THE

W O R K S

OF THE HONOURABLE

ROBERT BOYLE.

In SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE FOURTH.

A NEW EDITION.

H. Gravelot inv. et delin.

L O N D O N:

Printed for W. Johnston, S. Crowder, T. Payne, G. Kearsley, J. Robson, B. White, T. Becket and P. A. De Hondt, T. Davies, T. Cadell, Robinson and Roberts, Richardson and Richardson, J. Knox, W. Woodfall, J. Johnson, and T. Evans.

MDCCLXXII.

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MDCCLXXII.

THE

EXCELLENCY of THEOLOGY,

COMPARED WITH

NATURAL PHILOSOPHY.

(As both are OBJECTS of MEN'S STUDY.)

Difcourfed of in a LETTER to a FRIEND.

TO WHICH ARE ANNEXED

Some Occafional Thoughts about the Excellency and Grounds of the Mechanical Hypothefis.

The Publifher's Advertifement to the Reader.

WHEN I fhall have told the reader, that the following difcourfe was written in the year 1665, while the author, to avoid the great plague, that then raged in *London*, was reduced, with many others, to go into the country and and frequently to pafs from place to place, unaccompanied with moft of his books; it will not, I prefume, be thought ftrange, that in the mention of fome things taken from other writers, as his memory fuggefted them, he did not annex in the margent the precife places, that are referred to. And upon the fame fcore, it ought not to feem ftrange, that he has not mentioned fome late difcoveries and books, that might have been pertinently taken notice of, and would well have accommodated fome parts of his difcourfe; fince things, that may thus feem to have been omitted, are of too recent a date to have been known to him when he writ. But if it he demanded, why then a difcourfe finifhed fo long ago, did not come abroad much fooner? I muft acquaint the reader, that it was chiefly his real concern for the welfare of the ftudy he feems to depreciate, that kept thefe papers fo long by him. For he refifted for feveral years the defires of perfons, that have much power with him, and fuppreffed the following difcourfe, whilft he feared it might be mifapplied by fome enemies to experimental philofophy, that then made a noife againft it, without fuffering thefe papers to come abroad, till the addreffes and encomiums of many eminent foreign virtuofi, and their defire to be admitted into the Royal Society, had fufficiently manifefted, how little its reputation was prejudiced, or like to be endangered, by the atttempts of fome envious or mifinformed perfons. And to this reafon muft be added the author's backwardnefs to venture abroad a difcourfe of an unufual nature, on which account, among others, he declined to have his name prefixed to it; though now the book is printed, he finds caufe to fear, that it will not be long concealed; fince he meets with fome marginal references to other tracts of his, which (thefe papers having long lain by him) he forgot to have been fet down for private ufe, and which fhould not have been expofed to public view.

The AUTHOR's PREFACE.

I AM not fo little acquainted with the temper of this age, and of the perfons, that are likelieft to be perufers of the following tract, as not to forefee it to be probable enough, that fome will afk, for what reafon a difcourfe of this nature was written at all, and that others will be difpleafed, that it has been written by me.

THOSE, that would know, by what inducements my pen was engaged on this fubject, may be in great part informed by the epiftle itfelf, in divers places whereof, as efpecially about the beginning, and at the clofe, the motives, that invited me to put pen to paper, are fufficiently expreffed. And though feveral of thofe things are peculiarly applied, and (if I may fo fpeak) appropriated to the perfon the letter is addreffed to; yet that under-valuation, I would diffuade him from, of the ftudy of things facred, is not his fault alone, but is grown fo rife among many (otherwife ingenious) perfons) efpecially ftudiers of phyficks, that I wifh the enfuing difcourfe were much lefs feafonable than I fear it is.

BUT I doubt, that fome readers, who would think a difcourfe of this nature needlefs or ufelefs, may yet not be pleafed at its being written by one, whom they imagine the acceptance his endeavours have met with, ought to oblige to fpend his whole time in cultivating that natural philofophy, which in this letter he would perfuade to quit the precedency, they think it may well challenge, before all other forts of learning.

I am not unfenfible of the favourable reception, that the philofophical papers, I have hitherto ventured abroad, have had the happinefs to receive fromthe curious: but I hope, they will not be difpleafed, if I reprefent, that I am no lecturer, or profeffor of phyficks, nor have ever engaged myfelf, by any promife made to the publick, to confine myfelf never to write of any other fubject; nor is it reafonable, that what I did, or may write, to gratify other men's curiofity, fhould deprive me of mine own liberty, and confine me to one fubject; efpecially, fince there are divers perfons, for whom I have a great efteem and kindnefs, who think they have as much right to folicit me for compofures of the nature of this, that they will now have to go abroad, as the virtuofi have to exact of me phyfiological pieces. And though I be not ignorant, that, in particular, the following difcourfe, which feems to depreciate the ftudy of nature, may, at firft fight, appear fomewhat improper for a perfon, that has purpofely written to fhew the excellence and ufefulnefs of it; yet I confefs, that upon a more attentive confideration of the matter, I cannot reject, no, nor refift their reafons, who are of a quite differing judgment.

AND 1. My condition, and my being a fecular perfon (as they fpeak) are looked upon as circumftances, that may advantage an author, that is to write upon fuch a fubject as I have handled. I need not tell you, that as to religious books in general, it has been obferved, that thofe penned by lay-men, and efpecially gentlemen, have (*cæteris paribus*) been better entertained, and more effectual, than thofe of ecclefiafticks. And indeed it is no great wonder, that exhortations to piety, and diffuafions from vice, and from the lufts and vanities of the world, fhould be the more prevalent for being preffed by thofe, who have, and yet decline, the opportunities to enjoy plentifully themfelves the pleafures they diffuade others from. And (to come yet clofer to our prefent purpofe) though I will not venture to fay with an excellent divine, that whatever comes out of the pulpit, does with many pafs but for the foolifhnefs of preaching; yet it cannot well be denied, but that if all other circumftances be equal, he is the fitteft to commend divinity, whofe profeffion it is not; and that it will fomewhat add to the reputation of almoſt any ſtudy , and conſequently to that of things divine, that it is praiſed and preferred by thoſe, whoſe condition and courſe of life exempting them from being of any particular calling in the common-wealth of learning, frees them from the uſual temptations to partiality to this or that ſort of ſtudy, which others may be engaged to magnify, becauſe it is their trade or their intereſt, or becauſe it is expected from them; whereas theſe gentlemen are obliged to commend it, only becauſe they really love and value it.

But there is another thing, that ſeems to make it yet more fit, that a treatiſe on ſuch a ſubject ſhould be penn'd by the author of this: for profeſſed divines are ſuppoſed to be buſied about ſtudies, that even, by their being of an higher, are confeſſed to be of another nature, than thoſe, that treat of things corporal. And ſince it may be obſerved, that there is ſcarce any ſort of learned men, that is more apt to undervalue thoſe, that are verſed only in other parts of knowledge, than many of our modern naturaliſts, (who are conſcious of the excellency of the ſcience they cultivate,) it is much to be feared, that what would be ſaid of the pre-eminences of divinity above phyſiology, by preachers (in whom the ſtudy of the latter is thought either but a preparatory thing, or an excurſion) would be looked upon as the deciſion of an incompetent, as well as intereſted judge; and their undervaluations of the advantages of the ſtudy of the creatures would be (as their depreciating the enjoyment of the creatures too often is,) thought to proceed but from their not having had ſufficient opportunities to reliſh the pleaſures of them. But theſe prejudices will not lie againſt a perſon, who has made the indignation of nature ſomewhat more than a parergon, and having, by a not-lazy, nor ſhort enquiry, manifeſted, how much he loves and can reliſh the delight it affords, has had the good fortune to make ſome diſcoveries in it, and the honour to have them publickly, and but too complimentally, taken notice of by the virtuoſi. And it may not be impertinent to add, that thoſe, who make natural philoſophy their miſtreſs, will probably, be the leſs offended to find her in this tract repreſented, if not as an handmaid to divinity, yet as a lady of a lower rank; becauſe the inferiority of the ſtudy of nature is maintained by a perſon, who, even whilſt he aſſerts it, continues, if not a paſſionate, an aſſiduous courter of nature: ſo that as far as his example can reach, it may ſhew, that as on the one ſide a man need not be acquainted with, or unfit to reliſh the leſſons taught us in the book of the creatures, to think them leſs excellent than thoſe, that may be learned in the book of the ſcriptures; ſo on the other ſide, the preference of this laſt book is very conſiſtent with an high eſteem and an aſſiduous ſtudy of the firſt.

And if any ſhould here object, that there are ſome paſſages, which I hope are but very few that ſeem a little too unfavourable to the ſtudy of natural things; I might alledge for my excuſe the great difficulty, that there muſt be in comparing two ſorts of ſtudies, both of which a man much eſteems, ſo to behave one's ſelf, as to ſplit a hair between them, and never offend either of them: but I will rather repreſent, that in ſuch kind of diſcourſes, as the enſuing, it may juſtly be hoped, that equitable readers will conſider, not only what is ſaid, but on what occaſion, and with what deſign it is delivered. Now it is plain by the ſeries of the following diſcourſe, that the phyſeophilus, whom it moſt relates to, was by me looked upon as a perſon, both very partial to the ſtudy of nature, and ſomewhat prejudiced againſt that of the ſcripture; ſo that I was not always to treat with him, as with an indifferent man, but according to the advice given in ſuch caſes by the wiſe, I was (to uſe *Ariſtotle*'s expreſſion) to bend the crooked ſtick the contrary way, in order to the bringing it to be ſtrait, and to depreciate the ſtudy of nature ſomewhat beneath its true value, to reduce a great over-valuer, to a juſt eſtimate of it. And to gain the more upon him, I allowed myſelf now and then to make uſe of the contempt he had of the peripatetick and vulgar philoſophy, and in ſome paſſages to ſpeak of them more ſlightingly, than my uſual temper permits, and than I would be forward to do another occaſion; that, by ſuch a complaiſance for his opinions, I might have riſen to argue with him from them.

But to return to the motives, that were alledged to induce me to the publication of theſe papers, though I have not named them all, yet all of them together would ſcarce have proved effectual, if they had not been made more prevalent by the juſt indignation I conceived, to ſee even inquiſitive men depreciate that kind of knowledge, which does the moſt elevate, as well as the moſt bleſs, mankind, and look upon the nobleſt and wiſeſt employments of the underſtanding, as ſigns of weakneſs in it.

It is not, that I expect, that whatever can be ſaid, and much leſs what I have had occaſion to ſay here, will make proſelytes of thoſe, that are reſolved againſt the being made ſo, and had rather deny themſelves the excellenteſt kinds of knowledge, than allow, that there can be any more excellent, than what they think themſelves maſters of: but I deſpair not, that what is here repreſented, may ſerve to fortify in a high eſteem of divine truths thoſe, that have already a juſt veneration for them, and preſerve others from being ſeduced by injurious, though ſometimes witty inſinuations, to undervalue that kind of knowledge, that is as well the moſt excellent in itſelf, as the moſt conducive to man's happineſs. And for this reaſon I am the leſs diſpleaſed to ſee, that the following letter is ſwelled to a bulk far greater than its being but a letter promiſes, and than I firſt intended. For I confeſs, that when the occaſion happened, that made me put pen to paper, as I chanced to be in a very unſettled condition (which I fear has had too much influence on what I have written) ſo I did not deſign the inſiſting near ſo long upon my ſubject as I have done; but new things ſpringing up, if I may ſo ſpeak, under my pen, I was content to allow them room in my paper, becauſe writing as well for my own ſatisfaction, as for that of my friend, I thought it would not be uſeleſs to lay before my own eyes, as well as his, thoſe conſiderations, that ſeemed proper to juſtify to myſelf, as well as to him, the preference I gave divine truths (before phyſiological ones) and to confirm myſelf in the eſteem I had for them. And though I freely confeſs, that the following diſcourſe doth not conſiſt of nothing but ratiocinations, and conſequently is not altogether of an uniform contexture; yet that will, I hope be thought no more than was fit in a diſcourſe, deſigned not only to convince, but to perſuade: which if it prove ſo happy as to do, as I hope the peruſer will have no cauſe to regret the trouble of reading it, ſo I ſhall not repent that of writing it.

The INTRODUCTION.

SIR,

I Hoped you had known me better, than to doubt in good earneſt, how I reliſhed the diſcourſe your learned friend entertained us with yeſter-night. And I am the more troubled at your queſtion, becauſe your way of enquiring, how much your friend's diſcourſe obtained of my approbation, gives me cauſe to fear, that you vouchſafe it more of yours than I could wiſh it. But before I can ſafely offer you my ſenſe of the diſcourſes, about which you deſire to know it, I muſt put you in mind, that they were not all upon one ſubject, nor of the ſame nature: and I am enough his ſervant to acknowledge, without the leaſt reluctancy, that he is wont to ſhew a great deal of wit, when he ſpeaks like a naturaliſt, only of things purely phyſical; and when he is in the right, ſeldom wrongs a good cauſe by his way of managing it. But as for thoſe paſſages, wherein he gave himſelf the liberty of diſparaging the learned Dr. N. only because that doctor cultivates theological, as well as physical studies, and does both both oftentimes read books of devotion, and sometimes write them; I am not so much a courtier, as to pretend, that I liked them. 'Tis true he did not deny the doctor to be a learned, and a witty man, as indeed the wise providence of God has so ordered it, that to stop the bold mouths of some, who would be easily tempted to imagine, and more easily to give out that none are philosophers, but such as like themselves, desire to be nothing: else our nation is happy in several men, who are as eminent for humane, as studious of divine learning; and as great a veneration as they pay to *Moses*, and St. *Paul*, are as well versed in the doctrine of *Aristotle*, and of *Euclid*; nay, of *Epicurus* and *Des Cartes* too, as those, that care not to study any thing else. But though, for this reason, Mr. *N.* had not the confidence to despise the doctor, and some of his resemblers, whom he took occasion to mention; yet he too plainly disclosed himself to be one of those, who, though they will not deny, but that some, who own a value for theology, are men of parts; yet they talk, as if such persons were so, in spight of their being religiously given; that being, in their opinion, such a blemish, that a man must have very great abilities otherwise, to make amends for the disadvantage of valuing sacred studies, and surmount the disparagement it procures him. Wherefore, since this disdainful humour begins to spread, much more than I wish it did, among different sorts of men, among whom I should be glad not to find any naturalists; and since the question you asked me, and the esteem you have for your friend, makes me fear you may look on it with very favourable eyes; I shall not decline the opportunity you put into my hands of giving you together with a profession of my dislike of his practice, some of my reasons for that dislike; and the rather, because I may do it without too much exceeding the limits of an epistle, or those which the haste, wherewith I must write this, does prescribe to me. For your friend does not oppose, but only undervalue theology; and professing to believe the scriptures, (which I so far credit, as to think he believes himself when he says so) we agree upon the principles: so that I am not to dispute with him, as against an atheist, that denies the author of nature, but only against a naturalist, that over-values the study of it. And the truths of theology are things which I need not bring arguments for, but am allowed to draw arguments from them.

But though, as I am just now intimated, I design brevity; yet, for fear the fruitfulness, and importance of my subject, should suggest things enough to me, to make some little method requisite to keep them from appearing confused; I shall divide the following epistle into two distinct parts. In the former of which, I shall offer you the chief positive considerations, by which I would represent to you the study of divinity, as preferable to that of physick: And, in the second part, I shall consider the allegations, that I foresee your friend may interpose, in favour of natural philosophy. From which distribution you will easily gather, that the motives on the one hand, and the objections on the other, will challenge to themselves distinct sections, in the respective parts whereto they belong. So that, of the order of the particulars you will meet with, I shall not need to trouble you with any further account.

THE

THE Excellency of THEOLOGY: OR The Pre-eminence of the Study of Divinity above that of Natural Philoſophy.

THE FIRST PART.

TO addreſs myſelf then, without any farther circumſtance, or preamble, to the things themſelves, that I mainly intend in this diſcourſe, I conſider in the general, that there are ſcarce any motives accounted fitter to engage a rational man in a ſtudy, than that the ſubject is noble, that it is his duty to apply himſelf to it, and that his proficiency in it will bring him great advantages; ſo there is not any of theſe three inducements, that does not concur, in a very plentiful meaſure, to recommend to us the ſtudy of theological truths.

SECTION I.

AND firſt, the excellency and ſublimity of the object we are invited to contemplate, is ſuch, that none, that does truly acknowledge a deity, can deny, but that there is no ſpeculation, whoſe object is comparable, in point of nobleneſs, to the nature and attributes of God. The ſouls of inquiſitive men are commonly ſo curious, to learn the nature and condition of ſpirits, as that the over-greedy deſire to diſcover ſo much, as that there are other ſpiritual ſubſtances, beſides the ſouls of men, has prevailed with too many to try forbidden ways of attaining ſatisfaction; and many have choſen rather to venture the putting themſelves within the power of dæmons, than remain ignorant whether or no there are any ſuch beings: as I have learned by the private acknowledgments made me of ſuch unhappy (though not unſucceſsful) attempts, by divers learned men, (both of other profeſſions, and that of phyſick,) who themſelves made them in different places, and were perſons neither timorous, nor ſuperſtitious: (but this only upon the by.) And certainly that man muſt have as wrong, as mean a notion of the deity, and muſt very little conſider the nature and attributes of that infinitely perfect Being, and as little the nature and infirmities of man, who can imagine the divine perfections to be ſubjects, whoſe inveſtigation a man may (inculpably) deſpiſe, or be ſo
Pſal. cxlv. much as fully ſufficient for. Not only the ſcripture tells us, That his greatneſs is
Pſ. cxlvii. 5. incomprehenſible, and his wiſdom is inſcrutable; That he humbles himſelf to look in-
Pſ. cxiii. 6. to (or upon) the heavens and the earth; and, That not only this, or that man, but all
the nations of the world are, in compariſon of him, but like the ſmall drop of a bucket,
Iſa. xl. 15. or the ſmaller duſt of a balance: but even the heathen philoſopher, who wrote that eloquent book *de Mundo*, aſcribed to *Ariſtotle* in his riper years, ſpeaks of the power, and wiſdom, and amiableneſs of God, in terms little leſs lofty, though neceſſarily inferior to ſo infinitely ſublime a a ſubject; which they, that think they can, eſpecially without revelation, ſufficiently underſtand, do very little underſtand themſelves.

BUT perhaps your friend will object, that, to the knowledge of God there needs no other than natural theology; and I readily confeſs, being warranted by an apoſtle, that the

the γνωστόν τȣ̃ Θεȣ̃ was not unknown to the heathen philoſophers; and that ſo much knowledge of God is attainable by the light of nature, duly employed, as to encourage men to exerciſe themſelves, more than moſt of them do, in that nobleſt of ſtudies, and render their being no proficients in it, injurious to themſelves, as well as to their maker. But notwithſtanding this, as God knows himſelf infinitely better than purblind man knows him, ſo the informations he is pleaſed to vouchſafe us, touching his own nature, and attributes, are exceedingly preferable to any account, that we can give ourſelves of him, without him. And, methinks, the different proſpects we may have of heaven, may not ill adumbrate to us the differing diſcoveries, that may be made of the attributes of its maker. For as though a man may, with his naked eye, ſee heaven to be a very glorious object, enobled with radiant ſtars of ſeveral ſorts; yet, when his eye is aſſiſted with a good teleſcope, he cannot only diſcover a number of ſtars, (fixed and wandering,) which his naked eye would never have ſhewn him; but thoſe planets, which he could ſee before, will appear to him much bigger, and more diſtinct: ſo, although bare reaſon, well improved, will ſuffice to make a man behold many glorious attributes in the deity; yet the ſame reaſon, when aſſiſted by revelation, may enable a man to diſcover far more excellencies in God, and perceive them, than he contemplated before, far greater and more diſtinctly. And to ſhew how much a dim eye, illuminated by the ſcriptures, is able to diſcover of the divine perfections, and how unobvious they are to the moſt piercing philoſophical eyes, that enjoy but the dim light of nature; we need but conſider, how much more ſuitable conceptions and expreſſions concerning God are to be met with in the writings of thoſe fiſhermen and others, that penned the new teſtament, and thoſe illiterate Chriſtians, that received it, than among the moſt civilized nations of the world (ſuch as anciently the Greeks and Romans, and now the Chineſe and Eaſt-Indians) and amongſt the eminenteſt of the wiſe-men and philoſophers themſelves, (as *Ariſtotle*, *Homer*, *Heſiod*, *Epicurus*, and others.) Rom. i. 19

BESIDES that the book of ſcripture diſcloſes to us much more of the attributes of God, than the book of nature, there is another object of our ſtudy, for which we muſt be entirely beholden to theology: for though we may know ſomething of the nature of God by the light of reaſon, yet we muſt owe the knowledge of his will, or poſitive laws, to his own revelation. And we may gueſs, how curious great princes and wiſe men have been to inform themſelves of the conſtitutions eſtabliſhed by wiſe and eminent legiſlators; partly by the frequent travels of the ancient ſages and philoſophers into foreign countries, to obſerve their laws and government, as well as bring home their learning; and partly by thoſe royal and ſumptuous expences, at which that great and learned monarch *Ptolomeus Philadelphus* ſtuck not to procure an authentick copy of the law of Moſes, whom he conſidered but as an eminent legiſlator. But certainly that, and other laws recorded in the bible, cannot but appear more noble and worthy objects of curioſity to us Chriſtians, who know them to proceed from an omniſcient deity, who being the author of mankind, as well as of the reſt of the univerſe, cannot but have a far perfecter knowledge of the nature of man, than any other of the law-givers, or all of them put together can be conceived to have had.

BUT there is a farther diſcovery of divine matters, wherewith we are alſo gratified by theology: for beſides what the ſcripture teaches us of the nature and the will of God, it contains divers hiſtorical accounts (if I may ſo call them) of his thoughts and actions. The great *Alexander* thought himſelf nobly employed, when he read the Grecian actions in *Homer*'s verſes; and, to know the ſentiments of great and wiſe perſons upon particular occaſions, is a curioſity ſo laudable, and ſo worthy of an inquiſitive ſoul, that the ſouthern queen has been more praiſed and admired, for coming from the remoter parts of the earth, to hear the wiſdom of *Solomon.* Now the ſcripture does in many places give our curioſity a nobler employment, and thereby a higher ſatisfaction, than the King of *Macedon*, or the queen of *Sheba* could enjoy; for in many places it does, with great clearneſs and ingenuity, give us accounts of what God himſelf hath declared of his own thoughts, of divers particular perſons and things, and relates, what he, that knows and commands all things, was pleaſed to ſay and do upon particular occaſions. Of this ſort of paſſages are the things recorded to have been ſaid by God to *Noah*, about the ſinful world's ruin, and that juſt man's preſervation; and to *Moſes* in the caſe of daughters of *Zelophehad.* And of this ſort are the conferences, mentioned to have paſſed betwixt God and *Abimelech*, concerning *Abraham*'s Wife; betwixt God and *Abraham*, touching the deſtruction of *Sodom*; betwixt God and *Solomon*, about that king's happy choice; betwixt God and *Jonah*, about the fate of the greateſt city of the world: and above all theſe, thoſe two ſtrange and matchleſs paſſages, the one in the firſt book of *Kings*, touching the ſeducing ſpirit, that undertook to ſeduce *Ahab*'s prophets; and the other, that yet more wonderful relation of what paſſed betwixt God and Satan, wherein the deity vouchſafes, not only to praiſe, but (if I may ſo ſpeak with reverence) to glory in a mortal. And the being admitted to the knowledge of theſe tranſactions of another world, (if I may ſo call them) wherein God has been pleaſed to diſcloſe himſelf ſo very much, is an advantage afforded us by the ſcripture, of ſo noble a nature, and ſo unattainable by the utmoſt improvement we ourſelves can make of our own reaſon, that, did the ſcripture contain nothing elſe, that were very conſiderable, yet that book would highly deſerve our curioſity and gratitude.

