with very little, it would yet be hot enough to kindle the sulphur, if the air had access to it: to examine this, I say, we made two or three several trials, and found by them, that if some little while after the flame was extinguished, the receiver were removed, the sulphur would presently take fire again, and flame as vigorously as before. But I thought it might without absurdity be doubted, whether or no the agency of the air in the production of the flame might not be somewhat less, than these trials would persuade; because, that by taking off the receiver, the sulphur was not only exposed to fresh air, but also advantaged with a free scope for the avolation of those fumes, which in a close vessel might be presumed to have been unfriendly to the flame.

How far this doubt may, and how far it should, be admitted, we may be assisted to discern by the subjoined experiment, though made in great part for another purpose; which you will perceive by the beginning of the memorial I made of it, that runs thus;

EXPERIMENT IV.

A differing experiment to the same purpose with the former.

Having a mind to try, at how great a degree of rarefaction of the air, it was possible to make sulphur flame by the assistance of an adventitious heat, we caused such an experi-

experiment as the above-mentioned to be reiterated, and the pumping to be continued for ſome time after the flame of the melted flowers of brimſtone appeared to be quite extinguiſhed, and the receiver was judged by thoſe that managed the pump (and that upon probable ſigns) to be very well exhauſted. Then, without ſtirring the receiver, we let in at the ſtop-cock very warily a little air, upon which we could perceive, though not a conſtant flame, yet divers little flaſhes, as it were, which diſcloſed themſelves by their blue colour to be ſulphureous flames; and yet the air, that had ſufficed to rekindle the ſulphur, was ſo little, that two exſuctions more drew it out again, and quite deprived us of the mentioned flaſhes. And when a little air was cautiouſly let in again at the ſtop-cock, the like flaſhes began again to appear, which, upon two exſuctions more, did again quite vaniſh, though, upon the letting in a little freſh air the third time, they did once more re-appear.

Whether, and how far ſuch experiments as theſe may conduce to explicate what is related of fires ſuddenly appearing in long undiſcloſed vaults or caves to thoſe that firſt broke into them, I may perchance elſewhere conſider, but ſhall not here enquire, eſpecially being not yet fully ſatisfied of the truth of the matter of fact.

EXPERIMENT V.

About an endeavour to fire gunpowder in vacuo *with the ſun-beams.*

Whatever hath been hitherto delivered, will not, I preſume, make it unreaſonable to enquire, whether, what intereſt ſoever the air appears to have in the production of thoſe flames that are to laſt for ſome time, there may not eaſily be produced a momentary flame or flaſh, without any aſſiſtance from the air. Wherefore I employed ſome endeavours to diſcover, whether there were the ſame need of air to the going off of gunpowder, as to the inflammation of other bodies. And though my firſt attempt of this nature being unproſperous, it was concluded by the learned of the by-ſtanders, that I ſhould never be able to make a ſucceſsful one to kindle gunpowder in an exhauſted receiver; yet this did not hinder me from proſecuting a deſign, for whoſe feaſibility I conſidered, that it might be alledged *a priori* (as they uſe to ſpeak) that brimſtone, which is one of the ingredients of gunpowder, appears by ſeveral trials to be ſometimes capable of accenſion in our vacuum, and therefore probably may kindle the reſt. But how far the firing of powder, without the help of air, is poſſible, will be beſt judged by the experiments you will meet with under the third title; and how far it is more difficult to be kindled in our exhauſted receivers, than in the open air (which is an inquiry proper for this place) may be gueſſed by the ſubjoined trial; which, though it were made many years ſince (in the year 1660) before we had deviſed the mercurial gage, to examine how well the receiver was exhauſted, I ſhall yet afford it a room in this place, becauſe it was made in ſummer by the help of a burning-glaſs, which I could not employ to purpoſe in the winter ſeaſon, wherein the two following trials were made.

To give you then ſome account of that part of the experiment which concerns our preſent inquiry, I will ſubjoin a tranſcript of what I find regiſtered about it; which is to this purpoſe, and almoſt in theſe words: that, having conveniently placed three or four grains of gunpowder in the cavity of our receiver, and having carefully drawn out the air, we caſt the ſun-beams, united by a good burning-glaſs, upon the powder, and kept them there a pretty while to little purpoſe; till, at length, the powder, inſtead of taking fire, ſmoaking only, and melting like a metal, thoſe ſpectators that were of another opinion than I was yet convinced of, would have me leave off. The further event of ſuch trials more fully proſecuted you will find under the third title; all that will be pertinent to be here added being, that the newly recited experiment was not the ſingle one we made about that time, that diſcovered a great indiſpoſition even in gunpowder to be fired in our vacuum.

EXPERIMENT VI.

An attempt to fire gunpowder in vacuo, *by means of a hot iron.*

We took, by weight, what we judged a convenient quantity of gunpowder that was extraordinarily ſtrong and well made; and having in our receiver, that was capable of holding about ſixteen pounds of water, placed the formerly mentioned iron firſt heated red-hot, when the air appeared by the mercurial gage to have been diligently pumped out, we let down, by help of the turning-key, a ſmall piece of thin paper, wherein the powder had been put, till we ſaw it reached the plate, by whoſe heat we hoped the paper would be deſtroyed, and the powder made to go off. But though both the one and the other had been purpoſely well dried near the fire, before they were put into the receiver; the deſired exploſion of the powder did not enſue. Yet there appeared upon the iron plate a pretty broad blue flame, like that of brimſtone (whence it was judged the ſulphureous ingredient of the gun-powder, that was kindled) which laſted ſo very long, as we could not but wonder at it. But, at length, the powder not going off and the ſtill decaying heat of the iron forbidding us to wait any longer, we thought fit to take off the receiver, and found (as we expected) that the paper contiguous to the iron was, in part, deſtroyed by its heat; but moſt of the grains of the powder ſeemed not altered, and were found diſpoſed enough to be fired, notwithſtanding the conſumption of the brimſtone that had burned away.

APPENDIX.

To confirm the foregoing experiment, by ſhewing how great a diſpoſition to take fire there may be in gunpowder, that yet would not do ſo without air, I ſhall ſubjoin this obſervation.

Having reiterated the newly mentioned experiment after the like manner, and with the ſame receiver, and iron plate, as formerly, we did not find any exploſion to be made for ſo long a time, that thinking it in vain to wait any farther, we let in the air, which might perhaps, by the help of the remaining heat of the iron, procure the operation we at firſt deſired. The event was, that after nothing had enſued for a good while, and we ſcarce thought that ſuch a thing would happen; the powder ſuddenly went off with a great flaſh, and ſo ſhook the receiver that was yet ſtanding on the engine, as to endanger the throwing of it down. Which circumſtance I mention, to give you a caution that may prove uſeful, in caſe you try in cloſe veſſels experiments with gunpowder; ſince if they be not warily managed, they may ſometimes (as I have had occaſion to obſerve) prove dangerous enough; which will be the better diſcerned, if I add, that the powder that had this operation on a receiver (large enough to contain two gallons of liquor) was weighed before it was put in, and amounted but to one grain (though a greater

greater quantity might perhaps have been well enough ventured upon, if it had been but common gunpowder).

EXPERIMENT VII.

Reciting another way, whereby the firing of gunpowder in vacuo Boyliano *was attempted.*

To diverſify our ways of examining the indiſpoſedneſs of gunpowder to be fired in our vacuum, we thought fit to add to the foregoing trials that which followeth.

Into a pretty large and ſtrong glaſs-bubble we put a few ſmall corns of gunpowder, and having carefully exhauſted it, and ſecured it againſt the return of the air, we put it upon a pretty quantity of live coals ſuperficially covered with aſhes; by whoſe heat the ſulphureous ingredient of the powder was in part kindled, and burned blue for a pretty while, and with a flame conſiderably great (in proportion to the powder); upon whoſe ceaſing, the powder, which, when all was done, did not take fire, appeared to have ſent up, beſides the flame, a pretty deal of ſulphureous ſublimate, that ſtuck to the upper part of the glaſs, and being held againſt a candle we cauſed to be brought in (for the experiment had been purpoſely made in a dark place) it exhibited divers vivid colours like thoſe of the rainbow.

EXPERIMENT VIII.

About a trial made to fire gunpowder in our vacuum by the help of ſparks.

Though in the fourteenth of the long ſince publiſhed phyſico-mechanical experiments there is recited a trial made about kindling of gunpowder with a piſtol; yet I ſhall not forbear to ſubjoin the enſuing account, partly, becauſe the receiver we then employed, being about four times, if I miſ-remember not, as big as that we laſt made uſe of, it was very difficult to exhauſt the one ſo well as the other; and partly, becauſe we wanted ſome accommodations, with which we ſince furniſhed ourſelves, and (having not then deviſed the mercurial gage we employed in the making this laſt experiment) we could not then judge ſo well, as we ſince could, of the degrees to which the receiver was emptied. And, therefore, when in the relation of that fourteenth trial, there is mention made of one attempt that did ſucceed, among divers that did not; there is towards the cloſe an intimation given, that in ſpite of the great rarefaction that had been made in the air, there might yet be ſome little portion of it remaining in the receiver. I proceed then to the promiſed relation, which I find thus ſet down:

To proſecute the deſign of the foregoing experiment by a way ſomewhat differing from thoſe hitherto mentioned, we made, though not without difficulty, the enſuing trial; one of whoſe ſcopes you will find intimated at the cloſe of the relation.

We took a ſmall and very ſhort piſtol, and having well faſtened it with ſtrings to a great weight that was placed upon the iron-plate of our engine, we drew up the cock, and primed the pan with dry powder; then over both the weight and piſtol we whelmed a receiver capable of containing two gallons of liquor, and having carefully cemented it on, we cauſed the air to be diligently pumped out; having before put in a mercurial gage, to help us to diſcern when it was exhauſted. Laſtly, ordering the pump to be plied in the mean while, for fear fome air fhould fteal in, before the trial was completed, we did, by the motion of the turning-key, fhorten a ftring that was tied both to it and the trigger of the piftol, by which means we did as much as we could towards the firing of the powder in the pan; but though the pan were made to fly open, yet the powder did not go off; whereupon letting in the air, and cocking the piftol again, without taking it off the weight it was tied to before, we drew out a little air, to be fure that the receiver was clofely cemented on (which care we took in reference to another experiment) and then letting in the air at the top of the receiver, and ftopping it in with the turning-key, we did, by the help of that key, draw afide the trigger again; whereupon, though there had been no new powder put into the pan, nor any left in it, but only fome little that remained after the late trial, yet that readily took fire and flafhed in the pan; which made it the more probable, that, in the former trial, fparks of fire had been ftruck out by the collifion of the flint and fteel; which was the more credible, becaufe in another trial made the fame hour in the fame exhaufted receiver, two of the affiftants plainly faw a fpark or two fly out upon the falling of the cock, though I, that chanced to ftand in an inconvenient place, did not then perceive it. But afterwards, having caufed the experiment for my fuller fatisfaction to be repeated, I freed myfelf from need of trufting others eyes; fo that it appears, that notwithftanding the great indifpofition of gunpowder itfelf to be reduced into flame in our vacuum, yet even folid matter is not uncapable of being ignited there, if it be put into a motion fufficiently vehement.

If this experiment had not been fo very troublefome to make, I fhould have been invited to reiterate it, becaufe a not contemptible fcruple may be prevented, if the trial can be made to fucceed, in regard that the going off of the whole gunpowder, by the falling of a fpark or two only upon two or three of its grains, would argue, that the accenfion of the reft was made by the propagation of flame from the kindled grains to the reft; fo fmall a portion of ignited and fuddenly vanifhing matter, as is to be found in a fpark or two, being not likely to be able in fo very fhort a time to impart a vehement, or fo much as a fenfible heat, to the whole aggregate of grains, or at leaft a great part of them, as the focus of a burning-glafs, held long enough upon them to make them melt, may well be fuppofed to do.

EXPERIMENT IX.

Two ways of making aurum fulminans go off in our exhaufted receiver.

Because it is wont to be fuppofed, how juftly I here difpute not, that aurum fulminans, as the chymifts call it, is much of the nature of gunpowder, though by vaft odds ftronger than it; I thought it not unfit to make trial, whether it could be made to go off in our exhaufted receiver; and accordingly, about the time that the other experiment of firing gunpowder by the fun-beams was made, we alfo made trial of this; and that, as I remember, in the fame receiver, and with the fame burning-glafs. The event was, that, though the air had been pumped out, the concentrated beams of the fun made the aurum fulminans go off, and violently fcatter about the cavity of the receiver a yellowifh duft or powder, which other trials in the free air made us look upon as particles of the gold, that was the main ingredient of this odd compofition.

This experiment we reiterated a good while after in another place, and with other veffels, and yet with the like fuccefs.

But

But in regard thefe trials being made by the united fun-beams, it was unavoidable that our eyes would be before-hand affected with the vivid impreffions of fo glaring a light it feemed not fafe to determine, by the bare going off, or fhattering of the aurum fulminans, whether or no it afforded any flame or light upon its explofion; for, as we could not be fure of the affirmative, becaufe our eyes could not difcern any momentary flame or flafh; fo it feemed not fafe to conclude the negative; fince, though there had been fuch a flame, yet, if it had not been ftrong, it would not have been fenfible to our eyes, whilft pre-affected by a powerful light. Wherefore we refolved to make this trial in the night with an iron heated, but not candent (that its light might not eclipfe that, which the powder might afford); and having, after the manner already often recited, exhaufted a pretty large receiver, and let down by a ftring half a quarter of a grain (by weight) of good aurum fulminans of our own preparing, loofely tied in a little piece of thin paper (which paper, former trials to another purpofe kept us from fearing, that no hotter an iron than ours then was, would kindle) we found, as we expected, that after the powder had lain long enough upon the iron to be thoroughly heated, it went off all together, and, as the by-ftanders affirmed, with a flafh: but my face being accidentally turned to remove a light that I feared might difturb us, I could not fee the flafh myfelf, and therefore caufed the experiment to be made once more, to ground my narrative upon my own obfervation; which quickly affured me, that the luminous flafh produced upon the explofion was not only fenfible but confiderable.

THE SECOND TITLE.

Of the Difficulty of preferving FLAME without AIR.

SINCE it is generally, and, in moft cafes, juftly efteemed to be more eafy to preferve flame in a body that is already actually kindled, than to produce it there at firft; we thought fit to try, whether, at leaft, bodies already burning might not be kept in that ftate without the concurrence of air. And though in fome of our formerly publifhed phyfico-mechanical experiments, it happened that actual flame would fcarce laft a minute or two in our large pneumatical receiver; yet, becaufe it feemed not improbable, that mineral bodies once kindled might afford a vigorous and very durable flame, we thought fit to devife, and make the following trials; whence probably we might receive fome new informations about the diverfities, and fome other phænomena of flame, and the various degrees, wherein the air is neceffary or helpful to them.

EXPE-

EXPERIMENT I.

Reciting an attempt to preserve the flame of brimstone without air.

We put upon a thick metalline plate a convenient quantity of flowers of sulphur; and having kindled them in the air, we nimbly conveyed them into a receiver, and made haste to pump out some of the included air, partly for other reasons, and partly that the cavity of the receiver might be the sooner freed from smoke, which would, if plentiful, both injure the flame, and hinder our sight. As soon as the pump began to be plied, or presently after, the flame appeared to be sensibly decayed, and continued to be lessened at every exsuction of the air; and in effect it expired, before the air was quite drawn out. Nor did it, upon the early removal of the receiver, do any more than afford, for a very little while, somewhat more of smoke in the open air, than it appeared to do before.

The reiteration of this experiment presently after afforded us nothing new worth mentioning in this place.

EXPERIMENT II.

Relating a trial about the duration of the flame of sulphur in vacuo Boyliano.

To vary a little the foregoing experiment, and try to save some moments of time, which on these occasions is to be husbanded with the utmost care; having provided a cylinder of iron larger than the former, that it might by its bulk, being once heated, both contribute to the accension of the sulphur, and to the lasting of its flame, we made a trial, that I find registered to this effect:

We took a pretty big lump of brimstone, and tied it to the turning-key; and having got what else was necessary in a readiness, we caused the iron-plate to be hastily brought red-hot from the fire, and put upon a pedestal, that the flame might be the more conspicuous; and having nimbly cemented on the receiver, we speedily let down the suspended brimstone, till it rested upon the red-hot iron, by which being kindled, it sent up a great flame with copious fumes, which hindered us not from plying the pump, till we had, as we conjectured, emptied the receiver; which we could not do without withdrawing, together with the air, much sulphureous smoke, that was offensive enough both to the eyes and nostrils. But notwithstanding this pumping out of the air, though the flame did seem gradually to be somewhat impaired, yet it manifestly continued burning much longer, than by the short duration of other flames in our receivers, when diligence is used to withdraw the air from them, one could have expected. And especially one time (for the experiment was made more than once) the flame lasted, till the receiver was judged to be well exhausted; and some thought it did so survive the exhaustion, that it went not out so much for want of air, as fuel; the brimstone appearing, when we took off the receiver, either to have been consumed by the fire that fed on it, or to have casually run off from the iron, whose heat had kept it constantly melted.

In case you should have a mind to prosecute experiments of the nature of this and the precedent, it may not prove useless, if I intimate to you the following advertisements. 1. For the red-hot iron above-mentioned, we thought it not amiss to provide, instead of the melting-pot employed in the first experiment, a pedestal, if I may so call it, made of of a lump of dried tobacco-pipe clay, that the vehement heat of the iron might neither fill the receiver with the ſmoke of what it leaned on, nor injure the engine, if it ſhould reſt immediately upon that; and this pedeſtal ſhould be ſo placed, that the iron may be as far as you can from the ſides of the receiver, which elſe the exceſſive heat would endanger.

2. To the above-mentioned concave iron that was to receive the brimſtone, we did for ſome occaſions cauſe to be fitted a thick convex piece of iron, ſhaped almoſt like a flattiſh button; which was not to be uſed conſtantly, but upon occaſion, that, being laid red-hot over the melted brimſtone, it might increaſe the heat, and keep the flame from having ſo broad a ſuperficies, whereby it would conſume its fuel too faſt.

3. We ſometimes thought it expedient, for the clearer diſcerning of what ſhould happen in the receiver, to make the experiment by night, and remove the candles, when we were juſt about to pump, preſuming, that the flame would be conſpicuous enough by its own light; as indeed we found it to be, though its light were but dim, conſidering the greatneſs of the flame; whoſe colour, though it did not quite loſe its wonted blueiſhneſs, ſeemed yet to have received a great and ſomewhat odd alteration.

There is one great inconvenience, ſcarce avoidable in this experiment, *viz.* that the fumes aſcending very copiouſly do quickly much darken the receiver, and if the trial be long continued, line it with a kind of flower of brimſtone, which obſcures it much more, and therefore ought to be carefully wiped away, whenſoever the receiver is taken off; upon which account you will not, I preſume, wonder, if you ſhall find the phænomena of theſe experiments not always to be the very ſame with what you meet with in this paper; ſince, as it is very poſſible that we may not have been able to obſerve things ſo accurately by reaſon of the newly mentioned fumes and flowers; ſo it is not impoſſible that the difference, if there ſhall be any, of other men's obſervations from ours ſhould proceed from the ſame cauſe.

Before we paſs from this ſecond experiment, it will not be amiſs to take notice, that though the flames of brimſtone may be allowed to be ſomewhat more durable than the flames of vegetables are wont to be; yet it is not ſafe to conclude, that it was merely upon the account of their native vigour, that the flames above-mentioned laſted ſo long in our receiver.

For we ſeemed to obſerve that there was requiſite a very intenſe heat of the iron to make the ſulphur capable of flaming on it, when any conſiderable proportion of air was withdrawn. For which reaſon it ſeems expedient, according to what I lately intimated, that the iron that is to keep it melted, be of a good thickneſs, that it may the longer retain a competent heat; and we thought it contributed to the ſucceſsfulleſt trials we made, that in them we uſed, beſides the concave iron, the convex one mentioned in the ſecond note.

EXPERIMENT III.

Of the laſting of the flame of a metalline ſubſtance in the ſame vacuum.

Those ſulphurs, that chymiſts call metalline, being ſuppoſed by many to be of a much more fixed nature than common ſulphur, and it being indeed probable enough, that in them good ſtore of very minute particles are crowded together, I thought fit to try, whether a body, wherein a vulgar chymiſt would think the ſulphur of a metal to be the main ingredient, would afford in our vacuum a more vigorous or laſting flame, than

than that of common ſulphur. And, though I will not here trouble you with my particular ſcruples about the chymiſts doctrine concerning metalline ſulphurs, nor with the grounds, on which I deviſed the following inflammable ſolution of Mars (for I do not now give it a more determinate name) which ſome chymiſts will not perhaps diſlike; I ſhall here annex the enſuing tranſcript of the trial itſelf.

HAVING provided a ſaline ſpirit, which by an uncommon way of preparation was made exceeding ſharp and piercing, we put into a phial, capable of containing three or four ounces of water, a convenient quantity of filings of ſteel, which were not ſuch as are commonly ſold in ſhops to chymiſts and apothecaries (thoſe being uſually not free enough from ruſt) but ſuch as I had a while before cauſed to be purpoſely filed off from a piece of good ſteel. This metalline powder being moiſtened in the phial with a little of the menſtruum, was afterwards drenched with more; whereupon the mixture grew very hot, and belched up copious and ſtinking fumes; which, whether they conſiſted altogether of the volatile ſulphur of the Mars, or of metalline ſteams participating of a ſulphureous nature, and joined with the ſaline exhalations of the menſtruum, is not neceſſary to be here diſcuſſed. But whenceſoever this ſtinking ſmoke proceeded, ſo inflammable it was, that upon the approach of a lighted candle to it, it would readily enough take fire, and burn with a blueiſh, and ſomewhat greeniſh flame, at the mouth of the phial, for a good while together; and that, though with little light, yet with more ſtrength than one would eaſily ſuſpect.

THIS flaming phial, therefore, we conveyed into a receiver, which he, who uſed to manage the pump, affirmed, that about ſix exſuctions would exhauſt. And the receiver being well cemented on, upon the firſt ſuck the flame ſuddenly appeared four or five times as great as before; which I aſcribed to this, that upon the withdrawing of the air, and conſequently the weakening of its preſſure, great ſtore of bubbles were produced in the menſtruum, which breaking could not but ſupply the neck of the phial with ſtore of inflammable ſteams, which, as we thought, took not fire without ſome noiſe; upon the ſecond exſuction of the air, the flame blazed out as before, and ſo it likewiſe did upon the third exſuction, but after that it went out; nor could we rekindle any fire by haſtily removing the receiver; only we found, that there remained ſuch a diſpoſition in the ſmoke to inflammability, that holding a lighted candle to it, a flame was quickly rekindled.

EXPERIMENT IV.

Of the duration of the flame of ſpirit of wine impregnated with a metal in the exhauſted receiver.

BECAUSE it may, upon grounds not improbable, be thought, that well-dephlegmed ſpirit of wine, being a pure æthereal liquor, which does not, like combuſtible ſulphurs (whether vulgar or metalline) emit any viſible ſmoke to ſtifle the flame (into which it may, in the free air, be totally reſolved) if this ſpirituous, and thus qualified liquor could be duly aſſociated with a metalline body, the reſulting flame might be more than ordinarily vigorous and durable; I reſolved to make an experiment of this ſort, and having by a way, that I delivered in another paper [in a Paradox about the fuel of Flames] ſo united highly rectified ſpirit of wine with a prepared metal, that they would both afford a conſpicuouſly tincted flame; we put this mixture into a ſmall glaſs-lamp, made on purpoſe, and furniſhed with a very ſlender wick, which the mixture would not burn,

burn, whilſt there was liquor enough to imbibe it well; and putting this lighted lamp into a convenient place of a receiver, that was not ſmall, ſince it was able to contain about two gallons, or ſixteen pounds of water, we made haſte to cement on the glaſs to the engine, and yet found not in two or three ſeveral trials, that after the pump began to be moved, ſo little a quantity of tincted flame in that capacious glaſs laſted much, if at all, more than half a minute of an hour, eſtimated by a minute watch.

AND becauſe the receiver we then made uſe of ſeemed to me, by reaſon of its ſize, and ſome accommodations that belong to it, proper enough to be employed about other trials, concerning the relation between flame and air; I thought fit to try, with the ſame ſmall lamp and liquor, what other phænomena of that kind would be afforded by letting air in and out, according to the various exigencies of my particular aims.

BUT not having then, nor in ſome time after, the leiſure and opportunity of ſetting down things circumſtantially, I contented myſelf to take thoſe ſhort notes of the principal things, whereof I now ſubjoin the tranſcript.

WHEN the flame began to decay, the turning-key being now and then drawn almoſt out, the tincted flame laſted once a minute and a half, and another time longer.

THE turning-key being taken out in the beginning, the flame laſted two minutes or better.

A PIPE bedded in the cement at the bottom of the glaſs, and having at each end an open orifice almoſt of the bigneſs of that filled by the turning-key, which key was then removed from the top, the tincted ſpirit ſeemed to burn very conveniently, as if the flame would have burned very long, if we would have permitted it ſo to do.

THE orifice at the top being ſtopped with the turning-key, though the pipe were left open at the bottom, it plainly, in a ſhort time, ſeemed much to decay, and ready to expire; whereupon I cauſed one to blow conſtantly, yet but very gently, in at the pipe with a pair of bellows, and by this means, though we did not keep the flame vigorous, yet we kept it alive for above four minutes; and then obſerving it to be manifeſtly ſtronger than it was, when we began to refreſh it with the bellows, we ceaſed from blowing, and found, that though the glaſs pipe was ſtill left open, yet within about one minute the flame was quite extinguiſhed.

EXPERIMENT V.

Of the conſervation of flame under water.

THE better to examine the neceſſity of air to flame, I thought fit not only to make the ſeveral trials mentioned in this paper, whether it would live in a medium much thinner than air; but alſo to try, whether it would be able to continue in a medium many hundred times thicker than air, namely in water.

I DOUBTED not but many would think this both an eaſy and a needleſs inquiry, ſince eminent writers, both ancient and modern, tell us without ſcruple, that Naptha and Camphire will burn under water; but I had never the good fortune to be able to make them do ſo; and may be allowed to doubt, whether theſe writers, notwithſtanding their confidence, deliver what they affirm, upon experience not bare tradition. And though in celebrated authors I have met with divers receipts of making compoſitions, that will not only burn under water, but be kindled by it; yet I have found thoſe I had occaſion to conſider, to be ſo lamely, or ſo darkly (and ſome of them I fear ſo falſely) ſet down, that by the following compoſition, how ſlight ſoever it may ſeem, I have been able to do more, than with things they ſpeak very promiſingly of; ſince, though it will not be kindled by water, yet being once kindled, it will continue to burn under water.

And that there might be no ſuſpicion, that whilſt the mixture continued under water, it did only, as it were, vehemently ferment, or ſuffer a violent agitation of its parts without having them kindled, till in their aſcending they were actually fired by the contact of the air incumbent on the ſurface of the water; to obviate this ſuſpicion (I ſay) we were careful to try the experiment, not only in other veſſels, but in a large glaſs, the tranſparency of whoſe ſides, as well as that of the contained water, would permit us to ſee, for a while, the burning of our compoſition, which was ſometimes with a weight detained, and ſometimes with a forceps held, till it was conſumed, a good way under the ſurface of the water.

The way of making the experiment is this; we took of gunpowder three ounces, of well burned charcoal one drachm, of good ſulphur or flower of brimſtone a little leſs than half a drachm, of choice ſalt-petre near a drachm and a half; which ingredients being well reduced to powder, and diligently mingled without any liquor, either a large gooſe-quill, whoſe feathery part was cut off, or a piece of a tobacco-pipe, of two or three inches long, and well ſtopped at one end, had its cavity well filled with this mixture (inſtead of which, beaten gunpowder alone might ſerve, if it did not operate too violently, or waſte too ſoon) for the kindling whereof, the open orifice of the quill or pipe was carefully ſtopped with a convenient quantity of the ſame mixture, made up with as little chymical oil or water as would bring it to a fit conſiſtence. This wild-fire was kindled in the air, and the quill or pipe, together with a weight, to which it was tied to keep it from aſcending, was ſlowly let down to a convenient depth under water, where it would continue to burn, as appeared by the great ſmoke it emitted, and other ſigns, as it did in the air; becauſe the ſhape of the quill or pipe kept the dry mixture from being acceſſible to the water (that would have diſordered and ſpoiled it) at any other part than the upper orifice; and there the ſtream of kindled matter iſſued out with ſuch violence, as did inceſſantly beat off the neighbouring water, and kept it from entering into the cavity that contained the mixture, which therefore would continue burning till it was conſumed.

It is probable, that moſt men will conclude from this experiment, that air is not ſo abſolutely neceſſary to the duration of flame, as ſome other of our trials ſeem to argue; and that there ought to be a difference made between ordinary flames, and thoſe that burn with an extraordinary vehemency. But my deſign being, as I long ſince intimated, rather to relate trials than debate hypotheſes, I ſhall only add, that it may be pretended on the behalf of the opinion, that this experiment ſeems to diſprove, that, not to mention the air that may lurk in the pores of the water, or that which may be intercepted between the little grains of powder, whereof the mixture conſiſts, the ſaltpetre itſelf may be ſuppoſed to be of ſuch a texture, that, in its very formation, the corpuſcles that compoſe it may intercept ſtore of little aërial particles between the very minute ſolid ones which thoſe corpuſcles are made up of. And this inexiſtence of the air in nitre may be probably argued from the great windineſs of the flame that is produced upon the deflagration of nitre. According to this ſurmiſe, though our mixture burns under water, yet it does not burn without air, being ſupplied with enough to ſerve the turn by the numerous eruptions of the aërial particles of the diſſipated nitre itſelf.

On this occaſion I remember, that in another paper I relate, that for divers purpoſes, and among them to remove this ſuſpicion, I ſucceſsfully tried to reproduce nitre in *vacuo Boyliano*, that there might not be any air, or at leaſt any quantity worth heeding, intercepted between the convening particles, that by their coalitions made up the nitrous corpuſcles,

puſcles, which, in favour of the neceſſity of air to flame, may be pretended to be but ſo many little empty bubbles cloſe ſtopped, whoſe moiſter parts may, by the fire that kindles the nitre, be exceedingly rarefied, and in that eſtate emulate air, and violently burſt their little priſons, and throw about the fragments of them with force, and in numbers enough to make their aggregate appear ſuch a flame, as is wont to be made by unctuous and truly combuſtible bodies; and yet this rarefied ſubſtance that thus ſhatters the nitrous particles, may really be no true and laſting air, but only vehemently agitated vapours, which preſently, upon the ceſſation of the heat, return to liquor; as we ſee, that the vapours of an æolipile that iſſue out after the aërial particles have been expelled, though they make a great noiſe and a temporary wind near the hole they ſtream out at, and would perhaps, if that hole were cloſe ſtopped, break the æolipile; yet are not true and permanent air, but at a ſmall diſtance off the inſtrument return into water.

But though I could ſuggeſt other ſuſpicions and conjectures about the incluſion of air between the particles of ſaltpetre, yet I forbear to mention them in a writing deſigned to be chiefly hiſtorical.

EXPERIMENT VI.

Relating an odd phænomenon about the flame of a metal in our vacuum.

To the foregoing experiments made on purpoſe I ſhall add a phænomenon afforded us by chance, and yet not unworthy to accompany the reſt.

Whilst we were trying to kindle ſomething in our exhauſted receiver, it happened by ſome accident or other, that the combuſtible ſubſtance that was to be kindled, fell beſides the iron, whereby our intended trial was defeated. But whilſt we were conſidering what was to be done on this occaſion, and had not yet let in the air that had been pumped out, the lights alſo continuing yet removed; we were ſurprized to ſee ſomething burn, like a pale blueiſh flame, almoſt in the midſt of the cavity of the receiver, and at firſt ſuſpected it to be ſome illuſion of the eyes; but all the by-ſtanders perceiving it alike, and obſerving that it grew very broad, we looked at it with great attention, and found it to laſt much longer than I remember I have ſeen any flame do in an exhauſted receiver. I ſhould have ſuſpected it had proceeded from ſome brimſtone ſticking, without our heeding it, to ſome part of the iron, which we had formerly employed to kindle ſulphur in our receiver, had it not been, that, beſides other things, I remembered, that we had juſt before kept it red-hot in the fire, and conſequently muſt have burned away any little brimſtone, if there were any, that adhered to it; but though we much wondered, whence this our flame proceeded, I would not let any thing be done that might haſten its extinction; and at length, when it expired of itſelf, we let in the air, which had been till then kept out, and perceived upon the concave part of the iron (which we judged to be the place, where the flame had appeared) a piece of melted metal, which we concluded had been faſtened to the ſtring, that the fuel we deſigned to kindle had been tied to, in order to the letting it down to the more eaſily: and this made us conceive, that the ſtring happening to be burned by the exceſſive heat of the iron, the piece of metal fell into the cavity of it, and, by the ſame heat, the more combuſtible part, which the chymiſts call the ſulphur, was melted and kept on fire, and continued burning ſo long as we have related. The piece of metal was judged to be lead, but having not formerly obſerved ſuch diſpoſition in that metal to be inflamed, I conſidered it attentively, and perceived, that it was ſome fragment, that the operator had chanced to light on, of a mixture of lead and tin, that I had (a while before, for an experiment not at all belonging to our present subject) caused to be colliquated in a certain proportion. Upon whose account it seems, the mixture of the ingredients had acquired such a new texture, as, whether by making the bodies open one another, or by what other means soever, fitted the mass to afford us the phænomenon above recited. And though I made an unsuccessful trial with a mixture of lead and tin, to produce such a flame upon the heated iron in the open air; yet the newly related experiment may suffice to argue, that there may be flames of metalline sulphurs (as the chymists call them) that will be, at least, as easily produced without the concurrence of the air, as that of common sulphur, and continue to burn in our vacuum longer than it.

THE THIRD TITLE.

Of the strangely difficult Propagation of ACTUAL FLAME *in vacuo Boyliano.*

I HAVE more than once observed, that some bodies (whereof I make particular mention in another paper) though they will not be turned into flame by very intense heats, and those of very differing kinds, are yet very readily kindled by an actual flame. So that the propagation of flame to contiguous bodies, that, according to the hitherto observed, and unquestioned course of things, must thereby in a moment, as it were, be actually inflamed, seems to be not only very easy, but almost infallible; and yet, that this propagation is not easy, or is perhaps scarce possible to be performed without the assisting presence of the air, may be gathered from the next following experiments; at whose titles though you will probably be surprized, in regard that by the two first experiments of the first title of this tract, it will scarce be expected that sulphur should be kindled in our vacuum; yet I presume your wonder will cease, when I put you in mind, that I formerly took notice to you of my having sometimes met with such sulphur as would be kindled there; and it was whilst that well-disposed parcel of sulphur lasted, that I took the opportunity of making with the flame of it the trials to which I now proceed.

EXPERIMENT I.

An ineffectual attempt to make flame kindle spunk in an exhausted receiver.

HAVING placed the often-mentioned cylindrical plate of iron, first brought to be red-hot, in a receiver, capable of containing two gallons of water; and having also diligently pumped out the air, we kindled a little sulphur upon the heated plate, and then a piece a piece of dried ſpunk, tied to a ſtring, was, by the help of a turning-key, let down to the flame; and when the experiment was finiſhed, and the ſpunk was taken out, we found it in divers places, not manifeſtly altered ſo much as in colour; and in thoſe parts that had been moſt expoſed to the flame, it was turned to a ſubſtance very differing from aſhes, being black and brittle as tinder, and, like it, exceedingly diſpoſed to be kindled upon the touch of the fire.

EXPERIMENT II.

An unproſperous attempt to make flame kindle camphire without the help of air.

As a farther confirmation of the difficulty of propagating flame in our vacuum, we may annex the following trials.

INTO the lately mentioned receiver we conveyed the cylindrical plate of iron made uſe of in the former experiment; and when the air had been diligently pumped out, we did, by the help of the turning-key, let down upon the hot iron a piece of ſuch brimſtone, as would, in ſpite of ſo diſadvantageous a place, be kindled with that heat. A little above this ſulphur we had tied to the ſame ſtring a piece of camphire, that being a body exceedingly apt to take fire, if not, as it were to draw it, at the flame of lighted brimſtone. But our ſulphur melting with the heat of the iron cylinder, dropt unluckily from the ſtring it was faſtened to before, and for the moſt part fell off. And as ſoon as it came to the ground, where it was diſtant from the vehement heat of the metal, the flame expired, and that part of the ſulphur that happened to ſtick to the ſide of the iron, was inflamed by it. And I that chanced to be then in an inconvenient poſture for ſeeing the camphire, could not, becauſe of the ſmoke of the extinguiſhed brimſtone, well diſcern what became of it. But my amanuenſis that happened to be on the beſt ſide of the receiver, affirmed, he plainly ſaw the flame of the brimſtone reached the camphire, without being able to make it flame. Which ſeemed the leſs to be doubted of, becauſe the camphire was by help of the turning key let down low enough, and if it had afforded a flame, the difference of colours betwixt that and the blue flame of ſulphur would have made it very eaſy for me to have diſtinguiſhed them.

ANOTHER trial I would have thoroughly made to kindle one piece of ſulphur in our vacuum by the flame of another, tied a little lower in the ſame ſtring, that it might firſt touch the heated iron, and be thereby ſet on fire; but, though we could find nothing that was viſibly amiſs in the kind of ſulphur we then uſed, yet we were not able, even by a reiterated trial, to make it take fire upon the iron, where nevertheleſs it melted and ſeemed a little to boil.

A THIRD trial was not ſo unſucceſsful; for having in the well-exhauſted receiver let down upon the very hot iron a match made of a piece of card dipped in brimſtone, the lower extreme of it was kindled by the contact of the hot iron. But though the ſulphurated part of the match thus flamed away, yet the remaining part, which was a mere piece of card, was not thereby turned into flame, nor in moſt places ſo much as ſenſibly ſcorched or blacked; though, as I remember, the match had been purpoſely dried before-hand to facilitate its inflammation.

EXPE-

EXPERIMENT III.

A strange experiment upon gunpowder, shewing, that though it were fired itself, yet it would not fire the contiguous grains in vacuo Boyliano.

THE preceding trials may suffice to manifest the difficulty of communicating flame, without the help of air, from one body to another, even when the bodies to be kindled are of a very inflammable nature. But because there is no propagation of flame made in any bodies that we converse with here below, with any thing near such celerity, as in the contiguous grains of gunpowder; a great heap whereof will, almost in the twinkling of an eye, be turned into flame by propagation from any one small kindled grain; nothing seemed fitter to manifest how much flame is beholden to air, than if such an experiment could be made, as might shew, that, even amongst the contiguous grains of kindled gunpowder, flame would not be propagated without the help of air. How far a trial of this nature may be made in our engine, the following narratives will best declare.

WE took some paper, and laying it upon some convenient part of the plate of the engine, we made upon it a train of dry powder, as long as the glass would well cover; then, carefully fastening on the receiver with good cement, we solicitously pumped out the air; which done, we took a good burning-glass, and about noon cast the sun-beams through it upon the train of some gunpowder; where, though the indisposition to accension was so great, that the powder did not only smoke, but melt without going off, and the operator, though versed in such experiments, would not allow that it would signify any thing to continue the trial any longer; yet, upon my being obstinate to prosecute it, he, being willing to follow the experiment, rationally considered, that the receiver we had been hitherto fain to use, was so opacous as to resist the entrance of many of the beams that should have their operation upon the powder: whereupon taking a finer glass that was lately come in, we laid by the former, and employed that, which, by reason of its transparency, so little weakened the beams of the sun, that being, according to my direction, held obstinately upon the same parts of the train, they were able to fire several of them one after another. But though the sun could thus kindle the powder, yet it could not make the flame propagate, but only those parts that were melted, did at length kindle and fly away, leaving the rest unaltered, as I curiously observed, finding several little masses of colliquated matter in several places of the train, with the powder unchanged in all the other parts of the same train that lay in a direct line; besides that some of the little colliquated masses were contiguous to the rest of the powder, which appeared unchanged, and kindled readily, and flashed all away, as soon as I caused the burning-glass to be applied to it in the open air.

EXPERIMENT IV.

Reciting another attempt to confirm the former.

FOR further confirmation of so odd an experiment, I shall also add a short account of another made with gunpowder in our vacuum.

TO try, on an occasion that need not here be discoursed of, whether, by the help of one of those little instruments that are now used at *London*, to examine the strength of powder, we could find any difference made by the absence and presence of the air, in the

the resistance of the instrument, or the effects of the powder on it; we fastened it to a competently heavy and commodiously shaped weight of lead; and when it was carefully filled and primed with powder, we placed it in a receiver of a convenient bigness, whence we pumped out the air after the usual manner, and perhaps with more than usual diligence. But though, at length, after the powder had long resisted the beams of the sun, concentrated on it by a good double convex burning-glass, it did, as I expected, take fire at the touch-hole, and fill the receiver with smoke; yet this kindled powder could not propagate the flame to that which was in the box, how contiguous soever the two parcels were to one another. And when the instrument was taken out into the air (by which it appeared how free the touch-hole was) as soon as ever new-priming, with the same sort of powder, was put to it, the whole very readily went off; and when, for further satisfaction, we caused the instrument to be new charged, and upon its taking fire only at the touch-hole in the exhausted receiver, we ordered new priming to be added, without so much as taking the instrument out of the receiver, though afterwards the receiver was closed again, but without being exhausted of air; the powder, though closely shut up in the glass, did readily go off, as well that which was in the box or cavity of the powder-trier, as that which lay on the outward part of the instrument. And this trial, for the main, was repeated with the like success.

EXPERIMENT V.

Briefly mentioning two differing trials, with two differing events, to kindle gunpowder in our vacuum.

You will easily believe, that the event of the foregoing trials seemed strange enough to the ingenious persons that I had desired to be present at them; and perhaps, the attentive consideration of it may well enough suggest such odd suspicions and conjectures, as I have neither the leisure, nor the boldness to discourse of in this place.

But here I shall not dissemble my having, by a somewhat differing way, made a couple of trials, whereof, though the first may confirm the great indisposition of gunpowder to be kindled in our vacuum, yet the second seems to look another way.

The first is summarily set down in my notes to this purpose. [A few small corns of gunpowder being included in a very small bubble freed from its air, and secured against the return of it, or any other, and then applied warily to coals covered with ashes, did not go off, nor burn, but afforded a little yellow powder that seemed to be sulphur, and sublimed to the upper part of the glass.]

The latter event I found in the same paper to have been thus registered. [But two larger bubbles, though strong, whereof one had the air but in part, and the other carefully emptied, being provided, each of them with a greater quantity of powder (though scarce enough to promise such an effect) a while after they were put upon quick coals, each of them was blown in pieces, with a report almost like that of a musket; but though this was done in a dark place, yet we did not perceive, whether or no there were any real flame produced.]

The event of this trial seems at first sight to contradict the inference, that probably you have drawn from the foregoing experiments. But yet it may not be unworthy of our inquiry, whether this way of trial be as proper to give satisfaction to the curious, as that made with the sun-beams was. And I leave it to be considered, whether or no it may not be doubted, whether the going off of the gunpowder was caused by a successive, five, though extremely swift propagation of real flame, from the first kindled grains to the rest; or did not proceed from this, that the coals acting strongly at the same time on the whole area, or extent of the powder that was next to them, and this in the absence of the air, each grain was in that case, as it were, a little granado, and the heap of them, being uniformly enough acted on by the fire, they were made to go off, as to sense, all at once, as if there had been but a contemporary explosion made of them all together by the action of the external fire, rather than any true accension made by the flaming grains of the unkindled ones. As, I remember, I have tried, that even in the open air one may, with a burning-glass dexterously employed, make some part of a little parcel of aurum fulminans go off, whilst the neighbouring parts of the same parcel to which the focus does not extend with heat enough, will not be made to do so.

NEW EXPERIMENTS

About the RELATION betwixt AIR and the FLAMMA VITALIS of Animals.

(Sent to the same Person to whom the former Papers were addressed.)

THE twenty experiments hitherto set down under the three foregoing titles, by shewing the relation betwixt air and flame in general, may be serviceable to the inquirers into the nature of the vital flame in particular. But yet having had occasion to make some trials that more directly regard the requisiteness of air to the flamma vitalis or vital principle of animals; I shall now present you by themselves, as many as I could light on, without being solicitous that they should be quite differing from each other; because in so new and nice a subject, the affinity that may be found between some, either in regard of the subjects exposed to trial, or in the manner of making it, may be useful, if not necessary, to confirm things by the resemblance of events, or make us proceed cautiously and distinctly in pronouncing upon cases where the success was not uniform.

EXPE-

EXPERIMENT I.

Wherein the durations of the life of an animal, and of the flame of ſpirit of wine, included together in a cloſe veſſel, were compared.

We took ſome highly rectified ſpirit of wine, and put about a ſpoonful of it into a ſmall glaſs-lamp, conveniently ſhaped and purpoſely blown with a very ſmall orifice, at which we put in a little cotton-wick, which was but very ſlend.r.

We alſo provided a tall glaſs-receiver, which was in length eighteen inches, and contained above twenty pints of water. This receiver, which was open at both ends, was at the upper orifice (which was not wide) covered with a braſs-plate, faſtened on very cloſe with good cement, for uſes whoſe mention belongeth not to this place; and for the lower orifice, which was far the wideſt, we had provided a braſs-plate furniſhed with a competent quantity of the cement we employed to keep the air out of the pneumatical engine; by means of which plate and cement we could ſufficiently cloſe the lower orifice (though a wide one) of our receiver, and hinder the air from getting in at it.

These things being thus prepared, we took the ſmall glaſs-lamp above-mentioned, and having lighted it, we placed both it, and a ſmall bird (which was a green-finch) upon the braſs-plate, and in a trice faſtened it to the lower orifice of the receiver, and then watched the event; which was, that within two minutes (as near as we could eſtimate by a good minute-watch) the flame, after having ſeveral times almoſt quite diſappeared, was utterly extinguiſhed; but the bird, though for a while he ſeemed to cloſe his eyes, as though he were ſick, appeared lively enough at the end of the third minute; at which time, being unwilling to wait any longer by reaſon of ſome avocations, I cauſed him to be taken out.

After he had for a pretty while, by being kept in the free air, recovered and refreſhed himſelf, the former trial was repeated again, and at the end of the ſecond minute, the flame of the lamp went out; but the bird ſeemed not to be endangered by being kept there a while longer.

After this we put in, together with the ſame bird, two lighted lamps at once, viz. the former and another like it, whoſe flames, according to expectation, laſted not one whole minute, before they went out together. But the bird appeared not to have been harmed, after having been kept five or ſix times as long before we took off the receiver.

In the tall receiver above-mentioned we included a mouſe, with a lighted lamp filled with the ſpirit of wine; but before the experiment was near finiſhed, the mouſe, being at liberty within the glaſs, made a ſhift to blow out the flame; which being revived without taking out either the lamp or the animal, the ſpirit of wine burned about a minute longer, during which time the mouſe appeared not to be grown ſick, no more than it did afterwards, when, for ſome minutes after the extinction of the flame, he had been kept in the ſame cloſe and infected air.

Afterwards we placed the ſame mouſe in another receiver, which ſeemed to be by a third part leſs capacious than the former, and in it we alſo fixed a piece of ſlender wax-candle, ſuch as is wont to be made up in rolls, and employed to light tobacco. This candle continued burning in this new receiver but for one minute, during which time it emitted ſtore of ſmoke; but this not hindering the animal to appear lively enough, even after we had kept him much longer in that infected air, the ſame candle, without being taken out, was lighted again, but burned not ſo long as before; yet it ſufficed to darken the receiver, and therefore probably much to clog the included air, in which, nevertheleſs, the mouſe being kept, by our gueſs, eight or ten minutes longer, he appeared, neither when he was taken out, nor a while before, to have received any conſiderable harm by his detention there.

EXPERIMENT II.

Of the duration of the life of a bird, compared with the laſting of a burning candle and coal in our vacuum.

We took a green-finſh and a piece of candle of twelve to the pound, and included them in a great capped receiver, capable of containing about two gallons, or ſixteen pounds of water, which was very carefully cemented on to the pump, that no air might get in or out. In this glaſs we ſuffered the candle to burn till the flame expired (which it did in more than one trial, within two minutes or ſomewhat leſs); at which time the bird ſeemed to be in no danger of ſudden death; and, though kept a while longer in that clogged and ſmoky air, appeared to be well enough, when the receiver was removed. Afterwards, we put the ſame bird into the receiver with a piece of a ſmall wax taper, whoſe flame, though it laſted longer than the other, yet the bird outlived it; and it was judged he would have done ſo, though the flame had been much more durable. After this, we included the ſame bird with the firſt mentioned candle in the receiver, which we had cauſed to be often blown into with a pair of bellows, to drive out the ſmoke and infected air; and then beginnning to pump out the air, we found, that the flame began more quickly to decay, and the bird to be much more diſcompoſed, than in the former experiments; but ſtill the animal outlived the flame, though not without convulſive motions. The experiment we repeated with a piece of the fore-mentioned taper, and the ſame bird; which, though caſt into threatening ſymptoms upon the gradual withdrawing of the air, outlived, not only the flame, but the ſmoke too, that iſſued from the kindled wick, which circumſtance was alſo obſerved in the preceding trial.

Lastly, having freed the receiver from ſmoke, and ſupplied it with freſh air, we put in with the ſame bird a piece of charcoal of about two inches in length, and half an inch in breadth, which had been, juſt before it was put in, well blown with a pair of bellows, that it might be freed from aſhes, and thoroughly kindled; and made haſte to pump out the air. This diligence was continued not only till none of the fire could be diſcerned by any of the by-ſtanders, but till, in our eſtimation (which the event juſtified) it was irrecoverable by the admiſſion of the outward air; which at its coming in found the bird very ſick indeed, but yet capable of a very quick recovery. And this experiment was, with the ſame animal and coal re-kindled, tried over again with the ſame ſucceſs.

Whether this ſurvival of animals, not only to a flame, that emits ſtore of fuliginous ſteams, as in this trial, but to that, which is made of ſo pure a fuel as ſpirit of wine, that affords not ſuch ſteams, as in the former experiment; whether, I ſay, this ſurvival proceed from this, that the common flame and the vital flame are maintained by diſtinct ſubſtances or parts of the air; or, that common flame making a great waſte of the aërial ſubſtance they both need to keep them alive, cannot ſo eaſily as the other find matter to prey upon, and ſo expires, whilſt there yet remains enough to keep alive the more temperate

temperate vital flame; or, that both theſe cauſes, and perhaps ſome other, concur to the phænomenon, I leave to be conſidered.

EXPERIMENT III.

Of what happened to the light of glow-worms in the exhauſted receiver.

For the ſake of thoſe learned men, that have thought the light of glow-worms and other ſhining inſects to be a kind of effulſion of the biolychnium, or vital flame, that nature had made more luminous in theſe little animals than in others; and which a very eminent phyſician of the college of *London* affirms to have felt in a warm climate more than ſenſibly hot; I ſhall ſubjoin on this occaſion ſome trials made on glow-worms, which elſe ſhould be referred to thoſe experiments of mine about the relation betwixt air and light, that you were formerly pleaſed to publiſh.

We took two glow-worms, that ſhone vividly enough, eſpecially one of them, whoſe light appeared ſtrong and tincted, as if it had been tranſmitted through a blue glaſs: theſe we laid upon a little plate, which we included in a ſmall receiver of finer glaſs than ordinary, that we might the better ſee what would happen: and having for the ſame purpoſe removed the candles, that no other light might obſcure that of the inſects, we waited in the dark, till that was conſpicuous, and then ordered the air to be begun to be pumped out; and, as we expected, upon the very firſt exſuction there began to be a very manifeſt diminution of the light, which grew dimmer and dimmer, as the air was more and more withdrawn, till at length it quite diſappeared, though there were young eyes among the aſſiſtants. This darkneſs having been ſuffered to continue a long while in the receiver, we let in the air again, whoſe preſence, as we looked for, reſtored at leaſt as much light as its abſence had deprived us of. This experiment was repeated with one more of thoſe inſects; and the event was, that they all three gradually loſt their light by the exhauſtion of the receiver, and regained it, with ſome increaſe (as was judged) by the return of the air. And in this experiment we let in the air by degrees, and with an interval or two, to obſerve, as we did, that, as the diminution of light was greater and greater, when the air was more and more withdrawn, ſo the returning ſplendor was gradually increaſed, as we pleaſed to let in more and more air upon the worms.

EXPERIMENT IV.

Containing a variation and improvement of the foregoing trial.

But here I foreſaw, it might be ſuſpected, that the diſappearing of the light in our exhauſted receiver did not ſo much proceed from any real, though but temporary, extinction or eclipſe of it, as from this, that the glow-worms having, as I have often obſerved, a power of drawing the luminous part into the opacous part of their body, they might, finding themſelves prejudiced by the withdrawing of the air, hide their light from our eyes, without loſing it, till being again refreſhed by the return of the air, they might be invited to protrude it again into the tranſparent part of their tails. This ſcruple ſeeming grounded upon the nature of the thing, I thought it worth while to remove it by the help of another obſervation, that I long ſince made, and have mentioned elsewhere about glow-worms; which is this, that if they be killed whilst they are shining, their luminous matter may continue to shine for a good while after it is taken out of their bodies; and accordingly having put some of that we took out of the forementioned insects, upon a little paper, and included it in the receiver we employed, the candles being removed, we perceived it to shine vividly enough before the pump was set on work, and afterwards to grow dimmer and dimmer, as the air was more and more drawn out, till at length it quite vanished; and it re-appeared immediately upon the air's return. This experiment was reiterated twice more with the same success for the main. But we took notice, that the luminous matter, after the air was let in, seemed to us not only to have regained its former degree of light, but sensibly encreased it (as it once happened also in the experiment made on the living worms) which, whether it was caused by any real change made by the recess and access of the air in the matter itself, or by the greater accustomance of our eyes to the darkness of the place, I dispute not; and shall only add this phænomenon of one of our trials, that having a mind to see, whether a very little proportion of returning air would not suffice to restore some little light to the disappearing matter, it was somewhat strange to observe, that so very small a quantity of air, as was let in before the light was revived, was enough to make it become plainly visible, though but dim; in which state it continued, till we thought fit to let in more air upon it. Farther trials I could not make with these glow-worms, having received them but that night out of the country, and being the next morning to begin a journey.

EXPERIMENT V.

Wherein the former enquiry is farther prosecuted.

After the lately mentioned trials we made with the glow-worms, having procured two or three other of those insects, whereof one was judged to be as large as three ordinary ones, we found when we had brought them out of the country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a considerable degree of luminousness in the under part of his tail, we put him into the small receiver formerly mentioned, to try whether after the death of the animal, the shining matter would retain its former properties; but at the first time the air was pumped out after the usual manner, the light was not only not abolished, but continued vivid enough, and so it did when the air being let in, and again withdrawn, the trial was made a second time. But being unwilling to abandon the experiment till we tried it yet a little further, I caused the receiver to be exhausted yet once or twice more, and at length I perceived, that the light began to diminish, as the air was withdrawn; and last of all, it so disappeared, that the by-standers could not see it, whereas upon the re-admission of the air, the light shone vividly as before, if not more bright. This experiment was reiterated with the like success, and in both these times the like happened to the light of the dead one, and of a living one that we included with it, to be able to compare them together; though there were this disparity betwixt them, that the luminous part of the dead worm was much larger than that of the living, and the light of the latter appeared of a very greenish blue, whereas that of the former seemed to be of a white yellow.

EXPE-

EXPERIMENT VI.

Made to examine whether animals be heavier dead than alive.

IT is a received tradition, that bodies when dead are much heavier than the fame were when alive: the matter of fact being taken for granted, fome will perhaps afcribe the change to the utter inability of a dead body any way to affift thofe that endeavour to remove it. But, according to the general opinion, this difference proceeds from the total extinction or recefs of the fpirits vital and animal, which being fuppofed to be not only agil but light, leffened the weight of the body they enlivened; and flame being conceived to be the lighteft among bodies here below, it is not improbable, that fome will afcribe the phænomenon to the levity of the flame, which by being diffufed through the body of an animal, and vivifying it, deferves the name of vital. But I would not advife any to rely on this conceit, till they are duly fatisfied of the truth of the matter of fact; which becaufe I have not yet found, that any has endeavoured to try, I fhall on this occafion give you the following tranfcript of one of my notes about ftatical experiments.

A MOUSE, weighing about three drachms and a half, being put in one of the fcales of a very nice balance, was counterpoifed together with a ftring, that was tied about his neck like a noofe, and after a while, by drawing the ends of it, was there ftrangled. As foon as we judged him quite dead, we weighed him again, and though nothing was feen to fall from him; yet, contrary to the received tradition, that bodies are much heavier dead than alive, we found the weight to have loft about $\frac{7}{16}$ of a grain; which probably proceeded from the avolation of divers fubtile particles upon his violent and convulfive ftrugglings with death. But this was no more than an experiment of this kind, made fome years ago, induced me to expect and foretel.

AFTERWARDS in a larger balance, but a very good one, purpofely made for nice experiments, we took a very young catlin, of between ten and eleven ounces in weight, and caufed him to be ftrangled on the fame fcale, wherein he had been put. But he could not be difpatched fo foon as an ordinary full grown animal; fo that by that time he was quite dead, we found him not only not to be grown heavier, but lighter by four grains; which did not much furprize us, having elfewhere noted the life of fo very young creatures of that kind not to be eafily deftroyed for want of refpiration. And I remember, that, for trial's fake, another catlin of the fame litter with this I have mentioned, being included in a receiver, wherein another animal of that fize might probably have been difpatched in two or three minutes by the pumping out of air, was kept there fomewhat above a quarter of an hour before he appeared to be quite dead.

tioned elfewhere about glow-worms; which is this, that if they be killed whilft they are fhining, their luminous matter may continue to fhine for a good while after it is taken out of their bodies; and accordingly having put fome of that we took out of the forementioned infects, upon a little paper, and included it in the receiver we employed, the candles being removed, we perceived it to fhine vividly enough before the pump was fet on work, and afterwards to grow dimmer and dimmer, as the air was more and more drawn out, till at length it quite vanifhed; and it re-appeared immediately upon the air's return. This experiment was reiterated twice more with the fame fuccefs for the main. But we took notice, that the luminous matter, after the air was let in, feemed to us not only to have regained its former degree of light, but fenfibly encreafed it (as it once happened alfo in the experiment made on the living worms) which, whether it was caufed by any real change made by the recefs and accefs of the air in the matter itfelf, or by the greater accuftomance of our eyes to the darknefs of the place, I difpute not; and fhall only add this phænomenon of one of our trials, that having a mind to fee, whether a very little proportion of returning air would not fuffice to reftore fome little light to the difappearing matter, it was fomewhat ftrange to obferve, that fo very fmall a quantity of air, as was let in before the light was revived, was enough to make it become plainly vifible, though but dim; in which ftate it continued, till we thought fit to let in more air upon it. Farther trials I could not make with thefe glow-worms, having received them but that night out of the country, and being the next morning to begin a journey.

EXPERIMENT V.

Wherein the former enquiry is farther profecuted.

After the lately mentioned trials we made with the glow-worms, having procured two or three other of thofe infects, whereof one was judged to be as large as three ordinary ones, we found when we had brought them out of the country to *London*, that this great worm was dead, as far as we were able to judge, and finding him to retain a confiderable degree of luminoufnefs in the under part of his tail, we put him into the fmall receiver formerly mentioned, to try whether after the death of the animal, the fhining matter would retain its former properties; but at the firft time the air was pumped out after the ufual manner, the light was not only not abolifhed, but continued vivid enough, and fo it did when the air being let in, and again withdrawn, the trial was made a fecond time. But being unwilling to abandon the experiment till we tried it yet a little further, I caufed the receiver to be exhaufted yet once or twice more, and at length I perceived, that the light began to diminifh, as the air was withdrawn; and laft of all, it fo difappeared, that the by-ftanders could not fee it, whereas upon the re-admiffion of the air, the light fhone vividly as before, if not more bright. This experiment was reiterated with the like fuccefs, and in both thefe times the like happened to the light of the dead one, and of a living one that we included with it, to be able to compare them together; though there were this difparity betwixt them, that the luminous part of the dead worm was much larger than that of the living, and the light of the latter appeared of a very greenifh blue, whereas that of the former feemed to be of a white yellow.

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ADVERTISE-

ADVERTISEMENT.

These two following attempts falling into the hands of the author after the preceding experiments were printed, it was thought fit to annex them here for the affinity of the ſubject.

An ATTEMPT to produce Living Creatures in *Vacuo Boyliano*.

In reference to the opinion of thoſe naturaliſts, that hold the ſeeds of living creatures to be animated, and eſpecially to the hypotheſis of thoſe learned men that aſſert the flamma vitalis lately mentioned; it may be an enquiry of moment, whether or no in the ſeminal principles, or rudiments of animals, the manifeſt operations of life may be excited without the concurrence of the air, whoſe intereſt in the production and conſervation of flame may be gathered from the foregoing experiments. For, it ſeems likely to prove no inconſiderable diſcovery in reference to the lately mentioned hypotheſis, if it be found, that the principle of life in ſeminal rudiments needs, as well as other flames, the concurrence of the air to actuate it.

I thought fit, therefore, notwithſtanding the great and almoſt inſuperable difficulties, which it was eaſy enough for me to foreſee I ſhould meet with, to attempt the hatching of eggs in our vacuum: but though I made ſome unſucceſsful trials of this kind, in order to a diſcovery about reſpiration (not here to ſpeak of the attempts I made about the animation of putrid matter) yet leaving the mention of them to its proper place, I ſhall only take notice in this, what directly concerns the preſent inquiry. Conſidering then, that pregnant females cannot be made to live and bring forth young in our exhauſted receiver, and that the eggs of birds, and ſuch greater animals, do, in this colder climate of ours, require to be hatched by the incubation of the females, or other birds; I thought the fitteſt ſubjects I could both make choice of, and procure for the deſigned experiments, would be the eggs of ſilk-worms. For, having many years ſince tried ſeveral things about thoſe inſects, and among others found, that their eggs would be hatched, not only by the heat of one's body (though that be the uſual way) but by the warmth of the ſun even here in *England*, if they be kept till the ſpring be far enough advanced: remembering this, I ſay, I got a good number of ſilk-worms eggs; and having cauſed three conveniently ſhaped, but very ſmall receivers, to be purpoſely made, that differed very little (and that accidentally) either in ſize or figure, we conveyed into each of them, together with a ſmall ſtock of mulberry-leaves, ſuch a number of eggs, as we thought ſufficient to make one morally ſecure, that at leaſt ſome of them were prolifick: this done, we carefully exhauſted one of them, and ſecured it againſt the return of the air; the two others we left full of air: but having left in one a little hole for the air to come in and get out at, we ſtopped the other ſo cloſe, as to hinder all intercourſe between the included air and the external. All things being thus prepared, we expoſed the receivers to a ſouth-window, where they might lie quiet, and where I either came, or ſent to look on them from time to time; the ſpring being then ſo far advanced, that I ſuppoſed the heat of the ſun would be of itſelf ſufficient to hatch them in no long time.

As to the ſucceſs of this trial, my not being able to find any regiſter of the particular phænomena that occurred, keeps me from venturing to relate it very cicumſtantially; but this I remember in general, that both I and others took notice, that in the unexhauſted receivers there were divers eggs hatched into little inſects, that perforated their ſhells, and crept out of them; though afterwards, for want of change of food, or air, or both, few or none of them proved long-lived. But though the eggs in theſe receivers began to afford us little animals in a few days; yet the eggs in the exhauſted receiver did not, in many more, afford us any. And though I will not venture to ſay, how long preciſely we kept them in the ſame window, after ſome of the above-mentioned eggs were hatched; yet (if I much miſtake not) it was, from firſt to laſt, about three or four times as long; and I remember, we kept them till it was thought to no purpoſe to wait any longer, and agreed in imputing the not hatching of the eggs by the ſo long continued action of the ſun to the abſence of the air.

What other phænomena occurred to us in making this experiment, and another not unproſperous one upon the eggs of flies, you may expect, when I can light on my notes about them, or have my memory refreſhed by thoſe that aſſiſted at the making of them.

An ATTEMPT made upon GNATS in our Vacuum.

I Elsewhere mention, that it has been obſerved by a couple of our virtuoſi (whom I there name) and ſeveral times by me, that here in *England* multitudes of gnats are generated of little animals, that live, for a part of the ſummer, like fiſhes in the water; and conſidering, that by theſe a very unuſual paſſage is made from ſwimming to flying animals, I thought them very fit ſubjects, whereon to make the following experiment.

[Partly to try, whether at leaſt an animal already living and moving in our vacuum may be able to attain the perfection due to it, according to the courſe of nature; and partly to examine, whether, in caſe he ſhould attain it, at leaſt the lighter ſort of winged inſects, may be able to fly in that place; and partly to diſcover, whether an animal, that had long lived in our vacuum, would, when turned to a fly, be able to continue alive without reſpiration, he had never been accuſtomed to, in its priſtine form or ſtate; we took divers of thoſe little ſwimming creatures, which in autumn, eſpecially towards the end of it, are wont to be turned into gnats, and having put a convenient number of them together in a fit quantity of rain-water, wherein they had been found and kept, into a ſmall receiver, the air was pumped out, and the veſſel ſecured againſt its return, and then ſet aſide in a place, where I could obſerve, that the day after ſome of theſe little animals were yet alive and ſwimming to and fro, not without minute bubbles adhering to them; but at the end of a day or two after that, I could not perceive any of them to ſurvive their dead companions, nor did any of them recover, when freſh

fresh air was let in upon them. But though this experiment were the best I was then able to make, yet I resolved, if God should vouchsafe me life and health, to repeat it the ensuing autumn; that, wherein it was made, proving so cold and unseasonable, that a number of these little creatures, put up with water into another small receiver, died all within a few days, though none of the air was exhausted; and several, that I kept in an ordinary glass, that was divers times unstopped to give them fresh air, did yet perish at no ordinary rate. And I confess (as unkind as this trouble of mine may seem to the air) that the failing of this and some other experiments of producing animals in our exhausted receivers was the more unwelcome to me, because I had, and have still a great desire to see, if it be possible, what would happen to animals, which had been produced in a place free from the pressure of the atmosphere, as if they had been born in *Epicurus*'s imaginary intermundane spaces, upon their coming to be suddenly surrounded with our heavy air, and having their tenderly framed bodies exposed to its immediate pressure.]

NEW EXPERIMENTS ABOUT EXPLOSIONS.

(Annexed, by way of APPENDIX, to the former PAPERS.)

FORASMUCH as some of the learned men, that are the grand assertors of the flamma vitalis (whose opinion occasioned my presenting you the foregoing experiments) do also, with the justly famous Dr. *Willis*, explicate many of the motions of animals, especially those performed in the muscles, by the explosions made of certain juices or fluid substances of the body, when they come to mingle with each other: and forasmuch also as I do not remember, I have heard the maintainers of this hypothesis insist on other instances in favour of it, than the going off of gunpowder; which being not a liquor, but a consistent and brittle body, and requiring for its explosion either actual fire, or a far intenser heat, than can be supposed natural in men, and other animals; I was induced to suspect, they were not yet provided with better examples; and therefore I presume, it will be looked upon, as a thing neither useless, nor altogether impertinent, if, without offering to determine any thing about the truth of the opinion, I supply the embracers of it with two or three examples of explosions made by the bare mingling of liquors; which I shall borrow from the elsewhere-mentioned notes, that I drew up some years ago, in order to the improvement of some parts of physick.

EXPE-

EXPERIMENT I.

Of an explosion made with the spirits of nitre and wine.

We took spirit of nitre, so strong, that the fumes made the upper part of the glass it was kept in, always reddish; and having put but one ounce of it into a bolt-head, with a long neck, capable to contain, as we guessed, twelve or sixteen times as much, we caused an equal weight of alcohol, or highly rectified spirit of wine, to be taken, and a little of it being put to the spirit of nitre, it presently made so strong and quick an expansion or explosion, that some of it flew out of the glass, and hit against the ceiling of the room (where I saw the mark of it) and falling upon his face that held the glass, made him think (as he told me) that fire had fallen upon it, and made him run down the stairs like a madman, to quench the heat at the pump. Wherefore, bidding the laborant proceed more warily, I ordered him to put into the bolt-head but part of a spoonful of spirit of wine at a time; and yet, at each of a pretty many affusions that I staid to see the effect of, there would be a great noise, as of an ebullition, though no store of froth produced, and accompanied with so great a heat, that I could not hold the glass in my hand; and immediately there would issue out a copious and red smoke; to which when I caused a little candle to be held, though at near half a foot distance from the top of the bolt-head, it would presently take fire, and burn at the top of the bolt-head like a flame at the upper end of a candle, till I caused it to be blown out, that fresh spirit of wine might be poured in; which, when it was all mingled with the other liquor, the heat and conflict ceased.

Divers other phænomena relating to this experiment (by which I intended to make out more things than one) belong not to our present subject, and are already set down in other papers. But yet it will be pertinent to shew in this place, that the noise and ebullition produced in this mixture is not unaccompanied with a briskly expansive, or an explosive motion. To make then an experiment to this purpose, and yet avoid the danger, whereto the making of it unwarily might expose both the vessels and us, we put an ounce of such strong spirit of nitre, as is above-mentioned, into a moderately large bolt-head furnished with a proportionable stem, over the orifice of which we strongly tied the neck of a thin bladder; out of which most part of the air had been expressed, and into which we had conveyed a small phial, with a little highly rectified spirit of wine: then this phial, that before was closed with a cork, being unstopped, without untying or taking off the bladder, a small quantity, by guess not a quarter of a spoonful, of the alcohol of wine, was made to run down into the spirit of nitre, where it presently produced a great heat and commotion, and blew up the bladder, as far as it would well stretch, filling also the stem and cavity of the glass with very red fumes, which presently after forced their way into the open air, in which they continued for a good while to ascend in the form of an orange-coloured smoke.

EXPERIMENT II.

Of an explosion made with oil of vitriol and oil of turpentine.

If I had at hand the papers, you have divers times heard me speak of, about heat, I could give you the particulars of some trials about explosion, that perhaps you would think more pertinent than despicable: but, for want of those papers, I must content myself to tell you in general, that I remember, that I have, more than once, taken strong oil of vitriol, and common oil of turpentine, and warily mixed them in a certain proportion, by shaking them very well together; and that thereupon ensued (what I had reason to look for) so furious an agitation of the minute parts of the mixture, and so vehement or sudden expansion or explosion, as did not only seem strange to the spectators, but would have proved dangerous too, if I had not taken care before-hand, that the trials should be made in a place where there was room enough; and that even the operator, that shook the vessel, should stand at a convenient distance from the mixture.

EXPERIMENT III.

About an explosion made by two bodies actually cold.

I REMEMBER not, that I found the assertors of explosions in animals to have taken notice of a difficulty, which to me seems not uneasy to be observed, and yet very worthy to be cleared. For it is known, that fishes, and those especially of the vaster sort, can move and act in the waters with a stupendous force; and yet it is affirmed by those that pretend to know it, that the blood of most fishes is still actually cold: and I remember, I found the blood even of those I dissected alive to be so. From whence most men would argue, that even in the vast sea-monsters there can be made no explosions, these being still effected by or accompanied with an intense degree of heat.

IT were incongruous to my design to examine this difficulty, as it directly regards the explosions said to be made in animals: but speaking of explosions in general, perhaps I might do the favourers of vital ones (if I may so term them) no unacceptable piece of service, by experimentally shewing that it is not impossible, though it seem very unlikely, that explosions should be made upon the mixture of bodies, which, whilst they seem to put one another into a state of effervescence, are really cold, nay, colder than before their being mingled. Of these odd kind of mixtures, I remember I have in another * paper set down some trials, that I made to other purposes, as well with two liquors, as with a liquor and a solid body; which later sort I there mention my having made by an improvement of an experiment of the excellent Florentine virtuosi. And among those trials I find one, whose pertinency to the matter in hand invites me to annex as much of it, as is proper in this place.

THERE were put two ounces of powdered sal armoniac into a pretty large glass-tube, hermetically sealed at one end; into the same a slender glass-pipe, furnished with two ounces of oil of vitriol, was so put, that, when we pleased, we could make the liquor run out into the larger tube, which, after these things were done, was closed exactly, so that nothing might get in or out. My design was, that this instrument should be so warily inverted, that the operator might get out of the way, and the oil of vitriol, falling slowly upon the sal armoniac, should, without producing any heat, produce an explosion not dangerous to the by-standers. But whilst I was withdrawn to a neighbouring place to write a letter, the operator not staying for particular directions, rashly inverted the instrument, without taking care to get away; whence it happened, that as soon as ever the contained liquor, being too plentifully poured out, came to work on

* About the production or extrication of air.

the

the sal armoniac, wherewith it is wont to produce cold, there was so surprizing and vehement an expansion or explosion made, that with a great noise (which as the laborant affirmed, much exceeded the report of a pistol) the glasses were broken into a multitude of pieces, many of which I saw presently after, and a pretty deal of the mixture was thrown up with violence against the operator's doublet and his hat, which it struck off, and his face; especially about his eyes, where immediately were produced extremely painful tumors, which might also have been very dangerous, had I not come timely in, and (to add that upon the by) made him forthwith dissolve some saccharum Saturni in fair water, and with a soft spunge keep it constantly moistened by very frequently renewed applications of the liquor: by God's blessing upon which means, within an hour or two, the pain, that had been so raging, was taken away, and the fretting oil of vitriol was kept from so much as breaking the skin of the tumors that it had made.

The first part of the relation of this trial might have been omitted, or at least shortened, unless I had designed to communicate unto you a way of doing what I do not know to have been attempted by others, namely to put bodies together, when and by what degrees one pleases, after the glass, that contains them, has been hermetically sealed up; which mechanical contrivance, especially as it may be varied, may be, as I have tried, usefully applied to more purposes, than it were proper here to take notice of.

But to conclude with a word or two touching the foregoing experiment, I shall only add, that another time we made a like trial a safer way, by tying a bladder so to the top of a bolt-head, into which we had before-hand put the sal armoniac, that, by warily moving the bladder, whence the air had been expressed, we could make some of the sal armoniac, we had lodged in its folds, to fall upon the liquor, with which it presently made an explosive mixture, that quickly blew up the bladder.

But these, Sir, are bare conjectures, left to be, after a farther discussion (if you think them worthy of it) determined by you, to whom as these papers are addressed, so they are also submitted by the writer of them.

I am, SIR,

Yours, &c.

A N

HYDROSTATICAL DISCOURSE,

OCCASIONED BY THE

OBJECTIONS of the Learned Dr. *HENRY MORE*,

AGAINST SOME

EXPLICATIONS of NEW EXPERIMENTS made by Mr. *BOYLE*;

AND NOW

Publiſhed by way of PREFACE to the three enſuing TRACTS.

To the READER.

WHEN I determined to write this polemical diſcourſe, I did not forget, that when I firſt ventured ſome of my trifles abroad into the world, my friends obtained from me a promiſe, that after I ſhould have anſwered the two firſt, that ſhould expreſly write againſt me (which happened to be the learned *Linus* and Mr. *Hobbes*) to ſhew that I was not altogether unacquainted with a way of defending truths, I would afterwards write no book in anſwer to any that ſhould come forth againſt mine; for, not only my friends, but I, thought it enough for a perſon, that never was a gown-man, to communicate freely his thoughts and experiments to the curious, without deſpairing, that thoſe things that ſhould be evidently true, would be able to make their own way, and ſuch as were very probable, would meet with patrons and defenders, in ſo inquisitive an age as ours. And indeed I do not find that either upon the account of my writings, or ingenious men's opinion of them, I have had much cause to repent the keeping of my promise, notwithstanding the writings that have impugned some of mine, but without much prejudice, that I know of, either to the proposed truths, or the proposer of them. And therefore I should not at all have entered upon defence of what is attacked of mine by the learned Dr. *More*, if I had not supposed, that it would not require a book, but might be dispatched in a preface: for having by me some little tracts that should, though the doctor had never engaged me, have been imparted to the publick, and observing that the new experiments contained in one or other of them would by an easy application be brought to confirm my formerly delivered explications of other phænomena, and enervate the doctor's objections against them; I thought I might, without long troubling the reader, or myself, defend what I looked upon as truth, by answering some incidental passages of the doctor's discourse, and referring the reader, for the main points in controversy between us, to those experiments of the following tracts, which clearly contain the grounds of deciding them. But yet this consideration would not perhaps have engaged me to write the following preface, if the objections I was to answer had not been by a person of so much fame, proposed with so much confidence, and though with very great civility to me, yet with such endeavours to make my opinions appear not only untrue, but irrational and absurd, that I feared his discourse, if unanswered, might pass for unanswerable, especially among those learned men, who, not being versed in hydrostaticks, would be apt to take his authority and his confidence for cogent arguments; and who (not observing how liberal some men are of titles to the arguments, that please them) would make a scruple of thinking, that what is with great solemnity delivered for a demonstration in a book of metaphysicks, can be other than a metaphysical demonstration. The care therefore, that what I judge to be true, should not be made to pass for absurd, which is a degree beyond what is merely erroneous, by being so severely handled by a person of Dr. *More*'s fame and learning, induced me to begin the following paper; which should have been shorter than now it is, but that I was persuaded to lengthen it beyond what was either necessary or designed, that I might, by the addition of some few thoughts and experiments on the occasions that were suggested to me, endeavour to clear up and confirm some hydrostatical truths, that, I fear, are but by very few either assented to, or perhaps so much as understood, and so might make the reader amends for the trouble I was forced to give him in a dispute, which I apprehended he might otherwise think himself but little concerned in. And he will, I hope, easily discern, that I have no mind to burthen him in my preface with things not pertinent to the scope of it, if he take notice, that both for his sake and the learned doctor's (whose civility I would not leave unanswered) I have restrained myself to the defensive part, forbearing to attack any thing in his Enchiridium Metaphysicum, save the two chapters, wherein I was particularly invaded.

But though I have declined the delivering my opinion of the doctor's book, yet I dare not forbear owning my not being satisfied with that part of his preface, which falls foul upon Monsieur *des Cartes*, and his philosophy. For though I have often wished, that learned gentleman had ascribed to the divine Author of nature a more particular and immediate efficiency and guidance, in contriving the parts of the universal matter into that great engine we call the world; and though I am still of opinion that he might have ascribed more than he has to the supreme Cause, in the first origin and production of things corporeal, without the least injury to truth, and without much, if any, prejudice to his own philosophy; and though not confining myself to any sect, I do

not

not profess myself to be of the Cartesian: yet I cannot but have too much value for so great a wit as the founder of it, and too good an opinion of his sincerity in asserting the existence of a deity, to approve so severe a censure, as the doctor is pleased to give of him. For I have long thought, that in tenets about religion, though it be very just to charge the ill consequences of men's opinions upon the opinions themselves; yet it is not just, or at least not charitable, to charge such consequences upon the persons, if we have no pregnant cause to think they discern them, though they disclaim them. And since men have usually the fondness of fathers for the offspring of their own brains, I see not, why *Cartesius* himself may not have overlooked the bad inferences that may be drawn from his principles (if indeed they afford any such) since divers learned, and not a few pious persons, and professed divines of differing churches, have so little perceived, that the things objected are consequent to such principles, that they not only absolve them as harmless, but extol them as friendly and advantageous to natural religion. And I see not, why so great and radiant a truth, as that of the existence of a God, that has been acknowledged by so many mere philosophers, might not as well impress itself on so capable an intellect, as that of Monsieur *des Cartes*; or that so piercing a wit may not really believe he had found out new mediums to demonstrate it by. And since the learned *Gassendus*, though an ecclesiastick, had been able, as well safely, as largely to publish the irreligious philosophy of *Epicurus* himself; it seems not likely, that so dexterous a wit as that of Monsieur *des Cartes* could not have proposed his notions about the mechanical philosophy, without taking so mean a course to shelter himself from danger, as in the most important points, that can fall under man's consideration, to labour with great skill and industry to deceive abundance of ingenious men, many of whom appeared to be lovers of truth, and divers of them lovers of him also. And I am the more averse from so harsh an opinion of a gentleman, whose way of writing, even in his private letters, tempts me very little to it, because I cannot think him an atheist, and an hypocrite, without thinking him (what Dr. *More* has too much celebrated him) to call him a weak head, and almost as bad a philosopher as a man. For, as far as I understand his principles, some of the most important points of his philosophy (which, if it were needful, I could name) are interwoven with the truth of the existence of a God, or do at least suppose it, and are not demonstrable without it. But I must not prevent the Cartesians, who, now he cannot do it for himself, I doubt not will apologize for their master; though looking upon him as a great benefactor to, though not the first founder of the mechanical philosophy, I could not consent, by a total silence upon such an occasion, to become any way accessary to the blemishing of his memory.

AN

AN HYDROSTICAL DISCOURSE, &c.

SIR;

UPON the advertisement you gave me yesternight, that I was particularly concerned in the learned Dr. *More*'s *Enchiridium Metaphysicum*, I this day turned over the leaves of one, which I have freshly received from the reverend author himself; and and being assisted by the series of the titles, I quickly lighted on that part of the book, whose subject made me expect to find myself questioned there, as I presently found I was. For though that civil adversary is pleased to omit my name, and, the farther to disguise it, employs, instead of it, a great and unmerited encomium; yet by the book he cites, and the experiments against which he argues, it is very easily discoverable, that his objections are meant against me, who see yet no cause at all to be scrupulous to own my name, and the doctrine delivered in the passages he is pleased to oppose.

I DOUBT not but you will presently desire to know what I think of this much expected work; but when I have told you, that I have gained time to peruse only (and that but cursorily) the twelfth and thirteenth chapters, you will, I question not, excuse a person that does exceedingly want health, and yet wants not almost continual avocations, if I now content myself to give you my thoughts of that part of the newly-mentioned chapters, which properly relates to me; I say, that part of the chapters, because there are others, wherein I need not interest myself. For, to omit other paragraphs, the doctor has, in the former part of the twelfth chapter, thought fit to separate from my explication of the phænomena in question betwixt us that of the learned *Henricus Regius*; and the latter part of the same chapter he imploys in an ingenious dispute against those that would have the aërial particles act with perception and design, and (as he speaks) *pro ne rata*; which opinion you will easily believe I neither was of, nor am like Sect. 16, to adopt. 17.

IT remains then, that setting aside those discourses of the twelfth chapter, wherein it is needless, that I should make myself a party; I proceed to consider those paragraphs, which will be easily guessed to be levelled at my explications, and by which I must confess, I cannot at all be yet convinced of their being false ones. But in doing this, I shall not only, in compliance with my present haste, but also to express my respect to the learned doctor, forbear to say any more, than what I shall judge requisite to answer the objections that directly concern my own explications, without meddling, by way of retaliation, with his hypotheses or opinions, or endeavouring to set any passages of his writings at variance among themselves, or to take those little advantages, which are usually sought for by disputants.

I SHALL

I shall not trouble you, nor tire myself with any schemes, since the doctor has taken the pains to insert those, that are necessary for his purpose, in his book, and I have not my own at hand. Wherefore, not doubting, that you have by you those books of mine he refers to, and supposing, that you will, whilst you are reading, have also this book, with the inserted schemes before your eyes, I shall not spend time on any further preamble, but immediately enter upon the consideration of the objections I am to answer.

The FIRST SECTION.

CHAP. I.

THE first explication of mine, that the learned doctor animadverts upon in his twelfth chapter, is that which I give in the thirty-third of my physico-mechanical experiments, touching the spring and weight of the air; where I relate, that the sucker in the air-pump of our engine, having been forcibly depressed to the lower part of the brass cylinder, which yet was carefully closed at the top, so that the cavity of the cylinder was empty of air; this sucker, I say, would, in this case, appear spontaneously to remount towards the top of the cylinder, though it were clogged with a hundred pound weight to hinder its ascent. Which phænomenon I ascribed to this, that the sucker being, by the withdrawing of the air in the cylinder, freed from the wonted force of the springy air, that endeavoured to depress the internal part of it, was not enabled, by the appendant weight, to resist the pressure of an atmospherical cylinder equal in diameter to it, which, pressing against its lower or external surface, endeavoured to impel it up.

Now the doctor having, in the two first paragraphs, made a description of my engine, (which I shall now pass over) does in the third teach us, that the corporeal cause, if there be any, of the ascent of the sucker, must be either in the sucker itself, or in the almost exhausted cavity of the cylinder, or, lastly, in the external air. Which premised, he does in the same third section, and in the fourth, endeavour to prove at large, that the cause is to be derived neither from the one, nor from the other of the two first. And therefore I, that maintain neither of the opinions he disputes against, shall leave those paragraphs of his untouched. Nor shall I meddle with the fifth, sixth, and seventh, where he argues against the explication of some, that would solve the phænomenon upon some Cartesian grounds, and as well amply, as particularly against the solution, that he supposes would be given of it, congruously to his own sentiments, by the learned *Regius*. These discourses, I say, of the doctor's I leave untouched; because it is at length in the eighth paragraph, that he impugns that solution of the phænomenon, which he ascribes to me, whose opinion he first delivers, though not just in the terms I would express it myself; yet I dare say very sincerely, and so near my sense, that I shall forthwith pass from the eighth section to the beginning of the ninth, where he begins to propose his objections, which he is pleased to usher in with a compliment

pliment to me, that I should be very vain, if I looked upon as any thing more than a compliment.

To his first objection, proposed in these words, *Primò enim, si hæc solutio verè mechanica sit, quæ tandem causa verè mechanica assignari potest gravitationis singularum particularum, totiúsque atmosphæræ in suis locis? nam quod materiam subtilem attinet*, &c. I answer, that I did not in that book intend to write a whole system, or so much as the elements of natural philosophy; but having sufficiently proved, that the air we live in, is not devoid of weight, and is endowed with an elastical power or springiness, I endeavoured by those two principles to explain the phænomena exhibited in our engine, and particularly that now under debate, without recourse to a fuga vacui, or the anima mundi, or any such unphysical principle. And since such kind of explications have been of late generally called mechanical, in respect of their being grounded upon the laws of the mechanicks; I, that do not use to contend about names, suffer them quietly to be so: and to entitle my now examined explication to be mechanical, as far as I pretend, and in the usual sense of that expression, I am not obliged to treat of the cause of gravity in general; since many propositions of *Archimedes*, *Stevinus*, and those others that have written of staticks, are confessed to be mathematically or mechanically demonstrated, though those authors do not take upon them to assign the true cause of gravity, but take it for granted, as a thing universally acknowledged, that there is such a quality in the bodies they treat of. And if in each of the scales of an ordinary and just balance a pound weight, for instance, be put; he that shall say, that the scales hang still in æquilibrium, because the equal weights counterpoise one another; and in case an ounce be put into one of the scales, and not into the opposite, he that shall say, that the loaded scale is depressed, because it is urged by a greater weight than the other, will be thought to have given a mechanical explication of the æquilibrium of the scales, and their losing it, though he cannot give a true cause, why either of those scales tends towards the center of the earth. Since then the assigning of the true cause of gravity is not required in the staticks themselves, though one of the principal and most known of the mechanical disciplines; why may not other propositions and accounts, that suppose gravity in the air (nay prove it, though not *à priori*) be looked on as mechanical? Page 1

CHAP. II.