Geneſ. vi.
Numb. xxvii. 7.
Geneſ. xx.
Geneſ. xviii.
1 Kings iii.
Jonah iv.
1 Kings xxii, from ver. 19. to ver. 24.
Job i. 6, 7. &c.
Job. ii. 3.

AND on this occaſion, I muſt by no means leave unobſerved another advantage, that we have from ſome diſcourſes made us in the bible, ſince it too highly concerns us, not to be very great one; and it is, that the ſcripture declares to us the judgment, that God is pleaſed to make of ſome particular men, upon the eſtimate of their life and deportment. For though reaſon alone, and the grounds of religion in general, may ſatisfy us in ſome meaſure, that God is good and merciful, and therefore it is likely he may pardon the ſins and frailties of men, and accept of their imperfect ſervices; yet beſides that we do not know, whether he will pardon, unleſs we have his promiſe of it; beſides this, (I ſay) though by virtue of general revelation, ſuch as is pretended to in divers religions, we may be aſſured, that God will accept, forgive, and reward thoſe, that ſincerely obey him, and perform the conditions of the covenant, whether it be expreſs, or implicit, that he vouchſafes to make with them; yet ſince it is he, that is the judge of the performance of the conditions, and of the ſincerity of the perſon; and ſince he is omniſcient, and a Καρδιογνώστης and ſo may know more ill of us, than even we know of ourſelves; a concerned conſcience may rationally doubt, whether in God's eſtimate any particular man was ſo ſincere as to be accepted. But when he himſelf is pleaſed to give elogimus (if I may with due reſpect ſo ſtyle them) to *David*, *Job*, *Noah*, *Daniel*, &c. whilſt they were alive, and to others after they were dead, (and conſequently having finiſhed their courſe, were paſſed into an irreverſible ſtate) we may learn with comfort, both that the performance of ſuch an obedience, as God will accept, is a thing really practicable by men; and that even great ſins and miſdemeanors are not (if ſeaſonably repented of) certain evidences, that a man ſhall never be happy in the future life. And it ſeems to be for ſuch an uſe of conſolation to frail men, (but not at all to encourage licentious ones) that the lapſes of holy perſons are ſo frequently recorded in the ſcriptures. And bateing thoſe divine writings, I know no books in the world, nor all of them put together, that can give a conſidering Chriſtian, who has due apprehenſions of the inexpreſſible happineſs or miſery of an immortal ſtate

See Heb. v. 9.
Pſal. ciii. 17, 13.
Acts i. 21.
1 John. iii. 20.

ſtate in heaven or in hell, ſo great and well grounded a conſolation, as may be derived from three or four lines in St. *John*'s apocalypſe, where he ſays, "That he ſaw in heaven a great multitude, not to be numbered of all nations, and tribes, and people, and tongues, ſtanding before the throne, and before the lamb, clothed in white robes, with palms (the enſigns of victory) in their hands;" and the praiſes of God and of the lamb in their mouths. For from thence we may learn, that heaven is not reſerved only for prophets, and apoſtles, and martyrs, and ſuch extraordinary perſons, whoſe ſanctity the church admires, but that, through God's goodneſs multitudes of his more imperfect ſervants have acceſs thither.

Revel. vii. 9.

THOUGH the infinite perfections and prerogatives of the deity be ſuch, that theology itſelf can, no more that philoſophy, afford us another object for our ſtudies, any thing near ſo ſublime and excellent, as what it diſcloſes to us of God; yet divinity favours us with ſome other diſcoveries, namely, about angels, the univerſe, and our own ſouls, which, though they muſt needs be inferior to the knowledge of God himſelf, are, for the nobleneſs of their objects, or for their importance, highly preferable to any, that natural philoſophy has been able to afford its votaries.

BUT before I proceed to name any more particulars, diſcloſed to us by revelation, it will be requiſite, for the prevention or removal of a prejudice, to mind you, that we ſhould not make our eſtimates of the worth of the things we owe to revelation, by the impreſſions they are wont now to make upon us chriſtians, who learned divers of them in our catechiſms, and perhaps have ſeveral times met with moſt of the reſt in ſermons, or theological books. For it is not to be admired, that we ſhould not be ſtrongly affected at the mention of thoſe truths, which (how valuable ſoever in themſelves) were for the moſt part taught us when we were either children, or too youthful to diſcern and prize their excellency and importance. So that though afterwards they were preſented to our riper underſtanding, yet their being by that time become familiar, and our not remembering, that we ignored them, kept them from making any vigorous impreſſions on us. Whereas if the ſame things had been (with circumſtances evincing their truth) diſcovered to ſome heathen philoſopher, or other vertuous and inquiſitive man, who valued important truths, and had nothing but his own reaſon to attain them with, he would queſtionleſs have received them with wonder and joy. Which to induce us to ſuppoſe we have ſundry inſtances, both in the records of the primitive times, and in the recent relations of the converſion of men to Chriſtianity among the people of *China*, *Japan*, and other illiterate nations. For though bare reaſon cannot diſcover theſe truths, yet when revelation has once ſufficiently propoſed them to her, ſhe can readily embrace, and highly value divers of them; which being here intimated once for all, I now advance to name ſome of the revelations themſelves.

AND firſt, as for angels, I will not now queſtion, whether bare reaſon can arrive at ſo much as to aſſure us, that there are ſuch beings in *rerum natura*. For though reaſon may aſſure, that their exiſtence is not impoſſible, and perhaps too, not improbable; yet I doubt, whether 'twere to mere ratiocination, or clear experience, or any thing elſe but revelation, convey'd to them by imperfect tradition, that thoſe heathen philoſophers, who believed, that there were ſeparate ſpirits other than human, owed that perſuaſion; and particularly as to good angels, I doubt, whether thoſe antient ſages had any cogent reaſons, or any convincing hiſtorical proofs, or, in ſhort, any one unqueſtionable evidence of any kind, to ſatisfy a wary perſon ſo much as of the being (much leſs to give a farther account) of thoſe excellent ſpirits. Whereas theology is enabled by the ſcripture to inform us, that not only there are ſuch ſpirits, but a vaſt multitude of them; that they were made by God and Chriſt, and are immortal, and propagate not their ſpecies; and that theſe ſpirits have their chief reſidence in heaven,

Matth. xxvi. 53.
Dan. vii. 10.
John. i. 3.
Heb. i. 7.
Luke xx. 35, 36.
Col. i. 16.
Matth. xxiv. 36.
Mark xiii. 32.
Matt. xviii. 10.

heaven, and enjoy the vision of God, whom they constantly praise, and punctually obey, without having sinned against him; that also these good angels are very intelligent beings, and of so great power, that one of them was able in a night to destroy a vast army; that they have degrees among themselves, are enemies to the devils, and fight against them; that they can assume bodies shaped like ours, and yet disapear in a trice; that they are sometimes employed about human affairs, and that not only for the welfare of empires and kingdoms, but to protect and rescue single good men. And though they are wont to appear in a dazzling splendor, and an astonishing majesty yet they are all of them ministring spirits, employed for the good of the designed heirs of salvation. And they do not only refuse men's adoration; and admonish them to pay it unto God; but, as they are in a sense made by Jesus Christ, who was true man as well as God; so they do not only worship him, and call him simply, as his own followers were wont to do, the Lord, but stile themselves fellow servants to his disciples.

AND as for the other angels, though the Gentiles, as well philosophers as others, were commonly so far mistaken about them, as to adore them for true gods and yet many of them to doubt whether they were immortal; the scripture informs us, that they are not self-originated, but created beings; that however a great part of manknid worship them, they are wicked and impure spirits, enemies to mankind, and seducers of our first parents to their ruin; that though they beget and promote confusion among men, yet they have some order among themselves, as having one chief, or leader; that they are evil spirits, not by nature, but apostacy; that their power is very limited, insomuch that a legion of them cannot invade so contemptible a thing as a herd of swine, without particular leave from God; that not only good angels, but good men, may, by resisting them, put them to flight, and the sincere Christians, that worsted them here, will be among those, that shall judge them hereafter; that their being immortal, will make their misery so too; that they do themselves believe, and tremble at those truths, they would persuade man to reject; and that they are so far from being able to confer that happiness, which their worshippers expect from them, that themselves are wretched creatures, reserved in chains of darkness to the judgment of the great day; at which they shall be doomed to suffer everlasting torments, in the company of those wicked men, that they shall have prevailed on.

WE may farther consider, that as to things corporeal themselves, which the naturalist challenges as his peculiar theme, we may name particulars, and those of the most comprehensive nature, and greatest importance, whose knowledge the naturalist must owe to theology. Of which truths I shall content my self to give a few instances in the world itself, or the universal aggregate of things corporeal; that being looked upon as the noblest and chiefest object, that the physicks afford us to contemplate.

AND first, those that admit the truths revealed by theology, do generally allow, that God is not only the author, but creator of the world. I am not ignorant of what *Anaxagoras* taught, of what he called νοῦς — (and *Tully* mentions) in the production of the world; and that what many other Grecians afterwards taught of the world's eternity, is peculiarly due to *Aristotle*, who does little less than brag, that all the philosophers that preceded him were of another mind. Nor will I here examine (which I elsewhere do) whether, and how far, by arguments merely physical, the creation of the world may be evinced. But whether or no mere natural reason can reach so sublime a truth, yet it seems not, that it did actually, where it was not excited by revelation-discovery. For though many of the ancient philosophers believed the world to have had a beginning, yet they all took it for granted, that matter had none; nor does any of them, that I know of, seem to have so much as imagined, that any substance

Isa. vi. 2, 3. Matth. vi. 10. 2 Sam. xiv. 20. Mark xiii. 32. 2 Kings xix. 35. 1 Thess. iv. 16. Jude ix. Dan. x. 13, 21. Col. i. 16. Revel. xii. 7. Acts xii. 7, 8, 9, 10. Dan. x. 13. Acts xii. 11. 2 Kings vi. 17. Luke xxiv. 4. Judg. xiii. 6. Heb. i. 14. Rev. xix. 10. Rev. xxii. 9. Matth. xxviii. 6. Rev. xix. 10. John i. 3. Colos. i. 16. Matth. viii. 7. Luke iv. 33. John viii. 34. 1 Pet. v. 8. 2 Cor. xi. 3. Rev. xii. 9. Rev. xii. 7. Matth. xxv. 41. 1 John iii. 8. Jude 6. Mark v. 9, 10, 13. Jam. iv. 7. 1 Pet. v. 9. 1 Cor. vi. Matth. xxv. 41. Jam. ii. 19. 2 Pet. ii. 4. Jude 6. 13. Matth. xxv. 41.

could be produced out of nothing. Thoſe, that aſcribe much more to God than *Ariſtotle*, make him to have given form only, not matter to the world, and to have but contrived the pre-exiſtent matter into this orderly ſyſtem we call the univerſe.

NEXT, whereas very many of the philoſophers, that ſucceeded *Atiſtotle*, ſuppoſe the world to have been eternal; and thoſe, that believed it to have been produced, had not the confidence to pretend to the knowing how old it was; unleſs it were ſome extravagant ambitious people, ſuch as thoſe fabulous Chaldeans, whoſe fond account reached up to forty thouſand or fifty thouſand years: theology teaches us, that the world is very far from being ſo old by thirty or forty thouſand years as they, and by very many ages, as divers others have preſumed: and does, from the ſcripture, give us ſuch an account of the age of the world, that it has ſet us certain limits, within which ſo long a duration may be bounded, without miſtaking in our reckoning. Whereas philoſophy leaves us to the vaſtneſs of indeterminate duration, without any certain limits at all.

THE time likewiſe, and the order, and divers other circumſtances of the manner, wherein the fabrick of the world was compleated, we owe to revelation; bare reaſon being evidently unable to inform us of particulars, that preceded the origin of the firſt man; and though I do not think religion ſo much concerned, as many do, in their opinion and practiſe, that would deduce particular theorems of natural philoſophy from this or that expreſſion of a book, that ſeems rather deſigned to inſtruct us about ſpiritual than corporeal things. I ſee no juſt reaſon to embrace their opinion, that would ſo turn the two firſt chapters of *Geneſis*, into an allegory, as to overthrow the literal and hiſtorical ſenſe of them. And though I take the ſcripture to be mainly deſigned to teach us nobler and better truths, than thoſe of philoſophy; yet I am not forward to comdemn thoſe, who think the beginning of *Geneſis*, contains divers particulars, in reference to the origin of things, which though not unwarily, or alone to be urged in phyſicks, may yet afford very conſiderable hints to an attentive and inquiſitive peruſer.

AND as for the duration of the world, which was by the old philoſophers held to be indeterminable, and of which the Stoicks opinion, that the world ſhall be deſtroyed by fire, (which they held from the *Jews*) was phyſically precarious; theology teaches us expreſsly from divine revelation, that the preſent courſe of nature ſhall not laſt always, Τρόχον τῆς
but that one day this world, or at leaſt, this vortex of ours, ſhall either be aboliſhed γενέσεως. Jam. iii. 6.
by annihilation, or, which ſeems far more probable, be innovated, and, as it were transfigured, and that, by the intervention of that fire, which ſhall diſſolve and de- 2 Pet. iii.
ſtroy the prſent frame of nature: ſo that either way, the preſent ſtate of things, (as well 7, 10, 13.
natural as political) ſhall have an end.

AND as theology affords us theſe informations about the creatures in general; ſo touching the chiefeſt and nobleſt of the viſible ones, men, revelation diſcovers very plainly divers very important things, where reaſon muſt needs be in the dark.

AND firſt, touching the body of man; the Epicureans attributed its original, as that of all things elſe, to the caſual concourſe of atoms; and the Stoicks abſurdly and injuriouſly enough (but much more pardonably than their follower herein, Mr. *Hobbes*) would have men to ſpring up like muſhrooms out of the ground; and whereas other philoſophers maintain conceits about it, too wild to be here recited; the book of *Geneſis* aſſures us, that the body of man was firſt formed by God in a peculiar manner, of a terrreſtrial matter; and it is there deſcribed, as having been perfected before the Gen. ii. 7.
ſoul was united to it. And as theology thus teaches us, how the body of man had its firſt beginning; ſo it likewiſe aſſures us, what ſhall become of the body after death, Acts xxiv.
though bare natural reaſon will ſcarce be pretended to reach to ſo abſtruſe and difficult 15.

Acts xvii. 20, 32. an article as that of a resurrection; which, when proposed by St. *Paul*, produced among the Athenian philosophers nothing else but wonder or laughter.

Not to mention, that theology teaches us divers other things about the origin and condition of men's bodies; as, that all mankind is the offspring of one man and one woman; that the first woman was not made of the same matter, nor after the same manner as the first man, but was afterwards taken from his side; that both *Adam* and *Eve* were not, as many Epicureans and other philosophers fancied, that the first men were first infants; whence they did, as we do, grow by degrees, to be mature and compleat human persons, but were made so all at once; and that hereafter, as all mens bodies shall rise again, so they shall all (or at least, all those of the just) be kept from ever dying a second time.

Gen. ii. Acts xvii. 26. Gen. ii. 21, 22. Acts xxv. 15. Luke xx. 35, 36.

And as for the human soul, though I willingly grant, that much may be deduced from the light of reason only, touching its existence, properties and duration; yet divine revelation teaches it us with more clearness, and with greater authority; as sure he, that made our souls, and upholds them, can best know what they are, and how long he will have them last. And as the scripture expresly teaches us, that the rational soul is distinct from the body, as not being to be destroyed by those very enemies, that kill the body; so about the origin of this immortal soul (about which philosophers can give us but wide and precarious conjectures) theology assures us, that the soul of man had not such an origination, as those of other animals, but was God's own immediate workmanship, and was united to the body already formed: and yet not so united, but that upon their divorce, she will survive, and pass into a state, in which death shall have no power over her.

Matth. x. 28. Gen. ii. 7. Zek. xii. 1. Luke xx. 35. 36. Matth. xxv. 46.

I expect you will here object, that for the knowledge of the perpetual duration of separate souls, we need not be beholden to the scripture, since the immortality of the soul may be sufficiently proved by the sole light of nature, and particularly has been demonstrated by your great *Des Cartes*. But you must give me leave to tell you, that besides that a matter of that weight and concernment cannot be too well proved, and consequently ought to procure a welcome for all good mediums of probation; besides this, I say, I doubt many Cartesians do, as well as others, mistake both the difficulty under consideration, and the scope of *Des Cartes*'s discourse. For I grant, that by natural philosophy alone, the immortality of the soul may be proved against its usual enemies Atheists and Epicureans. For the ground, upon which these men think it mortal, being, that it is not a true substance, but only a modification of the body, which consequently must perish, when the frame or structure of the body, whereto it belongs, is dissolved; their ground being this, I say, if we can prove, by some intellectual operations of the rational soul, which matter, however modified, cannot reach, that it is a substance distinct from the human body, there is no reason, why the dissolution of the latter should infer the destruction of the former, which is a simple substance, and as real a substance as matter itself, which yet the adversaries affirm to be indestructible. But though by the mental operations of the rational soul, and perhaps by other mediums it may, against the Epicureans, and other mere naturalists, who will not allow God to have any thing to do in the case, be proved to be immortal in the sense newly proposed; yet the same proofs will not evince, that absolutely it shall never cease to be, if we dispute with philosophers, who admit, as the Cartesians and many others do, that God is the sole creator and perserver of all things. For how are we sure, but that God may have so ordained, that though the soul of man, by the continuance of his ordinary and upholding concourse, may survive the body, yet, as it is generally believed not to be created, till it be just to be infused into the body; so it shall be annihilated, when it parts with the body, God withdrawing at death that supporting

ſupporting influence, which alone kept it from relapſing to its firſt nothing. Whence it may appear, that notwithſtanding the phyſical proofs of the ſpirituality and ſeparableneſs of the human ſoul, we are yet much beholden to divine revelation for aſſuring us, that its duration ſhall be endleſs. And now to make good what I was intimating above concerning the Carteſians, and the ſcope of *Des Cartes*'s demonſtration, I ſhall appeal to no other than his own expreſſions to evince, that he conſidered this matter for the main as we have done, and pretended to demonſtrate, that the ſoul is a diſtinct ſubſtance from the body; but not that abſolutely ſpeaking it is immortal. *Cur* (anſwers that excellent author) *de immortalitate animæ nihil ſcripſerim, jam dixi in ſynopſi mearum Meditationum. Quod ejus ab omni corpore diſtinctionem ſatis probaverim, ſupra oſtendi. Quod vero additis, ex diſtinctione animæ a corpore non ſequi ejus immortalitatem, quia nihilominus dici poteſt, illam a deo talis naturæ factam eſſe, ut ejus duratio ſimul cum duratione vitæ corporeæ finiatur, fateor a me refelli non poſſe. Neque enim tantum mihi aſſumo, ut quicquam de iis, quæ a libera Dei voluntate dependent, humanæ rationis vi determinare aggrediar. Docet naturalis cognitio, &c. Sed ſi de abſoluta Dei poteſtate quæratur, an forte decreverit, ut humanæ animæ iiſdem temporibus eſſe deſinant, quibus corpora quæ illis adjunxit; ſolius Dei eſt, reſpondere.* And if he would not aſſume to demonſtrate by natural reaſon ſo much as the exiſtence of the ſoul after death, unleſs upon a ſuppoſition; we may well preſume, that he would leſs take upon him to determine, what ſhall be the condition of that ſoul after it leaves the body. And that you may not doubt of this, I will give you for it his own confeſſion, as he freely writ it in a private letter to that admirable lady, the princeſs *Elizabeth*, firſt daughter to *Frederick* king of *Bohemia*, who ſeems to have deſired his opinion on that important queſtion, about which he ſends her this anſwer, *Pour ce qui, &c. i. e.* As to the ſtate of the ſoul after this life, my knowledge of it is far inferior to that of monſieur (he means Sir *Kenelm*) *Digby*. For, ſetting aſide that, which religion teaches us of it, I confeſs, that by mere natural reaſon we may indeed make many conjectures to our own advantage, and have fair hopes, but not any aſſurance. And accordingly in the next clauſe he gives the imprudence, of quitting what is certain for an uncertainty, as the cauſe why, according to natural reaſon, we are never to ſeek death.

Des Cartes reſponſione ad objectiones ſecundas, pag. m. 95.

NOR do I wonder he ſhould be of that mind. For all, that mere reaſon can demonſtrate, may be reduced to theſe two things; one, that the rational ſoul, being an incorporeal ſubſtance, there is no neceſſity, that it ſhould periſh with the body; ſo that, if God have not otherwiſe appointed, the ſoul may ſurvive the body, and laſt for ever: the other, that the nature of the ſoul, according to *Des Cartes*, conſiſting in its being a ſubſtance, that thinks, we may conclude, that though it be by death ſeparate from the body, it will nevertheleſs retain the power of thinking. But now, whether either of theſe two things, or both, be ſufficient to endear the ſtate of ſeparation after death, to a conſidering man, I think may be juſtly queſtioned. For immortality or perſeverance in duration, ſimply conſidered, is rather a thing preſuppoſed to, or a requiſite of felicity, than a part of it; and being in itſelf an adiaphorous thing, aſſumes the nature of the ſtate or condition, to which it is joined, and does not make that ſtate happy or miſerable, but makes the poſſeſſors of it more happy, or more miſerable than otherwiſe they would be. And tho' ſome ſchool-men, upon airy metaphyſical notions, would have men think it is more eligible to be wretched, than not to be at all; yet we may oppoſe to their ſpeculative ſubtilties the ſentiments of mankind, and the far more conſiderable teſtimony of the Saviour of mankind, who ſpeaking of the diſciple, that betrayed him, ſays, "That it had been good for that man, if he had never been born." And eternity is generally conceived to aggravate no leſs the miſeries of hell, than it heightens the joys of heaven. And here we may conſider, firſt, that mere reaſon cannot ſo much as aſſure

Mark xiv. 21.