The next thing the doctor opposes to my explication, is a resolute denial, that there is any such gravitation as I pretend, of bodies, or their particles, in their proper places. But because, for the proof of his negation, he refers us to the next chapter, we shall hereafter have a fitter place than this to consider it in.

Thirdly, he tells us, we may justly doubt of the equal diffusion of the springy power, or the pressure of the air every way. In what sense, in some cases, I admit of a small inequality between the pressure of fluids against differing parts of a surrounded body, I have * elsewhere declared, and need not here discourse of; since in the case before us, and in the like, that pressure is inconsiderable enough to be safely neglected. And whereas our author thus argues, *Semotâ vi elasticâ, particulæ tamen atmosphæræ deorsum tenderent. Est igitur depressio quædam deorsum præter vim elasticam ipsi superaddita; sursum non item, sed elastica sola, estque suppar ratio in pressionibus transversis &* Page 139.

* See the Hydrostatical Paradoxes, especially Parad. 7.

obliquis: I presume he did not sufficiently consider our hypothesis and the nature of the pressure of fluid bodies, that have weight: for water, to which no springiness is ascribed, as there is to air, but which acts by its weight and fluidity, is able, upon the score of those qualities, to buoy up great ships, that the ebbing tide often leaves upon the strand.

Page 139. AND whereas the learned examiner proposes a fourth objection in these terms, *Quibus omnibus addas, difficile esse intellectu, si unius cylindri atmosphæræ pondus æqualis diametri cum embolo reflectione in fundum emboli derivetur, cur non quinque alii cylindri aeris, qui circumstant embolum, in ejus fundum eodem modo simul agere possunt, ita ut vis sursum impellens embolum sextuplo major sit, quàm hactenus ab hujus opinionis fautoribus existimata est. Quod si sit, tunc certè, siquo artificio fieri possit ut unius solius cylindri actio in embolum admitteretur, reliquorum quinque exclusa, & pari tamen facilitate embolus ascenderet, manifestum indicium esset, ne unum quidum cylindrum atmosphæræ agere in fundum emboli, sed totam hypothesin ingeniosam tantummodo esse fictionem.* I presume, hydrostaticians will think this might have been spared. For they will tell him, that there can be no more of a fluid press directly upward against the cylindrical orifice of a body immersed in that fluid, than a cylinder of that fluid of the same diameter with the orifice, the lateral pressures bearing against the lateral parts of the cylinder. And therefore if you invert, for instance, a pipe open at both ends, and filled to a height with oil, with common water; the oil, that is kept up by the pressure of the water upwards, will keep at the same height as to sense, whether the vessel, that contains the water, be broad or narrow, provided it be somewhat larger than the orifice of the pipe.

AND now, to invalidate yet further the precedent objections made by the doctor, I shall add, that it need not be thought incredible, that the atmosphere by its weight, or the spring of the air compressed by that weight, should be able to raise up four-score or an hundred pounds, hanging at the sucker; since I have * manifested two or three years ago, by a clear and cogent experiment, that a little air in a bladder will, by its mere spring, be able to heave up a weight of a hundred pounds, and this without the help of any rarefaction by heat. By which experiment may be also confirmed, what I delivered a while since about the endeavour of the air, that is wont to be included in our brass cylinder, by expanding itself to thrust away the sucker (which, in regard of the structure of the pump, it can do no otherwise than downwards) with a depressing force, equivalent to the pressure upwards of the atmosphere, against the external part of the same sucker.

CHAP. III.

BUT I shall not insist upon the foregoing objections, because the learned doctor himself tells, that their attempts may seem to be but light skirmishes in comparison of that which follows. Whereunto I shall therefore apply my attention.

THIS grand objection our learned adversary takes from the already often-mentioned ascent of the sucker clogged with a hundred pounds weight, and recommends by this Page 140 introduction: *Etenim ex ipsis phænomeni visceribus robustissimum jam contra omnem mechanicam illius solutionem argumentum eruo, & quod non solum contra vim aeris elasticam suprà dicto modo explicatam militat, sed etiam contra Cartesianum illum aeris conatum Page 140 nixùmque, &c.* Which premised, the argument itself is thus proposed: *Est enim* (says

* See Continuat. of New Exper. Physico-Mechan. Exper. 48.

he) *juxta hujus experimenti phænomenon, vis illa aeris elastica nixúsque expansorius) major multo, quàm quæ fieri potest a rerum natura, quàmque quotidianis illis phænomenis congruit. Nam si nixus hic elasticus tantam vim elasticam haberet, ut plus centum pondo plumbum sursum possit propellere, omnes profectò rerum terrestrium compages tantâ violentiâ comprimerentur, ut nullæ, nisi quæ admodum firmiter compactæ sint, tantæ compressioni resistere possent, quin refrigerentur, vel partium collisione ita contererentur, ut brevi tempore perirent, &c.*

Though this objection be specious enough, yet it presents me with no difficulty, that I was not well aware of; as I presume you will easily perceive by what you will meet with in the following papers, especially that which consists of experiments and considerations about the differing pressures of solids, weights, and ambient fluids. The nature of which pressure and its equality (as far as in our controversy it is needful to be supposed) will, I hope, satisfy you of the invalidity of the proposed objections; especially since the doctrine it impugns, namely the weight and pressure of the atmosphere, is not a bare hypothesis, but a truth made out by divers experiments, by which even professed opposers of it have publickly acknowledged themselves to be convinced.

CHAP. IV.

In the next paragraph (which is the eleventh) the learned doctor adds a further objection, wherein he supposes, that there is laid upon a wooden scale, of the same diameter with the above-mentioned sucker, a lump of butter of the same largeness with the scale. Whence he argues, that if our hypothesis take place, the butter must be pressed against by two cylinders of air, the one pressing it upwards, the other downwards, and the pressure of them both amounting to two hundred pounds. But, says he, the butter is not pressed at all, as appears by this, that no serous humour is squeezed out of it towards the edges, not so much as in those parts that lie parallel to the horizon, whence the conclusion seems easy to be deduced.

But in the twelfth paragraph, the doctor himself proposes a solution, which he might easily foresee I would employ to invalidate his argument; namely, that the air pressing, as well against the sides of the butter, as against the top and bottom, hinders the mass from horizontally extending itself. And whereas, by way of reply to this subterfuge, as it is called in the margent, he subjoins, *Cui respondeo, quòd tamen hoc nihil prohibet, quo minùs in omnes partes horizontales exprimatur humor serosus & lacteus, si revera esset ulla hujusmodi pressura elastica, qualis fingitur:* the reply is easy, that the pressure of the ambient air, which is a fluid more subtil than butter-milk, will as well hinder the starting out of that liquor, as of the parts of the butter itself; as he will easily grant, that attentively considers the nature of the thing, and remembers how air keeps water from running out at the little holes of a gardener's watering-pot closed at the top. What the objector adds about the extrusion of what he calls a subtiler element (supposed to be harboured in the butter) by the pressure of the atmosphere, in case it had any, I think it would not be difficult to answer, if we considered, that a great and undeniable pressure, applied to water, does not sensibly condense it, or deprive it of its fluidity, because of the grossness and strength of its parts. But the argument being but transiently mentioned by the author, and grounded upon a Cartesian supposition, that I never employed, I leave it to those, that may think themselves concerned (which I am not) to make a solemn answer to. Page 142.

Page 143. And whereas our learned examiner superadds, *Quod tametsi butyri massa in disci lignei speciem reducta, cujus margo centum vicibus areâ sit minor, interque duas laminas ligneas ejusdem formæ ac latitudinis posita, filis suspenderetur in aere tanquam in lance, ita ut pressura aeris elastica, quà ab infra, quà desuper, ducentis fere vicibus excessura sit pressionem in marginem butyri, butyrum tamen nihilo arctiùs comprimetur per vim aeris elasticam, nec aliter hîc afficietur quàm antea:* he seems not to have sufficiently considered the laws of the hydrostaticks, according to which, supposing the pressure of the atmosphere that he rejects, the butter ought not to be deprived of its shape. For the pressure of the ambient air, being equal on all sides, if we suppose the superficies of the butter to be distinguished into a multitude of little equal portions, each of these, whether they be situated horizontally, or on the edges, can be pressed against but by an atmospherical pillar equal to its basis; and the horizontal portions, if I may so call them, cannot be thrust out of place, without there be at the same time squeezed out some of the lateral portions, which yet cannot be so displaced, because they also are, with equal force, pressed inwards by little aërial pillars, whose bases are contiguous to them, and bear against them. Which answer, though of itself sufficient, may be much confirmed by the instance, you will hereafter meet with, of a lump of butter, that kept its irregular shape, in spight of a great and manifest pressure of the water that surrounded it.

And this answer may suffice to disprove what the doctor annexes in the beginning of the thirteenth paragraph, about the vast excess of pressure, which the air exercises upon the flat and horizontal surfaces of the above-mentioned lump of butter, in comparison of the pressure the marginal parts of its surface can be exposed to. What he adds, and illustrates with a scheme, about the hand's being assisted with the pressure of the air, it concerns not me to answer. But whereas among the places, where the elastical power of the air is understood not to reach, he reckons a pail-full of water, with a lump of butter put in it; he supposes that, which our hydrostaticks will by no means allow, and which is disproved by several, both of our former experiments, and by those you will meet with in the following papers. By which it appears, that the pressure of the atmosphere is exercised, as indeed I do not see what should hinder it from being, even upon bodies that are quite immersed under water; and by which, added to what has been hitherto discoursed in answer to the learned doctor's objections, you will easily judge, how deservedly he shuts up the arguments, we have been examining, with this
Page 143. conclusion. *Adeo ut extra omnem controversiam positum videatur, quòd nulla est ejusmodi vis elastica in aere, qualem è doctis nonnulli supponunt, multoque minus tam fortis, ut centum librarum pondus superet. Quod erat demonstrandum.*

CHAP. V.

But this is not all the doctor urges against me in this chapter; for in the fourteenth paragraph he seconds his former argument by another, drawn from this experiment of mine, that having taken two round marbles, whose surfaces, that were to be contiguous, were as well ground very flat, as carefully polished; and having placed them one directly upon the other, they did in a horizontal posture so firmly cohere, without the help of any glue, or viscous body *, that the upper marble being pulled up, would take up the lower, though clogged with a weight of fourscore and odd pounds.

* See the History of Fluidity and Firmness, p. 222. of the second edition.

This experiment, when I many years ago firſt publiſhed it, I referred to the action of the atmoſphere, which preſſing equally and ſtrongly againſt the ſurfaces of both the marbles, except where they were contiguous, the higher could not be drawn directly upwards from the lower (and conſequently muſt be followed by it) by a leſs force, than that which was equivalent to the weight of as great a cylinder of the atmoſphere as leaned upon the upper marble.

This experiment thus explained, though it hath been judged a very favourable one to the hypotheſis, on whoſe behalf I alledged it, does yet to the juſtly famous doctor ſeem a very conſiderable argument againſt it, though for this judgment of his he urges only this reaſon, that if the force, with which the air preſſes the lower marble againſt the upper, be able to ſuſtain that marble, though clogged with the great weight abovementioned, the ſame preſſure of air would much more eaſily ſupport a plate of wood brought to a true plain, and not loaded with any weight, if the wooden plate were ſubſtituted to the lower marble, and, inſtead of it, applied to the upper.

But ſince the experiment, as I propoſed it, did upon trial ſucceed very well, it had not been amiſs, if the learned examiner had conſidered it as it was really and ſucceſsfully made, and ſhewed why the preſſure of the ambient air was not able to hinder the ſeparation of the marbles: and his needleſs ſubſtitution of a wooden plate, inſtead of the lower marble, eaſily ſuggeſts a ſuſpicion, that there may lie ſome fallacy, though not intended by him, in the variation he propoſes of the experiment. And he ſeems to have himſelf had thoughts of this kind, by taking notice, that it may be anſwered on our behalf, that a wooden plate cannot be ſo exactly applied to the upper marble, but that there will be a little air intercepted between it and the bottom of that ſtone. And though, having granted that it may be ſo, he employs two pages to ſhew, that this intermediate air could not keep the preſſure of the atmoſphere from ſupporting the unclogged plate of wood, if it had been that preſſure, which, when there was no ſuch intermediate air, had ſuſtained the lower marble with all the appendant weight; yet I confeſs, his proofs ſeem not to me to be anſwerable to the aſſurance he uſes in ſpeaking of them. His examples taken from gunpowder and wind you will eaſy judge not to be very proper, where we are not conſidering a force, that acts by a ſudden and vaniſhing impetus, but a conſtant and equal preſſure. And as to his other inſtance, which is taken from five men, that thruſt againſt the ſixth (ſtanding with his back to a wall) who is but as ſtrong as any one of them; I anſwer, that neither is this example near-enough of-kin to our caſe. For each of theſe five men is ſuppoſed to have an equal power of thruſting, proper to himſelf, and independent from all, or any of the other four. And the ſixth man is likewiſe ſuppoſed to reſiſt but by his own ſingle force, without having his power of re-acting increaſed by the force wherewith the others thruſt againſt him. But in our caſe the thing is quite otherwiſe; for ſuppoſing that ſome aërial particles be ſo placed, that a ſolid body hinders them to recoil or expand themſelves, we are to conſider, that as the contiguous corpuſcles of air preſs againſt them, not by their own ſingle weight or preſſure, but as they tranſmit the action of all the other particles of the air, which by their weight or preſſure thruſt them on; ſo the aërial particles, contiguous to the ſolid body, reſiſt not barely by that force, which they would have if they were not compreſſed, but by virtue of the ſpringineſs they acquire upon the ſcore of the forcible inflection they ſuſtain from the action of the corpuſcles, that either mediately or immediately thruſt againſt them; and conſequently, in proportion to that external force, the elaſticity of theſe compreſſed particles will be encreaſed, as we ſee, that a bow, or other ſpringy body, the more it is bent by an external force, the greater power it has to reſiſt further compreſſion. Upon which grounds it need to be no wonder,

der, that a small portion of air, being almost included in a solid body, and having some (though but very little) time been exposed to the outward air, should be capable of resisting the pressure of as much of the whole atmosphere, as can come to press against it. For, this pressure of the atmosphere being continual, if the springiness of the aërial particles were not now great enough to resist that pressure, they must necessarily have been before-hand inflected or compressed by it, till the endeavours of the one and the other were reduced to an equipollency. Of which I shall give you an instance in so obvious a body as a bubble at the top of the water. For though there be but a little air included in a very thin and transparent film of water, yet this little air is so well able to resist the weight of all the atmosphere that can come to bear against it, that all the pressure of it is not able to make the film shrink, or become wrinkled; which it would do, if the corpuscles of the internal air were not reduced to a springiness, which makes its power of resisting equal to the endeavour of the external atmosphere to compress it. And to let you see, that we may well conceive such a springiness of the air included in the bubbles, I have elsewhere related, how, by barely withdrawing the pressure of the ambient air from glass-bubbles, hermetically sealed with air in them, not compressed beyond its usual state, the spring of the internal air would make the bubbles fly in pieces: and this will happen to stronger glasses than bubbles, as you will find in one of the former experiments *. And if we would illustrate what we are debating of by an example, it should not be by considering, as the doctor does, the endeavour of five men against the sixth, that hath his back to the wall; but that of five bladders full of air, piled up, and resting upon a sixth. For in this case, whatever force or power of pressing we suppose in the incumbent bladders, they all bear jointly upon the lower, which continuing at a stand, must thereby be so compressed, as to be able to resist their joint endeavours; as it is manifest, because otherwise it would not continue in that state, but be farther compressed; which is against the supposition.

This notion about pressure and resistance I have the more particularly deduced, because I found many modern naturalists, and even hydrostaticians themselves, to be great strangers to it. For which reason I shall add, that I have evinced it by purposely devised experiments in the continuation of the physico-mechanical experiments † about the air. Were it not for this, I should perhaps have spared myself the labour of setting down these thoughts, as not necessary to the solution of the doctor's objections. For he admits a layer, or (as he aptly speaks) an area of aërial particles to be interposed between the upper marble and the wooden plate; and therefore the flatness and stiffness of those two bodies must keep them from an immediate contact, as well at the edges, as by the help of the same area they do elsewhere; and consequently, that interposed air may communicate with the ambient air. From whence the laws of the hydrostaticks (which I have elsewhere shewn) will allow me to conclude, that the weight of the atmosphere endeavours to depress the upper surface of the wooden plate; and so what the examiner urges of the inconsiderable resistance, that the few aërial particles, interposed between the flat bodies, can make to the great pressure of the column of air, that thrusts the wooden against the marble plate, would not conclude, though our former answer could not have been made; since the resistance, made by the interposed aërial particles to the pressure upwards of the atmosphere, is not, in our present supposition, made by those particles alone, but by the weight of the lateral and superior part of the atmosphere, exercised by the intervention of these particles. Which being so, what the learned doctor

* See the Tract about the Pressure of the Air's Spring on Bodies under Water.

† Exper. xxv, and elsewhere.

adds,

dds, that the weight of the wooden plate itself is here of no consideration, must needs e a mistake. For the two equal atmospherical pressures, the one against the upper urface of the wooden plate, and the other against the lower, countervailing, and consequently frustrating the endeavour of each other, the gravity of the wood itself will suffice to make it fall, as well as if it were pressed against by neither of them. And from this discourse you will easily judge, whether the doctor had reason to say as he does, *Quam ab omni ratione igitur absonum est, ut superficies illa sive area aerearum particularum, quæ insinuant se laminam ligneam inter & marmor, solidam columnam hujusmodi particularum, vi elastica sursum enitentium, contra laminam ligneam obnitendo vincat, ipsamque laminam in terram deturbet.* Page 146.

CHAP. VI.

WHAT he adds in the sixteenth number against those, that fancy the aërial particles to be endowed with perception, and to act with design *pro re nata*, does not all concern me; and what he adds in the next paragraph, wherewith he concludes his twelfth chapter, I shall altogether pass by, as far as it concerns the extravagant conceit he opposes. But because at the close of the paragraph he makes an inference, which comprizes our opinion also; since he concludes, that the experiment by him alledged, *Certissimum est indicium, particulas aerias nec cum consilio nec sine consilio inferius marmor sustinere nec suffulcire:* it will not be amiss to shew, that our opinion is undeservedly included in the inference; which I shall do by briefly solving the phænomenon the doctor lays so much weight on. For if we conceive with him, that the two flat marbles formerly mentioned be suspended, and that to the lower of them, a flat wooden plate of the same shape and extent be applied; I see no cause to wonder, why the two marbles should stick together, and not the lower of them to the wooden plate. For, as I lately noted, there being an area, or bed of aërial particles interposed betwixt the marble and the wood, the weight of the atmosphere, exercised by the intervention of those aërial corpuscles, ought to be æquipollent to the pressure of the atmospherical cylinder that bears against the lower surface of the plate; which consequently by its own weight must drop down: whereas there being no such layer of aërial particles interposed betwixt the two marbles, the pressure of the ambient atmosphere, which touches them every where, save where their polished surfaces are contiguous, must keep them strongly coherent. Page 150.

I PRESUME I need not mind you, that hitherto I have discoursed upon supposition, that the doctor experimentally knows, what he delivers concerning the non-adhesion of an exactly smooth wooden plate to a marble one; and upon his concession, that because of the want of sufficient congruity between the surfaces of two bodies, there is a bed of aërial corpuscles interposed between them. But now, I think, it will not be unfit to take notice to you, that though to illustrate, on this occasion, a subject that is generally so little understood, as the exercise of pression among fluid bodies, I have answered my learned adversary's objections, as I had nothing more to say for my explication of the suspension of coherent marbles, than what I many years since delivered in the little tract by him cited; yet I have since abundantly confirmed that explication by the 50th of the experiments published in my continuation; which if the doctor had been pleased to read, perhaps he would have received the same satisfaction, that other learned men have done; since there I experimentally shew, that the undermost marble, with-

without the accustomed clog, would, upon the bare withdrawing of the sustaining air, drop off from the upper. And whereas the two marbles in our vacuum would not cohere, as soon as the formerly excluded air was let in upon them, it did by its supervening pressure make them stick together very strongly.

THE SECOND SECTION.

CHAP. I.

I PROCEED now to the second of those two chapters, that I am interested to consider, in which the learned examiner is pleased to attack three or four of my hydrostatical opinions and explications; in the defence whereof, I hope, I shall be the less put to exercise your patience, because the learned doctor himself is pleased to grant me almost as much as I need desire concerning the truth of the hypothesis, whereon my paradoxes and explications are founded. For whereas the main thing I supposed in my hydrostatical papers is, that in water, though stagnant, the superior parts do actually, though not always prevalently, gravitate upon the inferior, or (if you will) press upon them, even when they do not sensibly depress them; the doctor in divers places allows this hypothesis to be consonant to the principles of the mechanical philosophy; and accordingly having shewed that in a suspended tub of water the whole liquor gravitates upon the
Page 161. bottom of the tub; he subjoins, *Jam verò cum tota hæc aqua constet ex particulis aqueis non compactis vel concretis, sed solutis à se invicem, impossibile est, ut omnes fundum situlæ premant, nisi infima quæque ab omnibus superioribus prematur, quemadmodum clarè demonstravimus in secunda sectione hujus capitis; nempe, si nullæ causæ nisi purè mechanicæ (quales sunt motus localis, magnitudo, figura,* &c.) *in edendo hoc phænomeno se intermiscent.*

AND elsewhere in the same chapter he speaks thus of the gravitation of liquors (towards
Page 152. the close of the second paragraph.) *Necesse utique est, ut partes singulæ graviteat, cum totius sit gravitatio, si non sit aliquid immateriale principium in rerum natura,* &c. And adds, at the beginning of the next number; *sanè atque huic externi motûs hypothesi, & gravitationis elementorum in propriis locis inde necessariò emergentis, apprimè consonum est primum illud experimentum, quod scriptor profert in paradoxis suis hydrostaticis.*

AND now, Sir, I presume you do not much wonder, if I think these concessions reach the main thing I pretend to. For though I do as freely and heartily, as the doctor himself, who, I dare say, does it very sincerely, admit, or rather assert an incorporeal being, that made and governs the world; yet all that I have endeavoured to do in the explication of what happens among inanimate bodies, is to shew, that supposing the world to have been at first made, and to be continually preserved by God's divine power and wisdom; and supposing his general concourse to the maintenance of the laws he has established in it, the phænomena, I strive to explicate, may be solved mechanically, that is, by the mechanical affections of matter, without recourse to nature's abhorrence of a vacuum, to substantial forms, or to other incorporeal creatures. And therefore, if I have shewn, that the phænomena, I have endeavoured to account for, are explicable by the motion, bigness, gravity, shape, and other mechanical affections of the small parts of liquors, I have done what I pretended; which was not to prove, that

that no angel or other immaterial creature could interpose in these cases; for concerning such agents, all that I need say, is, that in the cases proposed we have no need to recur to them. And this being agreeable to the generally owned rule about hypotheses, that *entia non sunt multiplicanda absque necessitate*, has been by almost all the modern philosophers of different sects thought a sufficient reason to reject the agency of intelligences, after *Aristotle*, and so many learned men, both mathematicians and others, had for many ages believed them the movers of the celestial orbs.

CHAP. II.

But you will tell me, that the doctor's concessions will not avail me, since he urges against the gravitation of the elements in their proper places, which gravitation he would have to be suspended by his incorporeal principle, an experiment which, he says, is most manifestly repugnant to our hypothesis. He conceives then, that in a tub or pail-full of water, with a perfectly cylindrical cavity, whose diameter is of sixty-two parts, there is violently kept at the bottom, by the help of a stick, a round plate of wood, whose diameter amounts but to sixty-one of those parts; and that as soon as ever the stick is removed, the wooden plate will emerge to the top and float. *Quod*, says he, *prorsus impossibile esset, si omnes partes aquæ ab (F G) ad (H J) non solùm junctim fundum vasis, sed singulæ singulas in eadem serie subjectas actu premerent.* To which assertion he immediately subjoins this argument to prove it by; *Cum diameter laminæ ligneæ (H M) partes* Page 155. *61, habeat æquales, diameter vasis (H I) habeat 62, manifestum est, quod superficies fundi vasis ad superficiem laminæ se habet ut 3844, ad 3721; quorum differentia est 123. Itaque rotundum intervallum inter latera vasis & marginem laminæ ligneæ habet se ad aream laminæ, ut 123 ad 3721, hoc est, area laminæ ligneæ excedit aream dicti intervalli plusquam triginta vicibus. Ac proinde aqua incumbens ligneæ laminæ excedit magnitudine aquam incumbentem dicto intervallo inter marginem laminæ & latera vasis plus quàm triginta vicibus, pondúsque sive pressio hujus alterius pondus pressionemque vincit plusquam triginta vicibus. Adeò ut impossibile sit, ut aqua incumbens prædicto intervallo ita premat aquam ipsi subjectam, ut hujus vi sublevetur lamina, quam vis tricies major deprimit. Quod* (says he, by way of inference) *æque absonum atque absurdum phænomenon esset*, &c.

How little this ratiocination agrees with the experiments I have formerly told you of, about the cases wherein light bodies will be detained under water, or emerge to the top of it, you will easily perceive, if you compare the one with the other, which you may quickly do, if you please to compare the doctor's discourse with the following narratives of those trials *, to which alone I might therefore refer you. But yet in the mean time, you may, if you think fit, consider a little, whether the argument, whereon the doctor lays so much stress, be any more than a paralogism.

First then, since according to his computation the area of the interval, between the sides of the vessel and the edges of the round board, is 123 of such parts, whereof the area of the board amounts to 3721; it is evident that there must be room enough for the water to pass between the sides of the vessel and the edges of the board, which is supposed on all hands to be of some wood lighter in specie than water, since else it would not emerge upon the withdrawing of the stick.

Next, this board or wooden plate is not here intimated, or supposed to be (and indeed in practice can scarce be) made exactly congruous to the bottom of the vessel, and conse-

* See the tract of the positive or relative levity of bodies under water. Exp. I. &c.

quently the water may get in between them; for which cauſe it is neceſſary to keep the wooden plate forcibly down with a ſtick, which elſe were needleſs. And conſequently this interpoſed water will communicate with the laterally ſuperior water in the veſſel, which ſuperior water may, according to the laws hydroſtatical, by the intervention of the interpoſed, exerciſe its preſſure upwards againſt the lower ſurface of the wooden plate.

Thirdly, the doctor's ſcheme allows and aſſiſts us to conceive (which we may do however) an imaginary plane of water to be parallel to the bottom of the veſſel, and to paſs along the bottom of the board; ſo that of the water that lies between this plane and the bottom of the veſſel, one part is covered by the wooden plate; and the other between the edges of that and the ſides of the tub, is covered with the incumbent water only.

CHAP. III.

These things being premiſed, I thus argue; it is manifeſted by hydroſtaticians after *Archimedes*, that in water, thoſe parts that are moſt preſſed, will thruſt out of place thoſe that are leſs preſſed; which both agrees with the common apprehenſions of men, and might, if it were needful, be confirmed by experiments. It is alſo evident that that part of the above-mentioned imaginary plane, that is covered by the wooden plate, muſt be preſſed by a leſs weight than the other part of the ſame plane; becauſe the wood being bulk for bulk lighter than water, the aggregate of the wood and water incumbent on the covered part of the ſame plane muſt be lighter in ſpecie than the water alone, that is incumbent on the uncovered part of the ſame plane; and conſequently this uncovered part being more preſſed than the other part of the plane, the heavier muſt diſplace the lighter, which it cannot do but by thruſting up the board, as it does, when the external force that kept it down is removed. And, to add this upon the by, this greater preſſure againſt the bottom than againſt the top of bodies immerſed in water ſpecifically heavier than they, is a true reaſon of their emerſion, as I have elſewhere ſhewn. So that there happens no more in this caſe than what uſually happens in the aſcenſion of bodies in liquors ſpecifically heavier than themſelves, on the account of the newly mentioned difference of preſſure. And it is with an expreſs, or ſuppoſed exception of ſuch a difference, which in many other caſes may be ſafely neglected, that I deſire you to take notice of in moſt places of this diſcourſe I ſpeak of the preſſure of ambient fluids on immerſed ſolids, as uniform or every way equal.

It is true, that, according to the doctor's ſupputation, if the ſolid cylinder, conſiſting of the wooden plate, and all the water directly incumbent on it, were put into an ordinary balance, it would there many times outweigh the hollow cylinder of water alone, that leans upon the uncovered part of the imaginary plane. And that is it that ſeems to have deceived the learned doctor. But there are divers hydroſtatical caſes, wherein the phænomenon depends not ſo much upon the abſolute weight of the compared bodies, as upon their reſpective and their ſpecifick gravity; on whoſe account it is, that a ſmall pebble, for inſtance, that weighs not a quarter of an ounce, will readily ſink to the bottom of the river, on whoſe ſurface a log of wood of a hundred pounds in weight will float. It is a rule in hydroſtaticks, that when two portions of water, or any other homogeneous liquor preſs againſt each other, the prevalency will go, not according to the abſolute weight, but the perpendicular height of thoſe portions. And accordingly we find, that if a ſlender pipe of glaſs, being filled with water, have its lower orifice unſtopped at the bottom

of

of a veſſel of water, which contains much more of that liquor than the pipe; yet if this laſt named water were, for inſtance, two feet high, and that in the veſſel but one, the water in the pipe will readily ſubſide, till it come almoſt to a level with the external water, though it cannot do ſo without raiſing the whole maſs of water that ſtagnated in the veſſel.

And now I ſhall ſubjoin an experiment, which, though at firſt it may ſeem ſlight, and was made in leſſer glaſſes and quantities than I would have employed, if I could have procured better accommodations, has the advantage of requiring no curious inſtruments, and yet I hope will ſerve for an ocular proof of the fallacioufneſs of that reaſoning the doctor is ſo ſtrangely confident of.

We took an open-mouthed glaſs, ſuch as ſome call jars, and ladies often uſe to keep ſweetmeats in, which was three inches and a half, or better in diameter, and ſomewhat leſs in depth, and had the figure of its cavity cylindrical enough. Into this having put ſome water to cover the protuberance wont to be at the bottom of ſuch glaſſes, we took a convenient quantity of bees-wax, and having juſt melted it, we poured it cautiouſly into the glaſs, warmed before-hand to prevent its cracking, till it reached to a convenient height. This veſſel, and the contained liquors we ſet aſide to cool, in expectation, that when the heat that had dilated the wax, was gone, it would ſhrink from the glaſs, and conſequently leave a little interval every where between the concave ſuperficies of the veſſel, and convex of the hardened wax; which accordingly came to paſs, and ſaved me the labour of getting the wax ſhaped for my purpoſe with tools; which might have been done, but not without trouble and leſs exactneſs. And now it was eaſy for me to try the experiment I deſigned; for pouring in warily ſome water between the glaſs and the wax, ſo that it filled all the interval left between thoſe two bodies, both at the bottom and the ſides, the wax was made preſently to float, being viſibly lifted up from the bottom, and its upper part appearing a little above the level of the water, which was no more than I did, and had reaſon to expect, according to the true principles of hydroſtaticks. For water being ſomewhat, though but little heavier in ſpecie than wax, and that which was poured into the bottom and ſtagnated there, being preſſed by the collateral water, every way interpoſed between the concave part of the glaſs, and the convex of the wax (ſo that this collateral liquor anſwered what I lately called a hollow cylinder of water in the doctor's experiment) that part of the ſtagnant water that was leaned upon by the wax, being leſs preſſed than the other part of the ſame ſtagnant water was by the water incumbent on it; this latter muſt diſplace the former, which it could not do, but by raiſing up the wax that leaned upon it. And yet this collateral water was ſo far from being heavier than the wax its preſſure impelled up, that both the collateral and the ſtagnant water all together, being weighed in good ſcales, amounted to little above a quarter of the weight of the wax, which happened by reaſon of the narrowneſs of the veſſel, which, if it had been wide enough, I doubt not but the experiment would have ſucceeded, though the wax had outweighed the collateral water ten times more than in our experiment it did. But that the ſolid body exceeded almoſt four times the weight, not only of the collateral, but the ſtagnant liquor too, does ſufficiently overthrow the doctor's ratiocination. Whoſe fallacioufneſs will yet further appear by two other improvements, among others, which I made of one experiment.

For, I. though we poured in more and more water, as long as the veſſel would contain any, the cylinder of wax was but lifted higher and higher from the bottom of the glaſs, but did not appear raiſed, more than at the firſt, above the upper ſurface of the water; which argues, that it was not at all the quantity of the inferior water, which was continually increased, but the pressure of the collateral water, which continued still at the same height in reference to that wax, that caused the elevation of the body.

And, II. to manifest yet more clearly the doctor's mistake, I devised the following trial. We took a round plate of lead about the thickness of a shilling, and having made it stick fast to the bottom of the cylinder of wax, to make this body sink the more directly, we placed one after another, upon the upper part of the wax, divers grain weights (first wetted to keep them from floating) till we had put on enough to make the wax subside to the bottom; for the facilitating whereof we had pared off its edges; by this means, the glass having been at first almost filled with water, there swam about an inch or better of that liquor above the upper surface of the wax. And lastly, we took off by degrees the grain weights that we had put on, till we saw the wax, notwithstanding the adhering lead, rise, by degrees, to the top of the water, above which some part of it was visibly extant.

From this experiment I thus argue: it is manifest, that, according to the doctor's supposition, here was incumbent upon the wax a cylinder of an inch in height, and of the same diameter or breadth with the round surface of the wax; whereas upon the removing part of the water, that lay at the bottom when the wax began to rise, there was incumbent no greater weight than that of the collateral water, and as much of the superior and stagnant, as was directly incumbent upon that collateral water (and would have deserved the same name, if we had supposed the convex surface of the wax to have been continued upwards as high as the glass reached). But now, whereas, according to the doctor's ratiocination, this cylinder of water incumbent on the wax, being an inch deep, and a good deal above three inches broad, must press the wax with a greater weight by several times, than that which the lateral and hollow cylinder of this stagnant water could have upon the rest of the collateral water; yet the height of this aggregate of collateral waters being the same with that of the wax and the water swimming upon it, the difference of the pressure was so small, that barely taking off a weight of four or five grains, the wax would, notwithstanding the pressure of the water incumbent on it, be impelled up and made to float; and by the like weight, put again upon it, it would be made to sink, and by another removal of such a weight (for I purposely reiterated the trial more than once) it would, though slowly, re-ascend. And these phænomena do so much depend upon a mechanical æquipollence of pressure, that even four grains would not have been necessary to make the wax rise or sink, if it had not been for some little accidental impediments that are easily met with in such narrow glasses; for otherwise in a larger vessel we have made the same lump of wax readily enough sink or float, by the putting in or taking off a single grain or perhaps less.

By this you may see, that for the regulation of hydrostatical things, nature has her balance too, as well as art; and that in the balance of nature the statical laws are nicely enough observed.

You may also take notice, upon the by, how little the weight of the cylinder of water upon a body immersed in stagnant water is considerable, whilst there is a pressure of collateral water to counterbalance it; since in this last trial, though the cylinder of incumbent water did continually increase or decrease in length, whilst the lump of wax was sinking or emerging; yet the same despicable weight of a grain or less, that was just able to depress it beneath the upper surface of the water, did by its pressure or removal procure its sinking to the very bottom, or rising again to the top, and on both occasions with an equal slowness, bating that little acceleration of motion that ought to happen upon another account, and which therefore is to be observed in the wax, during its rising as well as during its sinking.

CHAP.

CHAP. IV.

SOME other phænomena I produced, by varying the hitherto mentioned experiment, which are very favourable to our notions about hydroſtaticks. But, ſince they do not directly concern the preſent controverſy, I ſhall in this place only annex a couple, the former whereof affords an eaſy confirmation of that paradox, which we lay as the ground of divers others, and the contrary whereof is maintained not only by doctor *More*, but by many other famous and learned men; namely, that in ſtagnant water the upper parts do actually preſs the lower.

WE took then a very ſlender pipe of glaſs, whoſe cavity was narrower than that of an ordinary gooſe-quill, that heterogeneous liquors may not be able to get by one another in it. This pipe, near one end, was bent upwards like a ſiphon, that it might have a ſhort leg, as parallel as the artificer could make it to the longer. Into this crooked pipe we put a little oil, and then held it perpendicularly in a ſomewhat deep and wide-mouthed glaſs, filled partly with water and partly with a lump of wax, of the bigneſs and ſhape of that already mentioned; that ſo the preſſure of the incumbent water upon the open orifice of the ſhorter leg, might impel the oil into the longer leg, ſomewhat above the ſurface of the water in the veſſel; which it was convenient ſhould be done, that we might the better ſee the motions of the oil, and which we knew muſt be done by the courſe we took; both becauſe oil is lighter in ſpecie than water, and conſequently required not an equal height of water to counterbalance it; and becauſe, in very ſlender pipes, water is wont to aſcend a little above the level of the external water, whereinto they are immerſed. The pipe being, as was ſaid, held upright, it was eaſy to take notice by a mark fixed on the outſide, to what height the oil reached in it.

Now if we conceive a horizontal plane, parallel to the bottom of the veſſel, to paſs by the baſis of the floating wax, it is evident by what has been formerly ſhewn, that, of this imaginary plane, that part, on which the wax is incumbent, is as ſtrongly preſſed by the weight of the wax, as the lateral part of the ſame plane is by the weight of the water incumbent on it (otherwiſe theſe preſſures would not be æquipollent, but the wax would be raiſed); and conſequently that part of this plane that is placed directly over the orifice of the ſhorter leg of the pipe, is no more preſſed than any equal portion of that part of the ſame plane that is covered by the wax. This body being taken out of the water, the liquor ſubſided a great way in the veſſel, and ſo did proportionably the oil in the longer leg of the pipe. And laſtly, having weighed out in a good pair of ſcales as much water as we found the wax to amount to, this liquor was, inſtead of the wax, poured into that which remained in the glaſs; whereupon the oil in the longer leg of the pipe was again impelled up very near to the former mark to which it had been raiſed by the wax. Whence we may gather, that the water newly put in, though in the air it weighed no more than the wax, yet it did as much preſs the water that lay beneath the forementioned imaginary plane, and conſequently that which was directly over the ſhorter leg of the pipe, as the wax that had been taken out, had done. And ſince we have already proved that the wax did conſiderably preſs that plane, it ought not to be denied, that the water alſo (which inſtead of it was able to impel upon the oil in the pipe) did in like manner preſs that plane; and conſequently, that water may be gravitated in water as well as a ſolid body, ſuch as wax is, can. And this is the firſt additional uſe, I told you, I would make of our experiment.

BUT, to come now to the ſecond, there is another phænomenon of it, viz. the abovementioned tenderneſs of nature's balance, whoſe uſe ſeems to be of no leſs general con-

cernment

cernment to the true doctrine of the hydroſtaticks. For, by duly conſidering that phænomenon, and reaſoning a while upon it, we may be helped to rectify that plauſible miſtake, which has long deluded both philoſophers and mathematicians, and does yet impoſe on moſt of them; namely, that a body does not actually gravitate when it does not deſcend. For we have ſeen already, and ſhall further ſhew by and by, that the ſunken wax, and the braſs grains that lie on it, do actually preſs or gravitate upon the ſubjacent water and bottom of the veſſel on which it is incumbent; and conſequently its preſſure being not ſurmounted by that of the collateral water, which is unable to raiſe it, muſt be as great as that of this collateral water. Therefore, when upon the removal of a ſingle grain, the wax, with its incumbent weight, is made to aſcend, and that but very ſlowly, it is evident, that it was ſo far from not gravitating before, becauſe it did not actually deſcend, that it retained its gravity even whilſt it aſcends; as may appear not only by the ſlowneſs of its motion upwards, proceeding from its being in nature's balance very little leſs heavy than it need be, to countervail the preſſure of the collateral water; but by this alſo, that if but a ſingle grain be laid on it, when it begins to riſe, its aſcenſion will be checked and hindered, which could not be done by the addition of ſo inconſiderable a weight, if the wax and the adhering metal did not, even during their aſcent, retain their former gravity, though that were fruſtrated as to the act of deſcending, or ſo much as keeping their ſtation by the prevailing preſſure of the collateral water; ſo that, ſince, as we found, the wax and adhering metal amounted to a good deal above four thouſand grains, it did in the balance of nature weigh, whilſt it was aſcending, not ſo much as a four-thouſandth part leſs than it did, whilſt it was actually deſcending.

C H A P. V.

I SHOULD beg your pardon, Sir, for having detained you ſo long with my reply to a ſingle objection of the doctor's, how pompouſly ſoever propoſed; but that I thought it not amiſs to do ſome ſervice to the true theory of hydroſtaticks, by taking this occaſion to preſent you ſome things that I thought not unlikely to illuſtrate ſome parts of that theory, though above what was neceſſary to anſwer the doctor's argument; to which, I confeſs, I was troubled to ſee ſo learned a man ſubjoin the following concluſion: *Hæc tam luculenta demonſtratio contra gravitationem particularum aquæ inter ſe quamvis junctæ ſitulæ fundum urgeant, ſi non ſit vera atque ſolida, equidem nec mei ipſius nec ullius unquam mortalis in poſterum ratiociniis credam.* But I hope he will not be as bad as his word, but will be pleaſed to conſider, as well as I do for him, that a man may be very happy in other parts of learning, and of greater moment, that has had the misfortune to miſtake in hydroſtaticks, a diſcipline, which very few ſcholars have been at all verſed in, and about which divers of thoſe few have had the misfortune to err, not only in the concluſions they have drawn, but in the very principles they have embraced.

To the foregoing argument, the doctor, though he declares he thinks it needleſs, adds in the fifth paragraph another, taken from the laſt experiment of my hydroſtatical paradoxes, by which he ingenuouſly acknowledges, that I ſeem at firſt ſight to have demonſtrated what I pretend to, about the gravitation of the upper parts of ſtagnant water upon the lower. And I am ſorry that I cannot, in return, acknowledge that his objection, at firſt ſight, ſeemed to me a cogent one, for neither at the ſecond nor third peruſal can I clearly diſcern where his ratiocination lies, ſuppoſing it to be meant for an anſwer to my experiment. And though I conſulted with ſome learned members of the Royal Society, whereof two are mathematicians, and one his particular friend; yet they all confeſſed he had

had not ſufficiently explained himſelf on this occaſion, nor could they ſhew me to what argumentation I might properly direct my reply. Only one of the doctor's correſpondents, having ſeriouſly peruſed his diſcourſe, and the annexed ſcheme, told me, that what ſeemed the moſt probable to him, was, that though the doctor was too civil to give me, *in terminis*, the lye; yet he did indeed deny the matter of fact to be true. Which I cannot eaſily think, the experiment having been tried both before our whole ſociety, and very critically, by its royal founder, his majeſty himſelf. But ſince you have your ſelf ſeen, and made it more than once, I need not ſpend words to convince you, that the matter of fact is true.

But after I had in vain ſought the doctor's meaning where I expected it, chancing lately to caſt my eyes on another place, where I ſaw my ſcheme repeated, I find this paſſage in the explication he endeavours to give of the phænomenon by his hylarchical principle: *Cum verò tam profundè immergitur tubus, ut ubturaculum tangat ſuperficiem V W, vis retractionis aeris ita augetur, ut etiam ponderis appenſi ſuperadditam depreſſionem ſuperet. Videtur igitur quaſi quædam ſurſum-ſuctio aeris in tubo contenti, & conformis ac contemporanea aquæ compulſio in obturaculum, quo tam firmiter in os valvulæ comprimitur, ibique cum appenſo pondere ſuſtentatur.* What conſiderable intereſt the ſuppoſed, but unproved retraction of the valve, or the air itſelf, can have in this phænomenon, I confeſs I do not diſcern; not being able to ſee, but that the experiment would ſucceed, when tried *in vacuo*, although all the atmoſpherical air were annihilated. But if I miſtake the doctor's meaning, I am to be excuſed, ſince I do it not willingly, and his own obſcurity has been acceſſary to it. Nor am I very apprehenſive of being unable to defend my account of an experiment, which (as you know) has had the good fortune to recommend the doctrine, for the proof whereof I deviſed it to many learned and curious perſons, ſeveral of which were ſufficiently indiſpoſed to admit it.

And to avoid all miſtakes and diſputes that may ariſe (which I think they muſt do needleſsly) upon the ſcore of the valve employed in our experiment, I ſhall remind you of another, that I remember I have ſometimes ſhewn you, and divers other virtuoſi, though I remember not whether I have mentioned it in any of my publiſhed writings. The ſum of this trial is, that an arbitrary quantity of quickſilver being, by ſuction, raiſed into a very ſlender glaſs-pipe, whoſe upper orifice is ſtopped with the experimenter's finger, to keep the mercury from falling before its time, the open end of the pipe with the mercury in it is thruſt into a competently deep glaſs of water till the little cylinder of mercury have, beneath the ſurface of the water, attained to a depth that is at leaſt fourteen times as great as the mercurial cylinder has of height. For then, the finger being removed from the upper orifice, the glaſs-pipe will be open at both ends, and there will be nothing to hinder the quickſilver's falling down to the bottom, but the reſiſtance of the cylinder of water that is under it, which cylinder can reſiſt but by virtue of the weight or preſſure of the ſtagnant water that is ſuperior to it, though but collaterally placed above it; and yet this water being by the pipe, whoſe upper part is higher than its ſurface, and acceſſible only to the air, kept from preſſing againſt the mercury any where but at the bottom of the pipe, and being about a fourteenth part of the weight of an equal bulk of mercury, it is able at that depth to make the ſubjacent water preſs upwards againſt the mercury, which is but a fourteenth part as high as the water is deep, with a force equivalent to that of the gravity wherewith the mercury tends downwards. And to manifeſt, that this phænomenon depends merely upon the æquilibrium of the two liquors; if you gently raiſe the lower end of the pipe towards the ſurface of the water, this liquor, being not then able to exerciſe ſuch a preſſure as it could at a further and greater depth, the mercury preponderating, will, in part, more or leſs, as the pipe is more

more or less raised, fall out to the bottom of the glass. But if, when the quicksilver is at the first depth, instead of raising the pipe, you thrust it down farther under the water, the pressure of that liquor against the mercury increasing with its depth will not only sustain the mercury, but impel it up in the pipe to a considerable distance from the lower orifice of it, and keep it near about the same distance from the surface of the laterally superior water. And this experiment may not only serve for the purpose, for which I here alledge it, but also, if duly considered and applied, may very much both illustrate and confirm the explication formerly given of the seemingly spontaneous ascent of the clogged sucker in our exhausted air-pump.

THE last argument the doctor urges against the gravitation of water in what they call its proper place, is deduced from what happens to the divers, who, in the middle of the sea, though the salt-water of that be much heavier, than that of fresh water rivers, do not find themselves oppressed, or so much as feel themselves harmed or compressed by the vast load of the incumbent water.

BUT that the equality of the pressures of an ambient fluid will go a great way towards the solving of this difficulty, you will find by the experiments and considerations you will meet with in the following * papers, to which, for that reason, I refer you. And though the doctor in this same paragraph objects, *tametsi hæc pressio æqualis sit, nihil tamen impedit, quò minùs subtiliores partes corporis magisque fluidas exprimat & elidat:* I remember I answered that exception before, by saying, that those liquors that he supposes should be squeezed out, cannot be so, because there is as great a pressure against those parts at which they should issue, as against any of the rest, if the parts that should be squeezed out, be not too spirituous and subtile, which if they be, I should gladly learn how the doctor knows, that no such minute and spirituous particles are really expelled; especially if that be observed, which we shall soon have occasion to relate, that a small animal, being vehemently compressed in water, seemed a little, though but a little, to shrink.

BUT that we may the more distinctly consider this grand argument, taken from the experience of the divers, that is wont to be employed by the schools, and others, for the vulgar opinion, and is now urged by the learned doctor to prove his; it will be convenient to observe, that it does, at once, both propose a question, and contain an objection, grounded upon the surmised insolublenes of that question.

AND to begin with the problem, " whence it is, that divers are so far from being killed " or oppressed by the weight of the incumbent water, that they are not so much as hurt " by it, nay, that they scarce feel it at all ?" we may take notice, that there is in it somewhat supposed, as well as somewhat demanded. For, in the question, it is taken for granted, that divers, though at never so great a depth, feel no pressure exercised against them by the water; which is an affirmation in point of fact, of whose truth I make some question, for the reasons I shall ere long have occasion to mention.

BUT it will clear the way for what is to follow, if I here divide the noble and difficult problem we are to consider, into two questions; the first, why a diver should not be oppressed and crushed to death by the pressure of the incumbent and ambient water? And the second, why at least he should not be made sensibly to feel it, by suffering some considerable inconvenience from it?

IN answer to the first of these questions, you will easily perceive that divers things may be pertinently applied, that you will meet with in the following paper, to shew the difference betwixt the pressure of fluid and that of solid bodies. And that *de facto* the

* The Author means the new experiments of the differing pressure of heavy solids and fluids.

pressure

preſſure of water may be exceeding great, without deſtroying an animal quite ſurrounded with that liquor, I have long ſince ſhewn in another * treatiſe, by the experiment of a little tadpole, which being, together with the water it ſwam in, included in a bent glaſs ſealed at one end, the animal was not killed or ſenſibly hurt, but only (according to what was lately noted by anticipation) ſeemed to ſhrink into ſomewhat, and but little leſſer dimenſions.

If it be here alledged, that this experiment makes rather againſt me than for me, the learned doctor having made uſe of it with a ſcheme to explain it in his ſixteenth paragraph; it will be fit for me to conſider his objection. Having then recited the matter of fact newly delivered, he adds, *Quod certè fieri non poſſet niſi juxta legem quartam contruſio particularum aquæ contra ſe invicem principio hylarchico inhiberetur & eluderetur. Atque hinc fit, ut quamvis aqua in tubo (A B C) vi trudis (G F) aliquantò facta ſit condenſatior, partes tamen ſic compreſſæ, ut propriùs ad ſe invicem accedant, nihilo inter ſe fiunt comprimentiores.* And then ſubjoining the following paſſage; *Neque enim ſequitur ex earum contactu, quod premant ſe invicem, quandoquidem particulæ, uti fit in duris corporibus, in unum coaleſcere poſſunt, & tamen non mutuò ſe premere* (wherein are ſome things that might be queſtioned, if it were neceſſary) he thus purſues his diſcourſe: *Cùm verò hic particulæ aquæ, ſi omninò premerent ſe invicem, preſſura in gyrinum, columnæ aqueæ, ducentos vel trecentos pedes, æneæ verò, plus viginti vel triginta pedes altæ, preſſionem adæquaret, luculentum eſt indicium, quod revera particulæ ſe invicem non premant. Nam planè eſt incredibile, columnam æneam pro corpore quidem gyrini latam, ſed altam viginti vel triginta pedes & amplius, gyrinoque ad perpendiculum incumbentem, omnia viſcera tam tenellæ gelatinæ non eſſe eliſuram.* Notwithſtanding which allegation I am apt to think you will judge the argument, from this experiment, to be more probable on my ſide than on the doctor's. For there being in our caſe an animal, exceedingly much more tender than a man, expoſed to a preſſure, which he affirms is ſo great, that if it were exerciſed on the tadpole, it ought to ſqueeze out all his guts, I think, I may pretend to have given a pertinent inſtance, that a diver may be at a conſiderable depth under water preſerved from being cruſhed to death by the weight of it. And whereas the doctor tells us, that the cauſe of the incolumity of the tadpole is, that the preſſure or contruſion of the particles of the water againſt one another is hindered or fruſtrated by the *principium hylarchicum*, I reply, that what I affirm is matter of fact, and evident (namely, that there was a great external force duly, and yet ineffectually applied to preſs to death, by means of the water, the animal ſwimming in it); but that this mechanical force was ſuſpended, or made ineffectual, by ſome inviſible and immaterial agent, is but the doctor's hypotheſis, and a thing, which, whether it be true or no, is at leaſt not manifeſt.

Having ſaid thus much about the firſt queſtion, I now proceed to the ſecond, "Why "divers, though at never ſo great a depth, complain not of the preſſure of the water, "nor ſuffer any harm nor inconvenience by it?"

And here, Sir, the queſtion highly meriting a particular curioſity, I ſhall not ſcruple in the more full enquiry I am now entering upon, as well ſometimes to employ and enlarge particulars already mentioned in the laſt of the following papers, as oftentimes to ſtrengthen them with new ones. And I ſhall alſo, for a while, ſuſpend my difference with the doctor, and addreſſing myſelf to you, who, I am ſure, will allow me, that water weighs in water, propoſe, according to my cuſtom, not as a dogmatiſt, but as an inquirer, ſome particulars that may tend to the ſolution of a problem, which I take to be as difficult as noble. Not that I doubt, but it muſt and will be explicated upon the

* The Author points at the Appendix to the Hydroſtatical Paradoxes.

mechanical principles; but partly, because the application of them to the solution will not offer itself to every seeker; and partly, because we are not yet well furnished, either with experiments made on bodies under water, or so much as with so competent an account of the matter of fact, as I think may keep wary men from hesitations about it. For what is commonly reported concerning the divers, is (as has above been intimated) grounded but upon their own relations and answers, perhaps amplified or procured by leading questions from persons, who are generally either slaves or ignorant men, taken from the less sober part of the illiterate vulgar, and prepossessed with the common opinion of the non-gravitation of water in its own place; and consequently are not like to make over-accurate observations, but prone to refer the inconvenient alterations they feel, to any other cause than the pressure of the water, which they are taught to be none at all. If observations about diving were made by philosophers and mathematicians, or, at least, intelligent men, who would mind more the bringing up out of the sea instructive observations, than shipwrecked goods, we should perhaps have an account of what happens to men under water, differing enough from the common reports.

You will in one of the following papers find mention of a learned physician of my acquaintance, that, upon his diving leisurely, perceived a constriction to be made of his thorax by the action of the surrounding sea-water.

A Spanish prelate that lived long in *America*, speaking of the deplorable condition of those wretched Indians that were employed by their inhuman masters about the fishing for pearls, gives us this account of them: "It is impossible that men should be able "to live any long season under the water, without taking breath, the continual cold "piercing them; and so they die commonly parbreaking of blood at the mouth, and "of the bloody-flux caused by the stomach. Their hair, which are by nature coal-"black, alter and become afterwards a branded russet, like to the hairs of sea-wolves, "&c."

Purch. Tom. IV Lib. 8. p. 1587.

And a general of the English in the *East-Indies*, being by them employed on an embassy to the emperor of *Japan*, has this passage concerning some female divers, that he met with in his voyage: "All along this coast, and so up to *Ozaca*, we found women "divers, that lived with their houshold and family in boats upon the water, as in *Hol-"land* they do the like. These women would catch fish by diving, which by net and "line they missed, and that in eight fathom depth. Their eyes, by continually diving, "grew as red as blood, whereby you may know a diving woman from all other women." I know it may be said, that these diseases may proceed from the coldness and moisture, or other qualities of the sea; nor would I confidently reject such a surmise; but it may also be possible, that the compression they suffered under water, might have, at least, a share in the production of these ill effects. For how are we yet certain, that the pressure of the water against their bodies, though it does not manifestly dislocate any solid or firm part, but only somewhat press inwards, as in the above-mentioned tadpole the outward skin and the fibres (both which will easily yield a little way, without being painfully stretched) may not, by straitening the vessels, and otherwise inconveniently alter the circulation of the blood and the motion of the humours, spirits, and other fluid parts of the body? And I am not sure that much of the cold that divers are wont to complain of, when under water, may not be a disaffection produced in the nervous and membranous parts, occasioned by the compression of the ambient water, there being divers things, and pressure among others, besides actual cold, that will make men complain of being cold; and in our case, this sensation may be excited, or assisted, by the hindering of the usual perspiration at the constipated pores of the skin. And it seems not impossible, that one, not so ignorant and heedless as divers are wont to be, may refer a new sensation,

Purch. Tom. I. Lib. 4. C. 1.

ſenſation, that really proceeds from preſſure, to other cauſes; ſince learned and intelligent men, when prepoſſeſſed (as theſe common divers uſually are) with the vulgar opinion about the non-gravitation of water and air in their natural places, do almoſt always refer * an experiment of my engine to ſuction, which is indeed the effect of the preſſure of the ambient (as I have † elſewhere clearly ſhewn) and affirm, that the pulp of the finger or hand is drawn up into a hollow pipe, into which it is indeed thruſt by the weight of the ambient air. But all theſe things I have mentioned, not as if I laid any great weight upon each of them, but to let you ſee that it was not altogether without cauſe that I complained of the incompetency of the hiſtory of what divers feel under water; eſpecially at great depths, where this want of information may be more conſiderable; for, as far as I have yet learned by peruſing voyages, and enquiring of travellers of my acquaintance, the places, where they are wont to dive for pearl, are but moderately deep, and indeed ſhallow, in compariſon of the great depths of the ſea; ſo that if we were furniſhed with as many relations of theſe profound places, as we have of the others, poſſibly the accounts would be different enough to render doubtful, or correct the received opinions about the conditions of divers at the bottom of the ſea. For, I remember, that a credible eye-witneſs, who, if I miſtake not, was the intelligent *Oviedo*, ſpeaking of the pearl-fiſhing on the American iſland of *Cubagua*, has, among many other notable obſervations, ſuch a paſſage as this: "But whereas the place is very deep, a man cannot "naturally reſt at the bottom, by reaſon of the abundance of airy ſubſtance, which is in "him, as I have oftentimes proved. For although he may by violence and force deſcend "to the bottom, yet are his feet lifted up again, ſo that he can continue no time there. "And therefore where the ſea is very deep, theſe Indian fiſhers uſe to tie two great ſtones "about them with a cord, on each ſide one, by the weight whereof they deſcend to the "bottom, and remain there, until them liſteth to riſe again, at which time they unlooſe "the ſtones, and riſe up at their pleaſure."

And now to come cloſer to the explication of our difficult problem; there yet occurs to me nothing more likely in order to it, than what I have already mentioned in the paper you will meet with about the differing preſſures, &c. And therefore it ſhall here ſuffice me to enlarge, and by further conſiderations and experiments confirm, what is there more ſummarily diſcourſed; namely, that the phænomenon may depend chiefly upon theſe two things, the uniform preſſure of the fluid ambient, and the robuſt texture of a human body expoſed to this preſſure.

In one of the following ‡ papers, you will find examples of the great preſſure that may be ſuſtained unharmed by ſuch frail bodies as eggs and thin glaſſes, that one would expect ſhould be broken in pieces thereby, provided the preſſure be exerciſed by the intervention of an ambient liquor, as water. And by the account, elſewhere referred to, of the tadpole, it ſeems highly probable that even that tender animal, when it ſeemed by ſome ſmall diminution of the bulk to be every way a little compreſſed inwards, was put to no conſiderable, or perhaps to any ſenſible pain or inconvenience, ſince it ſeemed to ſwim without any irregular motions, which would in likelihood have enſued, if it had been much harmed or incommodated. Which example, with thoſe formerly pointed at, may teach us, that there may be a vaſt difference betwixt the reſiſtance that a body can make, when compreſſed immediately by ſolid bodies, and when in the compreſſion every way

* The reaſon of which experiment may be gathered from the fourth Chapter of the Author's long ſince publiſhed Defence againſt *Linus*.

† In a paradox about ſuction.

‡ New experiments about the differing preſſure of heavy ſolids and fluids.

ambient fluids intervene. Which you will the less admire, if you consider, that by reason of the grossness, hardness, or rigidness of visible solid bodies, the pressure can never be made every where so equally, as by the parts of liquors, whose smallness, which renders them singly invisible, fits them to accommodate themselves far more closely and conveniently to all the superficial parts of the body immersed in them, and to have the force of the compressing body more uniformly distributed to them. But because the instances referred to are taken from bodies surrounded with water, I will take two or three about the resistance of bodies to violently compressed air; partly, because those made in our engine are wont to be performed with air, not condensed, but rarefied or expanded beyond its usual consistence; and partly, because it will not be denied, that the corpuscles of air may be really compressed or thrust against one another, since it is clear that they may be crouded into far less room than they possessed before, and bear so strongly against the glasses that imprison them, as not seldom, if too much compressed, to burst them in pieces.

Consider then, that among bodies not fluid, the swims of smaller fishes are likely to be judged none of the most able to resist compression, since they consist of bladders so thin and delicate, that a piece of fine Venice paper is very thick in comparison, and that they contain nothing in them but soft air, not compressed by any outward force. I caused one of these bladders, of above an inch in length, and proportionably great, to be taken out of a roach, and anointed it with oil to keep it supple, and preserve it from being pierced or softened by the water; and having by a weight of lead, fastened to the neck of it, let it down to the bottom of a hollow cylindrical tube, sealed at one end, and made purposely large, and about 56 inches long, for some hydrostatical experiments; we could not perceive, that by the weight of all the incumbent water it was manifestly compressed, or that it did discover the least wrinkle or other depression of the very thin membrane, though stuffed but with air. And this trial was made more than once with the same success; and yet, that this proceeded rather from the robustness of the bladder, that was able to resist the weight of a taller pillar of water, than from the non-gravitation of water in the upper part of the tube on that in the lower, we shewed, by presently letting down such a mercurial gage, as is described, and often mentioned in the Continuation of our New Experiments. For letting down this by a string to the bottom of a tube, the weight of the incumbent water forced up some of the mercury out of the open leg of the siphon into the sealed one, and consequently compressed the air included there, which, though it were not very much, yet it was very manifest. For the uncompressed air being three inches and $\frac{5}{8}$ in length, we judged it at the bottom of the tube about $\frac{5}{8}$ by the intrusion of the mercury that was impelled up; and to satisfy myself, and others, that if the incumbent water had been heavy enough, it would have visibly depressed the bladder in spite of any *principium hylarchicum*, since I could not have a tube long enough, the bladder was sunk into a crystal glass, that had a long and cylindrical neck, and was so well stuffed with a stopple, that was cylindrical too, that it was very difficult for any thing to get out betwixt it and the orifice of the glass; then a competent quantity of air being left above the water, the stopple was warily, and by degrees thrust down, and so lessening the capacity of the glass, compressed the air that was next it, and by the intervention of that, the water that was under it. And though there did not, upon a slight compression of the outward air, appear any sensible operation upon the bladder that was at the bottom of the water; yet, upon a farther intrusion of the stopple, the pressure being increased, the immersed bladder discovered not only one, but two considerably deep wrinkles, which presently disappeared upon the drawing up of the stopple. Upon whose being thrust in again, depressions were again to be seen on the swim. And we having

having been careful to convey into the fame glafs fuch a mercurial gage as has been lately fpoken of, we eftimated, by the condenfation of the air in the fealed leg of that gage, that the bladder had been expofed to a preffure, that might be equivalent to that of a pillar of about forty feet of water.

THIS, I hope, will leffen the wonder, that bodies of fo firm a texture as thofe of lufty men, fhould fupport the preffure of the water at fuch depths, as divers are wont to ftay at; fince we fee, what refiftance can be made by fo exceeding thin and delicate a membrane ftuffed only with air, in comparifon of the ftrong membranes and fibres of a man, ftuffed befides air, with more firm parts. I will not here urge, that great weights may be fuftained in the air by fuch tendons or cords of fibres, and by other fibres, as it were, interwoven into membranes, in comparifon of what an ordinary man would expect: but I fhall invite you to confider with me, that not only upon the account of the ftable parts of the human body, but of the fpirits too, it may refift very violent preffures (and fuch as perhaps have not yet been confidered) of a fluid body, not only without any manifeft contufion or diflocation of parts, but without any fenfe of pain; which I fuppofe you will grant me, if, confidering what great effects gufts of wind have upon doors, trees, nay mafts of fhips, blowing them down, nay breaking them; and that yet a man, without being extraordinary ftrong, will ftand againft the impetuofity of fuch a ftrong wind, and walk directly againft it, by virtue of the vigour of his mufcles and fpirits, without being thrown down, or bruifed by fo violent a current of air as beats upon him, but without fo much as complaining that he feels any pain; and this, though the wind that beats againft him, however it be a fluid body, yet becaufe it acts as a ftream, does not uniformly comprefs him, but invade only the fore-part of his body. Likewife, in the lifting up heavy weights by porters, carriers, and other lufty men, we may fee the flender tendons of the hands loaded with a hundred, or a hundred and fifty, or perhaps a far greater number of pounds, without having their fibres fo far compreffed or ftretched as to make the lifters complain of pain, though fometimes they may of difficulty. So that (as I could, if it were needful, confirm by other inftances) a human body is an engine of a much firmer ftructure than fcholars are wont to take notice of. And here let me add, that I doubt whether, if the ftructure of a man were not confiderably (though not perhaps equally) firm, he would, efpecially in a deep fea, be able to bear the preffure of the water, though not immediately applied, without pain. For (to give you one reafon more of my not acquiefcing in vulgar reports about diving) having feveral times converfed with a man, apt enough both to enquire and obferve, who got his living by taking up fhip-wrecked goods, he anfwered me, when I afked him, whether he felt any peculiar preffure againft the drums of his ears, which are membranes not fo well backed as thofe of other parts; that when he ftaid at a confiderable depth, as ten or twelve fathoms, under the furface of the fea, he felt a great pain in both his ears, which often put him to fhifts to leffen it; which, by his manner of defcribing it, I concluded was from the incompetent refiftance of the air, which he acknowledged to me, he found by manifeft tokens to be notably compreffed by the fuperior water. Which relation from fuch a perfon does not only confirm our explication, but likewife warrant us to doubt, whether the common reports that are made concerning divers be fit to be relied on, without further examen and obfervation.

IN the mean time, I fhall add two or three experiments more, to confirm the refiftance that animals may make to a great preffure, when exercifed by the mediation of a fluid body. And I the rather gave you an account of this way of making trials, becaufe it may be alfo helpful to difcover the refiftances of inanimate bodies, whofe fhape and confiftence we may choofe and vary, almoft at pleafure, to the preffure of (totally, or in

great

great part) ambient fluids. And if I had been furniſhed with a tube wide enough, and a quantity of mercury great enough, I might, by the way, have ſhewn you, that whatever the learned Dr. *More* is pleaſed to ſuppoſe, that to butter itſelf, even as conſiderable a preſſure may be ſo applied, as not to be able to make it yield thereunto. For on this occaſion I ſhall add, that I well remember, that, among other trials to the ſame purpoſe, I cauſed a piece of freſh butter, about the bigneſs of a ſmall hen-egg, to be brought to an irregular ſhape, that if the compreſſion were ſuch, as many would expect, the long corners, or ſolid angles, being at leaſt flatted, the butter might be reduced into a more capacious figure, and leſs remote from roundneſs. But, though having put this lump of butter into a bladder almoſt full of fair water, we proceeded, both in the ſame braſs cylinder, and much after the ſame manner, that I employed about the egg mentioned in the fourth experiment of the tract of the differing preſſure of heavy ſolids and fluids; yet I found, that after the plug had been loaded with a weight of lead of above fifty pounds, neither I, nor the operator, perceived the irregular figure of the butter to be altered. Nor was this the only trial of this kind I made with the like ſucceſs upon butter, though I dare not charge my memory with the circumſtances; and therefore I ſhall, without delay, proceed to what I was about to recite concerning the reſiſtance of animals.

We took then a common fleſh-fly, neither of the biggeſt ſort of all, nor of the leaſt, but of a middle ſize; and having put it into the ſhorter leg of a bent glaſs, which we cauſed to be hermetically ſealed at the end, there was put in as much mercury as filled that leg, and a part of the other, leaving little more than an inch of air between the quickſilver and the ſealed end, that there might be room both for the fly, and the condenſation of the air, and then with a little rammer, fitted for the purpoſe, we cauſed the mercury in the open leg to be thruſt againſt that in the ſealed leg, which thereupon did neceſſarily croud the air near the fly into leſs room; ſo that, by our gueſs, it was condenſed into about a third part of the ſpace, which it poſſeſſed before, and which it regained when the rammer was withdrawn; and though this were done more than once, yet not only the fly was thereby not killed, but not ſo much, that appeared as ſenſibly hurt; and I perceived her, whilſt ſhe was pent up, to move her legs, and to rub them one againſt the other, as it is uſual with that ſort of inſects to do of their own accord in the free air. Nor did I queſtion, but that, if the glaſs had not been inconveniently ſhaped to admit the rammer farther into it, the fly would have ſupported a far greater preſſure.

Another experiment, to the ſame purpoſe, we tried with water inſtead of mercury; but, whereas this laſt named liquor could neither wet nor drown our fly (for which reaſon I chiefly made choice of it) the other did firſt wet its wings, and ſoon after, by a miſchance, drown it. But firſt we had an opportunity to compreſs the air into a third, if not into a fourth part of its former dimenſions; and yet the fly continued to move divers of her parts, and eſpecially her legs, very vigorouſly, as if nothing troubled her, but her being, as it were, glued to the inſide of the glaſs by part of her wetted wings. And this, I hope, will keep the reſiſtance of divers to the ambient water from ſeeming incredible; ſince ſuch flies were able to reſiſt, and, for aught appeared, without harm or pain, the preſſure of the crouded particles of the air; though we gueſſed this to have been as much compreſſed by the force of the rammer, as it would have been by a cylinder of water of fifty, or between fifty and ſixty feet high. By which alſo we may be helped to conceive, how great a difference there is, whether the ſame preſſure be exerciſed by a ſolid, or by a fluid body. For, according to our eſtimate, the preſſure againſt the body of the fly was as great, as if a ſlender pillar of marble, having the fly for

for its baſe, and eighteen or twenty feet in height, had leaned upon the little animal; which, I preſume, you will eaſily think was more than enough to cruſh her to death.

But becauſe, though the foregoing trials are not like to be rejected by the ſkilful, yet they require a ſomewhat dextrous and nimble experimenter, and leave ſomething to his eſtimate, I will ſubjoin an experiment more eaſy to be made, and wherein the weight may be determined by meaſure, rather than conjecture, being made to be perpendicularly incumbent on the fly, or other animal. For the experiment may be as well made on other inſects, as worms, though ſome, that I had provided, chanced to miſcarry before they came to be uſed.

We took then ſome ordinary black flies (ſuch as uſe to haunt butchers ſtalls in warm ſeaſons) of a middle ſize (the length of the body and head of one animal, which for trial's ſake we meaſured, being about three eighths of an inch) and having placed one of them with the head upwards, that there was ſome diſtance left betwixt her and the ſealed end of the glaſs tube nine or ten inches long; we poured in quickſilver very ſlowly and cautiouſly, leſt the force of ſo heavy a body, acquired by the acceleration of its deſcent, ſhould, more than the mere weight itſelf of the liquor, oppreſs the fly. To this effect ſtooping the glaſs very much towards the horizon, and letting the mercury paſs into the tube thorough a funnel, whoſe lower part was very ſlender, that it might come down but by little and little, we at length got in as much mercury as the tube would receive, and then holding it upright, we watched, whether the fly would make any motions; and finding, that ſhe did manifeſtly ſtir notwithſtanding the incumbent mercury, we meaſured the height of the mercurial pillar, reaching from the middle of her body to the top of the liquor, and found it to be about eight inches; and the quickſilver being poured out, the fly appeared to be ſo lively and vigorous, that I doubted not, but if we had a longer glaſs, the experiment had been much more conſiderable. But, when afterwards I was able to procure a better tube, the ſeaſon of flies being almoſt quite paſt, I could ſcarce get any, and thoſe not briſk, as they are wont to be in ſummer. But however, we repeated the experiment with one of the beſt we could take of the above-mentioned ſize, and ordering the matter ſo, that the mercury incumbent on her (for there was ſome beneath her) appeared to be of a greater height than the formerly employed tube was of, we ſaw her move one or other of her little legs divers times, though the tube were held upright; and therefore meaſuring the height of the mercury above her, we found it to amount to ſixteen inches and better, and then freeing her from this preſſure, we obſerved, that ſhe immediately found her legs again, and moved up and down briſkly enough; but when ſhe was loaden with twenty-three or twenty-four inches of the ſame quickſilver (though the liquor were ſoon after poured out) ſhe gave no ſigns of life; which I ſuſpected might happen, not ſo much from her having been oppreſſed by the greatneſs of her weight, as from the great care of the operator to let down the mercury very obliquely and warily upon her. And this I was the rather confirmed in, becauſe, having got another fly of about the ſame bigneſs, though when ſhe was at the bottom of the quickſilver, ſhe ſeemed ſo compreſſed, as not to have any motion, we could take notice of, yet upon her being taken out of the glaſs, ſhe preſently appeared to be alive by walking about, and beginning to diſplay her wings, though the pillar of mercury, that had leaned upon her, amounted to above twenty-ſeven inches. And I preſume, the ſucceſs would have been much more conſiderable, if the experiment had been tried in the ſummer, when theſe creatures are briſk and lively, and not, as it was, in the winter; beſides that probably theſe little animals were hurt or weakened by the violence, that would ſcarce fail to be uſed in catching them, and puting them into a place and poſture in the glaſs, as was required; the actual

coldneſs

coldness of the quicksilver perhaps also making them somewhat torpid, whilst it touched them so many ways. And it must not be here omitted, that a fly, that seemed but about half so big as one of those hitherto mentioned, being well placed, with some mercury under it, in a glass pipe held upright, sustained a mercurial pillar of somewhat above twenty-five inches; and though she was not observed to move under so great a weight, yet when once it was taken off, she did not appear hurt, much less crushed to death by it, and probably would have escaped under a much greater weight, if the tube, which was too large, had not already employed all the stock of mercury we then had at hand. But I do presume, that what we did try, will be available to our purpose, since we see clearly, that so small an animal as a fly may survive so great a pressure, and that she could not only live, but was able to move such long and slender bodies as her legs, when she was pressed against by above sixteen inches of mercury, and consequently by a weight, equivalent to a pillar of water of above eighteen feet and a half, which being above five hundred and ninety times her own length, and, according to the estimate our measure suggested, many times more her own height; so that a diver six feet tall (which is somewhat more than an ordinary man's stature) to have as many times his height of water above him, as our fly might have had, and yet have moved under it, must dive, at least in fresh water, to near a hundred fathom, which is a far greater depth, perhaps by five or six times, than, for aught I could learn by enquiry, the divers either for coral or pearl are wont to descend.

And now, Sir, having tendered you the likeliest conjectures, that occurred to me about the solution of this difficult problem; I shall return to doctor *More*, and consider the objection he frames from the supposed insolubleness of it. And on this occasion, I shall have two or three things to represent to you.

The first is, that there would be much more weight in what he objects, if our assertion of the gravitation of water in water were, like the *principium hylarchicum*, a mere hypothesis advanced, without any clear positive proof; whereas our doctrine is not only elsewhere directly proved by particular experiments, but by the very controverted one of the tadpole; to elude whose force, so ingenious a person is fain to fly to a principle, that, to say here no more, is not physical. And from this first of the things I lately mentioned, I shall hasten to the second, because it will require to be longer insisted on.

I shall then further represent, that whatever power he is pleased to suppose at the bottom of the sea, to suspend the impression of the incumbent water, I think, that supposition ought to give place, if not to our former ratiocinations, yet to experience itself, which shews, there really is a great pressure exercised by the water at the bottom of the sea. I remember that a friend of the learned doctor's and mine, who is so eminent a virtuoso, as to have been often president of the royal society, related a while since to me, that a mathematical friend of his, whom he named, having had an opportunity to try an experiment, I have in vain endeavoured to get tried for me, had the curiosity to let down in a deep sea a pewter-bottle, with weight enough to sink it, that he might try, whether any sweet water would strain in at the orifice or any other part; but when he had pulled it up again, he was much surprized to find the sides of his pewter-bottle very much compressed, and, as it were, squeezed inward by the water. I also, not long since, enquired of an observing acquaintance of mine, that has a considerable estate in *America*, whether he had not tried to cool his drink, when he sailed through the torrid zone, by letting down the bottles to a great depth into the sea, and if he did, in what condition he found them when they were drawn up again. To which he answered, that he had several times employed that expedient for the refrigeration of his drinks, but was at first amazed to find the corks, with which the strong stone-bottles had

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had been well ſtopped before, ſo forcibly and ſo far thruſt in, that they could ſcarce have been ſo violently beaten in with a hammer, and it was ſcarce poſſible to get them out. And another ingenious perſon, that practiſes phyſick in the *Indies*, having the like queſtion put to him, anſwered me, that he had ſome while ſince had the curioſity to try, in a very deep part of the ſea, whether any freſh water would ſtrain into ſtone-bottles through a thick cork ſtrongly ſtopped in, and having let it down with a convenient weight to one hundred fathom, was much diſappointed, when he drew it up, by finding, that the preſſure of the water at ſo vaſt a depth had quite thruſt down the cork into the cavity of the bottle (which elſe perhaps would have been cruſhed to pieces); an effect, which he would ſcarce have expected from the ſtrokes of a mallet. And if to all this it be objected, that it was not the preſſure, but the coldneſs of the water, that did the recited feats, by condenſing the included air, and obliging nature to do the reſt for fear of a vacuum; I will not launch into the controverſy, whether nature do any thing *ob fugam vacui*, but only anſwer, that I cannot find, by the relations of the divers, or otherwiſe, that it is ever ſo cold at the bottom of the ſea, as it is frequently above ground in winter, when great fiſhes are commonly ſaid to return to the deep parts of the ſea for warmth; and yet, in the ſharpeſt winters, I never obſerved corks to be driven in by the cold of the ambient; nay, I purpoſely tried with a frigorifick mixture, that very intenſe degrees of cold, ſuch as would quickly freeze many liquors, would not occaſion the breaking of thin bubbles of glaſs, purpoſely blown at the flame of a lamp and hermetically ſealed.

And to ſhew *ad oculum* (as they ſpeak) that water may preſs more and more, as it grows deeper, againſt the ſtopple of a bottle, though the veſſel be inverted, I will ſubjoin this experiment. Becauſe we have no water hereabouts, that is near deep enough to force in a cork, as the ſea-water did in the above recited trials, I thought of a way of ſo cloſing the glaſs veſſel, as that the ſtopple ſhould keep aſunder the air in the veſſel and the outward water, and hinder all immediate intercourſe between them, and alſo make ſome reſiſtance againſt the preſſure of the external water, and yet be capable of freely moving up and down, and ſo be a good ſuccedaneum to a ſolid ſtopple. Taking then a glaſs phial, furniſhed with a ſomewhat long cylindrical neck, whoſe cavity was large in proportion to the reſt of the veſſel, we put into it as much quickſilver as would in the neck make a ſhort mercurial pillar of between half an inch and an inch; then a piece of very fine bladder, dipped in oil, was ſo tied over the orifice of the glaſs, that no mercury could fall down, or get out, nor water get in at the orifice, and yet the bladder, by reaſon of its great limberneſs, might be eaſily thruſt up towards the cavity of the phial, or depreſſed by the weight of the mercury. This little inſtrument, firſt furniſhed with a weight of lead to ſink it, being inverted, the mercury deſcended into the neck, and cloſed the orifice as exactly as a ſtopple, and yet, with its lower part, depreſſed the bladder beneath the horizontal plane, that might be conceived to paſs by the orifice; then the glaſs being a while kept in the water, that the included air might be brought to the temperature of the ſurrounding liquor, and by a ſtring let further down into the ſame glaſs veſſel to about two feet in height, the preſſure of the liquor againſt the orifice of the phial did by degrees drive up the bladder and the mercurial ſtopple into the cavity of the neck, as was manifeſt by the aſcenſion of the quickſilver; and when the inſtrument was leiſurely drawn up again, the weight of this mercury made it ſubſide and plump up the bladder again as before. An experiment a-kin to this, and therefore fit to confirm it, I have delivered in another diſcourſe *.

* See the Paradox about ſuction.

And here I ſhall ſubjoin what very opportunely occurred to me ſince the writing of the laſt page. Meeting caſually with an ingenious mechanician (whom you will find I have elſewhere † mentioned) that deviſed a ſuit of cloaths and other accommodations, wherein I once ſaw him let down into the water, by whoſe help, and that of a boat, he could, and did continue there a great while, at a conſiderable depth under water, and there work; I aſked him afreſh (to obtain fuller informations than formerly) whether he felt not the preſſure of the water againſt his breaſt and belly; to which he anſwered me (more circumſtantially than he had before) that when he was about four or five yards under water, though but in the river *Thames*, his breaſt and abdomen were ſo compreſſed, that there being hardly room enough left for the free motion of his lungs, he could ſcarce fetch his breath, and was neceſſitated to make them draw him quickly up, and that, among his later trials to improve his engine, having for remedy hereof cauſed a kind of armour for the cheſt and back to be made of copper, though the ſtiffneſs of the metal defended him from receiving any miſchief in thoſe parts, yet in the others, where only the leather, though ſtrong, was interpoſed, when he came to the depth of about ſix fathom, though in freſh water, he found a great preſſure againſt his legs and arms and all the other parts againſt which the water was able to thruſt the leathern ſuit inwards. And this preſſure being found by him, as he told me, pretty equal, againſt all the expoſed parts (for from the other, which were more yielding and obnoxious, the armour kept it off) he received no miſchief from it, nor yet much incommodity (and ſome he might expect from the ſtiffneſs and unequal yielding of the leather): ſo that he could ſtay under water, though not ſtill at ſo great a depth, about two hours or longer. And upon the whole matter he anſwered me, that he was well ſatisfied by his trials, that the ambient water endeavoured to preſs him and his diving ſuit every way inwards. Whether the coldneſs of the water had any intereſt in this phænomenon, I particularly enquired of the engineer; but he replied, that by reaſon of the tightneſs of his diving ſuit or inſtrument, the warm ſteams of his body, that were pent in, and other concurring circumſtances, kept him from feeling any cold, and made him ſometimes feel a greater heat than he wiſhed. He has promiſed me, before it be very long, to make for me a trial or two, that I propounded to him, from whoſe ſucceſs, if he can but reduce them to experiment, I hope to be able to preſent you a farther confirmation of our hypotheſis. In the mean time, the things already recited, together with the preceding experiments, may well ſuffice for our preſent purpoſe. For, by what hath been ſaid, it appears, that water does actually preſs againſt bodies, whether ſpecifically lighter or heavier than itſelf, placed under water, and that this preſſure increaſes with the height of the water above the emerſed bodies. And this being ſo, it is not more neceſſary for me, than for men of other opinions, to give a clear reaſon, why divers can reſiſt ſo great a preſſure of the incumbent water. And the preſſure of the water in our recited experiment having manifeſt effects upon inanimate bodies, which are not capable of prepoſſeſſions, or giving us partial informations, will have much more weight with unprejudiced perſons, than the ſuſpicious, and ſometimes diſagreeing accounts of ignorant divers, whom prejudicate opinions may much ſway, and whoſe very ſenſations, as thoſe of other vulgar men, may be influenced by prediſpoſitions, and ſo many other circumſtances, that they may eaſily give occaſion to miſtakes. I know, that learned men, that never were converſant in hydroſtaticks, are wont to think it very difficult, if not impoſſible, to conceive, how ſo weak a thing, as they fancy an animal to be, ſhould avoid the being oppreſſed, or ſo much as harmed by ſo great a weight

† In the tract of the differing preſſure of heavy ſolids and fluids.

of

of water. But they that shall attentively consider what has been offered towards the removal of this difficulty, and remember how little they would have believed, that there is so great a difference, as we have by the tadpole, the fly, and other instances shewn there really is between the pressure of solid and of fluid bodies, will, I presume, be apt to think it fit, that if, for want of a sufficient history of matters of fact, any scruple remain about the solution we have offered from the nature of the uniform pressure of fluids, and the firm structure of the human body, we should, to remove those remaining scruples also, rather range about for other physical helps to solve more completely the problem, about such a thing as compression, which is an action purely corporeal and mechanical, than for want of a ready and complete solution to fly to the immediate interposition of an immaterial and intelligent, yet created agent, to explain clearly whose manner of working would be a much more difficult task, than the solution of the phænomenon without it.

And now, Sir, having presented to you the reflections I thought requisite to write upon the learned doctor's discourses against my hypothesis and explications, relating to the gravitation and pressure of fluids, I have little more to trouble you with in this paper. For though in the latter part of the thirteenth chapter the doctor is pleased to spend divers pages in the explication of divers of my hydrostatical phænomena by the agency of that incorporeal director, that he calls *principium hylarchicum*; yet since these explications of his are rather attempts to accommodate the phænomena to the hypothesis, than objections directly levelled against my solutions, I shall altogether forbear to examine them; the main thing that I intended in this paper, according to what I told at the beginning, being to shew that the arguments urged against the mechanical solutions of the experiments by me recited, do not evince any of them to be erroneous. And I have neither the design nor the leisure solicitously to examine the doctor's hylarchical principle. Of which I shall only say, that though he tells us, it is *paratum ad movendum quoquo versum materiam pro data occasione*; yet since he also tells us, *Quò particulæ molis corporeæ, sive stabilis sive fluidæ, à principio hylarchico in unam aliquam partem omnes junctim urgeri possunt & premi, quamvis singulæ singulas in nullam partem premant, quodque pro magnitudine molis major minorve totius sit pressio*; and that the force, by which it endeavours to keep the elements in their true and natural consistence, though it be very great, is not invincible: I see no need we have to fly to it, since such mechanical affections of matter, as the spring and weight of the air, the gravity and fluidity of the water, and other liquors, may suffice to produce and account for the phænomena, without recourse to an incorporeal creature, which it is like the Peripateticks, and divers other philosophers, may think less qualified for the province assigned it, than their fuga vacui, whereto they ascribe an unlimited power to execute its functions. I leave it therefore to you, Sir, to judge, which of the two ways of explicating an hydrostatical phænomenon, the learned doctor's, or that which I have made use of, relishes most of the naturalist. And I shall only tell you, that if I had been with those Jesuits, that are said to have presented the first watch to the king of *China*, who took it to be a living creature, I should have thought I had fairly accounted for it, if, by the shape, size, motion, &c. of the spring-wheels, balance, and other parts of the watch I had shewn, that an engine of such a structure would necessarily mark the hours, though I could not have brought an argument to convince the Chinese monarch, that it was not endowed with life. From which comparison you will easily gather, that what I have thought myself concerned to do in this place, was not to demonstrate in general, that there can be no such thing, as the learned doctor's *principium hylarchicum*, but only to intimate, that, whether there be or not, our hydrostaticks do not need it. Nor do I think it necessary

Page 175.
Page 167.
Ibid.

to the doctor's grand and laudable design, wherein I heartily wish him much success of proving the existence of an incorporeal substance. For, as I think, truth ought to be pleaded for only by truth; so I take that which the doctor contends for, to be evincible in the rightest way of proceeding by a person of far less learning than he, without introducing any precarious principle; especially experience having shewn, that the generality of heathen philosophers were convinced of the being of a divine architect of the world, by the contemplation of so vast and admirably contrived a fabrick, wherein, yet taking no notice of an immaterial *principium hylarchicum*, they believed things to be managed in a mere physical way, according to the general laws, settled among things corporeal, acting upon one another. And after this I have nothing more to say, but that I would not have any thing that I have said, misconstrued to the learned doctor's prejudice. For it is not necessary, that a great scholar should be a good hydrostatician. And a few hallucinations about a subject, to which the greatest clerks have been generally such strangers, may warrant us to dissent from his opinion, without obliging us to be enemies to his reputation. And therefore, if you have found any thing in this paper inconsistent with a just tenderness of that, you have not only my consent, but my desire to alter it, as an expression that doth not well comply with my intentions of not appearing any farther his adversary in our debate, than the desire of shewing myself a friend to the truth I was to defend, should exact of,

SIR,

Yours, &c.