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us absolutely, that the soul shall survive the body; for the truth of which we have not only *Cartesius's* confession, lately recited, but a probable argument, drawn from the nature of the thing, since, as the body and soul were brought together, not by any mere physical agents, and since their association and union, whilst they continued together, was made upon conditions, that depended solely upon God's free and arbitrary institution; so, for aught reason can secure us of, one of the conditions, of that association may be, that the body and soul should not survive each other. Secondly, supposing, that the soul be permitted to outlive the body, mere reason cannot inform us, what will become of her in her separate state, whether she will be vitally united to any other kind of body or vehicle; and if to some, of what kind that will be, and upon what terms the union will be made. For possibly she may be united to an unorganized, or very imperfectly organized body, wherein she cannot exercise the same functions she did in her human body. As we see, that even in this life the souls of natural fools are united to bodies, wherein they cannot discourse, or, at least, cannot philosophize. And it is plain, that some souls are introduced into bodies, which, by reason of paralytical and other diseases, they are unable to move, though that does not always hinder them from being obnoxious to feel pain. So that, for aught we naturally know, a human soul, separated from the body, may be united to such a portion of matter, that she may neither have the power to move it, nor the advantage of receiving any agreeable informations by its interventions, having upon the account of that union no other sense than that of pain. But let us now consider, what will follow, if I should grant, that the soul will not be made miserable, by being thus wretchedly matched. Suppose we then, that she be left free to enjoy what belongs to her own nature; that being only the power of always thinking, it may be well doubted, whether the exercise of that power will suffice to make her happy. You will perchance easily believe, that I love, as well as another, to entertain my self with my own thoughts, and to enjoy them undisturbed by visits and other avocations: I would, only accompanied by a servant and a book, go to dine at an inn upon a road, to enjoy my thoughts the more freely for that day. But yet, I think, the most contemplative men would, at least in time, grow weary of thinking, if they received no supply of objects from without, by reading, seeing, or conversing; and if they also wanted the opportunity of executing their thoughts, by moving the members of their bodies, or of imparting them, either by discoursing, or writing of books, or by making of experiments. On this occasion I remember, that I knew a gentleman, who was in *Spain* for a state-crime, which yet he thought an heroic action, kept close prisoner for a year in a place, where, though he had allowed him a diet not unfit for a person of note as he was; yet he was not permitted the benefit of any light, either of the day or candles, and was not accosted by any human creature, save at certain times by the gaoler, that brought him meat and drink, but was strictly forbidden to converse with him. Now, though this gentleman, by his discourse, appeared to be a man of a lively humour; yet being asked by me, how he could do to pass the time in that sad solitude, he confessed to me, that though he had the liberty of walking to and fro in his prison, and though, by often recalling into his mind all the adventures and other passages of his former life, and by several ways combining, and diversifying his thoughts, he endeavoured to give his mind as much variety of employment as he was able; yet that would not serve his turn, but he was often reduced, by drinking large draughts of wine, and then casting himself upon his bed, to endeavour to drown that melancholly, which the want of new objects cast him into. And I can easily admit, he found a great deal of difference between the sense he had of thinking when he was at liberty, and that, which he had, when he was confined to that employment, whose delightfulness, like fire, cannot last long, when it is, as his was, denied both fuel and vent.

vent. And, in a word, though I most readily grant, that thinking, interwoven with conversation and action, may be a very pleasant way of passing one's time; yet man being by nature a sociable creature, I fear that alone would be a dry and wearisome employment to spend eternity in.

BEFORE I proceed to the next section, I must not omit to take notice, that though the brevity I proposed to myself, keeps me from discoursing of any theological subjects, save what I have touched upon about the divine attributes, and the things I have mentioned about the universe in general, and the human soul; yet there are divers other things, knowable by the help of revelation, and not without it, that are of so noble and sublime a nature, that the greatest wits may find their best abilities both fully exercised, and highly gratified by making enquiries into them. I shall not name for proof of this the adorable mystery of the trinity, wherein it is, acknowledged, that the most soaring speculators are wont to be posed, or to lose themselves: but I shall rather mention the redemption of mankind, and the decrees of God concerning men. For though these seem to be less out of the ken of our natural faculties; yet it is into some things, that belong to the former of them, that the scripture tells us, *The angels desire to pry*; and 1 Pet. i. 12.
it was the consideration of the latter of them, that made one, that had been caught up into the mansion of the angels, amazedly cry out, ὦ βάθος, &c. Rom. xi. 33.

NOR are these the only things, that the scripture itself terms mysteries, though, for brevity sake, instead of specifying any of them, I shall content myself to represent to you in general; that since God's wisdom is boundless, it may, sure, have more ways than one to display itself. And though the material world be full of the productions of his wisdom; yet that hinders not, but that the scripture may be enobled with many excellent impresses, and, as it were, signatures of the same attribute. For, as I was beginning to say, it cannot but be highly injurious to the Deity, in whom all other true perfections, as well as omniscience, are both united and transcendent, to think, that he can contrive no ways to disclose his perfections, besides the ordering of matter and motion, and cannot otherwise deserve to be the object of men's studies, and their admiration, than in the capacity of a creator.

AND I think, I might safely add, that besides these grand and mysterious points I came from mentioning, there are many other noble and important things, wherein unassisted reason leaves us in the dark; which though not so clearly revealed in the scripture, are yet in an inviting measure discovered there, and consequently deserve the indagation of a curious and philosophical soul. Shall we not think it worth enquiring, whether the satisfaction of Christ was necessary to appease the justice of God, and purchase redemption for mankind? Or whether God, as absolute and supreme governor of the world, might have freely remitted the penalties of sin? Shall we not think it worth the enquiring, upon what account, and upon what terms, the justification of men towards God is transacted, especially considering how much it imports us to know, and how perplexedly a doctrine, not in itself abstruse, is wont to be delivered? Shall not we enquire, whether or no the souls of men, before they were united to their bodies, preexisted in a happier state, as many of the ancient and modern Jews and Platonists, and (besides Origen) some learned men of our times do believe? And shall not we be curious to know, whether, when the soul leaves the body, it do immediately pass to heaven or hell (as it is commonly believed,) or for want of organs be laid, as it were, asleep in an insensible and unactive state, till it recover the body at the resurrection, as many Socinians and others maintain? or whether it be conveyed into secret recesses, where, though it be in a good or bad condition, according to what it did in the body, it is yet reprieved from the flames of hell, and restrained from the beatifick vision till the day of judgment? (which seems to have been the opinion of many, if not most of the pri-

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mitive fathers and Christians.) Shall not we be curious to know, whether, at that great decretory day, the vast fabrick of the world, which all confess must have its frame quite shattered, shall be suffered to relapse into its first nothing, as several divines assert? or shall be, after its dissolution, renewed to a better state, and as it were, transfigured? And shall not we enquire, whether or no, in that future state of things, which shall never have an end, we shall know one another? (as *Adam*, when he awaked out of his profound sleep, knew *Eve*, whom he never saw before;) and whether those personal friendships and affections, we had for one another here, and the pathetic consideration of the relations (as of father and son, husband and wife, chaste mistress and virtuous lover, prince and subject,) on which many of them were grounded, shall continue? Or whether all those things, as antiquated and slight, shall be obliterated, and, as it were swallowed up? (as the former relation of a cousin a great way off is scarce at all considered, when the persons come so to change their state, as to be united by the strict bonds of marriage.)

Gen. ii. 21, 22, 23.

But it were tedious to propose all the other points, whereof the divine takes cognizance, that highly merit an inquisitive man's curiosity; and about which, all the writings of the old Greek and other heathen philosophers put together, will give us far less information, than the single volume of canonical scripture. I foresee, indeed, that it may nevertheless be objected, that in some of these enquiries, revelation incumbers reason, by delivering things, which reason is obliged to make its hypothesis consistent with. But besides, that this cannot be so much as pretended of all; if you consider, how much unassisted reason leaves us in the dark about these matters, wherein she has not been able to frame so much as probable determinations, especially in comparison of those probabilities, that reason can deduce from what it finds one way or other delivered in the scripture: if you consider this, I say, you will, I presume, allow me to say, that the revealed truths, which reason is obliged to comply with, if they be burdens to it, are but such burdens, as feathers are to a hawk, which, instead of hindering his flight by their weight, enable him to soar toward heaven, and take a larger prospect of things, than, if he had not feathers, he could possibly do.

And, on this occasion, Sir, the greater reverence I owe to the scripture itself, than to its expositors, prevails upon me to tell you freely, that you will not do right, either to theology, or (the greatest repository of its truths) the bible, if you imagine, that there are no considerable additions to be made to the theological discoveries we have already, nor no clearer expositions of many texts of scripture, or better reflections on that matchless book, than are to be met with in the generality of commentators, or of preachers, without excepting the antient fathers themselves. For there being, in my opinion, two things requisite, to qualify a commentator to do right to his theme, a competency of critical knowledge, and a concern for the honour and interest of Christianity in general, assisted by a good judgment, to discern and select those things, that may most conduce to it; I doubt, there are not many expositors, as they are called, of the scripture, that are not deficient in the former, or the latter of these particulars, and I wish there be not too many, that are defective in both.

That the knowledge of at least Greek and Hebrew is requisite to him, that takes upon him to expound writings penned originally in those languages, if the nature of the thing did not manifest it, you might easily be persuaded to believe, by considering, with what gross mistakes the ignorance of languages has oftentimes blemished, not only the interpretations of the school-men and others, but even those of the venerable fathers of the church. For though generally they were worthy men, and highly to be regarded, as the grand witnesses of the doctrines and government of the antient churches; most of them very pious, many of them very eloquent, and some of them (especially

(eſpecially the two criticks, *Origen* and *Jerom*) very learned; yet ſo few of the Greek fathers were ſkilled in Hebrew, and ſo few of the Latin fathers either in Hebrew, or Greek, that many of their homilies, and even comments, leave hard texts as obſcure as they found them; and, ſometimes miſled by bad tranſlations, they give them ſenſes exceeding wide of the true: ſo that many times in their writings they appear to be far better divines than commentators, and in an excellent diſcourſe upon a text, you ſhall find but a very poor expoſition of it; many of their eloquent and devout ſermons being much better encomiaſts of the divine myſteries they treat of, than unvailers. And though ſome modern tranſlations deſerve the praiſe of being very uſeful, and leſs unaccurate than thoſe, which the Latin fathers uſed; yet when I read the ſcriptures (eſpecially ſome books of the Old Teſtament) in their originals, I confeſs I cannot but ſometimes wonder, what came into the mind of ſome, even of our modern tranſlators, that they ſhould ſo much miſtake, and ſometimes injure certain texts as they do; and I am prone to think, that there is ſcarce a chapter in the bible (eſpecially that part of it, which is written in Hebrew,) that may not be better tranſlated, and conſequently more to the credit of the book itſelf.

THIS credit it miſſes of, not only by men's want of ſufficient ſkill in critical learning, but (to come to the ſecond member of our late diviſion) for want of their having judgment enough to obſerve, and concern enough to propoſe thoſe things in the ſcripture, and in theology, that tend to the reputation of either. For I fear there are too many, both commentators and other divines, that (though otherwiſe perhaps pious men) having eſpouſed a church or party, and an averſion from all diſſenters, are ſolicitous, when they peruſe the ſcripture, to take notice chiefly, if not only (I mean in points ſpeculative) of thoſe things, that may either ſuggeſt arguments againſt their adverſaries, or anſwers to their objections. But I meet with much fewer than I could wiſh, who make it their buſineſs to *ſearch the ſcriptures* for thoſe things (ſuch as unheeded prophecies, over-looked myſteries, and ſtrange harmonies) which being clearly and judiciouſly propoſed, may make that book appear worthy of the high extraction it challenges (and conſequently of the veneration of conſidering men) and who are ſolicitous to diſcern and make out, in the way of governing and of ſaving men, revealed by God, ſo excellent an œconomy, and ſuch deep contrivances, and wiſe diſpenſations, as may bring credit to religion, not ſo much as it is Roman, or proteſtant, or Socinian, but as it is Chriſtian. But (as I intimated before) theſe good affections for the repute of religion in general are to be aſſiſted by a deep judgment. For men, that want either that, or a good ſtock of critical learning, may eaſily over-ſee the beſt obſervations (which uſually are not obvious) or propoſed as myſteries, things that are either not grounded, or not weighty enough; and ſo (notwithſtanding their good meaning) may bring a diſparagement upon what they deſire to recommend. And I am willing to grant, that it is rather for want of good ſkill and good judgment, than good will, that there are ſo few, that have been careful to do right to the reputation of the ſcripture, as well as to its ſenſe. And indeed when I conſider, how much more to the advantage of thoſe ſacred writings, and of Chriſtian theology in general, divers texts have been explained and diſcourſed of by the excellent *Grotius*, by *Epiſcopius*, *Maſius*, Mr. *Mede*, and Sir *Francis Bacon*, and ſome other late great wits (to name now no living ones) in their ſeveral kinds, than the ſame places have been handled by vulgar expoſitors, and other divines: and when I remember too, that none of theſe newly named worthies was at once a great philoſopher, and a great critick; (the three firſt being not ſo well verſed in philoſophical learning, and the laſt being unacquainted with eaſtern tongues:) I cannot but hope, that when it ſhall pleaſe God to ſtir up perſons of a philoſophical genius, well furniſhed with critical learning, and the principles of

Ἐρευνᾶτε γραφάς. John v. 39.

true philosophy, and shall give them a hearty concern, for the advancement of his truths; these men, by exercising upon theological matters that inquisitiveness and sagacity, that has made in our age such a happy progress in philosophical ones, will make explications and discoveries, that will justify more than I have said in praise of the study of our religion, and the divine books, that contain the articles of it. For these want not excellencies, but only skilful unvailers. And if I do not tell you, that you should no more measure the wisdom of God couched in the bible, by the glosses or systems of common expositors and preachers, than estimate the wisdom he has expressed in the contrivance of the world by *Magirus*'s or *Eustachius*'s physicks; yet I shall not scruple to say, that you should as little think, that there are no more mysteries in the books of scripture, besides those, that the school-divines and vulgar commentators have taken notice of, and unfolded; as that there are no other mysteries in the book of nature, than those, which the same school-men (who have taken upon them to interpret *Aristotle* and nature too) have observed and explained. All the fine things, that poets, orators, and even lovers have hyperbolically said in praise of the beauty of eyes, will nothing near so much recommend them to a philosopher's esteem, as the sight of one eye skilfully dissected, or the unadorned account given of its structure, and the admirable uses of its several parts, in *Scheiner*'s *Oculus*, and *Des Cartes*'s excellent dioptricks. And though I do not think myself bound to acquiesce in, and admire every thing, that is proposed, as mysterious and rare by many interpreters and preachers; yet I think, I may safely compare several things in the books we call the scripture, to several others in that of nature, in (at least) one regard. For, though I do not believe all the wonders, that *Pliny*, *Ælian*, *Porta*, and other writers of that stamp, relate of the generation of animals; yet by perusing such faithful and accurate accounts, as sometimes *Galen de usu Partium*, sometimes *Vesalius*, sometimes our *Harvey* (*de Ovo*) and our later anatomists, and sometimes other true naturalists, give of the generation of animals, and of the admirable structure of their bodies, especially those of men, and such other parts of zoology, as *Pliny*, and the other writers I named with him, could make nothing considerable of; by perusing these, I say I receive more pleasure and satisfaction, and am induced more to admire the works of nature, than by all their romantick and superficial narratives. And thus (to apply this to our present subject) a close and critical account of the more vailed and pregnant parts of scripture, and theological matters, with such reflections on them, as their nature and collation would suggest to a philosophical, as well as critical speculator, would far better please a rational considerer, and give him a higher, as well as a better grounded veneration, for the things explained, than a great many of those slighter or ill founded remarks, wherewith the expositions and discourses of superficial writers, though never so florid or witty, gain the applause of the less discerning sort of men.

And here, on this occasion, I shall venture to add, that I despair not, but that a farther use may be made of the scripture, than either our divines or philosophers seem to have thought on. Some few theologues indeed have got the name of Supralapsarians, for venturing to look back beyond the fall of *Adam* for God's decrees of election and reprobation. But besides that, their boldness has been disliked by the generality of divines, as well as other Christians, the object of their speculation is much too narrow to be any thing near and adequate to such an hypothesis as I mean. For methinks, that the Encyclopedia's and Pansophia's, that even men of an elevated genius have aimed at, are not diffused enough to comprehend all, that the reason of a man, improved by philosophy, and elevated by the revelations already extant in the scripture, may, by the help of free ratiocination, and the hints contained in those pregnant writings (with those assistances of God's spirit, which he is still ready to vouchsafe to them, that duly seek them,) attain unto in this life. The gospel comprises indeed, and unfolds the whole mystery of man's redemption, as far forth as it (Acts xx. 27.) is necessary to be known for our salvation: and the corpuscularian or mechanical philosophy strives to deduce all the phænomena of nature form adiaphorous matter, and local motion. Bnt neither the fundamental doctrine of Christianity, nor that of the powers and effects of matter and motion, seems to be more than an epicycle (if I may so call it) of the great and universal system of God's contrivances, and makes but a part of the more general theory of things, knowable by the light of nature, improved by the information of the scriptures: so that both these doctrines, though very general, in respect of the subordinate parts of theology and philosophy, seem to be but members of the universal hypothesis, whose objects I conceive to be the nature, counsels, and works of God, as far as they are discoverable by us (for I say not to us) in this life.

For those, to whom God has vouchsafed the privilege of mature reason, seem not to enlarge their thoughts enough, if they think, that the omniscient and almighty God has bounded the operations of his power, and wisdom, aud goodness, to the exercise, that may be given them for some ages, by the production and government of matter and motion, and of the inhabitants of the terrestrial globe, which we know to be but a physical point in comparison of that portion of universal matter, which we have already discovered.

For I account, that there are four grand communities of creatures, whereof things merely corporeal make but one; the other three, differing from these, are distinct also from one another. Of the first sort are the race of mankind, where intellectual beings are vitally associated with gross and organical bodies. The second are dæmons, or evil angels; and the third good angels; (whether in each of those two kinds of spirits, the rational beings be perfectly free from all union with matter, though never so fine and subtile; or whether they be united to vehicles, not gross, but spirituous, and ordinarily invisible to us.)

Nor may we think, because angels and devils are two names quickly uttered, and those spirits are seldom or never seen by us, there are therefore but few of them, and the speculation of them is not considerable. For, as their excellency is great, (as we shall by and by shew) so for their number, they are represented in scripture as an heavenly host, standing on the right and left hand of the throne of God. And of the good angels, our saviour speaks of having more than twelve legions of them at his (Matth. xxvi. 53.) command. Nay, the prophet *Daniel* saith, that to the ancient of days, no less than (Dan. vii. 10.) millions ministred unto him, and hundreds of millions stood before him. And of the evil angels, the gospel informs us, that enough to call them a legion (which, you (Mark v. 9. Luke viii. 30.) know, is usually reckoned, at a moderate rate, to consist of betwixt six and seven thousand) possessed one single man. For my part, when I consider, that matter, how vastly extended, and how curiously shaped soever, is but a brute thing, that is only capable of local motion, and its effects and consequents on other bodies, or the brain of man, without being capable of any true, or at least any intellectual perception, or true love or hatred; and when I consider the rational soul as an immaterial and immortal being, that bears the image of its divine maker, being endowed with a capacious intellect, and a will, that no creature can force: I am by these considerations disposed to think the soul of man a nobler and more valuable being, than the whole corporeal world; which though I readily acknowledge it to be admirably contrived, and worthy of the almighty and omniscient author, yet it consists but of an aggregate of portions of brute matter, variously shaped and connected by local motion (as dough, and roles, and loaves, and cakes and vermicelli, wafers, and pie-crust, are all of them diversified meal; but without any

any knowledge either of their own nature, or of that of their author, or of that of their fellow-creatures.) And as the rational ſoul is ſomewhat more noble and wonderful, than thing merely corporeal, how vaſt ſoever it can be, and is of a more excellent nature, than the curiouſeſt piece of mechaniſm in the world, the human body; ſo to enquire what ſhall become of it, and what fates it is like to undergo hereafter, does better deſerve a man's curioſity, than to know what ſhall befal the corporeal univerſe, and might juſtly have been to *Nebuchadnezzar* a more deſirable part of knowledge, than that he was ſo troubled for want of, when it was adumbrated to him in the myſterious dream, that contained the characters and fates of the four great monarchies of the world. And as man is intruſted with a will of his own, whereas all material things move only as they are moved, and have no ſelf-determining power, on whoſe account they can reſiſt the will of God; and as alſo of angels, at leaſt ſome orders of them, are of a higher quality (if I may ſo ſpeak) than human ſouls; ſo it is very probable, that in the government of angels, whether good or bad, that are intellectual voluntary agents, that is required and employed far greater diſplays of God's wiſdom, power, and goodneſs, in the guidance of adiaphorous matter; and the method of God's conduct in the government of theſe, is a far nobler object for men's contemplation, than the laws, according to which the parts of matter hit againſt, and juſtle one another, and the effects or reſults of ſuch motions.

Dan. ii. 31, 32, &c.

AND accordingly we find in ſcripture, that, whereas about the production of the material world, and the ſetting of the frame of nature, God employed only a few commanding words, which ſpeedily had their full effects; to govern the race of mankind, even in order to their own happineſs, he employed not only laws and commands, but revelations, miracles, promiſes, threats, exhortations, mercies, judgments, and divers other methods and means; and yet oftentimes, when he might well ſay, as he did once by his prophet, "What could I have done more to my vineyard, that I have not done "it?" he had juſt cauſe to expoſtulate as he did in the ſame place, "Wherefore, when "I looked, that it ſhould bring forth grapes, brought it forth wild grapes?" and to complain of men, as by that very prophet he did even of *Iſrael*, "I have ſpread out "my hands all the day to a rebellious people." But not to wander too far in this digreſſion; what we have ſaid of men, may render it probable, that the grand attributes of God are more ſignally exerciſed, and made more conſpicuous in the making and governing of each of the three intellectual communities, than in the framing and upholding the community of mere bodily things. And ſince all immaterial ſubſtances are for that reaſon naturally immortal, and the univerſal matter is believed ſo too, poſſibly thoſe revolutions, that will happen after the day of judgment, wherein though probably not the matter, yet that ſtate and conſtitution of it, on whoſe account it is this world will be deſtroyed, and make way for quite new frames and ſets of things corporeal, and the beings, that compoſe each of theſe intellectual communities, will, in thoſe numberleſs ages they ſhall laſt, travel through I know not how many ſucceſſive changes and adventures; perhaps, I ſay, theſe things will no leſs diſplay, and bring glory to the divine attributes, than the contrivance of the world, and the œconomy of man's ſalvation, though theſe be (and that worthily) the objects of the naturaliſts and the divines contemplation. And there are ſome paſſages in the prophetical part of the ſcripture, and eſpecially in the book of the Apocalypſe, which, as they ſeem to intimate, that as God will perform great and noble things, which mechanical philoſophy never reached to, and which the generality of divines ſeem not to have thought of; ſo divers of thoſe great things may be, in ſome meaſure, diſcovered by an attentive ſearcher into the ſcriptures, and that ſo much to the advantage of the devout indagator, that St. *John*, near the beginning of his *Revelations*, pronounces them happy, that

Iſa. lxv. 2.

that read the matters containded in this prophecy, and * *observe* the things written therein. Which implies, that by heedful comparing together the indications couched in those prophetick writings, with events and occurrences in the affairs of the world, and the church, we may discover much of the admirable œconomy of providence in the governing of both: and I am prone to think, the early discoveries of such great and important things to be, in God's account, no mean vouchsafements, not only because the title of happy is here given to him, that attains them, but because of the two persons, to whom the great discoveries of this kind were made, I mean, the prophet *Daniel* and St. *John*; the first is by the angel said to be, on that account, a person highly favoured; and the other is, in the gospel, represented as our Saviour's beloved disciple. And you will the more easily think the foreknowledge of the divine dispensations gatherable from scripture to be highly valuable, if you consider, that, according to St. *Paul*, those very angels, that are called principalities and powers in heavenly places, learnt by the church some abstruse points of the manifold wisdom of God. But I must no longer indulge speculations, that would carry my curiosity beyond the bounds of time itself, and therefore beyond those, that ought to be placed to this occasional excursion.

Rev. i. 3.

Πολυποίκιλος σοφία τοῦ Θεοῦ Ephes. iii. 10.

AND yet, as on the side, I shall not allow myself the presumption of framing conjectures about those remote dispensations, which will not, most of them, have a beginning before this world shall have an end; so on the other side I would not discourage you, or any pious enquirer, from endeavouring to advance in the knowledge of those attributes of God, that may successfully be studied, without prying into the secrets of the future.

AND here, Sir, let me freely confess to you, that I am apt to think, that if men were not wanting to God's glory, and their own satisfaction, there would be far more discoveries made, than are yet attained to, of the divine attributes. When we consider the most simple, or uncompounded essence of God, we may easily be persuaded, that what belongs to any of his attributes (some of which thinking men generally admire) must be an object of enquiry exceeding noble, and worthy of our knowledge. And yet the abstruseness of this knowledge is not in all particulars so invincible, but that I strongly hope, a philosophical eye, illustrated by the revelations extant in the scripture, may pierce a great deal farther than has yet been done, into those mysterious subjects, which are too often (perhaps out of a mistaken reverence) so poorly handled by divines and schoolmen, that not only what they have taught, is not worthy of God (for that is a necessary, and therefore excusable deficiency) but too frequently it is not worthy of men, I mean, of rational creatures, that take upon them to treat of such high points, and instruct others about them. And I question not but your friend will the less scruple at this, if he call to mind those new and handsome notions about some of the attributes of God, that his master *Cartesius*, though but moderately versed in the scriptures, has presented us with. Nor do I doubt, but that a much greater progress might be made in the discovery of subjects, where, though we can never know all, we may still know farther if speculative geniuses would propose to themselves particular doubts and enquiries, about particular attributes and frame and examine hypotheses, establish theorems, draw corollaries; and (in short) apply to this study the same sagacity, assiduity, and attention of mind, which they often employ about enquiries of a very much inferior nature; insomuch, as *Des Cartes* (how profound a geometrician soever

* To render the original word (*observe*, or) *watch*, rather than *keep*, seems more congruous to the sense of the text, and is a criticism suggested to me by an eminent mathematician, as well as divine, who took notice, that the word τηρεῖ is used by the Greeks, as a term of art to express the astronomical observation of eclipses, planetary conjunctions, oppositions, and other celestial phænomena.

ever he were) confesses in one of his epistles, that he employed no less than six weeks to find the solution of a problem or question of *Pappus*. And *Pythagoras* was so addicted to, and concerned for geometrical speculations, that when he had found that famous proposition, which makes the 47th in *Euclid*'s First Book, he is recorded to have offered a hecatomb, to express his joy and gratitude for the discovery: which yet was but of one property of one sort of right-lined triangles. And certainly, if Christian philosophers did rightly estimate, how noble and fertile subjects the divine attributes are, they would find in them wherewithal to exercise their best parts, as well as to recompence the employment of them. But because, what I would dissuade, does not, perhaps, proceed only from laziness, but from a mistake; as if there were little to be known of so incomprehensible an object as God, save, that in general, all his attributes are like himself, infinite, and consequently not to be fully known by human understandings, because they are finite; I shall add, that though it be true, that by reason of God's infinity, we cannot comprehend him, that is, have a full and adequate knowledge of him; yet, we may not only know very many things concerning him, but, which is more, may make an endless progress in that knowledge. As no doupt, *Pythagoras* (newly mentioned) knew very well what a triangle was, and was acquainted with divers of its properties and affections, before he discovered that famous one. And though, since him, *Euclid*, *Archimedes*, and other geometricians have demonstrated, I know not how many other affections of the same figure, yet they have not to this day exhausted the subject: and possibly I (who pretend not to be a mathematician) may now and then, in managing certain æquations I had occasion for, have lighted upon some theorems about triangles, that occurred not to any of them. The divine attributes are such fruitful themes, and so worthy of our admiration, that the whole fabrick of the universe, and all the phænomena exhibited in it, are but imperfect expressions of God's wisdom, and some few of his other attributes. And I do not much marvel, that the angels themselves are represented in scripture, as employed in adoring God, and admiring his perfections. For even they being but finite, can frame but inadequate conceptions of him; and consequently must endeavour, by many of them, to make amends for the incompleatness of every one of them; which yet they can never but imperfectly do. And yet God's infinity can but very improperly be made a discouragement of our enquiries into his nature and attributes. For (not now to be expressed by a negative word, be not a positive thing in God) we may, notwithstanding his infinity, discover as much of him as our nature is capable of knowing: and what harm is it to him, that is drinking in a river, that he cannot drink up all the water, if he have liberty fully to quench his thirst, and take in as much liquor as his stomach can contain? Infinity therefore should not hinder us from a generous ambition to learn as much as we can of an object, whose being infinite does but make our knowledge of it the more noble and desirable, which indeed it is, in such a degree, that we need not wonder, that the angels are represented as never weary of their employment of contemplating and praising God. For, as I lately intimated, that they can have but inadequate ideas of those boundless perfections, and by no number of those ideas can arrive to make amends for the incompleatness of them; so it need not seem strange, that in fresh discoveries of new parts (if I may so call them) of the same object, it being such a one, they should find nobler and happier entertainments, than any where else variety could afford them.

Isa. vi. 2, 3. Luke ii. 13, 14. Rev. v. 11, 12.

SECTION

SECTION II.

HAVING thus taken notice of some particulars of those many, which may be employed to shew, how noble the objects are, that theology proposes to be contemplated; I now proceed to some considerations, that may make us sensible, how great an obligation there lies on us, to addict ourselves to the study of them.

YET, of the particulars, whereon this obligation may be grounded, I shall now name but two, they being indeed comprehensive ones, obedience, and gratitude.

AND first, let me represent, that it needs not, I suppose, be solicitously proved, that it is the will and command of God, that men should learn those truths, that he has been pleased to teach, whether concerning his nature or attributes, or the way, wherein he will be served and worshipped by man. For if we had not injunctions of scripture to that purpose, yet your friend is too rational a man to believe, that God would so solemnly cause his truths to be published to mankind, both by preaching and writing, without intention to oblige those (at least) that have the capacity and opportunity to enquire into some of them; and if it appear to be his will, that a person so qualified should search after the most important truths, that he hath revealed, it cannot but be their duty to do so. For though the nature of the thing itself did not lay any obligation on us, yet the authority of him, that commands it, would; since, being the supreme and absolute Lord of all his creatures, he has as well a full right to make what laws he thinks fit, and enjoin what service he thinks fit, as a power to punish those, that either violate the one, or deny the other; and accordingly it is very observable, that before *Adam* fell, and had forfeited his happy state, by his own transgression, he not Gen. ii. 16, only had a law imposed upon him, but such a law, as, being about a matter itself in- 17. different, (for so it was to eat, or not to eat, of the tree of life, as well as of any other,) derived its whole power of obliging from the mere will and pleasure of the law-giver. Whence we may learn, that man is subject to the laws of God, not as he is obnoxious to him, but as he is a rational creature, and that the thing, that is not a duty in its own nature, may become an indispensible one, barely by its being commanded. And indeed, if our first parent, in the state of innocency and happiness, wherein he tasted of God's bounty, without as yet standing in need of his mercy, was most strictly obliged, out of mere obedience, to conform to a law, the matter of which was indifferent in itself; sure we, in our lapsed condition, must be under a high obligation to obey the declared will of God, whereby we are enjoined to study his truths, and perform that, which has so much of intrinsick goodness in it, that it would be a duty, though it were not commanded; and has such recompences proposed to it, that it is not more a duty, than it will be an advantage.

BUT it is not only obedience, and interest, that should engage us to the study of divine things, but gratitude; and that exacted by so many important motives, that he, who said, *ingratum si dixeris, omnia dixeris,* could not think ingratitude so much worse than ordinary vices, as a contempt of the duty I am pressing would be worse than an ordinary ingratitude.

IT were not difficult, on this occasion, to manifest, that we are extremely great debtors unto God, both as he is the author, and the preserver of our very beings; and as he (immediately, or mediately,) fills up the measure of those continual benefits, with all the prerogatives, and other favors we do receive from him, as men; and the higher blessings, which (if we are not wanting to ourselves,) we may receive from him, as Christians.

But to shew, in how many particulars, and to how high a degree, God is our benefactor, were to launch out into too immense a subject; which it were the less proper for me to do, because I have, in other papers, discoursed of those matters already. (Seraph Love.) I will therefore single out a motive of gratitude, which will be peculiarly pertinent to our present purpose. For, whereas your friend does so highly value himself upon the study of natural philosophy, and despises not only divines, but statesmen, and even the learnedest men in other parts of philosophy and knowledge, because they are not versed in physicks; he owes to God that very skill, among many other vouchsafements. (Psal. xxxii. 9.) For it is God, who *made man unlike the horse and the mule, who have no understanding*, and endowed him with that noble power of reason, by the exercise of which, he attains to whatever knowledge he has of natural things above the beasts that perish. For that may justly be applied to our other acquisitions, which *Moses*, by God's appointment, told the Israelites concerning the acquists of riches; where he bids the people beware, that when their herds, and their flocks, and other treasures were multiplied, their heart be not lifted up, and prompt them to say, (Deut. viii. 10, 11, 12, 13, 14, 18.) "My power, and the might "of my hand, hath gotten me this wealth." But, (subjoins that excellent person, as well as matchless law-giver,) Thou shall remember thy Lord thy God, for it is he, that giveth thee power to get wealth. But to make men rational creatures, is not all God has done towards the making them philosophers. For, to the knowledge of particular things, objects are as well requisite as faculties; and if we admit the probable opinion of divines, who teach us, that the angels were created before the material world, as being meant by those *sons of God*, and *morning stars*, (Job xxxviii. 5, 6, 7.) that, with glad songs and acclamations, celebrated the foundation of the earth; we must allow, that there were many creatures endowed with, at least as much reason as your friend, who yet were unacquainted with the mysteries of nature, since she herself had not yet received a being. Wherefore, God having as well made the world, as given man the faculties, whereby he is enabled to contemplate it; naturalists are as much obliged to God for their knowledge, as we are for our intelligence to those, that writ us secrets in cyphers, and teach us the skill of decyphering things so written; or to those, who writ what would fill a page in the compass of a single penny, and present us to boot a microscope to read it. And as the naturalist hath peculiar inducements to gratitude, for the endowment of knowledge; so ingenuity lays this peculiar obligation on him to express his gratitude, in the way I have been recommending, that it is one of the acceptablest ways it can be expressed in; especially since, by this way, philosophers may not only exercise their own gratitude towards God, but procure him that of others. How pleasing men's hearty praises are to God, may appear, among other things, by what is said and done by that royal poet, whom God was pleased to declare a man after his own heart; (Psal. l. 23.) for he introduces God pronouncing, "Whoso offereth praise, glorifieth "me;" where the word our interpreters render offereth, in the *Hebrew*, signifies to sacrifice; with which agrees, that elsewhere those, that pay God their praises, are said to sacrifice to him (Hos. xiv. 2.) "the calves of their lips." And that excellent person, to whom God vouchsafed so particular a testimony, was so assiduous in this exercise, that the book which we, following the *Greek*, call *Psalms*, is, in the original, from the things it most abounds with, called *Sepher Tehillim*, i. e. the book of praises. And to let you see, that many of his praises were such, as the naturalist may best give, he exclaims, in one place, (Psal. civ. 24.) "How manifold are thy works, O Lord? how wisely hast thou made "them," (as *Junius* and *Tremellius* render it, and the *Hebrew* will bear;) and elsewhere, (Psal. xix. 1.) "The heavens declare the glory of God, and the firmament sheweth his "handy-work, &c." Again, in another place, (Psal. cxxxix. 14.) "I will praise thee, because I am "fearfully and wonderfully made. Marvellous are thy works, and that my soul

 "knoweth

"knoweth right well." And not content with many of the like expreſſions, he does ſeveral times, in a devout tranſport, and poetical ſtrain, invite the heavens, and the ſtars, and the earth, and the ſeas, and all the other inanimate creatures, to join with him in the celebration of their common maker. Which, though it ſeem to be merely a poetical ſcheme, yet, in ſome ſort, it might become a naturaliſt, who, by making out the power, wiſdom, and goodneſs of the Creator; and by reflecting thence on thoſe particulars, wherein thoſe attributes ſhine, may, by ſuch a devout conſideration of the creatures, make them, in a ſenſe, join with him in glorifying their author.

IN any other caſe, I dare ſay, your friend is not ſo ill natured, but that he would think it an unkind piece of ingratitude, if ſome great and excellent prince, having freely and tranſcendently obliged him, he ſhould not concern himſelf to know what manner of man his benefactor is; and ſhould not be ſolicitous to inform himſelf of thoſe particulars, relating to the perſon and affairs of that obliging monarch, which were not only in themſelves worthy of any man's curioſity, but about which the prince had ſolemnly declared, he was very deſirous to have men inquiſitive. And ſurely it is very diſingenious, to undervalue, or neglect the knowledge of God himſelf, for a knowledge, which we cannot attain without him, and by which he deſigned to bring us to that ſtudy we neglect for it: which is not only, not to uſe him as a benefactor, but as if he meant to puniſh him, (if I may ſo ſpeak,) for having obliged us, ſince we ſo abuſe ſome of his favours, as to make them inducements to our unthankful diſregard of his intentions in the reſt. And this ingratitude is the more culpable, becauſe the laws of ingenuity, and of juſtice itſelf, charge us to glorify the maker of all things viſible, not only upon our own account, but upon that of all his other works. For, by God's endowing of none but man here below, with a reaſonable ſoul, not only he is the ſole viſible being, that can return thanks and praiſes in the world, and thereby is obliged to do ſo, both for himſelf, and for the reſt of the creation; but it is for man's advantage, that God has left no other viſible beings in the world, by which he can be ſtudied and celebrated. For reaſon is ſuch a ray of divinity, that, if God had vouchſafed it to other parts of the univerſe beſides man, the abſolute empire of man over the reſt of the world muſt have been ſhared, or abridged. So that he, to whom it was equally eaſy to make creatures ſuperior to man, (as the ſcripture tells us of legions, and myriads of angels,) as to make them inferior to him, dealt ſo obligingly with mankind, as rather to truſt (if I may ſo ſpeak) our ingenuity, whether he ſhall reap any celebrations from the creatures we converſe with, than leſſen our empire over them, or our prerogatives above them.

BUT I fear, that, notwithſtanding all the excellency of revealed truths, and conſequently of that only authentic repoſitory of them, the ſcripture, you, as well as I, have met with ſome (for I hope there are not many) virtuoſi, that think to excuſe the neglect of the ſtudy of it, by alledging, that, to them, who are laymen, not eccleſiaſticks, there is required to ſalvation the explicit knowledge but of very few points, which are ſo plainly ſummed up in the apoſtles creed, and are ſo often and conſpicuouſly ſet down in the ſcripture, that one needs not much ſearch, or ſtudy, to find them there.

IN anſwer to this allegation, I readily grant, that through the great goodneſs of God, who is willing to have all men ſaved, and come to the knowledge of the truth, that is neceſſary to be ſo; there are much fewer articles abſolutely neceſſary to be by all men diſtinctly believed, than may be met with in divers long confeſſions of faith, ſome of which have, I fear, leſs promoted knowledge than impaired charity. But than it may be alſo conſidered, 1. That it is not ſo eaſy for a rational man, that will trouble 1 Tim. ii. 4.

himſelf to enquire no farther than the apoſtles creed, to ſatisfy himſelf upon good Joh. xiii. 7. grounds, that all the fundamental articles of Chriſtianity are contained in it. 2. That Heb. v. 9. the creed propoſes only the *credenda*, not the *agenda* of religion; whereas the ſcriptures were deſigned, not only to teach us what truths we are to believe, but by what rules we are to live; the obedience to the laws of Chriſtianity being as neceſſary to ſalvation, as the belief of its myſteries. 3. That beſides the things, which are abſolutely neceſſary, there are ſeveral, that are highly uſeful, to make us more clearly underſtand, and more rationally and firmly believe, and more ſteadily practice, the points, that are neceſſary. 4. And ſince whether or no thoſe words of our Saviour to the Jews, Joh. v. 39. ἐρευνᾶτε τὰς γραφὰς *, be to be rendered in the imperative or indicative mode; St. *Paul* Coloſſ. iii. 16. would have the word of Chriſt to dwell richly in us, (by which, whether he mean the holy ſcriptures then extant, or the doctrine of Chriſt, is not here material;) thereby teaching us, that ſearching into the matters of religion may become neceſſary, as a duty, though it were not otherwiſe neceſſary, as a means of attaining ſalvation. And indeed it is far more pardonable to want or miſs the knowledge of truths, than to deſpiſe or neglect it. And the goodneſs of God to illiterate or miſtaken perſons is to be ſuppoſed meant in pity to our frailties, not to encourage our lazineſs; nor is it neceſſary, that he, that pardons thoſe ſeekers of his truths, that miſs them, ſhould excuſe thoſe deſpiſers, that will not ſeek them.

But whether or no by this deſigned neglect of theology the perſons, I deal with, do ſufficiently conſult their own ſafety, I doubt they will not much recommend their ingenuity. For to have received from God a greater meaſure of intellectual abilities than the generality of Chriſtians, and yet willingly to come ſhort of very many of them, in the knowledge of the myſteries, and other truths of Chriſtianity, which he often invites us, if not expreſsly commands, to ſearch after, is a courſe, that will not reliſh of over-much gratitude. Is it a piece of that, and of ingenuity, to receive one's underſtanding and one's hopes of eternal felicity from the goodneſs of God, without being ſolicitous of what may be known of his nature and purpoſes by ſo excellent a way as his own revelation of them? to diſpute anxiouſly about the properties of an Prov. xxvi. 10. atom, and be careleſs about the enquiry into the attributes of the great God, who formed all things; to inveſtigate the ſpontaneous generation of ſuch vile creatures as inſects, than the myſterious generation of the adorable Son of God; and, in a word, to be more concerned to know every thing, that makes a corporeal part of the world, than the divine and incorporeal author of the whole?

And then, is it not, think you, a great piece of reſpect, that theſe men pay to thoſe truths, which God thought fit to ſend ſometimes prophets and apoſtles, ſometimes angels, and ſometimes his only Son himſelf, to reveal, that ſuch truths are ſo little valued by them, that rather than take the pains to ſtudy them, they will implicitly, and at adventures believe, what that ſociety of Chriſtians, they chance to be born and bred in, have, truly or falſely, delivered, concerning them? And does it argue a due regard to points of religion, that thoſe, who would not believe a propoſition in ſtaticks, perhaps about a mere point, the centre of gravity, or in geometry, about the properties of ſome nameleſs curve line, or ſome ſuch other things, (which to ignore, is uſually not a blemiſh, and about which to be miſtaken, is more uſually without danger) ſhould yet take up the articles of faith, concerning matters of great and everlaſting conſequence, upon the authority of men, fallible as themſelves, Acts xvii. 11. when ſatisfaction may be had without them from the infallible word of God? in this very unlike thoſe Bereans, whom the Evangeliſt honours with the title of Noble, that when the doctrines of the goſpel were propoſed to them, "they ſearched the ſcriptures "daily, whether thoſe things were ſo."

* Search, or, you ſearch the Scriptures.

AGAIN, if a man ſhould refuſe to learn to read any more, than juſt as much as may ſerve his turn, by intituling him to the benefit of the clergy, to ſave him from hanging, would theſe men think ſo ſmall a meaſure of literature, as he had acquired on ſuch an account, could prove that man to be a lover of learning; and yet a neglecter of the ſtudy of all not abſolutely neceſſary-divine truths, during one's life, becauſe the belief of the articles of the creed may make a ſhift to keep him from being doomed to hell for ignorance after his death, will not by (what in a learned man muſt be) ſo pitiful a degree of knowledge be much better intitled to that ingenuous love of God and his truths, that becomes a rational creature and a Chriſtian.

THE ancient prophets, though honoured by God with direct illuminations, were yet very ſolicitous to find out and learn the very circumſtances of the evangelical diſpenſations, which yet they did not know. (1 Pet. i. 10, 11.) And ſome of the goſpel myſteries are of ſo noble and excellent a nature, that "the angels themſelves deſire to look into them." (1 Pet. i. 12.) And though all the evangelical truths are not preciſely neceſſary to be known, it may be both a duty not to deſpiſe the ſtudy of them, and a happineſs to employ ourſelves about it. It was the earneſt prayer of a great king, and no leſs a prophet, that his eyes might be opened to behold (not the obvious and neceſſary truths, but) the wondrous things of God's law. (Pſal. cxix. 18.) He is pronounced happy in the beginning of the Apocalypſe, (Rev. i.) that reads and obſerves the things contained in that dark and obſcure part of ſcripture. And it is not only thoſe truths, that make articles of the creed, but divers other doctrines of the goſpel, that Chriſt himſelf judged worthy to be concluded with this epiphonema, "He that hath ears to hear, let him hear;" (Mat. xi. 15. Mark iv. 9, 23. Luke viii. 8.) on which the excellent *Grotius* makes this juſt paraphraſe, *Intellectus nobis a Deo potiſſimum datus eſt, ut eum intendamus documentis ad pietatem pertinentibus.*

SECTION III.

I COME now to our third and laſt inducement to the ſtudy of divine things, which conſiſts in, and compriſes the advantages of that ſtudy, which do as much ſurpaſs thoſe of all other contemplations, as divine things tranſcend all other objects. And indeed, the utility of this ſtudy is ſo pregnant a motive, and contains in it ſo many invitations, that your friend muſt have as little ſenſe of intereſt, as of gratitude, if he can neglect ſuch powerful and ſuch engaging invitations.