A N

AN HYDROSTATICAL LETTER,

Written *February* 13, 167$\frac{2}{3}$.

CONTAINING

A Dilucidation of an Experiment of the Honourable Author of theſe TRACTS, about a Way of weighing Water in Water, upon the Occaſion of ſome Exceptions made to it by Mr. *George Sinclair* *.

* In his Hydroſtaticks, printed at *Edinburgh*, 1672, p. 146. ff.

To the READER.

WHEN this diſcourſe was juſt finiſhing in the preſs, there came to the publiſher's hands a dilucidation of an experiment of the honourable author of theſe tracts, about a contrivance of his for eſtimating the weight of water in water, formerly publiſhed in Number L. of the *Philoſophical Tranſactions*, and by the following diſcourſe cleared from the exceptions to be met with in Mr. *George Sinclair*'s book, entitled *The Hydroſticks*, &c. printed at *Edinburgh*, 1672. Which dilucidation, becauſe of the affinity of the ſubject, was thought fit to be here annexed.

AN

AN

HYDROSTATICAL LETTER, &c.

SIR,

CALLING this night in *Paul's Church-yard* for the ingenious Mr. *Ray's Travels*, that you yesterday commended to me, I was also shewn a new treatise, that I never saw before, of a learned gentleman, and hastily running over the index, found an experiment of mine declared insufficient; and though, being hindered to make haste home, it be so late, that, far from having time to peruse the book itself (which I tell you, that you may not now expect any character of it from me) I have been scarce able to read over, more than once, what directly concerns me in it; yet I shall adventure to say something about it this night, for fear I should not, in so busy a time as this, be allowed to do it to-morrow.

WHEREAS then the learned objector, having recited my experiment about weighing water in water, as you were pleased to publish it in a book enriched with so many better things, the *Philosophical Transactions*, begins his animadversions with saying, that "herein is a great mistake;" I shall not in that much oppose him; for possibly the dispute between us is not much more than verbal. And because my experiment coming abroad by itself, and supposing thing, that I had formerly proved, and published, but which were not expresly referred to in it, I wonder not, that my meaning should not by all readers be fully understood. And therefore, to explain myself on this occasion, give me leave both to repeat my opinion, and to shew you on what occasion, and how far, I designed to confirm it by this experiment. My opinion then was, and still is, that as water is a heavy fluid, so it does retain its gravitation and power of depressing; by which I mean a tendency downwards (whatever the cause of that gravity be) whether it have under it a body either specifically heavier or lighter than itself, or equiponderant to it. For I see not what should destroy or abolish this gravity, though many things may hinder some effects of it. And therefore I suppose, that water retains its gravity not only in air, but in water too, and in heavier liquors, and consequently, by virtue of this, the liquor presses upon them; but if a surrounded fluid have, upon the score of its specifick gravity, an equal, or a stronger tendency downwards, than water, it will, by virtue of that, be able to impel up this liquor, or to keep it from actually descending: so that a portion of water, supposed to be included in a vessel of the same specifick weight with water, this portion, I say, placed in a greater quantity of the same water, will neither rise nor fall, as I have elsewhere shewn; but yet it retains its gravity there; only this gravity is kept from making it actually descend by the contrary action of the other water, whose specifick gravity is supposed equal: as when a just balance is loaded with a pound weight in each of its scales, though neither of the weights actually descend, being hindered by its counterpoise, yet each retains its whole weight, and with it presses the scale it leans upon; so that our lately mentioned included portion of water does really press the subjacent water, though it does not actually depress

Numb. L.

depreſs it, or (as pethaps a ſchool-man would phraſe it) does gravitate on it, but not pregravitate. Nor do I think, that the only way of judging, whether a body gravitates, is to obſerve, whether it actually deſcends, ſince in many caſes its gravity may be proved by the reſiſtance it makes to heavy bodies, which if it were not one, would raiſe it: as may be declared by what I juſt now noted about equal weights in a balance. And for want of this diſtinction I have known even learned men, treating of hydroſtatical things, miſtake both me and the queſtion.

THE next thing I had to tell you, is, that the adverſaries I had to deal with, both in print, and in diſcourſe, denied, that in ſtanding water, the upper parts did preſs or gravitate upon the lower; and though they could not but grant, that the whole weight of the water did gravitate upon the bottom of the veſſel; yet they would have the parts of it to do ſo *actione communi* (as they ſpeak) and fancied I know not what power of nature to keep the homogeneous portions of water, as well as other elements, from preſſing one another, when it is in its proper place. Againſt this opinion (which I preſume my learned adverſary and I agree in oppoſing) it was alledged, beſides other things, which I found many, otherwiſe good ſcholars, were not fitted to underſtand, that if a glaſs phial or bottle, well ſtopped, were deeply immerſed under water, it would ſtrongly tend upwards; but if it were dextrouſly unſtopped, when it was thus immerſed, ſo as the water could get in, abſtracting from or allowing for the weight of the glaſs itſelf, it would by the water, that crouds in and thruſts out the air, be made ſtrongly to tend downwards, and continue ſunk. But this not ſatisfying, becauſe it was pretended, that the reaſon of the empty bottle's emerging, when ſtopped, was the poſitive levity of the air it was filled with, and the ſinking of it, when unſtopped, was from the receſs of the ſame air, that by the intruding water was driven with large bubbles out of the bottle; I thought this evaſion might be obviated by contriving an experiment, wherein the water ſhould be plentifully and ſuddenly admitted into the glaſs, and yet no air expelled out of it (which circumſtance I therefore took notice of, where I ſay, "no bubble of "air appeared to emerge or eſcape through the water") ſo that if then the glaſs, that was kept up before, ſhould fall to the bottom, with a gravtation amounting to a conſiderable weight in reſpect of its capacity, the ſinking of it could not by them be aſcribed, as they ſuppoſe, with poſitive levity, but to the weight of the admitted water, which, when thus weighed, would be invironed with water of the ſame kind: and to ſhew, that this admitted water might have a conſiderable weight, notwithſtanding the place it was in, I employed a pair of ſcales after the manner that is recited in the experiment.

BY what I have been diſcourſing, you may conceive, that however my expreſſions diſagree with thoſe of my adverſary, the diſtance of our opinions is not ſo wide, as at firſt ſight it ſeems. For he allows, as well as I, that the ſuperior parts of water do by their gravity (for I know not on what other ſcore they can do it) preſs the inferior. But this he would not have amount to this expreſſion, "that water weighs or gravi-"tates in water;" whereas I ſcruple not to cloath my ſenſe in that expreſſion, becauſe I think water does always exerciſe its gravity, though it does not always pregravitate, or actually deſcend, being often, as I noted above, either impelled up by an oppoſite and prepollent weight, or hindered from deſcending by the reſiſtance of other water, that counterpoiſes it: ſo that, if he thinks, that in my experiment I meant to propoſe a method of making water deſcend in water, and weigh it in that liquor with a pair of ſcales, juſt as if I would weigh in the ſame water a piece of lead, or a portion of mercury, which are bodies much heavier in ſpecie than water, either he miſtakes my intention, or I did not ſufficiently declare it. But that which I deſigned to ſhew, and, for

for aught I can yet see, have shewn, was, that by the help of an ordinary balance it may be made appear, that water admitted into the glass-bubble, I employed, did make the glass-bubble weigh so much heavier than it did before that liquor entered into it; and that this new weight, that was manifested by the balance, was not due, as my adversary supposed, to such a recess of the air, as I mentioned a while ago.

And now, Sir, it will be proper to take notice of some passages in the objector's discourse, in order to dilucidate the subject of it. Whereas he says (page 149, and 150.) "Take a piece of wood, that is lighter in specie than water, and add weight to "it by degrees, till it become of the same weight with water; knit it with a string to "a balance, and weigh it in water; and you will find the whole weight supported by "the water:" I answer, that this does not at all overthrow my opinion, but agrees very well with it. For suppose the weight you add to the light wood be lead, it cannot be said, that the metal loses its native ponderosity, whilst it rests in the water; and the reason, why it descends not, is, that it and the wood it is joined to, are hindered by the counterpoise of the collateral water, which, by its pressure, would raise the surface of the water, whereon the floating or swimming body leans, if it were not hindered by the weight of these incumbent solids: and this resistance of theirs to the endeavour upwards of the water, being exercised only upon the account of their gravity, shews, that they do in my sense gravitate, though not pregravitate.

Again, if you please to consider the case put by the objector, page 151, and cast your eyes upon his scheme, which, supposing you to have his book, I shall, for brevity

Page 151. sake, make use of at present; you will find him thus argue: "Now, I say, it is six "ounces of the weight (*B*) that makes this alteration, and turns the scales: for if twelve "ounces sink the glass below the water, when it is full of air, and no water in it, "then surely six are sufficient to sink it, when it is half full. And the reason is, be"cause there is a less potentia, or force, in six inches of air, by the one half, to "counterpoise a weight of twelve ounces, than in twelve inches of air. Therefore "this air being reduced from twelve inches to six, it must take only six ounces to "sink it."

To which I answer, that I know not yet what, on this occasion, he means by a potentia, or force, in six inches of air, to counterpoise a weight of twelve ounces. For by the term counterpoise, where the question is about weighing, one would think he speaks of weight; and yet air, according to the vulgar opinion, is positively light; according to us, though it have a gravity, yet in our case that must amount to so little, that what air the bubble needed to fill it, could not weigh at most above four or five grains, which therefore might safely be neglected. But, according to my opinion, the reason of the phænomenon is clear enough, without meddling with the potentia of the air. For if we conceive a horizontal plane to divide the water mentally, and pass by the bottom of the suspended bubble; before the little stem be taken off, there is a far greater pressure upon the other parts of that plane, than upon that which lies under the bubble, in regard they are pressed by the weight of the collateral water (*A*, *L*, *G*, *D*, *M*, *C*,) whereas the other is pressed only by the weight of a body very much lighter than its equal bulk of water: so that, to keep the bubble from being forcibly buoyed up, there was requisite eighteen ounces of lead, that make up the plummet (*B*,) to detain it under water, and keep the beam of the balance horizontal; that when access is given (at *C*) to the neighbouring water, it is by the weight of the collaterally superior water impelled into the cavity of the bubble, where the air, being much rarefied before, could not resist its ingress, and thereupon, six ounces of water getting in, that part of of the imaginary plane, on which the bubble was incumbent, is pressed by a greater weight

weight than formerly by six ounces, and consequently, there needs the like weight in the opposite scale of the balance, to reduce the scale to an æquilibrium. And if we suppose, with our author, the glass to be completely full of water, and the counterpoise in the scale (*O*) to need six ounces more to make a new æquipondium, the account of the phænomenon will be the same, as, if you attentively consider it, you will clearly perceive. And the reason why the additional weight of six ounces is required, will be, that the upper half of the bubble, that before contained less than three or four grains weight of air, being now filled with water, amounted to six ounces more of water than formerly, and so the counterpoise, in the opposite scale (*O*) will need the weight of six ounces to make a new æquipondium.

Congruously to this explication, when the examiner says, "Now I enquire, whether Page 152.
"these eighteen ounces are the æquipondium of the water within the glass, or of the "weight of the lead (*B*)? It is impossible they can counterpoise both, seeing the water "is now twelve, and *B* eighteen. It must then either be the counterbalance of the water, or the counterbalance of the lead. It cannot be the first, because twelve cannot "be in æquipondio with eighteen; it must then be in the second: or if these eighteen "ounces in the scale (*O*) be the counterpoise of the water within the glass, I enquire "what sustains the weight of the lead (*B*)? the weight of it cannot be sustained by the "water, because it is a body naturally heavier than water, it must therefore be sustained "by the balance." I answer, that this specious objection seems (for it is somewhat obscurely worded) to be founded upon a mistake of my meaning in the question. However, as to the phænomenon itself; according to my sense, the eighteen ounces in the scale (*O*) are the counterpoise of the eighteen ounces, that hang from the opposite and æquidistant scale, and make up the leaden plummet (*B*) which answer, I see not how our author prevents. But then you will ask, what counterpoises the water in the bubble, which alone weighs twelve ounces? I answer, that it is the gravitation of the collateral water, which presses the other parts of the lately-mentioned imaginary plane, as much as the water in the bubble, the weight of the glass being here not reckoned by either of us; and the water incumbent on the bubble does press that part of the plane on which they lean; so that there being in all thirty ounces to be sustained, the eighteen of the plummet, and the twelve contained in the glass, the lead, that hangs in the water, is counterpoised by eighteen ounces in the scale, and the water in the bubble by the pressure of the collateral water.

But you will say, that it appears not, that the included water presses at all, since it does not at all descend. To which I answer, that as long as the water was getting into the cavity of the bubble, so long it did manifestly gravitate upon the subjacent plane, and actually descend, raising the counterpoise in the scale: but when, by adding more weight to that counterpoise, things are brought to a new æquilibrium, there is no reason, why the gravitation of the water should again change the now regained æquipondium. Suppose, in the two scales of a balance there were placed two equally capacious and equiponderant phials, whereof one is quite full, and the other almost full; it is evident, that the full vessel will keep the scale it leaned upon depressed, and if you gently pour in as much water into the unfilled, as the filled has more than it, the scale, that was formerly kept raised, will be now depressed, till the beam be brought to be horizontal; to which posture when it is once brought, the æquilibrium will continue: and yet it will not be said, that though the added water, whilst it was filling the glass, depressed the scale it belonged to, yet it lost its weight, or, which in my sense is all one, did not gravitate upon the scale, when the balance was come to an æquilibrium, because then this water did no longer depress it. And how much the water in our bubble does, notwithstanding its immersion, gravitate, would be visible, if, by supposition, it were all annihilated, and no other suffered to supply its room. For then the subjacent part of the imaginary plane being much less pressed, than immediately before, the weight of the collaterally superior water would strongly impel up the bubble, if it were not kept in its place by a proportionable addition of weight to the plummet. Nor should it seem a strange thing, that I should say, that the thirty ounces, lately mentioned, should be counterbalanced partly by the weight in the opposite scale, and partly by the water that fills the immersed bubble; since this notion may be warranted even by the common practice of weighing heavy solids hydrostatically. For if you would, for instance, weigh a lump of copper of nine pounds in common water, the metal, hanging by a horse-hair under water, will need, according to my elsewhere mentioned experiments, either just or near about eight pounds in the opposite scale, to keep the balance horizontal, so that the whole nine pounds, that the lump weighed in the air, is counterpoised partly by the eight pounds newly mentioned in the opposite scale, and partly by the weight, or resistance following from weight, of as much of the water as the copper fills the room of; which, as experience shews, is one pound: and if we should conceive water in a vessel adiaphorous, as to gravity and levity, to be substituted in the place of the metalline lump, it would weigh as much as the ninth part of the copper lump weighed in the air, and the same counterpoise of eight pounds would maintain the æquilibrium.

What the learned objector has, at the close of his discourse about the natural and artificial balance, could not without prolixity, and is not here necessary to be dwelt upon; especially since you will see, in what I suppose you have now received from the press, in answer to the ingenious Dr. *More*, what is to be said on that subject, according to my hypothesis. Wherefore; though my learned adversary does in the 152d page conclude, "That water cannot weigh in water," and asserts "that the pressure of "water is one thing, and water to weigh in water is another;" yet, as I said at first, I conceive much of our difference may be verbal; and, in my sense, when water presses subjacent water, because it does so upon the score of its gravity, it gravitates in water, though it does not pregravitate, that is, actually descend. And since it is in the sense of this last expression, that our author, if I mistake him not, speaks of weighing in water, his conclusion, that water cannot weigh in water, does not contradict me, who affirm not, that water does so weigh in water. Whether we shall agree in all other points of Hydrostaticks, you will easily believe, that I cannot yet tell; though by the expression he is pleased to use in the 146th page, to usher in his objection with, it is probable we may. And as to the now-dispatched debate, if I have employed some words in another sense than he, I presume he is so equitable as to consider, that I did not write of these things after having seen this book of his, but some years before; and have since found those expressions justified by the use that eminent writers have thought fit to make of them. And however I am glad, that he has given me this opportunity of clearing my experiment, and declaring by examples, as well as words, the opinion it relates to; especially, if it seems to others, that I omitted to express myself so fully; my design being, as I formerly told you, to convince such adversaries, as I then had met with, by shewing, that the above-recited phænomena of the emersion and sinking of a glass phial depended upon the gravity of the water, and not upon the positive levity of the air.

NEW

NEW EXPERIMENTS

Of the POSITIVE or RELATIVE

LEVITY OF BODIES UNDER WATER.

IT is obvious even to the vulgar, as well as to philoſophers, that if wood, wax, or another body that is lighter in ſpecie than water, and naturally floats upon it, be detained under water, it will, upon removal of that force, emerge to the top. And this it does ſo readily, and, as it ſeems, ſpontaneouſly, that not only the Peripatetick ſchools, but the generality of philoſophers, both ancient and modern, do, as well as the vulgar, aſcribe this aſcenſion of lighter bodies in water to an internal principle, which they therefore call poſitive levity.

BUT this principle was not always ſo univerſally received among philoſophers, as in later ages it proved to be; *Democritus*, and ſeveral of the ancients, both atomiſts and others, admitting no abſolute, but only a relative or reſpective levity; which opinion ſome of the moderns have ingeniouſly attempted to revive.

BUT, becauſe whatever wit they may have employed in arguing, yet the ſchools ſeem to have the advantage in point of experience, the obvious inſtances given by the Peripateticks having neither been ſolved by real and practical variations of the ſame inſtances, nor counterbalanced by new experiments of a contrary tendency; the importance and difficulty of the ſubject invited me to attempt, when I was upon hydroſtatical trials, whether I could experimentally ſhew, that whatever becomes of the general queſtion about poſitive levity, we need not admit it for the true and adequate cauſe of the emerſion of wood, and ſuch lighter bodies, let go under water.

EXPERIMENT I.

THE inſtance that is wont to be urged to prove the poſitive levity of wood in water, ſeems to me to have been too perfunctorily made to be ſafely acquieſced in. For even as it is propoſed with advantage by a learned foreign mathematician, I cannot think it accurate enough to determine the preſent controverſy: for I will readily allow him to ſuppoſe, that in caſe a flat board, as for inſtance, a trencher, have its broad ſurface kept by a man's hand or other competent force upon the horizontal bottom of a tub full of water, if the hand or other body that detained it be removed, it will ordinarily happen, that the trencher will haſtily aſcend to the ſurface of the water. But I do not perceive, that a deciſive experiment of this kind is eaſy (not to ſay poſſible) to be made with such materials. For the wood, whereof both the trencher and the bottom of the barrel consists, are supposed to be lighter in specie than water; and to be so, they must be of a porous and not very close texture. To which agrees very well, that the solider woods, as lignum vitæ, Brasil, &c. whose texture is more close and compact, will not float on water, but sink in it: and therefore, if there be not much more care used, than I have yet heard that any experimenter has employed, to bring the surfaces of the trencher, and the bottom of the barrel, to a true flatness, and as much smoothness, as they can be brought to, I shall not think the trial so accurately made, as it might be; not to say, which I suspect, that though it be mentally, yet it is scarce practically possible to bring such porous bodies, as those of the lighter woods, to be fit for such a contact as might be necessary to make the trial accurately. And in case that were actually done, I should be kept from expecting, with my adversaries, the emersion of the trencher, by the experiment by and by to be recited, and by the true reason of it.

I THINK then that the cause why, in ordinary instances, wood, wax, and other bodies specifically lighter than water, being let go at the bottom of a vessel full of that liquor, emerge to the top, is chiefly, that there is no such exquisite congruity and contact between the lowermost superficies of the wood, and the upper surface of the bottom of the vessel, but that the lateral parts of the water, being impelled by the weight of the parts of the same liquor incumbent on them, are made to insinuate and get between the lower parts of the wood and the bottom of the vessel, and so lift or thrust upwards the wood, which bulk for bulk is less heavy than the water that extrudes it.

THAT this is the reason of the emersion or ascension of bodies, lighter in specie than the fluids they swim in, is most consonant to the laws of * Hydrostaticks, as I have elsewhere shown. But whereas the whole force of the argument of those I dispute with, consists in a supposition, that, because the trencher (formerly spoken of) is placed upon the bottom of the barrel, no water can come between to buoy it up, whence they conclude, it must ascend by an internal and positive principle of levity, I thought fit to make the experiment after another, and, if I mistake not, a better manner.

WE took then two round plates of black marble shaped like cheeses, which had those superficies that were to be clapped together ground very flat, and polished very carefully, that the stones being laid one upon the other, might touch in as many of the superficial parts, as the workman could bring them to do; that, whilst they were in that position, the uppermost being taken up, the other would stick to it, and ascend with it. And to keep out the water the better, the internal surfaces were, before they were put together, lightly, and but very lightly, oiled; which did not hinder them from most easily sliding along one another, either forward or backwards, or to the right, or to the left, as long as the contiguous surfaces were kept horizontal.

THESE things being done, a blown bladder, of a moderate size, was fastened to the upper marble, and both of them were let down to the bottom of a tub of water, where, by the help of an easy contrivance, the lower marble was kept level to the horizon. And now the patrons of positive levity would have concluded, that the bladder, being a body, granted to be by vast odds lighter than wood, and being in an unnatural place beneath the surface of the water, should, of its own accord, and with impetuosity, emerge; but I expected a contrary event, because the bladder being tied to the upper marble, so that both of them might in our case be considered as one body, the water could not impel them up, in regard that the close contact of the surfaces of the two marbles kept

* See the Hydrostatical Paradoxes.

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the water from being able to infinuate itfelf between them, and confequently from getting underneath the upper marble, and preffing againft the lower fuperficies of it. And to fhew that this was the reafon of the bladder's not emerging, I caufed one of the byftanders to thruft his arm down to the bottom of the tub, and with his hand to make part of the oiled furface of the upper marble flide off, on any fide, from that of the lower, which, by reafon of the fmoothnefs and flipperinefs of the furfaces, he found moft eafy to do. But the contact ftill continuing according to a greater part of the furfaces than was requifite, I bid him yet flide, but by flow degrees, more and more of the upper marbles from the lower, till at length, when, according to his guefs, the marbles touched but in one half of their furfaces, the endeavour of the water to extrude the bladder full of air being ftronger than the refiftance, which the contact, but of part of the furfaces of the ftones, was able to make, they were fuddenly disjoined, and the bladder was by the extruding water impetuoufly, as it were, fhot up, not only to the top of the water, but a good way beyond it.

WITH thefe marbles we made feveral other experiments of this kind, moft commonly letting down the marbles both together; but once or twice at leaft placing the upper marble under water upon the lowermoft already fixed to the bottom of the barrel.

THAT it was not the weight of the upper marble, nor want of lightnefs, whether pofitive or relative, of the air included in the bladder, that kept it from afcending, was plain; not only upon the newly-mentioned impetuous emerfion of it, upon the disjoining of the marbles, but by this, that the bladder would lift up from the lower parts of the water, not only the upper ftone, when it touched not the other, but a weight of feven or eight pounds hanging at it.

AND that a *fuga vacui* was not an adequate caufe of the cohefion of the marbles in our experiment, may be argued from this, that whether or no nature do any thing, at any time, out of abhorrence of a vacuum (which may be much difputed); yet, in our cafe, this abhorrency could not be well pleaded by its affertors, fince many of them hold it to be unlimited, and the more modeft, to be at leaft capable of lifting up prodigious weights; whereas, in our experiment, the levity of a bladder, that could not raife ten pounds weight, was fufficient to disjoin the marbles, when they yet touched one another according to half their furfaces.

EXPERIMENT II.

To fhew now, whether it is not rather the gravity and preffure of the water, or other ambient fluid, than the pofitive levity of a lighter body in fpecie than it, that makes the immerfed body afcend to the furface of the liquor, I devifed this experiment:

WE took a bladder, out of which a great part of the included air had been expreffed, and tying the neck of it very clofe, that none of the remaining air might get out, we faftened to it a confiderable weight of fome very ponderous body, as lead or iron. By the help of this we funk the bladder to the bottom of a wide mouthed glafs, full of water, that the furface of the liquor might be a good deal higher than the upper part of the bladder: this wide mouthed glafs we included in a great receiver (whofe orifice muft be very large to be able to admit fuch a veffel) which I caufed to be carefully cemented on to the engine. The main fcope of this experiment was to fhew, that though the air included in the bladder was very far from being able, by its abfolute levity, to lift up fo great a weight, as the bladder was clogged with, yet the fame air, continually included in the bladder, would, by its mere expanfion, without any new

external

external heat, acquire a power of afcending in fpight of that weight; which afcenfion therefore muft be attributed to the water, which, according to the laws hydroftatical, ought, *cæteris paribus*, to refift, or buoy up more potently thofe immerfed bodies, that being lighter in fpecie, than it, poffefs the greateft place in it, and hinder the more water from acquiring its due fituation: as we fee, that among hollow fpheres of gla and metal, equally thick and well ftopped, there is a much heavier weight requifite to fink a large one than a fmall one. For the profecution of this trial, we began to pump the air out of the great receiver; and its preffure upon the furface of the water being thereby more and more leffened, (according to what we elfewhere more fully declare) the fpring of the included air began by degrees to diftend the fides of the bladder, till at length that veffel of air fwelling every way, took up fo much more room in the water than it did before, that the water was able to lift the bladder and the annexed weight to the top, and detain it there, till we thought fit to let in again fome of the excluded air, which forcing that in the bladder to fhrink in its dimenfions, the weight was prefently able to fink it to the bottom.

And here it may be noted, that if, inftead of hanging fo great a weight at the neck of the bladder, we faftened but a moderately heavy piece of lead, fuch as would only ferve to fink the bladder, and keep it at the bottom of the water, fo that the aggregate of the bladder, air, and metal, was but a little heavier than a bulk of water equal to them; then, upon the firft fuck or operation of the pump, which could withdraw but a fmall part of the air in the receiver, the air in the bladder fuddenly expanding itfelf, would forthwith be impetuoufly extruded by the water, though after fome reciprocations it would float in its due pofition, till upon the return of a little outward air, fometimes as little as we could conveniently let in, it would immediately fubfide.

But this is not fo neceffary to be infifted on, as it is to take notice, that I forefaw it may be objected, that the afcenfion of the weight was not effected by the preffure of the water, but by this, that rarity and levity being qualities exceedingly of kin, the great rarefaction of the air might proportionably increafe the levity of it, and confequently enable it to perform much greater things than it could do before.

I will not here difpute, whether, generally fpeaking, a body rarefied without heat would, in vacuo, or in a fluid not heavier in fpecie, than the body when rarefied, merely, by fuch a greater diftance of its parts as may fuffice to entitle it to rarefaction, become really heavier or lighter than before. I will not, I fay, difcufs this queftion here, where it may ferve my turn to fatisfy the recited objection by the following experiment.

EXPERIMENT III.

About the neck of a conveniently fhaped phial capable to hold fome few ounces of water, I caufed to be carefully tied the neck of a fmall bladder, whence the air had been diligently expreffed, fo that the bladder, being very limber of itfelf, and probably made more fo, as well as more impervious to air and water, by the fine oil we had caufed it to be rubbed with, lay upon the orifice of the phial like a fkin clapped together with many folds and wrinkles.

This done, we let down the phial into a conveniently fhaped veffel full of water, and the phial, being poifed beforehand for that purpofe, funk perpendicularly in the liquor, till the neck of the glafs was partly above and partly beneath the furface of the water: then covering the external glafs with a large receiver, we caufed the air to be pumped out, and as the preffure of that was gradually withdrawn, the air in the float-

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ing phial did little by little expand itself into the bladder, and unfolded the wrinkles of it, till at length it became full blown, without altering the erected posture of the glass it leaned upon. But this great expansion being made above the water, and consequently in a medium not heavier than the included air, gave that highly rarefied air no such increase of levity, as enable us to perceive that it made so much as the neck of the glass arise higher in the water than it did before. Nor did we take notice, that the return of the air into the receiver, by reducing the air in the bladder to its former unrarefied estate, made the glass sink deeper than before. But when the experiment was tried with the same glass and bladder, at the bottom of the water, then, upon the pumping out the air, the bladder being dilated under water, was, after a while, carried up to the top, and took up with it about eight or ten ounces, that had been to clog it, fastened to the bottom of the phial.

NEW EXPERIMENTS

About the PRESSURE of the

AIR's SPRING

ON

BODIES UNDER WATER.

I DO not think it were difficult for an intelligent peruser of our physico-mechanical experiments, to find there divers phænomena, whence it may be deduced that bodies under water, though kept by that liquor from the immediate contact of the air, may yet be exposed to its pressure, whether the air act as having a weight, or as a spring. But because not only the vulgar, but philosophers, have been so long and generally possessed with an opinion, that a fluid so little heavy as the air, cannot by its weight act upon a liquor, that is, like water, bulk for bulk, a thousand times heavier than it; and because also it seems yet more strange, that a little air, perhaps not amounting to a scruple or drachm in weight, should in its ordinary state of laxity act considerably upon bodies, which, being covered with water, seem, by the interposition of that liquor, to be fenced from the incumbent air; it may be worth while to add three or four hydrostatical experiments, to confirm a truth that very few are yet acquainted with; and add to the proofs already given of the power of the spring of the air, some of the operations we have discovered it to have upon bodies placed under water.

THERE are two sorts of trials that I shall employ to shew, that a small quantity of inclosed air may, by its pressure (which in our cases must depend upon its spring) have a con-a confiderable operation upon bodies under water, notwithftanding the interpofition of that liquor.

For this preffure we fpeak of may be manifefted, in the firft place, by what it directly and pofitively operates upon bodies covered with water; and, in the next place, by the things that regularly enfue upon the removal of the inclofed air, or the weakening of its fpring.

EXPERIMENT I.

To begin with the former way of fhewing the preffure of the air, I thought it fufficient, in regard of the trials to be referred to the fecond way, to make the following experiment.

We took a fquare glafs phial, gueffed to be capable of holding between half a pint and a pint of water; the neck of this we luted on carefully and ftrongly (for elfe it would have been buoyed up) over the orifice of the fmall pipe, at which the air paffes in our engine out of the receiver into the pump; then whelming over this glafs a great receiver, we luted it ftrongly to the engine (that it might as well keep in the water as keep out the air) and at the top poured in as much water as fufficed to inviron the internal receiver (if I may fo call it) and cover it to a pretty height. This done, we exactly clofed with a turning-key the hole in the great receiver, at which the water had been poured in, that no air might get in or out that way. And laftly, we began to pump out the air contained in the internal receiver; to the end that that air, which by the above-mentioned pipe had communication with the external air, might no longer by its preffure affift the glafs to refift the preffure, which the incumbent and inclofed air, by virtue of its fpring, conftantly exercifes upon the fubjacent water, and by its intervention upon the fides and bottom of the internal receiver.

And as we expected not, that this glafs by its own fingle force, fhould refift the preffure of the air inclofed in the upper part of the great receiver, notwithftanding the interpofition of the water, fo the event fully juftified our conjecture; for at the firft exfuction, which could not be fuppofed to have well emptied the internal glafs, this veffel was, by the preffure of the fuperior air upon the circumftant water, broken into I know not how many pieces. And the fame experiment, though with a little flower fuccefs, was repeated with a ftronger internal glafs.

EXPERIMENT II.

I proceed now to the fecond way of manifefting the preffure of inclofed air upon bodies under water, which is by fhewing the phænomena exhibited by thofe bodies upon the removal or leffening of that preffure.

Having fqueezed out of a moderately fized bladder the greateft part of its air, we tied the neck of it very clofe, and then faftening to it a competent weight, we placed it at the bottom of the talleft and largeft glafs we could cover with our great receiver, that fo, though the incumbent air were pumped out, none of the water might be pumped out with it, but ftill retain the fame height above the bladder. Having then poured upon the bladder as much water as would fwim a great way above the upper part of it, we covered this glafs of water with a great receiver, which being carefully cemented on to to the engine, the pump was set a work, and as the air, which by its spring pressed upon the surface of the included water, was by degrees pumped out, so the air that was imprisoned in the bladder, did gradually expand itself at the bottom of the water, as if no such liquor had interposed between them otherwise than by its weight, upon whose account it must be allowed to give some little impediment to the expansion of the bladder, in proportion to the height it had above it.

The event of our experiment was such as was expected, namely, that the immersed bladder was at length full blown, by the dilatation of the air inclosed in it; and by its intumescence made a considerable part of the water run over by the sides of the glass that before contained it all. And when access was given again to the external air, the internal being compressed, the bladder was presently reduced to its wrinkled state.

EXPERIMENT III.

We took a small but fine bladder, whose neck was strongly tied up, when it was, by guess, about half full of air; this we put into a short brass cylinder, the lower of whose bases was closed with a brass-plate, and the other left open; this open orifice we afterwards stopped, but not exactly, with a cylindrical plug, that was somewhat less wide than it, and was by a rim at the top hindered from reaching too deep into the cavity of the cylinder, that it might not do mischief to the bladder that lay there beneath it; upon this plug we placed an almost conically shaped weight of lead, and this pile of several things being so placed upon our engine, that we could cover it with a great receiver, we carefully cemented on this vessel, and at the top of it poured in so much water as would serve to fill the vacant part of the brass cylinder, and the cavity of the engine to such a height, that it covered all the leaden weight, which was several inches high, except a rim, which was fastened to the top of it for the convenienter removing of it.

All this being done, the pump was set to work, and long before we had exhausted the air of the receiver, that, which was inclosed in the lank bladder, had by degrees displayed so vigorous a spring, that it had heaved up the weight that lay upon it to a notable height, and kept it there till the air was let in from without, to assist its being depressed by the leaden weight, which amounted to no less than about 28 pounds.

EXPERIMENT IV.

There remained yet one trial to be made, which, in case it should succeed, seemed likely to appear as great an evidence of the force of the air's spring upon bodies under water, as could be reasonably desired of us; it having been looked upon by many virtuosi, as the considerablest instance of the force of the air's spring, even when no water intervened in the trial.

To satisfy, therefore, our curiosity, we took a copper vessel of a cylindrical shape, and a considerable height; into this, being first almost filled with water, we put a square glass-phial, capable, by guess, to hold nine or ten ounces of water, and exactly stopped with a cork and a close cement; this phial, by a competent weight, was detained at the bottom of the water, from whose upper surface it was considerably distant; then the copper vessel being placed upon the engine, and included in a great receiver well cemented on, the air was by degrees pumped out; but before it was quite exhausted, the glass at the bottom of the water was, by the spring of the air included in it, burst into many pieces, not without great noise, and a kind of smoke or mist that appeared above the surface of the water.

Another glass of the same sort had been broken after the same manner in another vessel; but having afforded us no particular phænomenon, I barely mention it, to shew that we made more than one trial of this kind.

The consequence that will naturally result from the three last experiments, is this, that since barely upon the withdrawing of the pressure of the included air (which was perhaps but very little in quantity) the air residing in the immersed bodies did, by virtue of its spring, expand itself so forcibly as we have recited, and perform notable things, the air above the water must have exercised a very powerful pressure upon the surface of it, since (setting aside the weight of the water, of small moment in our trials) it must have been, at least, equivalent to (and probably much exceeded) that force of the immersed air, whose exercise it was able totally to hinder.

And from hence it may be easily deduced, that the weight of the atmosphere acts upon bodies under water, notwithstanding that the interposed liquor is, by vast odds, heavier in specie than air; for we have just now proved the pressure of inclosed air (which consists in its spring) upon bodies under water; and it is manifest, that the strength of the spring of this inferior air, we make our trials with, is caused by the weight of the superior air, which bends and compresses those little aërial springy particles, whereof our air consists; so that the weight of the atmosphere being equivalent to the spring of the inferior air (for else it could not compress it as much as it does) must lean upon the surface of the subjacent water, with a force equivalent to the spring of that part of it that is contiguous to the water.

This experiment brings into my mind another, that I once made, which, though not properly hydrostatical, yet relating to positive levity, may perhaps be not uselesly added on this occasion; wherefore I shall here subjoin a transcript of the phænomenon that belongs to our present purpose, as it is registered soon after the experiment was made.

[To examine, by a visible experiment, the common doctrine, that a portion of air, by being much dilated, rarefied, or expanded, does acquire a new and proportionable degree of positive levity, I devised to put in practice the following way:

We took a bladder of a moderate size that was very fine and limber, that it might be the lighter and more easily distended. The most part of the air being squeezed out of the bladder, the neck of it was tied up very close, that no air might get out of it, nor any external air get into it. This limber bladder was hung at one of the scales of a balance, whose beam had been purposely made more than ordinarily short, that the instrument (which yet was ticklish enough) might be suspended, and capable of playing in the cavity of a great receiver, into which we conveyed it, having first carefully counterpoised the bladder with a metalline weight put into the opposite scale.

This done, the air was pumped out, and, as that was withdrawn, the bladder was more and more expanded by the spring of the internal air, till at length, when the receiver was well exhausted, it appeared to be quite full. Notwithstanding which great dilatation of the included air, it did not appear by the depression of the opposite scale, to be grown manifestly lighter than it was at first. And the bladder seemed also to retain the same weight, after it had, by the air that was let into the receiver, been compressed into its former wrinkled state.]

NEW

NEW EXPERIMENTS

ABOUT THE

DIFFERING PRESSURE

OF HEAVY

SOLIDS AND FLUIDS.

SINCE not only in vulgar ſpectators of phyſico-mechanical experiments, but even among ſome learned men, it has proved a great impediment to men's freely acquieſcing in the doctrine founded on thoſe phænomena, that if the atmoſphere could really exerciſe ſo great a preſſure, as we aſcribe to it, it would unavoidably oppreſs and cruſh all the bodies expoſed to it, and conſequently neither other animals nor men would be able to move under ſo great a load, or ſubſiſt in ſpite of ſo forcible a compreſſion.

THIS I readily grant to be a plauſible objection; but I ſuppoſe the force of it will be taken away by the following conſiderations put together.

AND firſt, the power of preſſing, that we aſcribe to the air, is not a thing deduced, as too many other conſequences in phyſick are, from doubtful ſuppoſitions or bare hypotheſes, but from real and ſenſible experiments. And therefore ſince we have clear and poſitive proofs of the preſſure of the air, though we could not explain how men and other animals are not deſtroyed by it; yet we ought rather to acknowledge our ignorance in a doubtful problem, than deny what experience manifeſts to be a truth; as is generally practiſed in treating of the attractive and other powers of the loadſtone, which are freely acknowledged, even by thoſe that confeſs themſelves unable to explicate them; though, if experience did not ſatisfy us of them, they were liable to divers more conſiderable objections than any that is urged againſt the preſſure of the air.

SECONDLY, but though it be not abſolutely neceſſary that we ſhould anſwer the above-recited objection otherwiſe, than by thus declaring that the ſpring of the air is not to be rejected for it; yet we will endeavour very much to leſſen it, if not quite remove the difficulty, before we put an end to the diſcourſe.

I CONSIDER then, thirdly, that they that urge the lately mentioned objection againſt the great preſſure of the air, ſeem not to be aware that we were conceived and born in places expoſed to the preſſure of the atmoſphere, and therefore how great ſoever that preſſure appeared to be, it ought not to cruſh us now, ſince when we were but embryos, or new-born babes, and conſequently very much more weak and tender than we now are, we were able to reſiſt it, and not only live, but grow in all dimenſions in ſpite of it.

If there were any place about the moon, or some other of the cœlestial globes that some learned men fancy to be inhabited, that has no atmosphere or equivalent fluid about it, and where yet men could be generated a new, if one of those men should be supposed to be transported thence, and set down upon our earth, there might be made an experiment fitted for our controversy. In the mean time, I doubt, that since nature is not observed to make things superfluously strong, such a human body being not made to resist any weight or pressure of air, would be of so tender and compressible a make, that it would easily be crushed inwards by our atmospherical pressure. And though we cannot give an instance of this kind, yet we make trials somewhat analogous to it in our pneumatical engine. For when we place water in our receiver, and pump out the air that was above it, there will be generated a multitude of bubbles, some of which, when the air is carefully withdrawn, will be of a strange and scarce credible bigness; these bubbles being generated where the air cannot press upon them, these dimensions are so natural to them, that if the receiver be supposed not to leak, nor other unfriendly accidents to intervene, they would (for aught we know) last a good while; since I have elsewhere shewn, that the spring of highly dilated air did continue for many months, and a bladder would for no less time continue blown and filled in our vacuum by a little air that was left in it, when the ambient air began to be withdrawn from it. And yet the large bubbles above-mentioned, when once the outward air is suffered to come in upon them, are thereby so violently compressed, that in a trice they shrink into dimensions too small to keep them so much as visible; and if I could have succeeded in my attempt of producing such living bodies as I endeavoured (but did not expect) in our vacuum, I suppose the success would have confirmed what I have been saying.

Fourthly, but you will tell me, that so great a weight and pressure, as I assign the atmosphere, must needs make a man feel pain, and, if not, otherwise dislocate some of the parts, must, at least, press the whole body inward.

But, first, being accustomed to the pressure from our very birth, and even before it, so early and long an accustomance hinders us from taking notice of it; those pressures only being sensible to us, that are made so by some additional cause, which, by making a new impression, excites us to take notice of it. So we are not sensible of the weight of the cloaths we are accustomed to wear; and so a healthy man is not sensible of the heat in his heart, because it is constant there, and the sentient parts of the heart have been still used to it, whereas that heat oftentimes has been very considerable; and when in living dissections a man puts his finger into the heart of an animal, which probably has a fainter, or at least no stronger degree of heat than a human heart, he will feel in his fingers, accustomed to the air, a manifest degree of heat, if they be but in their usual temper. 2. I have elsewhere proved by experiments, that a cubick inch of air, for instance, has as strong a spring, as suffices to enable it to resist the weight of the whole atmosphere, as far as it is exposed thereunto; for else it would be more compressed than *de facto* it is. And, 3. I have also shewn, that a very little portion of air, though it will much sooner lose its spring by expansion than a greater, yet it will resist further compression as much as a greater. And, 4. I have also shewn, that in the pores of the parts of animals, whether fluid or consistent, as in their blood, galls, urines, hearts, livers, &c. there are included a multitude of aërial corpuscles, as may appear by the numerous bubbles afforded by such liquors, and the swelling or expansion of the consistent parts in our exhausted receiver. 5. To this we may add, that, besides the bones, whose solidity is not questioned, a much greater part of the human body, than is wont to be imagined, does really consist of membranes and fibres, and the coalitions and contextures of these; and

that

that these substances are, by the providence of the most wise Author of things, made of a much closer and stronger texture, than those, that have not tried, will be apt to think; as I could make probable by the great force that bladders will endure, and the very great weight that tendons of no great thickness will lift up or sustain, and by other things, that I shall not now insist on. Lastly, there is a far greater difference than men are wont to suspect, between the effects of the pressures made upon bodies by incumbent, or otherwise applied solid weights, and those that they suffer from heavy, but every way ambient fluids; as will appear by the experiments to be mentioned by and by.

From the particulars contained in these considerations, we may be assisted to shew, why it is not necessary, that the pressure of the atmosphere, though as great as we suppose it, should oppress and crush the bodies of men, that live under it; for the solidity of the bones, and the strong texture of the membranes and fibres, and the spring of the aërial particles that abound in the softer, as well as in the fluid parts of bodies, is equivalent to the pressure of as much of the atmosphere as can exercise its pressure against them, and makes the frame of a human body so firm, that it may well resist the pressure of the outward air, without having any part violently dislocated, whilst the external pressure is exercised but by the air, which being but an invironing fluid, presses it equally (as to sense) on every side. And because our bodies have been produced in the atmosphere, and from our very birth exposed, without intermission, to the pressure of it; our continual accustomance to this pressure, and the firmness of their structure, keep us from being sensible of the weight or pressure. And that it was not impertinent for me to mention the firmness of the frame of our bodies on this occasion, I shall manifest by an instance, that will upon another account also be proper for this place.

We know, that multitudes of men have had occasion to pass over high mountains; and besides, that I have been myself upon the *Alps* and *Appennines*, I have enquired of travellers that have visited the Asian and American mountains, and some, that have been upon the top of the Pick of *Teneriffe* itself; but though divers of them took notice of a great difference in the air at the top and bottom, as to some other quality, as coldness and thinness; yet I never met with, nor heard of any that took notice of a difference, as to the weight of air he sustained, or that complained that when he was come down to the foot of the mountain, he felt any greater compression from the air, than at the top. And yet the experiments made, as well by others as by ourselves, sufficiently witness, that on more elevated parts of the earth, which have a less height of the atmosphere incumbent on them, the weight and pressure of the air is not so great as below. And on very high mountains, it is not unlikely that this difference may be very considerable, since, when the Torricellian experiment was made near *Clermont* in *France*, upon the *Puy de Domme* (which is none of the highest mountains in the world, being found, by the ingenious makers of that observation, to be but about 500 fathoms) they found the difference of the mercury, at the top and bottom, to amount to about three inches; and consequently, if the trial had been made with water instead of quicksilver, the difference would have been about three feet and a half in the perpendicular height of the water. And it is very probable, that in much higher mountains, the difference of the mercurial cylinders height, at the top and bottom, may be much greater; and at the bottom of some very deep well or mineral groove, which may, without improbability, be supposed to be placed at, or near the foot of one of these mountains, if we conceive the baroscope to be let down, the variation of the height of the mercurial cylinder may be yet much more considerable; and yet we find not, that the diggers in the deepest mines, in mountainous countries, are sensible of being leaned on or compressed by any unusual weight. But not here to build on any thing but matter of fact, it appears by the newly-named.

named obfervation, that, when a man was at the bottom of the hill, he had as much greater weight of air leaning upon his head, than he had at the top, as was equal to the height of an imaginary veffel full of water, which having his head for bafis, were three feet and a half high; which is fo confiderable a weight as could not but have been, not only fenfible, but very troublefome and uneafy to fupport. And what has been faid of the gravity of a pail of water, that leaned on his head, may be proportionably applied to his fhoulders, arms, &c.

Whence I think I may infer, that the reafon why fuch a weight was not felt by the man it compreffed, was not, that the air that preffed him, was not confiderable, but that the preffure was exercifed after the uniform manner of fluid bodies.

And this may fuffice to fhew, that there is no neceffity that the compreffion of the atmofphere fhould make it impoffible to live in it. But becaufe it is obferved, that thofe that dive to great depths under water, are not oppreffed by the great weight of the incumbent water, and the caufe of this ftrange phænomenon is not fo eafy to be affigned, and therefore has been made one of the two grand arguments whereon the non-gravitation of water in water, and air in air, has been, and ftill remains founded: I fhall here offer fomething *ex abundanti* towards the folution of that noble and difficult problem.

And, firft, that what is obferved by the divers does not evince that water does not weigh in water, I have elfewhere * proved by fuch reafons and experiments, as had the good fortune to convince eminently learned men, that were fufficiently prepoffeffed with the vulgar opinion; and in the fame treatife I have given a clear account, why a bucket full of water is not felt confiderably heavy, whilft it is under water, in comparifon of what it is whilft it is drawn up into the air; which is the other phænomenon, that I frefhly intimated the common opinion to be founded on.

Next, I do not think it ftrange, that that follows not, which it is objected fhould follow from our hypothefis; namely, that a diver fhould be violently depreffed to the bottom of the water, by the weight of fo great a pillar of the fea as is placed perpendicularly over his body. For if we imagine a plane fo to cut the fea-water, as to pafs by the diver's body; then as that part of the plane on which his body leans will be preffed by it, together with the water that is perpendicularly incumbent on it; fo all the other parts of the fame plane will be preffed by equally tall pillars of water perpendicularly incumbent on them; and confequently, if the man's body were juft equiponderant to an equal bulk of water, it and the water, that leans on it, would be fuftained by the preffure of the collateral water incumbent on the other parts of the fame plane (as may be eafily underftood by what I have elfewhere † faid). And therefore there is no reafon why the divers bodies fhould be more forcibly depreffed than its depreffion is refifted. It is true, that this body will fink, but that is becaufe it is not only, as we lately fuppofed it, æquiponderant to an equal bulk of water, but heavier than that. But then, fince the water, by its gravity and refiftance takes off as much of the weight of the diver's body, whilft that is immerfed, as a quantity of water equal to it would weigh in the air, the fubfiding of the human body by its own weight ought to be but flow, becaufe that being not in fpecie much heavier than water, it can fink but by virtue of the furplufage of weight that it has above water. And, in effect, I have been informed by fwimmers, that in the fea, whofe water, by reafon of the faltnefs, is fpecifically heavier than the common water, they could hardly dive when they had a mind; the falt-water did fo much fupport them. And having, becaufe I had no conveniencies to make

* See the Hydroftatical Paradoxes. † See Appendixes to the Hydroftatical Paradoxes.

trials

trials upon the parts of human bodies, examined the weight of parts of other animals in air and water, I found the overplus of the weight of the animal substances above an equal bulk of water to be but very small. And this may suffice to take off the wonder, why, though water may be admitted to gravitate in water, yet divers are not depressed by that which leans upon them; the endeavour they use to keep themselves from sinking, by striking the resisting water with their arms and legs, easily compensating their weak tendency downwards, which the small surplusage of gravity above-mentioned gives them.

But it seems to me far more difficult to render a reason, why those that are a hundred feet beneath the surface of the sea, are not crushed inwards, *especially in their chests and* abdomens, or at least so compressed as to endure a very great pain.

To clear up or lessen this difficulty, I have two things to offer.

1. I confess, that I am not entirely satisfied about the matter of fact; for I do not yet know, whether it fares alike with the divers in all depths under water; for, according to the answers I obtained from persons, that had been, one of them at the coral-fishing in the *Streights*, and the other at the pearl-fishing near *Manar*, I do not find that the divers are wont to descend to the greatest depths of the sea, which if they did, perhaps they would find a notable difference.

And in small, or but moderate depths, those that dive without engines usually make such haste, or are so confounded, or have their minds so intent upon their work, that they take not notice of such lesser alterations, as else they might observe, especially they being persons void of curiosity and skill to make such observations. Which I the rather mention, because having met with a learned physician, that living by the sea-side in a hot climate, delighted himself much in diving; and enquiring of him, whether he felt no compression, when he passed out of the air into the water, he answered me, that when he dived nimbly as others use to do, he took not notice of it, but when he let himself sink leisurely into the water, he was sensible of an unusual pressure against his thorax, which he several times observed.

A man that gets his living by fetching up goods out of wrecked ships, complained to me, that if with his diving-bell he went very deep into the sea, and made some stay there, he found himself much incommodated; which though he imputed to the coldness of the water, yet by the symptoms he related, I was inclined to suspect that the pressure of it upon the genus nervosum might have an interest in the troublesome effect. And I have been assured by an eminent virtuoso of my acquaintance, that he was lately informed by a person, whose profession it is to fetch up things from the bottom of the sea by the help of a diving-bell, that several times, when he descended to a great depth under the surface of the water, he was so compressed by it, that the blood was squeezed out at his nose and eyes; which relation seems to favour our conjecture, and would much more confirm it, if I were sure that the effect was no way caused by some fermentation or other commotion in the blood itself, occasioned by the great density, or other alterations of the air he breathed in and out, or by some other operation of the ambient medium distinguishable from the compression of the water, though perhaps conjoined with it.

And on this occasion I remember, that questioning an engineer, who had made use of an engine to go under water, quite differing from the diving-bell; he answered me, that when he came to a considerable depth, he found the pressure so great against the leathern case, wherein he descended, and by that means against his belly and thorax, that he feared it would have spoiled him, which forced him to make haste up again. But this observation, to have much built upon it, should be further enquired into.

These

THESE things, and not these only, make me wish that what is felt by those that dive to great depths, and stay at them, might be more heedfully observed by intelligent men, that, being fully informed, what is true in point of fact, we may the better and more cheerfully indagate the reasons.

IN the mean while, taking things as they are thought to appear, I shall propose two things towards the solution of our difficulty; namely, the firmness of the structure of a human body, and the uniformity of the pressure made by fluids.

OF the first of these I shall add but little to what has been already said, where I spoke of the resistance made by our bodies to the compression of the atmosphere; only shall here take notice, that whereas the membranes are very thin parts, and therefore seem unfit to make any great resistance; we have tried, that if a piece of fine bladder were fastened to the orifice of a brass-pipe, of about an inch in diameter, we could not, by drawing the air from beneath it, make the weight of the atmosphere break the bladder, though the weight were perhaps equivalent to an erected cylinder of water, of the wideness of the orifice, and about thirty feet high, and were indeed such, that divers men that laid their hands on the orifice, when the air was pumped out from beneath, complained, that they were not able to lift off their hands again, till some of the air was re-admitted.

BUT the main thing I shall propose, towards the solving of the difficulty we are considering, is the uniformity wherewith fluid bodies press upon the solid ones that are placed in them. And because I remember not to have met with experiments purposely made to shew, how this sort of pressure is more easy to be resisted than that of solids against solids, I shall subjoin the following trials.

EXPERIMENT I.

IN the short cylinder of brass above-mentioned, we put a fine bladder tied so close at the neck, that none of the air (whereof it was about half full) could probably get out. Which we did, to the end that the hen-egg, we were to bed in it, might lie soft, and have its sides almost covered with the limber and flaccid bladder and contained air; this done, we covered the remaining part of the egg with another bladder, that nothing that was hard might come to bear immediately upon the shell; then we put the wooden plug into the cylinder, and a weight upon the plug, which is to be done very slowly and warily, lest the quick descent of the weight should make the plug break the egg it leans on. Lastly, the cylinder thus fitted, being covered with a large receiver, and the air being drawn out, that air, which was tied up in the bladders, by degrees expanded itself so strongly, as to lift up the plug and the incumbent weight to a pretty height, and keep it there, till the external air was re-admitted.

Now since it will be readily granted, and appears by divers experiments elsewhere related, that the air in such cases expands itself vigorously every way, it appears by the recited trial, that it pressed against the egg with the same force that it pressed proportionably against the bottom of the plug, and that force was more than sufficient to lift up the weight, which (together with the plug) amounted to about thirty pounds, and yet the egg being taken out, appeared perfectly whole and no way harmed; whereas, upon the same egg, if I mistake not, or at least another of the same kind, laying warily a while after small weights, one upon another, the egg was crushed to pieces by about four pounds weight. This experiment, though it seemed considerable to those that saw it, and may prevent an objection, for which reason I here mention it; yet will appear in no way ſtrange to them, that conſider that the weight of the atmoſphere which the egg ſupported before it was put into the cylinder, was more than equivalent to ſuch a preſſure of the air, as may ſuffice to lift up the plug; wherefore I thought fit to make further trials of a differing nature.

EXPERIMENT II.

We took a glaſs-bubble of about an inch and half in diameter, which we cauſed to be blown at the flame of a lamp, that it might be far more thin and eaſy to break than the thinneſt phials that are wont to be blown in the glaſſer's furnaces. This bubble we included between bladders, as we did the egg in the former experiment; and then having warily put the plug into the cylinder, ſo as it might preſs upon the bladder that invironed the glaſs, we leiſurely put the weights upon the plug, till they, together with the plug, amounted to thirty pounds or more, which being removed, the plug was taken out, and the glaſs-bubble, though it were extraordinarily thin, perhaps no thinner than fine white paper, was taken out whole.

EXPERIMENT III.

But leſt the great reſiſtance of ſo thin a glaſs, which yet was not hermetically ſealed, ſhould be aſcribed to the ſphericalneſs of its figure, we employed, inſtead of it, the ſhell of an egg, whence by a hole, made at one end of it, the yolk and white had been taken out. This empty and imperfectly cloſed ſhell we handled, as we did the glaſs-bubble in the former experiment; and, notwithſtanding the great leaden weight, that leaned by the intervention of the plug upon the ſoft body that environed it, it was taken out, not only uncruſhed together, but, for aught we could perceive, without the leaſt crack.

EXPERIMENT IV.

And to ſhew, that what we obſerved about the nature of the compreſſion of fluid bodies will hold as well in water as air, though it ſeemed difficult to make the trial with the accommodations we then had, we thought upon the following expedient.

Into a limber bladder, almoſt full of water, we put a hen egg, and tying the neck very ſtrait, that nothing might get in or out, we ſo placed the bladder in the braſs cylinder, that the egg might not be immediately touched by any thing that was hard; then putting the plug into the cylinder, we warily and leiſurely heaped upon it flat-bottomed weights of lead conveniently ſhaped, till they amounted (if both I and another miſremember not) to about ſeventy-five pounds; notwithſtanding all which, the egg was taken out ſound and uncracked; and probably might have ſupported a much greater preſſure, if we had been furniſhed with more weights of a commodious figure to heap upon it.

If we compare with this what was noted at the cloſe of the firſt experiment, about the breaking of an egg with four pounds weight, when no fluid body was interpoſed, it will be obvious to conclude how great a difference there is between the reſiſtance that a body may make to the preſſure of ſolid bodies, that bear hard againſt ſome parts, and not againſt others; and its reſiſtance to others that compreſs it uniformly, or in all places alike. For though it be denied, and that, I think, upon very inſufficient grounds, that bodies under water are preſſed by the incumbent water, becauſe, as it is pretended, the elements gravitate not in their proper place; yet this objection cannot be pretended to take place in our laſt experiment, where the main thing that leaned upon the water which ſurrounded the egg, being not a pillar of homogeneous water, but a great and ſolid weight of lead, the included egg muſt, by the intervention of the water, have been compreſſed. Nor were eggs the only bodies we endeavoured to cruſh after this manner, the trial having been alſo made upon a ſubſtance more ſoft, and of a very irregular ſhape.

To apply this now to divers, when they are at a moderate depth under water; it ſeems not improbable that the ſtructure of their bodies ſhould be robuſt enough, not to be violated by the preſſure of the incumbent, and otherwiſe ambient water. For we have ſeen by the former experiment, and eſpecially by the laſt recited, that a body, eaſy to be broken inwards by an incumbent ſolid weight, will remain entire and unaltered, in point of figure, under a very much greater weight, that compreſſes it after the manner of an ambient fluid. And though it would ſeem to many, that even in our ſuppoſition, the thorax being, as they think it, a kind of empty ſpace in the body, the ribs and muſcles ought, by the weight of the water, to be cruſhed into the great cavity intercepted between them; yet it is to be conſidered on the other ſide, that the air contained in the cheſt, eſpecially when its ſpring is increaſed by thoſe accidental cauſes that may take place, when men are deep under water, particularly the preternatural heat, which the want of the uſual reſpiration is apt to produce, will very much help the cheſt to reſiſt the preſſure, as they will eaſily grant, that have tried the reſiſtance that air makes, to be conſiderably compreſſed under water, the difficulty of farther compreſſing it ſtill encreaſing, as in ſprings it ought to do, the more it is compreſſed. And I further obſerve, that the ſtructure of the thorax is much more firm than men are wont to ſuppoſe; as appears by the very great ſolid weights that ſome men do, for gain, or to ſhew their ſtrength, ſuffer to be laid on their breaſts, without receiving any miſchief thereby. And if I ſhould admit, that at great depths the water had ſome little compreſſive operation upon the cheſt; yet that can be no other than the preſſing the parts a little inwards, and that the ſtructure of the thorax itſelf, fitted by nature for conſtriction and dilatation (as may appear in vehement takings in and blowings out of the air) may admit with ſmall inconvenience. To which purpoſe I recal to mind, what I lately mentioned concerning the phyſician that found his thorax ſomewhat compreſſed when he leiſurely dived; as alſo what I have * elſewhere delivered concerning a tad-pole, which ſwimming in water, that was ſtrongly compreſſed by an external force, ſeemed, thorough the glaſs that contained the water, to be ſomewhat leſſened in bulk, and yet not killed, nor ſenſibly cruſhed, notwithſtanding its great tenderneſs. And if there were parts of a human body that were of a texture too weak, and too diſproportionate to the reſt, I think it poſſible, that this compreſſion inwards might be great enough to be very ſenſible to the divers. For having purpoſely enquired of a certain man, whoſe trade it was to fetch up goods out of ſhips caſt away, by the help of a diving inſtrument, he told me, that when he was at a conſiderable depth under water, as about ten or twelve fathoms, he found, ſuitably to my conjecture, ſo great a preſſure againſt the drums or thin membranes of his ears, which were not ſufficiently counterpreſſed from within, as put him to a great deal of pain, till he had found ſome contrivances to leſſen the inconvenience. Nor was this man the only diver that has complained of this troubleſome preſſure, which ſeems to

* In the Appendix to Hydroſt. Paradox.

argue,

argue, that, at leaſt at great depths under water, the firmneſs of the ſtructure of a man's body does concur with the uniformity of the fluid's preſſure, to keep him from being hurt by the incumbent, and otherwiſe ambient air.

But I ſhall now ſay no more of the problem about divers, ſince (beſides that the matter of fact is not yet, in my opinion, accurately enough ſtated and determined) the true ſolution of it is not neceſſary to give a reaſon, why the weight of the air, a fluid ſo much lighter than water, ſhould not oppreſs nor cruſh the bodies of animals; though what has been already ſaid, about the reſiſtance of bodies under water, may ſerve very much to confirm the reaſons I propoſed, why we that live in the atmoſphere are not ſenſibly compreſſed, much leſs oppreſſed by its weight.

SOME OBSERVATIONS

ABOUT

SHINING FLESH,

Both of VEAL and of PULLET,

AND THAT

Without any ſenſible Putrefaction in thoſe Bodies.

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Yesterday, when I was about to go to bed, an amanuenſis of mine, accuſtomed to make obſervations, informed me, that one of the ſervants of the houſe, going upon ſome occaſion into the larder, was frighted by ſomething of luminous, that ſhe ſaw (notwithſtanding the darkneſs of the place) where the meat had been hung up before. Whereupon, ſuſpending for a while my going to reſt, I preſently ſent for the meat into my chamber, and cauſed it to be placed in a corner of the room capable of being made conſiderably dark, and then I plainly ſaw, both with wonder and delight, that the joint of meat did, in divers places, ſhine like rotten wood or ſtinking fiſh; which was ſo uncommon a ſight, that I had preſently thoughts of inviting you to be a ſharer in the pleaſure of it. But the late hour of the night did not only make me fear to give you too unseasonable a trouble, but being joined with a great cold I had got that day by making trial of a new telescope, you saw, in a windy place, I durst not sit up long enough to make all the trials that I thought of, and judged the occasion worthy of. But yet, because I effectually resolved to employ the little time I had to spare, in making such observations and trials as the accommodations I could procure at so inconvenient an hour would enable me, I shall here give you a brief account of the chief circumstances and phænomena that I had opportunity to take notice of.

1. Then I must tell you, that the subject we discourse of was a neck of veal; which, as I learned by enquiry, had been bought of a country-butcher on the Tuesday preceding.

2. In this one piece of meat I reckoned distinctly above twenty several places, that did all of them shine, though not all of them alike, some of them doing it but very faintly.

3. The bigness of these lucid parts was differing enough, some of them being as big as the nail of a man's middle finger, some few bigger, and most of them less. Nor were their figures at all more uniform, some being inclined to a round, others almost oval, but the greatest part of them very irregularly shaped.

4. The parts that shone most, which it was not so easy to determine in the dark, were some grisly or soft parts of the bones, where the butcher's cleaver had passed; but these were not the only parts that were luminous; for by drawing to and fro the medulla spinalis, we found that a part of that also did not shine ill; and I perceived one place in a tendon to afford some light; and lastly, three or four spots in the fleshy parts, at a good distance from the bones, were plainly discovered by their own light, though that were fainter than in the parts above-mentioned.

5. When all these lucid parts were surveyed together, they made a very splendid shew; but it was not so easy, because of the moistness and grossness of the lump of matter, to examine the degree of their luminousness, as it is to estimate that of glow-worms, which being small and dry bodies, may be conveniently laid in a book, and made to move from one letter or word to another. But by good fortune, having by me the curious transactions of this month, I was able so to apply that flexible paper to some of the more resplendent spots, that I could plainly read divers consecutive letters of the title.

6. The colour that accompanied the light was not in all the same, but in those which shone liveliest, it seemed to have such a fine greenish blue, as I have divers times observed in the tails of glow-worms.

7. But notwithstanding the vividness of this light, I could not, by the touch, discern the least degree of heat in the parts whence it proceeded; and having put some marks on one or two of the more shining places, that I might know them again, when brought to the light, I applied a sealed weather-glass, furnished with tincted spirit of wine, for a pretty while, and could not satisfy myself, that the shining parts did at all sensibly warm the liquor; but the thermoscope, though good in its kind, being not fitted for such nice experiments, I did not build much upon that trial.

8. Notwithstanding the great number of lucid parts in this neck of veal, yet neither I, nor any of those that were about me, could perceive, by the smell, the least degree of stink, whence to infer any putrefaction; the meat being judged very fresh and well-conditioned, and fit to be dressed.

9. The floor of the larder, where this meat was kept, is almost a story lower than the level of the street, and it is divided from the kitchen but by a partition of boards, and is

is furnished but with one window, which is not great, and looks towards the street, which lies northward from it.

10. The wind, as far as we could observe it, was then at south-west, and blustering enough. The air by the sealed thermoscope appeared hot for the season. The moon was passed its last quarter. The mercury in the barometer stood at $29\frac{3}{16}$ inches.

11. We cut off with a knife one of the luminous parts, which proved to be a tender bone, and being of about the thickness of a half crown piece, appeared to shine on both sides, though not equally; and that part of the bone, whence this had been cut off, continued joined to the rest of the neck of veal, and was seen to shine, but nothing near so vividly as the part we had taken off did before.

12. To try, whether I could obtain any juice or moist substance from this, as I have several times done from the tails of glow-worms; I rubbed some of the softer and more lucid parts (which I caused to be purposely cut off) as dexterously as I could, upon my hand, but I did not at all perceive any luminous moisture was thereby imparted; though the flesh seemed, by that operation, to have lost some of its light.

13. I caused also a piece of shining flesh to be compressed betwixt two pieces of glass, to try how well the contexture of it would resist that external force; but I did not find the light to be thereby extinguished during the short time I could allot to the experiment.

14. But supposing, that high rectified spirit of wine might so alter the contexture of the body it permeated, as to destroy its faculty of shining, I put a luminous piece of veal into a crystalline phial, and pouring on it a little pure spirit of wine, that would have burned all away, after I had shaken them together, I laid by the glass, and in about a quarter of an hour, or less, I found that the light was vanished.

15. But water would not so easily quench our seeming fires; for having put one of them into a China cup, and almost filled it with cold water, the light did not only appear, perhaps undiminished, through that liquor, but above an hour after was vigorous enough not to be eclipsed, by being looked upon at no great distance from a burning candle, that was none of the smallest; and probably the light would have been seen much longer if we could have afforded to watch out its duration.

16. Whilst these things were doing, I caused the pneumatical engine to be prepared in a room without fire (that the experiment might be tried in a greater degree of darkness); and having conveyed one of the largest luminous pieces into a small receiver, we caused the candles to be put out, and the pump to be plied in the dark; but the diminution of light, after the pump seemed to have been employed for a competent while, appeared so inconsiderable (whether because our eyes had leisure to be fitted to that dark place, or for what other cause soever) that I began to suspect that the instrument having been managed in the dark, had leaked all the while. Wherefore causing the lights to be brought in, and a mercurial gage to be put into the receiver, when we were sure that this glass was well cemented on to the engine, the candles being removed, the pump was set a-work again; and then opening my eyes, which I had kept closed against the light of the candles, I could perceive, upon the gradual withdrawing of the air, a discernible and gradual lessening of the light, which yet was never brought quite to disappear (as I long since told you, the light of rotten wood and glow-worms had done) or to be so near vanishing as one would have expected; though, upon the bringing in of the candles again, it appeared by the gage, that the pump had been diligently applied. But the room being once again darkened; by the hasty increase of light that had disclosed itself in the veal, upon this letting in of the air to the exhausted receiver, it appeared more manifestly than before, that the decrement, though but slowly made,

made, had been conſiderable. This trial we once more repeated with a not unlike ſucceſs; which, though it convinced us, that the luminous matter of our included body was more vigorous, or tenacious than that of moſt other ſhining bodies, yet it left us ſome doubts that the light would have been much more impaired, if not quite made to vaniſh, if the ſubject of it could have been kept long enough in our exhauſted receiver; but the unſeaſonable time of the night reducing me at length to go to bed, I could not ſtay to proſecute this, or any other trial.

17. ONLY, whilſt I was undreſſing, this further obſervation occurred, that ſuppoſing there might be, in the ſame larder, more joints of the ſame veal than one, ennobled with this ſhining faculty, it was found, that a leg of veal, which was cauſed to be brought into my chamber, had ſome ſhining places in it; though they were but very few, and faint, in compariſon of thoſe that were conſpicuous in the above mentioned neck.

18. WHAT further phænomena this morning might have afforded me, I cannot tell, having been haſtily called up, before day, for a niece, that I am very juſtly, and exceedingly concerned for; who was thought to be upon the point of death, and whoſe almoſt gaſping condition had too much affected and employed me, to leave me any time for philoſophical entertainments, that require a calm, if not a pleaſed mind. Only this I took notice of, becauſe the obſervation could not coſt me a minute of an hour, that whilſt they were bringing me candles for to riſe by, I looked upon a clean phial, that I had laid upon the bed by me, after a piece of our luminous veal had been included in it, and found it to ſhine vividly at that time, which was between four and five of the clock this morning; ſince when I have made no one obſervation or trial.

POSTSCRIPT.

19. NEAR two days after I had made the fore-mentioned obſervations, thoſe horrid ſymptoms of my niece's diſeaſe, that had ſo much alarmed the phyſicians, and me, being, through God's goodneſs, conſiderably abated, I began to reſume the thoughts of our ſhining veal; and though having, in the hurry I was in, forgotten to take any order about it, I found it was already diſpoſed of; yet the piece I lately mentioned to have been included in a phial, being preſerved in it, I looked upon it the third day (incluſively) after we had firſt obſerved the meat it was cut off from to be luminous; and I found it to ſhine in the dark as vigorouſly as ever. The fourth day its light was alſo conſpicuous; ſo that I was able, in a dark corner of the room, to ſhew it, even in the day-time, to three or four very ingenious phyſicians, all of them, ſave one, members of the Royal Society; and, I preſume, I need not remind you, that the following night, I invited you to be a ſpectator of it, though before that time the light had begun to decay, and the offenſive ſmell to grow ſomewhat ſtrong: which ſeems to argue, that the diſpoſition, upon whoſe account our veal was luminous, may very well conſiſt both with its being, and not being in a ſtate of putrefaction, and conſequently is not likely to be derived merely from the one, or the other. The fifth day, in the morning, looking upon it when I awaked, and before the curtains were opened, it ſeemed to ſhine better, than it had done the day preceding. The ſame night alſo, it was manifeſt enough, though not vivid, in the dark. When I awaked the ſixth day in the morning, after the ſun was riſen, I could, within the curtains, perceive a glimmering light. But the ſeventh day, which was yeſterday, I could not, late at night, diſcern any light at all.

You

You saw too much in what a condition I was, when you did me the favour to visit me, to expect that I should presume to entertain you with any speculations about the cause of these unusual apparitions of light. It is true indeed, that in some notes I formerly mentioned to you, I endeavoured to make it probable, that whether light depend upon a particular kind of impulse, propagated through a transparent medium, or upon a diffusion of extremely little parts from the luminous body, or upon the action of some other corporeal agent; whatever the efficient be, the effect is produced in a mechanical way. But though I had these papers by me, yet, to determine what peculiar kind of motions or other operations nature really employed in the production of a light, which seemed not clearly, by what I shall presently note, referable either to the particular and settled constitution of the animals, whose flesh shined (as in our glow-worms, and some American flies) or to that intestine and unusual motion of the parts that causes or accompanies putrefaction in rotten wood, or fishes; since, upon the first and liveliest appearance of the light, there was not any (at least that could be taken notice of by the senses); to determine this, I say, it seemed to me so difficult a task, that I shall willingly leave the solution of such abstruse phænomena, as some of ours, unattempted; especially since I may, God permitting, make an historical mention of them the day after to-morrow, at the meeting of the Royal Society; where, I doubt not, much more, and more to the purpose, will be said, and considered, than I have vanity to think myself capable of offering. Only, for the prevention of some needless conjectures, to which, without this previous advertisement, one might upon plausible grounds indulge, I shall, in the mean while add, and conclude with one observation more, which may possibly take off our thoughts from striving to deduce the shining of our veal from the peculiar nourishment, or constitution, or properties, of that individual calf, whose flesh, &c. was luminous. For, having several nights sent purposely into the larder, to observe, whether any veal, since brought thither, or any other meat, did afford any light, a negative answer was always brought me back; save at one time, which happened to be within less than forty-eight hours of that, at which the luminousness of the veal had been first taken notice of; for at this time there was, in the same larder, a conspicuous light seen in a pullet that hung up there, which having caused to be brought up into a darkened place in my chamber, in the night-time, I perceived four or five luminous places; which were not indeed near so large as those of the veal, but were little less vivid than they. All of these I took notice to be either upon, or near the rump; and that which appeared most like a spark of fire, shone at the very tip of that part. Yet was not this fowl mortified, nor at all ill scented, but so fresh, that the next day I found it very good meat. But whether this may reasonably lead to a suspicion, that the peculiar constitution of the air in that larder, and at that time, may as well deserve to be taken into consideration, as the peculiar nature of the animals, whose flesh did shine, is a question, that I, who have scarce time to name it, must not presume to do any more than name. And therefore, as soon as I have begged your pardon for this tedious though hasty scribble, I shall, without ceremony, subscribe myself, &c.

A NEW

A

NEW EXPERIMENT

CONCERNING

An Effect of the varying Weight of the ATMOSPHERE upon ſome Bodies in the Water; ſuggeſting a Conjecture, that the very Alterations of the Air, in point of Weight, may have conſiderable Operations, even upon Men's Sickneſs or Health.

Firſt publiſhed in the *Philoſophical Tranſactions*, N° 91. p. 5156, for *February* 24, 1672-3.

THOUGH many things have, by ingenious men, been already obſerved, as to the power and operations of the atmoſphere's weight upon liquors that are expoſed to it in Torricellian tubes, or other veſſels, cloſed at one end, and near the top, either empty or unfilled with any viſible body; yet men ſeem not to have much enquired what effects the very variation of this weight of the atmoſphere may have on the liquors which it preſſes, in other veſſels than ſuch as baroſcopes and pumps. And yet when I remember, how much of air appears by our engine to be inviſibly harboured in the pores, not only of water, but of the blood, ſerum, urine, gall, and other juices of the human body; and that (as I have elſewhere experimentally ſhewn) the preſſure of the atmoſphere, and the ſpring of the air, work upon liquors, and on bodies immerſed in thoſe liquors, as well as upon ſolid ones, immediately expoſed to the air, I am prone to ſuſpect that the very alterations of the atmoſphere, in point of weight, may, in ſome caſes, have ſome not contemptible operations, even upon men's ſickneſs or health; as when the ambient air, for inſtance, grows ſuddenly very much lighter than it was before, or than it was wont to be, the ſpirituous and aërial particles that are plentifully harboured in the maſs of blood, will naturally ſwell that liquor, and ſo may diſtend the greater veſſels, and not a little after the celerity and manner of the circulation of the blood by the capillary arteries and veins. By which alteration, that divers changes may happen in the body, will not ſeem improbable to thoſe that know in general, how important a thing the manner of the

the circulation of the blood may be there, though, as to its particular effects, I leave them to the speculation of physicians; and shall only add, that to keep this conjecture of mine (for I propose it as no other) from seeming as groundless as extravagant, I will annex an experiment, that you will not perhaps dislike, just as I find it registered among some of my loose papers.

I CAUSED to be blown, at the flame of a lamp, three small round glass bubbles, about the bigness of hazel-nuts, and furnished each of them with a short and slender stem, by whose means they were so nicely poised in water, that a very small change of weight would make them either emerge, if they but lightly leaned on the bottom of the vessel, or sink, if they floated on the top of the water.

THIS being done at a time, when the atmosphere was of a convenient weight (and such a season is not ordinarily difficult to be chosen within some reasonable time, to him that wants neither attention nor a good baroscope) I put them in a wide-mouthed glass, furnished with common water, and leaving them in a quiet place, where yet they were frequently in my eye, and were suffered to continue many weeks, or some months, I observed, as I expected, that sometimes they would be at the top of the water, and remain there for divers days, or perhaps weeks; and sometimes would fall to the bottom, and after having continued there for some time, longer or shorter, they would again emerge. And though sometimes, especially if I removed the vessel that contained them to a southern window, they would rise to the top, or fall to the bottom of the water, according as the air was hot or cold; yet it was not difficult to distinguish these motions from those produced by the varying gravity of the atmosphere. For when the beams of the sun, or heat of the ambient air, by rarefying the air included in the bubbles, made that air drive out some of the water, and consequently made the whole bubble, consisting of glass, air, and water, somewhat lighter than a bulk of water equal to it, though the bubble did necessarily swim as long as the included air was thus rarefied, yet when the absence of the sun, or any other cause, made the air lose its adventitious warmth, there would ensue a condensation of the air again, and thereupon an intrusion of more water (to succeed the air) into the glass, and consequently a sinking of the bubble; and this would commonly happen at night, if it did not happen sooner. But when it was upon the account of the varying weight of the atmosphere that the bubbles either rose or fell, it appeared by the baroscope, that the atmosphere was so heavy, or so light, that they ought to do so. Insomuch, that I divers times predicted, whether I should find the mercury in the baroscope high or low, by observing the situation and posture of the bubbles; and consulting that instrument, it verified my conjectures. And though, whilst the atmosphere was not too considerably either light or heavy, the changes of the air, as to heat or cold, would, as I was saying, place the bubbles sometimes at the top, and sometimes at the bottom of the water, within the compass of a day; yet, if the atmosphere were either very heavy, or very light, the bubbles would continue at the bottom, or at the top of the water for many days together, in case the atmosphere did not in all that time change its gravity. And I remember, that I did, for curiosity's sake, when the quicksilver was high in the baroscope, put the glass two or three days in a south window about noon, and for a good while after, and that in sun-shining weather; and yet even then the bubbles did not emerge, though it appeared by a good sealed weather-glass, which I kept in the same window, that the ambient air was much warmer than at other times, when I had observed the bubbles to keep at the top of the water.

N.B. 1. It being very difficult to poise several bubbles precisely, as well one as another, I thought it not strange, that all the three bubbles did not constantly (though for the most part they did) rise and fall together; but sometimes two of them, and now and then, though seldom, one alone would sink or emerge, when the change of the weight of the atmosphere was not considerable enough to operate sensibly upon the rest; and of such instances, I have had opportunity to observe one or two within these last three days; and therefore it is not amiss to poise a greater number of bubbles together, that after trial made of all, the fittest may be chosen. Which advertisement will appear the more proper, because of what is to be added in the following note.

2. I HAVE observed it sometimes to happen, that a bubble that floated when it was first poised, would, after a while, subside without any manifest cause; or if it were made to sink by such a cause, it would continue at the bottom of the water, though that cause were removed; which difficult phænomenon seeming to depend upon a kind of imbibition made of certain particles of an aërial nature, by the water, the consideration of it belongs to another place, not to this; where it may suffice, that the experiment did sometimes actually answer expectation, as that above-related did; wherein my main drift is to shew, that since, as the atmosphere was heavier or lighter, it is capable to work upon bodies under water, so as to procure their sinking, or their emersion; the air, though a fluid a thousand times lighter, must lean or press upon the water itself, by whose intervention it produces these effects; which confirms what I elsewhere teach, that the atmosphere is incumbent, as a heavy body, upon the terraqueous globe.

3. BESIDES the other circumstances, upon whose account this experiment may fail of success, the season of the year, wherein it is tried, may, for aught I know, be considerable. For which reason I shall here add this advertisement, that I choose, but do not confine myself, to make my trials about the beginning of the spring, as a time wherein notable alterations of the air, as well to weight as to other things, are the likeliest to be frequent.

ESSAYS

E S S A Y S

OF THE

STRANGE SUBTILTY,
GREAT EFFICACY,
DETERMINATE NATURE

OF

E F F L U V I U M S.

To which are annexed

New Experiments to make FIRE and FLAME ponderable:

TOGETHER WITH

A Discovery of the PERVIOUSNESS of GLASS.

An ADVERTISEMENT to the READER.

IT is hoped the reader will not think it strange, not to meet with, in the following papers, a more close and uniform contexture of the passages that make them up, if he be seasonably informed of the rise and occasion of penning them, which was this. The author having many years ago written an essay about an experiment he made of nitre, by whose phænomena he endeavoured to exemplify some parts of the corpuscular philosophy, especially the production of qualities; he afterwards threw together divers occurring thoughts and experiments, which he supposed might be employed by way of notes, to prove or illustrate those doctrines, and especially those that concerned the qualities of bodies; and among these, observing those that are called occult, to be subjects uncultivated enough (at leaſt, in the way that ſeemed to him proper) he propoſed to handle them more largely than moſt of the reſt; and in order to that deſign he judged it almoſt neceſſary to premiſe ſome conſiderations and experimental collections about the nature and power of effluviums, about the pores of bodies and figures of corpuſcles, and about the efficacy of ſuch local motions as are wont either to be judged very faint, or to be paſſed by unheeded. For he had often looked upon theſe three doctrines of effluvia, of pores and figures, and of unheeded motions, as the three principal keys to the philoſophy of occult qualities. But having hereupon made ſuch collections, as upon review appeared too large to paſs for notes on ſo ſhort a text, he was induced to draw them * into the form (they now appear in) of eſſays; but he would not put himſelf to the trouble of doing it, with care to keep them from retaining much of their firſt want of exact method and connexion. Nor was the author ſolicitous to finiſh them up, in regard that his other ſtudies and occaſions made him perceive, that in what he had deſigned about occult qualities, he had cut himſelf out more work, than probably he ſhould, during many years, have opportunity to ſet upon in earneſt, and complete. And in this condition theſe papers lay for divers years (as is well known to ſeveral that ſaw them, or even tranſcribed ſome of them) and might have continued to do ſo, if the author had not been induced to let them come abroad, partly by conſidering, that though the ſubjects (however he handled them) were as well important as curious, yet he did not find himſelf prevented by others in what he had to publiſh about them; and partly by the references he had made to them in ſome other papers, that he had promiſed his friends, wherein ſeveral things here delivered are vouched, and others ſuppoſed. And becauſe the notes concerning the poroſity of greater bodies, and the figurations of minute particles, together with the paper about unregarded motions, having been long laid aſide among other neglected papers, were ſome of them miſſing, and others ſo miſuſed, that they could not eaſily be made ready to accompany thoſe that now come abroad; the author, that he might keep this book from having its dimenſions too diſproportionate, was content to add to the thickneſs of it, by ſubjoining one of thoſe little tracts, that lay by him, concerning flame, becauſe of the affinity betwixt the preceding doctrine about effluviums in general, and experiments that ſhew, in particular, the ſubtilty and efficacy of thoſe of fire and flame. And though to that tract itſelf there belong another, deſigned to examine, whether the matter of what we call the ſun-beams, may be brought to be ponderable; yet ſuppoſing this hitherto cold and wet ſummer to be like to be as unfriendly to the trials to be made with burning-glaſſes, as of late years ſome other ſummers have proved, he was eaſily prevailed with, not to make thoſe experiments that were ready, wait any longer for thoſe that probably will not in a ſhort time be ſo; eſpecially ſince thoſe that now come abroad, have no dependency upon the others.

* And ſome that were publiſhed anno 1669, under the title of the Atmoſpheres of Conſiſtent Bodies.

OF THE STRANGE SUBTILTY OF EFFLUVIUMS.

CHAP. I.

WHETHER we suppose, with the ancient and modern atomists, that all sensible bodies are made up of corpuscles, not only insensible, but indivisible; or whether we think with the Cartesians, and (as many of that party teach us) with *Aristotle*, that matter, like quantity, is indefinitely, if not infinitely divisible; it will be consonant enough to either doctrine, that the effluvia of bodies may consist of particles extremely small. For if we embrace the opinion of *Aristotle*, or *Des Cartes*, there is no stop to be put to the subdivision of matter into fragments still lesser and lesser. And though the Epicurean hypothesis admit not of such an interminate division of matter, but will have it stop at certain solid corpuscles, which, for their not being further divisible, are called atoms, ἄτομοι; yet the assertors of these do justly think themselves injured, when they are charged with taking the motes, or small dust, that fly up and down in the sun-beams, for their atoms; since, according to these philosophers, one of those little grains of dust, that is visible only when it plays in the sun-beams, may be composed of a multitude of atoms, and exceed many thousands of them in bulk. This the learned *Gassendus*, in his notes on *Diogenes Laertius*, makes probable by the instance of a small mite, which, though scarce distinctly discernible by the naked eye, unless when it is in motion, does yet, in a good microscope, appear to be a complete animal, furnished with all necessary parts; which I can easily allow, having often in cheese-mites very distinctly seen the hair growing upon their legs. And to the former instance I might add, what I have elsewhere told you of, a sort of animals far lesser than cheese-mites themselves, namely those that may be oftentimes seen in vinegar. But what has been already said may suffice for my present purpose, which is only to shew that the wonderful minuteness I shall hereafter ascribe to effluvia, is not inconsistent with the most received theories of naturalists. For otherwise, in this essay, the proofs I mean to employ, must be taken, not *à priori*, but *à posteriori*. And the experiments and observations I shall employ on this occasion, will be chiefly those that are referrible to one of the following heads.

1. The strange extensibility of some bodies, whilst their parts yet remain tangible.
2. The multitude of visible corpuscles that may be afforded by a small portion of matter.
3. The smallness of the pores, at which the effluvia of some bodies will get in.

4. THE ſmall decrement of bulk, or weight, that a body may ſuffer by parting with great ſtore of effluvia.

5. THE great quantity of ſpace that may be filled, as to ſenſe, by a ſmall quantity of matter, when rarefied or diſperſed.