FOR, in the firſt place, theological ſtudies ought to be highly endeared to us, by the delightfulneſs of conſidering ſuch noble and worthy objects, as are therein propoſed.

THE famous anſwer given by an excellent philoſopher, who being aſked, what he was born for? replied, "To contemplate the ſun," may juſtly recommend their choice, who ſpend their time in contemplating the maker of the ſun, to whom that glorious planet itſelf is but a ſhadow. And perhaps that philoſopher failed more in the inſtance than in the notion: for his anſwer implies, that man's end and happineſs conſiſt in the exerciſe of his nobleſt faculties on the nobleſt objects. And ſurely the ſeat of formal happineſs being the ſoul, and that happineſs conſequently conſiſting in the operations of her faculties; as the ſupreme faculty of the mind is the underſtanding, ſo the higheſt pleaſures may be expected from the due exerciſe of it upon the ſublimeſt and worthieſt objects. And therefore I wonder not, that though ſome of the ſchool-men would aſſign the will a larger ſhare in man's felicity, than they will allow the intellect; yet the generality of them are quite of another mind, and aſcribe the pre-eminence, in point of felicity, to the ſuperior faculty of the ſoul. But whether or no this opinion be true in all cafes, it may, at leaft, be admitted in ours: for the chief objects of a Chriftian philofopher's contemplation being as well the infinite goodnefs, as the other boundlefs perfections of God, they are naturally fitted to excite in his mind an ardent love of that adorable Being, and thofe other joyous affections and virtuous difpofitions, that have made fome men think happinefs chiefly feated in the will. But having intimated thus much by the way, I pafs on to add, that the contentment afforded by the affiduous difcovery of God and divine myfteries has fo much of affinity with the pleafures, that fhall make up men's bleffednefs in heaven it felf, that they feem rather to differ in degree than in kind. For the happy ftate even of angels is by our Saviour reprefented by this employment, "That they continually fee "the face of his father, who is in heaven." And the fame infallible teacher, intending elfewhere to exprefs the celeftial joys, that are referved for thofe, who for their fake denied themfelves fenfual pleafures, employs the vifion of God as an emphatical periphrafe of felicity, "Bleffed, faid he, are the pure in heart, for they fhall fee "God." And as *Ariftotle* teaches, that the foul doth after a fort become that, which it fpeculates, St. *Paul* and St. *John* affure us, that God is a transforming object, and that in heaven "we fhall be like him, for (or, becaufe) we fhall fee him, as he is." And though I readily admit, that this beatifick vifion of God, wherein the underftanding is the proper inftrument, includes divers other things, which will concur to the complete felicity of the future life; yet I think, we may be allowed to argue. that that ravifhing contemplation of divine objects will make no fmall part of that happy eftate, which in thefe texts take its denomination from it.

Math. v. 8.

1 John iii. 2. Cor.

I have above intimated, that the fcripture attributes to the angels themfelves tranfports of wonder and joy upon the contemplation of God, and the exercifes they confider of his wifdom, juftice, or fome other of his attributes. But left, in referring you to the angels, you fhould fay, that I do in this difcourfe lay afide the perfon of a naturalift, in favour of divines; I will refer you to *Des Cartes* himfelf, whom I am fure your friend will allow to have been a rigid philofopher, if ever there were any. Thus then fpeaks he in that treatife, where he thinks he employs a more than mathematical rigor; and where he was obliged to utter thofe (I had almoft faid paffionate) words, I am going to cite from him, only by the impreffions made on him by the tranfcendent excellency of the object he contemplated, *Sed priufquam* (fays he) *hoc diligentius examinem, fimulque in alias veritates, quæ inde colligi poffunt, inquiram, placet hic aliquandiu in ipfius Dei contemplatione immorari, ejus attributa apud me expendere, & immenfi hujus luminis pulchritudinem, quantum caligantis ingenii mei acies ferre poterit, intueri, admirari, adorare. Ut enim in hac fola Divinæ Majeftatis contemplatione fummam alterius vitæ felicitatem confiftere fide credimus; ita etiam jam ex eadem, licet multo minus perfecta, maximam, cujus in hac vita capaces fimus, voluptatem, percipi poffe experimur.*

Medit. tertia fub finem.

BUT as high a fatisfaction as the ftudy of divine things affords by the noblenefs of its object, the contentment is not much inferior, that accrues from the fame ftudy, upon the fcore of the fenfe of a man's having in it performed his duty. To make actions of this nature fatisfactory to us, there is no need, that the things we are employed about, fhould in themfelves be excellent or delightful; the inward gratulations of confcience for having done our duties is able to gild the bittereft pills, and like the wood, that grew by the waters of *Marah*, to correct and fweeten that liquor, which before was the moft diftafteful. Thofe ancient Pagan heroes, whofe virtues may make us blufh, being guided but by natural reafon, and innate principles of moral virtues, could find the moft difficult and moft troublefome duties, upon the bare account of their being duties, not only tolerable, but pleafant. And though to deny fome lufts be, in our Saviour's efteem, no lefs uneafy, than for a man to pluck out his right eye, or cut off his right

Exod. xv. 25.

Math. v. 29, 30.

2 hand;

hand; yet even ladies have with ſatisfaction choſen, not only to deny themſelves the greateſt pleaſures of the ſenſes, but to ſacrifice the ſeat of them, the body itſelf, to preſerve the ſatiſfaction of being chaſte. Nor are they only the dictates of obedience, that we comply with in this ſtudy, but thoſe of gratitude; and that is a virtue, that has ſo powerful an aſcendant upon ingenuous minds, that thoſe, whoſe principles and aims were not elevated by religion, have, in acknowledgment to their parents and their country, courted the greateſt hardſhips, and hazards, and ſufferings, as if they were as great delights and advantages. And a grateful perſon ſpends no part of his life to his greater ſatisfaction, than that, which he ventures or employs for thoſe to whom he is obliged for it; and oftentimes finds a greater contentment even in the difficulteſt acknowledgments of a favour, than he did in receiving of it.

ANOTHER advantage, and that no mean one, that may accrue from the contemplation of theological truths, is, the improvement of the contemplator himſelf in point of piety and virtue. For, as the goſpel is ſtiled, the myſtery of godlineſs; and St. *Paul* 1 Tim. iii. 16.
elſewhere calls what it teaches, the truth, which is according to godlineſs, that is, a doc- Tit. i. 1.
trine framed and fitted to promote the intereſt of piety and virtue in the world: ſo this character and encomium belongs (though perhaps not equally) to the more retired truths diſcovered by ſpeculation, as well as to thoſe more obvious ones, that are familiarly taught in catechiſms and confeſſions of faith. I would by no means leſſen the excellency and prerogatives of fundamentals; but, ſince the grand and nobleſt engagements to piety and virtue are a high veneration for God and his Chriſt, and an ardent love of them; I cannot but think, that thoſe particular enquiries, that tend to make greater diſcoveries of the attributes of God, of the nature, and offices, and life of our Saviour, and of the wiſdom and goodneſs they have diſplayed in the contrivance and effecting of man's redemption, do likewiſe tend to encreaſe our admiration, and inflame our love, for the poſſeſſors of ſuch divine excellencies, and the authors of ſuch invaluable benefits. And as the brazen ſerpent, that was but a type of one of the goſpel Numb. xxi. 9.
myſteries, brought recovery to thoſe, that looked up to it; ſo the myſteries themſelves, being duly conſidered, have had a very ſanative influence on many, that contemplated them. Nor is it likely, that he, that diſcerns more of the depth of God's wiſdom and goodneſs, ſhould not, *cæteris paribus*, be more diſpoſed than others to admire him, to love him, to truſt him, and ſo to reſign up himſelf to be governed by him: which frame of mind both is itſelf a great part of the worſhip of God, and doth directly tend to the production and increaſe of thoſe virtues, without the practice of which, the ſcripture plainly tells us, that we can neither obey God, nor expreſs our love to him. And from this bettering of the mind by the ſtudy of theology, will flow (to add that upon the by) another benefit, namely, that by giving us a higher value for God and his truths, it will endear heaven to us, and ſo not only aſſiſt us to come thither, but heighten our felicity there.

I know it may be ſaid, that the melioration of the mind is but a moral advantage. But give me leave to anſwer, that beſides that it is ſuch a moral advantage, as ſuppoſes an intellectual improvement, whoſe fruit it is, a moral benefit may be great enough, even in the judgment of a mere philoſopher, and an Epicurean, to deſerve as much ſtudy as natural philoſophy itſelf. And that you may not think, that I ſpeak this only, becauſe I write in this epiſtle as a friend to divines, I will tell you, that *Epicurus* himſelf, who has now a-days ſo numerous a ſect of naturaliſts to follow him, ſtudied phyſicks, and writ ſo many treatiſes about them for this end, that by knowing the natural cauſes of thunder, lightning, and other dreadful phænomena, the mind might be freed from the diſquieting apprehenſions men commonly had, that ſuch ſtrange and formidable things proceeded from ſome incenſed deity, and ſo might trouble the mind, as well

well as the air. This account I have been giving of *Epicurus*'s deſign, is but what ſeems plainly enough intimated by his own words, preſerved us by *Laertius*, near the end of his phyſiological epiſtle to *Herodotus*, where recommending to him the conſideration of what he had delivered about phyſical principles in general, and meteors in particular, he ſubjoins, *Si enim ab iſtis non diſceſſerimus, tum id unde oritur perturbatio, quodque metum ingerit, recta cum ratione ediſſeremus, noſque ab ipſis eximemus.* And to this in the cloſe of his meteorological epiſtle to *Pythocles*, his beſt interpreter, *Gaſſendus*, makes him ſpeak conſtantly in theſe words, *Maxime veró dede teipſum ſpeculationi principiorum, ex quibus conſtant omnia, & infinitatis naturæ, aliorumque his cohærentium. Inſuper veró criteriorum, affectuumque animi, & ſcopum illius, in quem iſta ediſſerentes collineavimus, attende, tranquillitatem intelligo ſtatumque mentes imperturbatum.* But this is not all the teſtimony I can give you from *Epicurus* himſelf to the ſame purpoſe; for among his *Ratæ Sententiæ*, preſerved us by *Laertius*, (himſelf reputed an Epicurean) I find one, that goes further; *Si nihil*, ſays he, *conturbaret nos quod ſuſpicamur, veremurque ex rebus ſublimibus, neque item quod ex ipſa morte, ne quando nimirum ad nos pertineat aliquid, ac noſſe præterea poſſemus, qui Germani fines dolorum atque cupiditatum ſint* (οὐκ ἂν προσδεόμεθα φυσιολογίας) *nihil phyſiologiâ indigeremus.* Thus far the teſtimony of *Epicurus*, of whoſe mind though I am not at all, as to what he would intimate, "That phyſiology is either proper to free the mind from the belief of a provident "deity, and the ſoul's immortality, or fit for no other conſiderable purpoſes;" yet this uſe we may well make of theſe declarations, that in *Epicurus*'s opinion a moral advantage, that relates to the government of the affections, may deſerve the pains of making enquiries into nature. And ſince it hence appears, that a mere philoſopher, who admitted no providence, may think it worth his pains, to ſearch into the abſtruſeſt parts of phyſicks, and the difficulteſt phænomena of nature, only to eaſe himſelf of one troubleſome affection, fear; it need not be thought unphiloſophical, to proſecute a ſtudy, that will not only reſtrain one undue paſſion, but advance all virtues, and free us from all ſervile fears of the Deity; and tend to give us a ſtrong and well grounded hope in him; and make us look upon God's greateſt power, not with terror, but with joy.

Diogenis Laertii lib. 10.

THERE is yet another advantage belonging to the ſtudy of divine truths, which is too great to be here pretermitted. For whereas there is ſcarce any thing more incident to us whilſt we inhabit our *(batté chômer)* cottages of clay, and dwell in this vale of tears, than afflictions; it ought not a little to endear to us the newly mentioned ſtudy, that it may be eaſily made to afford us very powerful conſolations in that otherwiſe uneaſy ſtate.

Job. iv. 19.

I know it may be ſaid, that the ſpeculations, about which the naturaliſt is buſied, are as well pleaſing diverſions, as noble employments of the mind. And I deny not, that they are often ſo, when the mind is not hindered from applying it ſelf attentively to them; ſo that afflictions ſlight and ſhort may well be weathered out by theſe philoſophical avocations; but the greater and ſharper ſort of afflictions, and the approaches of death, require more powerful remedies, than theſe diverſions can afford us. For in ſuch caſes, the mind is wont to be too much diſcompoſed, to apply the attention requiſite to the finding a pleaſure in phyſical ſpeculations; and in ſickneſſes, the ſoul is oftentimes as indiſpoſed to reliſh the pleaſures of merely human ſtudies, as the languiſhing body is to reliſh thoſe meats, which at other times were delightful: and there are but few, that can take any great pleaſure to ſtudy the world, when they apprehend themſelves to be upon the point of being driven out of it, and in danger of loſing all their ſhare in the objects of their contemplation. It will not much qualify our ſenſe of the burning heat of a fever, or the painful gripes of the cholick, to know, that the three

three angles of a triangle are equal to two right ones; or, that heat is not a real quality (as the ſchools would have it,) but a modification of the motion of the inſenſible parts of matter; and pain not a diſtinct, inherent quality in the things, that produce it, but an affection of the ſentiment. The naturaliſt's ſpeculations afford him no conſolations, that are extraordinary in, or peculiar to the ſtate of affliction; and the avocations, they preſent him with, do rather amuſe the mind from an attention to leſſer evils, than bring it any advantages to remove or compenſate them, and ſo work rather in the nature of opiates, than of true cordials.

BUT now, if ſuch a perſon as Dr. *N.* falls into adverſity, the caſe is much otherwiſe; for we muſt conſider, that when the ſtudy of divine things is ſuch as it ought to be, though that in itſelf, or in the nature of the employment, be an act or exerciſe of reaſon; yet being applied to, out of obedience, and gratitude, and love to God, it is upon the account of its motives and its aim, an act of religion; and as it proceeds from obedience, and thankfulneſs, and love to God, ſo it is moſt acceptable to him; and upon the account of his own appointment, as well as goodneſs, is a moſt proper and effectual means of obtaining his favour; and then I preſume, it will eaſily be granted, that he, who is ſo happy as to enjoy that, can ſcarce be made miſerable by affliction. For not now to enter upon the common place of the benefits of afflictions to them, that love God, and to them, that are loved by him, it may ſuffice, that he, who (as the ſcripture Pſal. ciii.
ſpeaks) knows our frame, and has promiſed thoſe, that are his, that they ſhall not be 14. 1 Cor. x.
overburdened, is diſpoſed and wont to give his afflicted ſervants, both extraordinary com- 13.
forts in afflictions, and comforts appropriated to that ſtate. For though natural philoſophy be like its brighteſt object, the ſtars, which, however the aſtronomer can with pleaſure contemplate them, are unable, being mere natural agents to afford him a kinder influence than uſual, in caſe he be caſt upon his bed of languiſhing, or into priſon; yet the almighty and compaſſionate maker of the ſtars, being not only a voluntary, but the moſt free agent, can ſuit and proportion his reliefs to our neceſſities, and alleviate our heavieſt afflictions by ſuch ſupporting conſolations, that not only they can never ſurmount our patience, but are oftentimes unable ſo much as to hinder our joy; and when death, Job xviii.
that king of terrors, preſents itſelf, whereas the mere naturaliſt ſadly expects to be de- 14.
prived of the pleaſure of his knowledge, by loſing thoſe ſenſes and that world, which are the inſtruments and the objects of it; and perhaps, diſcovering beyond the grave nothing but either a ſtate of eternal deſtruction, or of eternal miſery, fears either to be confined for ever to the ſepulchre, or expoſed to torments, that will make even ſuch a condition deſirable; the pious ſtudent of divine truths is not only freed from the racking apprehenſions of having his ſoul reduced to a ſtate of annihilation, or caſt into hell, but enjoys a comfortable expectation of finding far greater ſatisfaction than ever, in the ſtudy he now rejoices to have purſued; ſince the change, that is ſo juſtly formidable to others, will but bring him much nearer to the divine objects of his devout curioſity, and ſtrangely elevate and enlarge his faculties to apprehend them.

AND this leads me to the mention of the laſt advantage belonging to the ſtudy I would perſuade you to; and indeed, the higheſt advantage, that can recommend any ſtudy, or invite men to any undertaking; for this is no leſs than the everlaſting fruition of the divine objects of our ſtudies hereafter, and the comfortable expectation of it here. For the employing of one's time and parts, to admire the nature and providence of God, and contemplate the divine myſteries of religion, as it is one of the chief of thoſe homages and ſervices whereby we venerate and obey God; ſo it is one of thoſe, to which he hath been pleaſed to apportion no leſs a recompence, than (that which can have no greater) the enjoyment of himſelf. The ſaints and angels in heaven [illegible]

have

Luke i. 11, 26. Acts x. 4, 5, 6. 1 Pet. i. 12.

have divers of them been employed to convey the truths of theology, and are solicitous to look into those sacred mysteries; and God hath been pleased to appoint, that those men who study the same lessons, that they do here, shall study them in their company hereafter. And doubtless, though heaven abound with unexpressible joys, yet it will be none of the least, that shall make up the happiness, even of that place, that the knowledge of divine things, that was here so zealously pursued, shall there be compleatly attained. For those things, that do here most excite our desires, and quicken the curiosity and industry of our searches, will not only there continue, but be improved to a far greater measure of attractiveness and influence. For all those interests, and passions, and lusts, that here below either hinder us from clearly discerning, or keep us from sufficiently valuing, or divert us from attentively enough considering, the beauty and harmony of divine truths, will there be either abolished, or transfigured: and as the object will be unveiled; so our eye will be enlightened, that is, as God will there disclose those worthy objects of the angels curiosity, so he will enlarge our faculties, to enable us to gaze, without being dazzled, upon those sublime and radiant truths, whose harmony, as well as splendor, we shall be then qualified to discover, and consequently with transports to admire. And this enlargement and elevation of our faculties will, proportionably to its own measure, increase our satisfaction at the discoveries it will enable us to make, For theology is like a heaven, which wants not more stars than appear in it, but we want eyes, quick-sighted and piercing enough to reach them. And as the milky way, and other whiter parts of the firmament, have been full of immortal lights from the beginning, and our new telescopes have not placed, but found them, there; so, when our Saviour, after his glorious resurrection, instructed his apostles to teach the gospel, it is not said, that he altered any thing in the scriptures of *Moses* and the prophets, but only opened and enlarged their intellects, that they might understand the scriptures, and the royal prophet makes it his prayer, "That God would be pleased to open his eyes, that "he might see wonderful things out of the law;" being (as was above intimated) so well satisfied, that the word of God wanted not admirable things, that he is only solicitous for the improvement of his own eyes, that they might be qualified to discern them.

Luke xxiv. 45. Psal. cxix. 18.

I had almost forgotten one particular about the advantages of theological studies, that is too considerable to be left unmentioned: for as great as I have represented the benefits accruing from the knowledge of divine truths; yet to endear them to us, it may be safely added, that, to procure us these benefits, the actual attainment of that knowledge is not always absolutely necessary, but a hearty endeavour after it may suffice to entitle us to them. The patient chemist, that consumes himself and his estate in seeking after the philosopher's stone, if he miss of his idolized elixir, had as good, nay better, have never sought it, and remains as poor in effect, as he was rich in expectation. The husbandman, that employs his seed and time, to obtain from the ground a plentiful harvest, if, after all, an unkind season happen, must see his toil made fruitless;

——— *longique perit labor irritus anni.*

Too many patients, that have punctually done and suffered for recovery, all, that physicians could prescribe, meet at last with death, instead of health. You know what entertainment has been given by skilful geometricians to the laborious endeavours, even of such famous writers as *Scaliger*, *Longomontanus*, and other Tetragonists; and that their successor Mr. *Hobbes*, after all the ways he has taken, and those he has proposed, to square the circle, and double the cube, by missing of his end, has, after his

his various attemps, come off, not only with diſappointment, but with diſgrace. And (to give an inſtance even in things celeſtial) how much pains has been taken to find out longitudes, and make aſtrological predictions with ſome certainty, which for want of coming up to what they aimed at, have been uſeleſs, if not prejudicial to the attempters?

BUT God (to ſpeak with St. *Paul* on another occaſion) "that made the world, and Acts xvii.
"all things therein, and is lord of heaven and earth," ſeeks not our ſervices, as 24, 25.
though he "needed any thing, ſeeing he giveth life, and breath, and all things:" his ſelf-ſufficiency and bounty are ſuch, that he ſeeks in our obedience the occaſions of rewarding it, and preſcribes us ſervices, becauſe the practice of them is not only ſuitable to our rational nature, but ſuch as will prevail with his juſtice, to let his goodneſs make our perſons happy. Agreeably to this doctrine we find in the ſcripture, Jam. ii. 21.
that *Abraham* is ſaid to have been "juſtified by faith, when he offered his ſon *Iſaac* upon the altar," (though he did not actually ſacrifice him) becauſe he endeavoured to do ſo; although God graciouſly accepting the will for the deed, accepted alſo of the blood of a ram inſtead of *Iſaac*'s. And thus we know, that it was not *David*, but *Solomon*, that built the temple of *Hieruſalem*, and yet God ſays to the former of thoſe 2 Chron. vi.
kings (as we are told by the latter) "Foraſmuch as it was in thine heart to build an 8, 9.
"houſe for my name, thou didſt well in that it was in thine heart, notwithſtanding "thou ſhalt not build the houſe, &c." And if we look to the other circumſtances 2 Sam. vii.
of this ſtory, as they are delivered in the ſecond book of *Samuel*, we ſhall find, that upon *David*'s declaration of a deſign to build God an houſe, God himſelf vouchſafes to honour him, as he once did *Moſes*, with the peculiar title of his ſervant; and com- verſe 5.
mands the prophet to ſay to him, "Alſo the Lord tells thee, that he will make thee verſe 11.
"an houſe;" to which is added one of the graciouſeſt meſſages, that God ever ſent to any particular man. By which we may learn, that God approves and accepts even thoſe endeavours of his ſervants, if they be real and ſincere, that never come to be actually accompliſhed: good deſigns and endeavours are our part, but the events of thoſe, as of all other things, are in the all diſpoſing hand of God, who, if we be not wanting to what lies in us, will not ſuffer us to be loſers, by the defeating diſpoſitions of his providence; but crown our endeavours, either with ſucceſs, or with ſome other recompence, that will keep us from being loſers, by miſſing of that. And indeed, if we conſider the great elogies, that the ſcripture, as well frequently as juſtly, gives God's goodneſs (which it repreſents as over, or as above all his works) and that his Hab. i. 13.
purer eyes puniſh, as well as ſee, the murder and adultery of the heart, when thoſe intentional ſins are hindered from advancing into actual ones; we can ſcarce doubt, but he, whoſe juſtice puniſhes ſinful aims, will allow his infinite goodneſs to recompenſe pious attempts: and therefore our Saviour pronounces them bleſſed, that hunger and Matth. v. 6.
thirſt after righteouſneſs, aſſuring them, that they ſhall be ſatisfied, and thereby ſufficiently intimating to us, that an earneſt deſire, after a ſpiritual grace (and ſuch is the knowledge of divine things) may intitle a man to the compleat poſſeſſion of it, if not in this life, yet in the next, where we ſhall not any more walk by faith, but by 2 Cor. v. 7.
ſight, and obtain as well a knowledge as other endowments, befitting that glorious ſtate, wherein the purchaſer of it for us, aſſures us, that we ſhall be [ἰσάγγελοι] equal, Luke xx. 36.
or like to the angels.

THE conſiderations, Sir, I have hitherto laid before you, to recommend the ſtudy of divine truths, have, I hope, perſuaded you, that it is on many accounts, both noble and eligible in itſelf; and therefore I ſhall here conlude the firſt part of this diſcourſe. And in regard, that the under-valuation *Phyſiophilus* expreſſes for that excellent employment, ſeems to flow (chiefly at leaſt) from his fondneſs and partiality

 for

for natural philosophy; it will next concern us to compare the ſtudy of theology with that of phyſicks, and ſhew, that the advantages, which your friend alledges in favor of the latter, are partly much leſſened by diſadvantageous circumſtances, and partly much outweighed by the tranſcendent excellencies of theological contemplations: the ſtudy whereof will thereby appear to be not only eligible in itſelf, but preferable to its rival. And I muſt give you warning to expect to find the ſecond part, which the making this compariſon challenges to itſelf, a good deal more prolix than the firſt; not only becauſe it often requires more trouble, and more words to detect and diſprove an error, than to make out a truth; but alſo becauſe, that divers things tending to the credit of divinity, and which conſequently might have been brought into the firſt part of this diſcourſe, were thought more fit to be interwoven with other things, in the anſwers made to the objections examined in the ſecond.

THE Excellency of THEOLOGY: OR The Pre-eminence of the Study of Divinity above that of Natural Philoſophy.

THE SECOND PART.

I SHALL, without preamble, begin this diſcourſe, by conſidering the delightfulneſs of phyſicks, as the main thing, that inveigles your friend, and divers other virtuoſi, from reliſhing, as they ought, and otherwiſe would, the pleaſantneſs of theological diſcoveries. And to deal ingenuouſly with you, I ſhall not ſcruple to acknowledge, that though the addreſs I have made to nature has laſted ſeveral years, and has been toilſome enough, and not unexpenſive; yet I have been pleaſed enough with the favours, ſuch as they are, that ſhe has from time to time accorded me, not to complain of having been unpleaſantly employed. But though I readily allow the attainments of naturaliſts to be able to give philoſophical ſouls ſincerer pleaſures, than thoſe, that the more undiſcerning part of mankind is ſo fond of; yet I muſt not therefore allow them to ſurpaſs, or even equal the contentment, that may accrue to a ſoul qualified by religion, to reliſh the beſt things moſt from ſome kind of theological contemplations.

THIS, I preſume, will ſufficiently appear, if I ſhew you, that the ſtudy of phyſiology is not unattended with conſiderable inconveniencies, and that the pleaſantneſs of it may be, by a perſon ſtudious of divinity, enjoyed with endearing circumſtances.

BUT before I name any of the particular reaſons, that I am to repreſent, I fear it may be requiſite to interpoſe a few words, to obviate a miſtake, which, if not prevented may have an ill aſpect, not only upon the firſt ſection, but upon a great part of the following diſcourſe. For I know, that it may be ſaid, that whereas I alledge divers things, to leſſen the lately mentioned delightfulneſs of the ſtudy of phyſick, and to depreciate ſome other advantages, by which the following ſections would recommend it, ſome

ſome of the ſame things may be objected againſt the delighfulneſs of the ſtudy of divinity. But this objection will not, I preſume, much move you, if you conſider the argument and ſcope of the two parts of this letter. For in the former I have ſhewn by poſitive proofs, that the ſtudy of theology is attended with divers advantages, which belong to it, either only as ſome of them do, or principally as others. And now in the ſecond part I come to conſider, whether what is alledged in behalf of the ſtudy of philoſophy, deſerve to counter-balance thoſe prerogatives or advantages; and therefore it neither need be, nor is my deſign, to compare, for inſtance, the delightfulneſs of the two ſtudies, theology and phyſicks, but by ſhewing the inconveniencies, that allay the latter, to weaken the argument, that is drawn from that delightfulneſs, to conclude it preferable to the ſtudy of theology. So that my work, in this and the following ſections, is, not ſo much to inſtitute compariſons, as to obviate or anſwer allegations. For ſince I have in the paſt diſcourſe grounded the excellency of the ſtudy of divinity, chiefly upon thoſe great advantages, that are peculiar to it; my reaſonings would not be fruſtrated, though it ſhould appear, that in point of delightfulneſs, certainty, &c. that ſtudy ſhould, in many caſes, be liable to the ſame objections with the ſtudy of nature, ſince it is not mainly for theſe qualities, but, as I was ſaying, for other and peculiar excellencies, that I recommended divinity. And therefore, ſuppoſing the delightfulneſs, &c. of that and of phyſicks to be allayed by the ſame, or equal inconveniencies or imperfections; that ſuppoſition would not hinder the ſcales to be ſwayed in favour of divinity, upon the ſcore of thoſe advantages, that are unqueſtioned, and peculiarly belong to it. I know not, whether I need add, that notwithſtanding this, you are not to expect, that I ſhould give philoſophy the wounds of an enemy. For my deſign being not to diſcourage you, nor any ingenious man, from courting her at all, nor from courting her much, but from courting her too much, and deſpiſing divinity for her, I employ againſt her not a ſword to wound her, but a balance, to ſhew, that her excellencies, though ſolid and weighty, are leſs ſo, than the preponderating ones of theology. And this temper and purpoſe of mine renders my taſk difficult enough to have, perhaps, ſome right to your pardon, as well as ſome need of it, if I do not every way ſteer ſo exactly, as equally to avoid injuring the cauſe I am to plead for, and diſparaging a ſtudy, which I would ſo little depreciate, that I allow it a great part of my inclinations, and not a little ſhare of my time. And having ſaid this, to keep the deſign of this diſcourſe from being miſunderſtood, I hope we may now proceed to the particulars, whoſe ſcope we have been declaring.

RETURNING then to what I was about to ſay before this long, but needful, advertiſement interrupted me, I ſhall reſume my diſcourſe of the delightfulneſs of the ſtudy of phyſicks, about which I was going in the firſt place to tell you, that I know you and your friend will freely grant me, that the knowledge of the empty and barren phyſiology, that is taught in the ſchools, as it exacts not much pains to be acquired, ſo it affords but little ſatisfaction when attained. And as I know you will give me leave to ſay this; ſo, being warranted by no ſlight experience of my own, I ſhall take leave to ſay alſo, that the ſtudy of that experimental philoſophy, which is that, whereof your friend is ſo much enamoured, is, if it be duly proſecuted, a very troubleſome and laborious employment. For, (to mention at preſent but this) that great variety of objects the naturaliſt is not only by his curioſity, but by their ſecret dependances upon one another, engaged to conſider, and ſeveral ways to handle, will put him upon needing, and conſequently upon applying himſelf to ſuch a variety of mechanick people, (as diſtillers, druggiſts, ſmiths, turners, &c.) that a great part of his time, and perhaps all his patience, ſhall be ſpent in waiting upon tradeſmen, and repairing the loſſes he ſuſtains by their diſappointments, which is a drudgery greater

 than

than any, who has not tried it, will imagine, and which yet being as inevitable as unwelcome, does very much counter-balance and allay the delightfulneſs of the ſtudy we are treating of. In which ſo great a part of a man's care and time muſt be laid out in providing the apparatus's neceſſary for the trying of experiments.

But this is not all. For when you have brought an experiment to an iſſue, though the event may often prove ſuch as you will be pleaſed with, yet it will ſeldom prove ſuch as you can acquieſce in. For it fares not with an inquiſitive mind in ſtudying the book of nature, as in reading of *Æſop*'s fables, or ſome other collection of apologues of differing ſorts, and independent one upon another; where, when you have read over as many at one time as you think fit, you may leave off when you pleaſe, and go away with the pleaſure of underſtanding thoſe you have peruſed, without being ſolicited by any troubleſome itch of curioſity to look after the reſt, as thoſe, which are needful to the better underſtanding of thoſe you have already gone over, or that will be explicated by them, and ſcarce without them. But in the book of nature, as in a well-contrived romance, the parts have ſuch a connection and relation to one another, and the things we would diſcover are ſo darkly or incompleatly knowable by thoſe, that precede them, that the mind is never ſatisfied till it comes to the end of the book; till when all that is diſcovered in the progreſs, is unable to keep the mind from being moleſted with impatience, to find that yet concealed, which will not be known, till one does at leaſt make a further progreſs. And yet the full diſcovery of nature's myſteries is ſo unlikely to fall to any man's ſhare in this life, that the caſe of the purſuers of them is at beſt like theirs, that light upon ſome excellent romance, of which they ſhall never ſee the latter parts. For indeed (to ſpeak now without a ſimile) there is ſuch a relation betwixt natural bodies, and they may in ſo many ways (and divers of them unobſerved) work upon, or ſuffer from one another, that he who makes a new experiment, or diſcovers a new phænomenon, muſt not preſently think, that he has diſcovered a new truth, or detected an old error. For, (at leaſt if he be a conſidering man) he will oftentimes find reaſon to doubt, whether the experiment or obſervation have been ſo ſkilfully and warily made in all circumſtances, as to afford him ſuch an account of the matter of fact, as a ſevere naturaliſt would deſire. And then, ſuppoſing the hiſtorical part no way defective, there are far more caſes than are taken notice of, wherein ſo many differing agents may produce the exhibited phænomenon, or have a great influence upon the experiment or obſervation, that he muſt be leſs jealous than becomes a philoſopher, to whom experiments do not oftentimes as well ſuggeſt new doubts, as preſent new phænomena.

And even thoſe trials, that end in real diſcoveries, do, by reaſon of the connection of phyſical truths, and the relations that natural bodies have to one another, give ſuch hopes and ſuch deſires of improving the acquiſts we have already made, to the explicating of other difficulties, or the making of further diſcoveries, that an inquiſitive naturaliſt finds his work to encreaſe daily upon his hands, and the event of his beſt toils, whether it be good or bad, does but engage him into new ones, either to free himſelf from his ſcruples, or improve his ſucceſſes. So that, though the pleaſure of making phyſical diſcoveries is, in itſelf conſidered, very great; yet this does a little impair it, that the ſame attempts, which afford that delight, do ſo frequently beget both anxious doubts, and a diſquieting curioſity. So that, if knowledge be, as ſome philoſophers have ſtiled it, the aliment of the rational ſoul, I fear I may too truly ſay, that the naturaliſt is uſually fain to live upon ſallads and ſauces, which, though they yield ſome nouriſhment, excite more appetite than they ſatisfy, and give us indeed the pleaſure of eating with a good ſtomach, but then reduce us to an unwelcome neceſſity of always riſing hungry from the table.

Of

OF divers things, that leſſen the delightfulneſs of phyſiological ſtudies, I do ſo amply diſcourſe in other papers, that I might well remit you thither; but indeed it is not neceſſary, that I ſhould inſiſt on this argument any further. It is true, that ſuch a reference might be very proper, if the myſteries of theology and phyſick were like thoſe of theology and necromancy, or ſome other part of unlawful magick, whereof the former could not be well reliſhed without an abhorrence of the latter. But as the two great books, of nature and of ſcripture, have the ſame author; ſo the ſtudy of the latter does not at all hinder an inquiſitive man's delight in the ſtudy of the former. The doctor I am pleading for, may as much reliſh a phyſical diſcovery, as *Phyſiophilus*; nay, by being addicted to theology and religion, he is ſo far from being uncapable of the contentments accruing from the ſtudy of nature, that beſide thoſe things, that recommend it to others, there are ſeveral things, that peculiarly endear it to him.

FOR 1. he has the contentment to look upon the wonders of nature, not only as the productions of an admirably wiſe author of things, but of ſuch an one, as he intirely honours and loves, and to whom he is related. He, that reads an excellent book, or ſees ſome rare engine, will be otherwiſe affected with the ſight or the peruſal, if he knows it to have been made by a friend, or a parent, than if he conſiders it but as made by a ſtranger, whom he has no particular reaſon to be concerned for. And if *Reboboam* did not as well degenerate from the ſentiments of mankind, as from his family, he could not but look upon that magnificent temple of *Solomon* with another eye, than did the throngs of ſtrangers, that came only to gaze at it, as an admirable piece of architecture, whilſt he conſidered, that it was his father, that built it. And if, as we ſee, the ſame heroick actions, which we read in hiſtory, of ſome great monarch, that ſtrangers barely and unconcernedly admire, the natives of his country do not only venerate, but affectionately intereſt themſelves therein, becauſe they are his country-men, and their anceſtors were his ſubjects: how much may we ſuppoſe the ſame actions would affect them, if they had the honour to be that prince's children? We may well therefore preſume, that it is not without a ſingular ſatisfaction, that the contemplator, we are ſpeaking of, does in all the wonders of nature diſcover, how wiſe, and potent, and bountiful, that author of nature is, in whom he has a great intereſt, and that ſo great an one, as both to be admitted into the number of his friends, and adopted into the number of his ſons, and is thereby in ſome meaſure, concerned in all the admirations and praiſes, that are paid, either by himſelf or others, to thoſe adorable attributes, that God has diſplayed in that great maſter-piece of power and wiſdom, the world. And when he makes greater diſcoveries in theſe expreſſes and adumbrations of the divine perfections, the delightfulneſs of his contemplation is proportionably increaſed upon ſuch an account, as that, which endears to the paſſionate lover of ſome charming beauty an excellent, above an ordinary, picture of her; becauſe that the ſame things, that make him, as it does other gazers, look upon it as a finer piece, make him look upon it as the more like his miſtreſs, and thereby entertain him with the ſublimer ideas of the beloved original; to whoſe tranſcendent excellencies he ſuppoſes, that the nobleſt repreſentations muſt be the moſt reſembling.

AND there is a farther reaſon, why our contemplator ſhould find a great deal of contentment in theſe diſcoveries. For we have in our nature ſo much of imperfection, and withall ſo much of inclination to ſelf-love, that we do too confidently proportion our ideas of what God can do for us, to what we have already the knowledge, or the poſſeſſion of. And though, when we make it our buſineſs, we are able with much ado ſomewhat to enlarge our apprehenſions, and raiſe our expectations beyond their wonted pitch; yet ſtill they will be but ſcantly promoted and heightened, if thoſe things

things themſelves be but mean and ordinary, which we think we have done enough, if we make them ſurpaſs. A country villager, born and bred in a homely cottage, cannot have any ſuitable apprehenſions of the pleaſures and magnificence of a great monarch's court. And if he ſhould be bid to ſcrue up his imagination to frame ideas of them, they would be borrowed from the beſt tiled houſe he had ſeen in the market-towns, where he had ſold his turnips or corn, and the wedding feaſt of ſome neighbouring farmer's daughter. And though a child in the mother's womb had the perfect uſe of reaſon, yet could it not in that dark cell have any ideas of the ſun or moon, or beauties or banquets, or algebra, or chemiſtry, and many other things, which his elder brothers, that breath freſh air, and freely behold the light, and are in a more mature eſtate, are capable of knowing and enjoying. Now among thinking men, whoſe thoughts run much upon that future ſtate, which they much ſhortly enter into, but ſhall never paſs out of; there will frequently and naturally ariſe a diſtruſt, which, though ſeldom owned, proves oftentimes diſquieting enough. For ſuch men are apt to queſtion, how the future condition, which the goſpel promiſes, can afford them ſo much happineſs as it pretends to; ſince they ſhall in heaven but contemplate the works of God, and praiſe him, and converſe with him; all which they think may, though not immediately, be done by men here below, without being happy. But he, that by teleſcopes and microſcopes, dexterous diſſections, and well employed furnaces, &c. diſcovers the wondrous power and ſkill of him, that contrived ſo vaſt and immenſe a maſs of matter into ſo curious a piece of workmanſhip as this world, will pleaſingly be convinced of the boundleſs power and goodneſs of the great architect. And when he ſees, how admirably every animal is furniſhed with parts requiſite to his reſpective nature; and that there is particular care taken, that the ſame animal, as for example, man, ſhould have differing proviſions made for him, according to his differing ſtates within the womb, and out of it, (a human egg, and an embrio, being much otherwiſe nouriſhed and fitted for action, than is a (compleat) man;) he, I ſay, who conſiders this, and obſerves the ſtupendous providence, and excellent contrivances, that the curious priers into nature (and none but they) can diſcover, will be as well enabled as invited to reaſon thus within himſelf; that ſure God, who has with ſuch admirable artifice framed ſilk-worms, butterflies, and other meaner inſects, and with ſuch wonderful providence taken care, that the nobler animals ſhould as little want any of all the things requiſite to the compleating of their reſpective natures; and who, when he pleaſes, can furniſh ſome things with qualifications quite differing from thoſe, which the knowledge of his other works could have made us imagine, (as is evident in the load ſtone, and in quick-ſilver among minerals, and the ſenſitive plant among vegetables, the camelion among animals, &c.) this God, I ſay, muſt needs be fully able to furniſh thoſe he delights to honour, with objects ſuitable to their improved faculties, and with all, that is requiſite to the happineſs he intends them in their glorified ſtate; and is able to bring this to paſs by ſuch amazing contrivances, as perhaps will be quite differing from any, that the things we have yet ſeen ſuggeſt to us any ideas of. And ſure he, that has in ſo immenſe, ſo curious, and ſo magnificent a fabrick, make ſuch proviſion for men, who are either deſperately wicked, or but very imperfectly good, and in a ſtate, where they are not to enjoy happineſs, but by obedience and ſufferings to fit themſelves for it, may ſafely be truſted with finding them in heaven employments and delights becoming the felicity he deſigns them there; as we ſee, that here below he provides as well for the ſoaring eagle, as for the creeping caterpillar, (and is able to keep the ocean as fully ſupplied with rivers, as lakes or ponds are with ſprings and brooks.) And as a ſtate of celeſtial happineſs is ſo great a bleſſing, that thoſe things, that afford us either greater aſſurances, or greater foretaſtes of it, are

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of the number of the greateſt contentments and advantages, that ſhort of it we can enjoy; ſo it is hard for any divine to receive ſo much of this kind of ſatisfaction, as he, who by ſkilfully looking into the wonders of nature, has his apprehenſions of God's power and manifold wiſdom (as an apoſtle calls it) elevated and enlarged. As when the queen of *Sheba* had particularly ſurveyed the aſtoniſhing prudence, that *Solomon* Epheſ. iii. 10. diſplayed in the ordering of his magnificent court, ſhe tranſportedly concluded thoſe ſervants of his to be happy enough to deſerve a monarch's envy, that were allowed the honour and priviledge of a conſtant and immediate attendance on him.

SECTION II.

I DOUBT not but you have too good an opinion of your friend, not to think, that you may alledge in his favour, that the chief thing, which makes him prefer phyſiology to all other kind of knowledge, is, that it enables thoſe, who are proficients in it, to do a great deal of good, both by improving of trades, and by promoting of phyſick itſelf. And I am too mindful of what I writ to *Pyrophilus*, to deny, either that it can aſſiſt a man to advance phyſick and trades, or that, by ſo doing, he may highly advantage mankind. And this I (who would not leſſen your friend's eſteem for phyſicks, but only his partiality) willingly acknowledge to be ſo allowable an endearment of experimental philoſophy, that I do not know any thing, that to men of a humane, as well as ingenious diſpoſition, ought more to recommend the ſtudy of nature, except the opportunity it affords men to be juſt and grateful to the author both of nature and of man. I do not then deny, that the true naturaliſt may very much benefit mankind; but I affirm, that, if men be not wanting to themſelves, the divine may benefit them much more. It were not perchance either unſeaſonable, or impertinent, to tell you on this occaſion, that he, who effectually teaches men to ſubdue their luſts and paſſions, does as much as the phyſician contribute to the preſervation of their bodies, by exempting them from thoſe vices, whoſe no leſs uſual than deſtructive effects are wars, and duels, and rapines, and deſolations, and the pox, and ſurfeits, and all the train of other diſeaſes, that attend gluttony and drunkenneſs, idleneſs and luſt; which are not enemies to man's life and health barely upon a phyſical account, but upon a moral one, as they provoke God to puniſh them with temporal as well as ſpiritual judgments; ſuch as plagues, wars, famines, and other publick calamities, that ſweep away a great part of mankind; beſides thoſe perſonal afflictions of bodily ſickneſs, and diſquiets of conſcience, that do both ſhorten men's lives, and imbitter them. Whereas piety having (as the ſcripture aſſures us) promiſes both of this life, and of that, which is to come, thoſe teachers, that make men virtuous and religious, by making them temperate, and chaſte, and inoffenſive, and calm, and contented, do not only procure them great and excellent diſpoſitions to thoſe bleſſings, both of the right hand and of the left, which God's goodneſs makes him forward to beſtow on thoſe, who by grace and virtue are made fit to receive them; but do help them to thoſe qualifications, that by perſerving the mind in a calm and cheerful temper, as well as by affording the body all, that temperance can confer, do both lengthen their lives, and ſweeten them. Theſe things, I ſay, it were not impertinent to inſiſt on; but I will rather chuſe to repreſent to you, that the benefits, which men may receive from the divine, ſurpaſs thoſe, which they receive from the naturaliſt, both in the nobleneſs of the advantages, and in the duration of them.