BUT though to theſe diſtinct heads I ſhall deſign diſtinct chapters, yet you muſt not expect to find the inſtances ſolicitouſly marſhalled, but ſet down in the order they occurred to me; ſuch a liberty being allowable in a paper, where I pretend not to write treatiſes, but * notes.

CHAP. II.

AMONG many things that are groſs enough to be the objects of our touch, and to be managed with our hands, there are ſome that may help us to conceive a wonderful minuteneſs in the ſmall parts they conſiſt of.

I DO not remember what *Cardan*, and ſince him, another writer, have delivered about the thinneſs and ſlenderneſs to which gold may be brought. And therefore, without poſitively aſſenting to, or abſolutely rejecting what may have been ſaid about it by others, I ſhall only borrow, on this occaſion, what I have mentioned on † another, upon my own obſervation; namely, that ſilver, whoſe ductility and tractility are very much inferior to thoſe of gold, was, by my procuring, drawn out to ſo ſlender a wire, that, when we meaſured it, which was ſomewhat troubleſome to do, with a long and accurate meaſure, we found, that eight yards of it did not yet fully counterpoiſe one grain; ſo that we might add a grain more without making the ſcale, wherein it was put, manifeſtly preponderate, notwithſtanding the tenderneſs of the balance. Whence we concluded, that a ſingle grain of this wire amounted to twenty-ſeven feet, that is, three hundred and twenty-four inches. And ſince experience informs us, that half an Engliſh inch can, by diagonal lines, be divided into one hundred parts, great enough to be eaſily diſtinguiſhed, even for mechanical uſes, it follows, that a grain of this wire-drawn ſilver may be divided into ſixty-four thouſand eight hundred parts, and yet each of theſe will be a true metalline, though but ſlender and ſhort cylinder, which we may very well conceive to conſiſt yet of a multitude of minuter parts. For though I could procure no gilt wire near ſo ſlender as our newly-mentioned ſilver wire; yet I tried, that ſome, which I had by me, was ſmall enough to make one grain of it fourteen feet long; at which rate an ounce did amount to a full mile, conſiſting of one thouſand geometrical paces of five feet a piece, and ſeven hundred and twenty feet over and above. And if now it be permitted to ſuppoſe the wire to have been, as in probability it might have been, further drawn out to the ſame ſlenderneſs with the above-mentioned ſilver wire, the inſtance will ſtill be far more conſiderable; for in this caſe, each of thoſe little cylinders, of which ſixty-four thouſand eight hundred go to the making of one grain, will have a ſuperficial area, which, except at the baſis, will be covered with a caſe of gold; which is not only ſeparable from it by a mental operation, but perhaps alſo by a chymical one. For I remember, that from very ſlender gilt wire, though I could get none ſo ſlender as this of mere ſilver, I did, more than once, for curioſity's ſake, ſo get out the ſilver, that the golden films, whilſt they were in a liquor that plumped them up, ſeemed to be ſolid wires of gold; but when the liquor was withdrawn, they appeared, as indeed they were,

* This Eſſay was deſigned to be but a part of the author's notes upon his Eſſay about Saltpetre.

† In a paper about Improbable Truths.

to

to be oblong, and extremely thin and double membranes of that metal, which, with an inſtrument that had been delicate enough, might have been ripped open, and diſplayed, and been made capable of further diviſions and ſub-diviſions. To this I ſhall add, that each of the little ſilver cylinders I lately ſpoke of, muſt not only have its little area, but its ſolidity; and yet I ſaw no reaſon to doubt, but that it might be very poſſible, if the artificer had been ſo ſkilful and willing, as I wiſhed, to have drawn the ſame quantity of metal to a much greater length, ſince even an animal ſubſtance is capable of being brought to a ſlenderneſs much ſurpaſſing that of our wire, ſuppoſing the truth of an obſervation of very credible perſons, critical enough in making experiments, which, for a confirmation, and an improvement of our preſent argument, I ſhall now ſubjoin. An ingenious gentlewoman of my acquaintance, wife to a learned phyſician, taking much pleaſure to keep ſilk-worms, had once the curioſity to draw out one of the oval caſes (which the ſilk-worm ſpins, not, as it is commonly thought, out of its belly, but out of the mouth, whence I have taken pleaſure to draw it out with my fingers) into all the ſilken wire it was made up of, which, to the great wonder, as well of her huſband, as herſelf, who both informed me of it, appeared to be, by meaſure, a great deal above three hundred yards, and yet weighed but two grains and a half; ſo that each cylindrically ſhaped grain of ſilk may well be reckoned to be at leaſt one hundred and twenty yards long.

Another way, I remember, I alſo employed to help men, by the extenſibility of gold, the better to conceive the minuteneſs of the parts of ſolid bodies.

We took ſix beaten leaves of gold, which we meaſured one by one with a ruler purpoſely made for nice experiments, and found them to have a greater equality in dimenſions, and to be nearer true ſquares than could be well expected; the ſide of the ſquare was in each of them exactly enough three inches and $\frac{2}{8}$ or $\frac{1}{4}$, which number being reduced to a decimal fraction, viz. $\frac{3|25}{1|00}$, and multiplied by itſelf, affords $\frac{10|5625}{1|0000}$ for the area, or ſuperficial content of each ſquare leaf; and this multiplied by 6, the number of the leaves amounts to $\frac{63|3750}{1|0000}$ ſquare inches, for the area of the ſix leaves. Theſe, being carefully weighed in a pair of tender ſcales, amounted all of them to one grain and a quarter; and ſo one grain of this foliated gold was extended to ſomewhat above fifty inches; which differed but about a fifth part from an experiment of the like nature, that I remember I made many years ago in a pair of exact ſcales; and ſo ſmall a difference may very well be imputed to that of the pains and diligence of the gold-beaters, who do not always work with equal ſtrength and ſkill, nor upon equally fine and ductile gold.

Now if we recal to mind what I was lately ſaying, of the actual diviſibility of an inch into an hundred ſenſible parts, and ſuppoſe an inch ſo divided, to be applied to each ſide of a ſquare inch of the leaf-gold newly mentioned, it is manifeſt, that by ſubtle parallel lines, drawn between all the oppoſite points, a grain of gold muſt be diviſible into five hundred thouſand little ſquares, very minute indeed, but yet diſcernible by a ſufficient ſharp-ſighted eye. And if we ſuppoſe an inch to be divided into two hundred parts, as I lately told you it was in a ruler I employ, then, according to the newly-recited way, the number of the ſquares, into which a ſingle grain is capable of being divided, will amount to no leſs than two millions.

There is yet another way that I took to ſhew, that the extenſibility, and conſequently the diviſibleneſs of gold, is probably far more wonderful than by the lately mentioned trial it appears.

For this purpoſe, I went to a great refiner whom I uſed to deal with for purified gold and ſilver, and enquired of him how many grains of leaf-gold he was wont to allow to an ounce of ſilver, when it was to be drawn into gilt wire as ſlender as an hair. To this

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he answered me, that eight grains was the proportion he allowed to an ounce, when the wire was to be well gilt; but if it were to be more slightly gilt, six grains would serve the turn. And to the same purpose I was answered by a skilful wire-drawer. And I remember, that desiring the refiner to shew me an ingot of silver, as he did at first gild it; he shewed me a good fair cylindrical bar, whereon the leaf-gold that overlaid the surface, did not appear to be, by odds, so thick as fine Venetian paper; and yet comparing this with gilt wire, which I also desired to see, the wire appeared to be the better gilt of the two; possibly, because the gold, in passing through the various holes, was by the sides of them not only extended, but polished, which made it look more vividly than the unpolished leaves that gilded the ingot.

So that, if we suppose an ounce of the gilt wire formerly mentioned to have been gilt with six grains of leaf-gold, it will appear, by an easy calculation, that at this rate one ounce of gold, employed on gilding wire of that slenderness, would reach between ninety and an hundred miles. But if now we further suppose, as we lately did, that the slender silver wire, mentioned at the beginning of this chapter, were gilt; though we should allow it to have (because of its exceeding slenderness) not, as the former, six grains, but eight grains of leaf-gold to an ounce of silver, it must be acknowledged, that an hollow cylinder, or sheath of gold, weighing but eight grains, may be so stretched, that it will reach to no less than sixty times as much in weight of silver wire, as it covers [I said sixty times, for so often is eight contained in four hundred and eighty, the number of grains in an ounce]; and consequently a grain of that wire having been found to be twenty-seven feet long, the ounce of gold would reach to seven hundred seventy-seven thousand six hundred feet, that is, an hundred fifty-five miles and above a half. And if we yet further suppose this superficial, or hollow cylinder of gold, to be slit all along, and cut into as slender lists or thongs as may be, we must not deny that gold may be made to reach to a stupendous length. But we need not this last supposition to make what preceded it an amazing thing; which yet, though it be indeed stupendous, and seem incredible, ought not at all to be judged impossible, being no more than what, upon the suppositions and observations above laid down, does evidently follow.

C H A P. III.

After what has been said of the minuteness of tangible objects, it will be proper to subjoin some instances of the smallness of such as yet continue visible. But, in regard these corpuscles are singly too little to have any common measure applied to any of them, we must make an estimate of their minuteness by the number of those, into which a small portion or fragment of matter may be actually divided, the multitude of these being afforded by so inconsiderable a quantity of matter, sufficiently declaring, that each of them, in particular, must be marvelously little.

Among the instances, where the smallness of bodies may be deduced from what is immediately the object of sight, it may not be unfit to take notice of the evaporation of water, which though it be granted to consist of gross particles, in comparison of the spirituous and odoriferous ones of divers other liquors, as of pure spirit of wine, essential oils of spices, &c. yet to shew, that a small quantity of it may be dispersed into a multitude of manifestly visible corpuscles, I thought upon, and more than once tried the rarefaction of it into vapours by help of an æolipile, wherein, when I made the experiment the last time, I took the pains to register the event as follows:

We

We put an ounce of common water into an æolipile, and having put it upon a chafing-diſh of coals, we obſerved the time, when the ſtreams of vapours began to be manifeſt. This ſtream was, for a good while, impetuous enough, as appeared by the noiſe it made, which would be much encreaſed, if we applied to it, at a convenient diſtance, a kindled brand, in which it would blow up the fire very vehemently. The ſtream continued about a quarter of an hour, ſixteen minutes, or better, but afterwards the wind had pauſes and guſts for two or three minutes before it quite ceaſed. And by reaſon of the ſhape of the æolipile (which being framed chiefly for other purpoſes, was not ſo convenient for this) a great portion of the vapours condenſed in the upper part of it, and fell down in drops; ſo that ſuppoſing that they alſo had come out in the form of wind, and the blaſt had not been intermitted toward the latter end, I gueſſed it might have continued uninterruptedly eighteen or twenty minutes. Note, that applying a meaſure to the ſmoke, that came out very viſible in a form almoſt conical, where it ſeemed to have an inch or more in diameter, it was diſtant from the hole of the æolipile about twenty inches; and five or ſix inches beyond that, though it were ſpread ſo much, as to have four or five inches in diameter, yet the not uniform, but ſtill-cohering clouds, which was the form, wherein the vapours appeared, were manifeſt and conſpicuous.

After the rarefaction of water, when it is turned into vapours, we may conſider that of fewel, when it is turned into flame; to which purpoſe, I might here propoſe ſeveral trials, as well of our own as others, about the prodigious expanſion of ſome inflammable bodies upon their being actually turned into flame. But in this place to mention all theſe, would perhaps too much intrench upon another paper; and therefore I ſhall here propoſe to your conſideration but one inſtance, and that very eaſy to be tried, of which I find this account among my *adverſaria*.

Having oftentimes burnt ſpirit of wine, and alſo oil in glaſs lamps, that for certain uſes were ſo made, that the ſurface of the liquor was ſtill circular, it was obvious to obſerve, how little the liquor would ſubſide by the waſte, that was made of it, in about half a quarter of an hour. And yet if we conſider, that the naked eye, after ſome exerciſe, may, as I have often tried, diſcern the motions of a pendulum, that ſwings faſt enough to divide a ſingle minute of an hour into two hundred and forty parts, and conſequently half a quarter of an hour into one thouſand eight hundred parts; if we alſo conſider, into how many parts of the time employed by a pendulum, the vibrations, ſlow enough to be diſcernible by the eye, may be mentally ſubdivided; and if we further conſider, that, without intermiſſion, the oil is preyed upon by an actual flame, and the particles of it do continually furniſh a conſiderable ſtream of ſhining matter, that with a ſtrange celerity is always flying away; we may very well conceive, that thoſe parts of flame into which the oil is turned, are ſtupendouſly minute, ſince, though the waſting of the oil is in its progreſs too ſlow to be perceived by the eye, yet it is undoubted, that there is a continual decrement of the depth of the oil, the phyſical ſurfaces whereof are continually and ſucceſſively attenuated and turned into flame; and the ſtrange ſubtilty of the corpuſcles of flame would be much the ſtronglier argued, if we ſhould ſuppoſe, that inſtead of common oil the flame were nouriſhed by a fewel ſo much more compact and durable, as is that inflammable ſubſtance made of a metalline body, of whoſe laſtingneſs I have elſewhere made particular mention *, after having taught the way of preparing it.

* In ſome papers about Flame.

HAVING in a pair of tender ſcales carefully weighed out half a grain of good gunpowder, we laid it on a piece of tile, and whelmed over it a veſſel of glaſs (elſewhere deſcribed, and often mentioned) with a braſs plate to cover the upper orifice of it. Then having fired the gunpowder, we obſerved that the ſmoke of it did opacate, and, as to ſenſe, ſo fill the whole cavity of the glaſs, though its baſis were eight inches, its perpendicular height above twenty inches, and its figure far more capacious, than if it were conical; and this ſmoke, not containing itſelf within the veſſel, iſſued out at two or three little intervals, that were purpoſely left between the orifice of the veſſel and the plate that lay upon it. This cover we then removed, that we might obſerve how long the ſmoke would continue to aſcend; which we found it would do for about half a quarter of an hour, and during near half that time, viz. the three firſt minutes, the continually aſcending ſmoke ſeemed to be, at its going out, of the ſame diameter with the orifice at which it iſſued; and it would aſcend ſometimes a foot, ſometimes half a yard, ſometimes two feet or more into the air, before it would diſperſe and vaniſh into it.

Now if we conſider that the cavity of this round orifice was two inches in diameter, how many myriads of viſible corpuſcles may we eaſily conceive thronged out at ſo large an outlet, in the time above-mentioned, ſince they were continually thruſting one another forwards? and into ſo many viſible particles of ſmoke muſt we admit, that the half grain of powder was ſhattered, beſide thoſe multitudes, which, having been turned into actual flame, may probably be ſuppoſed to have ſuffered a comminution, that made them become inviſible. And though I ſhall not attempt ſo hopeleſs a work, as to compute the number of theſe ſmall particles, yet to make an eſtimate, whereby it would appear to be exceeding great, I thought fit to conſider, how great the proportion was between the ſpaces, that to the eye appeared all full of ſmoke, and the dimenſions of the powder, that was reſolved into that ſmoke. Cauſing then the glaſs to be filled with common water, we found it to contain above two-and-twenty pints of that liquor, and cauſing one of thoſe meaſures to be weighed, it was found to weigh ſo near a pound (of ſixteen ounces) that the computation of the whole water amounted to at leaſt one hundred and ſixty thouſand grains, and conſequently three hundred and twenty thouſand half grains. To which if we add, that this gunpowder would readily ſink to the bottom of water, as being (by reaſon of the ſalt-petre and brimſtone, that make up at leaſt ſix parts of ſeven of it) in ſpecie heavier than it, and in likelihood twice as heavy (for it is not eaſy to determine it exactly) we may probably gueſs the ſpace to which the ſmoke reached, to exceed five hundred thouſand times that which contained the unfired powder; and this, though the ſmoke, being confined in the veſſel, was thereby kept from diffuſing itſelf ſo far, as by its ſtreaming out it ſeemed likely, that it would have done.

To theſe inſtances from inanimate bodies, I ſhall ſubjoin one more taken from animals. Whereas then men have with reaſon wondered, that ſo ſmall a body as a cheeſe-mite, which by the naked eye is oftentimes not to be taken notice of, unleſs it move (if even then it be ſo) ſhould, by the microſcope, appear to be an animal furniſhed with all neceſſary parts; whereas this, I ſay, has given juſt occaſion to conclude, that the corpuſcles that make up the parts of ſo ſmall an animal, muſt themſelves be extremely ſmall; I think the argument may be much improved by the following conſideration. Thoſe that have had the curioſity to open from time to time eggs, that are ſat upon by a hatching hen, cannot but have obſerved, how ſmall a proportion, in reference to the bulk of the whole egg, the chick bears; when that which the excellent *Harvey* calls *punctum ſaliens*, diſcloſes the motion of the heart, and the colour of the blood; and that

that even about the seventh or eighth day, the whole chick now visibly formed bears no great proportion to the whole egg, which is to supply it with aliment, not only for its nourishment, but speedy growth for many days after.

To apply this now to the matter in hand; having several times observed, and shewn to others, that cheese-mites themselves are generated of eggs, if we conceive, that in these eggs, as in ordinary ones, the animal at its first formation bears but a small proportion to the bulk of the whole egg, the remaining part being to suffice for the food and growth of the embryo probably for a pretty while; since, if an ingenious person, that I desired to watch them, did not misinform me, they used to be about ten or twelve days in hatching; this whole egg itself will be allowed to be but little, in reference to the mite it came from, how extremely and unimaginably minute may we suppose those parts to be, that make up the alimental liquors, and even the spirits, that passing through the nerves, or analogous parts, serve to move the limbs and sensories of but, as it were, the model of such an animal, as, when it rests, would not, perhaps, itself, to the naked eye be so much as visible; and in which we may presume the nobler sort of stabler parts to be of an amazing slenderness, if we consider, that, though in other hairy animals, the optick, or some other of the larger nerves do, I know not how many times, in thickness and circuit, surpass a hair of the same animal; yet in a cheese-mite, though none of the largest of those creatures, we have divers times manifestly seen, as is before intimated, single hairs that grow upon the legs.

Another way there is, that I employed to give men cause to think, that the invisible effluvia of bodies, that wander through the air, may be strangely minute; and this was by shewing how small a fragment of matter may be resolved into particles minute enough to associate themselves in such numbers with a fluid so much more dense than air, as water is, as to impart a determinate colour to the whole liquor. What I did with cochineal in prosecution of this design, my experiments about colours may inform you; but I shall now relate the success of an attempt made another way, for which perhaps some of your friends, the chymists, will thank me; though I was not solicitous to carry on the experiment very far with gold, not because I judged that less divisible into a number of coloured particles, but because I found, as I expected, that the paleness of the native colour of the gold may make it in the end less conspicuous, though, if I had then had by me a menstruum, as I sometimes had, that would dissolve gold blood-red, perhaps the experiment with gold would have surpassed that, which it is now time I should begin to relate, as soon as I have hinted to you by the way, that, for variety's sake, I made a trial with copper calcined *per se*, that I might not be accused of having omitted to employ a metal, whose body chymists suppose to be much opened by calcination. And though the event were notable, even in comparison of that of the experiment made with cochineal, yet my conjectures inclined me much to prefer the way described in the following account.

We carefully weighed out in a pair of tender scales one grain of copper not calcined, but barely filed; and because, as we made choice of this metal for its yielding in most menstruums a blue, which is a deep and conspicuous colour, we also chose to make a solution, not in aqua fortis, or aqua regis, but the spirit of sal armoniack (as that is an urinous spirit) having found by former trials, that this menstruum would give a far deeper solution than either of the others. This lovely liquor, of which we used a good proportion, that all the copper might be thoroughly dissolved, we put into a tall cylindrical glass of about four inches in diameter, and by degrees poured to it of distilled water, which is more proper in this case than common water, which has oftentimes an inconvenient saltishness, till we had almost filled the glass, and saw the colour grow ſomewhat pale, without being too dilute to be manifeſt; and then we warily poured this liquor into a conical glaſs, that it might be the more eaſy to fill the veſſel ſeveral times to the ſame height. This conical glaſs we filled to a certain mark four times conſecutively, weighing it, and the liquor too, as often in a pair of excellent ſcales purpoſely made for ſtatical experiments, and which, though ſtrong enough to weigh ſome pounds in each ſcale, would, when not too much loaden, turn with about one grain. Theſe ſeveral weights of the glaſs, together with the contained liquor, we added together, and then carefully weighing the empty glaſs again, we deducted four times its weight from the above-mentioned ſum, and thereby found the weight of the liquor alone, to be that, which reduced to grains, amounted to 28,534, ſo that a grain of copper, which is not full half ſo heavy in ſpecie as fine gold, communicated a tincture to 28,534 times its weight.

But now, if you pleaſe to take notice, that the ſcope of my experiment was to ſhew, into what a number of parts one grain of copper might be divided; you will allow me to conſider, as I did, that this multitude of parts muſt be eſtimated by the proportion, not ſo much in weight as in bulk, of the tinging metal to the tinged liquor; and conſequently, ſince that divers hydroſtatical trials have informed me, that the weight of copper to the weight of water of the ſame bulk is *proximè* as nine to one, a grain-weight of copper is in bigneſs but the ninth part of as much water as weighs a grain; and ſo the formerly-mentioned number of the grains of water muſt be multiplied by nine, to give us the proportion between the tinging and tinged bodies, that is, that a ſingle grain of copper gave a blueneſs to above 256,806 parts of limpid water, each of them as big as it. Which, though it may ſeem ſtupendous, and ſcarce credible, yet I thought fit to proſecute the experiment ſomewhat farther, by pouring all the liquor out of the tall cylindrical glaſs into another clean veſſel, whence filling the conical glaſs twice, and emptying it as often into the ſame cylindrical glaſs, the third time I filled the conical glaſs with colourleſs diſtilled water, and pouring that alſo into the cylindrical glaſs, we found the mixed liquor to have yet a manifeſt, though but a pale blueneſs. And laſtly, throwing away what was in the cylindrical glaſs, we poured into it, out of the ſame conical glaſs, equal parts of diſtilled colourleſs water, and of the tincted liquor we had formerly ſet a-part in the clean veſſel; and found that though the colour were very faint and dilute, yet an attentive eye could eaſily diſcern it to be blueiſh; and ſo it was judged by an intelligent ſtranger, that was brought in to look upon it, and was deſired to diſcover of what colour he thought it to be. Whereby it appears, that one grain of copper was able to impart a colour to above double the quantity of water above-mentioned.

This experiment I have allowed myſelf to be the longer and more particular in relating, both becauſe I know not that any ſuch has been hitherto either made or attempted, and becauſe it will probably gratify your chymiſts, that love to have the tinctures of metals believed very diffuſive; and becauſe if circumſtances were not added, it would ſeem to you as well incredible, as perhaps it does ſeem ſtupendous, that a portion of matter ſhould be able to impart a conſpicuous colour to above 256,806 times its bulk of water, and a manifeſt tincture to above 385,200 (for ſo it did, when the proportion of the tinged part to the whole mixture made of it, and the untinged part, was as 2 to 31) and a faint, but yet diſcernible and diſtinguiſhable colour, to above five hundred and thirteen thouſand ſix hundred and twenty times its bulk of water.

CHAP.

CHAP. IV.

It were easy for me (*Pyroph.*) to give you several instances, to shew, that the effluvia of liquors may get in at the pores of bodies, that are reputed of a close texture; but I shall at present forbear to mention such examples, not only because they belong to another place *, where I take notice of them, but because many such would not seem so remarkable, nor be so considerable to our present purpose, as a few taken from bodies that are not fluid.

And first, it is delivered by writers of good credit, that several persons (for the experiment does not hold in all) by barely holding for some time dried cantharides in their hands, have been put to much pain at the neck of the bladder, and have had some other parts ministering to the secretion of urine sensibly injured. That this is true, I am induced to believe, by what I have elsewhere related to you of the unwelcome experiment I had of the effect of cantharides applied but outwardly to my neck, and that unknown to me, upon the urinary passages; and that these operations are due to material effluxes, which, to get into the mass of blood, must pass through the pores of the skin, you will not, I presume, put me to prove.

Scaliger Exercit. 186. relates, that in *Gascony*, his country, there are spiders of that virulency, that if a man treads upon them to crush them, their poison will pass through the very soles of his shoes. Which story, notwithstanding the reputation of the author, I should perhaps have left unmentioned, because of a much stranger about spiders, which he relates in the same section, but that I met with one that is analogous in the diligent *Piso*'s late history of *Brasil*; where, having spoken of another venemous fish of that country, and the antidotes he had successfully used to cure the hurts it inflicts, he proceeds to that fish the natives call *Amoreatim*, of one kind whereof, called by the Portugals *Peize Sola*, his words are these; *Quæ mira sanè efficacia non solum manum vel levissimo attractu, sed & pedem, licet optimè calceatum, piscatoris incautè pisciculum conterentis, paralysi & stupore afficit, instar torpedinis Europeæ, sed minus durabili.* Lib. 5. cap. 14.

What I shall ere long have occasion to tell you of the power of the *Torpedo*, and some other animals, to affect the hand and arm of him that strikes them, seems applicable to the matter under consideration: for, though their affecting the striker at a distance may very well be ascribed to the stupefactive, or other venemous exhalations, that expire (and perhaps are as it were darted) from the animal irritated by the stroke, and are breathed in together with the air they infect; yet their benumbing, or otherwise affecting the arm that struck them, rather than any other part, seems to argue, that the poisonous steams get in at the pores of the skin of the limb, and so stupefy, or otherwise injure, the nervous and musculous parts of it.

Other examples belonging to this section may be referred hither from divers other places in these papers about occult qualities, and therefore I shall only add here, that most remarkable proof, That some emanations, even of solid bodies, may be subtil enough to get through the pores, even of the closest bodies; which is afforded us by the effluvia of the loadstone, which are by magnetical writers said to penetrate, without resistance, all kind of bodies. And though I have not tried this in all sorts, yet having tried it in metals themselves, I am apt to think, the general rule admits of very few exceptions, especially, if that can be fully made out, which is affirmed about the perviousness of

* A Discourse of pores of bodies, and figures of corpuscles.

glass to the effluxions of the loadstone. For, not only glass is generally reputed to be as close a body as any is, but (which weighs more with me) I have by trials purposely made, had occasion to admire the closeness of very thin pieces of glass. But the reason why I just now expressed myself with an If, was, because I was not entirely satisfied with the proof wont to be acquiesced in, of the perviousness of glass; namely, that in dials and sea-compasses, that are covered with plates of glass, the needle may be readily moved to and fro by a loadstone held over it. For these plates being commonly but fastened on with wax, or at best with cement, a sceptick may pretend, that the magnetical effluvia pass not through the glass, but through that much more pervious matter that is employed to secure the commissures, only from the access of the air. To put then the matter past doubt, I caused some needles to be hermetically sealed up in glass-pipes, which being laid upon the surface of water (whereon, by reason of the bigness of the cavities, they would lightly float) the included needles did not only readily feel the virtue of an externally applied loadstone (though but a weak one) but complied with it so well, that I could easily, by the help of the needle, lead, without touching it, the whole pipe, this was shut up in, to what part of the surface of the water I pleased. And I also found that by applying a better loadstone to the upper part of a sealed pipe, and a needle in it, I could make the needle leap up from the lower part, as near to the loadstone, as the interposed glass would give it leave.

But I thought it would be more considerable, to manifest that the magnetical effluvia, even of such a dull body as the globe of the earth, would also penetrate glass. And though this seem difficult to be tried, because no ordinary loadstone, nor any iron touched by it, was to be employed to work on the included iron; yet I thought fit to attempt it after this manner: I took a cylindrical piece of iron, of about the bigness of one's little finger, and between half a foot and a foot long (for I had formerly observed, that the quantity of unexcited iron furthers its operation upon excited needles) and having hermetically sealed it up in a glass pipe but very little longer than it, I supposed that if I held it in a perpendicular posture, the magnetical effluvia of the earth, penetrating the glass, would make the lower extream of the iron answerable to the north pole; and therefore having applied this to the point of the needle in a dial, or sea-compass, that looked towards the north (for authors mean not all the same thing by the northern pole of a needle, or loadstone) I presumed it would, according to the laws magnetical (elsewhere mentioned) drive it away, which accordingly it did. And having for farther trial inverted the included iron (so that the end, which was formerly the lowermost, was now the uppermost) and held it in a perpendicular posture, just under the same point of the needle, that extreme of the iron-rod, which before had driven away this point, being by this inversion become, in a manner, a south-pole did, according to the same laws) attract it: by which sudden change of poles, merely upon the change of situation, it also appeared, that the iron owed its virtue only to the magnetism of the earth, not that of another loadstone, which would not have been thus easily alterable. And this experiment I the more particularly relate, because this is not the only place where I have occasion to make use of it.

CHAP. V.

Another proof of the great subtilty of effluviums may be taken from the small decrement of weight or bulk, that a body may suffer by parting with great store of such emanations.

That

THAT bodies, which infused in liquors impregnate them with new qualities suitable to those of the immersed bodies, do so by imparting to them somewhat of their own substance, will, I presume, be readily granted by those that conceive not how one body should communicate to another a solitary and naked quality, unaccompanied by any thing corporeal to support and convey it. But I would not have you think, *Pyrophilus*, that the only matter of fact I have to countenance this notion, is that experiment, which has convinced divers chymists and physicians, otherwise not friends to the corpuscular philosophy, that medicines may operate without any consumption of themselves. For though divers of these, some of them learned men, have confidently written, that glass of antimony, and crocus metallorum, being either of them infused in a great proportion of wine, will make it vomitive; and if that liquor be poured off, and new be poured on, every new portion of such liquor will be impregnated with the same virtue, and this though the liquor be changed a thousand times, and yet the antimonial glass or crocus will continue the same, as well in weight as virtue; and though thence some of them, especially chymists, argue, that some metals work without imparting any thing substantial, but only, as *Helmont* speaks of some of his arcana, by irradiation: yet, I confess, I have some doubts, whether the experiment have been competently tried, and shall not fully acquiesce in what has been said, till some skilful experimenter deliver it upon his own trial, and acquaint us too, with what instruments, and what circumspection he made it. For besides that the ingeniousest physicians I have questioned about it, acknowledged the taste, and sometimes the colour of the wine, to be altered by the infused mineral, I could not acquiesce in the affirmation of an ordinary chymist, or apothecary, or even physician, if he should barely aver that he had weighed an antimonial medicine before it was put to infuse, and after the infusion ended, and observed no decrement of weight. For I have had too much experience (as I elsewhere mention) of the difficulty of making exact statical trials; not to know that such scales as are wont to be employed by chymists and apothecaries in weighing drugs, are by no means fit to make trials with the nicety, which that I am speaking of requires: it being easy, even with the better sort of such unaccurate scales, especially if they be not suspended from some fixed thing, but held with the hand, to mistake half a grain, or a grain; and perhaps a greater quantity, and at least more than by divers of the experiments of this essay appears necessary to be spent upon the impregnating of a considerable proportion of liquor, with corporeal effluxions. Besides that if, when the beaten crocus, or glass, be taken out of the wine to be weighed again, the experimenter be not cautious enough to make allowance for the liquor that will adhere to the medicament, it is plain that he may take notice of no decrement of weight, though there may be really effluviums of the mineral amounting to several grains, imbibed by the liquor. And though he be aware of this, and dry the powder, yet it is not so easy, even for a skilful man, to be sure that none of the more viscous particles of the liquor stick to the mineral, and being sensible upon the balance, though not to the eye or hand, repair the recess of those emetick corpuscles, that diffused themselves into the menstruum. And the sense of these difficulties put me upon the attempting to make so noble an experiment with excellent scales, and the care that it deserves: but, after a long trial, an unlucky accident frustrated at last my endeavours. But though, till competent relators give us an account of this matter upon their own trial, and repeat the infusion very much oftener, than, for aught I find, any man has yet done, I must not acquiesce in all that is said of the impregnation of wine, or other liquors by antimonial glass and crocus metallorum; yet, that after divers repeated infusions, the mineral substance should not be sensibly diminished in bulk or virtue, may well suffice to make this instance, though not the only or chief that may be be brought for our purpose, yet a pertinent one to it. For, that there is a powerful emetick quality imparted to the liquor, is manifest by experience; and that the mineral does not impart this virtue, as it were, by irradiation, but by substantial effluxion, seems to me very probable; not only because I conceive not, how this can be done otherwise, but because, as it is noted above, the wine does oftentimes change colour by being kept a competent time upon the mineral, as if it drew thence a tincture; and even when it is not discoloured, I think it unsafe to conclude, that the menstruum has not wrought upon it. For I have kept good spirit of vinegar for a considerable time, upon finely powdered glass of antimony made *per se*, without finding the spirit to be all tinged, though it is known, that antimonial glass is soluble in spirit of vinegar, as mine afterwards appeared to be, by a longer digestion in the same liquor. But there may be a great number of minute particles dissolved in the menstruum before they be numerous enough to change the colour of it. And with this agrees very well what is observed, that though too great a quantity of the prepared antimony be put into the liquor, yet it will not be thereby made too strongly emetick. For the wine, being a menstruum, will, like other menstruums, be impregnated but to a certain measure, without dissolving the overplus of the matter that is put into it; and Mars, which is a harder and heavier body than glass of antimony, is itself in part soluble in good Rhenish, or other white wine (and that in no long time) and sometimes even in water.

I do not therefore reject the emetick infusion, as unfit to have a place in this chapter, but till the experiment have been a little more accurately made, I think it inferior, as to our purpose, to some of the instances to be met with in the next chapter, and perhaps also to that mentioned by *Helmont*, and tried by more than one of my acquaintance, concerning the virtue of killing worms, that mercury imparts to the water or wine, wherein it has been long enough infused, or else for a while decocted. Though quicksilver given in substance is commended as an effectual medicine against worms, not only by many professed * spagirists, but by divers † methodists of good note. And though some other things, chymical and philosophical, keep me from being of their opinion, who think, that in this case the mercury impregnates the liquor, as it were, by irradiation, rather than in a corporeal manner; yet the eye does not perceive, that even limpid water takes any thing from clean and well-purged mercury, which we know, that divers corrosive liquors themselves will not work upon.

To this instance I must add one, that is yet freer from exceptions, which is, that having for curiosity sake suspended in a pair of exact scales, that would turn with a very small part of a grain, a piece of ambergris bigger than a walnut, and weighing betwixt an hundred and six score grains, I could not in three days and a half, that I had opportunity to make the trial, discover, even upon that balance, any decrement of weight in the ambergris; though so rich a perfume, lying in the open air, was like in that time to have parted with good store of odoriferous steams. And a while after suspending a lump of assa fœtida five days and a half, I found it not to have sustained any discernible loss of weight, though, in spite of the unfavourable cold weather, it had about it a neighbouring atmosphere replenished with fœtid exhalations. And when twelve or fourteen hours after, perhaps upon some change of weather, I came to look upon it, though I found, that in that time the æquilibrium was somewhat altered, yet the whole lump had not lost half a quarter of a grain; which induced me to think, that there may perhaps be steams discernible even by our nostrils, that are far more subtil than the odorous exhalations of

* As Quercetanus, Libavius, Zabata, Burggravius. † As Vidius, Paræus, Cæsalpinus, &c.

spices

spices themselves. For having, in very good scales, suspended in the month of *March* an ounce of nutmegs, it lost in about six days five grains and a half. And an ounce of cloves, in the same time, lost seven grains and five-eighths.

You will perhaps wonder, why I do not prefer, to the instances I make mention of in this chapter, that, which may be afforded by the loadstone, that is acknowledged continually to emit multitudes of magnetical steams without decrement of weight. But though I have not thought fit to pass this wholly under silence, yet I forbear to lay so much stress on it, not only because my balances have not yet satisfied me about the effluvia of loadstones (for I take them not all to be equally diffusive of their particles) but because I foresee it may be doubted, whether loadstones, like odorous bodies, do furnish afresh of their own, all the corpuscles that from time to time issue from them; or, whether they be not continually repaired, partly by the return of the magnetical particles to one pole, that sallied out of the other; and partly by the continued passage of magnetical matter, supplied by the earth, or other mundane bodies, which make the pores or channels of the loadstone their constant thorough-fares.

I doubt not but it will make it more probable, that a small quantity of matter being scattered into invisible effluvia, may be exceedingly rarefied and expanded, if it can be made appear, that this little portion of matter shall, for a considerable time, emit multitudes of visible parts, and that in so close an order among themselves, as to seem in their aggregate but one entire liquor, endowed with a stream-like motion, and a distinct superficies, wherein no interruption is to be seen, even by an eye placed near it. To devise this experiment, I was induced, by considering, that hitherto all the total dissolutions, that have been made of pigments, have been in liquors naturally cold, and consisting probably of much less subtil, and certainly of much less agitated parts, than that fluid aggregate of shining matter, that we call flame; whereas I argued, that if one could totally dissolve a body composed of parts so minute as those of a metal, into actual flame, and husband its flame so, as that it should not immoderately waste, I should thereby dissolve the metal in a far more subtil menstruum than our common water, or aqua fortis, or aqua regis, or any other known menstruum I have yet employed. And consequently, the attenuation and expansion of the metal in this truly igneous menstruum would much surpass, not only what happens in ordinary metalline solutions, but possibly also what I have noted in the third chapter of this essay, about the strange diffusion of copper dissolved in spirit of urine and water. In prosecution of this design, I so prepared one single grain of that metal, by a way that I elsewhere teach, that it was dissolved in about a spoonful of an appropriated menstruum. And then having caused a small glass lamp to be purposely blown to contain this liquor, and fitted it with a socket and wick, we lighted the lamp, which, without consuming the wick, burnt with a flame large enough, and very hot, and seemed to be all the while of a greenish blue, as if it were but a finer and shining solution of copper. And yet this one grain of prepared metal tinged the flame, that was from moment to moment produced, during no less than half an hour and six minutes. And now if we consider, that in this flame there was an uninterrupted succession of multitudes of coloured particles newly extricated, and flying off in every of those many parts wherein a minute of time may either actually or mentally be divided; and if we consider flame as a light and very agitated body, passing with a stream upwards through the air, and if we also consider the quantity of liquor, that would (as I shall by and by tell you) run through a pipe of a much lesser diameter than that flame, within the compass of the fore-mentioned time: what a quantity of the streaming fluid, we call flame, if it could have been preserved, and collected into one body, may we suppose, would appear to have iſſued out of one grain of copper in the ſpace of thirty-ſix minutes; and what a multitude of metalline corpuſcles may we ſuppoſe to have been ſupplied for the tinging of that flame, during ſo long a time? ſince a cylindrical ſtream of water falling but through a very ſhort pipe of glaſs, conſtantly ſupplied with liquors, did paſs at ſuch a rate, that though the aqueous cylinder ſeemed more ſlender by half, or perhaps by two-thirds, or better than the flame, yet we eſtimated, by the help of a minute-watch, and a good pair of ſcales, that, if I had had conveniencies to let it run long enough, the water effluxed in thirty-ſix minutes, the time of the flame's duration, would have amounted to above nine gallons, or, reckoning a pint of water to contain a pound of ſixteen ounces, ſeventy-two pounds.

C H A P. VI.

THE laſt ſort of inſtances I ſhall propoſe to ſhew the ſtrange ſubtilty of effluvia, is of ſuch as diſcover the great quantity of ſpace that may, by a ſmall quantity of matter, when rarefied or diſperſed, be either filled as to ſenſe, or, at leaſt, made (as they ſpeak) the ſphere of its activity.

To manifeſt this truth, and thereby as well confirm the foregoing chapter, as make out what is deſigned in this, I ſhall endeavour to ſhew, and help your imagination to conceive, how great a ſpace may be impregnated with the effluxions of a body, oftentimes without any ſenſible, and oftener without any conſiderable decrement in bulk, or weight, of the body that affords them. And in order to this, though I ſhall not pretend to determine preciſely how little the ſubſtances I am to inſtance in, would waſte upon the balance, becauſe you will very eaſily ſee, they are not that way to be examined; yet I preſume you will as eaſily grant, that the decrement of weight would be but inconſiderable, ſince, of ſuch light ſubſtances, the loſs even of bulk is ſo; which laſt clauſe I ſhall now attempt to make good, by ſetting down ſome obſervations, partly borrowed from the writings of approved phyſicians, and partly, that my friends and I have made about the durable evaporation of ſuch ſmall particles of the effluxions of animals, as are actually not to be diſcerned by the eye to have any of thoſe things ſticking to them, which are ſo very long in flying ſucceſſively away.

IT is wont to be ſomewhat ſurpriſing to men of letters, when they firſt go a hawking with good ſpaniels, to obſerve with how great ſagacity thoſe dogs will take notice of, and diſtinguiſh by the ſcent, the places where partridges, quails, &c. have lately been. But I have much more wondered at the quick ſcent of an excellent ſetting-dog, who, by his way of ranging the fields, and his other motions, eſpecially of his head, would not only intimate to us the kinds of game, whoſe ſcent he chanced to light on, but would diſcover to us where partridges have been, though perhaps without ſtaying in that place, ſeveral hours before, and aſſiſt us to gueſs how long they had been gone before we came.

I HAVE had ſtrange anſwers given me in *Ireland*, by thoſe who make a gain, if not an entire livelihood, by killing of wolves in that country (where they are paid ſo much for every head they bring in) about the ſagacity of that peculiar race of dogs they employ in hunting them; but not truſting much to thoſe relators, I ſhall add, that a very ſober and diſcreet gentleman of my acquaintance, who has often occaſion to employ blood-hounds, aſſures me, that if a man have but paſſed over a field, the ſcent will lie, as they ſpeak, ſo as to be perceptible enough to a good dog of that ſort for ſeveral hours after. And an ingenious hunter aſſures me, that he has obſerved, that the ſcent

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of a flying and heated deer will sometimes continue upon the ground from one day to the next following.

And now we may consider these three things; first, that the substance left upon the grass, or ground, by the transient tread of a partridge, hare, or other animal, that does but pass along his way, does probably communicate to the grass, or ground, but some of those effluxions that transpire out of his feet, which being small enough to escape the discernment of the eye, may probably not amount to one grain in weight, or perhaps not to the tenth part of it. Next, that the parts of fluid bodies, as such, are perpetually in motion, and so are the invisible particles that swim in them, as may appear by the dissolution of salt or sugar in water, and the wandering of aqueous vapours through the air, even when the eye perceives them not. And thirdly, that though the atmosphere of one of these small parcels of the exhaling matter we are speaking of may oftentimes be exceeding vast, in comparison of the emittent body, as may be guessed by the distance at which some setters, or blood-hounds, will find the scent of a partridge, or deer; yet in places exposed to the free air, or wind, it is very likely, that these steams are assiduously carried away from their fountain, to maintain the forementioned atmosphere for six, eight, or more hours, that is, as long as the scent has been observed to lie, there will be requisite a continual recruit of steams succeeding one another: and, that so very small a portion of matter, as that, which we were saying the *fomes* of these steams may be judged to be, being sensibly to impregnate an atmosphere incomparably greater that itself, and supply it with almost continual recruits, we cannot but think that the steams it parts with, must be of an extreme, and scarce conceiveable minuteness.

And we may further consider, that the substances which emit these steams, being such as newly belonged to animals, and were, for the most part, transpired through the pores of their feet, must be in likelihood a far more evaporable and dissipable kind of bodies, than minerals or adust vegetables, such as gunpowder is made of; so that if the grains of gunpowder emit effluviums capable of being, by some animals, perceived at a distance by their smell, one may probably suppose, that the small grains of this powder may hold out very many times longer to supply an atmosphere with odorable steams, than the corpuscles left on the ground by transient animals.

Now though it be generally agreed on, that very few birds have any thing near so quick a sense of smelling, as setting-dogs, or blood-hounds, yet, that the odour of gunpowder, especially when assisted by the steams of the caput mortuum of powder formerly fired in the same gun, may by fowls be smelt at a notable distance, particularly when the wind blew from me towards them, I often persuaded myself I observed, especially as to crows, when I went a shooting; and was confirmed in that opinion, both by the common tradition, and by sober and ingenious persons much exercised in the killing of wild-fowl, and of some four-footed beasts.

I had forgotten to take notice of one observation of the experienced *Julius Palmarius:* whence we may learn, that beasts may leave upon the vegetables, that have touched their bodies for any time, such corpuscles, as though unheeded by other animals, may, when eaten by them, produce in them such diseases as the infected animals had. For this author writes, in his useful tract *De morbis contagiosis*, that he observed horses, beeves, sheep, and other animals, to run mad upon the eating of some of the straw on which some mad swine had lain.

And now to resume and prosecute our former discourse, you may take notice, that the effluvia, mentioned to have been smelt by animals, are, though invisible, yet big enough to be the objects of sense; so that it is not improbable, that among the steams, that no ſenſe can immediately perceive, there ſhould be ſome far more ſubtil than theſe, and conſequently capable of furniſhing an atmoſphere much longer, without quite exhauſting the effluviating matter that afforded them.

Lib. VI. Obſer. 22 FORESTUS, an uſeful author, recites an example of peſtilential contagion long preſerved in a cobweb.

ALEXANDER BENEDICTUS writes alſo, that at *Venice* a flock-bed did for many years harbour a peſtiferous malignity to that degree, that when afterwards it came to be beaten, it preſently infected the by-ſtanders with the plague.

Lib. IV. de Feb. cap. 3. AND the learned *Sennertus* himſelf relates, that in the year one thouſand five hundred and forty-two, there did in the city of *Uratiſlavia*, vulgarly *Breſlaw*, where he afterwards practiſed phyſick, die of the plague, in leſs than ſix months, little leſs than ſix thouſand men, and that from that time the peſtilential contagion was kept folded up in a linen cloth about fourteen years, and at the end of that time being diſplayed in another city, it began a plague there, which infected alſo the neighbouring towns, and other places.

Lib. III. Con. 17. TRINCAVELLA makes mention of a yet laſtinger contagion, which occaſioned the death of ten thouſand perſons, that lay lurking in certain ropes, with which, at *Juſtinopolis*, thoſe, that died of the plague, had been let down into the graves.

BUT though none of theſe relations ſhould, to ſome criticks, appear ſcarce credible, it may be objected, that all theſe things, wherein this contagion reſided, were kept cloſe ſhut up, or at leaſt were not expoſed to the air. Wherefore having only intimated, that the exception, which I think is not irrational, would, though never ſo true, but leſſen the wonder of theſe ſtrange relations, without rendering them unfit for our preſent purpoſe, I ſhall add, that though it is the opinion of divers learned phyſicians, that the matter harbouring contagion cannot laſt above twenty, or a few more days, if the body it adheres to be expoſed to the free air and the wind; and though I am not forward to deny, that their judgment may hold in ordinary caſes; yet I muſt not deny neither, that a contagion may ſometimes happen to be much more tenacious, and obſtinate: of which I ſhall give but that one, almoſt recent inſtance, obſerved by the learned *Diemerbroek*, in his own apothecary, who having but removed with his foot, from one ſide to the other of a little arbour in his garden, ſome ſtraw, that had lain under the pallet, on which near eight months before a bed had lain, wherein a ſervant of the apothecary's, that recovered, had been ſick of the plague; the infectious ſteams preſently invaded the lower part of his leg, and produced a pungent pain and bliſter, which turned to a peſtilential carbuncle, that could ſcarce be cured in a fortnight after, though, during that time, the patient were neither feveriſh, nor, as to the reſt of his body, ill at eaſe. This memorable inſtance, together with ſome others of the like kind, that our author obſerved in the ſame city of *Nimeguen*, obtained, not to ſay extorted, even from him, this confeſſion; which I add, becauſe it contains ſome conſiderable, and not yet mentioned circumſtances of the recited caſe: *Hoc exemplo medicorum doctrina de contagio in fomite latente ſatis confirmatur. Mirum tamen eſt, hoc contagium tanto tempore in prædicto ſtramine potuiſſe ſubſiſtere, utpote quod tota hyeme ventis & pluviis* (he adds in another place) *nivibus & frigori, expoſitum fuiſſet.*

(Lib. IV. de Peſte.)

AND now I will ſhut up this chapter with an inſtance, that ſome will think, perhaps, no leſs ſtrange than any of the reſt; which is, that though they that are ſkilful in the perfuming of gloves, are wont to imbue them with but an inconſiderable quantity of odoriferous matter, yet I have by me a pair of *Spaniſh* gloves, which I had by the favour of your fair and virtuous ſiſter *(F.)* that were ſo ſkilfully perfumed, that partly by her, partly by thoſe that preſented them her

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as a rarity, and partly by me, who have kept them ſeveral years; they have been kept about eight or nine and twenty years, if not thirty, and they are ſo well ſcented, that they may, for aught I know, continue fragrant divers years longer. Which inſtance if you pleaſe to reflect upon, and conſider, that ſuch gloves cannot have been carried from one place to another, or ſo much as uncovered, as they muſt often have been, in the free air, without diffuſing from themſelves a fragrant atmoſphere, we cannot but conclude thoſe odorous ſteams to be unimaginably ſubtil, that could for ſo long a time iſſue out, in ſuch ſwarms, from a little perfumed matter lodged in the pores of a glove, and yet leave it richly ſtocked with particles of the ſame nature; though, eſpecially by reaſon of ſome removes, in which I took not the gloves along with me, I forgot ever ſince I had them, to keep them ſo much as ſhut up in a box.

OF THE GREAT EFFICACY OF EFFLUVIUMS.

CHAP. I.

THEY that are wont, in the eſtimates they make of natural things, to truſt too much to the negative informations of their ſenſes, without ſufficiently conſulting their reaſon, have commonly but a very little and ſlight opinion of the power and efficacy of effluviums: and imagine that ſuch minute corpuſcles (if they grant that there are ſuch) as are not, for the moſt part of them, capable to work upon the tendereſt and quickeſt of ſenſes the ſight, cannot have any conſiderable operation upon other bodies. But I take this to be an error, which, as it very little becomes philoſophers, ſo it has done no little prejudice to philoſophy itſelf, and perhaps to phyſick too. And therefore though the nature of my deſign at preſent did not require it, yet the importance of the ſubject would invite me to ſhew, that this is as ill-grounded as prejudicial a ſuppoſition.

AND indeed if we conſider the ſubject attentively, we may obſerve, that though it be true, that, *cæteris paribus*, the greatneſs of bodies doth, in moſt caſes, contribute to that of their operation upon others, yet matter or body being, in its own preciſe nature, an unactive or moveleſs ſubject, one part of the maſs acts upon another, but upon the account of its local motion, whoſe operations are facilitated and otherwiſe diverſified by the

the ſhape, ſize, ſituation, and texture both of the agent and of the patient. And therefore if corpuſcles, though very minute, be numerous enough, and having a competent degree of motion, even theſe ſmall particles, eſpecially if fitly ſhaped, when they chance to meet with a body, which the congruity of its texture diſpoſes to admit them at its pores, and receive their either friendly or hoſtile impreſſions, may perform ſuch things in the patient, as viſible and much groſſer bodies, but leſs conveniently ſhaped and moved, would be utterly unable on the ſame body to effect.

And that you may with the leſs difficulty allow me to ſay, that the effluviums of bodies, as minute as they are, may perform conſiderable things, give me leave to obſerve to you, that there are at leaſt ſix ways, by which the effluviums of a body may notably operate upon another; namely, 1. By the great number of emitted corpuſcles. 2. By their penetrating and pervading nature. 3. By their celerity, and other modifications of their motion. 4. By the congruity and incongruity of their bulk and ſhape to the pores of the bodies they are to act upon. 5. By the motions of one part upon another, that they excite or occaſion in the body they work upon, according to its ſtructure. And, 6. by the fitneſs and power they have to make themſelves be aſſiſted, in their working, by the mere catholic agents of the univerſe. And though it may perhaps be ſufficiently proved, that there are ſeveral caſes wherein a body that emits particles, may act notably upon another body, by this or that ſingle way, of thoſe I have been naming; yet uſually the great matters are performed by the aſſociation of two, three, or more of them, concurring to produce the ſame effect. Upon which ſcore, when I ſhall in the following paper refer an inſtance, or a phænomenon, to any one of the forementioned heads, I deſire to be underſtood as looking upon that but as the head to which it chiefly relates, without excluding the reſt.

C H A P. II.

Taking thoſe things for granted, that have, I hope, been ſufficiently proved in the former tract about the ſubtilty of effluviums, I ſuppoſe it will readily be allowed, that the emanations of a body may be extremely minute; whence it may be rightly inferred, that a ſmall portion of matter may emit great multitudes of them.

Now, that the great number of agents may in many caſes compenſate their littleneſs, eſpecially where they act, or reſiſt *per modum unius*, as they ſpeak, men would perhaps the more eaſily grant, if they took notice to this purpoſe of ſome familiar inſtances.

We ſee, that not only leſſer land-floods, that overflow the neighbouring fields, but thoſe terrible inundations, that ſometimes drown whole countries, are made by bodies ſingly ſo ſmall and inconſiderable as drops of rain, when they continue to fall in thoſe multitudes we call ſhowers.

So the aggregates of ſuch minute bodies as grains of ſand, being heaped together in ſufficient numbers, make banks, wherewith greateſt ſhips are ſometimes ſplit, nay, and ſerve in moſt places for bounds to the ſea itſelf.

And though a ſingle corn of gunpowder, or two or three together, are not of force to do much miſchief, yet two or three barrels of thoſe corns, taking fire together, are able to blow up ſhips and houſes, and perform prodigious things.

But inſtead of multiplying ſuch inſtances, afforded by bodies of ſmall indeed, but yet viſible bulk, I ſhall (as ſoon as I have intimated, that the above-mentioned drops of rain themſelves conſiſt of convening multitudes of vapours moſt commonly inviſible in their aſcent) endeavour to make out what was propoſed, by two or three inſtances drawn from the operations of inviſible particles.

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AND firſt, we ſee, that though aqueous vapours be looked upon as the fainteſt and leaſt active effluviums, that we know of; yet when multitudes of them are in rainy weather diſperſed thorough the air, and are thereby qualified to work on the bodies expoſed to it, their operations are very conſiderable, not only in the diſſolution of ſalts, as ſea-ſalt, ſalt of tartar, *&c.* and in the putrefactive changes they produce in many bodies, but in the intumeſcence they cauſe in oak and other ſolid woods; as appears by the difficulty we often find in and before rainy weather, to ſhut and open doors, boxes, and other wooden pieces of work, that were before fit enough for the cavities they had been adjuſted to.

I MIGHT here urge, that though the ſtrings of viols and other muſical inſtruments are ſometimes ſtrong enough to ſuſtain conſiderable weights, yet if they be left ſcrewed to their full tenſion (as it frequently happens) they are oftentimes, by the ſupervening of moiſt weather, made to break, not without impetuoſity and noiſe. But it may ſuit better with my preſent aim, if I mention on this occaſion (what I elſewhere more fully take notice of) being deſirous to try, what a multitude even of aqueous ſteams may do, I cauſed a rope, that was long, but not thick, and was in part ſuſtained by a pully, to have a weight of lead ſo faſtened to the end of it, as not to touch the ground, and after the weight had leiſure allowed it to ſtretch the cord as far as it could, I obſerved, that in the moiſt weather the wateriſh particles that did inviſibly abound in the air, did ſo much work upon and ſhorten the rope, as to make it lift up the hanging weight, which was, if I miſ-remember not, about an hundred pounds.

THE inviſible ſteams iſſuing out of the walls of a newly plaiſtered or whited room, are not ſenſibly prejudicial to thoſe that do but tranſiently viſit it, or make but a very ſhort ſtay in it, though there be a charcoal-fire in the chimney; but we have many inſtances of perſons, that by lying for a night in ſuch rooms, have been the next morning, or ſooner, found dead in their beds, being ſuffocated by the multitude of the noxious vapours emitted during all that time.

AND here I think it proper to obſerve, that it may much aſſiſt us to take notice of the multitude of effluvia, and make us expect great matters from them, to conſider that they are not emitted from the body, that affords them, all at once, as hail-ſhot out of a gun, but iſſue from it, as the vaporous winds do out of an æolipile well heated, or waters out of a ſpring-head in continued ſtreams, wherein freſh parts ſtill ſucceed one another; ſo that though as many effluxions of a body as can be ſent out at one time, were numerous enough to act but upon its ſuperficial parts, yet the emanation of the next minute may get in a little farther, and each ſmalleſt portion of time ſupplying freſh recruits, and perhaps urging on the ſteams already entered, the particles may at length get into a multitude of the pores of the invaded body, and penetrate it to the very innermoſt parts.

C H A P. III.

I COME now to ſhew, in the ſecond place, that the ſubtil and penetrating nature of effluviums may, in many caſes, co-operate with their multitude in producing notable effects; and that there are effluviums of a very piercing nature, though we ſhall not now enquire upon what account they are ſo, we may evince by ſeveral examples. For not only the inviſible ſteams of good aqua-fortis and ſpirit of nitre do uſually in a ſhort time, and in the cold, ſo penetrate the corks wherewith the glaſſes that contained them, were ſtopped, as to reduce them into a yellow pap; but alſo the emanations of mercury have been ſometimes found in the form of coagulated, or even of running mercury, in the heads or or very bones of thoſe gilders, or venereal patients, that have too long, or too unadviſedly, been expoſed to the fumes of it, though they never took quickſilver in its groſs ſubſtance. Chymiſts too, often find in their laboratories, that the ſteams of ſulphur, antimony, arſenick, and divers other minerals, are able to make thoſe ſtagger, or perhaps ſtrike them down, that without a competent warineſs unlute the veſſels, wherein they had been diſtilled or ſublimed; of which I have known divers ſad examples. And of the penetrancy, even of animal ſteams, we may eaſily be perſuaded, if we conſider how ſoon in many plagues the contagious, though inviſible exhalations are able to reach the heart, or infect other internal parts; though in divers of theſe caſes the blood helps to convey the infection, yet ſtill the morbifick particles muſt get into the body before they can infect the maſs of blood. And in thoſe ſtupefactions that are cauſed at a diſtance by the torpedo, the parts mainly affected ſeem to be the nervous ones of the hand and arm, which are of the moſt retired and beſt fenced parts of thoſe members. And there is a ſpirit of ſal armoniack, that I make to ſmell to, whoſe inviſible ſteams, unexcited by heat, are of ſo piercing a nature, that not only they will powerfully affect the eyes and noſtrils, and throats, and ſometimes the ſtomachs too (yet without proving vomitive) of the patients they invade, but alſo when a great cold has ſo clogged the organs of ſmelling, that neither ſweet nor ſtinking odours would at all affect them, theſe piercing ſteams have not only in a few minutes both made themſelves a way, and, which is more, ſo opened the paſſages, that ſoon after the patient has been able to ſmell other things alſo. And by the ſame penetrating ſpirit, a perſon of quality was, ſome time ſince, reſtored to a power of ſmelling, which he had loſt for divers years (if he ever had it equally with other men) I could eaſily ſubjoin examples of this kind, but they belong to other places. And here I ſhall only add, that the ſteams of water itſelf, aſſiſted by warmth, are capable of diſſolving the texture of even hard and ſolid bodies, that are not ſuſpected to be ſaline; as appears by the philoſophical calcination (as chymiſts call it) wherein ſolid pieces of hartſhorn are brought to be eaſily friable into powder, by being hung over waters, whilſt their ſteams riſe in diſtillation, and without the help of furnaces. The exhalations that uſually ſwim every night in the air, and almoſt every night fall to the ground in the form of dews (which makes them be judged aqueous) are in many places of the torrid zone of ſo penetrating a nature, that, as eye-witneſſes have informed me, they would, in a very ſhort time, make knives ruſt in their ſheaths, and ſwords in their ſcabbards, nay, and watches in their caſes, if they did not conſtantly carry them in their pockets. And I have known even in *England* divers hard bodies, into which the vapours ſwimming in the air have inſinuated themſelves ſo far as to make them friable throughout. But of the penetration of effluviums, I have given, in ſeveral places, ſo many inſtances, that it is not neceſſary to add any here. And therefore to ſhew, that, as I intimated at the beginning of this chapter, the penetrancy and the multitude of effluviums may much aſſiſt each other, I ſhall now ſubjoin, that we muſt not for the moſt part look upon effluviums, as ſwarms of corpuſcles that only beat againſt the outſides of the bodies they invade, but as corpuſcles, which by reaſon of their great and frequently recruited numbers, and by the extreme ſmallneſs of their parts, inſinuate themſelves in multitudes into the minute pores of the bodies they invade, and often penetrate to the innermoſt of them; ſo that, though each ſingle corpuſcle, and its diſtinct action, be inconſiderable, in reſpect of the multitude of parts that compoſe the body to be wrought on; yet a vaſt multitude of theſe little agents working together upon a correſpondent number of the ſmall parts of the body they pervade, they may well be able to have powerful effects upon the body that thoſe parts conſtitute; as, in the caſe mentioned in the former chapter, the rope would not probably have been enabled to raiſe ſo great a weight, though a vehement wind

wind had blown againſt it, to make it loſe its perpendicular ſtraightneſs, but a vaſt multitude of watery particles, getting by degrees into the pores of the rope, might, like an innumerable company of little wedges, ſo widen the pores, as to make the threads or ſplinters of hemp, the rope was made up of, ſwell, and that ſo forcibly, that the depending weight could not hinder the ſhortening of the rope, and therefore muſt of neceſſity be raiſed thereby. And I have more than once known ſolid, and even heavy mineral bodies burſt in pieces by the moiſture of the air, though we kept them within-doors carefully ſheltered from the rain.

C H A P. IV.

THAT the celerity of the motion of very minute bodies, eſpecially conjoined to their multitudes, may perform very notable things, may be argued from the wonderful effects of fired gunpowder, *aurum fulminans*, of flames, that inviſibly touch the bodies they work on, and alſo whirlwinds, and thoſe ſtreams of inviſible exhalations and other aërial particles we call winds. But becauſe inſtances of this ſort ſuit not ſo well with the main ſcope of this tract, I ſhall not inſiſt on them, but ſubjoin ſome others, which, though leſs notable in themſelves, will be more congruous to my preſent deſign. That the corpuſcles, whereof odours conſiſt, ſwim to and fro in the air, as in a fluid vehicle, will by moſt, I preſume, be granted, and may be eaſily proved. But I have elſewhere ſhewn that the motion of the effluviums of ſome ſufficiently odorous bodies has too little celerity to make a ſenſible impreſſion on the organs of ſmelling, unleſs thoſe ſteams be aſſiſted to beat more forcibly upon the noſtrils by the air, which hurries them along with it, when it enters the noſtrils in the form of a ſtream, in the act of inſpiration. And I have by familiar obſervation of hunters, fowlers, and partly of my own, made manifeſt, that ſetting-dogs, hounds, crows, and ſome other animals, will be much more affected with ſcents, or the odorous effluvia of partridges, hares, gunpowder, &c. when the wind blows from the object towards the ſenſory, than when it ſits the contrary way, which way ſoever the noſtrils of the animal be obverted, ſo the air be imbued with the odorous ſteams; and conſequently the difference ſeems to proceed from this, that when the noſtrils are obverted to the wind, the current of the air drives the ſteams forcibly upon the ſenſory, which otherwiſe it does not.

THAT there is a briſkneſs of motion requiſite, and more than ordinarily conducive to electrical attractions, may be argued from the neceſſity that we uſually find by rubbing amber, jett, and other electrical bodies, to make them emit thoſe ſteams, by which it is highly probable their action is performed; and though I have elſewhere ſhewn that this precedent rubbing is not always neceſſary to excite all electrical bodies; yet in thoſe that I made to attract without it, it would operate much more vigorouſly after attrition; which I conceive makes a reciprocal motion amongſt the more ſtable parts, and does thereby, as it were, diſcharge and ſhoot out the attracting corpuſcles; whoſe real emiſſion, though it may be probably argued from what has been already ſaid, ſeems more ſtrongly provable by an obſervation that I made many years ago, and which I have been lately informed to have been long ſince made by the very learned *Fabri*. The obſervation was this; that if, when we took a vigorouſly excited electrick, we did, at a certain nick of time (which circumſtances may much vary, but was uſually almoſt as ſoon as the body was well rubbed) place it at a juſt diſtance from a ſuſpended hair, or other light body, or perhaps from ſome light powder; the hair, &c. would not be attracted to the electrick, but driven away from it, as it ſeemed, by the briſkly moving ſteams that iſſue out of the amber or other light body.

This argument I could confirm by another phænomenon or two, of affinity with this, if I ſhould not borrow too much of what I have elſewhere noted about the hiſtory of electricity.

I know a certain ſubſtance, which, though made by diſtillation, does in the cold emit but a very mild and inoffenſive ſmell, but when the veſſel that holds it is heated, though no ſeparation of conſtituent principles appear to be thereby made (the body being in all uſual trials homogeneous) the effluviums will be ſo altered, that I remember a virtuoſo, that, to ſatisfy his curioſity, would needs be ſmelling to it, when it was heated, complained to me, that he thought the ſteams would have killed him, and that the effluviums of ſpirit of ſal armoniack itſelf were nothing near ſo ſtrong and piercing as thoſe.

And even among ſolid bodies, I know ſome, which, though abounding much in a ſubſtance, wherein ſome rank ſmells principally reſide, yet (if they were not chafed) were ſcarce at all ſenſibly odorous; but upon the rubbing of them a little one againſt the other, the attrition making them, as it were, dart out their emiſſions, would in a minute or two make them ſtink egregiouſly.

And as the celerity of motion may thus give a vigour to the emanations of bodies, ſo there may be other modifications of motion that may contribute to the ſame thing, and are not to be wholly neglected in this place. For as we ſee that greater bodies do operate differingly, according to ſuch and ſuch modifications; as there is a great difference between the effects of a dart, or javelin, ſo thrown, as that its point be always forwards, and the ſame weapon, if it be ſo thrown, that during its progreſſive motion the extremes turn about the center of gravity, or ſome inward parts, as it happens, when boys throw ſticks to beat down fruit from the tops of trees; ſo there is little doubt to be made, but, that in corpuſcles themſelves, it is not all one, as to their effects, whether they move with, or without rotation, and whether in ſuch or ſuch a line, and whether with, or without undulation, trembling, or ſuch a kind of conſecution; and in ſhort, whether the motion have, or have not this or that particular modification; which, how much it may diverſify the effects of the bodies moved, may appear by the motion that the aërial particles are put into by muſical inſtruments. For though the effects of harmony, diſcord, and peculiar ſounds, be ſometimes very great, not only in human bodies, but, as we ſhall ſhew in the following tract, in organical ones too; the whole efficacy of muſick, and of ſounds, that are not extraordinary loud and different, ſeems, as far as it is aſcribable to ſonorous bodies, to depend upon the different manners of motion whereinto that air is put, that makes the immediate impreſſion on our organs of hearing.

C H A P. V.

I should now proceed to ſhew, how the celerity and other modes, that diverſify the motion of effluviums, may be aſſiſted to make them operative by their determinate ſizes and figures, and the congruity or incongruity which they may have upon that ſcore, with the pores of the groſſer bodies they are to work on; but I think it not fit to encroach upon the ſubject of another * tract, where the relation between the figure of corpuſcles, and the pores of groſſer bodies, is amply enough treated of. And therefore I ſhall only, in this place, take notice of thoſe effects of lightning, which ſeem referrible, partly to the

* Of the Pores of Bodies, and Figures of Corpuſcles.

celerity

celerity and manner of appulſe, and partly to the diſtinct ſizes and ſhapes of the corpuſcles that compoſe the deſtructive matter, and to the peculiar relation between the particles of that matter, and the ſtructure of the bodies they invade. I know, that many ſtrange things that are delivered about the effects of what the Latins call *fulmen*, which our Engliſh word lightning does not adæquately render, are but fabulous; but there are but too many that are not ſo; ſome of which I have been an eye-witneſs of, within leſs than a quarter of an hour after that the things happened. And though it be very difficult to explicate particularly many of theſe true phænomena, yet it ſeems warrantable enough to argue from them, that there may be agents ſo qualified, and ſo ſwiftly moved, that notwithſtanding their being ſo exceedingly minute, as they muſt be, to make up a flame, which is a fluid body, they muſt, in an imperceptible time, pervade ſolid bodies, and traverſing ſome of them, without violating their texture, burn, break, melt, and produce other very great changes in other bodies, that are fitted to be wrought on by them. And of this, I muſt not forget to mention this remarkable inſtance; that a perſon curious enough to collect many rarities, bringing me one day into the ſtudy, where he kept the choiceſt of them, I ſaw there, among other things, a fine pair of drinking-glaſſes, that were ſomewhat ſlender, but extraordinarily tall; they ſeemed to have been deſigned to reſemble one another, and made for ſome drinking entertainment. But before I ſaw them, that reſemblance was much leſſened by the lightning, that fell between them in ſo ſtrange a manner, that, without breaking either of them, that I could perceive, it altered a little the figure of one of them, near the lower part of the cavity; but the other was ſo bent, near the ſame place, as to make it ſtand quite awry, and give it a poſture that I beheld not without ſome amazement. And I cannot yet but look upon it as a very ſtrange thing, and no leſs conſiderable to our preſent purpoſe, that nature ſhould, in the free air, make of exhalations, and that ſuch as probably, when they aſcended, were inviſible, ſuch an aggregate of corpuſcles, as ſhould, without breaking ſuch frail bodies as glaſſes, be able in its paſſage thorough them, that is, in the twinkling of an eye, to melt them; which to do is wont, even in our reverberatory furnaces, to coſt the active flames a pretty deal of time.

And this calls into memory, that upon a time, hearing not far off from me ſuch a clap of thunder, as made me judge and ſay, that queſtionleſs ſome of the neighbouring places were thunder-ſtruck, I ſent preſently to make inquiry; which having juſtified my conjecture, I forthwith repaired to the houſe, where the miſchief was done by ſomething, which thoſe that pretended to have ſeen it coming thither, affirmed to be like a flame, moved very obliquely. To omit the hurt, that ſeemed to have been done by a wind, that accompanied it, or was perhaps produced by it, to divers perſons and cattle; that, which makes me here mention it, was, that obſerving narrowly what had happened in an upper room, where it firſt fell, I ſaw, that it had, in more than one place, melted the lead in its paſſage, though that poſſibly outlaſted not the twinkling of an eye, without breaking to pieces the glaſs caſements, or burning, that I took notice of, either the bed, or hangings, or any other combuſtible houſehold-ſtuff; though, near the window, it had thrown down a good quantity of ſolid ſubſtance of the wall, through which it ſeemed to have made its paſſage in or out. And that which made me the leſs ſcruple to mention this accident, is, that having curiouſly pried into the effects of the fulmen, not only in that little upper room, but in other parts of the houſe, beneath whoſe lowermoſt parts it ſeemed to have ended its extravagant courſe, I could not but conclude, that if ſo be it were the ſame fulmen, it muſt have more than once gone in and out of the houſe, and that the line of its motion was neither ſtraight, nor yet reducible to any curve or mixed line, that I had met with among mathematicians; but that, as I then told ſome of my friends, it moved to and fro in an extravagant manner, not unlike the irregular and wriggling motion of thofe fired fquibs, that boys are wont to make by ramming gunpowder into quills. But about thunder, more perhaps elfewhere. I fhall here only add, that whereas it is a known tradition, which my own obfervations heedfully made feem now and then to confirm, that vehement thunder, if beer be not very ftrong, will ufually (for I do no not fay always) four it in a day or two; if this degeneration be not one of the confequences of the great and peculiar kinds of the concuffions of the air that happens in loud thunder (in which cafe, the phænomenon will belong to the next difcourfe) the effect may probably be imputed to fome fubtil exhalations diffufed thorough the air, which, penetrating the pores of the wooden veffels, whofe contexture is not very clofe, imbue the liquor with a kind of acetous *ferment*: which conjecture I fhould think much confirmed by a trial, it fuggefted to me, if I had made it often enough to rely upon it. For confidering that the pores of glafs are ftraight enough to be impervious (for aught I have yet obferved) to the fteams or fpirituous parts of fulphur, as well as to other odorous exhalations, I thought it worth trying whether there be any fulphureous fteams or other corpufcles diffufed thorough the air in time of thunder, that would not be too grofs to get in at fuch minute pores as thofe of glafs. And accordingly, having hermetically fealed up both beer and ale apart, I kept them in fummer-time, till there happened a great thunder, a day or two after which the beer, which we drank, that was good before, being generally complained of, as foured by the thunder, I fuffered my liquors to continue at leaft a day or two longer, that the fouring fteams, if any fuch there were, might have time enough to operate upon them, and then breaking the glaffes, I found not that the liquors had been foured, though we had purpofely forborn to fill the glaffes, to facilitate the degeneration of the liquors. Perhaps it will be pardonable, on this occafion, to mention a practice, which is ufual in fome places where I have been, and particularly employed by a great lady, that is, a great houfekeeper and is very curious and expert in divers phyfical obfervations; for, talking with her about the remedies of the fouring of beer, and other drinks, by thunder, which is fometimes no fmall prejudice to her, fhe affirmed to me that fhe ufually found the practice I was mentioning, fucceed; and, that before the then laft great thunder, of which I had obferved the effects upon beer, fhe preferved hers by putting, at a convenient diftance, under the barrels, chafing-difhes of coals, when fhe perceived that the thunder was like to begin, which practice, if it conftantly fucceed, may put one a confidering, whether the fire do not, by rarefying the air, and difcuffing the fulphureous or other fteams, by altering them, or by uniting with them the exhalations of the coals, or by fome fuch kind of way, render ineffectual thefe fouring corpufcles, which perhaps require a determinate bulk and fhape, befides their being crouded very many of them together, to have their full operation on barrelled liquors. But thefe things are but mere conjectures, and therefore I proceed.

C H A P. VI.

THE fifth way, whereby effluviums may perform notable things, is the motion of one part upon another, that they may excite or occafion in the body they work on, according to its ftructure.

I SHALL, in the following tract, have occafion to fay fomething of the motions into which the internal parts of inanimate bodies may put one another; but the examples now produced are defigned to manifeft the efficacy that effluviums may, on the newly-mentioned accounts, have on organical and living bodies. To which inftances, it would yet

yet be proper to premiſe, that even inanimate and ſolid bodies may be of ſuch a ſtructure, as to be very much alterable by the appropriated effluviums of other bodies, as may be inſtanced in the power, that I have known ſome vigorous loadſtones to have, of taking away, in a trice, the attractive virtue of an excited needle, or giving a verticity directly contrary to the former, without ſo much as touching it.

And we may pertinently take notice of the attractive virtue of the loadſtone, as that which may afford us an eminent example of the great power of a multitude of inviſible effluviums, even from bodies that are not great, upon bodies that are inorganical or liveleſs; for, taking it for granted, what both the Epicureans, Carteſians, and almoſt all other corpuſcularian philoſophers agree in, that magnetiſm is performed by corporeal emiſſions, we may conſider, that theſe paſſing unreſiſtedly thorough the pores of all ſolid bodies, and even glaſs itſelf, which neither the ſubtileſt odours, nor electrical exhalations are obſerved to do, ſeem to be almoſt incredibly minute, and much ſmaller than any other effluviums, though themſelves too ſmall to be viſible; and yet theſe ſo incomparably little magnetical effluxions proceeding from vigorous loadſtones, will be able to take up conſiderable quantities of ſo ponderous a body as iron; inſomuch, that I have ſeen a loadſtone, not very great, that would keep ſuſpended a weight of iron, that I could hardly lift up to it with one arm; and I have ſeen a little one, with which I could take up above eighty times its weight. And theſe effluvia do not only for a moment faſten the iron to the ſtone, but keep the metal ſuſpended as long as one pleaſes.

This being premiſed, I come now to obſerve, that the chief effects of effluvia belonging to the fifth head are wrought upon animals, which, by virtue of their curious and elaborate ſtructure, have their parts ſo connected and otherwiſe contrived, that the motions or changes that are produced in one, may have, by the conſent of parts, a manifeſt operation upon others, although perhaps very diſtant from it, and ſo framed, as to declare their being affected by actions that ſeem to have no affinity at all with the agents that work upon the part firſt affected.

I have ſhewn at large, in another * treatiſe, that a human body ought not to be looked upon merely as an aggregate of bones, fleſh, and other conſiſtent parts, but as a moſt curious, and a living engine, ſome of whoſe parts, though ſo nicely framed, as to be very eaſily affected by external agents, are yet capable of having great operations upon the other parts of the body they help to compoſe. Wherefore, without now repeating what is there already delivered, I ſhall proceed to deliver ſuch effects as are wrought on human bodies by theſe effluviums, without any immediate contact of the bodies that emit them.

And, firſt, not to mention light, becauſe its being, or not being a corporeal thing, is much diſputed, even among the moderns; it is plain, that our organs of ſmelling are ſenſibly affected by ſuch minute particles of matter as the fineſt odours conſiſt of. Nor do they always affect us preciſely as odours, ſince we ſee, that many perſons, both men and women, are by ſmells, either ſweet or ſtinking, put into troubleſome headachs.

If it were not almoſt ordinary, it would be more than almoſt incredible, that the ſmell of a pleaſing perfume ſhould preſently produce, in a human body, that immediately before was well and ſtrong, ſuch faintneſſes, ſwoons, loſs of ſenſible reſpiration, intumeſcence of the abdomen, ſeeming epilepſies, and really convulſive motions of the limbs, and I know not how many other frightful ſymptoms, that, by the unſkilful, are often taken for the effects of witchcraft, and would impoſe upon phyſicians themſelves, if

* The Uſefulneſs of Experimental Philoſophy.

their

their own, or their predeceſſors experience did not furniſh them with examples of the like phænomena produced by natural means. Thoſe ſymptoms manifeſt what the conſent of parts may do in a human body; ſince even morbifick odours, if I may ſo call them, by immediately affecting the organs of ſmelling, affect ſo many other parts of the *genus nervoſum*, as oftentimes to produce convulſive motions, even in the extreme parts of the hands and feet.

Nor is the efficacy of effluviums confined to produce hyſterical fits, ſince theſe inviſible particles may be able, and ſometimes as ſuddenly, as perfumes are wont to excite them, to appeaſe them; as I have very frequently, though not with never-failing ſucceſs, tried, by holding a ſpirit, I uſually make of ſal armoniack, under the noſtrils of hyſterical perſons. My remedy did not only often recover, in a trice, thoſe whoſe fits were but ordinary, but did, more than once, ſomewhat to the wonder of the by-ſtanders, relieve, within a minute or two, perſons of differing ages, and conſtitutions that were ſuddenly fallen down by fits, that the by-ſtanders judged epileptical, but I, hyſterical.

I attribute the good and evil operations of the fore-mentioned ſteams, rather in general to the conſent of the parts that make up the *genus nervoſum*, than to any hidden ſympathy or antipathy betwixt them and the womb, not only for other reaſons, not proper to be inſiſted on here, but becauſe I have known odours have notable effects even upon men. I know a very eminent perſon, a traveller, and a man of a ſtrong conſtitution, but conſiderably ſanguine, who is put into violent head-achs by the ſmell of muſk. And I remember, that one day being with him, and a great many other men of note, about a publick affair, a man that had a parcel of muſk about him, having an occaſion to make an application to us, this perſon was ſo diſordered by the ſmell, which to moſt of us was delightful, that, in ſpite of his civility, he was reduced to make us an apology, and ſend the perfumed man out of the room; notwithſtanding whoſe receſs, this perſon complained to me, a good while after, of a violent pain in his head, which I perceived had ſomewhat unfitted him for the tranſaction of the affair, whereof he was to be the chief manager. I know another perſon, whoſe happy muſe hath juſtly made him many admirers, that is ſubject to the head-ach upon ſo mild a ſmell as that of damaſk-roſes, and ſometimes even of red roſes; inſomuch, that walking one day with him in a garden, whoſe alleys were very large, ſo that he might eaſily keep himſelf at a diſtance from the buſhes, which bore many of them red roſes, he abruptly broke off the diſcourſe we were engaged in, to complain of the harm the perfume did his head, and deſired me to paſs into a walk that had no roſes growing near it.

If it were not for the ſex of the perſon, I could relate an inſtance that would be much more conſiderable, of the operation of roſes. For I know a diſcreet lady, to whom their ſmell is not unpleaſing (for ſhe anſwered me, that it was not ſo at all) but ſo hurtful, that it preſently makes her ſick, and would make her ſwoon, if not ſeaſonably prevented; and ſhe told me, that being once at a court, in which ſhe was a maid of honour, though ſhe herſelf did not know whence it came, ſhe found herſelf extremely ill on a ſudden, and ready to ſink down for faintneſs; but being then in diſcourſe with a perſon, whoſe high quality ſhe paid her profound reſpect to, her civility, that kept her from complaining, or withdrawing, might have been dangerous, if not fatal to her, had not the princeſs, who was ſpeaking with her, and who knew her antipathy to roſes, taken notice, that her face grew ſtrangely pale, and was covered with a cold ſweat. For thereby preſently gueſſing what might be the cauſe, which the ſick lady herſelf did not, ſhe aſked aloud, whether ſome body had not brought roſes (which were then in ſeaſon) into the bed-chamber, which queſtion occaſioned a ſpeedy withdrawing of a lady that ſtood at a diſtance off, and had about her roſes, which were not ſeen by the patient, who was by this means preſerved from falling into a ſwoon, though not from being for a while very much diſcompoſed.

But this you may tell me was the caſe of a woman who complained her malady affected her heart, not her head. Wherefore returning to what I was ſpeaking of before I mentioned her, I ſhall proceed to tell you, that as odours may thus give men the head-ach, ſo I have often found the ſmell of rectified ſpirit of ſal armoniack to free men, as well as women, from the fits of that diſtemper; and that ſometimes in ſo few minutes, that the perſons relieved could ſcarcely imagine they could ſo quickly be ſo.

To which I ſhall not add the trials that I have ſucceſsfully made upon myſelf, becauſe being, thanks be to God, very ſeldom troubled with that diſtemper, the occaſions I have had of making them have not been many. And though I have not always found ſo ſlight a remedy to work the deſired cure, yet that it does it often, even in men, is ſufficient to ſhew the efficacy of ſanative effluviums.

Now to manifeſt that ſteams do not operate only upon hyſterical women, or perſons ſubject to the head-ach, I will add ſome inſtances of the effects they may produce upon other perſons and parts.

It is but too well known an obſervation, that women with child have been often made to miſcarry by the ſtink of an ill-extinguiſhed candle, though perhaps the ſmoke aſcending from the ſnuff were diſſipated into the inviſible corpuſcles, a good while before it arrived at the noſtrils of the unhappy woman; and what violent and ſtraining motions abortions are frequently accompanied with, is ſufficiently known already.

I think I have elſewhere mentioned, that a gentleman of my acquaintance, a proper and luſty man, will be put into the fits of vomiting, by the ſmell of coffee boiled in water. I ſhall therefore rather mention that I know a phyſician, who having been, for a long time, when he was young, frequently compelled to take electuarium lenitivum, one of the gentleſt and leaſt unpleaſant laxatives of the ſhops, conceived ſuch a diſlike of it, that ſtill, as himſelf has complained to me, if he ſmell to it, as he ſometimes happens to do in apothecaries ſhops, it will work (now and then for ſeveral times) upwards and downwards with him.

I know another very ingenious perſon of the ſame faculty, that has been a traveller by ſea and land, who has complained to me, that the ſmell of the greaſe of the wheels of a hackney-coach, though it do but paſs by him, is wont to make him ſick, and ready to vomit.

Every body knows that ſmoke is apt to make mens eyes water, and excite, in the organs of reſpiration, that troubleſome and vehement commotion we call coughing. But we need not have recourſe at all to viſible fumes, for the production of the like effects; ſince we have often obſerved them, and repeated ſneezings, to-boot, to proceed from the inviſible ſteams of ſpirit of ſal armoniack, when phials containing that liquor, though they were perhaps but very ſmall, were approached too haſtily, or perhaps, too near to the noſtrils.

And, becauſe, in moſt of the foregoing inſtances, the chief effects ſeem to be wrought, by the conſent of parts, on the *genus nervoſum*, and the action of one of them upon the other, and thereby upon ſeveral other parts of the body, I will ſubjoin a remarkable inſtance of the operation of a mild and grateful odour upon the humours themſelves, and that in a man.

A famous apothecary, who is a very tall and big man, ſeveral times told me, that though he was once a great lover of roſes, yet having had occaſion to employ gre[illegible] quantities of them at a time, he was ſo altered by their ſteams, that now, if he co[illegible] among the roſe-buſhes, the ſmell does much diſcompoſe him. And the odour of [illegible]

(I [illegible]

(I mean incarnate-roſes, which we commonly call damaſk-roſes, though they be not the true ones) makes ſuch a colliquation of humours in his head, that it ſets him a coughing, and makes him run at the noſe, and gives him a ſore throat; and by an affluence of humours makes his eyes ſore; inſomuch, that during the ſeaſon of roſes, when quantities of them are brought into his houſe, he is obliged, for the moſt part, to abſent himſelf from home.

CHAP. VII.

ONE may ſhew on this occaſion, that as there might be conſiderable things performed by effluviums, as they make one part of a living engine work upon another by virtue of its ſtructure, ſo the action of ſuch inviſible agents may, in divers caſes, be much promoted by the fabrick and laws of the univerſe itſelf, upon this account, that, by the operation of effluvia upon particular bodies, they may diſpoſe and qualify thoſe bodies to be wrought upon, which before they were not fit to be, by light, magnetiſms, the atmoſphere, gravity, or ſome other of the more catholick agents of nature, as the world is now conſtituted. But not to injure another tract, I ſhall conclude this, when I ſhall have taken notice, that in the inſtances hitherto produced there has been a viſible local diſtance between the body that emits ſtreams, and that on which they work. But if I thought it neceſſary, it were not difficult to ſhew that one might well enough refer to the title of this tract divers effects of bodies, that are applied immediately to ours; ſuch as are blood-ſtones, cornelians, nephritick ſtones, lapis Malacenſis, and ſome amulets, and other ſolid ſubſtances, applied by phyſicians outwardly to our bodies. For in theſe applications the groſs body touches but the ſkin, and the great effects which I elſewhere relate myſelf to have ſometimes (though not often, much leſs always) obſerved to have followed upon this external contact or near application, may reaſonably be derived from the ſubtle emanations that paſs thorough the pores of the ſkin to the inward parts of the body; as is evident in thoſe, who by holding cantharides in their hands, or having them applied to ſome remote external part, have grievous pains produced in their urinary parts, as it has happened to me, as well as to many others. And to the inſinuation of theſe minute corpuſcles that get in at the pores of the ſkin, ſeems to be due the efficacy of ſome medicines that purge, vomit, reſolve the humours, or otherwiſe notably alter the body, being but externally applied; of which I could here give ſeveral inſtances, but that they belong more properly to another place, and are not neceſſary in this, where it may ſuffice to name the notorious power that mercurial ointments, or fumes, either together or apart, have of producing copious ſalivations, to ſhew, in general, that both the ſteams and the emanations of outwardly-applied medicinal bodies, may have ſome great effects on human ones.