BE it granted then, that the naturaliſt may much improve both phyſick and trades; yet ſince theſe themſelves were deviſed for the ſervice of the body, (the one to preſerve or or reftore his health, and the other to furnifh it with accommodations or delights;) the boafted ufe of natural philofophy, by its advancing trades and phyfick, will ftill be to ferve the body, which is but the lodging and inftrument of the foul, and which, I prefume, your friend, and which I am fure your felf will be far from thinking the nobleft part of man. I know it may be faid, nor do I deny it, that divers mechanical arts are highly beneficial, not only to the inventors, but to thofe places, and perhaps thofe ftates, where fuch improvements are found out and cherifhed. But though I moft willingly grant, that this confideration ought to recommend experimental philofophy, as well to ftates as to private perfons; yet many of thefe improvements do rather transfer than encreafe mankind's goods, and prejudice one fort of men as much as they advantage another, (as in the cafe of the eaftern fpices, of whofe trade the Portugals and Dutch by their later navigations, did, by appropriating it to themfelves, deprive the Venetians) or elfe do but increafe that, which, though very beneficial to the producers, is not really fo to mankind in general: of which we have an example in the invention of extracting gold and filver out of the ore, with mercury. For though it have vaftly enriched the Spaniards in the *Weft Indies*, yet it is not of any folid advantage to the world; no more than the difcovery of the Peruvian and other American mines; by which, (efpecially reckoning the multitudes of unhappy men, that are made miferable, and deftroyed in working them,) mankind is not put into a better condition than it was before. And if the philofopher's ftone itfelf, (fuppofing there be fuch a thing) were not an incomparable medicine, but were only capable of tranfmuting other metals into gold I fhould perhaps doubt, whether the difcoverer of it would much advantage mankind; there being already gold and filver enough to maintain trade and commerce among men; and for all other purpofes, I know not, why a plenty of iron, brafs, and quickfilver, which are far more ufeful metals, fhould not be more defirable. But not to urge this; we may confider, that thefe advancements of enriching trades do ftill bring advantages but to the outward man, and thofe many arts and inventions, that aim at the heightening the pleafures of the fenfes*, belong but to the body; and even in point of gratifying that, are not fo requifite and important, as many fuppofe; education, cuftom, &c. having a greater intereft, than moft imagine, in the relifh men have even of fenfitive pleafures. And as for phyfick, not to mind you, that it has been loudly, (how juftly, I here examine not,) complained of, that the new philofophy has made it far greater promifes than have yet been performed; I fhall only take notice, that fince all, that phyfick is wont to pretend to, is, to preferve health, or reftore it; there are multitudes in the world, that have no need of the affiftance the naturalift would give the phyfician; and a healthy man, as fuch, is already in a better condition, than the philofopher can hope to place him in, and is no more advantaged by the naturalift's contribution to phyfick, than a found man, that fleeps in a whole fkin, is by all the fine tools of a furgeon's cafe of inftruments, and the various compofitions of his cheft.

And as the benefits, that may be derived from theology, much furpafs thofe, that acrue from phyficks, in the noblenefs of the fubject they relate to; fo have they a great advantage in point of duration. For all the fervices, that medicines, and engines, and improvements can do a man, as they relate but to this life, fo they determine with it. Phyfick indeed, and chymiftry do, the one more faintly, and the other more boldly, pretend fometimes, not only to the cure of difeafes, but the prolongation of life; but fince none will fufpect, but that the mafters of thofe parts of knowledge would employ their utmoft fkill to protract their own lives, thofe, that remember, that *Solomon* and *Helmont* lived no longer, than millions, that were ftrangers to philofophy;

* See examples of this, in my notes about Senfation and Senfible Qualities.

losophy; and that even *Paracelsus* himſelf, for all his boaſted *Arcana*, is by *Helmont* and other chemiſts confeſſed to have died ſome years ſhort of fifty; we may very juſtly fear, that nature will not be ſo kind to her greateſt votaries, as to give them much more time than other men, for the payment of the laſt debt all men owe her. And if a few years reſpite could by a ſcrupulous and troubleſome uſe of diet and remedies be obtained; yet that, in compariſon of the eternity, that is to follow, is not at all conſiderable. But whereas within no great number of years, (a little ſooner, or a little later) all the remedies, and reliefs, and pleaſures, and accommodations, that philoſophical improvements can afford a man, will not keep him from the grave, (which within very few days will make the body of the greateſt virtuoſo as hideous and as loathſome a carcaſe, as that of any ordinary man;) the benefits, that may accrue to us by divinity, as they relate chiefly, though not only, to the other world; ſo they will follow us out of this, and prove then incomparably greater than ever, when they alone ſhall be capable of being enjoyed. So that philoſophy, in the capacity we here conſider it, does but as it were provide us ſome little conveniences for our paſſage, like ſome accommodations for a cabbin, which outlaſts not the voyage; but religion provides us a vaſt and durable eſtate, or, as the ſcripture ſtiles it, an unſhaken kingdom, when we are arrived at our journey's end. And therefore the benefits accruing from religion, may well be concluded preferable to their competitors, ſince they not only reach to the mind of man, but reach beyond the end of time itſelf; whereas all the variety of inventions, that philoſophy ſo much boaſts of, as whilſt they were in ſeaſon they were deviſed for the ſervice of the body, ſo they make us buſy, and pride ourſelves about things, that within a ſhort time will not (ſo much as upon its ſcore) at all concern us.

SECTION III.

I EXPECT you ſhould here urge on your friend's behalf, that the ſtudy of phyſicks has one prerogative, (above that of divinity,) which, as it is otherwiſe a great excellency, ſo does much add to the delightfulneſs of it. I mean, the certainty, and clearneſs, and thence reſulting ſatisfactorineſs of our knowledge of phyſical, in compariſon of any we can have of theological matters, whoſe being dark and uncertain, the nature of the things themſelves, and the numerous controverſies of differing ſects about them, ſufficiently manifeſt.

BUT upon this ſubject, divers things are to be conſidered.

FOR firſt, as to the fundamental and neceſſary articles of religion, I do not admit the allegation, but take thoſe articles to be both evident, and capable of a moral demonſtration. And if there be any articles of religion, for which a rational and cogent proof cannot be brought, I ſhall for that very reaſon conclude, that ſuch articles are not abſolutely neceſſary to be believed; ſince it ſeems no way reaſonable to imagine, that God having been pleaſed to ſend not only his prophets and his apoſtles, but his only ſon into the world, to promulgate to mankind the Chriſtian religion, and both to cauſe it to be conſigned to writing, that it may be known, and to alter the courſe of nature by numerous miracles, that it might be believed; it ſeems not reaſonable, I ſay, to imagine, that he ſhould not propoſe thoſe truths, which he in ſo wonderful and ſo ſolemn a manner recommended, with at leaſt ſo much clearneſs, as that ſtudious and well-diſpoſed readers may certainly underſtand ſuch, as are neceſſary for them to believe.

2. THOUGH I will not here engage myſelf in a diſquiſition of the ſeveral kinds, or, if you pleaſe, degrees, of demonſtration (which yet is a ſubject, that I judge far more

confiderable than cultivated,) yet I muft tell you, that as a moral certainty (fuch as we may attain about the fundamentals of religion) is enough in many cafes for a wife man, and even a philofopher to acquiefce in; fo that phyfical certainty, which is pretended for the truths demonftrated by naturalifts, is, even where it is rightfully claimed, but an inferior kind or degree of certainty, as moral certainty alfo is. For even phyfical demonftrations can beget but a phyfical certainty, (that is, a certainty upon fuppofition, that the principles of phyfick be true,) not a metaphyfical certainty, (wherein it is abfolutely impoffible, that the thing believed fhould be other than true.) For inftance, all the phyfical demonftrations of the antients about the caufes of particular phænomena of bodies fuppofe, that *ex nihilo nihil fit*; and this may readily be admitted in a phyfical fenfe, becaufe, according to the courfe of nature, no body can be produced out of nothing, but fpeaking univerfally it may be falfe, as Chriftians generally, and even the Cartefian naturalifts, afferting the creation of the world, muft believe, that, *de facto*, it is. And fo whereas *Epicurus* does, I remember, prove, that a body once dead cannot be made alive again, by reafon of the diffipation and difperfion of the atoms, it was, when alive, compofed of; though all men will allow this affertion to be phyfically demonftrable, yet the contrary may be true, if God's omnipotence intervenes, as all the philofophers, that acknowledge the authority of the New Teftament, where *Lazarus* and others are recorded to have been raifed from the dead, muft believe, that it actually did appear, and even all unprejudiced reafoners muft allow it to be poffible, there being no contradiction implied in the nature of the thing. But now to affirm, that fuch things, as are indeed contradictories, cannot be both true, or that *factum infectum reddi non poteft*, are metaphyfical truths, which cannot poffibly be other than true, and confequently beget a metaphyfical and abfolute certainty. And your mafter *Cartefius* was fo fenfible of a dependance of phyfical demonftrations upon metaphyfical truths, that he would not allow any certainty not only to them, but even to geometrical demonftrations, until he had evinced, that there is a God, and that he cannot deceive men, that make ufe of their faculties aright.

To which I may add, that even in many things, that are looked upon as phyfical demonftrations, there is really but a moral certainty. For when, for inftance, *Des Cartes* and other modern philofophers, take upon them to demonftrate, that there are divers comets, that are not meteors, becaufe they have a parallax leffer than that of the moon, and are of fuch a bignefs, and fome of them move in fuch a line, &c. it is plain, that divers of thefe learned men had never the opportunity to obferve a comet in their lives, but take thefe circumftances upon the credit of thofe aftronomers, that had fuch opportunities. And though the inferences, as fuch, may have a demonftrable certainty; yet the premifes they are drawn from having but an hiftorical one, the prefumed phyfico-mathematical demonftration can produce in a wary mind but a moral certainty, and not the greateft neither of that kind, that is poffible to be attained; as he will not fcruple to acknowledge, that knows by experience, how much more difficult it is, than moft men imagine, to make obfervations about fuch nice fubjects, with the exactnefs, that is requifite for the building of an udoubted theory upon them. And there are I know not how many things in phyficks, that men prefume they believe upon phyfical and cogent arguments, wherein they really have but a moral affurance; which is a truth heeded by fo few, that I have been invited to take the more particular notice of them in other papers, written purpofely to fhow the doubtfulnefs and incompleatnefs of natural philofophy; of which difcourfe, fince you may command a fight, I fhall not fcruple to refer you thither for the reafons of my affirming here, that the moft even of the modern virtuofi are wont to fancy more of clearnefs and certainty in their phyfical theories, than a critical examiner will find. Only, that you may not look upon this as a put off, rather than a reference, I will here touch upon a couple of ſubjects, which men are wont to believe to be, and which indeed ought to be, the moſt thoroughly underſtood; I mean the nature of body in general, and the nature of ſenſation.

AND for the firſt of theſe, ſince we can turn ourſelves no way, but we are every where environed, and inceſſantly touched by corporeal ſubſtances, one would think, that ſo familiar an object, that does ſo aſſiduouſly, and ſo many ways affect our ſenſes, and for the knowledge of which we need not enquire into the diſtinct nature of particular bodies, nor the properties of any one of them, ſhould be very perfectly known unto us. And yet the notion of body in general, or what it is, that makes a thing to be a corporeal ſubſtance, and diſcriminates it from all other things, has been very hotly diſputed of, even among the modern philoſophers, *& adhuc ſub judice lis eſt.* And though your favourite *Des Cartes*, in making the nature of a body to conſiſt in extenſion every way, has a notion of it, which it is more eaſy to find fault with, than to ſubſtitute a better; yet I fear, it will appear to be attended, not only with this inconvenience, that God cannot, within the compaſs of this world, wherein if any body vaniſh into nothing, the place or ſpace left behind it, muſt have the three dimenſions, and ſo be a true body, annihilate the leaſt particle of matter, at leaſt without, at the ſame inſtant and place, creating as much (which agrees very ill with that neceſſary and continual dependance, which he aſſerts matter itſelf to have on God for its very being;) but with ſuch other inconveniences, that ſome friends of yours, otherwiſe very inclinable to the Carteſian philoſophy, know not how to acquieſce in it: and yet I need not tell you, how fundamental a notion the deviſer of it aſſerts it to be. Neither do I ſee, how this notion of a corporeal ſubſtance will any more than any of the formerly received definitions of it, extricate us out of the difficulties of that no leſs perplexed, than famous controverſy, *de compoſitione continui.* And though ſome ingenious men, who perhaps perceive better than others, how intricate it is, have of late endeavoured to ſhew, that men need not be ſolicitous to determine this controverſy, it not being rightly propoſed by the ſchool-men, that have ſtarted it; and though I perhaps think, that natural philoſophy may be daily advanced without the deciſion of it, becauſe there is a multitude of conſiderable things to be diſcovered and performed in nature, without ſo much as dreaming of this controverſy; yet ſtill, as I would propoſe the queſtion, the difficulties, till removed, will ſpread a thick night over the notion of body in general. For either a corporeal and extended ſubſtance is (either really or mentally) diviſible into parts endowed with extenſion, and each of theſe parts is diviſible alſo in other corporeal parts, leſſer and leſſer, *in infinitum*; or elſe this ſubdiviſion muſt ſtop ſomewhere, (for there is no mean between the two members of the diſtinction;) and in either caſe the opinion pitched upon will be liable to thoſe inconveniences, not to ſay abſurdities, that are rationally urged againſt it by the maintainers of the oppoſite; the objections on both ſides being ſo ſtrong, that ſome of the more candid, even of the modern metaphyſicians, after having tired themſelves and their readers with arguing *Pro* and *Con*, have confeſſed the objections on both ſides to be inſoluble.

BUT though we do not clearly underſtand the nature of body in general; yet ſure we cannot but be perfectly acquainted with what paſſes within ourſelves in reference to the particular bodies we daily ſee, and hear, and ſmell, and taſte, and touch. But alas, though we know but little, ſave by the informations of our ſenſes; yet we know very little of the manner, by which our ſenſes inform us. And to avoid prolixity, I will at preſent ſuppoſe with you, that the ingenious *Des Cartes* and his followers have given the faireſt account of ſenſation, that is yet extant. Now, according to him, a man's body being but a well organized ſtatue, that, which is truly called ſenſation, is not performed by the organ, but by the mind, which perceives the motion produced in the organ; (for which reaſon he will not allow brutes to have ſenſe properly ſo called;) ſo that if you aſk a Carteſian, how it comes to paſs, that the ſoul of man, which he juſtly aſſerts to be an immaterial ſubſtance, comes to be wrought upon, and that in ſuch various manners, by thoſe external bodies, that are the objects of our ſenſes, he will tell you, that by their impreſſions on the ſenſories, they variouſly move the fibres or threads of the nerves, wherewith thoſe parts are endowed, and by which the motion is propagated to that little kernel in the brain, called by many writers the Conarion, where theſe differing motions being perceived by the there reſiding ſoul, become ſenſations, becauſe of the intimate union, and, as it were, permiſtion (as *Carteſius* himſelf expreſſes it) of the ſoul with the body.

But now, Sir, give me leave to take notice, that this union of an incorporeal, with a corporeal ſubſtance, (and that without a medium) is a thing ſo unexampled in nature, and ſo difficult to comprehend, that I ſomewhat queſtion, whether the profound ſecrets of theology, not to ſay the adorable myſtery itſelf of the incarnation, be more abſtruſe than this. For how can I conceive, that a ſubſtance purely immaterial, ſhould be united without a phyſical medium, (for in this caſe there can be none,) with the body, which cannot poſſibly lay hold on it, and which it can pervade, and fly away from at pleaſure, as *Des Cartes* muſt confeſs the ſoul actually does in death. And it is almoſt as difficult to conceive, how any part of the body, without excepting the animal ſpirits, or the Conarion, (for theſe are as truly corporeal, as other parts of the human ſtatue,) can make impreſſions upon a ſubſtance perfectly incorporeal, and which is not immediately affected by the motions of any other parts, beſides the *Genus Nervoſum.* Nor is it a ſmall difficulty to a mere naturaliſt (who, as ſuch, does not in phyſical matters take notice of revelations about angels,) to conceive, how a finite ſpirit can either move, or, which is much the ſame thing, regulate and determine the motion of a body. But that, which I would on this occaſion invite you to conſider, is, that ſuppoſing the ſoul does in the brain perceive the differing motions communicated to the outward ſenſes; yet this, however it may give ſome account of ſenſation in general, will not at all ſhew us a ſatisfactory reaſon of particular and diſtinct ſenſations. For if I demand, why, for inſtance, when I look upon a bell, that is ringing, ſuch a motion or impreſſion in the Conarion produces in the mind that peculiar ſort of perception, ſeeing, and not hearing; and another motion, though coming from the ſame bell, at the ſame time, produces that quite differing ſort of perception, that we call ſound, but not viſion; what can be anſwered, but that it was the good pleaſure of the author of human nature to have it ſo? And if the queſtion be aſked about the differing objects of any one particular ſenſe; as, why the great plenty of unperturbed light, that is reflected from ſnow, milk, &c. does produce a ſenſation of whiteneſs, rather than redneſs or yellowneſs? or why the ſmell of caſtor, or aſſa fœtida, produces in moſt perſons that, which they call a ſtink, rather than a perfume? (eſpecially ſince we know ſome hyſterical women, that think it not only a wholſome, but a pleaſing ſmell.) And if alſo you further aſk, why melody and ſweet things do generally delight us? and diſcords and bitter things do generally diſpleaſe us? Nay, why a little more than enough of ſome objects, that produce pleaſure, will produce pain? (as may be exemplified in a cold hand, as it happens to be held out at a juſt, or at too near a diſtance from the fire:) if, I ſay, theſe, and a thouſand other queſtions of the like kind, be aſked, the anſwer will be but the general one, that is already given, that ſuch is the nature of man. For to ſay, that moderate motions are agreeable to the nature of the ſenſory they are excited in, but violent and diſorderly ones (as jarring ſounds, and ſcorching heat) do put it into too violent a motion for its texture; will by no means ſatisfy. For beſides that this anſwer gives no account of the variety of ſenſations of the ſame kind, as of differing colours, taſtes, &c. but reaches only to pleaſure and pain; even as to theſe, it will reach but a very little way; unleſs the givers of it can ſhow, how an immaterial ſubſtance ſhould be more harmed by the briſker motion of a body, than by the more languid.

AND as you and your friend think, you may juſtly ſmile at the Ariſtotelians, for imagining, that they have given a tolerable account of the qualities of bodies, when they have told us, that they ſpring from certain ſubſtantial forms, though when they are aſked particular queſtions about theſe incomprehenſible forms, they do in effect but tell us in general, that they have ſuch and ſuch faculties, or effects, becauſe nature, or the author of nature, endowed them therewith; ſo, I hope, you will give me leave to think, that it may keep us from boaſting of the clearneſs and certainty of our knowledge, about the operations of ſenſible objects, whilſt, as the Ariſtotelians cannot particularly ſhow, how their qualities are produced, ſo we cannot particularly explicate, how they are perceived; the principal thing, that we can ſay, being in ſubſtance this, that our ſenſations depend upon ſuch an union or permiſtion of the ſoul and body, as we can give no example of in all nature, nor no more diſtinct account of, than that it pleaſed God ſo to couple them together. But I beg your pardon for having detained you ſo long upon one ſubject, though perhaps it will not prove time miſ-ſpent, if it have made you take notice, that in ſpight of the clearneſs and certainty, for which your friend ſo much prefers phyſicks before theology, we are yet to ſeek, (I ſay yet, becauſe I know not what time may hereafter diſcover) both for the definition of a corporeal ſubſtance, and a ſatisfactory account of the manner of ſenſation: though without the true notion of a body we cannot underſtand, that object of phyſicks in general, and without knowing the nature of ſenſation, we cannot know that, from whence we derive almoſt all that we know of any body in particular.

IF after all this your friend ſhall ſay, that *Des Cartes*'s account of body, and other things in phyſicks, being the beſt, that men can give, if they be not ſatisfactory, it muſt be imputed to human nature not to the Carteſian doctrine, I ſhall not ſtay to diſpute, how far the allegation is true; eſpecially ſince, though it be admitted, it will not prejudice my diſcourſe. For, whatſoever the cauſe of the imperfection of our knowledge about phyſical matters be, that there is an imperfection in that knowledge is manifeſt; and that ought to be enough to keep us from being puffed up by ſuch an imperfect knowledge, and from undervaluing upon its account the ſtudy of thoſe myſteries of divinity, which, by reaſon of the nobleneſs and remoteneſs of the objects, may much better than the nature of corporeal things, (which we ſee, and feel, and continually converſe with,) have their obſcurity attributed to the weakneſs of our human underſtandings. And if it be a neceſſary imperfection of human nature, that, whilſt we remain in this mortal condition, the ſoul being confined to the dark priſon of the body, is capable (as even *Ariſtotle* ſomewhere confeſſes) but of a dim knowledge; ſo much the greater value we ought to have for Chriſtian religion, ſince, by its means (and by no other without it) we may attain a condition, wherein, as our nature will otherwiſe be highly bleſſed and advanced; ſo our faculties will be elevated and enlarged, and probably made thereby capable of attaining degrees and kinds of knowledge, to which we are here but ſtrangers. In favour of which I will not urge the received opinion of divines, that before the fall (which yet is a leſs noble condition than is reſerved for us in heaven,) *Adam*'s knowledge was ſuch, that he was able at firſt ſight of them, to give each of the beaſts a name expreſſive of its nature; becauſe that, in ſpight of ſome ſkill (which my curioſity for divinity, not philoſophy, gave me)

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in the holy tongue, I could never find, that the Hebrew names of animals, mentioned in the beginning of *Genesis*, argued a (much) clearer insight into their natures, than did the names of the same or some other animals in Greek, or other languages: wherefore, as I said, I will not urge *Adam*'s knowledge in paradise for that of the saints in heaven, though the notice he took of *Eve* at his first seeing of her, (if it were not conveyed to him by secret revelation) may be far more probably urged, than his naming of the beasts: but I will rather mind you, that the proto-martyr's sight was strengthened so, as
Acts vii. 56. to "see the heavens opened, and Jesus standing at the right hand of God;" and when
2 Kings vi. 17. the prophet had prayed, that his servant's eyes might be opened, he immediately saw the mountain, where they were, all covered with chariots and horsemen, which, though mentioned to be of fire, were altogether invisible to him before. To which, as a higher argument, I shall only add a couple of passages of scripture, which seem to allow us even vast expectations as to the knowledge our glorified nature may be advanced to.
1 Cor. xiii. 12. The one is that, which St. *Paul* says to the *Corinthians*, "for now we see through a "darkly, but then face to face: now I know in part, but then shall I know even as also "I am known." And the other, where Christ's favourite-disciple tells believers,
1 Joh. iii. 2. "Beloved, now we are the sons of God, and it doth not yet appear what we shall be; "but we know, that when he shall appear, we shall be like him for we shall see him "as he is."