OF

OF THE DETERMINATE NATURE OF EFFLUVIUMS.

CHAP. I.

THE effluviums of bodies, *Pyrophilus*, being for the moſt part inviſible, have been wont to be ſo little conſidered by vulgar philoſophers, that ſcarce vouchſafing to take notice of their exiſtence, it is no wonder that men have not been ſolicitous to diſcover their diſtinct natures and differences. Only *Ariſtotle*, and, upon his account, the ſchools Lib. I. have been pleaſed to think that the two grand parts of our globe do ſometimes emit two Meteor. kinds of exhalations or ſteams; the earthy part affording thoſe that are hot and dry, cap.3.&4. which they name fumes, and very often, ſimply exhalations; and the aqueous part, others that are (not as many of his diſciples miſtake him to have taught cold and moiſt, but) hot and moiſt *, which they uſually call vapours, to diſcriminate them from the fumes or exhalations, though otherwiſe, in common acceptation, thoſe appellations are very frequently confounded.

BUT, though the *Ariſtotelians* have thus perfunctorily handled this ſubject, it would not become corpuſcularian philoſophers, who attribute ſo much as they do to the inſenſible particles of matter, to acquieſce in ſo ſlight and jejune an account of the emanations of bodies. And ſince we have already ſhewn, that beſides the greater and more ſimple maſſes of terreſtrial and aqueous matter newly mentioned, there are very many mixed bodies that emit effluviums, which make, as it were, little atmoſpheres about divers of them, it will be congruous to our doctrine and deſign to add in this place, that beſides the ſlight and obvious differences taken notice of by *Ariſtotle*, the ſteams of bodies may be almoſt as various as the bodies themſelves that emit them; and that therefore we ought not to look upon them barely under the general and confuſed notion of ſmoke, or vapours, but may probably conceive them to have their diſtinct and determinate natures, oftentimes, though not always ſuitable to that of the bodies from whence they proceed.

AND, indeed, the newly-mentioned diviſions of the ſchools give us ſo ſlight an account of the emanations of bodies, that, methinks, it looks like ſuch another, as if one ſhould divide animals into thoſe that are horned, and thoſe that have two feet; for, beſides that the diſtinction is taken from a difference that is not the conſiderableſt, there are divers animals, as many four-footed beaſts and fiſhes, that are not comprized in it;

* Cap. 3. Ἔστι γὰρ ἀτμίδος μὲν φύσις, ὑγρὸν καὶ θερμόν.

and each member of the divifion comprehends I know not how many diftinct forts of animals, whofe differences from one another are many times more confiderable, than thofe that conftitute the two fupreme genufes, the one having bulls and goats, and rhinocerofes, and deer, and elks, and certain fea-monfters, whofe horns I have feen; and the other genus comprifing alfo a greater variety, namely, a great part of four-footed beafts, and, befides men, all the birds (for aught we know) whether of land or water. And as it would give us but a very flender information of the nature of an elk, or an unicorn, to know that it is an horned beaft; or of the nature of a man, an eagle, or a nightingale, to be told that it is an hornlefs beaft; fo it will but very little inftruct a man in the nature of the fteams of quickfilver, or of opium, to be told, that they are vapours hot, or rather cold, and moift; or of the fteams of amber, or cantharides, or cinnamon, or tobacco, to be told, that they are hot and dry. For befides that there may be effluviums, which, even by their elementary qualities, are not of either of thefe two fupreme genufes (for they may be cold and dry, or cold and moift) thefe qualities are often far from being the nobleft, and confequently thofe, that deferve to be moft confidered in the effluviums of this, or that body; as we fhall by and by have occafion to manifeft.

C H A P. I.

And here it may not be improper to mention an experiment, that, I remember, I divers years fince employed to illuftrate the fubject of our prefent difcourfe.

I considered then, that fluid bodies may be of very unequal denfity, and gravity, as is evident in quickfilver, water, and pure fpirit of wine; which, notwithftanding their great difference in fpecifick gravity, may yet agree in the conditions requifite to fluid bodies. Therefore prefuming, that by what I could make appear vifible in one, what happens analogically in the other, may be ocularly illuftrated, I took fome ounces of roch-alum, and as much of fine faltpetre. I took fome ounces of each, becaufe, if the quantity of the ingredients be too fmall, the concoagulated grains will be fo too, and the fuccefs will not be fo confpicuous. Thefe being diffolved together in fair water, the filtrated folution was fet to evaporate in an open-mouthed glafs, and being then left to fhoot in a cool place, there were faftened to the fides, and other parts of the glafs, feveral fmall cryftals, fome octoedrical, which is the figure proper to roch-alum, and others of the prifmatical fhape of pure faltpetre; befides fome other faline concretions, whofe being diftinctly of neither of thefe two fhapes, argued them to be concoagulations of both the falts. And this we did, by ufing fuch a degree of celerity in evaporating the liquor, as was proper for fuch an effect. For, by another degree, which is to be employed, when one would recover the falts more diftinctly and manifeftly, the matter may, as I found by trial, be fo ordered, that the aluminous falt may, for the moft part, be firft coagulated by itfelf, and then, from the remaining liquor, curioufly fhaped cryftals of nitre may be copioufly obtained.

Trials like this we alfo made with other falts, and particularly with fea-falt, and with alum and vitriol; the phænomena of which you may meet with in their due places. For the recited experiment may, I hope, alone ferve to affift the imagination to conceive, how the particles of bodies may fwim to and fro in a fluid (which the air is) and though they be little enough to be invifible, may, many of them, retain their diftinct and determinate natures, and their aptnefs to cohere upon occafion; and others may, by their various occurfions and coalitions, unite into leffer corpufcles, or greater bodies, differ-

differing from the more ſimple particles that compoſed them, and yet not of indeterminate, though compounded figures.

C H A P. III.

These things being premiſed, we may now proceed to the particular inſtances of the determinate nature of effluviums; and theſe we may not inconveniently reduce to the three following heads, to each of which we ſhall aſſign a diſtinct chapter; the firſt of theſe I ſhall briefly treat of in this third chapter, and treat ſomewhat more largely of the others in the two following.

In the firſt place then, that the effluviums of many bodies retain a determinate nature oftentimes in an inviſible ſmallneſs, and oftener in ſuch a ſize, as makes them little enough to fly or ſwim in the air, may appear by this, that theſe effluvia being, by condenſation, or otherwiſe, re-united, they appear to be of the ſame nature with the body that emitted them. Thus in moiſt weather, the vapours of water, that wander inviſibly through the air, meeting with marble-walls, or pavements, or other bodies, by their coldneſs and other qualifications fit to condenſe and retain them, appear again in the form of drops of water; and the ſame vapours return to the viſible form of water, when they fall out of the air in dews or rains.

Quicksilver itſelf, if it be made to aſcend in diſtillation with a convenient degree of fire, will almoſt all be found again in the receiver in the form of running mercury. Which ſtrange and piercing fluid is in ſome caſes ſo diſpoſed to be ſtripped of its diſguiſes, and re-appear in its own form, that divers artificers, and eſpecially gilders, have found, to their coſt, that the fumes of it need not be, as in diſtillation, included in cloſe veſſels to return to their priſtine nature, mercury having been ſeveral times found in the heads, and other parts of ſuch people, who have, in tract of time, been killed by it, and ſometimes made to diſcover itſelf during the lives of thoſe, that dealt ſo much in it; of which I elſewhere give ſome inſtances. Wherefore I ſhall only obſerve at preſent, that it is a common practice, both among gilders, and ſome chymiſts, that, when they have occaſion to make an amalgam, or force away the mercury from one by the fire, they keep gold in their mouths, which, by the mercurial fumes that wander through the air, will now and then, by that time it is taken out of their mouths, be turned white almoſt, as if it had been ſilvered over.

A mass of purified brimſtone being ſublimed, the aſcending fumes will condenſe into what the chymiſts call *flores Sulphuris*, which is true ſulphur of the ſame nature with that formerly expoſed to ſublimation; and may readily, by melting, be reduced into ſuch another maſs.

And to give you another like example of dry bodies, I tried, that by ſubliming good camphire in cloſe veſſels, it would all, as to ſenſe, be raiſed into the upper veſſel, or part of the ſubliming glaſs, in the form of dry camphire, as it was before.

Nay, though a body be not by nature, but art, compounded of ſuch differing bodies, as a metal and another mineral, and two or three ſalts; yet, if upon purification of the mixture from its groſſer parts, the remaining and finer parts be minute enough and fitly ſhaped, the whole liquor will aſcend, and yet in the receiver altogether recover its priſtine form of a tranſparent fluid, compoſed of differing ſaline and mineral parts. This is evident in the diſtillation of what chymiſts call butter, or oil of antimony, very well rectified. For this liquor will paſs into the receiver diaphanous and fluid, though, beſides the particles of the ſublimate (which is itſelf a factitious compounded body) it abounds with antimonial corpuſcles, carried over and kept inviſible by the corroding ſalts; whatever *Angelus Sala*, and thoſe chymiſts that follow him, have affirmed to the contrary, as might be eaſily here proved, if this were a fit place to do it in.

I FOUND by enquiring of an ingenious perſon, that had an intereſt in a tin mine, that I was not deceived in gueſſing, that tin itſelf, though a metal, whoſe ore is of a very difficult fuſion, and which I have by itſelf kept long upon the cupel without finding it to fly away, would yet retain its metalline nature in the form of fumes or flowers. For this experienced gentleman anſwered me, that divers times they would take great ſtore of a whitiſh ſublimate from the upper part of the furnaces or chimnies, where they brought their ore to fuſion or wrought further upon it; and that this ſublimate, though perhaps elevated to the height of an ordinary man, would, when melted down, afford at once many pounds of very good tin. On which occaſion I ſhall add, that I have myſelf, more than once, raiſed this metal in the form of white corpuſcles by the help of an additament, that did ſcarce weigh half ſo much as it.

CHAP. IV.

THE ſecond way, by which we may diſcover the determinate nature of effluviums, is, by the difference that may ſometimes be obſerved in their ſenſible qualities. For theſe effluviums, that are endowed with them, proceed from the ſame ſort of bodies, and yet thoſe afforded by one kind of bodies being in many caſes manifeſtly differing from thoſe that fly off from another, this evident diſparity in their exhalations argues their retaining diſtinct natures, according to thoſe of the reſpective bodies whence they proceed.

I WILL not now ſtay to examine, whether in the ſteams, that are made viſibly to aſcend from the terreſtrial globe by thoſe grand agents and uſual raiſers of them, the ſun, and the agitation of the air, the eye can manifeſtly diſtinguiſh the diverſity of colours: but in ſome productions of art, ſuch different colours may be diſcovered in the exhalations, even without the application of any external heat to raiſe them. For, when ſpirit of nitre, for example, has been well rectified, I have often obſerved, that even in the cold the fumes would play in the unfilled part of the ſtopped phials it was kept in, and appear in it of a reddiſh colour, and if thoſe veſſels were opened, the ſame fumes would copiouſly aſcend into the air, in the form of a reddiſh or orange-tawny ſmoke; ſpirit, or oil of ſalt alſo, if it be very well dephlegmed, though it will ſcarce in the cold viſibly aſcend in the empty part of a phial, whilſt it is kept well ſtopped; yet, if the free air be allowed acceſs to it, it will, in caſe it be ſufficiently rectified, fly up in the form of a whitiſh fume. But this is inconſiderable, in compariſon of what happens in a volatile tincture of ſulphur I have elſewhere taught you to make with quick-lime. For, not only upon a ſlight occaſion, the vacant part of the phial will be filled with white fumes, though the glaſs be well ſtopped; but upon the opening the phial theſe fumes will copiouſly paſs out at the neck, and aſcend into the air in the form of a ſmoke, more white than perhaps you ever ſaw any. And both this and that of the ſpirit of ſaltpetre do, by their operation, as well as ſmell, diſcloſe what they are; the latter being of a nitrous nature (as is confeſſed) and the former, of a ſulphureous: inſo much, that having, for curioſity's ſake, in a fitly ſhaped glaſs, caught a competent quantity of the aſcending white fumes, I found them to have convened into bodies tranſparent and geometrically figured, wherein it was eaſy to diſcover, by their ſenſible qualities, that there were ſtore of ſulphureous particles mixed with the ſaline ones. That the liquors

liquors of vegetables, diſtilled *in balneo*, or in water, are not wont to retain any thing of the colour of the bodies that afforded them, is a thing eaſy to be obſerved in diſtillations made without retorts or the violence of the fire. But it may be worth while to make trial, whether the eſſential oil of wormwood aſcend coloured like the plant whence it is firſt drawn over with water in the alembic, or rectified *in balneo*. For I forgot to take notice of it, when upon ſome particularities, I obſerved in that plant, my curioſity led me to find, that not only in the firſt diſtillation in a copper alembic, tinned on the inſide, the oil came over green, but by a rectification purpoſely made in a glaſs veſſel, the purified liquor was not deprived of that colour.

The mention of theſe eſſential oils, as chymiſts call thoſe that are drawn in alembics, leads me to tell you, that though theſe liquors be but effluvia of the vegetables they are diſtilled from, condenſed again in the receiver into liquors; yet, as ſubtil as they are, many of them retain the genuine taſte of the bodies, whence the heat elevated them; as you will eaſily find, if you will taſte a few drops of the eſſential oil of cinnamon, for example, or of wormwood diſſolved by the intervention of ſugar, or ſpirit of wine, in a convenient quantity of water, wine, or beer. For by this means you have the natural taſte of this ſpice or herb. And wormwood is a plant, whoſe effluvia do ſo retain the nature of the body that parts with them, that I muſt not forbear to alledge here an obſervation of mine, that may ſhew you, that it is poſſible, though not uſual, that even without the help of the fire, the expirations of a body may communicate its taſte. For, among other things that I had occaſion to obſerve about ſome quantity of wormwood laid up together, I remember I took notice, and made others do the like, that coming into a room where it was kept, not only the organs of ſmelling were powerfully wrought upon by the corpuſcles, that ſwarmed in the air, but alſo the mouth was ſenſibly affected with a bitter taſte. Perhaps you will ſcarce think it worth while, that after this inſtance I ſhould add, that I found the expirations of amber, kept a while in pure ſpirit of wine, taſte upon the tongue like amber itſelf, when I chewed it between my teeth. But I chooſe to mention this inſtance, becauſe it will connect thoſe lately mentioned with another ſort, very pertinent to our preſent purpoſe. For, the expirations that I have obtained from amber, both with pure ſpirit of wine, and a more piercing menſtruum, did manifeſtly retain in both thoſe liquors a peculiar ſmell, with which I found it to affect the noſtrils, when, for trial's ſake, I excited the electrical faculty of amber by rubbing. And as for odours, it is plain, that the eſſential oils of chymiſts, well drawn, do many of them retain the peculiar and genuine ſcent of the ſpices or herbs that afforded them. And that theſe odours do really conſiſt of, or reſide in certain inviſible corpuſcles, that fly off from the viſible bodies, that are ſaid to be endowed with ſuch ſmells, I have elſewhere proved at large; and it may ſufficiently appear from their ſticking to divers of the bodies they meet with, and their laſting adheſion to them.

Other examples may be given of the ſettled difference of effluviums directly perceivable by human organs of ſenſe, as dull as they are; which laſt expreſſion I add, becauſe I ſcarce doubt, but that, if our ſenſories were ſufficiently ſubtil and tender, they might immediately perceive in the ſize, ſhape, motion, and perhaps colour too of ſome now inviſible effluviums, as diſtinguiſhable differences, as our naked eyes in their preſent conſtitution ſee, between the differing ſorts of birds, by their appearances, and their manner of flying in the air, as hawks, and partridges, and ſparrows, and ſwallows. To make this probable I will not urge, that in fine white ſand, whoſe grains by the unaſſiſted eye are not wont to be diſtinguiſhed by any ſenſible quality, I have often obſerved in an excellent microſcope, a notable diſparity as to bulk, figure, and ſometimes as to colour:

colour: and that in ſmall cheeſe-mites, which the naked eye can very ſcarcely diſcern, ſo far is it from not diſcovering any difference between them, one may (as was noted in the laſt eſſay) plainly ſee, beſides an obvious difference in point of bigneſs, many particular parts, on whoſe accounts, the ſtructure of thoſe moving points may difference them from each other. And I have ſometimes ſeen a very evident diſparity, even in point of ſhape, between the very eggs of theſe living atoms, as a poet would perhaps ſtile them. But theſe kinds of proofs (as I was ſaying) I ſhall forbear to inſiſt on, that I may proceed to countenance my conjecture by the effects of the effluviums, that are properly ſo called, upon animals.

And firſt, though the touch be reckoned one of the moſt dull of the five ſenſes, and be reputed to be far leſs quick in men, than in divers other animals; yet the groſs organs of that may, in men themſelves, even by accident, be ſo diſpoſed, as to be ſuſceptible of impreſſions from effluvia: of this, in another paper, I give ſome inſtances. And I know not, whether divers of the preſages of weather, to be obſerved in ſome animals, and the achs, and other pains, that in many crazy and wounded men, are wont to fore-run great changes of weather, do not often (for I do not ſay always) proceed, at leaſt in part, from inviſible, and yet incongruous effluxions, which, either from the ſubterraneal parts, or from ſome bodies above ground, do copiouſly impregnate the air. And, on this occaſion, it will not be impertinent to mention here what an experienced phyſician, being (if I much miſremember not) the learned *Diemerbroek*, relates concerning himſelf, who having been infected with the plague by a patient, that lay very ill of it, though by God's bleſſing, which he particularly acknowledges, upon a ſlight, but ſeaſonable remedy, he was very quickly cured, and that without the breaking of any tumor; yet it left ſuch a change in ſome parts of his body, that he ſubjoins this memorable paſſage, *Ab illo periculo ad contagioſos mihi appropinquanti in emunctoriis ſucceſſit dolor, vix fallax peſtis indicium.*

About Coſmical Suſpicions.

Two or three other obſervations of the like nature you meet with in another of my papers. And I ſhall now add, that I know an ingenious gentlewoman (wife to a famous phyſician) who was of a very curious and delicate complexion, that has ſeveral times aſſured me, that ſhe can very readily diſcover, whether a perſon, that comes to viſit her in winter, came from ſome place where there is any conſiderable quantity of ſnow; and this ſhe does, as ſhe tells me, not by feeling any unuſual cold (for if the ground be frozen, but not covered with ſnow, the effect ſucceeds not) but from ſome peculiar impreſſion, which ſhe thinks, ſhe receives by the organs of ſmelling. I might add, that I know alſo, as I may have formerly told you, a very ingenious phyſician, who falling into an odd kind of fever, had his ſenſe of hearing thereby made ſo very nice and tender, that he very plainly heard ſoft whiſpers, that were made at a conſiderable diſtance off, and which were not in the leaſt perceived by the healthy by-ſtanders, nor would have been by him before his ſickneſs. Which ſickneſs I mention as the thing, that gave his organs of hearing this preternatural quickneſs, becauſe, when the fever had quite left him, he was able to hear but at the rate of other men. And I might tell you too, that I know a gentleman of eminent parts and note, who, during a diſtemper he had in his eyes, had his organs of ſight brought to be ſo tender, that both his friends, and himſelf alſo, have aſſured me, that when he waked in the night, he could for a while plainly ſee and diſtinguiſh colours, as well as other objects, diſcernible by the eye, as was more than once tried, by pinning ribbands, or the like bodies, of ſeveral colours, to the inſide of his curtains in the dark. For if he were awakened in the night, he would be able to tell his bedfellow, where thoſe bodies were placed, and what colour each of them was of.

I HAVE mentioned these instances only to shew you, that if our sensories were more delicate and quick, they would be sufficiently affected by objects, that, as they are generally constituted, make no impressions at all upon them. For otherwise I know, that the species (as they call them) both of sounds and colours are not held by many of the moderns (from whom in that I dissent not) to be so much corporeal effluxions, trajected through the medium, as peculiar kinds of local motion conveyed by it. Therefore, I shall now confirm the conjecture I would countenance, by the discrimination made by the organs of other animals, of such effluvia, as to us men are not only invisible, but sensible. And therefore, partly to strengthen what I delivered, and partly to confirm what I am now discoursing of, it will not be impertinent to subjoin two or three relations, that I had from persons of very good credit, whom I thought likely to make me no unsatisfactory returns to my questions, about things they were very well versed in.

A PERSON of quality, to whom I am near allied, related to me, that to make a trial, whether a young blood-hound was well instructed (or as the huntsmen call it, made) he caused one of his servants, who had not killed, or so much as touched any of his deer, to walk to a country-town, four miles off, and then to a market-town, three miles distant from thence; which done, this nobleman did, a competent while after, put the blood-hound upon the scent of the man, and caused him to be followed by a servant or two, the master himself thinking it also fit to go after them to see the event; which was, that the dog, without ever seeing the man he was to pursue, followed him by the scent to the above-mentioned places, notwithstanding the multitude of market people, that went along in the same way, and of travellers, that had occasion to cross it. And when the blood hound came to the chief market town, he passed through the streets, without taking notice of any of the people there, and left not, till he had gone to the house, where the man, he sought, rested himself, and found him in an upper room, to the wonder of those that followed him. The particulars of this narrative, the nobleman's wife, a person of great veracity, that happened to be with him when the trial was made, confirmed to me.

ENQUIRING of a studious person, that was keeper of a red deer park, and versed in making blood-hounds, in how long time, after a man or deer had passed by a grassy place, one of those dogs would be able to follow him by the scent? he told me, that it would be six or seven hours: whereupon an ingenious gentleman, that chanced to be present, and lived near that park, assured us both, that he had old dogs of so good a scent, that if a buck had the day before passed in a wood, they will, when they come where the scent lies, though at such a distance of time after, presently find the scent and run directly to that part of the wood where the buck is. He also told me, that though an old blood-hound will not so easily fix on the scent of a single deer, that presently hides himself in a whole herd, yet if the deer be chased a little till he be heated, the dog will go nigh to single him out, though the whole herd also be chased. The above-named gentleman also affirmed, that he could easily distinguish, whether his hounds were in chase of a hare, or a fox, by their way of running, and their holding up their nose higher than ordinary, when they pursue a fox, whose scent is more strong. These relations will not be judged incredible by him, that reflects on some of the instances that have already, in the foregoing essay, been given of the strange subtilty of effluvia: to which I shall now add, that I remember, that to try, whether I could, in some measure, make art imitate nature, I prepared a body of a vegetable substance, which, though it were actually cold, and both to the eye and touch dry, did for a while emit such determinate and piercing, though invisible, exhalations, that having, for trial's sake,

ſake, applied to it a clear metalline plate, and that of none of the very ſofteſt kind neither, for about one minute of an hour, I found, that though there had been no immediate contact between them, I have purpoſely interpoſed a piece of paper to hinder it, yet there was imprinted on the ſurface of the plate a conſpicuous ſtain of that peculiar colour, that the body, with whoſe ſteams I had imbued the vegetable ſubſtance, was fitted to give a plate of that mixed metal. And though it be true, that in ſome circumſtances, the lately-mentioned inſtances about blood-hounds have a conſiderable advantage of this I have now recited, yet that advantage is much leſſened, not to ſay countervailed, by ſome circumſtances of our experiment. For, not to repeat, that the emittent body was firm and cold, the effect produced by the effluvium, that guided the ſetting-dog, was wrought upon the ſenſory of a living and warm animal; and ſuch an one, whoſe organs of ſmelling are of an extraordinary tender conſtitution above thoſe of men, and other animals, and probably the impreſſion was but tranſient; whereas, in our caſe, the inviſible ſteams of the vegetable ſubſtance wrought upon a body, which was of ſo ſtrong and inorganical a texture, as a compounded metal, though it were fenced by being lapped up in paper, notwithſtanding which theſe ſteams invaded it in ſuch numbers, and ſo notably, as to make their operation on it manifeſt to the eye, and conſiderably permanent too; ſince, coming to look upon the plate after the third day, I found the induced colour yet conſpicuous, and not like ſuddenly to vaniſh.

Hitherto, in this chapter, I have argued from the conſtant and ſettled difference of the ſenſible qualities of effluviums, that they do not always loſe their diſtinct natures, when they ſeem to have loſt themſelves by vaniſhing into air. But before I diſmiſs this ſubject, I muſt conſider an objection, which I know may be made againſt the opinion we have been countenancing. For it may be alledged, that there may be many caſes, wherein the effluviums of bodies are, in their paſſage through the air, ſenſibly altered, or do affect the organs of ſenſe, otherwiſe than each kind of them apart would do: nor is this difficulty altogether irrational. For it ſeems conſonant enough to experience, that ſome ſuch caſes ſhould be admitted; and therefore, in the foregoing diſcourſe I have, where I thought it neceſſary, forborn to expreſs myſelf in ſuch general and abſolute terms, as otherwiſe I might have done. But as for ſuch caſes, as I have inſiſted upon, and many more I ſhall now repreſent, that the objected alterations need not hinder, but that effluviums, at their firſt parting from the bodies, whence they take wing, if I may ſo ſpeak, may retain as much of the nature of thoſe bodies, as we have aſcribed to them; ſince the ſubſequent change may very probably be deduced from the combinations, or coalitions, of divers ſteams aſſociating themſelves in the air, and acting upon the ſenſory, either altogether and conjointly, or at leaſt ſo near it, that the ſenſe cannot perceive their operations as diſtinct. This I ſhall elucidate, but not pretend to prove, by what happens in ſounds and taſtes. For if, by way of inſtance, in a muſical inſtrument, two ſtrings tuned to an eighth, be touched together, they will ſtrike the ear with a ſound, that will be judged one, as well as pleaſing, though each of the trembling ſtring make a diſtinct noiſe, and the one vibrates as faſt again as the other. And if, into oil of tartar *per deliquium* you drop a due proportion of ſpirit of nitre, and exhale the ſuperfluous moiſture, the acid and alcalizate corpuſcles, that were ſo ſmall as to ſwim inviſibly in thoſe liquors, will convene into nitrous concretions, whoſe taſte will be compounded of, but very differing from, both the taſtes of the acid and tartarous particles; which particles may yet, for the moſt part, by a ſkilful diſtillation, be divorced again. And ſo, if to a ſtrong ſolution of pot-aſhes, or ſalt of tartar, you put as much in weight of ſal armoniack, as there is of either of thoſe fixed ſalts contained in the liquor; you may, beſides a ſubtil urinous ſpirit, that will eaſily come over in the diſtil.

diſtillation, obtain a dry *caput mortuum*, which is almoſt totally a compounded ſalt, differing enough from either of the ingredients, eſpecially the alcalizate, as well in taſte, as in ſome other qualities: this ſalt, freed from its fæces, being that diuretick ſalt, I ſeveral years ago gave quantities of ſome to chymiſts and phyſicians, from the moſt of whom I received great thanks, accompanied with the more acceptable accounts of the very happy ſucceſs they had employed it with, though uſually but in a ſmall doſe, as from ſix, eight, or ten grains to a ſcruple. But this being mentioned only upon the by, I ſhall proceed to tell you, that ſince I intimated to you already, that I would mention examples of ſounds and taſtes, only to illuſtrate what I have been delivering; I ſhall now add ſome inſtances by way of proof, of the coalition and reſulting change of ſteams in the air. It is eaſily obſervable in ſome noſegays, where the differing flowers happen to be conveniently mixed, that in the ſmell afforded by it, at a due diſtance, the odours of the particular flowers are not perceived, but the organ is affected by their joint action, which makes on it a confuſed, but delightful impreſſion. And ſo, when in a ball of pomander, or a perfumed ſkin, muſk, and amber, and civet, and other ſweets, are ſkilfully mixed, the coalition of the diſtinct effluvia of the ingredients, that aſſociate themſelves in their paſſage through the air, produce in the ſenſory one grateful perfume, reſulting from all thoſe odours. But if you take ſpirit of fermented urine, and ſpirit of wine, both of them phlegmatick, and mix them together, they will incorporate like wine and water, or any other ſuch liquors, without affording any dry concretions. But if you expoſe them in a convenient veſſel but to the mild heat of a bath, or lamp, the aſcending particles will aſſociate themſelves, and adhere to the upper part of the glaſs in the form of a white but tender ſublimate, conſiſting both of urinous and vinous ſpirits, aſſociated into a mixture, which differs from either of the liquors, not only in conſiſtence, taſte, and ſmell, but in ſome conſiderable operations performable by this odd mixture; which this is not the place to take further notice of. And if ſpirit of ſalt, and ſpirit of nitre be, by diſtillation, elevated in the form of fumes, ſo ordered, as to convene into one liquor in the receiver, this liquor will readily diſſolve crude gold, though neither the ſpirit of nitre alone, nor that of ſalt, would do ſo.

And that you may have an ocular proof of the poſſibility of the diſtinctneſs and ſubſequent commixture of the ſteams in the air, I ſhall now add an experiment, which I long ſince deviſed for that purpoſe, and which I ſoon after ſhewed to many curious perſons, moſt of whom appeared ſomewhat ſurprized at it. The experiment was, that I took two ſmall phials, the one filled with ſpirit of ſalt, but not very ſtrong, the other with ſpirit of fermented urine, or of ſal armoniack very well rectified: theſe phials being placed at ſome diſtance, and not being ſtopped, each liquor afforded its own ſmell, at a pretty diſtance, by the ſteams it emitted into the air, but yet theſe ſteams were inviſible. But when theſe phials (which ſhould be of the ſame ſize) came to be approached very near to each other, though not ſo as to touch; as when the two liquors are put together in the form of liquors, they will notably act upon one another; ſo their reſpective effluviums meeting in the air, would, anſwerably to the littleneſs of their bulk, do the like, and by their mutual occurſions, become manifeſtly viſible, and appear moving in the air like a little portion of ſmoke, or of a miſt, which would quickly ceaſe, if either of the phials were removed half a foot, or a foot from the other. And I remember, that, to add to the oddneſs of the phænomenon, I ſometimes made a drop of the ſpirit of ſalt hang at the bottom of a little ſtick of glaſs, or ſome other convenient body, and held this drop thus ſuſpended in the orifice of a phial, that had ſpirit of ſal armoniack in it, and was furniſhed with a ſomewhat long neck; for by this means, it happened as I expected, that the ascending urinous particles, though invisible before, invading plentifully the acid ones of the drop, produced a notable smoke, which, if the drop were held a little above the neck of the glass, would most commonly fly upwards to the height of a foot, or half a yard: but if the drop were held somewhat deep within the cavity of the neck, a good part of the produced smoke would oftentimes fall into the cavity of the phial, which was left in great part empty, sometimes in the form of drops, but usually in the form of a slender and somewhat winding stream, of a white colour, that seemed to flow down just like a liquor from the depending drop, till it had reached the spirit of sal armoniack; upon whose surface it would spread itself like a mist. But this only upon the by. As for the main experiment itself, it may be, as I have found, successfully tried with other liquors than these; but it is not necessary, in this place, to give an account of such trials; though perhaps, if I had leisure, it might be worth while to consider, whether these coalitions of differing sorts of steams in the air, and the changes resulting thence of their particular precedent quantities, may not assist us to investigate the causes of divers sudden clouds and mists, and some other meteorological phænomena, and also of divers changes that happen in the air, in reference to the coming in and ceasing of several either epidemical, or contagious diseases, and particularly the plague, that seem to depend upon some occult temperature and alterations of the air, which may be copiously impregnated by the differing subterraneal (not to add here, sideral) effluviums, that not unfrequently ascend into it, or otherwise invade it, with pestiferous, or other morbifick corpuscles, and sometimes with others of a contrary nature, and sometimes too, perhaps, neither the one sort of steams, which may be supposed to have imbued the air, is in itself deleterious; nor the other salutary, but becomes so upon their casual coalition in the air. You will perhaps think this conjecture of the resultancy of pestilential steams the less improbable, if I here add that odd observation, which was frequently made in the formerly mentioned plague at *Nimeguen*, by a physician so judicious as *Diemerbroek*; whose words are these; *Illud notatu dignum sæpissimè observavimus, nempe in illis ædibus, in quibus nulla adbuc pestis erat, si lintea-mina sordida aquâ & sapone nostrâte (ut in Belgio moris est) illic lavarentur, eo ipso die, vel interdum postridie, duos tres-ve simul peste correptos fuisse, ipsique ægri testabantur fœtorem aquæ saponatæ illis primam & maximam alterationem intulisse. Hoc ipsum quoque in meo ipsius hospitio infelix experientia docuit, in quo post lota linteamina statim gravem alterationem perceperunt plærique domestici, & proximè sequenti nocte tres peste correptæ, ac brevi post mortuæ fuere.* I omit the instances he further sets down to confirm this odd phænomenon, of which, though perhaps some other cause may be divided, yet, that I lately assigned, seems at least a probable one, if not the most probable; since, as it is manifest by daily experience, that the smell occasioned by the washing of foul linen with soap commonly used in the *Netherlands*, produces not the plague; so, by our learned author's observation it appears, either, that there were not yet any pestilential effluxions in the air of those places, which, on the occasions of those washings, became infected, or at least, that by the addition of the fetid effluvia of the soapy water, those morbifick particles, that were dispersed through the air before, had not the power to introduce a malignant constitution into the air, and to act as truly pestilential, till they were enabled to do so, by being associated with the ill-scented effluvia of the soap.

Tract. de Peste, Lib. II. cap. 3.

Whether also salutary, and, if I may so call them, alexipharmical corpuscles may not be produced in the air by coalition, might be very well worth our enquiry: especially if we had a competent historical account of the yearly ceasing of the plague at *Grand Cayro*. For, as I have elsewhere noted out of the learned *Prosper Alpinus*, who practised physick there, and, as I have also been informed by some of my acquaintance, who visited

visited that vast city, that almost in the midst of summer, as soon as the river begins to rise *, the plague has its malignity suddenly checked, even as to those that are already infected, and soon after ceases; so if other circumstances contradict not, one might guess, that this strange phænomenon may be chiefly occasioned by some nitrous, or other corpuscles, that accompany the overflowing Nile, and by associating themselves with what *Hippocrates* somewhere calls νοσερὰς ἀποῤῥοίας, disable them to produce their wonted pernicious effects. To which hypothesis suits well what is delivered by more than one traveller into *Egypt*, and more particular by our ingenious countryman Mr. *George Sandys*, who not only takes notice, that about the time of the overflowing of *Nilus*, whose abounding with nitre has been observed even by the antients, there is a certain moistening emanation diffused thorough the air. To prove, says he †, speaking of the overflowing of *Nilus*, that it proceedeth from a natural cause, this one, though strange, yet true experiment will suffice. Take of the earth of *Egypt* adjoining to the river, and preserve it carefully, that it neither come to be wet nor wasted, weigh it daily, and you shall find it neither more nor less heavy until the seventeenth of *June*, at which day it beginneth to grow more ponderous, and augmenteth with the augmentation of the river, whereby they have an infallible knowledge of the state of the deluge, proceeding without doubt from the humidity of the air, which having a recourse through all passable places, and mixing therewith, increaseth the same, as it increaseth in moisture.

THAT these sanative steams perform their effects merely because they are moist, I presume naturalists will scarce pretend; but that they may be of such a nature, as by their coalition with the morbifick corpuscles, to encrease their bulk and alter their figure, or precipitate them out of the air, or clog their agility, or pervert their motions, and, in a word, destroy all, or some at least, of those mechanical affections, which made those corpuscles pestilential: that, I say, these antidotal vapours (if I may so call them) may have these effects upon those that formerly were morbifick, and that so there may result from the association of two sorts of particles, whereof one was of a highly noxious nature, a harmless mixture, might here be made probable by several things; but that I hope what I have lately recited about the coalitions of the effluvia of spirit of salt, and of urine (liquors known to be highly contrary to each other) is not already forgotten by you.

AND the experiment, with which I am to conclude this essay, will, perhaps, make you think it possible, that the pestiferous steams, that have already passed out of the air, and invaded, but not too much vitiated, the bodies of men, may have their malignity much debilitated by the supervening of these antidotal particles. For in that experiment you will find, that the steams emitted into the air from the liquor there described, though that were actually cold, were able to reach, and manifestly to operate (and that probably by way of precipitation) upon corpuscles, that were fenced from them by the interposition of other bodies, not more porous than those of living men. Whether the fume or sulphur, which by many is extolled to prevent the infection of the air, do, by its acid, or other particles, disarm, if I may so speak, the pestilential ones, I have not now time to enquire: no more than whether in *Ireland*, and some few other countries, that breed or brook no poisonous animals, that hostility may proceed, at least, in great part, from

* The plague, which here miserably rageth, upon the first of the flood doth instantly cease, insomuch, as when five hundred die at *Cayro* the day before, which is nothing rare (for the sound keep company with the sick, holding death fatal, and, to avoid them, irreligion) not one doth die the day following, says Mr. *Sandys* in his travels, Lib. II.

† Mr. *Sandys* in the book above-cited.

the peculiar nature of the foil, which both from its fuperficial or deeper parts, conftantly fupplies the air with corpufcles deftructive to venemous animals. And fome other particulars, that may be pertinently enough confidered here, you may find treated on in other papers. And therefore at prefent I fhall only intimate in a word, that having purpofely made a vifible and lafting ftain on a folid body barely by cold effluvia, I did, by the invifible and cold fteams of another body, make, in two or three minutes, a vifible change in the colour of that ftain.

And as for the other part of the conjecture, viz. that meteors may fometimes be produced by the occurfions of fubterraneal effluvia, fome of them of one determinate nature, and fome of another, I think I could, to countenance it, give you divers inftances of the plentiful impregnation of the air at fome times, and in fome places, with fteams of very differing natures, and fuch as are not fo likely to be attracted by the heat of the fun, as to be fent up from the fubterraneal regions, and fometimes from minerals themfelves. But for inftances of this kind, I fhall, for brevity fake, refer you to another paper *, where I have purpofely treated of this fubject, and particularly fhewn, that though ufually the effluxions that come from under-ground, are ill-fcented, yet they are not always fo; and alfo, that fulphureous exhalations, even from cold, and, for the moft part, aqueous liquors, may retain their determinate nature in the air, and act accordingly upon folid bodies themfelves, to whofe conftitution thofe effluvia chance to be proportionate.

But one memorable ftory, not mentioned in that difcourfe, is too much to our prefent purpofe to be here omitted, efpecially having met with it in fo approved an author as the experienced *Agricola*, who having mentioned out of antient hiftorians the raining of white and red liquors, which they took (erroneoufly, I doubt not) for milk and blood, fubjoins, † *Ut autem majorem fidem habeamus annalium monumentis facit res illa decantata, quæ patrum memoriâ* (in another place he fpecifies the year our Lord) *in* Suevia *accidit*; *aer enim ille ftillavit guttas, quæ lineas veftes crucibus rubris quafi fanguineis imbuebant.* Which I the rather mention, becaufe it does not only prove what I alledge it for; but may keep what is lately and very credibly reported to have happened in divers places of the kingdom of *Naples*, foon after the fiery eruption of *Vefuvius*, from being judged a phænomenon either altogether fabulous (as doubtlefs many have thought it) or a prodigy without all example, as is prefumed, even by thofe that think it not miraculous. And to this I add, that it will be the lefs improbable, that the more agile corpufcles of fubterraneal falts, fulphurs, and bitumens, may be raifed into the air, and keep diftinct natures there, if fo fixed a body, as common earth itfelf, can be brought to fwim in the air. And yet of this the worthy writer newly quoted gives us, befides what annals relate, this teftimony upon his own knowledge: ‡ *Certè hîc Kempnicii undecimum abhinc annum menfe Septembri effluxerunt imbres, fic cum terra lutea commifti, ut eâ paffim plateas fcilicet ftratas viderem confperfas.*

And to fhew you, that in fome cafes the particles even of vegetable bodies may not fo foon perifh in the air, as they vanifh there, but may retain diftinct natures at a greater diftance, than one would think, from the bodies, that copioufly emit them; I fhall add, that having defired an ingenious gentleman, that went on a confiderable employment to the *Eaft-Indies*, to make fome obfervations for me in his voyage; he fent me, among other things, this remark: that having failed along the coaft of *Ceylon* (famous for cinna-

* An Effay of Subterraneal Exhalations.

† *Agric.* de nat. eorum, quæ effluunt è terra, Lib. XII. pag. 236.

‡ *Agric.* de nat. eorum, quæ è terrra effluunt, Lib. XII. pag. 263.

mon-

mon-trees and well-ſcented gums) though they coaſted it almoſt a whole day, the wind, that then chanced to blow from the ſhore, brought them a manifeſtly odoriferous air from the iſland, though they kept off many miles (perhaps twenty or twenty-five from the ſhore. Nor ſhould this be thought incredible, becauſe the diffuſion ſeems ſo diſproportionate to that of other bodies diſſolved by fluids; as, for inſtance, though ſalt be an active body, and reſoluble into abundance of minute particles, yet one part of ſalt will ſcarce be taſtable in an hundred parts of water. For ſenſibly to affect ſo groſs an organ, as that of our taſte, there is uſually required in ſapid particles a bigneſs far exceeding that which is neceſſary to the making bodies fit objects for the ſenſe of ſmelling, and which is here mainly to be conſidered, there is a great difference between the power a body has to impregnate ſo thin and fine a fluid as air, whoſe parts are ſo rare and lax, and that, which it has to impregnate liquors, ſuch as water or wine, whoſe parts are ſo conſtipated as to make it not only viſible and tangible, but ponderous. On which occaſion I remember, that having had a curioſity to try how far a ſapid body could be diluted, without ceaſing to be ſo, I found, by trial, that one drop of good chymical, and, as artiſts call it, eſſential oil of cinnamon, being duly mixed, by the help of ſugar, with wine, retained the determinate taſte of cinnamon, though it were diffuſed into near a quart of wine. So that, making a moderate eſtimate, I concluded, that upon the common ſuppoſition, according to which a drop is reckoned for a grain, one part of oil had given the ſpecifick taſte of the ſpice it was drawn from, to near fourteen thouſand parts of wine. By comparing which experiment with what I noted about the proportion of ſalt requiſite to make water taſte of it, you will eaſily perceive that there may be a very great difference, in point of diffuſiveneſs, between the little particles that make bodies ſapid; which may ſerve to confirm both ſome part of the firſt chapter of the foregoing eſſay of the ſubtilty of effluvia, and what I was lately ſaying, to ſhew it poſſible that antimonial glaſs might impart ſtore of ſteams to the emetick wine, without appearing, upon common ſcales, to have loſt of its weight; ſince we ſee that one drop of ſo light a body as oil may communicate, not inſenſible effluvia, but taſtable corpuſcles, to near a quart of liquor. But this is not all for which I mention our experiment; for I muſt now add, that beſides the almoſt innumerable ſapid parts of a ſpicy drop communicated to the wine, it thence diffuſed a vaſt number of odorous particles into the air, which both I and others perceived to be imbued with the diſtinct ſcent of cinnamon, and which, perhaps the liquor would have been found able to have aromatized for I know not how long a time, if I had had leiſure to proſecute the obſervation.

C H A P. V.

THE third and laſt way I ſhall mention of ſhewing the determinate nature of effluviums, is to be taken from the conſideration of their effects upon other bodies than the organs of our ſenſes (for of their operations upon theſe, we have already ſpoken in the foregoing chapter). For the effects that certain bodies produce on others by their effluviums, being conſtant and determinate, and oftentimes very different from thoſe which other agents, by their emiſſions, work upon the ſame, and other ſubjects, the diſtinct nature of the corpuſcles emitted may be thence ſufficiently gathered.

WE may, from the foregoing tract of the ſubtilty of effluvia, borrow ſome inſtances very pertinent to this place. For the temporary benumbedneſs or ſtupefaction, for example, produced in the fiſherman's foot by the * effluvia of the fiſh *Amoreatim*, men-

* See the Eſſay of the Subtilty of Effluviums, Chap. IV.

tioned by the ingenious *Piso*, manifests, that those stupefying emanations retained a peculiar and venomous nature during their whole passage through the shoe, stocking, and skin, interposed betwixt the fish and the nervous part of the foot benumbed by it. And though there are very few other bodies in the world that are minute enough to pass through the pores of glass, it is apparent, by the experiment there recited of the oblong iron hermetically sealed up in a glass-pipe, that the magnetical effluvia of the earth may retain their peculiar and wonderful nature, in a smallness that qualifies them to pass freely through the pores of glass itself.

But that I may neither repeat what you have already met with in the foregoing tract, nor anticipate what I have to say in the next, I will employ in this chapter some instances that may be spared from both.

That divers bodies of a venemous nature may exercise some such operations upon others by their effluviums transmitted through the air, as they are wont to do in their gross substance, is a truth, whereof, though I have not met with many, yet I have met with some examples among physicians.

Lib. VI. parte 7. cap. 1. The learned *Sennertus* observes, as a known thing, that the apprentices of apothecaries have been cast into profound sleeps, when in distilling opiat and hypnotick liquors they have received in at their nostrils the vapours exhaling from those bodies.

It is recorded by the † writers about poisons, that the root and juice of mandragora casts those that take it into a deep sopor, not unlike a lethargy. And though the apples of the same plant be thought to be much less malignant; yet *Levinus Lemnius* relates that it happened to him more than once, that having laid some mandrake-apples in his study, he was, by their steams, made so sleepy, that he could hardly recover himself; but the apples being taken away he regained alacrity, and threw off all drowsiness.

Among all poisons, there is scarce any whose phænomena are, in my opinion, more strange than those that proceed from a mad dog; and yet even this poison, which seems to require corpuscles of so odd and determinate a nature, is recorded by physicians to have been conveyed by exhalations. *Aretæus* writes, as a learned modern quotes him, *Quòd à rabido cane, qui in faciem, dum spiritus adducitur, tantummodò inspiraverit, & nullo modo momorderit, in rabiem homo agatur.* And as there are relations, among physicians, of animals, that have become rabiosi by having eaten of the parts or excrements of rabid animals; so *Cælius Aurelianus*, who writes, that some have been made to run mad, not by being bitten, but wounded only with the claws of a mad dog, tells us also of a man that fell into a hydrophobia (which is wont to be a high degree of the rabies, and by some of the ancienter writers was employed to signify that disease) without being bitten by a mad dog, but infected *solo odore ex rabido cane attracto*. By which odours, in this and other narratives of poisons, I understand not a bare scholastick species, but a swarm of effluvia, which most commonly are all, or at least some of them odorous. And though it may justly seem strange to many, that the venom of a mad dog should be communicated otherwise than by biting, which is supposed to be the only way he can infect by, it may appear less improbable, because *Matthæus de Gradibus* names a person, who, he says, proved infected after many days, by only having put his hand into the mouth of a mad dog, who did not bite him. And the formerly mentioned *Matthiolus* relates that he saw two that were made rabid without any wound, by the slabber of a mad dog, with which they had the misfortune to be besmeared.

Lib. III. Acutor. Morbor.

Lib. VI. part 6. cap. 2. Sennertus himself affirms of a painter of his acquaintance, that when he had opened a box, in which he had long kept included realgar, a noxious mineral, sometimes used

† In Explicatione Herbarum Biblicarum, cap. 2.

by

by painters, and not unknown to chymifts, and had unfortunately fnuffed in the fteams of it, he was feized with a giddinefs in his head and fainting fits, his whole face alfo fwelling, though by taking of antidotes he efcaped the danger.

DIVERS other examples we have met with in the writings of phyficians, which I forbear to add to thefe, becaufe, I confefs, I very much doubt the truth of them, though the deliverers of fome of them be men of note. But the probability of moft of the things already cited out of credible authors may be ftrengthened by what I fhall now fubjoin, as a further proof of the diftinct nature of effluvia; of which it will be a very confiderable proof, if medicines, which are of a milder and more familiar nature and operation than poifons, fhall yet be able in fome cafes to retain, in their invifible particles fwimming in the air, the fame, though not fo great power of purging, which is known to belong to them when their grofs body is taken in at the mouth. Of this I have elfewhere, on another occafion, given fome examples. To which I fhall now add, that I know a doctor of phyfick that is ufually purged by the odours or exhalations of a certain electuary, whofe cathartick operation, when it is taken in fubftance, is wont to be but languid. And another doctor of my acquaintance caufing good ftore of the root of black hellebore to be long pounded in a mortar, moft of thofe that were in the room, and efpecially the party that pounded it, were thereby purged, and fome of them ftrongly enough. And the learned *Sennertus*, fomewhere, affirms that fome will be purged by the very odour of colocynthis. And it is not to be paffed by unregarded, that in the cafes I have alledged, exhalations that are endowed with occult qualities (for thofe of cathartick medicines are reckoned among fuch) afcend into the air, without being forced from the bodies they belonged to by an external heat.

AND if I would in this place alledge examples of the operations of fuch effluvia, as do not pafs into the air, but yet operate only by the contact of the external parts of the body, I could give inftances, not only of the purgative, but the emetick qualities of fome medicines exerted without their being taken in at the mouth, or injected with inftruments.

THERE are alfo other forts of examples than thofe hitherto mentioned, that argue a determinate nature in the effluxions of fome bodies emitted into the air. Approved writers tell us, that the fhadow of a walnut-tree, with the leaves on it, is very hurtful to the head; and fome inftances they give us of great mifchief it has fometimes done. And though the fhadow, as fuch, is not likely to be guilty of fuch bad effects; yet the effluvia of the neighbouring plant may be noxious enough to the head. For I, that was not at all prepoffeffed with an opinion that it was fo, and therefore, without fcruple, reforted to the fhade of walnut-trees in a hot country, was, by experience, forced to think it might give others the head-ach, fince it did to me, who, thanks be to God, both was, and am ftill very little fubject to that diftemper. And this brings into my mind an obfervation that I have met with among fome ingenious travellers into the *Weft-Indies*, who obferve, in general, and, of late, a countryman of our own affirms it in particular, of the poifonous manchinello-tree, that birds will not only forbear to eat of the fruit of venemous plants, but, as to fome of them, will not fo much as light on the trees; which I therefore mention, becaufe, probably, nature inftructs them to avoid fuch trees by fome noxious fmell, or other emanation that offends the approaching birds. And I remember, that fome of our navigators give it for a rule to thofe that happen to land in unknown iflands, or coafts, that they may venture to eat of thofe parts of fruits which they can perceive the birds, like kind tafters, to have been pecking at before.

NICOLAUS FLORENTINUS (cited by *Sennertus*) tells us of a certain Lombard, that having in a houfe, that he named, at *Florence*, burned a great black-fpider at the flame of a candle,

candle, so unwarily, that he drew in the steams of it at his nostrils, presently began to be much disordered, and fell into a fainting fit, and, for the whole night, had his heart much disaffected, his pulse being so weak, that one could scarce perceive he had any; though afterwards he was cured by treacle, diamosc, and the powder of zedoary mixed together.

And I remember, that being some years ago in *Ireland*, I gathered a certain plant (peculiar to some parts of that country) which the natives call Maccu-buy, because of strange traditions that go about it; the chief of which I found, by trial, not to be true; but yet being satisfied that its operations were odd, and violent enough, I was willing to gratify the chief physician of the country, who was desirous I should propose to him some ways of correcting it; and whilst I was speaking of one, that required the pounding of it, he told me, on that occasion, that intending to make an extract of it with vinegar, he caused his man to beat it well in a mortar, which the man soon repented he had begun to do; and the doctor himself, though at a pretty distance off, was so wrought upon by the corpuscles that issued out into the air, that his head, and particularly his face, swelled to an enormous and disfiguring bulk, and continued tumid for no inconsiderable time after.

I have not leisure to subjoin many more instances, to shew the determinate nature of effluviums, small enough to wander through the air; nor perhaps will it be necessary, if you please but to consider these two things. The first, that many odoriferous bodies, as amber, musk, civet, &c. as they will, by the adhesion of their whole substance, perfume skins, linen, &c. so they will, in time, perfume some bodies disposed to admit their action, though kept at a distance from them. And the other is, that in pestilential fevers, and divers other contagious sicknesses, as the plague, small-pox, or measles, the same determinate disease is communicable to sound persons, not only by the immediate contact of the infected party, but without it, by the contagious steams that exhale from his body into the air. And having said this, and desired you to reflect upon it, I shall conclude this chapter with an experiment, that, possibly, will not a little confirm a great part of it.

Considering then with myself how I might best devise a way of shewing to the very eye, that effluvia, elevated without the help of heat, and wandering in the air, may both retain their own nature, and, upon determinate bodies, produce effects, that a vulgar philosopher would ascribe to occult qualities; I remembered, that I had found by trials (made to other purposes) that volatile and sulphureous salts would so work upon some acid ones, sublimed with mercury, as to produce an odd diversity of colours, but chiefly an inky one; on which account I judged it likely that my aim would be answered by the following experiment.

I took an ounce, or better, of such a volatile tincture of sulphur, as I have elsewhere * taught you to make of quick-lime, sulphur, and sal armoniack, and stopped it up in a phial capable of containing at least twice as much; then taking a paper whereon something had been written with invisible ink, I laid it down six inches off of the phial, which being unstopped, began, upon the access of the fire, to emit white fumes into it, and by these, what was written upon the paper, notwithstanding its distance from the liquor, quickly became very legible, though not quite so suddenly, as if a paper, written with the same clear liquor, were held at the like distance directly over the orifice of the phial. And having caused several pieces of clean paper to be written on, with a new pen

* The liquor here mentioned is, for the main, the same with that described by the author in his book of Colours.

dipped

dipped in the clear ſolution of ſublimate, made in water, it was pleaſant to ſee, how divers of the letters of ſeveral of theſe papers, being placed within ſome convenient diſtance of the phial, would be made plainly legible, and ſome of them more, ſome leſs blackiſh, according to their diſtances from the ſmoking liquor, and other circumſtances. But it was more ſurprizing to ſee, that when I held or laid ſome of theſe papers, though with the written ſide upwards, juſt upon, or over the orifice of the phial, though the contained liquor did not, by ſome inches, reach ſo high, yet the latent letters would become not only legible, but conſpicuous, in about a quarter of a minute of an hour, meaſured by a good watch fit for the purpoſe, as more than one trial aſſured me. And as it may be obſerved, that in ſome circumſtances the ſmoking liquor, and the ſolution of ſublimate, will make an odd precipitate, almoſt of a ſilveriſh colour, ſo in one or two of our trials, we found a like colour produced by the ſteams of that liquor, in ſome of the colourleſs ink. Nor is it ſo neceſſary to employ a viſibly ſmoking liquor for the denigrating of inviſible ink at a diſtance. For I have, to that purpoſe, with good ſucceſs, though not equal to that I have recited, employed a couple of liquors, wherein there was neither ſulphur, nor ſal armoniack, nor ſublimate. What other trials I made with our volatile tincture of ſulphur it is not neceſſary here to relate; only one experiment, which you will poſſibly think odd enough, I ſhall not omit; becauſe it will not only confirm the precedent trials, but alſo much of the foregoing eſſay, by ſhewing the great ſubtilty, and penetrating power of effluviums, that ſeem rather to iſſue out very faintly, than to be darted out with any briſkneſs.

Causing then ſomething to be written with diſſolved ſublimate upon a piece of paper, we folded the paper with the written ſide inwards, and then incloſed this in the midſt of ſix ſheets of paper laid one upon another, not placed one within another, and folded up in the form of an ordinary letter or packet, to be ſealed, that the edges of the incloſing paper, being inſerted one within the other, the fumes might not get into this written paper, but by penetrating through the leaves themſelves; this done, that ſide of the packet on which there was no commiſſure, and on which, were it to be ſent away, the ſuperſcription ſhould be written, was laid upon the orifice of the phial, which (as was before intimated) was ſome inches higher than the ſurface of the liquor, and left there about ten minutes; after which, taking off the folded papers, and opening them, we found that the ſteams had pervaded all the leaves, in which the written paper had been incloſed. For, though the leaves did not appear ſtained or altered, yet the formerly latent characters appeared conſpicuous. I have not time to diſcourſe, whether, and how far this experiment may aſſiſt us to explain ſome odd effects of thunder, or of that ſtrange phænomenon (glanced at in the foregoing chapter) which is ſaid to have happened lately in the kingdom of *Naples*, after the great eruption of *Veſuvius*, which is ſaid to have been followed by the appearing of the croſſes formerly mentioned, ſome of which have been found on the innermoſt parts of linen that had been carefully folded up. But of theſe, and the like things, I ſay, I have now no time to diſcourſe, whether any thing derivable from our experiment may be pertinently applied to their explication. For which reaſon I ſhall add no more, than that afterwards, for further trial, we took a printed book that chanced to be at hand, and which we judged the fitteſt for our purpoſe, becauſe the leaves being broad, they might the better preſerve a ſmall paper to be placed in the midſt of them, from being acceſſible to the exhalations ſide-wiſe, and having put the deſigned paper into this book, and held it to the orifice of the phial, though there were no leſs than twelve leaves between them, yet thoſe letters that happened to be the moſt rightly placed, were made inky in the ſhort ſpace of three minutes, at the utmoſt; though this liquor had been ſo long kept, and ſo often unſtopped to try concluſions with it, that it had probably loſt a good part of the moſt ſpirituous and piercing particles.

NEW EXPERIMENTS TO MAKE FIRE AND FLAME STABLE AND PONDERABLE.

A PREFACE; ſhewing the Motive, Deſign, and Parts of the enſuing TRACT.

THE inducements which put me upon the attempt expreſſed in the title of this eſſay, were chiefly theſe:

FIRST, I conſidered, that the interſtellar part of the univerſe, conſiſting of air and æther, or fluids analogous to one of them, is diaphanous; and that the æther is, as it were, a vaſt ocean, wherein the luminous globes, that here and there, like fiſhes, ſwim by their own motion, or like bodies in whirlpools are carried about by the ambient, are but very thinly diſperſed, and conſequently, that the proportion that the fixed ſtars and planetary bodies bear to the diaphanous part of the world, is exceeding ſmall, and ſcarce conſiderable, though we ſhould admit the ſun and fixed ſtars to be opacous bodies, upon the account of their terminating our ſight; which diffident expreſſion I employ, becauſe I have elſewhere ſhewn by two or three experiments, purpoſely deviſed, that a body may appear opacous to our eyes, and yet allow free paſſage to the beams of light.

I FURTHER conſidered, that there being ſo vaſt a diſproportion between the diaphanous part of the world, and the globes, about which it is every way diffuſed, and with which it is ſometimes in great portions mingled, as in the water, which, together with the earth, makes up the globe we inhabit; and the nature of a diaphanous body's being ſuch, that when the ſun, or any other luminous body illuſtrates them, that which we call light does ſo penetrate, and mix itſelf *per minima*, with them, that there is no ſenſible part of the tranſparent body unenlightened; I thought it worth the inquiry, whether a thing a thing so vastly diffused as light is, were something corporeal, or not? and, whether, in case it be, it may be subjected to some other of our senses, besides our sight, whereby we may examine, whether it hath any affinity with other corporeal beings that we are acquainted with here below?

I DID not all this while forget that the Peripateticks make light a mere quality, and that *Cartesius* ingeniously endeavours to explicate it, by a modification of motion in an æthereal matter; but I remembered too, that the Atomists of old, and of late the learned *Gassendus*, and many other philosophers, assert light to be corporeal; and, that some years since, though I declined to pass my judgment about the question, yet I had employed arguments that appeared plausible enough to shew that it was not absurd to suppose that the sun, which is the fixed star most known to us, might be a fiery body. And therefore doubting, whether the corporeity of light would be in haste determined by mere ratiocinations, I thought it very well worth the endeavouring to try, whether I could do any thing towards clearing the dispute of it by experiments; especially being persuaded, that, though such an attempt should be ineffectual, it would but leave the controversy in its former state, without prejudicing either of the contending hypotheses; and yet, if it should prove successful, the consequences of it would be very great and useful towards the explicating of divers phænomena in divers parts of natural philosophy, as in chymistry, botanicks, and (if there be any such) the allowable part of astrology. (Nor perhaps would it be impossible, by the help of slight theorical alterations, to reconcile the experiments I designed to either of the above-mentioned hypotheses, and so, as to the explication of light, to one another.)

To compass, then, what I aimed at, I thought it was fit, in the first place, to try what I could do by the union of the sun-beams, they being on all hands confessed to be portions (as I may so speak) of true and celestial light; and then I thought fit to try what could be obtained from flame; not only, because that is acknowledged to be a luminary, but because I hoped the difficulties I foresaw in the other trials might be, in some measure, avoided in those made with flame; and if both sorts of them should succeed, the latter and former would serve to confirm each other. According to the method I proposed of handling these two subjects, I should begin with some account of what I attempted to perform in the sun-beams. But the truth is, that when I chanced to fall upon the inquiry that occasioned this paper, besides that the time of the year itself was not over-favourable, the weather proved so extraordinary dark and unseasonable, that it was wondered at; so that, though I was furnished with good burning-glasses, and did several times begin to make trials upon divers bodies, as lead, quicksilver, antimony, &c. yet the frequent interposition of clouds and mists did so disfavour my attempts, that, however they were not all alike defeated, yet I could not prosecute the greatest part of them to my own satisfaction. And therefore, being unwilling to build on them as yet, I shall reserve an account of them for another opportunity; and now proceed to the mention of that sort of experiments, which, depending less on casualties, it was more in my power to bring to an issue.

I KNOW I might have saved both you and myself some time and pains, by omitting several of these trials, and by a more compendious way of delivering the rest. But I rather chose the course I have taken; partly, because the novelty and improbabilities of the truth I deliver, seem to require that it be made out by a good number of trials; partly, because I thought it might not be altogether useless to you, and your friends, to see upon what inducements the several steps were made in this inquiry; partly, because I was willing to contribute something towards the history, that now, perhaps, will be thought fit to be made of the increment or decrement that particular bodies may receive

 by

by being expofed to the fire; and partly, in fine, becaufe the incongruity of the doctrine here afferted to the opinions of the fchools, and the general prepoffeffions of mankind, made me think it fit, by a confiderable variety, as well as number of experiments, to obviate, as far as may be, the differing objections and evafions wherewith a truth fo paradoxical may expect to be encountered.

NEW EXPERIMENTS, &c.

THOUGH there be among the following trials, a diverfity, that invites me, as to rank them into four or five differing forts, fo to affign them as many diftinct fections; yet, for the conveniency of making the references there will be occafion to make betwixt them, I fhall wave the diftinction, and fet them down in one continued feries.

And becaufe I am willing to comply with my hafte, as well as to deal frankly, and without ceremony, with you, I fhall venture to fubjoin the naked tranfcripts of my experiments, as I had in an artlefs manner fet them down, with many others, for my own remembrance, among my *adverfaria*, without fo much as retrenching fome circumftances that relate lefs to my prefent argument than to fome other purpofes.

I shall then begin with the mention of a couple of experiments, which, though they might conveniently enough be referred to another paper, yet I fhall here fet them down, becaufe it feems very proper to endeavour to fhew, in the firft place, that flame itfelf may be, as it were, incorporated with clofe and folid bodies, fo as to increafe their bulk and weight.

TRIALS of the Firft Sort.

EXPERIMENT I.

[A Piece of copper-plate not near fo thick as a half-crown, and weighing two drachms and twenty-five grains, was fo placed with its broad part horizontal, in a crucible, whofe bottom had a little hole in it for fumes to get out at, that it could not be removed from its pofition, nor be eafily made to drop down or lofe its level to the horizon, though the crucible were turned upfide down; then about an ounce and half of common fulphur being put into a taller and broader crucible, that wherein the copper ftuck was inverted into the orifice of it, that the fulphur being kindled, the flame, but not the melted brimftone, in fubftance, might reach the plate, and have fome vent beyond it at the above-mentioned hole. This brimftone burned about two hours, in which time it feemed all to have been refolved into flame, no flowers of fulphur appearing to have fublimed into the infide of the upper crucible; and though the copper-plate were at a confiderable

confiderable diftance from the ignited fulphur, yet the flame feemed to have really penetrated it, and to have made it vifibly fwell or grow thicker; which appeared to be done by a real acceffion of fubftance; fince, after we had wiped off fome little adhering fordes, and with them divers particles of copper that ftuck clofe to them, the plate was found to weigh near two-and-thirty grains more than at firft, and confequently to have increafed its former weight by above a fifth part.]

EXPERIMENT II.

[HAVING, by refining one ounce of fterling filver with faltpetre, according to our way, reduced it to feven drachms or fomewhat lefs; we took a piece of the thus purified filver that weighed one drachm wanting two grains, and having ordered it as the copper-plate had been in the former experiment, after the flame of above one ounce and a quarter of fulphur (that quantity chancing to be fuitable to the capacity of the crucible) had, for about an hour and a half, beat upon it, the filver-plate feemed to the eye fomewhat fwelled, and the lower furface of it, that was next the flame, was brought to a great fmoothnefs, the weight being increafed to one drachm five grains and three quarters; which increafe of weight falling fo fhort of that, which was gained by the copper, I leave it to you to confider, whether the difference may be attributed to the clofenefs and compactnefs of the filver, argued by its being heavier in fpecie than copper; or to the greater congruity of the pores of copper to be wrought on by the fiery menftruum; or to fome other caufe.]

IF you fhould here afk me, by what rational inducements I could be led to entertain fo extravagant an expectation, as, that fuch a light and fubtle body as flame fhould be able to give an augmentation of weight to fuch ponderous bodies as minerals and metals; I fhall now, to avoid making anticipations here, or needlefs repetitions hereafter, return you only this anfwer; that the expectation you wonder at may juftly be entertained upon the fame, or fuch like inducements, as you may eafily difcover in another paper, intitled *Corollarium Paradoxum*. For, fuppofing, upon the grounds there laid, that flame may act upon fome bodies as a menftruum, it feems no way incredible, that, as almoft all other menftruums, fo flame fhould have fome of its own particles united with thofe of the bodies expofed to its action; and the generality of thofe particles being (as it is fhewn in the paradox about the fewel of flames) either faline, or of fome fuch piercing and terreftrial nature, it is no wonder, that being wedged into the pores, or being brought to adhere very faft to the little parts of the bodies expofed to their action, the acceffion of fo many little bodies, that want not gravity, fhould, becaufe of their multitude, be confiderable upon a balance, whereon one or two, or but few of thefe corpufcles, would have no vifible effect.

I COULD here, if it were expedient, mention fome odd fcruples about the preceding experiments, and fome alfo of the fubfequent; but left you fhould, with fome other of my friends, upbraid me with being too jealous and fceptical, I will not trouble you with them, but proceed to the next fort of trials, wherein, though the matter were not always manifeftly beaten on by a fhining flame; yet it was wrought on by that, which would be called flame, by thofe who take not that word ftrictly, but in a latitude, and which this igneous fubftance may more properly be ftiled, than it can be called common fire, this being vifibly harboured in burning coals, or other grofs materials, from which our metals were fenced. And I have elfewhere fhewn, by experiment, that vifibility is not in all cafes neceffary to actual flame, particularly when the eye receives a predominant impreffion from another light.

TRIALS

TRIALS of the Second Sort.

EXPERIMENT III.

[INTO a crucible whofe fides had been purpofely taken down to make it very fhallow, was put one ounce of copper-plates; and this being put into our cupelling-furnace, and kept there two hours, and then being taken out, we weighed the copper (which had not been melted) having firft blown off all the afhes, and we found it to weigh one ounce and thirty grains.]

EXPERIMENT IV.

[SUPPOSING that copper, being reduced to filings, and thereby gaining more of fuperficies in proportion to its bulk, would be more expofed to the action of the fire than when it is in plates, as it was formerly, we took an ounce of that metal in filings, and putting them upon a very fhallow crucible, and under a muffler, we kept them there about three hours (whilft other things that required fo long a time were cupelling); and afterwards taking them off, we found them of a very dark colour, not melted, but caked together in one lump, and increafed in weight (the afhes and duft being blown off) no lefs than about forty-nine grains. Part of which increment, above that obtained by the copper-plates in the former experiment, may not improbably be due to the longer time, that in this experiment the filed copper was kept in the fire.]

EXPERIMENT V.

[BEING willing to fee, whether calcined hartfhorn, that I did not find eafy to be wrought on by corrofive menftruums, would retain any thing of the flame, or fire, to which it fhould be expofed; we weighed out one ounce of fmall lumps of hartfhorn, that had been burnt till they appeared white, and having put them into a crucible, and kept them in a cupelling-furnace for two hours, whilft fome metals were driving off there by the violence of the fire; we found, that when they were taken out they had loft fix or feven grains of their former weight; perhaps either becaufe, notwithftanding the external whitenefs of the lumps, the internal parts of fome of them might not be fo exquifitely calcined, but retain fome oleaginous or other volatile fubftance; or becaufe, having omitted to ignite them well before they were weighed, they may have fince their firft calcination imbibed fome moift particles of the air. Which conjecture feemed the likelier, becaufe having kept them a while in the fcales they were weighed in, they did within two or three hours make it fomewhat preponderate. On which occafion I fhall add, that, at the fame time, with the hartfhorn we put in one ounce of well-heated brick, and kept that likewife in the furnace for above two hours; at the end of which, weighing it whilft it continued hot, we did not find it to have either fenfibly got or loft; but, fome time after it feemed upon the balance to have imbibed fome, though but very little moifture from the air.]

EXPERIMENT VI.

[Upon a good cupel we put one ounce of Engliſh tin of the better ſort, and having placed it in the furnace under the muffler, though it preſently melted, yet it did not forſake its place, but remained upon the concave ſurface of the cupel, till at the end of about two hours, it appeared to have been well calcined; and then being taken out and weighed by itſelf, the ounce of metal was found to have gained no leſs than a drachm.]

EXPERIMENT VII.

[An ounce of lead was put upon the cupel, made of calcined hartſhorn, and placed under the muffler, after that the cupel was firſt made hot, and then weighed. This lead did not enter into the cupel, but was turned into a pretty kind of litharge on the top of it, and broke the cupel, whereby ſome part of the cupel was loſt in the furnace, and yet the reſt, together with the litharge, weighed ſeven grains more than the ounce of lead and the heated cupel did, when they were put in.]

But, becauſe, though this trial ſhewed that ſome weight was gained either by the metal or cupel, or both, yet it did not by this appear what either of them acquired; it ſeemed fit to ſubjoin a further trial.

EXPERIMENT VIII.

[We took a cupel about two ounces in weight, made of about ten parts of bone-aſhes, and one of charcoal-aſhes, made up together with ale. This was by itſelf put in a cupeling-furnace, under a muffler; and the laborant, well-verſed in weighing, was ordered to take it out, when it was throughly and highly heated, and to weigh it whilſt it was in that condition, I being then preſent; this being done, it was forthwith placed again under the muffler, where ſome metalline bodies were cupelling, and kept there for about two hours, at the end of which time it was taken out red-hot, and preſently put into the ſame balance, as before, which was already faſtened to a gibbet; where having cauſed the adhering aſhes to be blown off, I found, that whereas, when it was firſt taken from under the muffler, we had but two ounces and two grains, now the ſame weight being put into the oppoſite ſcale, it had gained very near one-and-twenty grains. And here note, that it was not without ſome cauſe, that I was careful to have the cupel weighed red-hot. For I had a ſuſpicion, that, notwithſtanding the dryneſs of the bone, it might receive ſome little alteration of weight by imbibing ſome little particles wandering in the air; which ſuſpicion the event juſtified. For leaving the cupel counterpoiſed to cool in the balance, in a ſhort time it began ſenſibly to preponderate; and ſuffering it to continue there nine or ten hours, till we had occaſion to uſe the balance, I found it at the end of that time to be about three grains heavier than before.]

This was not the only trial we made about the augmenting the weight of cupels; but this being the faireſt, and exempt from thoſe miſchances, from which the other were not altogether free, I ſhall content myſelf to have ſet down this; in the mention of which I thought fit to take notice of the increaſe of the weight of the cupel after it had lain in the ſcales, and alſo that we weighed it at firſt, whilſt it was throughly hot, becauſe thoſe thofe circumftances, as not being fufpected, may eafily be left unthought on, even by fkilful experimenters; and yet the weighing of the cupel, when it had been well nealed, and the not weighing it foon enough after it is taken from the fire, may keep thofe that fhall reiterate this experiment, from making it cautioufly and accurately enough. For if the former circumftance be omitted, that which the cupel may feem to have loft of its fubftance, was nothing but the adventitious moifture of the air; and if the latter circumftance be neglected, the weight it may feem to have gained from the fire, was indeed due to the waterifh particles of the air. I could wifh alfo that trial were made, whether the fuccefs would be the fame in cupels made in differing forts of bone-afhes, and other materials, wont to be employed for that purpofe. For that I had not opportunity to do.

EXPERIMENT IX.

Iron being a metal, that experience had informed me will more eafily be wrought on by fluids that have particles of a faline nature in them, than is commonly believed; it was not unreafonable to expect that flame would have a greater operation on it (efpecially if it were before-hand reduced to fmall parts) than on any of the bodies hitherto defcribed. Which fuppofition will be confirmed by the fhort enfuing note.

[Four drachms of filings of fteel being kept two hours on a cupel under a muffler, acquired one drachm fix grains and a quarter increafe of weight.]

EXPERIMENT X.

[A piece of filver, refined in our own laboratory, being put upon a cupel under a muffler, and kept there for an hour and half, whilft other things were refining, was taken out and weighed again, and, whereas before it weighed three drachms, thirty-two grains and a quarter, it now weighed, in the fame fcales, three drachms, thirty-four grains and a half, or but little lefs.]

Finding this memorial, among divers others about the weight of bodies expofed to the fire, I thought it not amifs to annex it in this place; though finding it to be but fingle, I would not have it to be relied on, till further trial have been made, to difcover whether it was more than a cafual and anomalous experiment; and if the filver had not been refined, I fhould have fufpected that the copper that was blended with it, as it is ufually blended with common filver, might have occafioned the increafe of weight.

POSTSCRIPT.

Since the foregoing experiment was firft fet down, meeting with an opportunity to reiterate the trial once more, we did it with half an ounce of filings of filver, well refined with lead in our own laboratory, and kept it about three hours upon the cupel; after the end of which time taking it out, we found it to be of a lefs pleafant colour than it was of before, and melted (though not fo perfectly) into a lump, which weighed four drachms and fix grains; and yet the fuccefs being fo odd, and, if it prove conftant, of fuch moment, I could wifh the trial were further repeated in differing quantities of the metal.

EXPE-

EXPERIMENT XI.

[We took a drachm of filings of zink or ſpelter, and having put it upon a cupel under a muffler, we kept it there in a cupelling-fire about three hours (having occaſion to continue the cupellation ſo long for other trials); then taking it off the cupel, we found it to be caked into a brittle and dark coloured lump, which looked as if the filings had been calcined. This being weighed in the ſame ſcales gained full ſix grains, and ſo a tenth part of its firſt weight.]

EXPERIMENT XII.

Among our various trials upon common metals, we thought fit to make one or two upon a metal brought us from the *Eaſt-Indies*, and there called Tutenâg, which name being unknown to our European chymiſts, I have elſewhere endeavoured to give ſome account of the metal itſelf; whence I ſhall borrow the enſuing note, as directly belonging to our preſent purpoſe.

[Two drachms of filings of tutenâg being put upon a cupel, and kept under the muffler for about two hours, the filings were not melted into a lump of metal, but looked as if ceruſs and minium, being powdered, had been mingled together; ſome of the parts appearing diſtinctly white, and others red: the calx, being put into the balance, appeared to have gained twenty-eight grains and a quarter. Another time the experiment being re-iterated with the like circumſtances, we found that two drachms of the filed tutenâg gained the like increaſe of weight, abating leſs than one grain.]

So that this Indian metal ſeems to have gained more in the fire, in proportion to its weight, than any we have hitherto made trial of.

EXPERIMENT XIII.

[Being deſirous to confirm, by a clear experiment, what I elſewhere deliver contrary to the vulgar opinion of thoſe that believe, that in all cupellations almoſt all the lead that is employed about them, does, together with the baſer metals that are to be purged off from the ſilver or gold, fly away in ſmoke, as indeed, in ſome ſort of cupellations, a good proportion may be blown off that way; we took two ounces of good lead, and one drachm of filings of copper, and having cauſed a cupel to be ignited, and nimbly taken out of the furnace, and weighed, whilſt it was very hot, it was preſently put back, together with the two metals laid on it, into the cupelling-furnace, where having been kept for about two hours, it was taken out again, and it was found, according to what (as I elſewhere * note) uſes to happen, in ſuch circumſtances, to have nothing on the ſurface of it worth weighing diſtinctly in the ſcales, in which the cupel, with what was ſunk into it, amounted to four ounces, three drachms, and eleven grains, which wanted but nine grains of the whole weight of the cupel and the two metals, when they were all three together committed to the fire.] So that, though we make a liberal allowance for the increment of weight that may, with any probability, be ſuppoſed to have been at-

* Eſſay the Sixth, of the Uſefulneſs of Natural Philoſophy.

tained by the cupel, and what was put upon it, yet it will eafily be granted, that very much the greater part of the metals was not driven off in fumes, but entered into the fubftance of the cupel.

TRIALS of the Third Sort.

AFTER having fhewn, that either flame, or the analogous effluxions of the fire, will be, what chymifts would call corporified with metals and minerals expofed naked to its action; I thought it would be a defirable thing to difcover, whether this flame or igneous fluid were fubtle enough to exercife any fuch operation upon the light bodies fheltered from its immediate contact, by being included in clofe veffels; but it being very difficult to expofe bodies in glaffes to fuch vehement fires, without breaking or melting the glafs, and thereby lofing the experiment; I thought fit, firft, to employ crucibles carefully luted together, that nothing might vifibly get in or out; and of that attempt I find among my notes the following account.

EXPERIMENT XIV.

[WE took an ounce of fteel frefhly filed from a lump of that metal, that the filings might not be rufty, and having included them betwixt two crucibles, as formerly, kept them for two hours in a ftrong fire, and fuffered them to continue there till the fire went out; the crucibles being unluted, the filings appeared hard-caked together, and had acquired a dark colour, fomewhat between black and blue, and were increafed five grains in weight.]

THE foregoing experiment being the firft I mention of this kind, it will not be amifs to confirm it, by annexing the following memorial.

[AN ounce of filings of fteel being put between the crucibles luted together, after they had been kept about an hour and half in the fire, were taken out, and being weighed, were found to have gained fix grains.]

EXPERIMENT XV.

[Two ounces of copper-plate were put into a new crucible, over which a leffer was whelmed, and the commiffures were clofed with lute, that nothing might fall in. After the fame manner, two ounces of tin were included betwixt crucibles, and alfo two ounces of lead; thefe being put into the cupelling-furnace, were kept in a ftrong fire about an hour and a half, while fomething elfe was trying there. And then being taken out, the event was, that the copper-plates, though they ftuck together, were not quite melted, and feemed, fome of them, to have acquired fcales like copper put into a naked fire, and the two ounces had gained eight grains in weight. The lead had broke through the bottom of the crucible, and thereby hindered the defigned obfervation. The tin acquired fix grains in weight, and was, in part, brought to a pure white calx, but much more of it was melted into a lump of a fine yellow colour, almoft like gold, but deeper.]

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The profecution of this trial, as to the copper-plates, you will meet with in Experiment XXI. to which I therefore refer you.

N. B. Because lead, in cupellation, enters the cupel, we were willing to try, if we could fo far hinder it from doing fo, as to make fome eftimate what change of weight the operation of the fire would make in it; and therefore being able already to make a near guefs how much a quantity of tin may gain by being calcined on a cupel, and remembering alfo, from fome of my former trials, the indifpofition which tin gives lead to cupellation, we mixed a drachm of tin with two ounces of lead, and expofing the mixture (in a cupel) to the fire under a muffler, we firft brought it to fufion, and then it feemed at the top dry, and fwelled, and difcoloured; notwithftanding which, having continued the operation a good while, becaufe of other things that were to be done with the fame fire, we were not lucky enough to bring the experiment to an iffue worth the relating here, in reference to the fcope above-propofed, though in relation to another, the fuccefs was welcome enough.]

EXPERIMENT XVI.

[Supposing, that if copper were beaten into thinner plates than thofe we lately ufed, and kept longer in the fire, this would have a more confiderable operation upon them, we took one ounce of very thinly hammered pieces of copper, and putting them betwixt two crucibles (one whelmed over another) as in Experiment XV. with fome lute at the corners of the juncture, to keep the fire from coming immediately at the metal, we kept them in the cupelling-furnace about three hours, and then disjoining the veffels, we found the metal covered with a dark and brittle fubftance, like that defcribed in the above-recited experiment. Which fubftance, when fcaled off, difclofed a finely coloured metal, which, together with thefe burned fcales, amounted to one-and twenty grains above the weight that was firft put in.]

If, when thefe things were doing, I had been furnifhed with a very good lute, which is no fuch eafy thing to procure, as chymifts that have not frequently employed vulgar lutes, are wont to think; I would have made a trial of the enfuing experiment, for a good while, in the naked fire, notwithftanding that divers metalline minerals will fcarce be brought to fufion in glaffes, efpecially without fuch a fire, whofe violence makes them break the veffels. For I thought, that by making a fit choice of the metals to be employed, I could prevent that inconvenience; but wanting the accommodations I defired, and yet prefuming, that in a fand-furnace I might by degrees adminifter heat enough to melt fo fufible a metal as fine tin, and keep it in fufion; I refolved to make fome trials, firft upon that, and then upon another metal. For though I was not fure of being then able to profecute the experiment far enough; yet I hoped, I might, at leaft, fee fome effects of my firft trial, which would enable me to guefs what I was to expect from a complete one.

EXPERIMENT XVII.

[We took then a piece of fine block-tin, and in a pair of good fcales weighed out carefully half a pound of it; this we put into a choice glafs retort, and kept it for two days, or thereabouts, in a fand-furnace, which gave heat enough to keep the metal in fufion without cracking the glafs. Then taking out the mixture, we carefully weighed it it in the fame fcales, and found the fuperficies a little altered (as if it were difpofed to calcination) and the weight to be increafed about two grains, or fomewhat better.]

EXPERIMENT XVIII.

[The other experiment I tried in glaffes, was with mercury, hoping, that, if I could make a precipitate *per fe*, in a hermetically fealed glafs, I fhould, by comparing the weight of the precipitate, and the quickfilver that afforded, have a clear experiment to my purpofe; and I fhould have no bad one, if I could but make it fucceed with a glafs, though not fealed, yet well ftopped; inftead of thofe infernal glaffes (as they call them) which are commonly ufed, and wont to be left open (though fome flightly ftop them with a little paper, or cotton); but though, partly, that I might a little diverfify the experiment, and make it the more likely to fucceed in one or other of the glaffes, I divided the mercury, and diftributed it amongft feveral of them, and but a little to each, the fuccefs did not anfwer expectation, the hermetically fealed glaffes being unluckily broken; and the precipitation in the others proceeding fo flowly, that I was, by a remove, obliged to leave the trial imperfect; only I was encouraged (in cafe of a future opportunity) to renew it another time, by finding, that moft of the glaffes, though tall, and ftopped with fit corks, afforded fome very fair precipitate, but not enough to anfwer my defign.]

TRIALS of the Fourth Sort.

MOST of the experiments hitherto recited having been made, as it were, upon the by with others, whofe exigencies it was fit thefe fhould comply with; very few of the expofed bodies were kept in the cupelling-fire above two hours, or thereabouts. Upon which account, I thought fit to try, how much fome bodies that had been already expofed to the fire, would gain in weight, by being again expofed to it; efpecially confidering, that moft calcinable bodies (for I affirm it not of all) which yield rather calces than afhes, by being without additament reduced in the fire to fine powder, feemed to be by that operation opened, or (as a chymift would fpeak) unlocked, and therefore, probably, capable of being further wrought upon, and increafed in weight, by fuch a menftruum, as I fuppofed flame and igneous exhalations to be. And about this conjecture, I fhall fubjoin the enfuing trials.

EXPERIMENT XIX.

[One ounce of calx of tin that had been made *per fe*, for an experiment in our own laboratory, being put in a new cupel, and kept under the muffler for about two hours, was taken out hot, and put into the fcales, where the powder appeared to have gained in weight one drachm, and thirty-five grains, by the operation of the fire, which made it alfo look much whiter than it did before, as appeared by comparing it with fome of the calx that

that had not been expofed to the fecond fire; no part of the putty was, as we could perceive, melted by the vehemence of the fire, much lefs reduced into metal.]

EXPERIMENT XX.

[Out of a parcel of filings of fteel that had been before expofed to the fire, and had its weight thereby increafed fome grains, not fcruples; we took an ounce, and having expofed it at the fame time with the calx of tin, and, for the fame time, kept it in the fire, we took it out at the two hours end; and found the weight to be increafed two drachms, and two-and-twenty grains. The filings were very hard baked together, and the lump being broken, looked almoft like iron.]

EXPERIMENT XXI.

The following experiment, though it may feem in one regard but a continuation of the fifteenth, yet it has in this fomething peculiar from all the foregoing, that not only it affords an inftance of the increafe of weight obtained by a metal at the fecond time of its being expofed to the fire, but fhews alfo, that fuch an increment may be had, though this fecond ignition be made in clofe veffels.

[Some of the copper mentioned in Experiment XV. being accidentally loft, one ounce and four drachms of what remained was included betwixt two crucibles and expofed to a ftrong fire for two hours, and fuffered to continue there till the fire went out; when it was taken out, it appeared to have gained ten grains in weight, and to have upon the fuperficial parts of the plates (as we obferved) divers dark-coloured flakes, fome of which ftuck to the metal, but more, upon handling it, fell off.]

And here I fhall conclude one of the two parts of our defigned treatife; for, though, I remember, that thefe were not all the trials that were made and fet down upon the fubject hitherto treated of; yet thefe are the chief, that having efcaped the mifchances which befel fome others, I can meet with among my promifcuous memorials; whofe number, when I drew them together, I could fcarce increafe, having by all thefe, and other trials of differing kinds, wafted my cupels and commodious glaffes, where I could not well repair my lofs. Whether I fhould have been able by reduction, fpecifick gravity, or any other of the ways, which I had in my thoughts, to make any difcovery of the nature of the fubftance, that made the increment of weight in our ignited bodies; the want as well of leifure, as of accommodations requifite to go through with fo difficult a tafk, keeps me from pretending to know. But thefe three things, I hope, I may have gained by what has been delivered; the firft, that we fhall henceforth fee caufe to proceed more warily in the experiments we make with metals in the fire, efpecially by cupellation. The next, that it will juftify, and perhaps, procure an eafier affent to fome paffages in my other writings, that have relation to the fubftance, whatever it be, that we are fpeaking of. And the third (which is the principal) that it will probably excite you, and your inquifitive friends, to exercife their fagacious curiofity in difcovering what kind of fubftance that is, which, though hitherto overfeen by philofophers themfelves, and, being a fluid far more fubtle than vifible liquors, and able to pierce into the compact and folid bodies of metals, can yet add fomething to them that has no defpicable weight upon the balance, and is able, for a confiderable time, to continue fixed in the fire.

ADDI-

ADDITIONAL EXPERIMENTS,

About ARRESTING and WEIGHING of

IGNEOUS CORPUSCLES.

EXPERIMENTS to difcover the increafe in weight of bodies, though inclofed in glaffes, being thofe that I confidered as likelieft to anfwer what I defigned in the hitherto profecuted attempt, and finding the feventeenth experiment, as well as the next (tried upon mercury) to be very flow, and its performance not to be very great, I began to call to mind, what, many years ago, experience had fhewn me poffible to be performed, as to the managing glafs-veffels, even without coating them, in a naked fire, provided a wary perfon were conftantly employed to watch them. And fuppofing hereupon, that in no longer time than a laborant might, without being tired, hold out to attend a glafs, a metal expofed in it to a naked fire might afford us a much more profperous trial than that lately referred to, I afterwards refolved, when I fhould be able to procure fome glaffes conveniently fhaped, to profecute my defign; in purfuance of which, though I had not any furnaces fitted for my purpofe, I directed a laborant to make the following trials.

EXPERIMENT I.

[We took eight ounces (*Troy* weight) of block-tin, which being cut into bits, was put into a good round phial with a long neck, and then warily held over quick coals, without touching them, till it was melted; after which, it was kept almoft continually fhaken, to promote the calcination, near an hour, the metal being all the while in fufion, and the glafs kept at fome diftance from the thoroughly kindled coals. The moft part of this time the orifice of the phial was covered with a cap of paper (which fometimes fell off by moving the glafs) to keep the air, and fteams of the coals, from getting into the neck. And at the end of this time, he, that held the glafs, being tired, and having his hand almoft fcorched, the phial being removed from the fire, was broken, that we might take out the metaline lump, which had a little darkifh calx here and there upon the upper furface, but much more beneath, where it had been contiguous to the bottom of the glafs; then putting all this, carefully freed from little fragments of broken glafs, into the fame balance with the felf-fame counterpoife I had ufed before, I found, according to my expectation, an increafe of weight, which amounted to eighteen grains, that the tin had acquired by this operation.]

EXPE-

EXPERIMENT II.

[This done, we ſeparated the calx for fear of loſing it, and having melted the metal in a crucible, that by pouring it out it might be reduced to thin plates, capable of being cut in pieces, and put into ſuch another phial as the laſt; we weighed it again, together with the lately reſerved calx, but found, that notwithſtanding all our care, we had loſt three grains of the eighteen we had gained. This done, we put the metal into another phial. But in regard the neck was ſhorter than that of the former, and could not, like it, be long held in one's hand; and becauſe alſo I was willing to ſee what intereſt the ſhaking of melted tin has in the quickneſs of the calcination, the glaſs, which had a ſtopple of paper put to it to keep out ſmoke and air, was held at ſome diſtance from the coals, only whilſt the tin was melting; and then was warily laid upon them, and kept there for two hours; at the end of which it was again taken off, and the metal weighed with the ſame counterpoiſe and balance, as formerly; and then it appeared to amount to eight ounces, twenty-four grains, and to have much more ſeparable calx than at the firſt time. Nor did I much wonder, that the weight ſhould be increaſed, in this laſt operation, but nine grains in two hours, and in the former, twice ſo many in half the time; ſince, during the two hours, the glaſs was kept in one poſture, whereas, in the firſt operation, it was almoſt perpetually ſhaken all the while it was kept in fuſion. And it is obſerved, that the agitation of melted minerals will much promote the effect of the fire upon them, and conduce to their calcination.

EXPERIMENT III.

Though theſe trials might well ſatisfy a perſon not very ſcrupulous, yet to convince even thoſe that are ſo, I undertook, in ſpite of the difficulties of the attempt, to make the experiment in glaſſes hermetically ſealed, to prevent all ſuſpicions of any acceſſion of weight accruing to the metal, from any ſmoke or ſaline particles getting in at the mouth of the veſſel. And in proſecution of this deſign, I thought upon a way of ſo hermetically ſealing a retort, that it might be expoſed to a naked fire, without being either cracked, or burſt; an account of which trial was ſet down.

[Eight ounces of good tin, carefully weighed out, was hermetically ſealed up in a new ſmall retort, with a long neck, by which it was held in one's hand, and warily approached to a kindled charcoal fire, near which the metal was kept in fuſion, being alſo ever now and then ſhaken for almoſt half an hour, in which time it ſeemed to have acquired on the ſurface ſuch a dark colour, as argued a beginning of calcination, and it both emitted fumes, that played up and down, and alſo afforded two or three drops of liquor in the neck of the retort. The laborant being not able to hold the glaſs any longer, it was laid on quick coals, where the metal continued above a quarter of an hour longer in fuſion; but before the time was come, that I intended to ſuffer it to cool, in order to the removing it, it ſuddenly broke in a great multitude of pieces, and with a noiſe like the report of a gun; but (thanks be to God) it did no harm neither to me, nor others, that were very near it. In the neck we found ſome drops of a yellowiſh liquor, which a virtuoſo, that taſted it, affirmed to be of an odious, but peculiar ſapor; and as for the ſmell, I found it to be very ſtinking, and not unlike that of the diſtilled oil of fiſh.]

But though our firſt attempt of this kind had thus mi arrie d, we were not thereby diſcouraged, but, in proſecution of the ſame deſign, made the enſuing trial.

EXPERIMENT IV.

[The tin, which had been before (in the firſt, or ſome ſuch experiment) partly calcined in a glaſs, being melted again in a crucible, that it might be reduced to pieces ſmall enough to be put into another glaſs, was put again into the ſcales, and the ſurpluſage being laid aſide, that there might remain juſt eight ounces; theſe were put into a bolt-head of white glaſs, with a neck of about twenty inches long, which being hermetically ſealed (after the glaſs had been a while kept over the fire, leaſt that ſhould break by the rarefaction of the air) the metal was kept in fuſion for an hour and a quarter, as (being hindered by a company of ſtrangers from being there myſelf) the laborant affirmed. Being unwilling to venture the glaſs any longer, it was taken from the fire, and when it was grown cold, the ſealed end was broken off: but before I would have the bottom cut out, I obſerved that the upper ſurface of the metal was very darkly coloured, and not at all ſmooth, but much and very oddly aſperated; and the lower part had between the bottom, and the lower part of the lump, a pretty deal of looſe dark-coloured calx, though the neighbouring ſurface, and ſome places of the lump itſelf, looked by candle-light (it being then night) of a golden colour. The lump and calx together were weighed in the ſame ſcales carefully, and we found the weight to have increaſed twenty-three grains, and better, though all the calx we could eaſily ſeparate, being weighed by itſelf, amounted not to four ſcruples, or eighty grains.

For confirmation of this experiment, I ſhall ſubjoin another, wherein but a quarter of ſo much metal was employed, with ſuch ſucceſs, as the annexed memorial declares.

EXPERIMENT V.

[Two ounces of filings of tin were carefully weighed, and put into a little retort, whoſe neck was afterwards drawn ſlenderly out into a very ſmall apex: then the glaſs was placed on kindled coals, which drove out fumes at the ſmall orifice of the neck for a pretty while. Afterwards the glaſs, being ſealed up at the apex, was kept in the fire above two hours; and then being taken off, was broken at the ſame apex; whereupon I heard the outward air ruſh in, becauſe, when the retort was ſealed, the air within it was highly rarefied. Then the body of the glaſs being broken, the tin was taken out, conſiſting of a lump, about which there appeared ſome grey calx, and ſome very ſmall globules, which ſeemed to have been filings melted into that form. The whole weighed two ounces, twelve grains, the latter part of which weight appeared to have been gained by the operation of the fire on the metal. In the neck of the retort, where it was joined to the body, there appeared a yellowiſh and clammy ſubſtance thinly ſpread, which ſmelt almoſt like the fœtid oil of tartar.

EXPERIMENT VI.

To vary the foregoing experiments by making trials on a mineral, that is held to be of a very metalline nature, but is not a true metal, nor will be brought to fuſion by ſo mode-

moderate a heat, as will ſuffice to melt tin, and yet has parts leſs fixed than tin, as being far more eaſily ſublimable, we thought fit to make the following experiment.

[We took an ounce of filings of zink, carefully weighed; and having as carefully put them into a round bolt-glaſs, we cauſed the neck to be drawn out very ſlender, and then ordered the laborant to keep it upon quick coals for the appointed time. Afterwards returning home, I called for the glaſs, which, he ſaid, he had kept four hours upon the coals; anſwering me alſo, that there did, for a great part of the time, ſmoke appear to aſcend from the zink, and get out at the unſtopped apex. And in effect I obſerved, that the upper part of the glaſs was lined with flores or ſublimate of a darkiſh grey. The glaſs being dexterouſly cut aſunder, we took out not only the filings of zink, ſome of which were melted into little globules, but the flores too; and yet weighing all theſe in the ſame ſcales we had uſed before, we found five grains and ſomewhat better wanting of an ounce. Which we the leſs wondered at, becauſe of the continuance of the lately mentioned exhalations emitted by the filed mineral.]

EXPERIMENT VII.

For more ample confirmation of the truth diſcovered by what I have been reciting about tin, I thought fit to try the like experiment upon another metal, which, though of ſomewhat more difficult fuſion than tin, I had reaſon to think might, if employed in a moderate quantity, and warily managed, be kept melted in glaſs without breaking it. And accordingly, having carefully weighed out four ounces of good lead, cut beforehand into pieces little enough for the orifice of the glaſs, I cauſed them to be put into a ſmall retort with a long neck, wherein was afterwards left but an orifice not much bigger than a pin's head: then leaving directions with the laborant what to do, becauſe I was myſelf called abroad, at my return he brought me, together with the glaſs, this account; that he had kept it over and upon the coals two hours, or better, and then ſuppoſing the danger of breaking the glaſs was over, he had ſealed it up at the little orifice newly mentioned, and kept it on the coals two hours longer. Before the glaſs (which I found to be well ſealed) was broken, I perceived the pieces of lead to have been melted into a lump, whoſe ſurface was dark and rugged, and part of the metal to have been turned into a dark-coloured powder or calx: all this being taken out of the retort, was weighed in the ſame balance, whereon the lead appeared to have gained by the operation ſomewhat above thirteen grains.

EXPERIMENT VIII.

To ſhew that metals are not the only bodies that are capable of receiving an increaſe of weight from the fire, I thought fit to make upon coral a trial, whereof my memorial gives me this account.

[Little bits of good red coral, being hermetically ſealed up in a thin bubble of glaſs, after two drachms of them had been weighed out in a pair of nice ſcales, were warily kept at ſeveral times over and upon kindled coals, and at length being taken out for good and all, were found of a very dark colour, and to have gained in weight three grains and about a half.]

EXPERIMENT IX.

One experiment there is, which, though it might have come in more properly at another place, is not to be omitted in this, becauſe it may invite us to conſider, whether, in the foregoing experiments, excepting thoſe made on lead and tin in ſealed veſſels, there may not be more of the fire adherent to, or incorporated with the body expoſed to it, than one would conclude barely from the recited increments of their weight. For having taken very ſtrong freſh quick-lime, provided on purpoſe for choice experiments, and expoſed it, before the air had time to ſlake it, upon the cupel, to a ſtrong fire, where it was kept for two hours; I found that it had increaſed in weight even ſomewhat beyond my expectation. For being ſeaſonably put into the balance, the lumps, that weighed, when expoſed, but two drachms, amounted to two drachms and twenty-nine grains; which makes this experiment a pregnant one to our purpoſe. For by this it appears, that notwithſtanding a body may for many hours, or even for ſome days, be expoſed to a very violent fire, yet it may be ſtill capable of admitting and retaining freſh corpuſcles; ſo that, though well-made lime be uſually obſerved to be much lighter than the ſtones whereof it is made, yet this lightneſs does not neceſſarily prove, that, becauſe a burnt lime-ſtone has loſt much of its matter by the fire, it has therefore acquired no matter from the fire; but only infers, that it has loſt far more than it has got. And this may give ground to ſuſpect, that in moſt of the foregoing trials, the acceſſion of the fiery particles was greater (though in ſome more, in others leſs ſo) than the balance diſcovered; ſince, for aught we know, divers of the leſs fixed particles of the expoſed body might be driven away by the vehemence of the heat; and conſequently the igneous corpuſcles, that faſtened themſelves to the remaining matter, might be numerous enough not only to bring the acceſſion of weight, that was found by the ſcales, but to make amends for all the fugitive particles, that had been expelled by the violence of the fire. And ſince ſo fixed a body as quick-lime is capable of being wrought upon by the igneous effluvia, ſo as that they come to be, as it were, incorporated with it, it may, perchance, be worth conſidering, whether in other calcined, or incinerated bodies, the remaining calces, or aſhes, may not retain more than the bare impreſſion (unleſs that be ſtretched to mean ſome participation of a ſubſtance) of the fire. Whether theſe particles, that adhere to, or are mingled with the ſtony ones of the lime, may have any thing to do in the heat and tumult, that it produced upon the ſlaking of lime, this is not a fit place to examine. And though by this experiment, and thoſe made in ſealed retorts, which ſhew that what is afforded by fire may in a corporeal way invade, adhere, and add weight to even fixed and ponderous bodies, there is a large field opened for the ſpeculative to apply this diſcovery to divers phænomena of nature and chymiſtry; yet I ſhall leave this ſubject unmeddled with in this place.

A DIS-

A DISCOVERY OF THE PERVIOUSNESS OF GLASS TO PONDERABLE PARTS OF FLAME.

With ſome Reflections on it by way of COROLLARY.

THAT I might obviate ſome needleſs ſcruples, that may be entertained by ſuſpicious wits upon this circumſtance of our additional experiments, "That the glaſſes "employed about them were not expoſed to the action of mere flame, but were held "upon charcoals" (which to ſome may ſeem to contain but a groſſer kind of fire); and that alſo I might, by diverſifying the way of trial, render ſuch experiments both more fit to afford corollaries, and more ſerviceable to my other purpoſes, I attempted to make it ſucceed with a body ſo thin and diſengaged from groſs matter, as mere flame is allowed to be, knowing, that by going cautiouſly with it to work, one might handle a retort without breaking it, in ſpite of a violent agitation of kindled matter.

EXPERIMENT I.

SUPPOSING then that good common ſulphur, by reaſon of its great inflammability, and the vehemency and penetrancy of its flame, would be a very fit fuel for my purpoſe, I provided a ſmall double veſſel ſo contrived, that the one ſhould contain as many coals, as was neceſſary to keep the ſulphur melted, and that the other, which was much ſmaller,

smaller, and shaped like a pan, should contain the brimstone requisite for our trial; and lastly, that these two should be with a convenient lute so joined to one another, that all being closed at the top, save the orifice of the little pan (the fire and smoke of the coals having their vent another way) no fire should come at the retort to be employed, but the flame of the burning brimstone. Then two ounces of filings of tin being heedfully weighed out, and put into a glass retort provided for such trials, and made fit to be easily sealed up at the neck, when the time should be convenient, the sulphur (which ought to be of the purer sort) was kindled, and the glass by degrees exposed to it; where it continued, as the laborant informed me (the smell of brimstone, peculiarly offensive to me, forbidding me to be present) near two hours before the metal melted; after which, he kept the retort near an hour and half more, with the metal melted in it. Then bringing it me to look upon, I perceived a pretty deal of darkish calx at the bottom, and partly too upon the surface of the far greater part of the metal, which now lay in one lump. The part of the retort that had been sealed, being broken off, we first took out the calx, and then the lump, and putting them into the scales they had been formerly weighed in, found them to have made a very manifest acquist of weight, which, if both the laborant and I be not mistaken (for the paper which should inform us, is now missing) amounted to four grains and a half, gained by the recited operation. Afterwards, we being grown more expert in making such trials, the experiment was repeated with the same quantity of filings of the same metal: at the end of the operation (which in all lasted somewhat above three hours) having broken off the sealed neck of the retort, we found that a good proportion of dark-coloured calx had been produced. This being weighed with the uncalcined part of the metal, the two ounces we first put in, appeared to have acquired no less than eleven grains and a half (and somewhat better).

Such superstructures, both for number and weight, may possibly in time be built on this and the like experiments, that I shall venture to obviate, even such a scruple, as is like to be judged too sceptical. But I remember, that, considering upon occasion of some of the experiments, formerly recited, that though it were very improbable, yet it did not appear impossible, that the increment of weight, acquired by bodies exposed in glass-vessels to the fire, might proceed, not from the corpuscles of fire, but from the particles of the glass itself, loosened by the power of so intense a heat, and forcibly driven into the inclosed body; I was content to take a couple of glasses, whereof one was shaped into a little retort, and having weighed them, and then having kept them for a considerable time upon kindled coals, and then weighed them again, I could gather little of certainty from the experiment (the retort at one time seeming to have acquired above half a grain in the fire) save that there was no likelihood at all, that so considerable an encrease of weight, as we divers times obtained in close vessels, should proceed from the glass itself, and not from the fire.

EXPERIMENT II.

Because it seems evident enough, that whatever chymists tell us of the hypostatical sulphur, common brimstone is a body heterogeneous enough, having in it some parts of an oily or inflammable nature, and others acid, and very near of kin to the spirits of vitriol; I thought fit to vary our experiment, by making it with a liquor that is generally reputed to be as homogeneous as chymists themselves are wont to render any, I mean with a spirit of wine, or some such liquor as will totally flame away without affording

affording soot, or leaving any drop of phlegm behind it. In prosecution of this design, we carefully weighed out an ounce of filings of block-tin, and put them into a glass retort, fit for the purpose, whose neck was afterwards drawn out to a great slenderness; and we also provided a conveniently shaped metalline lamp, such as that the flame of this ardent spirit might commodiously burn in it, and yet not melt or crack it; which lamp, though furnished with a cotton wick, afforded no soot, because, as long as it was supplied with liquor enough, it remained unburnt. These things being in readiness, the retort was warily approached to the flame, and the metal was thereby in a short time melted. After which, the glass being kept exposed to the same flame for near two hours in all, the sealed apex of the retort was broken off, and there appeared to have been produced a not inconsiderable quantity of calx, that lay loose about the remaining part of the tin, which, upon its growing cold, was hardened into a lump. This, and the calx, being taken out of the retort with care, that no little fragment of glass should at all impose upon us, was weighed in the same scales as formerly, and found to have gained four grains and a half, besides the dust, that stuck in the inside of the retort, of which we reckoned enough to make about half a grain more; so that of so fine and pure a flame, as of this totally ardent spirit, enough to amount to five grains, was arrested, and in good measure fixed by its operation on the tin it had wrought upon.

EXPERIMENT III.

For confirmation of the former trial, wherein we had employed the *spiritus ardens* of sugar, we made the like experiment with highly rectified spirit of wine, only substituting an ounce of lead instead of one of tin. The event, in short, was this; that after the metal had been for two hours or better kept in the flame, the sealed neck of the retort being broken off, the external air rushed in with a noise (which shewed the vessel to have been very tight) and we found pretty store of the lead, for it was above seven scruples, turned into a greyish calx, which together with the rest of the metal being weighed again, there was very near, if not full six grains of increase of weight acquired by the operation.

1. N. B. The lump of lead, that remained after the newly recited operation, being separated from the calx, was weighed and cut in pieces, that it might be put into a fresh retort, wherein it was again exposed to the flame of spirit of wine, that I might satisfy myself, whether probably the whole body of the lead might not, by repeated operations, or (perhaps by one continued long enough) be reduced to calx. And though after the retort (whose neck had been drawn out) had been kept in the flame for about two hours, it was, by the negligence of a foot-boy, unluckily broken, and some of the calx lost; yet we made a shift to save about five grains of it (whose colour was yellowish; which was enough to make it likely, that if we had had conveniency to pursue the operation to the utmost, the whole metal might have been calcined by the action of the flaming spirit.

2. N. B. And lest you should be induced by some chymical conceit to imagine, that the particles that once belonged to flame, did make more than a coalition with those of the lead, and by a perfect union were really transmuted into the metal whose weight they encreased; I shall add, that (according to a method elsewhere delivered) I examined the seven scruples of calx, mentioned to have been made in the third experiment, by weighing them in air and water, and thereby found, as I expected, that though the absolute gravity of the metal had been encreased by the particles of flame, that stuck fast

faſt to it, yet this aggregate of lead and extinguiſhed flame had loſt much of its ſpecifick gravity. For whereas lead is wont to be, to water of the ſame bulk, as about eleven and an half to one; the ſubtil calx of lead was to water of the ſame bulk, little, if at all, more than as nine to one.

THESE are not the only experiments I made of the operation of mere flame upon bodies incloſed in glaſſes; but theſe, I ſuppoſe, are ſufficient to allow me to comply with my preſent haſte, and yet make good the title prefixed to this paper. For whence can this encreaſe of abſolute weight (for I ſpeak not of ſpecifick gravity) obſerved by us in the metals expoſed to the mere flame, be deduced, but from ſome ponderable parts of that flame? And how could thoſe parts invade thoſe of the metal incloſed in a glaſs, otherwiſe than by paſſing through the pores of that glaſs? But becauſe I judge it unphiloſophical, either to be more careful, that what one writes ſhould appear ſtrange, than be true; or to be forward to advance the repute of ſtrangeneſs to the prejudice of the intereſt of truth, though it be perhaps but a remote one, or a collateral one; I ſhall deal ſo impartially, as to ſubjoin on this occaſion two or three ſhort intimations, that may prove both ſeaſonable for caution, in reference to the porouſneſs of glaſs, and give a hint or two in relation to other things.

I DO not then, by the foregoing experiments, pretend to make out the poroſity of glaſs any farther, than is expreſſed in the title of this paper; namely, in reference to ſome of the ponderable parts of flame. For otherwiſe I am not at all of their mind, that think glaſs is eaſily penetrable, either, as many do, by chymical liquors; or, as ſome, by quickſilver; or, as others, at leaſt by our air; thoſe opinions not agreeing with the experiments I made purpoſely to examine them, as you may find in another paper.

AGAIN, if we compare the increaſe we obſerve to be made in the weight of the bodies, that we expoſe to the naked fire, and thoſe of the ſame or the like kinds, that we included in glaſſes, or ſo much as in crucibles; it may be worth conſidering, whether this difference in acquired weight may not give cauſe to ſuſpect, that the corpuſcles, whereof fire and flame conſiſts, are not all of the ſame ſize, and equally agitated, but that the interpoſed veſſel keeps out the groſſer particles like a kind of ſtrainer, though it gives paſſage to the minuteſt and moſt active.

I OFFER it alſo to conſideration, whether this perviouſneſs of glaſs, even to the minute particles that pervade it, and their adheſion to the metal they work on, does neceſſarily imply pores conſtantly great enough to tranſmit ſuch corpuſcles: or, whether it may not be ſaid that glaſs is generally of a cloſer texture, than when in our experiments the pores are opened by the vehement heat of the flame, that beats upon it, and in that ſtate may let paſs corpuſcles too big to permeate glaſs in its ordinary ſtate; and, that this penetration is much aſſiſted by the vehement agitation of the igneous parts, which, by the rapidneſs of their motion, both force themſelves a paſſage through the narrow pores of the glaſs, and pierce deep enough into thoſe of the included body, to ſtick faſt there (as hail-ſhot thrown with one's hand againſt a board will paſs off from it, but being ſhot out of a gun will pierce it, and lodge themſelves in it): and I know a menſtruum, that does not work upon a certain metal, whilſt the liquor is cold, or but faintly heated; and yet by intending the heat, would be made to turn it into a powder or calx (for it does not properly diſſolve it).

PERHAPS it may not be amiſs to add on this occaſion, that though glaſs be generally acknowledged to have far ſmaller pores, than any other matter wont to be implied to make veſſels, that are to be expoſed to the fire; yet, till I be farther ſatisfied, I ſhall forbear both to determine, whether the rectitude, that ſome philoſophers ſuppoſe in the pores of glaſs, as it is a tranſparent body, or rather in their ranks or rows, may facilitate the perviouſneſs we above obſerved in glaſs, and to conclude from the foregoing experiments, that ponderable parts of flame will be able, as well to paſs through the pores of metalline veſſels, as thoſe of glaſs. For though, with a ſilver veſſel, made merely of plate, without ſoder, I made two or three trials (of which you may command an account) in order to the reſolving of theſe doubts; yet by an accident, which, though it were not a ſurprizing one, was unlucky enough to defeat my endeavours, I was kept, for want of fit accommodations, from bringing my intended trials to an iſſue.

And now having endeavoured by the foregoing advertiſements, to prevent the having unſafe conſequences drawn from our experiments; it remains, that I briefly point at three or four corollaries, that may more warily be deduced from them. To which, if I get time, I may ſubjoin a hint or two about further enquiries.

COROLLARY I.

Confirming this paradox, that flame may act as a menſtruum, and make coalitions with the bodies it works on.

The experiments we have made and recited, of the permeating of flame (as to ſome of its parts) through glaſs veſſels, and of its working on included metals, may much confirm the paradox I have elſewhere propoſed, that flame may be a menſtruum, and work on ſome bodies at the rate of being ſo; I mean, not only by making a notable comminution and diſſipation of the parts, but by a coalition of its own particles with thoſe of the fretted body, and thereby permanently adding ſubſtance and weight to them. Nor is it repugnant to flame's being a menſtruum, that in our experiment the lead and tin, expoſed to it, were but reduced to powder, and not diſſolved in the form of a liquor, and kept in that ſtate. For, beſides that the interpoſed glaſs hindered the igneous particles from getting through in plenty enough; I conſider that it is not neceſſary, that all menſtruums ſhould be ſuch ſolvents, as the objection ſuppoſes. For whether it be (as I have ſometimes ſuſpected) that menſtruums, that we think ſimple, may be compounded of very differing parts, whereof one may precipitate what is diſſolved by the other; or for ſome other cauſe, I have not now time to diſcuſs. Certain it is, that ſome menſtruums corrode metals and other bodies, without keeping diſſolved all, or perhaps, any conſiderable part; as may be ſeen, if you put tin in a certain quantity of aqua fortis, which will in a very ſhort time reduce it almoſt totally to a very white ſubſtance, which, when dry, is a kind of calx. And ſo by a due proportion of oil of vitriol, abſtracted from quickſilver by a ſtrong fire, we have divers times reduced the main body of the mercury into a white powder, whereof but an inconſiderable part would be diſſoluble in water. And ſuch a white calx I have had by the action of another fretting liquor on a body not metalline.

And having thus cleared our paradox of the oppoſed difficulty, my haſte would immediately carry me on to the next corollary, were it not, that there is one phænomenon belonging to this place, that deſerves to be taken notice of. For whether it be, as ſeems probable, from the vehement agitation of the permeating particles of flame, that violently tear aſunder the metalline corpuſcles, or from the nature of the igneous menſtruum (which being, as it were, percolated through glaſs itſelf, muſt be ſtrangely minute) it is worth obſerving, how ſmall a proportion, in point of weight, of the addi-

tional

tional adhering body may ſerve to corrode a metal, in compariſon of the quantity of vulgar menſtruums, that is requiſite for that purpoſe. For whereas we are obliged to employ, to the making the ſolution of crude lead, ſeveral times its weight of ſpirit of vinegar, and (though not ſo many times) even of aqua fortis, it was obſerved in our experiment, that, though the lead was encreaſed but ſix grains in weight, yet above ſix ſcore of it were fretted into powder, ſo that the corroſive body appeared to be about the twentieth part of the corroded.

COROLLARY II.

Propoſing a paradox about calcination, and calces.

ANOTHER conſequence, deducible from our diſcovery of the perviouſneſs of glaſs to flame, may be this, that there is cauſe to queſtion the truth of what is generally taken for granted about calcination, and particularly of the notion, that not only others, but chymiſts themſelves, have entertained about the calces of metals and minerals. For whereas it is commonly ſuppoſed, that in calcination the greater part of the body is driven away, and only the earth, to which chymiſts add the fixed ſalt, remains behind; and whereas even mechanical philoſophers (for two or three of them have taken notice of calcination) are of opinion, that much is driven away by the violence of the fire, and the remaining parts, by being deprived of their more radical and fixed moiſture, are turned into dry and brittle particles: whereas theſe notions, I ſay, are entertained about calcination, it ſeems, that they are not well framed, and do not univerſally hold; ſince, at leaſt, they are not applicable to the metals our experiments were made on. For, it does not appear by our trials, that any proportion, worth regarding, of moiſt and fugitive parts, was expelled in the calcination; but it does appear very plainly, that by this operation the metals gained more weight than they loſt; ſo that the main body of the metal remained entire, and was far from being, either as a peripatetick would think, elementary earth, or a compound of earth and fixed ſalt, as chymiſts commonly ſuppoſe the calx of lead to be. From which very erroneous hypotheſis they are wont to infer the ſweet vitriol of lead, which they call *ſaccharum Saturni*, to be but the ſweet ſalt of it extracted only by the ſpirit of vinegar, which does indeed plentifully enough concur to compoſe it. Whence I conclude, that the calx of a metal even made as they ſpeak, *per ſe*, that is, by fire without additament, may be, at leaſt in ſome caſes not the *caput mortuum*, or *terra damnata*, but a magiſtery of it. For, in the ſenſe of the moſt intelligible of the chymical writers, that is properly a magiſtery, wherein the principles are not ſeparated, but the bulk of the body being preſerved, it acquires a new and convenient form by the addition of the menſtruum, or ſolvent, employed about the preparation. And, not here to borrow any argument from my notes about particular qualities, you may gueſs how true it is that the greateſt part of the body, or all the radical moiſture, is expelled in calcination, which therefore turns the metal into an arid, unfuſible powder; by this, that I have ſeveral times, from calx of lead, reduced corporal lead. And I remember, that having taken what I gueſſed to be but about a third, or fourth part of the calx of lead, produced by the third experiment, I found by a trial purpoſely deviſed, that without any flux-powder, or any additament, but merely by the application of the flame of highly rectified ſpirit of wine, there could, in a ſhort time, be obtained a conſiderable proportion of malleable lead; whereof the part I had the curioſity to examine, was true malleable lead; ſo little was the arid powder, whence this

this was reduced, deprived by the foregoing calcination of the supposed radical moisture requisite to a metal. The consideration of what may be drawn from this reduction, in reference to the doctrine of qualities, belongs not to this place.

COROLLARY III.

One use, among the rest, we may make, by way of corollary, of the foregoing discovery, which is in reference to a controversy warmly agitated among the corpuscular philosophers themselves. For some of them, that follow the Epicurean or atomical hypothesis, think that when bodies are exposed in close vessels to the fire, though the igneous corpuscles do not stay with the bodies they invade, yet they really get through the pores of the interposed vessels, and permeate the included bodies in their passage upwards; whereas others, especially favourers of the Cartesian doctrine, will not allow the atomists igneous corpuscles, which they take to be but vehemently agitated particles of terrestrial matter, to penetrate such minutes pores as those of glass; but do suppose the operation of the fire to be performed by the vehement agitation made of the small parts of the glass, and by them propagated to the included bodies, whose particles, by this violent commotion, are notably altered, and receive new textures, or other modifications.

But our experiments inform us, that though neither of the two opinions seems fit to be despised, yet neither seems to have hit the very mark: though the Epicurean hypothesis comprize somewhat more of the truth, than the other. For though it be not improbable that the brisk agitation, communicated by the small parts of the glass to those of the body contained in it, may contribute much to the effect of the fire; and though, by the small increment of weight we found in our exposed metal, it is very likely, that far the greater part of the flame was excluded by the close texture of the glass; yet, on the other side, it is plain that igneous particles were trajected through the glass, which agrees with the Epicureans; and they, on the other side, mistook in thinking that they did but pass through, and divide, and agitate the included bodies; to which, nevertheless, our experiments shew, that enough of them, to be manifestly ponderable, did permanently adhere.

Whether these igneous corpuscles do stick, after the like manner, to the parts of meat, dressed by the help of the fire, and especially roast-meat, which is more immediately exposed to the action of the fire, may be a question which I shall now leave undiscussed, because I think it difficult to be determined, though, otherwise, it seems worthy to be considered, in regard it may concern men's health to know, whether the coction of meat be made by the fire, only as it is a very hot body, or whether it permanently communicates any thing of its substance to the meat exposed to it: in which last case, it may be suspected, that not only the degree and manner of application of a fire, but the nature of its fuel, may be fit to be considered.

COROLLARY IV.

The experiments above recited give us this further information, that bodies very spirituous, fugitive, and minute, may, by being associated with congruous particles, though of quite another nature, so change their former qualities, as to be arrested, by a solid and

and ponderous body, to that degree, as not to be driven away from it by a fire intenſe enough to melt and calcine metals.

Exp. III. N. B. 2. For the foregoing trials (taking in what I lately delivered of the leſſened ſpecifick gravity of calcined lead) ſeems plainly enough to diſcover, that even the agitated parts of flame, minute enough to paſs through the pores of glaſs itſelf, were, as it were, entangled among the metalline particles of tin and lead, and thereby brought to be fixed enough to endure the heat, that kept thoſe metals in fuſion, and little by little reduced them into calces: which is a phænomenon, that one would not eaſily look for, eſpecially conſidering how ſimple a texture that of lead or tin may be ſuppoſed to be, in compariſon of the more elaborate ſtructures of very many other bodies. And this phænomenon, which ſhews us what light and fugitive particles of matter may permanently concur to the compoſition of bodies ponderous and fixed enough, may perchance afford uſeful hints to the ſpeculative; eſpecially if this ſtrict combination of ſpirituous and fugitive ſubſtance with ſuch, as being groſs or unwieldy, are leſs fit than organized matter, to entangle or detain them, be applied (as it may be with advantage) to thoſe aggregates of ſpirituous corpuſcles, and organical parts, that make up the bodies of plants and animals. And this hint may ſuggeſt a main inference to be drawn from the operations of the ſunbeams on appropriated ſubjects, ſuppoſing it to prove like that of flame on tin and lead.

And now having diſpatched our corollaries, we might here enquire, whether all the particles of fire and flame, that are ſubtil, and agitated enough to penetrate glaſs, and faſten themſelves to included bodies, be reduced by ignition to the ſame nature, or elſe retain ſomewhat of their proper qualities? which enquiry I have ſome cauſe not to think ſo undeterminable, as at firſt bluſh it may appear. For one of the ways that may be propoſed for this examen, is already intimated at the cloſe of the third experiment; which ſhews, that we may compare the ſpecifick gravity of the calces of the ſame metal, made in glaſſes by the operation of flames, whoſe fuels are of very differing natures. And I ſaid, one of the ways, becauſe it is not the only way I could name, and have partly tried. But though I might ſay more concerning expedients of this kind, and could perhaps propound other enquiries, that may reaſonably enough be grounded upon the hitherto recited phænomena (and thoſe of ſome other like trials) yet I muſt not unſeaſonably forget, that the purſuit of ſuch diſquiſitions would lead me much farther, than I have now the leiſure to follow it.

A LET-

A LETTER CONCERNING AMBERGRIS, AND ITS BEING A VEGETABLE PRODUCTION.

First publiſhed in the *Philoſophical Tranſactions*, N° 97, p. 6113, for *September* 13, 1673.

SIR,

SOME occaſions calling me this afternoon up to *London*, I met there with a very intelligent gentleman, who was ready to go out of it; but before he did ſo, he willingly ſpared me ſome time to diſcourſe with him about ſome of the affairs of our Eaſt-Indian Company, of which he was very lately deputy-governor; and, his year being expired, is ſtill one of the chief of the court of committees, which a foreigner would call directors, that manage all the affairs of that conſiderable ſociety. And, among other things, talking with him about ſome contents of a journal lately taken in a Dutch Eaſt-Indian prize, I learned from him, that he, who underſtands that language very well, is now peruſing that manuſcript, and, among many other things recorded there, that concern the œconomical and political affairs of the ſaid Dutch Company, he met with one phyſical obſervation which he thought ſo rare, that remembering the curioſity I had expreſſed for ſuch things, he put it into Engliſh, and tranſcribed it for me, and immediately drawing it out of his pocket, he preſented me the ſhort paper, whereof I now ſhew you the copy: upon peruſal of which, you will very eaſily believe, that not only his civility obliged me, but the information it brought me, ſurprized me too. For the ſeveral trials and obſervations of my own, about ambergris, have long kept me from acquieſcing either in the vulgar opinions, or thoſe of ſome learned men concerning it; yet I confeſs, my experiments did much leſs diſcover what it is, than this paper has done, in caſe we may ſafely and entirely give credit to its information, and that it reach to all kinds of ambergris. And, probably, you will be invited to look on this account, though not as complete, yet, as very ſincere, and, on that ſcore, credible, if you conſider, that this was not written by a philoſopher, to broach a paradox, or ſerve an hypotheſis, but by a merchant, or factor, for his ſuperiors, to give them an account of a matter of fact; and that this paſſage is extant in an authentic journal, wherein the affairs of the company were, by publick order, from time to time, regiſtered, at their chief colony, *Batavia*. And it appears by the paper itſelf, that the relation was not looked upon as a doubtful thing, but as a thing, from which a practical way may be deduced to make this diſcovery eaſily lucriferous to the Dutch Company. And I could heartily wiſh, that in thoſe countries that are addicted to long navigations, more notice, than is uſual, were taken and given of the natural rarities that occur to merchants and ſeamen. On which occaſion, I remember, when I had, in compliance with my curioſity, put myſelf into our Eaſt-Indian Company, and had, by their civility to me, been choſen of their committee, as long as my health allowed me to continue ſo, I had the opportunity in ſome regiſter books of merchants, Engliſh and Dutch, to obſerve ſome things, which would eaſily juſtify this wiſh of mine, if my haſte, and their intereſt, would permit me to acquaint others with them. But to return to our account of ambergris, I think you will eaſily believe, that if I had received it not by a paper, but immediately from the writer, I ſhould, by propoſing divers queſtions, have been enabled to give you a much more ſatisfactory account, than this ſhort one contains. But the obliging perſon that gave it me, being juſt going out of town, I could not civilly ſtay him to receive my queries about it; which though (God permitting) I may propoſe, ere long, if I can light on him again, yet I fear he has given me in theſe few lines, all that he found about this matter. However, this relation, as ſhort as it is, being about the nature of a drug ſo precious, and ſo little known, will not, I hope, be unwelcome to the curious; to whom none is ſo like to convey it ſo ſoon, and ſo well, as M. *O.*; whoſe forwardneſs to oblige others by his various communications, challenges returns of the like nature from others, and particularly from his affectionate humble ſervant.

Follows the Extract itſelf out of a Dutch journal, belonging to the Dutch Eaſt-Indian Company.

"AMBERGRIS is not the ſcum, or excrement of the whale, &c. but iſſues out "of the root of a tree, which tree, how far ſoever it ſtands on the land, always "ſhoots forth its roots towards the ſea, ſeeking the warmth of it, thereby to deliver "the fatteſt gum that comes out of it: which tree, otherwiſe, by its copious fatneſs, "might be burned and deſtroyed. Wherever that fat gum is ſhot into the ſea, it is ſo "tough, that it is not eaſily broken from the root, unleſs its own weight, and the "working of the warm ſea doth it, and ſo it floats on the ſea.

"THERE was found by a ſoldier, $\frac{7}{8}$ of a pound, and by the chief, two pieces weighing five pounds. If you plant the trees where the ſtream ſets to the ſhore, then the "ſtream will caſt it up to great advantage. *March* 1, 1672, in *Batavia:* journal ad"vice from——"

TRACTS,

T R A C T S,

CONSISTING OF

OBSERVATIONS about the SALTNESS of the SEA.
An Account of a STATICAL HYGROSCOPE, and its USES.

TOGETHER WITH

An APPENDIX about the FORCE of the AIR's MOISTURE.
A FRAGMENT about the NATURAL and PRETERNATURAL STATE of BODIES.

TO ALL WHICH IS PREMISED

A SCEPTICAL DIALOGUE about the POSITIVE or PRIVATIVE NATURE of COLD.

WITH

Some EXPERIMENTS of Mr. *BOYLE*'s referred to in that Diſcourſe.

OF THE POSITIVE OR PRIVATIVE NATURE OF C O L D.

A SCEPTICAL DIALOGUE between CARNEADES, THEMISTIUS, ELEUTHERIUS, PHILOPONUS.

SECTION I.

Eleuth. MAY one be allowed to aſk *Carneades*, what book it is he is reading with ſo much attention?

Carn. THE queſtion, *Eleutherius*, is very allowable, and as eaſily anſwered, by ſaying, that what I was reading, is our friend Mr. *Boyle*'s newly publiſhed Hiſtory of cold.

Them. YOUR readineſs, *Carneades*, to anſwer, encourages me alſo to aſk you a queſtion; which ſhall not be, as probably you expect it ſhould, how you like this new piece? for

for I know you would be too kind to the author, not to tell me, that he has detected some old errors, and made discovery of some new truths: but my question shall be about what is my wonder, as well as that of divers others, who think it strange, that a writer that has delivered so many effects and other phænomena of cold, should omit to tell us so much, as whether he asserts it to be a positive quality, or a bare privation of heat; as, since *Cardan* (in his treatise *De Subtilitate)* some other learned men, and especially *Cartesius*, have maintained.

Carn. You will not wonder if a person that you look upon, and I confess not injuriously, as a friend to Mr. *Boyle*, tell you, that this author, by the many histories he has presented us, and by his not seeming to dare to determine the controversy you have mentioned, shews, that he was more solicitous to lessen his ignorance, than to pretend to knowledge: and upon the observation I have made of his humour in general, I presume one principal reason of his silence may be, that he has not yet completed the trials he had designed about cold; and thinks that in abstruse subjects, such as this is, it is not so convenient to deliver a positive opinion of the nature of it at the beginning, as to reserve it for the latter end, after the history of the phænomena; when the nature of the thing enquired into may, as it were, spontaneously result from the considerations suggested by the precedent matters of fact surveyed together.

Eleuth. If such a wariness were indeed the motive of your friend's silence, I shall easily excuse it; and perhaps think too, that the like would not mis-become naturalists on many other occasions. And yet I do not dislike *Themistius*'s question; for it is one thing to venture upon declaring the adequate nature of cold, and another to determine, whether it be a positive, or a privative quality? the latter attempt importing a much less venture than the former.

Carn. I will not pretend to know the very reasons that induced the author silently to pass by this controversy; but having been once present, when he had occasion to discourse of it, I then conjectured, that among his experiments of cold, that are not yet published, there may be some uncommon ones, that may have suggested to him scruples, which obliged him to forbear declaring himself, till he had cleared them, which those that are unacquainted with such trials, may probably have never thought of.

Them. If what you call a controversy, were indeed worthy of that name, I should not unwillingly allow of your friend's silence; but the opinion broached by *Cardan*, and adopted by Mr. *Des Cartes* and others, seems to me so devoid not only of reason, but of all appearance of it, that methinks one that has delivered such considerable effects of cold, as Mr. *Boyle* has done, may well ascribe to their cause, at least a positive nature; and without at all being guilty of boldness, reject an opinion, that is not only barely an error, but an extravagance, and perhaps a plain absurdity.

Carn. Possibly the gentleman we are speaking of, may be wary and sceptical enough to reckon among difficult things, not only the declaring the adequate nature of cold, and the manner of its operations; but the demonstrating whether it be a positive quality or not. And though I will not take upon me to know his thoughts about that subject, which, perhaps, are grounded upon some of his peculiar experiments and notions; yet, for discourse sake, I am content to debate with *Themistius*, Whether or no the opinion, he so severely censures, be not only erroneous, as, for aught appears, Mr. *Boyle* himself may be found to have thought it; but also, as *Themistius* would have it, absurd.

Them I readily accept of your offer; for it cannot be an unpleasant entertainment to observe the arts, whereby one, that I know will not speak impertinently, will endeavour to make reason elude the clearest testimonies of sense. And though I might press you

you with the concurrent authority of *Aristotle*, and all the philosophers that have lived between his time and those of that extravagant fellow *Cardan*; yet I shall rather employ, to convince you, the authority and reasons of a grand leader among your new philosophers, who being a great broacher of paradoxes, and having upon that score written books expresly against *Aristotle*, was not like to have sided with him, unless the evidence of truth had, as it were, necessitated him to do so.

Carn. I PRESUME, you mean the learned and subtle *Gassendus*, whom I am glad you have pitched upon for your cause's champion, not only because, in defending the common opinion, he waves the common practice of troubling his readers with a multitude of authorities, which to me, in such a case as this, would signify very little, and betakes himself to arguments; but because, being so modern and judicious a writer, we may well suppose him to have summed up and improved what can be said in behalf of the cause he maintains. Upon which account, I shall be excused from answering impertinent objections against the opinion I defend, and from the trouble of ranging about, among other authors, for more weighty arguments than those, which the disproving of his will shew to be unsatisfactory.

Them. I AM glad you named the author, I meant *Carneades*, for I apprehended you had not met with what he says upon this subject; because I could scarce imagine that an intelligent person, after having read his arguments, will doubt of a truth he hath so clearly evinced by them. But since I perceive you have seen what he has written, I shall, without farther preamble, propose his reasons to you, though not in the very same order wherein he has couched them.

Eleuth. BUT before you begin them, give me leave to ask *Carneades* a short question, whose answer will, I suppose, conduce, if not be necessary, to the clearing of the state of the controversy betwixt you. For it is one thing to deny belief to the received opinion, that cold is a positive quality, and another thing to assert, that it is but a privation of heat; since, if *Carneades* does undertake the latter of these two, he must bring positive arguments to prove cold to be but a negative thing. Whereas, if he content himself to play a doubting part, it may suffice him, being in effect but a defendant, to shew, that the proofs brought to conclude cold to be a positive quality are not cogent.

Carn. I ACKNOWLEDGE your question, *Eleutherius*, to be pertinent, and not unseasonable. And, I presume, you will not be surprized, that a person accused of scepticism answers it by declaring that he undertakes not to demonstrate, that cold must be a privative or negative quality, and thinks it sufficient for his turn, to shew that the arguments brought to evince it to be a positive one, are not concluding. And, since you have already diverted *Themistius* from beginning so soon as he intended, it will not be amiss, that I continue that suspension a little longer, to prevent, what I know we both hate, verbal controversies; which yet may very easily spring from undetermined acceptions of words, as ambiguous as I have observed heat (of which I now make cold but a privation) to be.

WE may therefore consider, that the word heat being made use of to signify, as well the operations of that quality upon other bodies (as when the heat of the fire makes water boil, or that of the sun melts wax, and hardens clay) as its operations upon the sense of man (as when a moderate degree of heat is said to cause pleasure, and an excessive one to produce pain); this term, I say, as Mr. *Boyle* also has somewhere noted, may be employed sometimes in a more absolute and indefinite sense, and sometimes in a more confined and respective sense; in the latter of which, it is estimated by its relation to the organs of feeling of those men that judge of it. Upon which account, men are wont to esteem no body hot, but such an one, the agitation of whose small parts is brisk enough

enough to encrease or surpass that of the particles of the organ that touches it; for if that motion be more languid in the object, than in the sentient, the body is reputed cold; as may appear by this, that if the same person put one of his hands, when it is hot, and the other when it is cold, into lukewarm water, that liquor will feel cold to the warm hand, and warm to the cold.

Eleuth. So that, according to this doctrine, methinks, one may, for brevity sake, conveniently enough apply to your two fold notion of heat, those expressions which some schoolmen employ about certain qualities, of any of which they say, that it may be either materially or formally considered. And by analogy to their doctrine, since heat is a tactile quality, and, as such, imports primarily a relation to the organ of touching, that relation, with what depends upon it, may pass for that, which is the *formale*, in the quality, called heat; and its effects and operations upon other bodies, may supply us with a notion of heat, materially taken.

Carn. I do not always quarrel, *Eleutherius*, with terms borrowed from the schools, if they be as much more short and expressive than others, as they are more unusual, or even barbarous. But there is another distinction of heat, partly grounded upon that already proposed, which, because it may be of use in our future discourse, will not be unfit to be here intimated. For we may consider, that though, for the most part, a hot body is taken in the vulgar sense, for that, wherein the degree of heat is sensible to our organs of feeling; yet, in a looser sense, and which, for distinction sake, we may call philosophical, because concluded by reason, though not perceived by sense, a body may be conceived not to be destitute of heat, even when the degree of that quality is not great enough to be felt by the touch; provided it can produce, in some degree, those other operations, which, when more intense, are acknowledged to proceed from manifest heat. For elucidation of which we may alledge, that in very frosty, and yet clear weather, the sun may be judged to warm the air, when it melts snow, and thaws ice; though, perhaps, many men, especially of tender constitutions, feel in their fingers and toes much stiffness, and more pain, upon the account of cold. To this I may add the common observation, if you grant the truth of it, that snow melts much sooner upon land newly turned up by the plow, than, *cæteris paribus*, in the neighbouring ground; which argues a warmth in that newly exposed earth; though, according to the touch, it would questionless appear cold. But we may be furnished with a clearer, and more pregnant instance, by but recalling to mind what was just now mentioned of the warmth of tepid water, which was not to be felt by a hot hand, but produced there a contrary sensation of cold. Which instance I therefore scruple not to repeat, because it affords an experiment in favour of that premised distinction, which, I think, may also have this ground in reason, that a considerable heat is often requisite to be sensible to our hands, &c. which are continually irrigated with the circulating blood that comes very warm out of the heart, and enlivened by animal spirits plentifully supplied from the brain.

If *Eleutherius* thinks fit to accommodate this distinction in the vulgar, and in the philosophical sense to his heat, formally and materially taken, I leave him to his liberty. And I shall also leave it to you both, Gentlemen, to accommodate to cold, *mutatis mutandis*, as they speak, what has been said about the distinctions of heat; because, I fear, *Themistius* thinks himself to have been too long detained already from proposing his arguments, which he may now begin to do as soon as he pleases.

SEC-

SECTION II.

Them. I WILL then, with your permission, begin with that argument of *Gassendus*, which I am able to give you in his own words; because, upon the occasion of Mr. *Boyle*'s book, I made a transcript of what he says, to evince the positive nature of cold; and and having the transcript yet about me, it is easy for me to tell you, that it is this *: *Ii sunt frigoris effectus, quales habere privatio, quæ actionis est incapax, non potest.*

THIS argument, though he begins not with it, I choose to make the first, because I think it of such weight, that, though it were the only one he could alledge, it would serve his turn and mine, since it is drawn from the effects of cold, which, though he mentions them but in few and general words, experience shews to be both so manifold and so considerable, that if *Carneades* employ an hundred times as much time to answer the argument they afford, as I have done to recite it, he will, I think, do no more than would be necessary, and perhaps not enough to be sufficient. For cold affects the organs of feeling, and sometimes causes great pain in them, condenses air and water, and breaks bottles that are too well stopped, congregates both homogeneous and heterogeneous things, increases hunger, checks fermentation in liquors, produces heat by antiperistasis, in deep cellars, mines, &c. and yet freezes men and beasts to death, dismantles whole woods and forests of their leaves, and does (I know not how many) other feats; among which it is not the least admirable, though one of the most common, that it turns the fluid and yielding waters of rivers and lakes, and sometimes of part of the sea itself, not too far from the shore, into firm and solid ice, which is often in northern climates strong enough, not only to be travelled upon by merchants with their carriages, but to be fought upon by whole armies with their trains of artillery. From which, and other instances, it is manifest, that effects so numerous and great cannot proceed from a mere privation, or any negative thing, but require a considerable, and therefore sure a positive quality to produce them.

Carn. THIS objection, *Themistius*, is, I confess, a considerable one, and of more weight than any of the rest, if not than all of them put together; but, as I think it very worthy to be answered, so I think it very possible to be well answered; and to give you my reasons for my so thinking, I shall distinctly consider in the argument the two particulars which it seems to consist of.

AND first we are told, that if cold be but a privation, it cannot be the object of sense. To clear this difficulty, which, I know, you will think it very hard, if at all possible to do, I must beg your leave to observe something about sensation in general; not as designing an entire and solemn discourse of that subject, but because the particular remark I am about to make is necessary to the solution of our present difficulty. I observe then, that that, which, at least in such cases as we are speaking of, produces in the mind those perceptions which we call sensations of outward objects, is the local motion caused by means of their action upon the outward organs in some internal part of the brain, to which the nerves belonging to those organs correspond; and the diversity of sensations may be referred to the differing modifications of those internal motions of the brain, either according to their greater or lesser celerity, or other circumstances, as our friend Mr. *Boyle* has somewhere exemplified in the variety of sounds; whereof some are grave, some sharp, some harmonious and pleasant, some jarring and offensive; and yet all this

* Vid. Gassend. Physicam. Sect. I. Lib. VI. cap. 6.

ſtrange variety proceeds from the variations of thoſe ſtrokes or impulſes which the air, put into motion by ſonorous bodies, gives to the ear.

To this it will be conſonant, that as the air, or rather the mind by the intervention of the air, is differingly affected by a very grave ſound, and a very acute one; though the former proceed from the want of that celerity of motion in the undulating air, which is to be found in the latter; which ſlowneſs, or imminution of motion, does, as ſuch, participate of, or approach to the nature of reſt; ſo in the ſenſory of feeling there may, upon the contact of a cold body, be produced a very differing perception from that, which is cauſed by the contact of a hot body; and this, though the thing perceived, and by us called coldneſs, conſiſts but in a leſſer agitation of the parts of the cold body, than of thoſe of the hot body, in reſpect of our hands or other organs of feeling.

And this leads me, for the farther clearing of this matter, to repreſent to you, that ſince it is manifeſt, that bodies in motion are wont to communicate of their motion to thoſe more ſlow bodies they happen to act upon, and to loſe of their own motion by this communicating of it; ſince this, I ſay, is ſo, if, for inſtance, a man take a piece of ice in his hand, the agitation of the particles of the ſenſory will, in good part, be communicated to the corpuſcles of the ice, which, upon that account, will quickly begin to thaw; and the contiguous parts of the hand loſing of the motion they thus part with to the ice, there needs nothing elſe to leſſen the agitation they had before. And there needs no more than this ſlackening or decrement of agitation, to occaſion in the mind ſuch a new and differing perception, as men have tacitly agreed to refer to coldneſs.

Eleuth. It ſeems by this diſcourſe, *Carneades*, that you think that ſenſation is properly and ultimately made in, or by the mind, or diſcerning faculty; which, from the differing motions of the internal parts of the brain, is excited and determined to differing perceptions; to ſome of which men have given the names of heat, cold, or other qualities. So that, according to you, if a conſiderable change or variation be made in the moſt ordinary, or in the former motion or modification of motion of the parts of a ſenſory, and conſequently of the parts that anſwer them in the brain, new ſenſations will be produced, whatever the cauſe of this alteration be, whether privative or poſitive.

Carn. You do not miſapprehend my thoughts, *Eleutherius*, and what you ſay gives me a riſe to illuſtrate this matter yet a little farther, by obſerving, that the ſenſories may be ſo accuſtomed to be affected after a certain manner by thoſe external objects, whoſe operation on them is very familiar, or perhaps almoſt conſtant, that the privation, or the bare imminution of the wonted operation leaves the parts of the ſenſory, for want of it, in a different diſpoſition from what they formerly were in; which change in the ſenſory, if it be not too ſmall, will be attended by a perception of it in the mind. To declare and confirm this by an example, we may conſider, that though darkneſs be confeſſedly a privation of light, and the degrees of it gradual imminutions of light; yet the eye, that is, the perceptive faculty, by the intervention of the eye, may well enough be ſaid to perceive both light and darkneſs, that is, both a poſitive thing and the privation of it. And it is obvious, that the motion of a ſhadow, which is a gradual privation of light, is plainly, and without difficulty, diſcoverable by the eye; of which the reaſon may be eaſily deduced from what I have been lately ſaying. And to ſhew you, that there is on theſe occaſions ſuch a change made in the organs of ſeeing, as is viſible even to by-ſtanders, I ſhall need but to appeal to the experiment of making in the day-time a boy or girl look towards an enlightened window, and then towards an obſcure part of the room; for when the latter comes to be done, you will plainly perceive, that for want of ſuch a degree of light as was wont to come in at the pupil, and ſtraighten a little

little that perforation of the uvea; that round circular hole, or, as you know they call it, apple of the eye, will grow very manifeftly larger than it was before, and then it will appear again, if the eye be expofed to a lefs fhaded light.

THIS obfervation may be feconded by what happens to a man, when coming out of the fun fhine, where the fun-beams much contract his pupil to fhut out an exceffive light that would be offenfive to the organ, he comes prefently into a dark room, where he muft continue fome time before he can fee others, as well as he is feen by them, whofe pupils have had time to be fo enlarged, as in that darker place to let in light enough to make objects vifible to their eyes, which are not fo to his, whofe pupils are yet contracted by the light they were but juft before expofed to. To this I might add divers other phænomena, explicable upon the fame grounds; but I fhall rather choofe to relate to you an uncommon accident, which happening to eyes fomewhat unufually difpofed, does more remarkably difcover what alteration darknefs, or a privation of light, may have upon thofe organs. I know a very learned man, who is no lefs ftudious of mathematicks, and other real parts of knowledge, than fkilled in thofe, which are taught of the fchools; this virtuofo, who feemed to me to have fomething peculiar in his eyes, confeffed, and complained to me, that if he come, though but out of a moderate light of the open air, into a room that is any thing dark, he does not only feel fuch an alteration, as other men are wont to do on the like occafion; but is fo powerfully affected by it, that he thinks he fees flafhes of fire before his eyes, and feels a troublefome difcompofure in thofe parts, that fometimes lafts an hour or two together, if he fo long continue there.

Eleuth. I KNOW not, *Carneades*, whether after this you will think it any great confirmation of your opinion, that *Ariftotle* has fomewhere this faying, that, *Oculus cognofcit lucem & tenebras.*

Carn. I THANK you, *Eleutherius*, for fo pertinent an allegation, though not for my own fake, yet for theirs that will more eafily receive a truth upon the teftimony of *Ariftotle*, than that of nature. And now, I hope, that *Themiftius* will confent, that, difmiffing the argument hitherto examined, we proceed to the next.

SECTION III.

Them. SINCE you will have it fo, I fhall comply at prefent, and the rather, becaufe, not only I forefee there will be occafion to fpeak of it again, but becaufe you experimental philofophers that are wont fo much to cry up the informations you think you receive from fenfe, fometimes, in fpite of contrary dictates of reafon, will, I hope, be prevailed with by the argument I am about to propofe, which is fo manifeftly grounded upon fenfe, that without denying that we do feel what we feel, we cannot deny cold to be a pofitive quality. For thus *Gaffendus* moft convincingly argues; *Cùm per hyemem immittimus manum in labentis fluminis aquam, quod frigus in ea fentitur, non poteft dici mera privatio, aliùdque prorfus effe apparet fentiri aquam frigidam, & fentiri non calidam. Et fi eandem aquam gelari, fentietur haud dubiè frigidior: an dices hoc effe nihil aliud quàm minùs calidam fentiri? Atqui calida jam antea non erat: quomodo ergo potuit minùs calida effici?*

Carn. I WILL not fay, *Themiftius*, his argument is not fpecious, but you, perhaps, or at leaft *Eleutherius*, will not affirm it to be more than fpecious, if you pleafe to confider, with me, two or three things that I have to fuggeft about it.

And first, to shew *Themistius*, that, whatever he was just now intimating, experimental philosophers do not prefer the immediate impressions made on the senses to the dictates of reason, though they think the testimony of the senses, however sometimes fallacious, much more informing than the dictates of *Aristotle*, which are oftentimes, and that groundlesly, repugnant to them; I will represent to you, that the organs of sense, considered precisely as such, do only receive impressions from outward objects, but not perceive what is the cause and manner of these impressions, the perception, properly so called, of causes belonging to a superior faculty, whose property it is to judge, whence the alterations made in the sensories do proceed, as may easily be proved, if I had time and need to do so, by many instances, wherein the senses do, to speak in the usual phrase, mis-inform, and, as far as in them lies, delude us, and therefore must be rectified by reason. As when the eye represents a straight stick, that has part of it under water, as if it were crooked; and two fingers, laid cross over one another, represent us a single bullet, or a button, rolled between them, as if there were a couple; so that it is very possible (for I forbear saying it is true, having not yet proved it) that though the sensory be very manifestly, and vehemently, affected upon the contact of cold water, or other cold bodies, yet the cause of that impression, or affection, is, and may be judged and determined by reason to be, other than that which the sense may to an inconsiderate person suggest. As when a child, or one that never heard of the thing before, first sees a stick, whereof one part is in the air, and the other under water, he will presently, but erroneously, conclude that phænomenon to be caused by the sticks being crooked or broken.

Next we may consider, that sensations may, in divers cases be made, as well from alterations that may happen in the internal parts of the body, as from those that are manifestly produced in the external organ by external objects and agents; as may appear by hunger, thirst, the titillation of some parts of the body, barely upon venereal thoughts, and (which belongs directly to our present argument) the great coldness that we have known hysterical women complain of in their heads and backs, and the great and troublesome degree of cold, which we every day observe, upon the first invasion of the fits of agues, especially quartans; which troublesome symptoms, that sometimes last for several hours, are therefore commonly called the cold fits.

And now it would be seasonable for me to call upon you to remember (and add to what I have now said) that, which, at the beginning of our conference, I took notice to you of, about sensation in general; if I did not presume that those things are yet fresh enough in your memory, to allow me to proceed directly to answer the objection, which I shall do, though not like a school-man, yet like a naturalist, by giving an account of the proposed phænomenon, without having recourse to that hypothesis, which it is urged to evince.

I observe, then, that though in the respective sense above-mentioned, water, wherein the objection supposes the hand to be plunged, be cold, in regard its parts are less agitated, than the spirits and blood harboured in the hand; yet, in a philosophical sense, it is not quite destitute of heat, since it is yet water, not ice, and would not be a liquor, but by reason of that various agitation of its minute parts, wherein fluidity, a quality essential to liquors, consists. Upon the score of this respective coldness of the water, the hand is refrigerated; for the spirits and juices of that organ meeting in the water, with particles much less agitated than they are, communicate to them some part of their own agitation, and thereby lose it themselves, upon which decrement of wonted agitation such a change is made in the sensory, and, though not so manifestly in some other

parts

parts of the body, as is perceived by the animadversive faculty under the notion of coldness; sensation (whatever obscure definitions are wont to be given of it) being indeed an internal perception of the changes that happen in the sensories.

And if now, as the objection supposes, the water wherein the hand is plunged comes to be more refrigerated than before, the spirits, blood, and other parts of the hand, finding the aqueous corpuscles more slowly moved than formerly, must, according to the laws of motion (according to which, a body, that meets another much more slowly moved than itself, communicates to it more of its motion, than if it were less slowly moved) transfer to them a greater measure of their own motion, and consequently themselves come to be deprived of it; and upon this increase of the slowness of motion in the parts of the hand there follows a new and proportionable perception of the mind, and so a more vehement sensation of cold. But though it be not to be admired, that the bare slowness of motion in the object should be discernible by sense, albeit it seems to participate of rest, which, with you, passes for a privation, since the ear perceives, when a voice grows faint, and when a sharp sound degenerates into a flat one; and we can perceive by the hand (abstracting from heat and cold) the celerity or slowness of bodies, that in their passage strike upon it, as for instance, of winds, or streams; yet this is not the only thing I think fit to be taken notice of on this occasion. For I consider farther, that besides the most consistent and stable parts of the hand, there are, from the heart and the brain, fresh blood and spirits continually transmitted to the hand; and the former of these, the blood, is, according to the laws of its circulation, and after it has received a great change in the much refrigerated hand, carried back through other parts to the heart; whence it is, in the same circulation, distributed to the whole body. To which may be added, that when the great refrigeration of the hand happens, external agents may contribute to the effects of it, as I shall by and by have occasion to shew.

If then you please to remember, that upon the turning one's eye to the dark part of a room less enlighted than the window, though darkness be but a privation, and though the obscurity of that part be not absolute, but consist only in a less degree of light; yet the action of the spirits, and other parts of the body, is so changed, upon occasion of the light's acting more faintly than was usual upon the organ, that the pupil is immediately and manifestly dilated, and in some cases, as in that which I mentioned to you of a learned man, much considerabler effects ensue; you will not wonder, that, where not only the spirits, but the blood (whence those spirits are generated) that circulates through the whole body, and upon whose disposition all the other parts so much depend, is very much disaffected, there should be felt a great alteration in the hand, which is the most immediately exposed to the action of the cold water. And for the reasons newly given, it ought to be as little strange, that in other parts of the body, the disordered, and not circulating blood, should have its wonted action on them considerably altered; since the more stable parts, and especially those external ones, that are most exposed to the cold, have their pores straightened, and consequently their texture somewhat altered; on the same occasion on which the wonted agitation of the spirits, with the particles that compose the blood, is notably lessened. And that such causes may produce great effects in a human body, you will be more prone to admit, if you consider the disorders that happen in the cold fit of an ague, and oftentimes, upon the shutting up of those excrementitious steams that are wont to be discharged by insensible transpiration; to whose being stopped in the body, by the constriction of the pores, which chiefly happens through cold, some learned physicians, especially the famous *Sennertus*, impute the cause of most fevers, as indeed experience itself does but too frequently shew it to be guilty of many.

Phil.

Phil. I CONFESS, *Carneades*, you have said some things, that I thought not on before; but yet *Gassendus*'s argument seems to be such, that I fear it will be hard to hinder many from saying, that if cold be but a privation of heat, it is a privation of a strange nature; for it may be introduced into bodies that were not hot before, nay, in some cases, into such as are naturally cold, and also by consequence must have been put into a preternatural state to be at any time hot.

Carn THIS objection, *Philoponus*, being in effect so much the same with that of *Gassendus*, that it differs from it but in the dress you give it, it will scarce require a peculiar and distinct answer; and therefore, as soon as I have reminded you of the distinction that we have formerly made of the vulgar and philosophical sense of the word cold, I shall need to alter but a little what I said before, by telling you, that since fluidity consists in the various agitation of the insensible corpuscles of a liquor, and that heat consists in a tumultuary, but a more vehement agitation of the insensible parts of a body, and so, that hot water scarce differs otherwise than gradually from that, which is cold to sense; if cold be taken in the larger and philosophical sense, it may well be said, that as long as water retains the form of water, and so continues to be a fluid body, though it may be very cold to the touch, yet it is not absolutely or perfectly cold, and therefore is capable of a farther degree of coldness, which it receives when brought to congelation; for till then it was not destitute of those agile corpuscles that were requisite to keep it fluid; and till then, *Gassendus* himself must acknowledge that it was not absolutely or perfectly cold; because he, as you may remember, did in his former (but lately-mentioned) argument ascribe the glaciation of water to the invasion of those that he calls corpuscles of cold.

Eleuth. GIVE me leave to add, *Carneades*, that it is not every glaciation itself that brings liquors to be perfectly cold in the philosophical sense of that expression, and quite expels or subdues all the agile particles that were in the water before it was turned into ice. For I think, that to effect this change, it is sufficient that so many of these restless particles be destroyed or disabled, that there remains not enough of them to keep the water in a state of fluidity, so that the surplusage may yet continue in the frozen liquor, and whilst they are there, perform several things, as the making it evaporable in the air, and even odorous, and by their recess or destruction the ice may grow yet more cold. And as this notion suits very well with the differing degrees of hardness, that we find in differing portions of ice, sometimes upon the account of the matter (as frozen water is harder than frozen oil) and sometimes upon that of the different degrees of cold in the same water, or other matter (as our friend somewhere observes) so it may be highly confirmed by an experiment I saw him make, but that is not yet published.

THE sum of the experiment was this; that he first put an hermetically sealed thermoscope into a glass, broader at the top than at the bottom, and greased the inside with tallow, that ice might not strongly stick to it. In this glass was put water, more than enough to cover the ball of the instrument; and that water being warily frozen, notice was taken, whereabouts the tincted spirit of wine rested in the stem; after which, the instrument and the ice being removed into the open air, upon an exceeding frosty morning, the ice was taken off from the ball, and presently after, the tincted liquor, as the maker of the trial expected, subsided a pretty way (the length of the instrument considered) below the former mark; which argued that he rightly guessed that such a degree of cold as is sufficient to turn water into ice, may not produce a body perfectly cold; this ice itself keeping the inclosed ball, in a sense, warm, by fencing off the air, which, at that time (even in our temperate clime) by the effect, appeared to be colder than the very ice. And, methinks, it may strengthen *Carneades*'s discourse, to represent, that there

there is no sufficient cause, why many things that are reckoned among privations or negations, by the Peripateticks themselves, as well as cold is by *Carneades*, may not admit of degrees; as may be exemplified by deafness, ignorance, and divers other things. And to bring a case, not very unlike that under consideration, we may take notice of a total eclipse of the moon, which you know always happens when she is at the full. For darkness in the air being acknowledged to be a privation or negation of light, when the earth, interposed between the moon and the sun, has eclipsed her, for instance, nine digits (as astronomers speak) men generally complain of darkness in the air, though there remain a considerable part of the discus, or the hemisphere of the moon, obverted to us, yet enlightened by the sun; but when the interposed earth proceeds to cover the remaining three digits, and so makes the eclipse total, the darkness also is said and esteemed to be much encreased; nor would men otherwise be persuaded, though *Themistius* should tell them that the air cannot have grown darker, though it were dark before; and indeed though the air was more and more darkened in proportion to the increase of the eclipse, yet it was never completely darkened till it became total. But I fear I dwell too long upon one argument.

SECTION IV.

Eleuth. LET me therefore, *Carneades*, sum up what I take to be your doctrine, and tell these gentlemen, that I think you do not look upon the sensation of cold as a thing effected by an entire privation, properly so called, and considered as such; but that, according to you, that slowness of motion in the particles of cold water, which the hand finds when it is thrust into that liquor, does occasion the spirits, and the corpuscles of the blood, to part with to those of the water a considerable share of their own surplusage of agitation, whereby they lose it themselves; upon which is consequent a perception of this change made in the hand, which, if it be very great, is also frequently accompanied with some sensible change in other parts of the body, occasioned chiefly by the frequent returns of the circulating and highly refrigerated blood to the heart, whence it is dispersed to the whole body. According to which doctrine, the sensation of cold is but a perception of the lessened agitation of the parts of the hand, either stable or fluid, especially of the blood; which alterations are in great part produced, not by the coldness of the water, as cold is a privation, but from the new modification of the action of the blood and spirits upon the nervous and membranous parts, the constriction of whose pores concurs to that modification. And if I do not misunderstand your opinion, *Carneades*, methinks it may be confirmed by this, which I have known observed by experienced surgeons, that by too strict ligatures unskilfully made, an arm, for instance, may be gangrenated; in which case, all the proper and immediate effect of the ligature is but the constriction of the part, though that constriction being unusual and excessive, it proves the occasion of the mortifying of the hand and arm, by hindering the free and usual access of the blood and spirits to that limb; upon which, by the depraved action of the parts of the body one upon another, and the concurrence of external agents, there ensues a mortification or gangrene of the part, which, if due remedies be not timely employed, is communicated to other parts, and kills the man.

Carn. WHATEVER become of your instance, *Eleutherius*, I thank you for your readiness to propose it in favour of my hypothesis, which you will easily judge not to be much concerned in the close of the excellent *Gassendus* his argument, for the positive nature of cold. For though these words of his ——

Them.

Them. You may save yourself the trouble of naming of them now, since, whatever they may seem to you, I profess I look upon them, as containing a distinct argument, which I shall therefore propose in its due place hereafter; but in the mean time, and before we leave the argument you would have us dismiss, give me leave to remind you, *Carneades*, of some part of your former discourse, and to take thence a rise to tell you, that you who told us that we ought not to consider the operations that qualities have upon our own sensories only, but also what they do to other bodies, will, I hope, allow me to demand, how a privation, or, if you will, how an imminution of motion can produce the hundredth part of those effects, which we daily see produced by cold in the bodies that are about us.

Carn. I thought, *Themistius*, I had intimated to you already what might have prevented your question; but since I see it is otherwise, you shall not find me backward to explain myself a little more fully. I do not pretend, that either an absolute privation of motion in a body, or a slowness of motion in the parts of it, is, as such, the proper efficient cause of the effects, vulgarly but unduly ascribed to cold alone; for, in my opinion, cold is rather the occasion, that the true efficient cause of such effects, which, I think, are properly to be ascribed to those physical agents, whose actions or operations happen to be otherwise modified, than else they would have been upon the occasion of that imminution or slackness of agitation which they meet with in cold bodies, by occasion of which they are both deprived themselves of the agitation they communicate to such slow bodies, and thereby act no longer, as, were it not for that loss, they would, and by a natural consequence of this change, which is made in themselves, they do also, though less notably, modify the action of other bodies upon them: from which unusual alterations happening in a world so framed as this of ours is, and governed by such laws, respecting motion and rest, as are observed among bodies, there must, in all probability, result many new, and some of them considerable phænomena. For though quiescent bodies seem not to have any action, which among corporeal substances seems to be performed only by local motion; yet bodies quiescent themselves may concur to great effects, both by determining the motions of other bodies, this or that way, or by receiving their motion totally, or in part, and so depriving the formerly moving bodies of it. Thus the arches of a bridge, though immoveable themselves, by guiding the water of the river that beats against them, may occasion a rapid and boisterous stream, capable to drive the greatest mills, and perform more considerable effects, though the river, before it met with them, ran calmly enough, as is evident at *London* bridge, especially when the water is near a low ebb. And now I have mentioned water, I will add, that though water itself be not a quiescent body, but, being a liquor, has its parts in perpetual motion among themselves; yet since that agitation is exceeding slow, in comparison of the swiftness of a cannon-bullet, in respect whereof the calm surface of the water participates of the nature of a quiescent body, bullets themselves shot from out of guns elevated but little above the level of the water (upon which score they make but a very sharp angle with it); these bullets, I say, do not unfrequently rebound from the surface of the water, and consequently, even these so wonderfully swift bodies receive a new determination from it.

Eleuth. One may add, *Carneades*, to your instances, that, in a tennis-court, the wall, against which balls are strongly impelled by a racket, contributes much to the mischief that those balls do often to by-standers in the gallery, as the wall, though itself unmoved, gives a new determination to the moving ball, and by its resistance makes it rebound or reflect at an angle equal to that of the ball's incidence. And this concurrence of the wall to such effects is the more evident, because of this other circumstance, which also befriends your opinion, that, if the impelled ball, instead of hitting against the wall, hits

against

againſt the net, this, by yielding, deprives the ball of its impetus, and hinders the reflection that would elſe enſue.

Carn. You have, I confeſs, ſomewhat prevented me, *Eleutherius*, but yet not altogether; for though I was going to propoſe the example of a ball, yet it was in ſomewhat a differing way; for I was about to propoſe to *Themiſtius* the example of a ball, which, if it be forcibly and perpendicularly thrown againſt the hard ground, has its determination ſo altered, that whereas it moved before towards the centre of the earth, it immediately, with almoſt the like ſwiftneſs of motion, tends directly upwards. And if on the other ſide you throw the ball, not againſt a hard, but againſt a muddy piece of ground, it will not rebound, loſing its own motion, by communicating it to the parts of the yielding mud; as may be in ſome meaſure illuſtrated by the great commotion made in a ſmall pond of water, when a ball (or a round ſtone) being but gently let fall upon the ſurface of it, has its motion thereby deadened, and transferred to the parts of the liquor, which, perhaps, will be viſibly agitated at the remoteſt brink of the pond.

Eleuth. THESE examples may conduce much to explicate your doctrine, *Carneades*; but ſince *Themiſtius* himſelf was ſo equitable a while ago, as to allow you much time to defend ſuch a paradox as yours againſt *Gaſſendus*'s argument, I ſhall, with your leave, of which I doubt not, to the examples already mentioned, add this one more. Suppoſe upon a ſtream that runs through ſome town, which is not very rare, there were built a number of differing mills, ſome for the grinding of corn, others for the fulling of cloth, others for the moving of bellows to melt ores and metals, others for forging of ſword-blades, others for making of paper, and others for other uſes; and ſuppoſe, that an enemy coming to beſiege this town ſhould ſucceſsfully imitate *Cyrus*'s ſtratagem, when by ſuddenly diverting the courſe of *Euphrates* he took *Babylon*; would it not be conſequent to this diverſion of the water into ſome lower place, and this ceaſing of the ſtream to run in its former channel, that the action of all theſe mills, by which ſo many differing operations were performed, muſt of neceſſity ceaſe too? though the beſiegers do not produce this change by any poſitive and direct violence that they offer to the mills, but only by hindering them from receiving the wonted impulſes which were requiſite to keep them in motion.

Carn. I DISLIKE not your inſtance, *Eleutherius*, which yet will not altogether render uſeleſs what I was going to ſay about a wind-mill, which will illuſtrate one part of my doctrine, for which your water-mill does not ſeem to have been intended. And, that this example may the better do ſo, I will ſuppoſe a wind-mill to be built in ſome low place near the bank of your ſtream, which ſtream we will ſuppoſe to be liable, as ſome others are, upon the falling of great and ſudden rains upon the neighbouring hills, to overflow its banks, in caſe the increaſe of the water be not then hindered by the wind-mill's lifting up conſtantly ſome parts of it, and conveying it away by pipes or otherwiſe; and then let us ſuppoſe, what really ſometimes happens, that the wind ſhould ſo ceaſe, that there ſhould not blow any wind ſtrong enough to move the ſails for a great while together; will it not hence manifeſtly follow, that by reaſon of this abſence of the wind, which abſence has the nature of a privation or negation of a ſtream-like motion in the air, not only there will be a ceaſing of thoſe effects and operations, whatever they were, that were wont to be performed within the mill itſelf, but alſo there will be a durable intermiſſion of that main work of the mill, whereby it carried off ſuch a quantity of water; which work ceaſing with the wind, whilſt the flowing in of the water does not ceaſe too, but continues, as formerly, the ſtill-increaſing water muſt bear down or overflow its wonted banks, or other boundaries, and by its unruly effuſions drown the neighbouring parts, and produce the diſorders, that is, the new phænomena naturally conſequent to an inundation made by ſuch a quantity of water. And if the water conveyed away by means of the mill, through pipes or channels, were employed to water grounds, or other particular uſes, the growth or fertility, at leaſt of the vegetables, that water was requiſite to nouriſh, or the other uſes, to which it was neceſſary, muſt conſequently be much, if not totally, hindered.

Phil. I KNOW not, whether we may not refer to the ſubject of your diſcourſe, what may be obſerved in paralytick affections, where a little viſcous or narcotick humour, obſtructing, or otherwiſe diſaffecting one part of a nerve, though its proper and immediate action be only to hinder, or weaken the ſpirits, that were wont, in competent plenty, to paſs freely along the nerve to the muſcles whereto it leads; yet the action of the other parts of the body, and the relaxation of the fibres, do oftentimes produce a tremulous motion in the limbs, and particularly the hands; and ſometimes alſo the mouth, neck and other parts, are drawn awry in an odd and frightful manner.

Carn. THOUGH I approve of *Philoponus*'s fancy, yet I think a more quick and notable inſtance to the ſame purpoſe may be taken, from what happens to birds, and rats, and cats, and ſuch kind of warm animals, in Mr. *Boyle*'s engine. For as the air by the agitation of its parts, or that of ſome ethereal ſubſtance that pervades it, entertains the fluidity of water, and other aqueous liquors; and when that agitation is hindered, or too much leſſened, water ceaſes to be fluid, and upon that divers violent effects enſue, wont to be aſcribed to glaciation: ſo the bodies of warmer animals, having been born in the air, and perpetually expoſed to the action of it (though that be ſeldom heeded) when being placed in the receiver of the air-pump, and by the operation of that inſtrument, which withdraws the former air, and keeps out the new, the air, that was wont continually to act upon them, is kept from doing ſo any longer, though this abſence, or not touching of the air, be but a privative or negative thing, yet, by reaſon of the ſtructure of the animal, his ſpirits and humours, aſſiſted by the concourſe of more general cauſes, are brought to act ſo differingly from what they were wont to do, that the blood and juices ſwell, the ſtomach vomits, the animal grows faint and ſtaggers, the limbs, and at length the whole body are convulſed, the circulation is ſtopped, and at laſt the animal killed; and all this done in a very few minutes of an hour, without the viſible intervention of any poſitive agent.

Eleuth. WHAT you ſay, *Carneades*, concerning the quick and violent death of warm animals in Mr. *Boyle*'s engine, puts me in mind of an experiment I ſaw made in that inſtrument upon cold animals, which, methinks, may well illuſtrate the compariſon we lately employed of a wind-mill. For as thoſe great artificial engines loſe their motion, and the operations depending on it, if that ſtream of air, we call the wind, be held from keeping them going; ſo inſects, and ſome other cold animals, have their differing motions ſo dependent upon the contact of the air, that, as ſoon as ever they are deprived of it (by the engine we are ſpeaking of) divers ſorts of them will lie moveleſs, as if they were dead; and I have known ſeveral of them, that were put in together, continue in that ſtate for many hours, as long as it pleaſed our friend to with-hold the air; but when once he thought fit to let a ſtream of air enter the receiver, theſe ſeemingly dead animals, as worms, bees, flies, &c. like ſo many little wind-mills of nature's (or rather, her great author's) making, were ſet a moving in various manners (as creeping, flying, &c.) ſuitable to their differing ſpecies.

Carn. So that, to ſum up, in a few words, the reſult of theſe inſtances, and the reſt of the paſt diſcourſe on the ſame ſubject, it appears by what has been ſaid, that the effects undeſervedly aſcribed to cold need not, in our hypotheſis, be referred to a privation, but to thoſe poſitive agents, or active cauſes, which, by their own nature, are determined

determined to act otherwise, or suffer otherwise from one another, in cases, where there is a great hinderance, or ceasing of wonted agitation, than where there is not.

SECTION V.

Them. It may, perhaps, now be time to put *Carneades* in mind, that, in what he has been discoursing all this while, he has proposed answers but to a couple of *Gassendus*'s arguments, and left the rest untouched.

Carn. I should readily grant, *Themistius*, that I have dwelt too long upon so few arguments, if I did not hope, that by fully answering them, and giving the company a particular account of my notions concerning cold, I might very much shorten and facilitate the remaining part of my task, which engages me to return answers to the other arguments you speak of, the grounds of solving which, I think, I have already laid in the past discourse. And therefore you may go on to propose the next argument of *Gassendus*, as soon as you please.

Them. And I shall do it, *Carneades*, in that learned man's own words, which I well remember to be these: *Fac manum immitti in aquam nunc calidam, nunc frigidam; quamobrem manus intra istam, non intra illam refrigeratur? An quia calor manûs intra frigidam retrahitur, manûsque proinde relinquitur calida minùs? At, quidnam calor refugit, quod intra frigidam reperiatur? nonne frigus? at si frigus est tantùm privatio, quidnam calor ab illa metuit? privatio sanè nihil est, atque adeò nihil agere, unde ejus motus incutiatur, potest.* Gassend. Lib. VI. cap. 6.

Carn. This objection, *Themistius*, may indeed puzzle many school-philosophers, but will easily admit an answer in my hypothesis. For that does not oblige, or so much as tempt me to ascribe (as a Peripatetick would do) to a mere quality (for such is heat) both a knowledge of its danger, and a care, and skill, to preserve itself from its enemy, the cold, by a retreat inwards. For, agreeably to what I lately delivered, it is obvious for me to explicate the phænomenon thus: when a man puts his hand into warm water, the agitation of the corpuscles of that liquor surpassing that of the spirits, blood, and other parts of his hand, cannot but excite in him a sense of heat; but when he puts the same hand into cold water, the case ought to be much altered, not by any imaginary retreat of the spirits, but the communication of motion, by other parts, to the surrounding water, by which means, there must be in the hand a great lessening of the former agitation of its parts, the perception or sense of which decrement of motion is that which we call the feeling of cold.

Eleuth. I think, indeed, *Carneades*, that though this argument may be considerable against those, that the learned framer of it might have in his eye, it is but invalid against you. But can you as well decline the force of that other objection, which *Gassendus* more insists on, and which seems as directly to oppose you, as any other adversaries of his hypothesis?

Them. I presume, *Eleutherius*, you mean that cogent argument, which *Gassendus* proposes, and prosecutes more fully than the rest, deducing it from the way of artificially freezing water by a mixture of snow and salt, placed about the outside of the glass, that contains the liquor. For, from this practice, he rationally concludes, that since this frigorifick mixture is, through the glass, able to freeze the water into ice, it may as justly be affirmed to act by corpuscles of cold, as fire can be to act by calorifick corpuscles, when kindled coals, placed on the outside of the glass, make the contained water boil. And this cogent argument will, I hope, prove the more satisfactory to *Carneades*, ſince it is not drawn from what he would call a diſputable peripatetick notion, but from the ſame quiver, whence he affects to take his ſhafts, experience itſelf.

Carn. I FREELY acknowledge, gentlemen, this argument to be very plauſible; but that it is clear and cogent, I muſt not grant, till I be better ſatisfied that it is ſo.

AND I ſhall ſcarce think it as evident, that ice, and ſalt, act by a poſitive quality, as that burning coals do ſo, though cold ſeems as well to be produced by the former, as heat by the latter. For innumerable experiments ſhew, that heat, in the fire eſpecially, is a poſitive quality, conſiſting in a tumultuary and vehement agitation of the minute parts of the body, that is ſaid to be hot, and producing alſo in the bodies, that it is communicated to, a local motion, which is manifeſtly a poſitive thing. This is ſo evident, in the heating of bodies by mere attrition, the ſmoking and melting of divers bodies in the ſun-beams (eſpecially at fit times of the day, and year) the ſudden boiling and diſſipation of water, oil, &c. dropped on a red-hot iron, and many other obvious inſtances, that it were a needleſs work to go about to prove it, eſpecially, ſince both *Themiſtius*'s Peripateticks, and *Gaſſendus* himſelf, who ſo often diſagree about other things, agree in confeſſing, that heat is a poſitive quality.

Them. BUT remember, *Carneades*, that the grounds on which they do ſo, are the ſame on which *Gaſſendus* juſtly builds the propoſition, that cold alſo is a poſitive quality.

Carn. I DID not forget that, *Themiſtius*; for I was about to ſubjoin to what I laſt ſaid, that it is evident, not only by the confeſſion of my adverſaries, but by that (which to me is much more conſiderable) of nature herſelf, proclaiming it in the inſtances I juſt now mentioned, that heat is a poſitive quality; whereas, that cold likewiſe is ſo, does not appear to me by the experiment of artificial congelations. For in this, all that is clear in matter of fact, is, that ſnow, or beaten ice, and ſalt, are put about a veſſel full of water, or other aqueous liquor, and that, within a while after, this water begins to be turned into ice; but that this glaciation is performed by ſwarms of atoms of cold, that permeating the glaſs, invade and harden the liquor, is not perceived by ſenſe but concluded by a ratiocination, the cogency of which I am allowed to examine, without affronting the certainty of ſenſe, that not being concerned in the caſe. If then an intelligible way can be propoſed of fairly explicating the phænomenon, beſides that inſiſted on by *Gaſſendus*, the objection drawn from this experiment againſt my hypotheſis will be invalid. And ſuch an explication monſieur *Des Cartes* ingeniouſly gives in his meteors: *Quia materia ſubtilis* (ſays he) *partibus hujus aquæ circumfuſa craſſior aut minus ſubtilis, & conſequenter plus virium habens, quàm illa quæ circa nivis partes hærebat, locum illius occupat, dum partes nivis liqueſcenda partibus ſalis circumvolvuntur. Facilius enim per ſalſæ aquæ quàm per dulcis poros movetur, & perpetuò ex corpore uno in aliud tranſire nititur, ut ad ea loca perveniat, in quibus motui ſuo minùs reſiſtitur: quo ipſo materia ſubtilior ex nive in aquam penetrat, ut egredienti ſuccedat, & quum non ſatis valida ſit ad continuandam agitationem hujus aquæ, illam concreſcere ſinit.*

Lib. meteor. cap. 3.

Phil. I LEAVE *Themiſtius* to conſider, whether this explication be without exception; but I confeſs it is not without analogy, and that even amongſt the four firſt qualities themſelves. For when we chymiſts have a mind to dry (for inſtance) the calces, or precipitates, or other powders, from which we have filtrated the liquors we employ to waſh or dulcify them, it is uſual either to put the filtres wherein theſe powders remain almoſt in the form of mud, or to ſpread the ſtuff itſelf upon brown paper, or pieces of brick, or chalk, which much haſten the exſiccation of the things laid upon them, not by any drying particles which they emit into the ſoft ſubſtances, but by imbibing the ſuperfluous parts of the liquor, and thereby freeing from them the ſubſtances to be dried. And I remember I have ſeen our friend Mr. *Boyle*, by immerſing a piece of ſoft crumb

crumb of bread into an actually cold liquor, that would haſtily imbibe its aqueous corpuſcles, and dry it in a minute, or two, of an hour, ſo as to make it feel hard.

Eleuth. THESE inſtances bring into my mind another chymical experiment, that I have ſeen made by the ſame gentleman, which was, that by putting into weak ſpirit of wine a ſufficient quantity of ſalt of tartar, he quickly dephlegmed the ſpirit without diſtillation, or ſo much as heat. And this will the better illuſtrate the *Carteſian* explication, becauſe it is manifeſt, by the change that will be made of the moſt part of ſalt of tartar into a liquor, that will not mix with the now dephlegmed ſpirit of wine, that the reaſon of the operation is, that the aqueous particles of the phlegmatick ſpirit, finding, it ſeems, more convenience, or facility, to continue their motion among the fixed corpuſcles of the ſalt, than the vinous ones of the ſpirit, paſs into the alkaly, and diſſolve it; and thereby deſert the liquor, through which they were diffuſed before. And I know another ſaline body, that ſo unites with water, as not to be, by the eye, diſtinguiſhable from it, and yet is of ſuch a texture, that water is ſo much leſs diſpoſed to mingle with it, than with ſpirit of wine itſelf, that it will forſake the body it kept in agitation, to paſs into this ſpirit; and ſo leave that which it kept in the form of a liquor before, to appear in the form of a conſiſtent body; which inſtance comes ſome what nearer, than the former, to the experiment of glaciation.

Carn. THOUGH what you have recited, gentlemen, be not unwelcome to me, yet, I think, I can propoſe you an experiment fitter to dilucidate the Carteſian explication. For, I remember, that our common friend, having a mind to ſhew that a ſmall proportion of agile matter, inviſibly diffuſed through a body, that would be otherwiſe conſiſtent, may bring it to, and keep it in the ſtate of fluidity; deviſed and ſhewed me the following experiment. He took camphire broken into ſmall bits, and caſting a convenient quantity of it upon aqua fortis, ſuffered it to float there, till, without heat, the camphire was diſſolved into a liquor, and it looked and felt like an oil, which, though ſhaken with the aqua fortis, would emerge to the top again. If this oil were kept well ſtopped, that the ſpirits of the menſtruum might not evaporate, it would (as he affirmed trial had taught him) continue long fluid, he having ſometimes kept it a year, or two, or more. And that it is the agile ſpirits of the aqua fortis, that keep the camphire fluid, he has made probable by divers things, that I muſt not now ſtay to recite. And that the quantity of theſe agile particles is but ſmall, I am induced to think by this, among other things, that when I have made a ſmall parcel of but moderate aqua fortis turn a pretty proportion of camphire into oil, and ſeparated that oil from it, I could, by caſting freſh camphire on the ſame menſtruum, reduce that alſo into the form of oil. Now, that theſe fluidifick ſpirits (if I may ſo call them) are not ſenſibly warm (no more than the Carteſian *materia cæleſtis)* in water, is manifeſt to the touch: and whereas I at firſt ſuſpected, that the reaſon why the pouring of this oil into water doth preſently reduce it into camphire again, might be the coldneſs of the water; I after thought, upon a farther information, that the reaſon rather was, that the nitrous ſpirits being diſpoſed to paſs out of the oil into the water, this liquor readily imbibed and diluted them, and conſequently, diſabled ſo many of them, that thoſe that remained, could not do their former work any longer: ſince he had tried purpoſely, that the reduction of the oil into camphire would preſently be made, though that liquor were not poured into cold water, but hot: ſo that the agitation that it received from the particles of the menſtruum, though not to our touch ſenſibly warm, was much more efficacious, than that which it received from the heat of the water.

Eleuth. I KNOW not, whether beſides the inſtances that have been now propoſed, one may not alledge ſuch an argument alſo in favour of the Carteſian opinion about cold,

as

as would not be insignificant, though it should be made appear, that cold may sometimes be produced by, or upon the emission of corpuscles, that in some sense may be called frigorifick. For there may be corpuscles of such a nature, as to size, shape, and other attributes, as to be fit to enter the pores, and pierce even into the inward parts of water, and some other bodies, so as to expel the calorifick corpuscles they chance to meet with, or to clog, or hinder their activity, or on some other account, considerably to lessen that agitation of the minute parts, by which the fluidity of liquors, and the warmth of other bodies, are maintained. But even in such cases, though the agent, and the actions that produce coldness, be positive things; yet the nature of coldness itself may consist in a privation. As when a man is killed by a bullet, his death is effected by a positive, and even impetuous action, and yet death itself is but a privation of life. If also, in a dark room, a man cast cold water upon a burning coal, though the water act by its positive quality of moisture, and, by virtue of that, extinguish the fire, and, by that means, destroy the light, yet the darkness, that is consequent upon this action, is not a positive thing, but a privation.

SECTION VI.

Phil. The pause you here made, gentlemen, makes me think it seasonable to put the company in mind, that it begins to grow late, and therefore to call upon *Themistius* to produce what he has yet to alledge out of *Gassendus*.

Them. The philosopher you have named, has indeed another weapon to destroy the error about cold, which he confutes. And this argument, like a two-edged sword, that cuts on both sides, does not only confirm what he maintains, but destroy the chief objection that can be made by his adversaries. The argument I speak of, he proposes in these terms: *Tametsi multa videantur ex sola caloris absentia frigescere, nihilominus nisi frigus extrinsecus introducatur, non tam profectò frigescere quàm decalescere sunt censenda. Esto enim lapis, lignum, aut aliquid aliud, quod nec calidum, nec frigidum sit, id ubi fuerit admotum igni calefiet sanè; at cùm deinceps calor excedet, neque frigidum ullum circumstabit, non erit cur dicas ipsum frigefieri potius quàm minùs calidum fieri, rediréve in suum statum.*

Carn. Whether this contain not a dispute *de modo loquendi*, I shall leave the company to judge, by what I shall return in answer to it. I say then, that it seems to me, that there is in the discourse an obscurity, if not an ambiguity, though, I am confident, not affected by the candid *Gassendus*. But to answer as directly as I can; if we speak only of a coldness, as to sense, I see not, why water, or wood, or any such body, that is heated by the fire, may not, upon its removal thence, be said to grow cold, and not barely to *decalescere*, in our philosopher's sense of that word. For the heat and coldness of water, in reference to sense, consisting, as I lately shewed in this, that the particles of it are more or less agitated, than the hand that is immersed in it, they need nothing else to make the liquor grow cold, than such an imminution of the brisk motion of its corpuscles, that they cease to be as much agitated, as those of our organs of feeling: and if this already impaired agitation be still more and more lessened, the liquor will still grow colder and colder, without the help of any positive cause, until at length the agile parts, that kept it fluid, being quite expelled, or disabled, the form of the liquor comes to be exchanged for that of ice.

Phil. But what say you to that part of *Gassendus*'s argument, where he proposes an adiaphorous body, which, when affected with an adventitious heat, would not grow cold

cold by the bare removal, or cessation of that heat, unless it were refrigerated by an agent, that were positively and actively cold?

Eleuth. I SAY, *Philoponus*, this supposition should not be made, and that I know of no such adiaphorous body. For since, as I have been obliged to inculcate, those bodies must be cold, as to sense, whose parts are less agitated than those of our hands, and consequently metals, stone, wood, and other solid bodies, and also water, wine, and all other unmingled liquors we know, being heated by the fire, will grow cold again of themselves, because the adventitious motion ceasing by degrees, either upon the recess of the igneous corpuscles, or the imparting of the extraneous agitation to the air, or other contiguous bodies, the stone, or water, &c. will again have so much fainter an agitation, than that of a man's sensory, as to be by him judged cold: and because almost all the species of permanent bodies here below, that are known, have, in what is called their natural state, a less degree of agitation in their insensible parts, than men's organs of feeling are wont to have, those bodies may be said to be naturally cold, and therefore ought not to be supposed to be indifferent to cold or heat.

Phil. BUT whether or no nature do really afford us an adiaphorous body; yet surely the mind is able to conceive one, and therefore *Gassendus* may be allowed to suppose such bodies, and *Carneades* may be obliged to answer what he argues upon that supposition.

Carn. IT is one thing to propose an adiaphorous body, as barely an intelligible, or a possible thing; and another, to give instances of it, as *Gassendus* has done in particular bodies, in which that indifference is not to be found. And it is this last kind of supposition, that I disallowed in *Gassendus*'s argument. But if a body should be proposed, as adiaphorous in reference to heat or cold, I might say, without prejudice to my cause, that if such a body should be carried into a hot place, it might there grow warm; and if it should be removed back again, and kept, till it lost that new adventitious heat, it might rather *decalescere*, than grow cold as to sense. But the reason is, because it is not every degree of imminution of heat, that is able to denominate a body cold, but such a degree as reduces the parts of it to a fainter motion, than is at that time in those of our organs of feeling; and till this be done, or at least very near done, the proposed body is still (if I may so speak) in the state of heat, as to sense: which last words I add, because, that in reference to other bodies, it may then be notably refrigerated. As lead, that has but heat enough to keep it in fusion, may, by the pouring on of such water, as to a man's hand would feel hot, be brought to grow hard, which loss of fluidity is also the natural effect of cold, though perhaps, both the metal, and the liquor, be yet as to sense considerably hot.

Eleuth. So that, according to you, none of the kinds of bodies, that are actually known in nature, are adiaphorous as to sense. On which occasion let me note by the by, that the frequent variations of sense must render it but an uncertain standard of heat and cold: and upon supposition, that there were an adiaphorous body in reference to our sense; yet it would not be so in reference to all other bodies, or, in the phrase of our *Verulam*, speaking of heat, *in ordine ad universum*. And for what remains, the controversy grounded on *Gassendus*'s argument seems to be rather verbal, than real, and may be determined, or composed, by settling the distinct acceptions of the words cold and heat.

SECTION

SECTION VII.

Phil. WHEREFORE I wish that we might not waste the little time that is left us, upon niceties of no greater concernment; and I think this short time would be better employed, if *Carneades* would be pleased to tell us a little more particularly, what he supposes to be the thing, that withheld Mr. *Boyle* from delivering an opinion about the nature of cold.

Eleuth. YET, methinks, it is but fair, that *Carneades*, who has all this while been confined to the answering another's arguments, should now take his turn to propose his own.

Carn. I FIND, in each of your motions, gentlemen, something so equitable, and so expedient, that I shall in part comply with both. And that I may hasten to do what *Philoponus* desires, I shall do no more than briefly point at two things, that may be alledged in favour of the hypothesis I defend. For if you reflect upon what we have already discoursed, we may take notice of things there, that will scarce be well accounted for by being ascribed to positive cold, but may be far better explained agreeably to our hypothesis. And must add, in the next place, that I, who sustained the person of a respondent, may pretend to have sufficiently discharged my office, if I have shewn the invalidity of all the opponents arguments; and it is his part, who asserts a positive thing in nature, to make it good, whereas he that denies it, needs not alledge any other reason why he does so, than the authority of that justly received axiom in philosophizing, *Entia non sunt multiplicanda absque necessitate.* And, I hope, there will need no other engine to demolish an ill-formed and proofless opinion about cold, than an axiom so solid and efficacious, that in the opinion of almost all the modern naturalists it has been able to abolish such potent and immense bodies as the *primum mobile* itself, and a superior orb or two, the least of which contained that firmament, in comparison whereof the whole earth is but a point. And not only so, but the same axiom has banished the angels and intelligences from the cœlestial orbs, that *Aristotle* and his followers had assigned them to turn about; or rather hath released those noble and happy spirits from the drudgery, to which the philosophers of so many ages had needlesly doomed them.

Eleuth. I THE less distrust the validity of the axiom you alledge, because I observe it to be the ground, on which is built a great part of the reformation of philosophy, that is introduced by the moderns. For one of the main things that first moved considering men to seek for more satisfactory opinions, than those of the peripatetick schools, was, that these obtruded a great many tenets in philosophy, that were not only unproved, but unnecessary to the explication of the phænomena of nature; as it were not difficult to shew.

BUT I see *Philoponus* preparing to renew the motion he lately made, in which the shortness of time makes me now think it seasonable to join with him, I being no less desirous than he to know, what may be the motives of your friend's declining to declare himself fully about the nature and cause of cold.

Carn. I HAVE already intimated to you, at the beginning of our conference, that he is himself the fittest person to be addressed to for satisfying this enquiry. But not to be altogether silent on this occasion, I shall tell you, that, as far as I can guess, he waits till farther trials and speculations have resolved him in some points, wherein he is not yet satisfied: for, being of a temper backward enough to acquiesce without sufficient evidence, when the enquiry is difficult, and the subject important; he seems to me to be be kept in ſuſpenſe, both by ſome ſpeculative doubts, and the phænomena of divers experiments, ſome of which are not delivered in his book. It would be now improper to mention the ſcruples and heſitancies they have occaſioned in him; though of thoſe, I have heard him ſpeak of, I ſhall name ſome inſtances that occur the moſt readily. As I remember I heard him make enquiry, as to thoſe that would have cold produced by corpuſcles of cold; whether, and on what account, thoſe little fragments of matter are cold? whether thoſe frigorifick particles, that muſt in multitudes crowd into water to turn it into ice, have gravity or levity, or are indifferent to both? And how any of the three anſwers, that may be made to this enquiry, will agree to ſome phænomena, that may be produced? what ſtructure the corpuſcles of cold can be of, that ſhould make them frigorifick to that innumerable variety of bodies they are ſaid to pervade? And, whether the frigorifick faculty of theſe corpuſcles be looſable, or not? As alſo, whether or no they be primitive bodies; and if it be ſaid, they are not, whether there was not cold in the world before they were produced, and whence that cold could proceed? And if it were ſaid, they are primitive bodies, he demanded, how it came to paſs, that by putting a certain factitious body actually warm, into water that was alſo warm (both which appeared by a good ſealed weather-glaſs) there ſhould preſently be produced an actual coldneſs (diſcernible by the ſame thermoſcope)? Theſe, and I know not what other ſcruples and difficulties, ſuggeſted to him by his thoughts, or his experiments, were the things, that, I ſuppoſe, prevailed with a man of his temper to forbear for a while the declaring of his ſentiments about cold, leſt the event of ſome farther trial ſhould ſhew him cauſe to retract them.

Phil. WHAT you have freſhly intimated, *Carneades*, of Mr. *Boyle*'s having other heſitations, than thoſe you have named and ſuggeſted by experiments, not publiſhed in his hiſtory, does, I confeſs, the more excite my curioſity to have, at leaſt, a taſte of thoſe perplexing phænomena.

Carn. You may eaſily gueſs, *Philoponus*, by what I have told you already, that you are not to expect a full ſatisfaction from me on this occaſion. But yet, that your curioſity may not be fruſtrated, I ſhall venture to acquaint you with two phænomena, which were, I ſuppoſe, none of the leaſt motives of his backwardneſs to declare himſelf. But though ſome body perhaps thinks, that the grounds of ſolving theſe phænomena, and moſt of the newly recited ſcruples, may be picked out of ſome things, that may already have paſſed among us in this conference; yet, becauſe we have not now time to enter upon a diſcuſſion of this matter, I am willing you ſhould ſuſpend the debate, till we have occaſion to meet another time; and therefore I ſhall now only acquaint you with a couple of experiments, that he ſet down for a virtuoſo, who was to ſolve the two main problems ſuggeſted by them. The firſt whereof was, whence water ſhould, upon congelation, acquire ſo vaſt a force, as he found it had, to lift up great weights, and burſt containing bodies; though it ſeemed by ſeveral circumſtances, that the motion of the water is very much diminiſhed, when it is changed into ice. And the ſecond problem is thus conceived; if, as a briſk agitation of a body's inſenſible parts produces heat, ſo the privation of that motion is, as *Cardan*, and the Carteſians would have it, the cauſe of cold; whence is it, that, if certain bodies be put together, there will be a manifeſt and furious agitation of the ſmall parts, and yet, upon this conflict, the mixture will not grow hot, but ſenſibly and even conſiderably cold? The narratives themſelves, of the experiments, are too long to be now read over to you. And therefore, I ſhall leave the paper, that contains them, among you, to be peruſed at your leiſure, between this and our next meeting, till when I muſt bid you farewell; only deſiring

 you

you in the mean while to remember, that, as I have but acted a part imposed upon me in our past conference, so, notwithstanding any thing that I have said in my assumed capacity, I reserve to myself the right of appearing as little pre-engaged, as any of you at our next meeting.

TWO PROBLEMS ABOUT COLD,

Grounded on NEW EXPERIMENTS;

And proposed in a LETTER to a FRIEND.

To my very Learned Friend Mr. *J. B.*

SIR,

I PRESUME, that you will not be surprized to be told, that I send you the inclosed papers, not only that I might gratify your curiosity, but that you may by them be enabled to help me to satisfy my own; and therefore I shall accompany the historical transcripts I made of the following experiments, as I found them registered for my own remembrance, with some of the doubts suggested to me by some of the phænomena that occurred. But yet I shall not trouble you with all the difficulties that at first troubled me, but reduce the exercise, I desire to give your sagacity to the solution of two problems. And I will begin with propounding that first, which is grounded upon the last of the two following papers, because, though the historical part of that be much the longest, yet the grounds of my quere concerning it will be much more briefly pro-
Prob. II. posed, the experiment itself naturally suggesting this problem; " How, upon the mixture of two or three bodies, such as those mentioned in the paper, there should manifestly ensue a great and tumultuary agitation of small parts, and yet even during this confli[illegible], not any sensible heat, but a considerable degree of cold, be produced," and that even in the internal parts of the mixture?

THE

The inducements to make this problem need not be far fetched, it being obvious enough, that, according to the corpuſcularian philoſophy, which you and I agree in, a briſk, and various agitation of the minute parts of a body is that, which makes it hot, both in reference to our ſenſories, and to its operations on other bodies. But I doubt the riſe of the problem is much more eaſy to be underſtood, than the cauſe of the phænomenon, about which I will not aſk you, whether one may not aſſert, that local motion is, in its own nature, a generical thing, which may be ſo diverſified by circumſtances, that one kind of modification of it, as it is made in corpuſcles of ſeveral ſizes, and ſhapes, may be the cauſe of heat, and another, that of cold? or elſe, whether we may ſuppoſe, that cold is a poſitive thing, and operates by real corpuſcles of cold, which happening to abound, and yet to be locked up in the bodies, whoſe mixtures I employed, they are, by the great conflict that diſſolves the texture of the claſhing ſalts, ſeparately put into motion, and that in ſuch numbers, that though really there would be a heat produced by the briſk and confuſed agitation of ſome of the parts, yet that heat is not only concealed, and checked, but maſtered by the over-powering operation of the frigorifick corpuſcles. But to aſk you about this, or any other particular way of ſolving our phænomenon, were to forget, that my aim is to learn not your opinion of this, or that particular conjecture, or fancy, about our problem, but in general, how it may be beſt reſolved, and what you think to be the true cauſe of ſo odd an effect.

Having thus diſpatched the little I had to ſay about the paper, that ſuggeſted the ſecond problem, I will now ſuppoſe, that you have read the phænomena, that contain the riſe of the firſt, to which I ſhall proceed, without farther preamble, ſince the queſtion, or problem, that theſe naturally call for, is, "Whence this vaſt force of "freezing water proceeds?" Prob. I.

For the breaking of reſiſting bodies being to be made by a violent local motion, and cold, according to the judgment even of the moderns, either conſiſting in, or at leaſt, being accompanied with a privation, or a great imminution of motion, it ſeems very difficult to conceive, how cold ſhould make water to exert ſo wonderful a force. I know the learned *Gaſſendus*, and divers other philoſophers teach us, that glaciation is performed by the entering of ſwarms of corpuſcles of cold, as they call them, into the liquor. But I much doubt, whether, from this hypotheſis, a good ſolution of our phænomenon will be derived, ſince theſe atoms of cold ſeem not barely, as ſuch, to make that expanſion of the water, which is required in the experiment by me recited. For I ſee, that though water will be more and more refrigerated, according as the air grows colder and colder, yet, till it be brought to an actual glaciation, all the ſwarms of the frigorifick atoms in it are ſo far from expanding it, that they more and more condenſe it. And even that degree of cold, which deſtroys fluidity, though it expands water, does not do it merely by the multitudes of the frigorifick corpuſcles, that invade the pores of the lately fluid body, ſince pure ſpirit of wine, and almoſt all chymical oils, though expoſed to the ſame degree of cold, that turns water into ice, or, as I have tried, unto a far greater than is neceſſary to do ſo, will be but the more condenſed by thoſe ſwarms of particles. But, which is more conſiderable, I have carefully obſerved, that, beſides common or expreſſed oils, chymical oil of aniſeeds itſelf, being frozen, or concreted by an intenſe degree of cold, will not be expanded, but notably condenſed, and accordingly grow ſpecifically heavier than before. And this was one thing that kept me from expecting the removal of our difficulty from the ingenious explication given of freezing by the Carteſians, when they teach, that the eel like particles, whereof they ſuppoſe water to conſiſt, are very remiſsly agitated, and their want of pliantneſs makes their contexture leſs cloſe; which ſeems not to agree with the lately mentioned

 trials.

trials. And though these eel-like particles should lose all their flexibleness, though in that case, it may probably be said, that they would take up less room than before, if nothing oppose their expansion. yet it does not thence appear, how they should acquire so vast a power to expand themselves in spite of opposition, as we have shewn water, by freezing, does acquire.

I did not hope to resolve our problem by the help of a vulgar supposition, that well stopped vessels are broken in frosty weather *ob fugam vacui*, since I found that supposition to be erroneous by divers experiments, some of which are mentioned in the history of cold.

It seemed less improbable, that some assistance to the solving of our difficulty might be given by two other things. Whereof the first is, that, for aught I have yet observed, no liquor but water, or that which participates of water, by having aqueous particles separable from it, will be made to swell by cold; nor will water itself do so upon every degree of cold, but only upon so great an one as actually turns it into ice. And the second is, that upon the glaciation of water, and aqueous liquors, we may observe in the ice many bubbles, greater or smaller, intercepted between the solid parts, and supposed to be full of air (I say, supposed, because, upon trial, I found them to have yielded but a small proportion of common air); which supposition, if true, would perhaps invite one to suspect, that the air contained in these bubbles might have an interest in our phænomenon; since I have found, by trial purposely made, that air congregated into visible, though not great portions, may exercise a considerable elasticity, which appeared not whilst it was invisibly dispersed through the water.

And if I did not suppose, both that you had taken notice, that there are wont to be numerous particles of springy air dispersed through the pores of water; and that you had considered, whether the want of pliantness, occasioned by cold in the aqueous corpuscles, whilst they are yet agitated and brandished by some permeating matter; and whether, upon the change of the pores, that we may conceive to be made in freezing water, either by the recess of one sort of subtil corpuscles, or the admission of another, or the closer constipation of the grosser parts, there may not be produced in corpuscles, that compose water) to say nothing of the intermixed air, or the concretions, or the coalitions, occasioned by the cold) a springiness capable to make many little bodies, endowed with it, exert a great force against the sides of the vessel, that oppose their joint endeavour to expand themselves: if, I say, I did not believe, that these, and the like suspicions, had occurred to you, as well as to me, together with the difficulties, wherewith each of them seems to be incumbered, I would acquaint you with what thoughts and trials occurred to me about these, and the like conceits. But I not daring to think this could prove other than a needless work, I must remember, that my business, in this paper, is to propose difficulties, not the ways of solving them; it being, from your kindness and sagacity, that these are as well expected, as desired, by,

SIR,

Your, &c.

AN

AN ATTEMPT

To MANIFEST and MEASURE the

GREAT EXPANSIVE FORCE OF FREEZING WATER.

CONSIDERING, when I writ the hiſtory of cold, that, though divers phænomena might induce an attentive obſerver to think, that freezing water had an expanſive force, yet I had not met with any that endeavoured, or even propoſed to meaſure it, whether, becauſe they reflected not on it at all, or judged not the force conſiderable; I, who looked with other eyes upon it, thought fit to repair that omiſſion, but was then ſo ill furniſhed with requiſites for doing it fully, that, I remember, I complained of it in my hiſtory of cold. And though, even afterwards, when the time of the year was favourable, I could not procure ſuch accommodations as my deſign exacted; yet, thinking an imperfect way of meaſuring to be better than none, I preferred, to the making no attempt at all, the endeavouring to do what the leaſt defective inſtruments I could procure would permit me, towards the making an eſtimate by known meaſures, of the expanſive power of freezing water. For though I did not expect I ſhould be able accurately to define it, yet I hoped I ſhould make ſuch an eſtimate, as to know, that force not to be, as one would think it, faint and contemptible, but very great and conſiderable.

I REMEMBER on this occaſion, that to manifeſt the force of freezing water, I cauſed the barrel of a ſhort gun to have a ſcrew fitted to the noſe of it, by which we might exactly ſtop it, as we did the touch-hole another way; then filling the barrel with common water, and cloſing it accurately by the help of the ſcrew, we laid it in a conveniently-ſhaped veſſel, wherein we encompaſſed it with a frigorifick mixture (of ſnow, or ice, and ſalt) and in a ſhort time we found, as we expected, the barrel to be burſt, part of the ice appearing along the gaping ſlit that had been made in the body of the iron by the freezing water, which, by this effect, ſeemed to emulate the juſtly-admired force of kindled gun-powder. But the deſign of this ſhort paper tending not ſo much to prove, as (in ſome ſort) to meaſure the expanſive force of water, I ſhall ſubjoin the tranſcripts of two or three experiments, made chiefly for that purpoſe.

EXPE-

EXPERIMENT I.

[THERE was taken a ſtrong cylinder of braſs, whoſe cavity was two inches in diameter; into this was put a bladder of a convenient ſize, with a quantity of water in it, that the neck of the bladder (which I had taken care to have oiled) being ſtrongly tied, the water might not get out into the cavity of the cylinder, nor be capable of expanding itſelf ſome other way than upwards. Then into this cylinder was fitted a plug of wood, turned on purpoſe, which was ſomewhat leſs in diameter than the cylindrical cavity, that it might riſe and fall eaſily in it. Upon the upper part of this plug was laid a conveniently ſhaped flat body, upon which were placed divers weights to depreſs the plug, and hinder its being liſted up by the expanſion wont to be made in water that is made to freeze; then a frigorifick mixture being afterwards applied to the cylinder, it appeared, within half an hour, or ſomewhat more, by a circle, that had been purpoſely traced on the ſide of the plug, where it was almoſt contiguous to the orifice of the cylinder, that the water in the bladder began to expand itſelf, and about two hours after, having occaſion to ſhew the experiment to ſome inquiſitive perſons, the circle appeared to have been heaved up, in my eſtimate, about $\frac{3}{8}$, if not half of an inch, notwithſtanding all the weights that endeavoured to hinder the aſcenſion, though theſe weights amounted to 115 pounds, which were all the determinate weights we could then procure, beſides a brick, and ſome other things, that were eſtimated at five pounds more; nor did I doubt that a far greater load would not have hindered its expanſion.]

EXPERIMENT II.

[WE took a braſs cylinder, whoſe dimenſions were three inches eight tenths in diameter, and in depth four inches. Into this we put a fine bladder of a convenient ſize, almoſt filled with water, and ſtrongly tied about the neck; upon this bladder we put the wooden plug to ſtop up the orifice, as much as was convenient, and upon the plug we put a piece of a flat board for the weights to ſtand upon. Theſe things being prepared, we conveyed the cylinder with all that belonged to it, ſave the board, into a large wooden bowl, where we applied to the cylinder a good quantity of the frigorifick mixture, made with beaten ice and bay ſalt; and having firſt marked with a circular line the edge or contact, where the orifice or lip of the cylinder touched the plug, we laid on the weights upon the board; and when by their weight they had depreſſed the plug till the cover of it leaned upon the cylinder, we diſpoſed ourſelves to attend the iſſue of the trial. The event whereof was this, that when the action of the frigorifick mixture had produced ſome ice in the water included in the bladder, that liquor appeared to have dilated itſelf ſtrongly enough to begin to raiſe the plug with the ſuper-incumbent weights, and by degrees they were, by the growing ice, raiſed, till the mark, diligently made on the plug, where the edge of the cylinder touched it, was about a tenth part of an inch above the ſtation it had before the plug had been depreſſed. Then we took out the bladder, and found the cylinder of water within the bladder not to be wholly turned into ice, but to contain ſome quantity of unfrozen water in the parts about the centre, which liquor, if we had not ſo ſoon deſiſted from the experiment (as for certain reaſons we did) might probably have raiſed the weights ſomewhat higher. But as it was, the ice in length was but three inches and about $\frac{1}{8}$, and yet ſo ſmall a quantity of ice ſufficed to raiſe,

raise, besides the board they leaned on, as many weights of lead as amounted to an hundred pounds avoirdupois.]

EXPERIMENT III.

[The day after the above-mentioned experiment was made, to try yet farther the expansive force of freezing water, the same was reiterated after the manner above delivered, but with this difference, that having procured more weight, when the plug was lifted up $\frac{3}{8}$, or somewhat better (which plug began sensibly to rise within half, or three quarters of an hour, after the frigorifick mixture was applied) it was loaded with a weight of two hundred pounds, and a fifteen pound-piece of lead, and other bodies, as boards, &c. to lay the weights upon, which being also weighed by themselves, came to fifteen pounds more, so that the whole amounted to two hundred and thirty pounds; and if the hundred pounds were both of them, as their bulk and shape invited us to guess, of that sort of weights which are called the great hundred, containing an hundred and twelve pounds a-piece, twenty-four pounds must be added to the sum, which would thereby be made up two hundred and fifty-four pounds.]

A NEW EXPERIMENT ABOUT THE PRODUCTION OF COLD By the CONFLICT of BODIES, appearing to make an EBULLITION.

AND now, that we are searching after the nature of cold, I am put in mind, that I have sometimes wondered at a certain experiment that is so anomalous, and seems so little of kin to the usual phænomena of cold, that though I do not particularly teach the way of making it, because I could not do it without discovering something in chymistry, that cogent considerations forbid me at present to publish; yet I cannot forbear to relate, on this occasion, the matter of fact, both because it may afford considerable hints to sagacious inquirers, and because it seems so little congruous to most theories of the causes of cold, that it may make the framers of theories more wary, and help also

to

to excuſe my backwardneſs to propoſe hypotheſes about cold in a reſolute and confident way.

THE experiment is this: we took three ſaline bodies, each of them purified by the fire; and whereas there are divers bodies, that being mingled together acquired a heat, which neither of them had apart; and whereas it is ſaid by ſome, that there are a few, which being blended together, make a mixture ſomewhat colder than either of themſelves, theſe ſalts of ours being put together in due proportion, do, upon their mixture, produce that, which the eye judges to be a great effervеſcence; but though the hiſſing noiſe be loud, and though the numerous bubbles ſuddenly generated will make the matter apt to overflow the glaſs, if the one be not capacious, and the other be not put in by little and little; yet even whilſt this ſeeming ebullition laſts, the glaſs, which one would expect to find very hot (as uſually happens upon the mixture of the ſalt of tartar, and ſpirit of nitre, and upon the confuſion of the like ſaline bodies diſpoſed to produce together ſuch efflоreſcencies) inſtead of growing hot, does, if it be held in one's hand, feel much cooler than before, and that in a wonderful degree; inſomuch, that even in winter the outſide of the glaſs would quickly be covered with great drops of dew, which after a while would unite, and trickle down by their own weight. And this we could make to laſt for a great while, by caſting in by degrees more and more of one of the ingredients on the other. And beſides that, this copious dew on the outſide of the glaſs, reached as high as the mixture within, which argued whence it proceeded; beſides that, purpoſely looking on the bottom of the glaſs, whoſe outſide was concave, we found no ſuch drops of dew there, becauſe the vapours of the external air could not, in any quantity, have acceſs to it; which ſhewed the dew, conſpicuous elſewhere, not to come from the tranſudation of the finer parts of the mixture through the pores of the glaſs; beſides theſe things, I ſay, I remember, that having ſometimes purpoſely wiped off the dew here and there with my handkerchief, the dry parts of the glaſs would in no long time regain freſh drops of dew. And this odd experiment we did for the main repeat, not only in the preſence of an induſtrious chymiſt (whoſe trials unexpectedly gave us the riſe of the experiment) but alſo alone, and at differing ſeaſons of the year.

I SHALL add, that having afterwards, about the middle of *November*, thought fit to vary a little, and repeat the experiment, becauſe I could then make uſe of a ſealed weather-glaſs, which I had not at hand when I made the former trials; I took two deep glaſſes, into the one of which I put a good quantity of fair water, and in the other I made ſuch a mixture, as I was lately mentioning; and having by a ſtring (to prevent the altering of the temper of the included air by the warmth of my fingers) let down the weather-glaſs into the water, that the liquor ſhut up in the inſtrument might be cooled by the ambient water; after it had ſtaid there a reaſonable time, I took it out, by the ſtring that was faſtened to the upper part of it, and letting it down into the mixture, that was then hiſſing, and filling the veſſel that contained it with multitudes of ſucceſſively emerging and haſtily vaniſhing bubbles; I perceived nevertheleſs, that the coldneſs of the ſeemingly effervеſcent mixture made the impriſoned tincted liquor to ſubſide ſo low, that from four inches and three quarters (or thereabout) at which height it ſtood in the carefully divided ſtem, when the weather glaſs was taken out of the water, it fell in a ſhort time lower than to one inch and an half. And becauſe I foreſaw that this might ſeem ſcarce credible, eſpecially if I ſhould relate how ſwiftly the impriſoned liquor ſubſided at the beginning; I ſhall annex, that, for farther ſatisfaction of others, I removed the thermometer out of the mixture into the common water again, where it ſoon reached to ſomewhat above four inches and a half; and not content with that, I put it a ſecond a second time into some of the frigefactive mixture before it had done foaming, in which it fell, as before, somewhat below an inch and a half, and, presently after, almost as low as to an inch. And having once more put it back into the glass that contained the water, the included liquor re-ascended to above four inches and a half, and this in an excellent sealed weather-glass, whose stem was not in all above ten inches long, with a ball proportionably big. And for farther confirmation, I took notice, that, whilst the mixture, by its hissing noise, and its strangely numerous bubbles, seemed to be in a state of ebullition, the outsides of the glass that contained it, were, as far as the mixture reached, so plentifully bedewed with the condensed vapours of the ambient air, that their weight carried them down in little streams, which left round about the bottom of the vessel a pretty quantity of liquor, that appeared by its taste not to have been made by the transudation of any of the sharp and saline liquors that were agitated within the glass. There remained only one scruple, which was suggested to me by the remembering of a circumstance, which, however, at the making of the fore-mentioned trials, I had not minded, and which possibly most observers would have neglected; but calling to mind that the water I had made use of to immerse the weather-glass in, was brought out of a room, wherein a fire was wont (though not constantly) to be kept, whereas the ingredients of the mixture were kept, and put together in a chamber, which, though contiguous to the former, had no chimney in it; I thought fit, for greater circumspection sake, to let the water stand all night in this last-mentioned chamber, that the ambient air might have the same operation upon it, as upon those bodies that were to be ingredients of the mixture; and then repeating the formerly-recited experiment, though I thought it needless to spend time to watch, as before I had done, the greatest difference in cold betwixt the water and the bubbling mixture; yet by making removes of the weather-glass to and fro, from one liquor to another, it sufficiently appeared, that the greater coldness, remarkable in the mixture, did not before proceed in any considerable degree (if in any degree at all) from the water's not having been kept in the same room with it.

So that by these different trials it seems manifest, that the coldness of the mixture was not a deception of the sensory, since it would be discovered by the operation it had, not only upon the vapours of the air on the outside of the glass, but upon the thermometer itself, placed in the midst of the mixture, which this last-named circumstance argues to have been cold throughout, and even in its innermost parts.

And to shew how much this strange coldness depended upon the peculiar texture of the mixture, or the structure of its component corpuscles, and the peculiar kind of motion that was excited in the tumultuating particles; I shall here subjoin a relation, which probably will not appear despicable; namely, that, in the first place, I took some of the acid liquor, the rest of which I had made use of to make the mixture, whereof I have been speaking; and put a convenient quantity of fair water, which had been kept a night or two in the same room (wherein was no chimney) with it, that there might be no cause of suspicion, that the one had been exposed to a more or less cold air than the other; and yet these two liquors did scarce sensibly differ in coldness; though to discover whether they did or no, I removed from one to another of them a good sealed weather-glass, with a very slender stem.

And, in the next place, I took a convenient quantity of the pure salt I had so often employed, and cast it into a glass full of water, which I had kept many hours in the same room with it, and wherein I had a little before placed a sealed weather glass, that the included liquor might be brought to the temper of the ambient liquor; but upon this injection, the tincted liquor of the thermoscope subsided so little, as not to make me look upon this salt as being itself extraordinarily cold, since other obvious salts (that I have at other times cast into water to cool it a little) and even sea-salt would (according to my estimate) have refrigerated it as much, if not more. Nor did I observe the glass wherein I was wont to keep store of our salt (though I had often occasion to handle it) disclose to the touch any remarkable degree of coldness; so that the coldness of our hissing mixture could not be attributed to that of either of the ingredients apart, but was a quality emerging upon their being blended. Now, when I thus made these preparatory trials, having afterwards placed in the same window (of the chamber last-mentioned) a couple of glasses, with common water in one, and in the other some of that mixture, of whose frigefactive power I had very recently made trial; I left them to stand there together all night, and left also standing by them such a sealed weather-glass as I have been mentioning; and the next morning, when all the visible commotion or agitation of the minute parts of the contrary salts of the mixture was quieted, I put the weather-glass, first, into one of those two liquors, and then into the other, and after removed it back into the former again, without perceiving any difference worth minding, betwixt the coldness of the mixture, and that of common water; and with much the like success I repeated the trial, after the water and the other liquor had stood in the same room (unfurnished with a chimney) for near two days and nights.

And for farther confirmation, I shall add, that having instead of the salt, which I hitherto made use of, taken some of the spirit that was wont to come over together with that salt, and did so abound with it, that a good deal of it lay undissolved at the bottom of the liquor; having, I say, employed this saline-spirit, instead of the salt itself, and having for trial's sake mixed with it another spirit, drawn in my own laboratory for the purpose, which to me seemed as like, as could be made, to that, which I had all this while made use of; I found, that the mixture of these two liquors, though it produced far fewer bubbles than I was wont to have, instead of growing cold, grew lukewarm, and quickly impelled the liquor in the weather-glass, from a little above three inches, to as much above eight; and yet, besides that this last spirit was, as far as I could perceive, and that after the same manner, drawn from the same materials with that I had used all this while; the smell and taste (which are both of them peculiar and odd enough) concurred to manifest the two spirits to be of the same kind.

And, for farther proof, I shall add, that to satisfy myself the more fully, I took a parcel of the same liquor, I had lately employed with success in making the frigorifick mixture; and yet even this liquor, which with the dry salt would questionless have produced a frigefactive mixture, as well as the rest had done, which I had a little before taken out of the same phial; this liquor, I say, put to a new portion of the saline spirit above-mentioned, though they did not produce minute bubbles numerous enough to make a foam; yet the mixture, instead of growing very cold, grew manifestly lukewarm, not only in the judgment of the touch, but by its operation on a good sealed weather-glass, carefully, and for a competent while employed to examine the temper of it. Whereas, on the contrary, having purposely kept some of the frigorifick spirit by the fire-side, till its temper was so altered, that it nimbly enough rarefied and impelled up the spirit of wine contained in a sealed weather-glass, immersed in it, and having into this liquor cast some of the frigorifick salt, even whilst the spirit of wine was rising, and would probably have risen a pretty while longer; this injected salt, when it began to be dissolved, did not only give a check to the rising liquor, and quickly put a stop to its ascent; but, as I expected, soon made it subside again, till it fell about three inches or more (which was very much in a short weather-glass) beneath the station where the spirit of wine had rested, before the liquor was set by the fire-side; nay, afterwards, I tried, that a frigorifick salt, being well warmed by the fire side, did, with an appropriated

liquor,

liquor, that was alſo warmed, produce a coldneſs manifeſtly perceivable by the weather-glaſs. So that in theſe caſes a body but moderately cold, nay, actually warm, haſtily reduced one, actually warm, or at leaſt tepid, to a far greater degree of actual coldneſs than itſelf had.

THESE are ſome of the experiments I tried with the liquors and ſalts, of which, upon allowable conſiderations, I muſt now forbear to ſet down the way of preparing; but, that even at preſent I may not be altogether wanting to the curious, I deviſed a way of making a ſuccedaneum to this experiment, which I ſhall here willingly annex, as that, which though it be much inferior to what I may one day be at liberty to acquaint the reader with; yet it will ſhew the main thing intended, by manifeſting, that cold may, by the mingling of bodies, be produced or increaſed to a degree exceeding that of either of the bodies that compoſed the mixture; and this, though at the ſame time a ſeeming effervescence be made by the bodies that thus refrigerate each other.

I TOOK then very good ſalt of tartar, and putting to it a convenient quantity of ſpirit of vinegar, I did, whilſt the mixture was hiſſing (but ſeemed to the touch to have refrigerated the glaſs that contained it (immerſe into it the ball of a good ſealed thermoſcope furniſhed with ſpirit of wine. And, though the weather-glaſs were not much above a foot long, yet the coldneſs of this mixture made the tincted liquor deſcend haſtily enough two inches and almoſt a half. And to ſhew farther, that this mixture was actually colder than cold water, removing the weather-glaſs out of the mixture into that liquor, the tincted ſpirit began to re-aſcend, and that ſo nimbly, that in about three minutes (that the ball of the thermoſcope ſtaid under water) the ſpirit of wine had re-aſcended about an inch and a half, if not more. And to try, whether this coldneſs of the mixture did proceed from or depend upon ſome texture of the parts that was not very permanent, and yet did not quite degenerate, immediately after the ingredients had ceaſed to work upon one another; I remember, that near an hour after the ebullition of the ſpirit and ſalt of tartar was over, the thermoſcope being removed out of the common water, where it had ſtood immerſed, into the mixture, deſcended about half an inch or more. For want of ſalt of tartar I could not begin the experiment anew, and ſo am not ſure it will always ſucceed uniformly *. But yet to give myſelf what further ſatisfaction I could, by trying the ſame experiment in ſuch a way as might diſcover whether or no the phænomenon did not depend upon, or require ſome peculiar texture in the fixed ſalt that had been employed; I took ſome alcali (made by diſſolving pot-aſhes in fair water, and reducing them by coagulation to a white ſalt) and pouring ſpirit of vinegar to it, I found, that this mixture did not, whilſt it hiſſed, grow at all colder, but rather ſomewhat warmer. And, for farther ſatisfaction, immerſing into it the ball of the newly-mentioned weather-glaſs, I found that it aſcended in a ſhort time about an inch, and, being removed into the water, deſcended about half an inch; and by making removes of it from one of theſe liquors into the other, two or three times more, I found, that the ſpirit of wine did riſe and fall according to what has been newly obſerved, but its motions upwards and downwards were both leſs than before, and more ſlow.

* The Author's warineſs was not here amiſs, he having afterwards found that this experiment did not always ſucceed.

OBSERVATIONS AND EXPERIMENTS ABOUT THE SALTNESS OF THE SEA.

The FIRST SECTION.

CHAP. I.

THE cause of the faltnefs of the fea appears, by *Ariftotle*'s writings, to have bufied the curiofity of naturalifts before his time; fince which, his authority, perhaps, much more than his reafons, did, for divers ages, make the fchools, and the generality of naturalifts, of his opinion, till towards the end of the laft century, and the beginning of ours, fome learned men took the boldnefs to queftion the common opinion; fince when the controverfy has been kept on foot, and, for aught I know, will be fo, as long as it is argued on both fides but by dialectical arguments, which may be probable on both fides, but are not convincing on either. Wherefore, I fhall here briefly deliver fome particulars about the faltnefs of the fea, obtained by my own trials, where I was able; and where I was not, by the beft relations I could procure, efpecially from navigators.

FIRST then, whereas the Peripateticks do, after their mafter *Ariftotle*, derive the faltnefs of the fea from the aduftion of the water by the fun-beams, it has not been found, that I know of, that where no falt, or faline body, has been diffolved in, or extracted by water expofed to the fun or other heat, there has been any fuch faltnefs produced in it, as to juftify the *Ariftotelian* opinion. This may be gathered, as to the operation of the fun, from the many lakes and ponds of frefh water to be met with, even in hot countries, where they lie expofed to the action of the fun. And as for other heats, having out of curiofity diftilled off common water in large glafs bodies and heads, till all the

the liquor was abſtracted, without finding, at the bottom, the two or three thouſandth part, by my gueſs, of ſalt, among a little white earthy ſubſtance that uſually remained. And though I had found a leſs inconſiderable quantity of ſalt, which, I doubt not, may be met with in ſome waters, I ſhould not have been apt to conclude it to have been generated out of the water, by the action of the fire, becauſe I have, by ſeveral trials purpoſely made, and elſewhere mentioned, found, that in many places (and I doubt not, but if I had farther tried, I ſhould have found the ſame in more) common water, before ever it be expoſed to the heat of the ſun or other fire, has in it an eaſily diſcoverable ſaltneſs of the nature of common ſalt, or ſea-ſalt, which two I am not here ſolicitous to diſtinguiſh, becauſe of the affinity of their natures, and, that in moſt places, the ſalt eaten at table, is but ſea-ſalt freed from its earthy and other heterogeneities, the abſence of which makes it more white than ſea-ſalt is wont to be with us. Theſe laſt words I add, becauſe credible navigators have informed me, that in ſome countries, ſea-ſalt, without any preparation, coagulates very white; of which ſalt I have had (from divers parts) and uſed ſome parcels.

But ſome of the champions of *Ariſtotle*'s opinion are ſo bold, as to alledge experience for it, vouching the teſtimony of *Scaliger* to prove that the ſea taſtes ſalter at the top than at the bottom, where the water is affirmed to be freſh. But as for the authority of *Scaliger*, though I take him to be an acute writer, yet I confeſs, that, for reaſons elſewhere given, I do not allow it that veneration, which I find given it by very learned men; nor am I over prone, even as to matters of fact, to acquieſce in what he tells us, when he neither ſignifies that he delivers things upon his own experience, or declares from what credible information from others he received them.

It is true, that having often obſerved that ſea-ſalt diſſolved in water is, upon the receſs of the ſuperfluous liquor, wont to begin its concretion, not as moſt other ſalts do, at either the lateral or lower parts of the veſſel, but at the top of the water, I will not think it impoſſible, that ſometimes in very hot climates, or weather, the ſea may taſte more ſalt at the top, than at ſome diſtance beneath it. But conſidering how great a proportion of the ſalt common water is wont to be impregnated with, before it ſuffers ſaline concretions to begin, and how far ſhort of that proportion the ſalt contained in the ſea-water is wont to be, inſomuch, that about *Holland*, a Dutch geographer or two have not found it to amount to the proportion of one to forty; and I in *England* found it to be no more than I ſhall hereafter ſpecify; it ſeems not unlikely, that *Scaliger*'s * obſervation was well made, and it muſt be very unlikely that it ſhould generally hold, if the ſaltneſs of the ſuperficial parts of the ſea be compared with that of the lower parts of it.

And yet I do not build my opinion wholly upon this argument of ſome modern philoſophers, that ſalt being a heavier body than water, muſt neceſſarily communicate moſt ſaltneſs to the loweſt parts.

For though this argument be a probable one, yet water being a fluid body, the reſtleſs agitation of whoſe corpuſcles makes them, and the corpuſcles they carry with them, perpetually ſhift places, whereby the ſame parts come to be ſometimes at the top, and ſometimes at the bottom; this conſideration, together with what was lately noted of the peculiar diſpoſition of diſſolved ſea-ſalt, to begin its coagulation upon the ſurface of the water, may make the argument, we are conſidering, ſuſpected not to be ſo cogent, as at firſt ſight one may think it. Which ſuſpicion I might ſomewhat countenance by ſubjoining, that in divers metals, and other tincted ſolutions, I have not uſually obſerved the upper part of the liquor to be manifeſtly deeper coloured than the lower; though,

* See the third Section towards the latter end.

between

between metalline bodies and their menstruums, the disproportion of specifick gravity does usually much exceed that, which I have met with, between sea-salt and common water.

CHAP. II.

It is urged out of *Linschotten* by a learned modern writer, that wanting fresh water near *Goa* (the Metropolis of the Portugals in the *East-Indies)* they make their slaves fetch it, by diving, from the bottom of the sea; which seems a clear evincement of the Peripatetick opinion. But, in this observation, I cannot acquiesce, for two reasons; the one, because, that though what is alledged, as matter of fact, were strictly true, yet so general a conclusion could not be safely drawn from that particular instance, since in other parts of the sea, the contrary has been found by experience, as I shall shew ere long. And other reasons than those given by the Peripateticks may be rendered of what happens at *Goa*, which reasons may extend to the like cases, if elsewhere they shall happen to be met with. For it may very well be, that springs of fresh water may arise in some parts of the surface of the earth that are covered with the sea, as they do in innumerable vallies and other places of the terrestrial surface that is not so covered; not to mention those springs that appear in divers places upon a low ebb, covered with the sea during the flood. The curious Hungarian † governor that gives us an account of the wonderful waters that ennoble his country, relates, that in the river *Vagus*, that runs by the fortress *Galgotium*, the veins of hot water spring up in the bottom of the river itself. *Neque in ripa tantùm*, says he, *eruuntur calidæ*, *sed etiam intra amnem*, *si fundum ejus pedibus suffodias*; *calent autem immodicè*, &c. Nay, I have been assured by more than our learned eye-witness, that there is a place upon the Neapolitan coast, where they (and, I think, a writer or two of those parts) observed the water to spring up hot beneath the surface of the sea, insomuch, that one of my relators thrusting in his hand and arm somewhat deeper than was convenient, found there an offensive degree of heat.

Besides (which is my second conjecture) as to the particular case of *Goa*, I had the curiosity to enquire of a great traveller, and a man of letters, that lived in that city and the neighbouring places, and gave me a pertinent account of them, especially of that place, whence the fresh water is fetched by the divers, which his curiosity led him to visit, and take special notice of; but I found by him, that the divers do not now think it needful to fetch their fresh water so low as from the bottom of the sea, and that, by the little depth, whence his and other men's curiosity caused it to be taken up, he judged it did not so much come from any fresh water springs rising at the bottom of the sea, as from a small river (whose name I do not remember) that not far from thence runs into the sea, with such a juncture of circumstances, that at the mentioned places, the fresh water does yet keep itself tolerably distinct, and is not yet so far made brackish, as not to continue potable, though not very good. Which conjecture of his I could make probable, by what I have had from eminent and observing men among our own navigators, touching the sliding of waters one over another, in some parts of the sea, especially near the mouths of rivers. But the discussion of this matter, and the particulars of the account given me of the situation of the place where water is dived for near *Goa*, would require more words than they would in this place deserve, unless the point under debate were more important to our present purpose.

† De admirandis Hungariæ Aquis.

I might

I MIGHT here pretend to a clear demonſtration, by experience of the contrary of what *Scaliger* delivers, by vouching the teſtimony of the learned *Patricius*, who affirms, that being upon the ſea, which takes its denomination from the iſland of *Crete* (now *Candia*) he did, in the company of a Venetian magiſtrate, *Mocinigo*, let down a veſſel (furniſhed with a weight to ſink it) to the bottom of the ſea, where, by the help of a contrivance, it was unſtopped, and filled with water there, which being drawn up, was found to be not freſh, but ſalt. This experiment, I ſay, I could oppoſe as a demonſtration againſt *Scaliger*; but though it be a very probable argument, and more conſiderable than any I have ſeen brought by the Peripateticks for their opinion, yet, I confeſs, it would be more ſatisfactory to me, if it would not permit me to ſuſpect, that in the drawing up of the veſſel through the ſalt water, though there had been freſh water taken in at the bottom, the taſte may have been altered by the ſubingreſſion of ſalt water, which being, bulk for bulk, heavier than freſh, would by its ponderouſneſs endeavour to ſink into the aſcending veſſel, and thereby more eaſily expel part of the freſh water, and mingle with the reſt. Wherefore, I ſhall confirm the ſaltneſs of the ſea at the bottom by ſome obſervations, that are not liable to the ſame objections as that of *Patricius*.

THE firſt is that of the perſon, whom I elſewhere mention, to be able, by help of an engine, to ſtay a conſiderable time at the bottom of the ſea; for of him I learned, among other things, that I deſired to be informed of touching that place, that he found the water to have as ſalt a taſte there as at the top.

THE next obſervation I obtained by means of a great traveller into the *Eaſt* and *Weſt-Indies*, who having had the curioſity to viſit the famous pearl-fiſhing at *Manar*, near the great *Cape* of *Comori*, anſwered me, that he had the ſame curioſity that I expreſſed to learn of the divers, whether they found the water ſalt at the bottom of the ſea, whence they fetch their pearl-fiſhes? and that he was aſſured by them that it was ſo; and the ſame perſon being aſked by me about the ſaltneſs of the ſea in a certain place under the Torrid Zone, which the relation of a traveller inclined me to think to abound extraordinarily with ſalt, affirmed to me, that not only the divers aſſured him, that the ſea was there exceeding ſalt at the bottom, but brought up ſeveral hard lumps of ſalt from thence, whereof the fiſhermen and others were wont to make uſe of to ſeaſon their meat, as he himſelf alſo did; which yet I may aſcribe not only to the plenty of ſalt already diſſolved in the water, but to the greater indiſpoſition, that ſome ſorts of ſalts, whereof this may be one, have to be diſſolved in that liquor.

To theſe I ſhall add this third obſervation: meeting with an inquiſitive engineer that had frequented the ſea, and had ſeveral opportunities to make obſervations of other kinds in deep waters, I deſired him that he would take along with him a certain copper veſſel of mine, furniſhed with two valves opening upwards, and let it down for me the next time he went to ſea; on which occaſion he told me, that, if I pleaſed, I might ſave myſelf the trouble of the intended trial, for, with a tin veſſel, very little differing from that I deſcribed unto him, he had had the curioſity, near the ſtraight of *Gibraltar*'s mouth (where he had occaſion to ſtay a good while) to fetch up ſea-water from the depth of about forty fathoms, and found it to be as ſalt in taſte as the water near the ſurface.

THESE obſervations may ſuffice to ſhew that the ſea is ſalt at the bottom in thoſe places where they were made; but yet I thought it was not fit for me to acquieſce in them, but rather endeavour to ſatisfy myſelf, by the beſt trial I could procure to be made, with my copper veſſel (as more ſtrong and fit than a tin one) what ſaltneſs is to be found in the water at the bottom of our ſeas, not only, becauſe it may more concern us to know, that, but chiefly, becauſe, though I deny not, that in the foregoing obſervations the

the taste may sufficiently prove, that the sea is salt at the bottom as well as the top, yet I thought the taste, by reason of the predispositions and other unheeded affections it is liable unto, no certain way to judge, whether the top and the bottom be as salt one as the other. Wherefore, I thought it would be more satisfactory to examine the sea-water by weight, than by taste; and in order thereunto, having delivered the above-mentioned instrument to the engineer I lately spake of, when he was going to sea, he sent me, together with it, a couple of bottles of sea-water, taken up, the one at the top, and the other at the bottom, at fifteen fathoms deep. The colour and smell of these two waters were somewhat differing; but when I examined them hydrostatically, by weighing a roll of brimstone first in one, and then in the other, I scarce found any sensible difference at all in their specifick gravities. So that if the degree of the saltness of sea-water may be safely determined by its greater or lesser weight, then so far forth as this single experiment informed me, the saltness is equal at the top and bottom of the sea; I said, if the degree, &c. because of what I shall hereafter take notice of about salts of less specifick gravity than sea-salt.

C H A P. III.

It follows now that I make out what I formerly intimated, that though it were granted, that near *Goa*, and perhaps in some other places, the divers may have found the water fresh at the bottom of the sea, it would not therefore necessarily follow, that the sea-water, generally speaking, is fresh at the bottom; for the observations lately mentioned sufficiently manifest the contrary; and as to those very few places (if really there have been any) where the sea-water has been found fresh at the very bottom, I think one may ascribe the taste of the water to the bubbling up of springs of fresh water at or near enough to those very places. I know this may appear a paradox, since it may seem altogether unlikely, that so small a stream of water, as can be afforded by a spring, should be able to force its way up in spite of the resistance of so vast a weight as that of the super-incumbent sea-water, especially since this liquor, by reason of its saltness, is heavier in specie than fresh water.

But this objection needs not oblige me to forsake my conjecture; for whatever most men believe, and even learned men have taught, to the contrary, it matters not how great the quantity of liquor be, which is laterally higher than the lower orifice of the pipe, or channel, that gives passage to the liquor that is to be impelled up into it; provided the upper surface of the liquor in the channel or pipe have a sufficient perpendicular height in reference to that of the stagnant water; for no more of all this fluid will hinder its ascent than the weight of such a pillar of the said fluid, as is directly super-incumbent on it. * *Stevinus*, and I, have, by differing ways, particularly proved, that, according to the laws of the true hydrostaticks, the prevalency of the two liquors that press against each other, is not to be determined according to the quantity of them, but to be adjudged to that which exceeds the other in perpendicular height; so that, considering the channel wherein a spring runs into the sea, as a long and inverted siphon, if that part of the either neighbouring, or more distant shore, whence the spring, or river, takes its course, be a neighbouring hill, or rock, or any other place considerably higher than that part of the bottom of the sea, or of the shore covered with the surface of the sea, at

* Vid. *Stevinum*, Prop. 10. Lib. IV. Staticcs. And see the Author's Hydrostatical Paradoxes.

which

which the channel which conveys freſh water terminates, that liquor will iſſue out in ſpite of the reſiſtance of the ocean.

To illuſtrate at once, and prove this paradox, I thought upon the following experiment. I took a veſſel of a convenient depth, and a ſiphon of a proportionable length, both of them of glaſs, that their tranſparency might permit us to ſee all that paſſed within them. Into the larger veſſel we put a quantity of ſea-water, and into the longer leg of the ſiphon, which had been for that purpoſe inverted, we poured a convenient quantity of freſh water, which we kept from running out at the ſhorter leg, by ſtopping the orifice of the longer with the thumb or finger; then this ſiphon being ſo placed in the greater veſſel, that the orifice of the ſhorter leg was a great deal beneath the ſurface of the ſalt water, and the ſuperficies of the freſh water in the longer leg was a pretty deal higher than that of the ſurrounding ſalt water, we unſtopped the orifice of the upper leg, whereby the water in the ſiphon, tending to reduce itſelf to an æquilibrium or equality of height, in both legs, the water in the upper leg being much higher and heavier than that in the other, did, by ſubſiding, drive away the water in the ſhorter leg, and make it ſpring out at the orifice of the ſhorter leg, in ſpite of the breadth and ſpecifick gravity of the ſalt water. And this impelling upwards of the freſh water laſted as long as the ſurface of that water, in the longer leg, retained its due height above that of the ſurrounding ſea-water; which circumſtance I expreſsly mention, becauſe there being a difference amounting to between a fortieth and fiftieth part, betwixt the ſpecifick gravity of our ſea-water and common freſh water, by reaſon of the ſalt, which makes the former the heavier, the freſh water in the longer leg of the ſiphon ought to be between a fortieth and fiftieth part higher than the ſurface of the ſea-water, to maintain the æquilibrium betwixt theſe two liquors.

To make the forementioned experiment the more viſible, I thought fit to perform it with freſh water tinged with braſil or logwood; but that it might not be objected, that thereby the ſpecifick gravity of the liquor would be altered or increaſed, I afterwards choſe to make it with claret-wine, which being a liquor lighter than common water, and of a conſpicuous colour, is very convenient for our purpoſe.

And when I made this trial, by placing the orifice of the ſhorter leg at a convenient diſtance below the ſurface of the ſea-water, it was not unpleaſant to obſerve, how, upon the removal of the finger that ſtopped the orifice of the longer leg, the quick deſcent of the wine contained in that leg impelled the coloured liquor in the ſhorter leg, and made it ſpring up, at its orifice, into the incumbent ſea-water, in the form of little red clouds, and ſometimes of very ſlender ſtreams. And as this ſhorter leg of the ſiphon was raiſed more and more towards the ſurface of the water, ſo there iſſued out more and more wine at the orifice of it; the liquor in the longer leg proportionably ſubſiding, but yet continuing manifeſtly higher than the ſurface of the ſalt water, than which it was in ſpecie much lighter.

¶ But here I muſt give an advertiſement to prevent a miſtake; for if the ſiphon be not exceeding ſlender, after the wine in the longer leg is fallen down to its due ſtation, a heedful obſerver may perceive, after a while, that though the ſiphon be kept in the ſame place, there will iſſue out of the ſhorter leg a little red ſtream, which proceeds not from the former impulſe of the wine in the longer leg, but from the ingreſs of the ſea-water, which being much heavier in ſpecie than wine, ſinks into the cavity of the ſiphon, and as it comes in on one ſide, thruſts up as much wine on the other ſide of the ſame cavity. But the red liquor that aſcends on this account may be diſcerned to do ſo, by its riſing more ſlowly, and after another manner than that which is impelled up by the ſudden fall of the tall cylinder of wine in the longer leg.

THE SECOND SECTION.

CHAP. I.

AS to the cause of the saltness of the sea, I therein agree with the learned *Gassendus* and some other modern writers, that the sea derives its saltness from the salt that is dissolved in it; but I take that saltness to be supplied, not only from rocks and other masses of salt, which at the beginning were, or in some places may yet be found, either at the bottom of the sea, or at the sides, where the water can reach them, but also (to say nothing here of what may, perhaps, be contributed by subterraneal steams) from the salt, which the rains, rivers, and other waters dissolve in their passage through divers parts of the earth, and at length carry along with them into the sea. For not only it is manifest enough that several countries afford divers salt springs, and other running waters, that at length terminate their course in the sea; but I have sometimes suspected, that very frequently the earth itself is impregnated with corpuscles, or, at least, rudiments of common salt, though no such thing be vulgarly taken notice of. Which suspicion may be confirmed (to omit what I have elsewhere delivered on another occasion) partly by the observation of some eminent chymists, who affirm themselves to have found a not inconsiderable quantity of exceeding saline liquor upon the evaporation of large quantities of some waters (for in some others I could not find it) and principally by the quantity of common salt that is usually found in the refining of saltpetre; though that be a salt, which Sir *Francis Bacon*, and other experienced writers teach, that almost every fat earth, kept from the sun and rain, and from spending itself in vegetation, will afford.

BUT having, on another occasion, sufficiently shewed †, that the earth does abound with common salt, in many more places than are wont to be taken notice of; and that it is probable, that by maturation, or otherwise, salt may daily grow in the earth, it will not be necessary to add, in this place, any thing to what I have said already, to prove that our common terrestrial salt, being dissolved, may suffice to make the sea-water brackish; and the rather, if we call to mind what has been formerly said about the possibility of springs rising beneath the surface of the sea, and of lumps of salt that were taken up by divers, undissolved, at the bottom of the sea; the ocean may receive supplies of salt from rocks and springs latent in its own bosom, and unseen even by philosophers. And this may be one reason, I conceive (for I deny not but that there may be others, as the very unequal heat of the sun, &c.) why some seas are so much salter than others, or, at least, why in some places the sea-water may be much salter than in others.

AND as we have seen that our common terrestrial salt may be copiously enough communicated to the sea, to impregnate it with as much saltness as we observe it to have; so I do not see that the difference between that salt and sea-salt is so great, but that it may well be supposed to be derived from those changes that the terrestrial salt may be liable to, when it comes into the sea. For that the marine salt, and the terrestrial do very well agree in the main things, may be argued from the resemblance both in shape, taste, &c. that may be observed between the grains that will be produced, if we expose each of them in a distinct glass to such a heat, as may slowly carry off the superfluous moisture, and suffer them to coagulate into cubical, or almost cubical grains; and the lesser differences that may be met with between these two salts, may well enough be

† In a Tract of Subterraneal Menstruums.

ſuppoſed producible by the plenty of nitrous, urinous, and other ſaline, to which, in ſome places, may be added bituminous bodies, that by land-floods, and otherwiſe, are from time to time carried into the ſea, and by ſeveral things that happen to it there, eſpecially by the various agitation it is put into by tides, winds, currents, &c. and (which I would by no means omit) by its being in vaſt quantities expoſed to the ſun and air.

C H A P. II.

We may juſtly be the more careful to determine whether the ſaltneſs of the ſea-water proceed from common ſalt diſſolved in it, becauſe if it appeared to be ſo, we might the more hopefully attempt to obtain by diſtillation ſweet water from ſea-water; ſince, if this liquor be made by the bare diſſolution of common ſalt in the other, it is probable that a ſeparation may be made of them, by ſuch a heat, as will eaſily raiſe the aqueous parts of ſea-water, without raiſing the ſaline, whoſe diſtillation requires a vehement heat, as chymiſts well know to their coſt. And ſuch a method of ſeparating freſh water from that which was ſalt, would make our doctrine of uſe, and be very beneficial to navigation, and conſequently to mankind. For, in long voyages, it is but too common for the makers of them, to be liable to hazards and inconveniencies, for want of freſh and ſweet water, whereby they are ſometimes forced to drink corrupt brackiſh water, which gives them divers diſeaſes, as particularly the ſcurvy, and the uſual effect of drinking ſalt water, the dropſy. And ſeamen are wont to receive ſo many other incommodities by the want of freſh water, that, to prevent or ſupply it, they are oftentimes forced to change their courſe, and ſail ſome hundreds of miles to a coaſt, not only out of their way, but unſafe in itſelf, and perhaps more dangerous, by being infeſted by pirates, or in the hands of enemies or ſavage people; by which means, they often loſe the benefit of their Monſoons, and much more eaſily other winds, and frequently their voyage. And theſe are inconveniencies which might be in good meaſure prevented, if potable, and at leaſt tolerably wholeſome water could be obtained by diſtillation, in the midſt of the ſea itſelf, to ſerve the ſeamen, till they could be ſupplied with naturally freſh water. To make ſome trials of this, I remember I took ſome Engliſh ſea-water, whence I was able to ſeparate betwixt a thirtieth and fortieth part of dry ſalt; and having diſtilled it in a glaſs head and body, with a moderate fire, till a conſiderable portion of it was drawn over, we could not diſcern any ſaltneſs in it by the taſte; and beſides that I found it ſpecifically lighter than ſuch water as is daily drank by perſons of quality at *London*, I expoſed it to more chymical examen, and did not by that find any thing of ſea-ſalt in it, though I have at ſeveral times, by the ſame way, manifeſtly diſcovered a ſaltneſs in inland waters that are drank obviouſly for ſweet waters. If I would have employed a ſtronger heat, and veſſels larger and lower, or otherwiſe better contrived for copious diſtillation, I might in a ſhorter time have obtained much more diſtilled water; but whether ſuch liquors will be altogether ſo wholeſome, experience muſt determine. Yet that ſea-water diſtilled even in no very artificial way, may be ſo far wholeſome, as not in haſte to be ſenſibly noxious, but at a pinch uſeful, at leaſt for a while, may be gathered from (what occurs to me ſince the writing of the laſt paper) the teſtimony of that famous navigator Sir *R. Hawkins*, who commanded a fleet in the *Indies* for Queen *Elizabeth*. For he, in the judicious account he gave the world of his voyage, wherein

THE SECOND SECTION.

CHAP. I.

AS to the cauſe of the ſaltneſs of the ſea, I therein agree with the learned *Gaſſendus* and ſome other modern writers, that the ſea derives its ſaltneſs from the ſalt that is diſſolved in it; but I take that ſaltneſs to be ſupplied, not only from rocks and other maſſes of ſalt, which at the beginning were, or in ſome places may yet be found, either at the bottom of the ſea, or at the ſides, where the water can reach them, but alſo (to ſay nothing here of what may, perhaps, be contributed by ſubterraneal ſteams) from the ſalt, which the rains, rivers, and other waters diſſolve in their paſſage through divers parts of the earth, and at length carry along with them into the ſea. For not only it is manifeſt enough that ſeveral countries afford divers ſalt ſprings, and other running waters, that at length terminate their courſe in the ſea; but I have ſometimes ſuſpected, that very frequently the earth itſelf is impregnated with corpuſcles, or, at leaſt, rudiments of common ſalt, though no ſuch thing be vulgarly taken notice of. Which ſuſpicion may be confirmed (to omit what I have elſewhere delivered on another occaſion) partly by the obſervation of ſome eminent chymiſts, who affirm themſelves to have found a not inconſiderable quantity of exceeding ſaline liquor upon the evaporation of large quantities of ſome waters (for in ſome others I could not find it) and principally by the quantity of common ſalt that is uſually found in the refining of ſaltpetre; though that be a ſalt, which Sir *Francis Bacon*, and other experienced writers teach, that almoſt every fat earth, kept from the ſun and rain, and from ſpending itſelf in vegetation, will afford.

BUT having, on another occaſion, ſufficiently ſhewed †, that the earth does abound with common ſalt, in many more places than are wont to be taken notice of; and that it is probable, that by maturation, or otherwiſe, ſalt may daily grow in the earth, it will not be neceſſary to add, in this place, any thing to what I have ſaid already, to prove that our common terreſtrial ſalt, being diſſolved, may ſuffice to make the ſea-water brackiſh; and the rather, if we call to mind what has been formerly ſaid about the poſſibility of ſprings riſing beneath the ſurface of the ſea, and of lumps of ſalt that were taken up by divers, undiſſolved, at the bottom of the ſea; the ocean may receive ſupplies of ſalt from rocks and ſprings latent in its own boſom, and unſeen even by philoſophers. And this may be one reaſon, I conceive (for I deny not but that there may be others, as the very unequal heat of the ſun, &c.) why ſome ſeas are ſo much ſalter than others, or, at leaſt, why in ſome places the ſea-water may be much ſalter than in others.

AND as we have ſeen that our common terreſtrial ſalt may be copiouſly enough communicated to the ſea, to impregnate it with as much ſaltneſs as we obſerve it to have; ſo I do not ſee that the difference between that ſalt and ſea-ſalt is ſo great, but that it may well be ſuppoſed to be derived from thoſe changes that the terreſtrial ſalt may be liable to, when it comes into the ſea. For that the marine ſalt, and the terreſtrial do very well agree in the main things, may be argued from the reſemblance both in ſhape, taſte, &c. that may be obſerved between the grains that will be produced, if we expoſe each of them in a diſtinct glaſs to ſuch a heat, as may ſlowly carry off the ſuperfluous moiſture, and ſuffer them to coagulate into cubical, or almoſt cubical grains; and the leſſer differences that may be met with between theſe two ſalts, may well enough be

† In a Tract of Subterraneal Menſtruums.

ſuppoſed

ſuppoſed producible by the plenty of nitrous, urinous, and other ſaline, to which, in ſome places, may be added bituminous bodies, that by land-floods, and otherwiſe, are from time to time carried into the ſea, and by ſeveral things that happen to it there, eſpecially by the various agitation it is put into by tides, winds, currents, &c. and (which I would by no means omit) by its being in vaſt quantities expoſed to the ſun and air.

C H A P. II.

We may juſtly be the more careful to determine whether the ſaltneſs of the ſea-water proceed from common ſalt diſſolved in it, becauſe if it appeared to be ſo, we might the more hopefully attempt to obtain by diſtillation ſweet water from ſea-water; ſince, if this liquor be made by the bare diſſolution of common ſalt in the other, it is probable that a ſeparation may be made of them, by ſuch a heat, as will eaſily raiſe the aqueous parts of ſea-water, without raiſing the ſaline, whoſe diſtillation requires a vehement heat, as chymiſts well know to their coſt. And ſuch a method of ſeparating freſh water from that which was ſalt, would make our doctrine of uſe, and be very beneficial to navigation, and conſequently to mankind. For, in long voyages, it is but too common for the makers of them, to be liable to hazards and inconveniencies, for want of freſh and ſweet water, whereby they are ſometimes forced to drink corrupt brackiſh water, which gives them divers diſeaſes, as particularly the ſcurvy, and the uſual effect of drinking ſalt water, the dropſy. And ſeamen are wont to receive ſo many other incommodities by the want of freſh water, that, to prevent or ſupply it, they are oftentimes forced to change their courſe, and ſail ſome hundreds of miles to a coaſt, not only out of their way, but unſafe in itſelf, and perhaps more dangerous, by being infeſted by pirates, or in the hands of enemies or ſavage people; by which means, they often loſe the benefit of their Monſoons, and much more eaſily other winds, and frequently their voyage. And theſe are inconveniencies which might be in good meaſure prevented, if potable, and at leaſt tolerably wholeſome water could be obtained by diſtillation, in the midſt of the ſea itſelf, to ſerve the ſeamen, till they could be ſupplied with naturally freſh water. To make ſome trials of this, I remember I took ſome Engliſh ſea-water, whence I was able to ſeparate betwixt a thirtieth and fortieth part of dry ſalt; and having diſtilled it in a glaſs head and body, with a moderate fire, till a conſiderable portion of it was drawn over, we could not diſcern any ſaltneſs in it by the taſte; and beſides that I found it ſpecifically lighter than ſuch water as is daily drank by perſons of quality at *London*, I expoſed it to more chymical examen, and did not by that find any thing of ſea-ſalt in it, though I have at ſeveral times, by the ſame way, manifeſtly diſcovered a ſaltneſs in inland waters that are drank obviouſly for ſweet waters. If I would have employed a ſtronger heat, and veſſels larger and lower, or otherwiſe better contrived for copious diſtillation, I might in a ſhorter time have obtained much more diſtilled water; but whether ſuch liquors will be altogether ſo wholeſome, experience muſt determine. Yet that ſea-water diſtilled even in no very artificial way, may be ſo far wholeſome, as not in haſte to be ſenſibly noxious, but at a pinch uſeful, at leaſt for a while, may be gathered from (what occurs to me ſince the writing of the laſt paper) the teſtimony of that famous navigator Sir *R. Hawkins*, who commanded a fleet in the *Indies* for Queen *Elizabeth*. For he, in the judicious account he gave the world of his voyage, wherein they were distressed, even in the admiral's ship, for want of fresh water, has this memorable passage, as I find it verbatim in our diligent *Purchas* *.

"ALTHOUGH our fresh water had failed us many days (before we saw the shore) by "reason of our long navigation without touching any land, and the excessive drinking "of the sick and diseased (which could not be excused); yet with an invention I had "in my ship, I easily drew out of the water of the sea sufficient quantity of fresh water, "to sustain my people, with little expence of fuel; for with four billets I stilled a "hogshead of water, and therewith dressed meat for the sick and whole. The water so "distilled we found to be wholesome and nourishing."

AND because the potableness of sea-water may concern the healths and lives of men, I shall here add to what I elsewhere deliver about my ways of examining, whether other waters participate of salt, two or three observations I made upon those few distilled liquors I had occasion to draw from sea-water. Having then, upon some of the distilled liquor, dropped a little oil of tartar per deliquium, I perceived no clouds at all, or precipitation to be made; whereas a small proportion of that liquor being dropped into the undistilled sea-water itself, it would presently trouble and make it opacous, and, though but slowly, strike down a considerable deal of a whitish substance (which, of what nature it is, I need not here declare); I found also, that a very small proportion of an urinous spirit, such as that of sal armoniac, would produce a whitish and curled substance (but not near so copious a one as the other liquor) in sea-water, not yet exposed to distillation, but not in the liquor drawn from it; which argued, that there were but few or no saline particles of sea-salt ascended with the water; for else these alkalizate and urinous salts would in all likelihood have found them out, and had a visible operation on them. And I farther remember, that when the distillation was made in glass vessels, with an easy fire, not only the first running, but the liquor that came over afterwards, was not perceived to be brackish, but good and potable. To which agrees very well, that by a hydrostatical trial I found our distilled sea-water to be lighter in specie than common conduit-water, though it exceeded that in specifick levity, less than it was surpassed in the same quality by distilled rain-water.

BUT to return to the subject, whence we have somewhat, but, I hope, not uselesly digressed; I know it may be objected, that if the terrestrial salts carried by springs, rivers, and land floods into the sea, were the cause of its saline taste, those waters themselves must be made salt by it, before they arrive at the sea. But besides that this objection will not reach the springs and rivers of salt water, that in several places, either immediately or mediately, discharge themselves into the sea; it might conclude against him, that should affirm this imported saltness to be the only cause of that of the sea; but it will not be of force against me, who take it to be only a partial cause, that by its accession contributes to the degree of saltness we observe in the sea, where this imported salt may join itself with the salt it finds there already, and being detained by it contribute to the brinyness of the water.

IF it be urged, that from hence it will follow, that the sea from time to time increases in saltness, I may suspend my answer till it appear by competent observation, that it does not; which, I think, men have not yet made trials that may warrant them to assert. And if the matter of fact were certain, I think it were possible to give a farther answer, and shew probable ways, how so small an accession of salt may be dispersed by nature, and kept from increasing too much.

* In lib. VII. p. 1378 of *Purchas*, out of Sir *R. Hawkins*'s voyage.

CHAP.

C H A P. III.

But now it is ſeaſonable to conſider, that the taſte of ſea-water is not ſuch a ſimple ſaline taſte as ſpring-water would receive from ſal gem, or ſome other pure terreſtrial ſalt diſſolved in it, but a bitteriſh taſte, that muſt be derived from ſome peculiar cauſe, that authors are not wont to take notice of. I am not aſſured by any obſervations of my own, that this receſſion, from a purely ſaline taſte, is likely to be of the very ſame kind, and to be equally, or very near equally met with in all ſeas (not to add a doubt, whether it be at all ſenſible in ſome). The cauſe both of the bitterneſs, and ſaltneſs too, of the ſea-water, is ſaid to be affirmed by the learned Mr. *Lidyat*, to be aduſt and bituminous exhalations aſcending out of the earth into the ſea. But that there is abundance of actual ſalt in the ſea-water, to give it its ſaline taſte and ponderouſneſs, the ſalt that the ſun does in many places copiouſly ſeparate from the ſaltleſs wateriſh parts, ſufficiently manifeſts. But as to the bitteriſh taſte, I think it no eaſy matter to give a true account of it, but am prone to aſcribe it, partly, to the operation of ſome catholick agents upon that vaſt body of the ocean, and partly, to the alteration that the ſalt receives from the mixture of ſome other things, among which bitumen may be one of the principal.

But though I have, in another * paper ſhewn, that in ſome places of the ſea there are conſiderable quantities of bitumen, or bituminous matter to be met with; yet I dare not derive the bitterneſs of the ſea only from bituminous exhalations, but in good part, at leaſt in ſome places, from the liquid and other bitumen that is imported by ſprings and other waters into the ſea; of which we have an eminent inſtance in that which our Engliſh call *Barbadoes* tar, according to the relation I had of it from an inquiſitive gentleman, who is one of the chief planters of the iſland, and took pleaſure to obſerve this liquid bitumen to be carried in conſiderable quantities from the rocks into the ſea; and I think it poſſible enough, that ſome of the ſprings that riſe under the ſurface of the ſea, may carry up with them bituminous matter, which may help to make the ſaltneſs of the ſea degenerate (of which more perhaps elſewhere) as I not long ſince made mention of ſprings, as well of hot, as cold water, riſing beneath the ſurface of the ſea. And this minds me to intimate here, that I have ſuſpected, that in ſome places the ſulphureous exhalations, and other emiſſions from the ſubmarine parts of the earth, may ſometimes contribute to change the ſaline taſte of the ſea-water; for I have elſewhere related, how, not only ſulphurous ſteams, but ſometimes actual flames, have broken through from the lower parts of the ſea to the uppermoſt; and have ſometimes taken pleaſure to make, by art, a rude imitation of that phænomenon. And partly ſome experiments of my own, and partly ſome other inducements, have perſuaded me, that divers times (for I do not ſay always) ſea-ſalt does not obſcurely participate of combuſtible ſulphur, of which I may ſpeak farther on another occaſion. But in regard, that the taſte of the ſea-water is not in all parts of the ocean uniform, it may here ſuffice to take notice, in general, that this difference of taſte may partly be cauſed by adventitious bodies of ſeveral kinds, of which it is probable, that in differing places the ſea-water does variouſly partake. And not to mention here the fragrant ſmell of violets, which has, by ſeveral, and particularly by an eminent perſon, of whom I enquired about it, been obſerved, in ſome hot countries, to proceed from ſea-ſalt; I have divers other inducements to think that it is uſually no ſimple ſalt, nor free from mixture. For by more ways than one, and particularly by

* In the Tract of Subterraneal Menſtruums.

cohobating

cohobating from it its own ſpirit, we have obtained a dry ſublimate, which ſeemed to be no pure but a compounded body.

And now to come to that, which I intimated might be one of the cauſes why the taſte of ſea-water is not the ſame with that of common ſalt diſſolved in freſh water; I ſhall add, that I have ſuſpected, that the various motion of the ſea, and its being expoſed to the action of the air and ſun, may contribute to give it a taſte other than ſaline; which ſuſpicion might be confirmed by the obſervation I elſewhere mention of the ſea ſalt, which, by barely being expoſed for many months to the air, and ſometimes, perhaps, put into a gentle agitation by a digeſtive heat, I found to have a very manifeſtly differing taſte from the ſimple ſolution of ſea-ſalt in common water.

I might here endeavour the farther confirmation of my diſcourſe, by what I have learned by inquiry from navigators, about the manifeſtly differing colours and other qualities, of the differing parts of the ſea, which ſeem to argue that it is not every where of ſuch a uniform ſubſtance as men vulgarly imagined, and that vaſt tracts of it are imbued with ſtupendous multitudes of adventitious corpuſcles, which, by ſeveral ways diverſifying its parts, keep it from being a ſimple ſolution of ſalt. But of this ſubject I have not leiſure to diſcourſe here; only becauſe it is generally thought, that the ſea-water is, by reaſon of the ſaltneſs it abounds with, uncapable of putrefaction, I will add, that having kept a pretty quantity of ſea-water, that I had cauſed to be purpoſely taken up between the Engliſh and French ſhores, in a good new rundlet, in a place where the ſummer-ſun beat freely upon it, it did, in a few weeks, acquire a ſtrongly ſtinking ſmell; though that the experiment had been more ſatisfactory, I wiſhed that it had been made in a veſſel of glaſs, or earth, inſtead of wood. But a much better obſervation I procured from a much eſteemed navigator of my acquaintance, who having ſailed often in the Indian and African ſeas, I inquired of him, whether he had ever, in thoſe hot climates, where the ſea is ſuppoſed to be very ſalt, obſerved it to ſtink, for want of agitation, or otherwiſe; to which he anſwered, that once being, though it was but in *March*, becalmed, in a place he named to me, for twelve or fourteen days, the ſea, for want of motion, and by reaſon of the heat, began to ſtink, inſomuch that he thinks, if the calm had continued much longer, the ſtench would have poiſoned him: they were freed from it as ſoon as the wind began to agitate the water, and broke the ſuperficies, which alſo drove away ſtore of the ſea-tortoiſes, and a ſort of fiſh, whoſe Engliſh name I know not, that before lay baſking themſelves on the top of the water.

And to this agrees very well the notable obſervation that I ſince met with, of the elſewhere commended Sir *R. Hawkins*, who, among other conſiderable things he takes notice of in his relations, has this paſſage, to our preſent purpoſe. * "Were it not "for the moving of the ſea by the force of winds, tides, and currents, it would corrupt "all the world. The experience I ſaw, *anno* 1590, lying with a fleet about the iſlands "of *Azores*, almoſt ſix months, the greateſt part of the time we were becalmed; with "which all the ſea became ſo repleniſhed with ſeveral ſorts of gellies, and forms of ſerpents, adders, and ſnakes, as ſeemed wonderful; ſome green, ſome black, ſome yellow, ſome white, ſome of divers colours, and many of them had life; and ſome "there were a yard and a half, and two yards long; which, had I not ſeen, I could "hardly have believed. And hereof are witneſſes all the company of the ſhips, which "were then preſent, ſo that hardly a man could draw a bucket of water clear of ſome

* *Purchas*'s Pilgrims in Sir *R. Hawkins*'s Obſervations.

cor-

" corruption. In which voyage, towards the end thereof, many of every ſhip fell ſick " of this diſeaſe, and began to die apace; but that the ſpeedy paſſage into our country " was a remedy to the crazed, and a preſervative for thoſe that were not touched."

THE THIRD SECTION.

CHAP. I.

AS for the various degrees of the ſaltneſs of the ſea, authors are wont to be ſilent of it, ſave that ſome navigators tell us, that they obſerved ſome ſeas to have a more, and others a leſs ſaline taſte; which, you will eaſily believe, has not afforded me much ſatisfaction. And, on the other ſide, my want of opportunity to make trials myſelf will confine me to acquaint you with no more than the few following obſervations.

1. To a learned man that was to ſail to places of differing latitudes in the Torrid Zone, I delivered a glaſs inſtrument, elſewhere deſcribed, fitted by the greater or leſſer emerſion of the upper part, to ſhew, accurately enough for uſe, the greater or leſs ſpecifick gravity of the ſalt-water it was put to ſwim in. This he put, from time to time, into the ſea-water, as he ſailed towards the *Indies*, whence he wrote me word, " That " he found, by the glaſs, the ſea-water to increaſe in weight, the nearer he came to the " Line, till he arrived at a certain degree of latitude, as he remembers, it was about the " thirtieth; after which, the water ſeemed to retain the ſame ſpecifick gravity, till he " came to the *Barbadoes* or *Jamaica*."

2. ANOTHER obſervation I obtained by inquiry of an ingenious perſon, and a ſcholar, at his return out of the *Eaſt-Indies*, who affirmed to me, that he, and a gentleman of my acquaintance, took up bottles full of ſea-water, both under the Equinoctial, and alſo off the *Cape of Good Hope*, which lies in about thirty-four degrees of ſouthern latitude, and found the waters of theſe diſtant parts of the ocean to be of the ſame weight. And though it may well be doubted, whether this obſervation, being made with ordinary bottles, were ſo exact as could be wiſhed; yet the perſons being curious, and making it for their own ſatisfaction; and my relator having, in both the recited places, filled with the ſea-water he took up, and weighed; having, I ſay, filled the ſame bottles, ſince this veſſel held two quarts (which muſt be above four pounds of ſalt water) if the diſparity of weight had been conſiderable, it would, in likelihood, have been found, at leaſt manifeſtly ſenſible, in ſuch a weight of liquor.

3. ENQUIRING of an obſerving perſon, that had been at *Moſambique*, which is thought to be one of the hotteſt places in the world, whether he did not there find the ſea to be more than ordinarily ſalt? he anſwered me, that coming thither in a great carack, when he came back from the town to the ſhip, he obſerved near two hands breadth of the veſſel to be above the ordinary part to which it uſed to ſink; inſomuch, that he took notice of it to the captain, as fearing that part of the lading had been by ſtealth carried to the ſhore; but the pilot, who had made thirteen or fourteen voyages to the *Indies*, aſſured him, what he had obſerved about the ſhip was not unuſual in that place, where the taſte itſelf diſcovered the water to be exceeding ſalt.

NOR

Nor need we ſcruple to think, that ſome ſea-waters may be very much more impregnated with ſalt than ours; for water will naturally diſſolve, and retain a far greater proportion of ſalt, than that which is commonly met with in the ſea. For whereas a thirty-fifth, or thirtieth, or at moſt a twenty-fifth part of ſalt, will make water more ſaline than is found in many ſeas, I am, by a friend of mine, that is maſter of a ſalt-work, informed, that the water of his ſprings afford him a twelfth part of good white ſalt, and that another ſpring, not far off, yields no leſs than an eighth part. To which (to avoid anticipation) I ſhall not here add, what I ſhall hereafter have occaſion to ſay of the fulleſt impregnation of water with common ſalt.

[Whilst I was reviewing theſe papers, there came ſeaſonably to my hands a letter written from *Muſilupatan*, on the gulf of *Bengala* in the *Eaſt-Indies*, by an ingenious gentleman, Sir *William Langhorn*, that is entruſted with the care of the Engliſh factories in thoſe parts; out of which letter the following paſſage is *verbatim* tranſcribed. "I did, in order to your command, cauſe ſome water to be ſaved under the Line, at "our firſt acceſs to it, intending, for want of good ſcales and weights (being none to "be come at aboard the ſhip) to have kept it, until it could be weighed, but by the "forgetfulneſs of a ſervant, it was thrown away. Off the *Cape*, in 37 d. 00 m. ſouthern "latitude, I ſaved ſome again, and, through the ſame want of weights, was fain to "keep it, until I came to the Line again; and then made the beſt ſhift I could for "weights, and compared it with the water there, filling the ſame bottle again to the "ſame height by a mark, and found it exactly the ſame weight. The weight I have "taken; but accounting this a journey of buſineſs, left thoſe notes, and moſt of the "like nature, behind me; in my next it ſhall be inſerted."]

CHAP. II.

It remains now, that, according to my promiſe, I ſet down what I obſerved myſelf concerning the ſaltneſs of our ſea between *England* and *France*; nor in compariſon with the ſaltneſs of other ſeas, whoſe waters I had not to compare with, but as to the proportion of ſalt contained in it to the water. And though one would think it very eaſy to make trials of this ſort, for a perſon not unacquainted with hydroſtatical practices, nor unfurniſhed with inſtruments, yet, I confeſs, that three or four trials that I made, not all of them the ſame way, made me find it more difficult, than was imagined, to arrive at any thing of certainty in this enquiry.

This you will eaſily believe, if I annex the ſubſtance of ſome experiments, that I remember I made about the gravity of ſea-water, which I had ordered to be taken up, ſome at the depth of about fifteen fathoms ſomewhat near our ſhore, and ſome in another place of the channel, between *England* and *France*.

The ſum of the firſt experiment is this: we took a phial, fitted with a long and ſtrait neck, purpoſely made for ſuch trials, and having counterpoiſed it, filled it to a certain height with common conduit-water: we noted the weight of that liquor, which being poured out, the phial was filled to the ſame height with ſea-water taken up at the ſurface, and by the difference between the two weights, the ſea-water appeared to be about a forty fifth part heavier than the other.

The ſecond trial (which was far more accurately made hydroſtatically) I find regiſtered to this effect: we carefully counterpoiſed in the ſcales, formerly made uſe of, a piece of ſulphur in the upper ſea-water, formerly mentioned; it weighed ℥ ſs + *gr*. x. ſs, and being alſo weighed in the ſea-water fetched from the bottom, gave us the ſame weight

weight ℥ ß + *gr.* x. ß which ſhewed thoſe two waters to be of the ſame ſpecifick gravity: and then to compare this with the gravity of common water, we weighed the ſame ſulphur in common conduit-water, and found it ℥ ß + *gr.* xv. ß: by which it appeared, that the ſea-water was but about a fifty-third part heavier than this water: which is ſuch a difference from the proportion found out by the former way of trial, that I could not well imagine what to attribute it to, unleſs the ſea-water by long ſtanding in a veſſel, which, though covered, was expoſed to the hot ſun, may both have been rarefied, and have had ſome ſeparation made of its ſaline or other heavier parts, on which ſcore that portion we took up for our trial, might appear lighter than elſe it would have done; or unleſs the experiment having been made in *London*, where great and ſudden rains and other accidents will ſometimes viſibly vary the conſiſtence of common water, the liquor, I then employed without examining it, might be more ponderous at that time than at another. To which latter ſuſpicion I was the more inclined, becauſe, having afterwards weighed the ſame piece of ſulphur by help of the ſame balance in diſtilled rain-water, I found the weight of the former liquor to exceed that of the latter by a good deal leſs than a thirty-fifth part; which ſeemed to make it probable, that if the water we chanced to employ, had been free from all ſaline and other heavy particles, the difference formerly mentioned betwixt this obſervation and the foregoing would not have been near ſo great as it was.

The laſt way I made uſe of to examine the proportion betwixt ſea-water and freſh, was chymical; whereof my regiſter affords me this account.

A pound avoirdupoiſe weight of the upper ſea-water was weighed out, and put into a head and body to be diſtilled in a digeſtive furnace *ad ſiccitatem*; and the diſtillation being leiſurely made, the bottom of the glaſs was almoſt covered with fair grains of ſalt, ſhot into cubical figures, and more white than was expected: in the reſt of the coagulated matter, we took not notice of any determinate ſhape. The ſalt being weighed, amounted to ℥ ß, avoirdupoiſe, and *gr.* x. At which rate, the proportion of the ſalt to the water will be that of 30 and $\frac{72}{100}$ to one, and ſo will amount to near the thirtieth part; which was ſo much greater than the former ways of trial made us expect, that I know not whether it may not be worth while to try, whether ſuch a ſlow abſtraction, as we employ of the ſuperfluous water, and our doing it in cloſe veſſels, may not have afforded us more ſalt, than elſe we ſhould have obtained.

To this relation I find this note ſubjoined: ſuſpecting that there may have ſomewhat elſe concurred to our finding ſo great a proportion of ſalt, I ſuffered that which had been weighed, to continue a while in the ſcale, and ſoon perceived, that, according to my conjecture, that ſcale began manifeſtly to preponderate, and that conſequently ſome of the unexpected weight of ſalt may be due to the moiſture of the air, imbibed after the ſalt was taken out of the glaſs, and laid by to be weighed: wherefore, cauſing it to be very well heated and dried in a crucible, we found it to weigh ℥iij + ß (that is 210 grains) upon which account the proportion of ſalt contained in the water, was a thirty-ſixth part, and ſomewhat above half of thoſe parts, and to expreſs it in the neareſt whole number, a thirty-ſeventh part.

From whence this greater proportion of ſalt by diſtillation, than our other trials invited us to expect, proceeded, ſeems not ſo eaſy to be determined; unleſs it be ſuppoſed (as I have ſometimes ſuſpected) that the operation the ſea-water was expoſed to in diſtillation, made ſome kind of change in it, other and greater than before-hand one would have looked for; and that, though the grains of ſalt we gained out of the ſea-water, ſeemed to be dry before we weighed it, yet the ſaline corpuſcles, upon their concreting into cubes, did ſo intercept between them many ſmall particles of water, as not to suffer them to be driven away by a moderate warmth; and consequently such grains of salt may have upon this account been less pure and more ponderous than else they would have been. And I might here add, that I sometimes make a certain artificial salt, which, though being dissolved in water, it will shoot into crystals finely shaped, and dry enough to be reducible into powder, yet coagulates water enough with it to make the water almost, if not quite, as heavy again as before. And I have been assured by a very learned eye-witness, that there is a sort of sea-salt, which they bring to some parts of *England*, from the coast of *Spain* or *Portugal*, which being here dissolved, and reduced by purification and filtration to a much whiter salt, will yield by measure somewhat above two bushels for one. But to satisfy the scruples and suspicions I could suggest, would require more trials than I have now time or opportunity to make. What has been already delivered, may give at least as scrupulous an account of the saltness of our *English* sea-waters, as most other experimenters would have thought it needful to give. And to make a determination with any certainty about the degrees of the sea's saltness in general, a great number of observations, made in different climates, and in distant parts of the ocean, would be necessary.

CHAP. III.

I KNOW not whether I may be so indulgent to my suspicions, as to wish, that observations were heedfully made, whether in the same sea, and about the same part of it, the waters be always equally salt? For, though that be taken for granted, yet since we have no good observations long since made to silence the suspicion, one may suspect, that, at least in many places, the saltness of the sea may continually, though but very slowly, increase by the accession of those saline corpuscles, that are imported by salt-springs, and those which rivers and land-floods do from time to time rob the earth of. And I suspect it to be not impossible, that this, or that part of the sea, may be sometimes extraordinarily, and perhaps suddenly, impregnated with an additional saltness from saline steams plentifully ascending into it, from those subterraneal fires, about which I have made it elsewhere probable, that they may burn beneath the bottom of the sea, and sometimes send forth copious exhalations into it. But it may prove the more difficult to discern this adventitious saltness, unless the taste, as well as balance, be employed about it; because the salt, that produces it, may be of such a nature, as to be much lighter in specie than common sea-salt. And the mention of this leads me to give you here the advertisement I promised you not long ago.

THAT though the weight of sea-water be as good a way as is yet employed (and better than some others) to determine what sea-water does most abound in salt; and though it be possible, that in our sea, and perhaps in almost all others, this way be not liable to any considerable uncertainty; yet I think it not impossible, that it may sometimes deceive us, especially in very hot regions; because I have observed, that there may be volatile salts, which, though by reason of their activity, they make smart impressions on the tongue, and give the water imbued with them a strong saline taste, yet they add very little, and much less than one would think, to its specifick gravity: as I have tried, by hydrostatically examining distilled liquors, abounding in volatile and urinous salts, some of which I found very little heavier than common water, and consequently nothing near so much heavier, as they would have been made, if they had been brought to so sharp a taste, by having nothing but common sea-salt dissolved in them: so that, if if in any particular place, by any other way, or from the ſteams of the earth beneath, ſome of which, I elſewhere ſhew, may be very analogous to thoſe afforded by ſal armoniack) the ſea ſhould be copiouſly impregnated with ſuch kind of light ſalts, the ſea water may be much more ſalt to the taſte, and yet be very little heavier. For confirmation of which I find among my notes, that weighing a ſealed bubble of glaſs, made heavy by an included metal, firſt in ſpirit of ſal armoniack, that taſted much ſtronger than ſea-water, it weighed ℥iij + *gr.* 51 $\frac{1}{4}$, and weighing this ſame body in fair water, it weighed but ℥iij + *gr.* 45 $\frac{3}{4}$; ſo that notwithſtanding its great ſaltneſs, the ſpirit was lighter than common water; though a good part of that comparative levity may probably be aſcribed to the liquor wherein the ſaline particles ſwarm, which, by diſtillation, was grown more defecated and light, than common, though clean, water.

But for a farther proof, we took a hard lump of ſal armoniack; and though we could not weigh it in water, becauſe that would have diſſolved part of it, yet by a way (I elſewhere teach) I found that weighing in the ſame liquor this lump of ſal armoniack, and a lump of good white ſea-ſalt (brought me as a curioſity out of the Torrid Zone) the proportion of the latter to a bulk of the liquor equal to it, was ſomething (though exceeding little) above that of two and a quarter to one, and the proportion of ſal armoniack to as much water, as was equal likewiſe to it, did not above a centeſm exceed that of one and $\frac{7}{10}$ to one; which falls ſo ſhort of the other proportion, as may juſtly ſeem ſtrange, eſpecially if it be conſidered, that the factitious ſal armoniak, the chymiſts generally uſe, and we employ, conſiſts in good part of ſea-ſalt, which abates much of the comparative levity it might have, if it were made up only of urinous and fuliginous ſalts, which were its other ingredients.

It were indiſcreet for me to propoſe any more ſuſpicions and trials fitted to clear them, unleſs I knew thoſe I have already mentioned would not paſs for extravagancies; and therefore, I ſhall here diſmiſs the ſubject of this tract of the ſaltneſs of the ſea, but that, ſince I have been diſcourſing of the degrees of it, it will not be impertinent to add, what is the greateſt meaſure of ſaltneſs, that I have brought water to, without the help of external heat. On this occaſion, I employed two differing ways: the one was, by putting into a well counterpoiſed phial two ounces of common water, and then putting into it, well dried and white common ſalt, and ſhaking them together, till the liquor would, whilſt cold, diſſolve no more: this liquor, thus glutted with ſalt, weighed 1150 grains, from which two ounces being deducted, the overplus of weight, ariſing from the diſſolved ſalt, amounting to 190 gr. ſo that a parcel of ſalt will, without heat, be diſſolved in about five times its weight, or very little more, of common water. By which proportion we made ſo ſtrong a brine, that divers pieces of amber being purpoſely let fall into, emerged, and floated on it. The other and better way, yet more tedious, that we made uſe of, was to let ſea-ſalt run *per deliquium* (as the chymiſts ſpeak) that is, to ſet it in ſome moiſt place, till it was diſſolved by the aqueous vapours that ſwim in the air. In this liquor we weighed a piece of ſulphur, which we alſo weighed in ſea-water, wherein, finding it to weigh much more than in the former liquor, it appeared, that the ſea-water was, in ſpecie, much lighter than the other; though how much their gravities differed, I cannot find among my notes, nor be informed by my memory.

And becauſe I have not, in any author, met with the proportion of ſea-ſalt to water of the ſame bulk, nor perceive, that hydroſtaticians themſelves have yet attempted any way to inveſtigate it (probably deterred by the eaſy diſſolubleneſs of ſalt in water) I ſhall here ſubjoin, that by the help of an expedient, I have elſewhere taught, I have examined a hard dry lump of ſea-ſalt, and found its proportion in weight, to common water of the fame bulk, to be almoft as 2 to 1 (for it exceeded the *ratio* of $1\frac{9}{10}$ to 1). And, I remember, I found the fpecifick weight of a hard and figured lump of *fal gem.* (which fort of falt I fuppofe to be fomewhat more pure and ponderous than fea-falt) to be to that of water (very near) as $2\frac{1}{8}$ to 1.

THE FOURTH SECTION

BELONGING TO THE TRACT, ENTITLED,

Relations about the Bottom of the SEA.

THE prefence of the air is not only fo neceffary to the life of many forts of animals, but it hath likewife fo great a ftroke in the growth of vegetables, efpecially of the larger forts, that, after what I had experimented about thefe matters (of which this is not the proper place to give an account) I thought fit to make enquiry about the vegetation and growth of plants of confiderable bulk, in thofe fubmarine regions, where, if there grow any, they muft do it remote from the free contact of an ambient air. And having not now the leifure to repeat what botanifts (of whofe books I am not now provided) deliver about leffer plants growing under water, I fhall now only prefent you with what information I could procure from navigators, about trees and fruits growing at the bottom of the fea.

To what I have elfewhere had occafion to fay to their opinion, that will not allow coral to be really a ftony plant, but a lifelefs concrete, that is always hard and brittle under water; I fhall now add, that, enquiring lately of an eminent and inquifitive perfon, that had fpent fome time upon the coaft of *Africa*, where he had been prefent at the fifhing of coral, and learning from his anfwer, that he had feen it not far from *Algiers*; I afked him, whether he had himfelf obferved the coral to be foft, and not red, when it was newly brought from the bottom of the fea. To which he replied, that he had found it foft and flexible; and that, as for the colour, it was for the moft part very pale, but with an eye of red, the bark being worfe coloured than the fubftance it covered was; but when the bark was taken off, and the other part expofed to the air, the expected rednefs of the coral difclofed itfelf.

WHEN I demanded whether he had obferved that any inky fap afcended to nourifh the ftony plant? and whether he had feen any thing like berries upon it? he ingenuoufly confeffed to me, he had not been fo curious, as purpofely to make enquiry into thofe particulars; but that he remembered, that having broken fome of the large pieces of coral, he took notice that the more internal fubftance was much paler than the other, and very whitifh; and that at the extreme parts of fome branches, or fprigs, he obferved little blackifh knobs, which he did not then know what to make of: and when I enquired what depth the fea was of in that place? he anfwered, that it was nine or ten fathom.

But

But as to the fruit of some kinds of coral, if I do not much mis-remember, I was, not long since, assured by a scholar, that navigated much in the east, that they divers times meet with in those seas a certain sort of coral, but not white, which bears a small fruit like a round berry, of a pleasant colour, and esteemed as rarities.

DISCOURSING with a person that made diving his trade, whether he had not met with any trees or fruit in the depths of the sea? he told me, that in a great ship, whereinto he descended, to recover thence some shipwrecked goods, he was surprized to find in several places a certain sort of fruit, that he knew not what to make of, for he found them of a slimy and soft consistence, about the bigness of apples, but not so round in shape; and when he brought them up into the air, as he did many of them, they soon began to shrink up like old rotten apples, but were much harder, and more shrivelled. And it is remarkable, that this happened in a cold northern sea.

ONE that made a considerable stay about *Manar*, a place I have often mentioned, answered me, that he learned from the divers, that in some places thereabouts, there grows at the bottom pretty store of a certain sort of trees, bearing leaves almost like those of laurel, as also a certain fruit; but of what virtue, or other use, he had not the curiosity to enquire.

I WAS also informed by an eye-witness, that near the famous coast of *Mosambique* in *Africa*, there grows at the bottom of the sea store of trees, that bears a certain fruit, which he describes to be very like that, which, in *America*, they are wont to call *Acayu*, the leaves also resembling those of that tree.

BUT the best welcomest information I could procure about submarine plants, is that which concerns the famous Maldivian nut, or Cocoa, which is so highly esteemed in the east, that some write, it is a great present from one king to another, and even much extolled in *Europe* by experienced physicians: for the origin of this dear drug is almost as much controverted, as the alexiterial virtues are extolled. Having then once the good fortune to meet with a man of letters, that had resided in those unfrequented islands, I found he had been as inquisitive, as I could reasonably expect, about these admired productions of the sea, and that he had often learned from the divers, that they are real nuts, or fruits, borne by a sort of cocoa-trees, that grow at the bottom of the sea, and are thence, either torn off, by the agitation of the water, or gathered by the divers. These fruits are smaller than most other sorts of cocoas, whose maturity they do not seem to arrive at. He thinks, the species may have been very differing from what it is, and may have come from nuts fallen into the sea, together with the ruin of some little islands undermined by the water, and so submerged; of which he told me, he saw, at least, three or four instances during his stay there. He told me, that whilst the fruit was under water, they observed no distinct shell and kernel, but the entire nut was so soft, that it may easily enough cut with a knife, and was eaten like their other fruits; but being kept about a week in the hot air, it grows solid, and so hard, as to require good steel tools to work upon it. He added, that though, even upon the place, the fairer sort be of very great esteem, yet not of any such prodigious price as is given out. And he presented me one about the bigness of a large egg, and a fragment of another, which are both very hard; but as for their virtues, I can yet say nothing upon trial, for want of having had fitting opportunities.

OTHER observations made at the bottom of the sea may hereafter follow.

A PARA-

A PARADOX OF THE NATURAL AND PRETERNATURAL STATE OF BODIES, Especially of the AIR.

I Know that not only in living, but even in inanimate bodies, of which alone I here discourse, men have universally admitted the famous distinction between the natural and preternatural, or violent state of bodies, and do daily, without the least scruple, found upon it hypotheses and ratiocinations, as if it were most certain, that, what they call nature, had purposely framed bodies in such a determinate state, and were always watchful, that they should not by any external violence be put out of it.

But notwithstanding so general a consent of men in this point, I confess I cannot yet be satisfied about it in the sense wherein it is wont to be taken. It is not, that I believe, that there is no sense in which, or in the account upon which, a body may be said to be in its natural state; but that I think the common distinction of a natural and violent state of bodies has not been clearly explained, and considerately settled, and both is not well grounded, and is oftentimes ill applied. For when I consider that whatever state a body be put into, or kept in, it obtains or retains that state, according to the catholick laws of nature, I cannot think it fit to deny, that in this sense the body proposed is in a natural state, but then, upon the same ground it will be hard to deny, but that those bodies which are said to be in a violent state, may also be in a natural one, since the violence they are presumed to suffer from outward agents, is likewise exercised no no otherwiſe than according to the eſtabliſhed laws of univerſal nature. It is true, that when men look upon a body as in a preternatural ſtate, they have an idea of it differing from that, which they had whilſt they believed it to be in a natural ſtate: but perhaps this difference ariſes chiefly from hence, that they do not conſider the condition of the body, as it reſults from the catholic laws ſettled among things corporeal, and relates to the univerſe, but eſtimate it with reference to what they ſuppoſe is convenient, or inconvenient, for the particular body itſelf. But however it ſeems to me, that men's determining a body to be in a natural or preternatural ſtate has much more in it, either of caſual, or of arbitrary, or both, than they are aware of. For oftentimes we think a body to be brought into a violent ſtate, not becauſe really the former was not ſo, but becauſe there is a notable change made in it by ſome agent, which we alſo take notice of; whereas before the action of that agent, if the body were under any violence, it was exerciſed by uſual, but often immanifeſt agents, though perhaps their compulſion were not leſs, but only leſs heeded. And ſometimes alſo no more is to be underſtood by a body's being forced from its natural ſtate, than that it has loſt that, which it had immediately, or a pretty while before ſome notable change. Which conjectures I ſhall now endeavour to confirm, but with great brevity.

I HAVE already ſhewn, that matter being devoid of ſenſe and appetite, cannot be truly and properly ſaid to affect one ſtate or condition more than another, and conſequently has no true deſire to continue in any one ſtate, or to recover it when once loſt; and inanimate bodies are ſuch, and in ſuch a ſtate, not as the material parts they conſiſt of, elected or deſired to make them, but as the natural agents, that brought together and ranged thoſe parts, actually made them. As a piece of wax is unconcerned, whether you give it the ſhape of a ſphere, or a cone, or a pillar, or a boat; and whether, when it has that form, you change it into any other; the matter ſtill retaining without willingneſs or unwillingneſs, becauſe without perception, that figure, or ſtate, which the laſt action of the agents (your fingers or inſtruments) determined it to, and left it in.

BUT this will be beſt underſtood, as well as confirmed, by particular examples. I need not tell you, that the moſt uſual inſtance alledged to ſhew, that a ſtate is natural to a body, and that being put out of it by external cauſes, it will, upon the ceſſation of their violence be reſtored thereunto, is, that water being heated by the fire, as ſoon as that adventitious heat vaniſhes, returns to its native coldneſs; and ſo when, by an exceſs of cold, it is congealed into ice, it does upon a thaw loſe that preternatural hardneſs, and recover the fluidity, that naturally belongs to it: and the ſame may be likewiſe ſaid of butter, which, being melted by external heat into a liquor, does upon the ceſſation of that heat grow a conſiſtent body again. But perhaps theſe inſtances will rather countenance our paradox than diſprove it. For as to the coldneſs, whereto water heated by the fire returns, when it is removed thence, it may be ſaid, that the acquired heat conſiſting but in the various and briſk agitation of the corpuſcles of the water by an external agent, it need be no wonder, that when that agent ceaſes to operate, the effect of its operation ſhould ceaſe too, and the water be left in its former condition, whether we ſuppoſe it to have been heated by the actual pervation of the corpuſcles of the fire, which muſt by degrees fly away into the air; or that the heat proceeds from an agitation imparted by the fire to the aqueous corpuſcles, which muſt, by degrees, loſe that new agitation, by communicating it little by little to the contiguous air and veſſel; ſo that if the former agitation of the particles of the water were, as is uſual, much more languid than that of our organs of feeling, in which faintneſs of motion the coldneſs of water conſiſted, there will be no need of any poſitive internal form, or any care of nature to account for the water's growing cold again. This will be confirmed by by the consideration of what happens to ice, which is said to be water brought into a preternatural state by an excess of cold. For I doubt it will not be easily demonstrated, that in reference to the nature of things, and not to our arbitrary ideas of them, ice is water preternaturally hardened by cold, and not water ice preternaturally thawed by heat. For if you urge, that ice left to itself will, when the frigorific agents are removed, return to water; I shall readily answer, that, not to mention the snow and ice, that lie all the summer long unthawed upon the tops of the Alps and other high mountains, I have learned, by enquiry purposely made, from a doctor of physick, who for divers years practised in *Muscovy*, that in some parts of *Siberia* (a large province belonging to the Russian emperor) the surface of the ground continues more months of the year frozen, by what is called the natural temperature of the climate, than thawed by the heat of the sun; and that a little beneath the surface of the ground, the water that chances to be lodged in the cavities of the soil, continues frozen all the year; so that, when in the heat of summer the fields are covered with corn, if then you dig a foot or two, perhaps less, you shall easily find ice and a frozen soil: so that a man born and bred in the inland part of that country, and informed only by his own observation, may probably look upon water as ice violently melted by that cœlestial fire, the sun, whose heat is there so vehement in their short summer, as to ripen their harvest in less time than in our temperate climates will easily be credited.

On the other side, we in *England* look upon melted butter, as brought into a violent state by the operation of the fire, and therefore think, that when being removed from the fire it becomes a consistent body again, it has but recovered its native constitution. Whereas there are divers parts of the *East Indies*, and, I doubt not, of other hot countries, whose inhabitants, if they should see consistent butter (as sometimes by the care and industry of the Europeans they may do) they would think it to be brought to a preternatural state by some artificial way of refrigeration. For in those parts of the *Indies* I speak of (though not in all others) the constant temper of the air being capable to entertain as much of agitation as suffices for fluidity in the parts of what, in our climate, would be butter, it would be in vain to expect, that, by being left to itself in the air, it should become a consistent body. And I have learned by diligent enquiry of seamen and travellers, both English, and others that were eye-witnesses of what they told me, that in divers parts of those hot regions, butter, unless by the Europeans, or their disciples, purposely made in the cold, is all the year fluid, and sold, or dispensed, not as consistent bodies, by weight, but as liquors, by measure. To strengthen this observation, I shall add what was affirmed to me by a learned man, that practised physick in the warmer parts of *America*, namely, that he met in some places with several drugs, which, though they there seemed to be balsams, as turpentine, &c. are with us, and retained that consistence in those climates; yet when they come into our colder regions, harden into gums, and continue such both winter and summer. On the other side, enquiring also of a traveller, versed in physical things, about the effects of great heat in the inland parts of *Africa*, where he had lately been; he told me, among other things, that resin of jalap, which, when he carried it out of *England*, was of a consistence not only dry, but brittle, did, when, and a while before he came to *Morocco*, melt into a substance like turpentine; so that some of it, that he had made up into pills, would no more at all retain that shape, but remain, as it were, melted all the while he staid in that city, and the neighbouring country, though when he came back to the borders of *Spain*, it returned to its former consistence. Which I the less wondered at, because, having had the curiosity to consider some parcels of gum lacca (of which sealing-wax is made) newly brought ashore from the *East Indies*, though it be a hard and solid gum, yet I found by several

instances,

niſtances, that, paſſing through the Torrid Zone, divers pieces of it, notwithſtanding the ſhelter afforded it by the great ſhip it came in, had been, by the heat of the climate, melted, and made to ſtick together, though afterwards they regained their former conſiſtence, though not altogether their former colour. And on this occaſion I ſhall add, that I learned by enquiry from a particular acquaintance of mine, who brought me divers rarities out of *America*, that having at the place where it was made, among other things, furniſhed himſelf with a quantity of the beſt aloes, he obſerved, that whilſt he ſailed through very hot climates, it was ſo ſoft, that, like liquid pitch, it would often have fallen out of the wide-mouthed veſſel he kept it in, if he had not from time to time been careful to prevent it. But when he came within a hundred leagues of the coaſt of *England*, it grew hard, and ſo continued, though this were in a very warm ſeaſon of the year, being about the dog-days.

For further confirmation of what has been hitherto diſcourſed, be pleaſed to conſider with me that moſt obvious body the air, or the atmoſphere we live and breathe in. For though ſeveral opinions and argumentations are founded upon what their authors call the natural and preternatural or violent ſtate of the air, yet he that conſiders, ſhall find it no eaſy thing to determine what ſtate of the air ought to be reputed its truly natural ſtate, unleſs in the ſenſe I formerly told you I employ that expreſſion in. I will not inſiſt on the heat and coldneſs of the air; for that being manifeſtly very differing in the heart of winter, and in the heat of ſummer, and in differing regions of the air, as at the top and bottom of high mountains, at the ſame time, and conſtantly in differing regions of the earth, as in *Barbary* and *Greenland*, it will not be ſo eaſy to determine, what ſtate is natural to the air. But that only, which I ſhall now conſider, is its ſtate or tone in reference to rarity and denſity. For ſince the air is believed to be condenſed by cold, and expanded by heat, I demand at what time of the year, and in what country, the air ſhall be reputed to be in its natural ſtate? For if you name any one time, as the winter or the ſummer, I will aſk, why that muſt be the ſtandard of the tone of the air rather than another ſeaſon, or at leaſt excluſively to all others? And the like difficulty may be made about the climate or the place. And theſe ſcruples are the more allowable to be propoſed, becauſe learned men have delivered, that in ſome countries the mercury in the Torricellian experiment is kept higher than in others (as in *Sweden* than in *Italy*) and our baroſcopes inform us, that oftentimes, in the ſame place and day, the quickſilver in the ſame inſtrument does conſiderably vary its height; which ſhews, that the air or atmoſphere muſt neceſſarily vary its weight, and therefore probably its degree of rarity or denſity.

But I have yet to propoſe a further conſideration in this affair: for what if it ſhall appear, that neither in winter nor in ſummer, in *Sweden* or in *Italy*, or in whatever country, region, or ſeaſon you pleaſe, the air we breathe in is in any other than a preternatural ſtate? nay, that even when we have vehemently agitated and expanded it by an intenſe heat of the fire, it is not yet violently rarefied, but yet violently conſtipated, unleſs, in our ſenſe before declared, you underſtand with me the preternatural ſtate of rarefaction in the air, in reference to the tone it had before the laſt notable change was produced in it. This will, I queſtion not, ſeem a ſurprizing, if not a wild, paradox: but yet to make it probable, I ſhall only deſire you to reflect upon two or three of my phyſico-mechanical experiments; and there you will ſee, firſt, that the air being a body abounding with ſpringy particles, not devoid of gravity, the inferior muſt be compreſſed by the weight of all the incumbent. And next, that this compreſſion is ſo great, that though by the heat of the fire neither others, nor we, could bring a portion of included air to be expanded to above fourſcore times its former ſpace; yet without heat,

heat, by barely taking off the preſſure of the ſuperior air, by the help of our pneumatical engine, the air was rarefied more than twice as much: and ſince thoſe experiments were publiſhed, I more than once rarefied it to above five hundred times its uſual dimenſions; ſo that if, according to what is generally agreed on and taught, a body be then in a preternatural ſtate, when, by an external force, it is kept in a condition, from which it inceſſantly tends to get free; and if it be then moſt near its natural ſtate, when it has the moſt proſperouſly endeavoured to free itſelf from external force, and comply with its never ceaſing tendency; if this be ſo, I ſay, then the air we live in is conſtantly in a preternatural ſtate of compreſſion by external force. And when it is moſt of all rarefied by the fire, or by our engine, its ſpring having then far more conveniency than before to diſplay themſelves, which they continually tend to do, it anſwerably approaches to its natural ſtate, which is to be yet leſs compreſſed or not at all. And I have carefully tried, for many months together, that when the air has been rarefied much more than even a vehement heat will bring it to be, yet if it were fenced from the preſſure of the external air, it would not ſhrink to its former dimenſions, as if it had been put into a violent ſtate, from whence nature would reduce it to them, but continued in that great and ſeemingly preternatural degree of extenſion, as long as I had occaſion to obſerve it. One might here ſhew, that this odd conſtitution of the air is ſo expedient, if not neceſſary for the motion, reſpiration, and other uſes of animals, and in particular of men, that the providence and goodneſs of the wiſe Author of the univerſe is thereby ſignally declared; if it were not improper, in ſuch a paper as this, to employ final cauſes. Wherefore to avoid the imputation of impertinence, I will conclude, by taking notice, that, from what has been delivered, we may learn two things conſiderable enough, if not in themſelves, yet to ſome paſſages of the treatiſe, whereof this paper makes a part. And firſt, we may deduce from what has been ſaid of the air, that, according to what is noted above, that may ſometimes generally be granted, and believed to be the natural ſtate of a body, not which it really affects to be in, or (to ſpeak more properly) has a tendency to attain, but that which it is brought into, and kept in by the action, or reſiſtance of neighbouring bodies, or by ſuch a concourſe of agents and cauſes, as will not ſuffer it to paſs into another ſtate. And the ſecond thing we may hence learn, is, that whatever men ſay of nature's never miſſing her aim, and that nothing violent is durable; yet, bating an inconſiderable portion of aërial particles at the upper ſurface, for aught we know, the whole maſs of the air we live in, and which invirons the whole terraqueous globe, has been from the world's beginning, and will be to its end, kept in a ſtate of violent compreſſion.

A STA-

A STATICAL HYGROSCOPE

Propoſed to be further Tried.

Together with

A BRIEF ACCOUNT

OF THE

UTILITIES OF HYGROSCOPES.

A

STATICAL HYGROSCOPE, &c.

In a LETTER to H. OLDENBURG, Eſq; Secretary to the ROYAL SOCIETY.

SIR,

THOUGH I writ to you from *Stanton*, an account of thoſe hygroſcopes, whereof I now preſent you one; yet ſince I remember, that it was in the year 1665, that I ſent you that paper, I fear you may, by this time, have forgotten much of what it contained, and thereby made it fit for me, in this letter, both to remind you of ſome former paſſages, and to add ſome obſervations, that lately occurred to me; and this the rather, becauſe I do not preſent you with this trifle, merely to gratify your curioſity, but that you, and ſome of your ingenious friends, may, by your remarks, help me to diſcover, to what inconveniencies our inſtrument is liable, how far they may be avoided, or leſſened, or what the uſes, or advantages, of it may be, notwithſtanding its inevitable inconveniencies, or imperfections.

Having had occaſion, amongſt other ſubjects relating to the air, to conſider its moiſture, and its dryneſs, I eaſily diſcerned, that they had no ſmall influence upon divers bodies, and, among the reſt, upon thoſe of men, as the ambient air we breathe in, either paſſes from one of thoſe qualities to the other, or even from one degree to the other, in the ſame quality.

Wherefore, I began to caſt about ſomewhat ſolicitouſly, for a way that might, better than any I had yet tried, or elſewhere met with, diſcover the changes of the air, as to moiſture and dryneſs, and the degrees of either quality. For which purpoſe, it ſeemed to me, that if a ſtatical hygroſcope could be had, it would be very convenient, in regard of its fitneſs, both to determine the degrees of the moiſture, or dryneſs of the air, and to tranſmit the obſervations made of them to others. Whereupon, conſidering further, that among bodies, otherwiſe well qualified for ſuch a purpoſe, that was likelieſt to give the ſenſibleſt informations of the changes of the air, which, in reſpect o its bulk, had the moſt of its ſurface expoſed thereunto; I quickly pitched upon a fine ſpunge, as that, which is eaſily portable, not eaſy to be divided or diſſipated, which, by its readineſs to ſoak in water, ſeemed likely to imbibe the aqueous particles, that it may meet with diſperſed in the air, and which, by its great porouſneſs throughout, has much more of ſuperficies, in reference to its bulk, than any body, not otherwiſe leſs fit for the intended uſe, that came into my thoughts.

If you recal to mind, when, and whence I firſt gave you notice, that I employed our little inſtrument, you will eaſily believe, that the inducements I had to pitch upon it, were, that I ſhould need but ſuch light and parable things, as I could eaſily both procure in the country (where I then was) and carry about with me, in the frequent removes I was obliged to make; and therefore, that I did not preſent this trifle as the beſt hygroſcope, that could be deviſed, or even as the beſt, that, perhaps, I myſelf could have propounded, if I would have framed an elaborate engine with wheels, ſprings, or equivalent weights, pullies, indices, and other contrivances, ſome of which I divers years ago made uſe of. For I little doubt, but that mechanical heads may frame hygroſcopes much curiouſer and perfecter, than that I now ſend you, or any other I have uſed, or ſeen, if they may be accommodated with ſufficient room, and dextrous artificers, that will work exactly according to directions; whereas, my deſign being not ſo much to make a machinal, or engine-like, as a ſtatical hygroſcope, and ſuch an one as may be ſimple, cheap, contained, and ſet up in a little room, eaſy to be made and tranſported, I thought it might be of ſome uſe, eſpecially to thoſe that are not furniſhed with curioſities and mechanical accommodations, if among the ſeveral forms of hygroſcopes that I had in my mind, I choſe one, that being ſtatical and eaſy, might be as commodious by its ſimplicity, as ſome others by their elaborateneſs; eſpecially if we conſider, that, as ſlight an inſtrument as it ſeems, it may be applied to various uſes, ſome of which are not ſlight, as will ere long be made probable.

If I ſhould be here told by one that grants the preferableneſs of ſtatical hygroſcopes in the general, that there are divers bodies, other than that pitched upon by me, whoſe weight may vary, when the temperature of the air is conſiderably altered, as to dryneſs and moiſture, and that, perhaps, among theſe, ſome one may be found, that may imbibe the aqueous particles of the air better than our ſpunge; I ſhall not reſolutely deny it, and therefore ſhall leave you to make trials with what other bodies you ſhall think fit, contenting myſelf to have ſuggeſted, in general, the conveniency of making hygroſcopes, where the differing changes of the air may be eſtimated by weight: but this I ſhall tell you, in favour of our ſpunge, that when I was conſidering what bodies were the fitteſt to be employed for the making of ſtatical hygroſcopes, I made trial of more

more than one, that ſeemed not the leaſt promiſing. I know, that common, or ſea-ſalt, will much relent in moiſt air, and ſalt of tartar will do it much more; but then thoſe ſalts, eſpecially the latter, will not ſo eaſily, as they ſhould, part with the aqueous corpuſcles they have once imbibed; and are in other regards (which it were not worth while to inſiſt on) leſs convenient than a ſpunge. I made trial alſo with lute-ſtrings, which were purpoſely choſen very ſlender, that they might have the greater ſurface, in reſpect of their bulk: theſe I found, at firſt, to do very well, as to the imbibing of the moiſture of the air, but afterwards they did not continue to anſwer my expectation. I cauſed likewiſe to be turned out of a light wood a cup, which, that it might leſs burden a tender balance, had, inſtead of a foot, a little button, to which a hair might be tied, to ſuſpend it by; and this cup being purpoſely turned very thin, that it might have much ſurface expoſed to the air, proved for a pretty while ſo good a hygroſcope, as invited me to make divers obſervations with it, ſome of which I have ſtill by me. It agreed alſo with ſeveral trials, that I had made on other occaſions, of the porouſneſs of ſuch bodies, that white ſheeps leather, ſuch as ſurgeons uſed to ſpread plaiſters upon, would be very convenient for my purpoſe. And indeed I found by many obſervations, whoſe ſucceſs you may command a ſight of, that if this leather were a ſubſtance as little obnoxious to corruption as a ſpunge, it would, by its copious imbibitions, and emiſſions of the aërial moiſture, be a fitter matter, than any other I had employed for a hygroſcope.

But taking all things together, I found no body ſo convenient for my purpoſe as a ſpunge; which you will, perhaps, the more eaſily believe, if I add, that to help me to make ſome eſtimate of the poroſity of it [we weighed out a drachm of fine ſpunge, and having ſuffered it to ſoak up what water it could, it was held in the air, not only whilſt the weight of the water would eaſily make it run out, but till it dropped ſo very ſlowly, that a hundred was reckoned after one drop, before another fell; then putting it into the balance it had been weighed in before, we found that as its dimenſions were increaſed to the eye, ſo its weight was increaſed upon the ſcale, amounting now to ſomewhat above two ounces and two drachms; ſo that one drachm of ſpunge, though it ſeemed not altogether ſo fine as the portion we had choſen out for our hygroſcopes, did imbibe and retain ſeventeen times its weight of water.]

Now when one is reſolved to employ a ſpunge, there will not need to be much added about the turning it into a hygroſcope. For, having weighed it, when the air is of a moderate temperature, it requires but to be put into one of the ſcales of a good balance, ſuſpended on a gibbet, as they call it, or ſome other fixed and ſtable ſupporter. For the ſpunge being carefully counterpoiſed, at firſt, with a metalline weight (becauſe that alters not ſenſibly with the changes of the air) it will, by its decrement, or increaſe of weight, ſhew how much the neighbouring air is grown dryer, or moiſter, in the place where the inſtrument is kept. The weight of the ſpunge may be greater, or leſs, according to the bigneſs and goodneſs of the balance, and the accurateneſs you deſire in the diſcoveries it is to make you. For my part, though I have, for curioſity's ſake, with very tender ſcales employed, for a good while, but half a drachm of ſpunge, and I found it to anſwer my expectation well enough; and though, when I uſed a bulk divers times as great, in a ſtronger, but proportionably leſs accurate balance, I found not the experiment ſucceſsleſs: yet after trials with differing quantities of ſpunge, I preferred, both to a greater and leſſer weight, that of a drachm, as not being heavy enough to overburden the finer ſort of goldſmiths ſcales, and yet great enough to diſcover changes conſiderably minute, ſince they would turn, diſcernibly, with a ſixteenth, or twentieth part, and manifeſtly, with half a quarter of a grain.

With

With ſuch hygroſcopes as theſe (wherein the balance ought to be ſtill kept ſuſpended and charged) I made ſeveral trials, as my removes and accommodations would permit, ſometimes in the ſpring, and ſometimes in the autumn, and ſometimes alſo in the ſummer and winter. But, neverthelefs, it would be very welcome to me, if you, and ſome of your friends, would be pleaſed to make trials yourſelves, and compare them with mine, and eſpecially take notice, if you can, whether, in any reaſonable tract of time, there will be any loſs, worth noting, of the ſubſtance of the ſpunge itſelf; I having not hitherto diſcovered any. In the mean time, to invite you to give yourſelves this trouble, after I have told you, that having once, among divers removes, had the opportunity to keep a drachm of ſpunge ſuſpended during a whole ſpring, and a great part of the preceding winter and ſubſequent ſummer, I did not think my pains loſt, though divers of the obſervations they afforded me have unhappily been ſo, among many other memorials about experiments of differing kinds; notwithſtanding which unſeaſonable loſs, I ſhall venture to ſuggeſt ſome things to you, that occurred to me about the utilities of the inſtruments I am treating of.

A

B R I E F A C C O U N T

OF THE

UTILITIES OF HYGROSCOPES.

THE uſe of a hygroſcope is either general, or particular; the former is almoſt coincident with the qualifications to be wiſhed for, and aimed at, in the inſtrument itſelf; the latter points out the particular applications that may be made of it, when it is duly qualified. Of each of theſe, I ſhall briefly ſubjoin what readily occurs to me.

The general uſe of a hygroſcope is, " To eſtimate the changes of the air, as to " moiſture, and dryneſs, by ways of meaſuring them, eaſy to be known, provided, and " communicated."

I might here pretend, that as theſe are the principal things that have been deſired in hygroſcopes, ſo it is obvious, from the deſcription and account we have given of our inſtrument, that theſe advantages belong to it in no very deſpicable degree: and that to make ſuch hygroſcopes, as will perform all theſe things in perfection, whatever it may ſeem to a mental contriver, will, I fear, prove no eaſy taſk to thoſe that really attempt it. To theſe things I might add, that if ſuch allowances be made, as what I have repreſented may invite you to grant, the qualifications lately mentioned, as deſirable in a hygroſcope, may, in a tolerable meaſure, be found in ours, when we ſhall come to mention the particular uſes of it. And as for that of conveying to others the obſervations made

made with it, you may pleaſe to conſider, that the things I employ to meaſure the degrees of dryneſs and moiſture in the air, being grains, parts of grains, and greater weights, the acceſſion of moiſture which the ſpunge receives, or the loſſes that it ſuffers, can be eaſily, and at the ſame time, both found and determined. And as the weights employed to determine theſe differences are eaſily procurable; ſo the obſervations made with them may (together with patterns, if it ſhould be needful, of the weights themſelves) with the ſame facility be communicated by letters even to remote parts. In which conveniency, whether, and how far our inſtrument has the advantage of that made with an oaten beard, and ſome others that I have employed, I leave you to conſider.

I MIGHT farther alledge, on the behalf of our inſtrument, that whereas, beſides the qualifications above mentioned, there is another, namely, durableneſs, which, though not ſo neceſſary to conſtitute a hygroſcope, yet is neceſſary, as will ere long appear, to ſome of the conſiderableſt uſes of it; and whereas ſuch a durableneſs is wiſhed, as may not only keep the inſtrument from having its ſubſtance rotted or corrupted by the air, but may alſo preſerve it in a capacity to continue pretty uniformly its informations of the air's moiſture, even when that increaſes very much, or laſts very long; whereas, I ſay, theſe things are much deſired in a hygroſcope, our ſpunge ſeems herein preferable to the oaten beard, lute-ſtrings, &c. For in thoſe and the like bodies, the ſelf-contracting, or relaxing power (as it is ſuppoſed) or the diſpoſition to imbibe, and part with the moiſture of the air uniformly, or after a due manner, is wont to be in no very long time altered or impaired; and particularly, when they have imbibed much aërial moiſture, they are very faintly affected by the ſupervening degrees of it, and ſo the operation is too diſproportionate to what the like cauſe would have produced, when the inſtrument was well diſpoſed; whereas, in our ſpunge, neither the degree of ſpringineſs, nor any ſuch-like quality is conſidered, and it is capable of imbibing ſo much more of the aqueous particles, than even moiſt airs and ſeaſons are wont to ſupply it with, that there is little fear that it will be glutted, or have its pores choaked up with them, ſo that the decrements and acceſſions of weight will be more proportionate to the degree of moiſture in the air, and more reducible to known and determinate meaſures.

BUT though theſe, and the like ſpecious things, may be repreſented in favour of our ſtatical hygroſcope; yet, to deal ingenuouſly with you, I much fear, that it will be very difficult to bring either ſtatical ones, or perhaps any other, to be ſo complete, as to ſatisfy a nice and ſevere critick. And you would perhaps eaſily aſſent to my opinion, if it were not too tedious to entertain you with all the ſpeculative doubts and ſcruples, as well mechanical as phyſical, which my accuſtomed diffidence has now and then ſuggeſted to me. But becauſe ſuch a ſceptical diſcourſe would be too tedious, and alſo ſomewhat improper, to be propoſed by one that would recommend hygroſcopes, I ſhall only now take notice of one great imperfection, which all that I have been acquainted with, are liable to; namely, that men have not yet found, nor perhaps ſo much as dreamed of ſeeking a ſtandard of the dryneſs and moiſture of the air, by relation to which hygrometers may at firſt be adjuſted, and ſo be compared with one another, as we ſee many of thoſe ſealed thermoſcopes that have been made and juſtned by Mr. *Shotgrave*, the dextrous operator of the Royal Society. I deny not, that, by virtue of a ſtandard to eſtimate moiſture by, I have endeavoured to remedy this inconvenience; but, as my hopes were but ſmall, ſo neither was my ſucceſs great, but I am not ſure that happier wits, or I myſelf, at ſome other and luckier time, may not more proſperouſly attempt it. In the mean while, perchance, you will not think it altogether nothing, if the trifle I preſent you, perform, at leaſt, ſome of the things deſired in a hygrometer leſs imperfectly than any you have yet met with. And that you may not be diſcouraged by what

what I have lately acknowledged of the defects of ſuch inſtruments, I think it now ſeaſonable to proceed to the mention of the particular uſes, for which, notwithſtanding any inevitable defects, a hygroſcope, and even ſuch a one as I now preſent you, may be made eaſily to ſerve.

U S E I.

To know the differing variations of weather in the ſame month, day, and hour.

It may be uſeful for divers purpoſes to know, both that the air is wont to be leſs moiſt at one part of the artificial day (and ſo of the night) than at any other, and at what particular time of the day or night it moſt uſually is ſo. And on this occaſion I remember, that uſually, when the weather was at a ſtand, it was obſerved that the ſpunge had manifeſtly gained in the night, though it were kept in a bed-chamber, and grew lighter again between the morning and noon. This obſervation, which was made towards the end of winter, would not hold, in caſe froſty nights or ſome other powerful cauſe intervened. It were not amiſs, alſo, to obſerve, whether there be not a correſpondence betwixt the hygroſcope and baroſcope; and if there be, in what kind of weather or conſtitution of air it is moſt or leaſt to be diſcerned. And this inquiry ſeems the more dubious, becauſe the ſame changes of the atmoſphere may, upon differing accounts, have either the like, or quite contrary operations upon theſe two inſtruments. For in ſummer, when the atmoſphere is uſually heavier, the hygroſcope is uſually lighter; ſome ſtrong winds, as with us the north-weſt, may make both the atmoſphere and baroſcope lighter, whereas ſoutherly winds, eſpecially if accompanied with rain, often make the atmoſphere lighter and the ſpunge heavier. And on the other ſide I obſerve, that eaſterly winds, eſpecially when they begin to blow in winter, though, by reaſon of their dryneſs, they are wont to make the hygroſcope lighter, yet they are wont, at leaſt here, at the weſt end of *London*, to make the baroſcope ſhew the air to be heavier. It were likewiſe fit to be obſerved, particularly by thoſe that live on the ſea-coaſt, whether the daily ebbing or flowing of the ſea do not ſenſibly alter the weight of the hygroſcope. It were very well worth while alſo to take notice, at what time of the day or night, *cæteris paribus*, the air is the moſt damp and moſt dry, and not only in ſeveral parts of the ſame day, but in ſeveral days of the ſame month; eſpecially on thoſe days, wherein the full and new moons happen. And this ſeems a more hopeful way of diſcovering, whether the full moon diffuſes a moiſture in the air, than thoſe vulgar traditions of the plumpneſs of oyſters and ſhell-fiſh, and brains in the heads of ſome animals, and of marrow in their bones, and divers other phænomena, which, as I have ſhewn in another paper, it is not eaſy to be ſure of. It may alſo be noted, whether monthly ſpring-tides, eſpecially when they fall out near the middle of *March* or *September*, have any ſenſible operation upon our inſtrument.

U S E II.

To know how much one year and ſeaſon is dryer or moiſture than another.

This cannot be ſo well performed by the hygroſcope made of an oaten beard, if they that have made uſe of them more than I, do complain with reaſon, that after ſome months

months (for I cannot tell you preciſely how many) they begin to dry up and ſhrink; ſo that their ſenſe of the varying degrees of the moiſture of the air is not ſo quick as before, and the informations they give of the degrees of it, eſpecially towards the outmoſt bounds of their power, to ſhew the air's alterations, recede more and more from uniformity. But the laſtingneſs and other convenient qualifications of our ſpunge making its capacity of doing ſervice more durable, may the better help us to compare the greateſt moiſture and dryneſs, both of the ſame ſeaſon, and of the ſeaſons of one year with the correſpondent ones of another. And if the weight of the ſpunge at a convenient time, when the temperature of the air is neither conſiderably moiſt, nor conſiderably dry, be taken for a ſtandard, a perſon, that ſhould think it worth his pains, may, by computing how many days at ſuch an hour, and how much at that hour, it was heavier or lighter than the ſtandard, and alſo by comparing the reſult of ſuch an account in one year with the reſult of the like account in another year, be aſſiſted to make a more particular and near eſtimate of the differing temperature of the air, as to moiſture and dryneſs, in one year than in another, and in any correſpondent ſeaſon or month, aſſigned in each of the two years propoſed. And how much the collation or continuance of ſuch obſervations, both in the ſame place, and alſo in differing countries and climates, may be of uſe to phyſicians in reference to thoſe diſeaſes, where the moiſture and dryneſs of the air has much intereſt; and the huſbandman to foreſee what ſeaſons will prove friendly or unkind to ſuch and ſuch ſoils and vegetables; it muſt be the work of time to teach us, though in the mean while we have no reaſon to deſpair, that the uſes to be made of ſuch obſervations may prove conſiderable. And the rather, becauſe if by help of the reſult of many obſervations men be enabled to foreſee (though at no great diſtance off) the temperature of a year, or even of a ſeaſon, it may advantage not only phyſicians and ploughmen, but other profeſſions of men, who receive much profit or prejudice by the dryneſs or exceſſive moiſture of the ſeaſons. And not to mention thoſe who cultivate hops, ſaffron, and other plants that are tender and bear a great price; ſuch a foreſight, as we are ſpeaking of, may be of great uſe to ſhepherds, who, in divers parts of *England*, are oftentimes much damnified, if not quite undone, by the rot of ſheep, which uſually happens through exceſs of moiſture in certain months of the year. And in order to the providing of foundations whereupon to build predictions, it may not be amiſs to regiſter the number, bigneſs, and duration of the conſiderabler ſpots that may at this or that time of the year happen to appear, or be diſſipated, on or near the ſun, or to take notice of any extraordinary abſence of them, and to obſerve whether their apparition or diſſipation produce any changes in the hygroſcope; which curioſity I ſhould not venture to propoſe, but that (as I elſewhere note) I find, that eminent aſtronomers have caſually obſerved great dryneſſes to attend the extraordinary abſence or fewneſs of the ſolar ſpots. And theſe perſons that are aſtrologically given, may, if they pleaſe, extend their curioſity in the uſe of this inſtrument, to obſerve whether eclipſes of the ſun and moon, and the great conjunctions of the ſuperior planets, have any notable operation upon it.

USE III.

To diſcover and compare the changes of the temperature of the air made by winds, ſtrong or weak; froſty, ſnowy, and other weather.

THIS may conveniently enough be done as to winds, either by our whole inſtruments, or (perhaps better and more ſafely) by the ſpunge alone, which may be taken off and hung by a ſtring, for as long time as is thought fit, in the wind, and then reſtored to its former place. For I found, by removing it into the wind, that it ſoon received a very conſiderable alteration in point of weight, as alſo it did, when removed out of a room into a garden where the ſun ſhined; for though the ſeaſon were not warm, it being then the month of *January*; yet in three quarters of an hour the ſpunge loſt the 24th part of its weight. We may alſo in ſome caſes uſefully ſubſtitute to a ſpunge a ſomewhat broad piece of good ſheeps-leather diſplayed to the wind. For this having, by reaſon of its thinneſs (or very ſmall depth) in proportion to its breadth, a very large ſuperficies immediately expoſed to the wind, we found it to be notably altered thereby, inſomuch, that half an ounce of well prepared ſheeps-leather (that we had long employed as an hygroſcope) being kept an hour in a place, where the ſun-beams might not beat upon it, did, in a ſtrong wind, vary in that ſhort time an eighteenth part of its original weight. But though I think it very poſſible to make ſuch obſervations of the temperature of particular winds, as will frequently enough prove ſo true as to be uſeful, at leaſt to thoſe that live in the places where they are made; yet I am of opinion, that, to be able to ſettle rules any thing general, to determine with any certainty the qualities of winds, according to the corners whence they blow, as from the eaſt or weſt, north-eaſt, ſouth-weſt, &c. there will be a great deal of warineſs required; and he that has not ſome competent ſkill in phyſicks and coſmography, will eaſily be ſubject to miſtakes in forming his rules. To countenance which advertiſement, I ſhall now make uſe but of theſe two conſiderations, whereof the firſt is; that winds that blow from the ſame quarter, are not in ſome countries of the ſame quality that they are in moſt others, the wind participating much of the nature of the region over which it blows in its paſſage to us. At the famous port of *Archangel* they obſerve, that whereas a northerly wind, almoſt every where elſe without the tropicks, produces froſt in winter, there it is wont to be attended with a thaw, ſo as to make the eves to drop. Of which the reaſon ſeems to be, that this wind comes over the ſea, which lies north from that place; and on the contrary, a ſoutherly wind, blowing over a thouſand or twelve hundred miles of frozen land, does rather increaſe the froſt than bring a thaw. This was by the inhabitants averred to the Ruſſian emperor's phyſician, who was more than once at *Archangel*, and from whom I had the account. The northern winds, that are elſewhere wont to be drying, are ſaid in *Egypt* to be moiſt. I remember Mr. *Sandys*, in his excellent Travels, giving an account of what he obſerved about the largeſt of the famed Egyptian pyramids, has this conſiderable paſſage; "Yet this hath been too great a morſel for time to devour, "having ſtood, as may be probably conjectured, about three thouſand and two hundred "years, and now rather old than ruinous: yet the north ſide moſt worn, by reaſon "of the humidity of the northern wind, which here is the moiſteſt." *Sandys* in *Purchas*'s Pilgrimage. And it is yet more conſiderable to our purpoſe what I find related by Monſieur *De Serres*, in his uſeful book of huſbandry, ſince by that it appears, that even in not very diſtant provinces of the ſame kingdom, the winds, that blow from the ſame quarter, may have very differing qualities and effects. For, ſpeaking of the changes of the air, in reference to huſbandry, in ſeveral parts of *France*, he informs us, that it is obſerved, that in the quarters about *Tholouſe* the ſouth wind dries the ground, and the north gives rains. Whereas on the contrary, from *Narbonne* to *Lyons*, all over *Provence* and *Dauphiné*, this laſt named wind cauſes dryneſs, and the other brings moiſture. And this may ſuffice for my firſt conſideration. My ſecond is this, that the vehemence or the faintneſs of the winds, though blowing over the ſame country, may much diverſify its operation on the hygroſcope; and the ſame wind, which, when it blows but faintly, or even moderately, is wont to appear moiſt by the hygroſcope, may, when vehement

Lib. VI. cap. 8. Sect. 3. Theat. d'Agricult. lib I. cap. 7.

vehement or impetuous, make the inſtrument grow lighter, diſcuſſing and driving away more vapours by the agitation of parts it makes in the ſpunge, than is countervailed by thoſe aqueous vapours that are brought along with it. But on ſuch things as theſe I have not leiſure to inſiſt, and therefore I ſhall proceed to take notice, in very few words, of ſome other operations of differing weathers on our inſtrument, and tell you, that froſty weather often made the hygroſcope grow lighter even at night; ſnowy weather, which laſted not long, added ſomething to the weight of the ſpunge. And it has been obſerved, that miſts and foggy weather uſed to add weight to it, even notwithſtanding froſt.

To which may be added an obſervation made by my amanuenſis, who having a convenienter chamber than mine (wherein a fire was daily made) was diligent and curious to ſet down the changes of the hygroſcope that was left in his lodging; for this obſervation makes it probable that a tranſient cloud in fair weather may be (for I ſay not that it always is) manifeſtly obſervable by our inſtrument. For, by his diary, it appears that the ninth of *September*, being for the moſt part a very fair ſun-ſhiny day, though about ten a clock in the morning the ſun ſhone brightly, the ſpunge began to preponderate, which unexpected phænomenon made him look out at the window, where he diſcovered a cloud that darkened the ſun, but after a while, that being paſt, the balance returned to an æquilibrium. On this occaſion I ſhall intimate, that I have more than once or twice obſerved, eſpecially in ſummer, that when the air grew heavier, the hygroſcope either continued at a ſtand, or perhaps, alſo grew lighter; as if, when ſuch caſes happen, the effluvia that get into the air, either from the terreſtrial or ſome other mundane globe, were not fit, like vapours, to enter and lodge in the pores of the ſpunge, and ſo were corpuſcles of another nature, with which, when we find by the baroſcope that the air is plentifully ſtocked, it may be worth while to obſerve, whether any, and if any, what kind of meteor, as wind, or rain itſelf, or hail, or in the winter ſnow or froſt, will commonly be ſignified and produced.

U S E IV.

To compare the temperature of differing houſes, and differing rooms in the ſame houſe.

As this is of great uſe, both in reſpect of men's health, eſpecially if they be of a tender, or ſickly conſtitution, and in reſpect of conveniency for the keeping fleſh, ſweet-meats, and ſeveral ſorts of wares and goods, and even houſehold-ſtuff, that are ſubject to be indamaged by moiſt air; ſo it is readily and manifeſtly derivable from our inſtrument. For, by removing it into ſeveral houſes, or into ſeveral parts of the ſame houſe, and letting it ſtand in each a competent time, to be affected with the temperature of the air of that particular place, we have divers times obſerved a notable difference, as you may gueſs by the two or three notes I met with among ſome old papers.

October 13. [THREE or four days ago, a piece of fine ſpunge being taken out of a cabinet, and clipped, till it came to weigh juſt half a drachm in a nice pair of ſcales, and a warm room, was afterwards removed into a neighbouring room deſtitute of a chimney (and yet within three or four yards of a chimney ſeldom without fire); this ſtatical hygroſcope, conſiſting of the ſcales and the frame they hung on, was yeſterday night removed into the former room, and the ſpunge was found to have gained three grains and $\frac{1}{8}$, or better, and conſequently more than a tenth part, in reference to its firſt weight; but being ſuffered to ſtand in this warm room, in leſs than twelve hours it loſt a grain

 and

and about $\frac{1}{8}$ of its former weight, though the time it ſtood in this room were, for the moſt part, night and rainy weather.]

[We took a piece of very fine ſpunge, which formerly had weighed juſt a drachm, but having been many months kept in a very warm room, where fires were kept every day, it was grown much lighter; for, removing it into an upper chamber in a neighbouring houſe, and weighing it in tender ſcales, in the evening it was found to want of a drachm, four grains, and $\frac{1}{4}$ of a grain; and though there was a fire in the room, and the ſcales ſtood not far from it, yet, in a ſhort time (the day being foggy and rainy) the ſpunge viſibly depreſſed its ſcale $\frac{3}{8}$, and the next morning was found to want but one grain and a half of a drachm, ſo that it had gained about three grains and a quarter, and the following evening, being the ſecond of *January*, it weighed one drachm, a grain, and almoſt half a grain. So that in about one natural day the ſpunge had acquired ſix grains from the moiſture of the air, that is, a tenth part of its firſt weight (I mean a drachm) and a greater proportion in reference to the weight it had the day before. The third of *January*, the weather being yet moiſt, the weight exceeded two grains, but about three or four of the clock in the afternoon it began to loſe of that great weight, which diminiſhed more by the next morning, the weather having changed that night, and become ſomewhat froſty.]

In another paper I alſo find this note. [The drachm of a ſpunge, that had for divers weeks been kept in a dry room, was (*January* the tenth) carried out into a room, where fire is not wont to be kept, the weather being extraordinarily foggy; this morning, being brought into the former room, though now the weather be clear (yet not froſty) it appears to have gained in weight about eleven grains; yet it ſoon loſt two grains by ſtanding in this room all the while in the balance.]

USE V.

To obſerve in a chamber the effects of the preſence or abſence of a fire in a chimney or ſtove.

This is eaſily done, and the more eaſily, if the room be ſmall. For in ſuch chambers, I have often obſerved a moderate fire to alter the weight of the inſtrument placed at a diſtance from it, after it had been well kindled but a very little while; but in wet weather, if the fire were not ſeaſonably renewed with freſh fuel, the decay of it would, in no long time, begin to be diſcernible by the inſtrument.

USE VI.

To keep a chamber at the ſame degree, or at an aſſigned degree of dryneſs.

Supposing the alteration of weight in our ſpunge to depend only upon the degree of the moiſture of the air, the laſt named uſe will be but an obvious corollary of the former. For, if a convenient part of the room be choſen for the hygroſcope, and it be kept conſtantly there, it is eaſy, by caſting one's eye on it from time to time, to perceive when it will be requiſite to increaſe or moderate the fire, ſo as to keep the ſpunge at that weight it was of, when the temperature of the air of the chamber, as to dryneſs and moiſture, was ſuch as was deſired. I will not trouble you with ſome ſcruples, which, I confeſs, the

the consideration of this use of our instrument suggested to me, because I have not now the leisure to discuss them. I had thoughts to try, whether, and how far a good quantity of salt of tartar, or even dried sea-salt, being kept in a closet or some closer room, might, by imbibing, lessen the moisture of the air in it, but I did not perfect any observation of this kind. But I will add to what I have already referred to this sixth head, that I have sometimes noted with pleasure, how manifest and great a change in the weight of our spunge would be made when the room was washed, and a good while after, notwithstanding that a good fire was kept in it, to hasten the drying of it.

Besides the hitherto mentioned uses of our hygroscope, I know not, whether there may not be divers others, and whether we may not, by a little altering and helping it, make it capable of shewing us some difference betwixt steams of differing natures, as those of water, spirit of wine, chymical oils, and perhaps new kinds of substances (such as we have not yet taken notice of) in the air, in which I confess, I suspect there may sometimes be dispersed store of corpuscles, that I do not yet well know what to think of. For I have more than once observed (not without some wonder) the hygroscope not to be affected with the alteration of weather, answerably to what the manifest constitutions or variations of it seem plainly to require; whether unobserved corpuscles performed this, by making the other steams in point of figure, or size, incongruous to the minute pores of the spunge, and so unfit to enter them; or by dissipating, or otherwise procuring the avolation of more of the watery particles than they could countervail, I now examine not. And I am not sure, but by associating this instrument with the thermoscope, baroscope, and some others that may be proposed, it might be so improved, as to help us to foresee divers considerable things, that either are themselves changes of the air, or are wont to be consequences of them; as sickly and healthful constitutions of the air, both as to man and cattle; and healthful, barren, or plentiful seasons in particular places or countries; and perhaps also strong hurricanes, earthquakes, inundations, and their ill effects, especially those accidents that depend much upon the surcharge of the air, with other exhalations and moist vapours, which operate before sensibly upon our instrument, and therefore may be discernible by it a good while before they arrive at that height, that makes them formidable meteors. And if it were but the foretelling approaching rain, this very thing may, on divers occasions, prove very serviceable, and recommend our instrument, which often receives much earlier impressions from the steams that swim up and down in the air, than our senses do; so that I have been able to foresee a shower of rain, especially in dry weather, a not inconsiderable while before it fell.

And here I should dismiss our subject, which I have already dwelt on longer than I designed, but that remembering a caution I gave you, when I was speaking of winds, I think it but fit to add two or three lines, to keep you from being by that advertisement discouraged from endeavouring to make, in the general, such hygroscopical observations, as may be reduced to hypotheses. For, as I elsewhere discoursed concerning barometrical theories, if I may so call them; so I shall here represent, concerning hygroscopical ones, that if a theory or hypothesis that is itself rational, be found agreeable to what happens the most usually in observation; it ought not lightly to be rejected, or so much as laid aside, though sometimes we find particular instances that seem to call it in question. For it is very possible that the theory or hypothesis may be as good as a wise man would require about so mutable a subject as the weather. And the cause assigned by the hypothesis may really act suitably to what that requires, though a contrary effect ensue by reason of that cause's being accidentally mastered and over-ruled by some more powerful cause or agent, that happens for that time to invade the air. As we know that tides do, for the main, correspond with the motions of the moon (whose phases are therefore argued from

See Use III.

from them) and do generally ebb and flow at ſuch times, and in ſuch meaſures, as the theory that has been grounded on that correſpondency requires; but yet ſeamen find that in this or that particular harbour, or mouth of a river, fierce contrary winds, great land-floods, and other caſually intervening cauſes, do ſometimes both very much diſturb the regular courſe of the tides, and encreaſe or leſſen them.

A NEW EXPERIMENT AND OTHER INSTANCES OF THE EFFICACY OF THE AIR's MOISTURE.

Subjoined by Way of

APPENDIX to his STATICAL HYGROSCOPE.

SINCE it may probably recommend hygroſcopes to you, if that quality of the air, which theſe inſtruments are uſeful to give us an account of, be made appear to be more powerful, and have conſiderabler effects than is commonly believed; it will not be from my purpoſe to preſent you here ſome inſtances, that have led me to think that the effects of the moiſture of the air may be conſiderable, not only upon men's healths, but upon ſubjects far leſs tender, and leſs curiouſly contrived than human bodies. But I hope you will eaſily believe, that by the moiſture of the air, I mean not a mere and abſtracted quality, but moiſt air itſelf, or rather thoſe humid corpuſcles (chiefly of an aqueous nature) that abound, and rove to and fro in our common air.

THAT the moiſture of the air may have no ſmall influence, and uſually a bad one, upon men's healths, is that, which, though experience did not ſo often teach us, I ſhould venture to argue from what I have obſerved of the operation of moiſt air upon the dry and firmly contex parts of animal, and even in thoſe caſes, where, for want of time or other impediments, this moiſture cannot produce any ſenſible degree of putrefaction.

THAT the ſkins of animals may be eaſily invaded by the moiſt particles of the air, is the more probable, becauſe of the numerouſneſs of their pores, which may be concluded from

from their hairineſs, or their ſweat, or both. And I formerly obſerved to you, that I found ſheeps leather to imbibe the moiſture of the air, and increaſe in weight upon it, as plentifully as almoſt any body I expoſed to it.

But to ſhew you, that much cloſer membranes, and which nature made to be impervious to ſuch a liquor as urine itſelf, may be affected by the vapours of the air, I ſhall add, that having purpoſely taken pieces of bladders, fine and well blown, and, as far as appeared, of a very cloſe contexture, and counterpoiſed them in a good balance, I found, according to expectation, that they would conſiderably increaſe their weight in moiſt, and loſe it again in dry weather; ſo that I might have employed the moſt membranous part of a bladder (for I thought not fit to make uſe of the neck or the adjoining part) to make a ſtatical hygroſcope.

And as for other membranes and fibres, I ſhall have by and by occaſion to take notice, that even when they are ſtrongly and artificially wreathed together into gut-ſtrings, they may imbibe enough of the moiſture of the air to be broken by it. And I remember, I formerly told you, that I had obſerved lute-ſtrings to grow heavier in moiſt air.

And whereas bones are by all confeſſed to be the firmeſt and ſolideſt parts of animals, and, as it were, the pillars by which the fabrick is ſuſtained; yet it ſeems, that even they may be pierced into, and ſenſibly affected by the moiſture of the air. For I remember, that having cauſed the ſkeleton of a human body to be ſo made by a famous and very ſkilful artiſt, that, by the help only of ſlender wires artificially ordered, the motions which the muſcles make of the bones of a living body, might be well imitated in the ſkeleton, I obſerved, that though in dry and fair weather the flexures of the limbs might be readily made, yet in very moiſt weather the joints were not eaſily bent, as if the parts were grown ſtiff and rigid; which ſeemed to proceed hence, that moiſt particles of the air, having plentifully inſinuated themſelves at the pores into the bones, had every way diſtended them, and thereby made the parts bear hard againſt one another (which they did not at all before) at the junctures or articulations.

But it will be the more readily believed that the moiſture of the air may operate conſiderably upon the tender and curiouſly contrived bodies of men and other animals, if, proceeding to the obſervations I chiefly deſign, I make it appear that the moiſtening particles that rove up and down in the air, are able to exerciſe a notable (and, if I may ſo call it, a mechanical) force, even upon inanimate and inorganical bodies; which may well ſuggeſt a ſuſpicion, that hygroſcopes being the proper inſtruments to diſcover a quality in the air, whoſe efficacy reaches farther than is commonly taken notice of, they may in time be found uſeful to divers other purpoſes, beſides thoſe that relate to the health of men.

That wood, eſpecially when it has been ſeaſoned, is a ſolid of a ſtrong and firm contexture, if it were not obvious by the daily uſe made of it in building ſhips, houſes, &c. might be eaſily concluded from the weight or force required to alter its contexture, by making any conſiderable, or, perhaps, ſenſible compreſſion of it. And yet that wood may ſuffer a kind of divulſion of the multitude of its parts, and be manifeſtly diſtended by aqueous corpuſcles getting into its pores, I remember, I proved by this experiment. I got a piece of ſound and ſeaſoned wood of about an inch (or an inch and half) in diameter, to be by a ſkilful artiſt made cylindrical, and alſo a ring of ſome ſolid matter, as braſs or ivory, to be exactly turned to fit this cylinder, ſo that it might, without much eaſe, or much difficulty, be put on and taken off again; then we put the turned piece of wood into fair water, and left it to ſoak there for many hours; at the end of which it was viſibly ſwelled; and though I cannot now tell you (for want of a paper concerning that experiment) how much it was increaſed in diameter, yet I well remember, the

the increment was considerable, and that the ring that was adjusted to it before, was manifestly too little to be put again upon it, or with its orifice to cover the whole basis of the distended cylinder, which afterwards being dried in the air shrunk into a capacity of entering the ring again. And in this experiment I took notice, that the great intumescence of the wood was not produced all at once, or soon after it was put into water, but it swelled by degrees, and lay soaking there many hours before it arrived at its utmost distention, the aqueous corpuscles requiring, it seems, so much time to insinuate themselves sufficiently into the wood; which argues that the internal parts were likewise affected, though, when even they came to swell, they had a good thickness of wood about them to hinder their dilatation.

I EXPECT you should now tell me that this distention of so firm a body was made by water itself, and not by the humid vapours of the air. On which occasion I might represent to you, that by the sweating (as men commonly call the adhesion of waterish drops to the surface) of polished marble and some other cold and smooth bodies that sometimes happens even in the heat of summer, if they be cold, and the ambient air be moist enough, it appears, that both in hot weather the air may be plentifully stocked with aqueous vapours, and that these vapours need to do no more than convene together, to constitute visible and tangible water. And on this occasion, if I were sure I had not told you of it already, I should subjoin an experiment, which would detect the vulgar error of those that think the adhering drops, lately mentioned, to come from internal moisture derived by its pression or percolation from the marble or the other body they are fastened to; and at the same time I shall shew (what is not wont to be imagined) that in the heat of summer the air is furnished with invisible and yet aqueous steams. The experiment I long since tried in winter with snow and salt included in a glass vessel, and then put to dissolve in a balance. But because neither ice nor snow is at all easy to be come by among us in *England* in summer; and because, at that season, the air in fair weather is presumed to be dry as well as hot, I chose, within some days of Midsummer, and in clear sun-shiny weather, to make the following trial.

WE took a pint glass-bottle, and having put into it a convenient quantity of water (for room must be left for the salt) we placed them and four ounces of beaten sal armoniack in one scale of a good balance, and a counterpoise in the other; and then, putting the salt into the water, I observed, that though for a while the æquilibrium remained, yet when the frigorifick mixture had sufficiently cooled the outside of the bottle, the roving vapours of the air that chanced to pass along the surface of the vessel, were, by the contact of that cold body, arrested, and turned into a kind of a dew, which from time to time made the scale that held the glass preponderate more and more, and at length the drops growing greater and greater, ran down in small rivulets the sides of the glass, and in less than an hour, by my estimate, the condensed steams amounted to near a drachm, which weight was afterwards much increased within about two hours more; whereby it sufficiently appears, both that this dew came from without (since if it had been a transudation, it would not have added weight to the scale that received it) and that there is, even in clear summer weather, a vast number of moist particles dispersed through the air, since in about an hour's time, such a multitude of them, as the liquor produced, may be supposed to consist of, and may by heat be actually resolved into, could in course come to touch so small a surface as that of that part of so small a bottle, which contained the frigorifick mixture. For the rest of the vessel's surface was not cold enough to condense the vapours into liquor. But to return to what we were saying of wood swelled by water; because, notwithstanding these considerations, I am willing to allow that the experiment of the cylinder does not fully come home to our purpose, and

that

then I produced it not ſo much to prove directly the force of moiſt air, as to countenance what I am about to ſay, by ſhewing what a ſufficient number of aqueous corpuſcles may do in the ſolid wood they penetrate; I ſhall now add ſome inſtances of the force theſe particles may exerciſe upon ſolids, when they invade them but in the form of vapours.

That in this form the multitude, figures, and motions of theſe inſinuating particles may enable them to diſplay no ſmall force in their operations on ſome bodies, we have one inſtance, that often happens, though but ſeldom reflected on, in the breaking of the ſtrings of muſical inſtruments firſt brought to a good tenſion, upon the ſupervening of rainy weather. For the cauſe ſeems to be, that the vapours that then wander through the air, inſinuating themſelves into theſe ſtrings (which the muſician often forgets to let down or relax after having ſkrewed them up) diſtend and ſwell them, and thereby endeavour to ſhorten them, and that ſo forcibly, that they not ſeldom break with a ſmart noiſe and great violence; which, becauſe it happens without any viſible efficient, men commonly think and ſay that ſuch ſtrings break of themſelves. But to take no farther notice of this popular ſurmize, if we conſider how much weight ſome of thoſe bigger ſtrings, eſpecially of baſe viols, that have been obſerved to break in rainy weather, will require to ſtretch any of them to a rupture, you will eaſily be induced to think that this operation of the moiſt air exacts, and therefore argues more than a languid force.

But here, probably, you will tell me, that the inſtances you expected were concerning wood, which is a far ſolider body than gut-ſtrings. To this I ſay, that the newly-recited inſtance belongs directly to the title of this paper, and being above referred to, ought not to be pretermitted. And as to your expecting inſtances concerning wood, I might content myſelf to refer you to what is obſerved about the uneaſy opening and ſhutting ſome doors, well adjuſted to the door-caſe, in very rainy weather. But though this obſervation favours my deſign, yet I had rather give you inſtances in wood, purpoſely and carefully ſeaſoned. And therefore I ſhall now inform you of theſe two things; one, that I found by trial, as I have elſewhere noted, that wood, counterpoiſed in a good balance, would grow ſenſibly heavier in wet weather, and lighter again in dry; and the other, that, to ſatisfy myſelf yet further, I conſulted an ancient muſician to whom I had once been a diſciple, and a famous organ-maker, to know, whether they had not obſerved, that the wood itſelf, &c. of muſical inſtruments, would receive ſuch alterations from the moiſture of the air, as might be diſcerned by the ear? upon which inquiries, the maſter of muſick anſwered me, that though metalline ſtrings will not change with the weather like gut-ſtrings; yet virginals (for inſtance) though furniſhed with wire-ſtrings, will, for the moſt part of them (for ſome he has obſerved to be ſo well ſeaſoned that they are not altered by the weather) be out of tune in wet weather, the ſtrings generally then affording their notes ſharper than they ſhould, or are wont to do. And the organ-maker confeſſed to me, that, upon great changes of weather, divers organs would (after they had been long ago tuned) grow out of tune, and that not only the wooden pipes would be thereby ſwelled, but the metalline pipes untuned.

But if bodies be of ſuch a conſtitution, as not only to admit but aſſiſt the operation of the moiſt air, the penetrancy and efficacy of this may be found much more conſiderable than in the foregoing inſtances. For there are ſome kinds of thoſe marchaſites that yield vitriol, which, whilſt they lie under ground, or are covered with the ſea-water, on whoſe ſhores they are, in ſome places, to be found, retain a ſtone-like hardneſs, and are often taken for mere ſtones; and yet ſome credible perſons that are converſant about vitriol have caſually obſerved, that theſe, being expoſed to the air, would, in tract of time, be ſo penetrated by the moiſt particles of it, though perhaps not merely as moiſt, that (probably by the help of the vitriolate corpuſcles they met with among the ſtony matter) theſe hard and ſolid marchaſites are brought to ſwell ſo much as to burſt. That this will happen to ſuch kind of ſtones (though they be of a cloſe and heavy nature) by the help of rain, experience has perſuaded me; and that it may alſo happen even to very hard and ſtone-like marchaſites (for many are not ſuch) when they are merely expoſed to the air, I am apt to think, upon ſome trials of my own. For, from ſhining marchaſites, though but kept in my chamber-window, I have had vitriolate efflorèſcences that ſeemed to be produced by the action of the piercing moiſture of the air upon the mineral. And I remember, that very hard and heavy lumps that were of a marchaſitical ſubſtance, though not at all gliſtering, which ſeemed to be ſtony, were ſo diſpoſed to be wrought on by the air, that though they were kept partly in my own chamber, and partly in other covered places, yet in no very long time they were ſo penetrated by the moiſt corpuſcles of the air, that they were not only burſt, but broken into many pieces; inſomuch that many of them did of themſelves fall off from one another, and ſeveral of the divided portions would eaſily be crumbled betwixt one's fingers. And of ſome of theſe I have obſerved with pleaſure, that a vitriolate ſubſtance was produced more copiouſly in their innermoſt parts, than on or near their outſide. So that, when I conſidered how great an external force would have been requiſite to make ſuch a comminution of minerals ſo ſolid and hard, it was obvious for me to look upon the air's moiſture, as capable, when it meets with fitly-diſpoſed bodies, to exerciſe a far greater force than is wont to be conceived.

To theſe phænomena I might add ſome others to the ſame purpoſe; but becauſe the marchaſites and other bodies required to the producing of them, are not eaſy to be come by, and the ſucceſs often exacts a good length of time, I ſhall conclude this paper, by ſubjoining a far ſhorter experiment, that I deviſed not only to ſhew, in general, that the moiſture of the air may have a conſiderable efficacy, but to aſſiſt a virtuoſo to make ſome eſtimate in known meaſures of the mechanical force of the aërial moiſture. And though I now find, to my trouble, that I want ſome of the notes that concern the circumſtances, and the progreſs of the trial, yet enough having eſcaped to furniſh me with the following account of it, what I ſhall ſet down, may, I hope, at leaſt put you in the way of repairing my misfortune.

Thinking it then probable that ropes themſelves would conſiderably imbibe and diſmiſs the moiſture of the air, and that ſo, as to ſhrink in rainy weather, though clogged with a weight faſtened at the lower end, I was diſcouraged from attempting the following trial, by conſidering that the weight would ſtretch the rope, and conſequently hinder the preſumed effect of the air's moiſture to be perceived. For I ſuppoſed, that after a time, this unuſual ſtretch of the rope would ceaſe; and when the weight, as ſuch, could not lengthen it any more, it would then be capable of being contracted or relaxed, according as the weather ſhould be moiſt or dry, and ſo afford me a kind of hygroſcope. Upon theſe grounds I firſt cauſed a rope that was about twenty or twenty-two yards in length, but of no great thickneſs, to have one of its ends faſtened to an immoveable body at a convenient height from the ground, and then cauſed a pully to be ſo faſtened to another ſtable body, at the diſtance of eighteen or twenty yards from the firſt, that the rope, reſting upon the pully, lay almoſt horizontally. But to the end of that part of the rope, which from the pully reached within two or three feet of the ground, was faſtened, by a ring, a leaden weight of at leaſt fifty pounds. To which was alſo faſtened a light index placed horizontally, whoſe end moved along an erected board, which, by tranſverſe lines, was divided into inches and parts of inches, reaching both a good way upwards and down-

downwards, that the index might, within thoſe bounds, have room to play up and down, according to the alterations of the weather.

It being then ſummer, this trial was made in a garden, though partly under a pent-houſe, that the rope might be more expoſed to the air, than it would have been within doors; and two or three days, if I miſ-remember not the time, were ſpent before the weight had brought the rope to the utmoſt ſtretch it was able to give it, after which it began manifeſtly to ſhrink, and lengthen according to the weather. And I find, in one of my notes, that once I looked, when I was ready to go to bed, upon the ſuſpended weight, and marked how low it reached upon the divided board; and that a great part of the night having been rainy, looking again about an half an hour after eight in the morning, I found the cord ſo ſhrunk that the weight was raiſed above five inches, and yet the day growing dry and windy, and ſometimes warm, the weight had at night ſtretched the rope more than the moiſture had contracted it the day before.

Afterwards having procured a far greater weight, but therefore unapt to be near ſo much raiſed, I ſubſtituted it in the place of that formerly mentioned; and having ſuffered it to ſtretch the rope as far as it could, I made and regiſtered ſome obſervations, two whereof having been preſerved, I ſhall tranſcribe them juſt as I find them.

June the 4th. At half an hour after nine of the clock at night, I looked upon the hundred pounds weight that hung at the bottom of the rope, the weather being then fair, and a mark being put at that part of the erected board where the bottom of the weight touched; I perceived the ſky a while after to grow cloudy and overcaſt, but without rain; wherefore, going to view the weight again, I found it to be riſen a quarter of an inch, or more, and looking on my watch, perceived there had paſſed an hour and quarter ſince the mark was made.

June the 6th. Being not well yeſterday, the weight was obſerved by two of my ſervants, and it then reſted at the eleventh inch of the erected board. This morning, about eight of clock I viſited it myſelf, and found it to be riſen about half a quarter of an inch above the eighth inch, the morning being cloudy, though the ground very dry and duſty. The weather being more overcaſt, within ſomewhat leſs than an hour afterwards, I viſited the weight again (ſome ſcattered drops of rain then beginning to fall) and found it to be riſen about half an inch above the newly-mentioned eighth mark. How much more the rope would have been contracted in ſuch laſting moiſt weather as uſually happens in winter, I cannot ſay, having been reduced to break off the experiment, upon a removal I was, long before that ſeaſon, obliged to make.

I am ſorry I cannot add my other obſervations; but theſe I hope may ſuffice to let you ſee that the force of the air's moiſture is not ſmall, ſince it could raiſe ſuch a weight as an hundred pounds, eſpecially conſidering the ſlenderneſs of the rope it affected. For, having meaſured the diameter near the weight, I found it (as one of my notes informs me) to be but about the † third part of an inch.

† It was $\frac{3}{10}$ and 4 decimal parts of $\frac{1}{10}$

The End of the Third Volume.