WHAT has hitherto been discoursed, contains the first consideration, that I told you might be proposed about the certainty ascribed to the knowledge we are said to have of natural things; but this is not all I have to represent to you on this subject. For I consider further, that it is not only by the certainty we have of them, that the knowledge of things is endeared to us, but also by the worthiness of the object, the number of those, that are unacquainted with it, the remoteness of it from common apprehensions, the difficulty of acquiring it without peculiar advantages, the usefulness of it when attained, and other particulars, which it is not here necessary to enumerate. I persume, you doubt not but your friend does very much prefer the knowledge he has of the mysteries of nature (at many of which we have as yet but ingenious conjectures) to the knowledge of one, that understands the elements of arithmetick, though he be demonstratively sure of the truth of most of his rules and operations. And questionless *Copernicus* received a much higher satisfaction in his notion about the stability of the sun, and the motion of the earth, though it were not so clear, but that *Tycho*, *Ricciolus*, and other eminent astronomers have rejected it, than in the knowledge of divers of the theorems about the sphere that have been demonstrated by *Euclid*, *Theodosius*, and other geometricans. Our discovering, that some comets are not, as the schools would have them, sublunary meteors, but celestial bodies, and the conjectural theory, which is all, that hitherto we have been able to attain of them, do much better please both your friend, and you, and me, than the more certain knowledge we have of the time of the rising and setting of the fixed stars. And the estimates we can make, by the help of parallaxes, of the heights of those comets, and of some of the planets, though they are uncertain enough, (as may appear by the vastly different distances, that are assigned to those bodies by eminent astronomers;) yet these uncertain, measures of such elevated and celestial lights do far more please us, than that we can by the help of a geometrical quadrant, or some such instrument, take with far greater certainty the height of a tower or a steeple. And so a mathematician, when he probably conjectures at the compass of the terrestrial globe, and divides, though but unaccurately, its surface, first, into proportions of sea and land, and then into regions of such extents and bounds, and, in a word, skilfully plays the cosmographer; thinks himself much more nobly and pleasantly employed, than when, being reduced to play

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the surveyor, he does, with far more certainty, measure how many acres a field contains, and set out, with what hedges and ditches it is bounded. Now, that the knowledge of God, and of those mysteries of theology, that are ignored by far the greatest part of mankind, has more sublime and excellent objects, and is unattained to by much the greatest part even of learned men, and nevertheless is of unvaluable importance, and of no less advantage towards the purifying and improving of us here, and the making us perfect and happy hereafter, the past discourse has very much miscarried, if it have not evinced. Wherefore, as to be admitted into the privy-council of some great monarch, and thereby be enabled to give a probable guess at those thoughts and designs of his, that govern kingdoms, and make the fates of nations, is judged preferable to that clearer knowledge, that a notary can have of the dying thoughts and intentions of an ordinary person, whose will he makes: and as the knowledge of a skilful physician, whose art is yet conjectural, is preferable, to that of a cutler, that makes his dissecting knives, though this man can more certainly perform what he designs in his own profession, than the physician can in his: and (in fine) as the skill of a jeweller, that is conversant about diamonds, rubies, sapphires, and some other sorts of small stones, which being, for the most part brought us out of the *Indies*, we must take many things about them upon report, is, because of the nobleness of the object, preferred to that of a mason, that deals in whole quarries of common stones, and may be sure upon his own experience of divers things concerning them, which as to jewels we are allowed to know but upon tradition: so a more dim and imperfect knowledge of God, and the mysteries of religion, may be more desireable, and upon that account more delightful, than a clearer knowledge of those inferior truths, that physicks are wont to teach.

I must now mention one particular more, which may well be added to those, that peculiarly endear physicks to the divine, that is studious of them. For as he contemplates the works of nature not barely for themselves, but to be the better qualified and excited to admire and praise the author of nature; so his contemplations are delighrful to him, not barely as they afford a pleasing exercise to his reason, but as they procure him a more welcome approbation from his conscience, these distinct satisfactions being not at all inconsistent. And questionless, though *Esau* did at length miss of his aim, yet, while he was hunting venison for the good old patriarch, that desired it of him, besides the pleasure he was used to take in pursuing the deer he chased, he took a great one, in considering, that now he hunted to please his father, and in order to obtain of Gen. xxxvii.
him an inestimable blessing. So, when *David* employed his skilful hand and voice, in praising God with vocal and instrumental musick, he received in one act a double satisfaction, by exercising his skill and his devotion; and was no less pleased with those melodious sounds, as they were hymns, than as they were songs. And this example prompts me to add, that as the devout student of nature we were speaking of, does intentionally refer the knowledge he seeks of the creatures to the glory of the creator; so in his discoveries, that, which most contents him, is, that the wonders he observes in nature, heighten that admiration he would fain raise to a less disproportion to the wisdom of God; and furnish him with a nobler holocaust for those sacrifices of praise, he is justly ambitious to offer up to the deity. And as there is no doubt to be made, but that, when *David* invented (as the scripture intimates, that he did) new instruments Amos vi. 5.
of musick, there was nothing in that invention, that pleased him so much, as that they could assist him to praise God the more melodiously; so the pious student of nature finds nothing more welcome in the discoveries he makes of her wonders, than the rises and helps they may afford him, the more worthily to celebrate and glorify the divine attributes adumbrated in the creatures. And as a huntsman, or a fowler, if he meets

meets with ſome ſtrange bird or beaſt, or other natural rarity, thinks himſelf much the more fortunate, if it happen to be near the court, where he may have the king to preſent it to, than if he were to keep it but for himſelf, or ſome of his companions; ſo our devout naturaliſt has his diſcoveries of nature's wonders endeared to him, by having the deity to preſent them to, in the veneration they excite in the finder, and which they enable him to engage others to join in.

SECTION IV.

BUT I confeſs, Sir, I much fear, that that, which makes your friend have ſuch detracting thoughts of theology, is a certain ſecret pride, grounded upon a conceit, that the attainments of natural philoſophers are of ſo noble a kind, and argue ſo tranſcendent an excellency of parts in the attainer, that he may juſtly undervalue all other learning, without excepting theology itſelf.

YOU will not, I ſuppoſe, expect, that a perſon, who has written ſo much in the praiſe of phyſicks, and laboured ſo much for a little ſkill in it, ſhould now here endeavour to depreciate that ſo uſeful part of philoſophy. But I do not conceive, that it will be at all injurious to it, to prefer the knowledge of ſupernatural to that of mere natural things, and to think, that the truths, which God indiſcriminately expoſes to the whole race of mankind, and to the bad, as well as to the good, are inferior to thoſe myſterious ones, whoſe diſcloſure he reckons among his peculiar favours, and whoſe contemplation employs the curioſity, and, in ſome points, exacts the wonder of the very angels. That I may therefore repreſs a little the overweaning opinion your friend has of his phyſical attainments, give me leave to repreſent a few particulars conducive to that purpoſe.

AND firſt, as for the nobleneſs of the truths taught by theology and phyſicks, thoſe of the former ſort have manifeſtly the advantage, being not only converſant about far nobler objects, but diſcovering things, that human reaſon of itſelf can by no means reach unto; as has been ſufficiently declared in the foregoing part of this letter.

NEXT we may conſider, that, whatever may be ſaid to excuſe pride (if there were any) in *Moſchus* the Phœnician, who is affirmed to have firſt invented the atomical hypotheſis, and in *Democritus* and *Leucippus*, (for *Epicurus* ſcarce deſerves to be named with them) that highly advanced that philoſophy; and in Monſieur *Des Cartes*, who either improved, or at leaſt much innovated the corpuſcularian hypotheſis: whatever, I ſay, may be alledged on the behalf of theſe mens pride; I ſee no great reaſon, why it ſhould be allowed in ſuch as your friend; who, though ingenious men, are neither inventors, nor eminent promoters of the philoſophy they would be admired for, but content themſelves to learn what others have taught, or, at leaſt, to make ſome little further application of the principles, that others have eſtabliſhed, and the diſcoveries they have made. And whereas your friend is not a little proud of being able to confute ſeveral errors of *Ariſtotle*, and the ancients, it were not amiſs if he conſidered, that many of the chief truths, that overthrow thoſe errors, were the productions of time and chance, and not of his daring ratiocinations: for there needs no great wit to diſprove thoſe, that maintain the uninhabitableneſs of the torrid zone, or deny the antipodes, ſince navigators have found many parts of the former well peopled, and ſailing round the earth, have found men living in countries diametrically oppoſite to ours. Nor will it warrant a man's pride, that he believes not the moon to be the only planet, that ſhines with a borrowed light, or the galaxy to be a meteor; ſince that now the teleſcope ſhews us, that *Venus* has her full and wain like the moon, and that the milky

milky way is made up of a vaſt multitude of little ſtars, inconſpicuous to the naked eye. And indeed of thoſe other diſcoveries, that overthrow the aſtronomy of the ancients, and much of their philoſophy about the celeſtial bodies, few or none have any cauſe to boaſt, but the excellent *Galileus*, who pretends tohave been the inventor of the teleſcope: for that inſtrument once diſcovered, to be able to reject the ſeptenary number of the planets, by the detection of the four Satellites of *Jupiter*, or talk of the mountains and valleys in the moon, requires not much more excellency in your friend, than it would to deſcry in a ſhip, where the naked eye could diſcern but the body of the veſſel (to diſcry, I ſay) by the help of a proſpective glaſs, the maſts, and ſails, and deck, and perceive a boat towed at her ſtern: though indeed, *Galileo* himſelf had no great cauſe to boaſt of the invention, though we are much obliged to him for the improvement of the teleſcope, ſince no leſs a maſter of dioptricks than *Des Cartes*, does acknowledge with other writers, that perſpective-glaſſes were not firſt found out by mathematicians or philoſophers, but caſually by one *Metius*, a Dutch ſpectacle maker. On which occaſion I ſhall mind you, that to hide pride from man, divers others of the chief diſcoveries, that have been made in phyſicks, have been the productions, not of philoſophy, but chance, by which gun-power, glaſs, and, for aught we know, the verticity of the load-ſtone, (to which we owe both the *Indies*) came to be found in theſe later ages; as (more recently) the milky veſſels of the myſentery, the new receptacle of the chyle, and that other ſort of veſſels, which moſt men call the lymphæducts, were lighted on but by chance, according to the ingenuous confeſſion of the diſcoverers themſelves.

We may further conſider, that thoſe very things, which are juſtly alledged in the praiſe of the corpuſcularian philoſophy itſelf, ought to leſſen the pride of thoſe, that but make uſe of it. For that hypotheſis, ſuppoſing the whole univerſe (the ſoul of man excepted) to be but a great Automaton, or ſelf-moving engine, wherein all things are performed by the bare motion (or reſt) the ſize, the ſhape, and the ſituation, or texture of the parts of the univerſal matter it conſiſts of; all the phænomena reſult from thoſe few principles, ſingle or combined, (as the ſeveral tunes or chimes, that are rung on five bells,) and theſe fertile principles being already eſtabliſhed by the inventors and promoters of the particularian hypotheſis; all that ſuch perſons, as your friend, are wont farther to do, is but to inveſtigate, or gueſs by what kind of motions the three or four other principles are varied. So that the world being but, as it were, a great piece of clock-work, the naturaliſt, as ſuch, is but a mechanician; however the parts of the engine, he conſiders, be ſome of them much larger, and ſome of them much minuter, than thoſe of clocks or watches. And for an ordinary naturaliſt to deſpiſe thoſe, that ſtudy the myſteries of religion, as much inferior to phyſical truths, is no leſs unreaſonable, than it were for a watchmaker, becauſe he underſtands his own trade, to deſpiſe privy-counſellors, who are acquainted with the ſecrets of monarchs, and myſteries of ſtate; or than it were for a ſhip-carpenter, becauſe he underſtands more of fabrick of the veſſel, to deſpiſe the admiral, that is acquainted with the ſecret deſigns of the prince, and employed about his moſt important affairs.

That great reſtorer of phyſicks, the illuſtrious *Verulam*, who has traced out a moſt uſeful way to make diſcoveries in the intellectual globe, as he calls it, confeſſes, that his work was (to ſpeak in his own terms) *partus temporis potius quam ingenii*. And though I am not of his opinion, where he ſays in another place, that his way of philoſophizing does *exæquare ingenia*; yet I am apt to think, that the fertile principles of the mechanical philoſophy being once ſettled, the methods of enquiring and experimenting being found out, and the phyſico-mechanical inſtruments of working on nature's and art's productions being happily invented, the making of ſeveral leſſer improvements, eſpecially by rectifying of ſome almoſt obvious of ſupine errors of the ſchools, by the aſſiſtance of ſuch facilitating helps, may fall to the lot of perſons not endowed with any extraordinary ſagacity, or accuteneſs of parts. And though the inveſtigation, and clear eſtabliſhment of the true principles of philoſophy, and the deviſing the inſtruments of knowledge, be things, that may be allowed to be the proper work of ſublimer wits; yet, if a man be furniſhed with ſuch aſſiſtances, it is not every diſcourſe, that he makes, or thing, which he does by the help of them, that is difficult enough to raiſe him to that illuſtrious rank. And indeed, divers of the vulgar errors, as well as of ſcholars as other men, being mainly grounded upon the mere and often miſtaken authority of *Ariſtotle*, and perhaps ſome frivolous reaſons of his ſcholaſtic interpreters of ſuch precarious and ungrounded things, that to ruin them, does oftentimes require more of boldneſs than ſkill; it may perhaps be ſaid of your friend, in relation to his philoſophical ſucceſſes againſt ſuch vulgar errors, as I am ſpeaking of, what a Roman ſaid of *Alexander*'s triumph over the effeminate Aſiaticks, *Quod nihil aliud quam bene auſus ſit vana contemnere.* And in ſome caſes it happens that, when once a grand truth, or a happy way of experimenting has been found, divers phænomena of nature, that had been left unexplained, or were left mis-explained by the ſchools, did, in my opinion, require a far leſs ſtraining exerciſe of the mind to unriddle and explain them, than muſt have been requiſite to diſpel the darkneſs, that attended divers theological truths, that are now cleared up, and perhaps than I have myſelf, now and then, employed in ſome of thoſe attempts, to illuſtrate theological matters, that you may have met in ſome papers, that I have preſumed to write on ſuch ſubjects. And indeed the improvements, that ſuch virtuoſi, as your friend, are wont to make of the fertile theorems and hints, that have been preſented them by the founders, or prime benefactors of true natural philoſophy, are ſo poor and ſlender, and do ſo much oftener proceed from induſtry and chance, than they argue a tranſcendent ſagacity, or a ſublimity of reaſon, that, though ſuch perſons may have cauſe enough to be delighted with what they have done, yet they have none to be proud of it; and their performances may deſerve our thanks, and perhaps ſome of our praiſe, but reach not ſo high, as to merit our admiration; which is to be reſerved for thoſe, that have been either framers, or grand promoters, of true and comprehenſive hypotheſis, or (elſe) the authors of other noble and uſeful diſcoveries, many ways applicable.

It will not perhaps be improper to add on this occaſion, that, as our knowledge is not very deep, not reaching with any certainty to the bottom of things, nor penetrating to their intimate or innermoſt natures; ſo its extent is not very large, not being able to give us, with any clearneſs and particularity, an account of the celeſtial and deeply ſubterraneal parts of the world, of which all the others make but a very ſmall (not to ſay contemptible) portion.

For, as to the very globe, that we inhabit, not to mention, how many plants, animals, and minerals, we are as yet wholly ignorant of, and how many others we are but ſlenderly acquainted with; I conſider, that the objects, about which our experiments and enquiries are converſant, do all belong to the ſuperficial parts of the terreſtrial globe, of which the earth, known to us, ſeems to be but as it were the cruſt or ſcurf. But what the internal part of this globe is made up of, is no leſs diſputable, than of what ſubſtance the remoteſt ſtars we can deſcry, conſiſt: for even among the modern philoſophers ſome think, the internal portion of the earth to be pure and elementary earth, which, ſay they, muſt be found there, or no where. Others imagine it to be fiery, and the receptacle, either of natural or helliſh flames. Others will have the body of the terreſtrial globe to be a great and ſolid magnet. And the Carteſians on the other ſide, (though they all admit ſtore of ſubterraneal loadſtones) teach, that

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ſame globe was once a fixed ſtar, and that, though it have ſince degenerated into a planet, yet the internal part of it is ſtill of the ſame nature, that it was before; the change it has received proceeding only from having had its outward parts quite covered over with thick ſpots (like thoſe to be often obſerved about the ſun,) by whoſe condenſation the firm earth we inhabit was formed. And the miſchief is, that each of theſe jarring opinions is almoſt as difficult to be demonſtratively proved falſe as true. For, whereas to the centre of the earth there is, according to the modeſteſt account of our late coſmographers, above three thouſand and five hundred miles; my enquiries among navigators and miners have not yet ſatisfied me, that men's curioſity has actually reached above one mile or two at moſt downwards, (and that not in above three or four places) either into the earth or into the ſea. So that as yet our experience has ſcarce grated any thing deep upon the huſk, if I may ſo ſpeak, without at all reaching the kernal of the terraqueous globe.

AND alas! what is this globe of ours, of which itſelf we know ſo little, in compariſon of thoſe vaſt and luminous globes, that we call the fixed ſtars, of which we know much leſs? For, though former aſtronomers have been pleaſed to give us, with a ſeeming accurateneſs, their diſtances and bigneſſes, as if they had had certain ways of meaſuring them; yet later and better mathematicians will, I know, allow me to doubt of what thoſe have delivered. For ſince it is confeſſed, that we can obſerve no parallax in the fixed ſtars, nor perhaps in the higheſt planets, men muſt be yet to ſeek for a method to meaſure the diſtance of thoſe bodies. And not only the Copernicans make it to be I know not how many hundred thouſands of miles greater than the Ptolomeans, and very much greater than even *Tycho*; but *Ricciolus* himſelf, though a great Anti-Copernican, makes the diſtance of the fixed ſtars vaſtly greater, than not only *Tycho*, but, if I miſ-remember not, than ſome of the Copernicans themſelves. Nor do I wonder at theſe ſo great diſcrepances, (though ſome amount, perhaps, to ſome millions of miles) when I conſider, that aſtronomers do not meaſure the diſtance of the fixed ſtars by their inſtruments, but accommodate it to their particular hypotheſis. And by this uncertainty of the remoteneſs of the fixed ſtars you will eaſily gather, that we are not very ſure of their bulk, no not ſo much as in reference to one another; ſince it remains doubtful, whether the differing ſizes, they appear to us to be of, proceed from a real inequality of bulk, or only from an inequality of diſtance, or partly from one of thoſe cauſes, and partly from the other.

BUT it is not my deſign to take notice of thoſe things, which the famous diſputes among the modern aſtronomers manifeſt to be dubious. For I conſider, that there are divers things relating to the ſtars, which are ſo remote from our knowledge, that the cauſes of them are not ſo much as diſputed of, or enquired into, ſuch as may be among others, why the number of the ſtars is neither greater nor leſſer than it is? why ſo many of thoſe celeſtial lights are ſo placed, as not to be viſible to our naked eyes, nor even when they are helped by ordinary teleſcopes? (which extraordinary good ones have aſſured me of.) Why among the familiarly viſible ſtars, there are ſo many in ſome parts of the ſky, and ſo few in others? why their ſizes are ſo differing, and yet not more differing? why they are not more orderly placed, ſo as to make up conſtellations of regular or handſome figures (of which the triangle is, perhaps, the ſingle example) but ſeem to be ſcattered in the ſky as it were by chance, and have as confuſed configurations, as the drops, that fall upon one's hat in a ſhower of rain? To which divers other queſtions might be added, as about the ſtars, ſo about the interſtellar part of heaven, which ſeveral of the modern Epicureans would have to be empty, ſave where the beams of light (and perhaps ſome other celeſtial effluvia) paſs through it; and the Carteſians, on the contrary, think to be full of an æthereal matter, which ſome, that are otherwiſe favourers of their philoſophy, confeſs they are reduced to take up but as an hypotheſis. So that our knowledge is much ſhort of what many think, not only if it be conſidered intenſively, but extenſively, (as a ſchool-man would expreſs it.) For there being ſo great a diſproportion between the heavens and the earth, that ſome moderns think the earth to be little better than a point in compariſon even of the orb of the ſun; and the Carteſians, with other Copernicans, think the great orb itſelf, (which is equal to what the Ptolomeans called the ſun's orb) to be but a point in reſpect of the firmament; and all our aſtronomers agree, that, at leaſt, the earth is but a phyſical point in compariſon of the ſtarry heaven: of how little extent muſt our knowledge be, which leaves us ignorant of ſo many things, touching the vaſt bodies, that are above us, and penetrates ſo little a way even into the earth, that is beneath us, that it ſeems confined to but a ſmall ſhare of the ſuperficial part of a phyſical point! of which conſideration the natural reſult will be, that, though what we call our knowledge, may be allowed to paſs for a high gratification to our minds, it ought not to puff them up; and what we know of the ſyſtem, and the nature of things corporeal, is not ſo perfect and ſatisfactory, as to juſtify our deſpiſing the diſcoveries of ſpiritual things.

ONE of the former parts of this letter may furniſh me with one thing more, to evince the excellencies and prerogatives of the knowledge of the myſteries of religion; and that one thing is ſuch, that I hope I ſhall need to add nothing more, becauſe it is not poſſible to add any thing higher; and that is, that the pre-eminence above other knowledge adjudged to that of divine truths by a judge above all exception, and above all compariſon, namely, by God himſelf.

THIS having been but lately ſhown, I ſhall not now repeat it, but rather apply what hath been there evinced, by repreſenting, that if he, who determines in favour of divine truths, were ſuch an one, as was leſs acquainted, than our over-weening naturaliſts, with the ſecrets of their idolized phyſicks; or if he were, though an intelligent, yet (like an angel) a bare contemplator of what we call the works of nature, without having any intereſt in their productions, your friend's not acquieſcing in his eſtimate of things might have, though not a fair excuſe, yet a ſtronger temptation.

BUT when he, by whoſe direction we prefer the higher truths revealed in the ſcripture, before thoſe, which reaſon alone teaches us, concerning thoſe comparatively mean ſubjects, things corporeal, is the ſame God, that not only underſtands the whole univerſe, and all its parts, far more perfectly, than a watch-maker can underſtand one of his own watches, (in which he can give an account only of the contrivance, and not of the cauſe of the ſpring, nor the nature of the gold, ſteel, and other bodies his watch conſiſts of,) but did make both this great Automaton, the world, and man in it: we have no colour to imagine, that he ſhould either be ignorant of, or injuriouſly diſparage his own workmanſhip, or impoſe upon his favourite creature, man, in directing him what ſort of knowledge he ought moſt to covet and prize. So that ſince it is he, who framed the world, and all thoſe things in it we moſt admire, that would have us prefer the knowledge he has vouchſafed us in his word, before that, which he has allowed us of his works; ſure it is very unreaſonable and unkind, to make the excellencies of the workmanſhip a diſparagement to the author, and the effects of his wiſdom a motive againſt acquieſcing in the deciſions of his judgment; as if, becauſe he is to be admired for his viſible productions, he were not to be believed, when he tells us, that there are diſcoveries, that contain truths more valuable than thoſe, which relate but to the objects, that he has expoſed to all men's eyes.

SECTION

SECTION V.

I DOUBT, I ſhould be guilty of a moſt important omiſſion, if I ſhould here forget to conſider one thing, which I fear has a main ſtroke in the partiality your friend expreſſeth in his preference of phyſicks to theology; and that is, that he ſuppoſes he ſhall, by the former, acquire a fame, both more certain and more durable, than can be hoped for from the latter.

AND I acknowledge, not only with readineſs, but with ſomewhat of gratulation of the felicity of this age, that there is ſcarce any ſort of knowledge more in requeſt, than that which natural philoſophy pretends to teach; and that among the awakened and inquiſitive part of mankind, as much reputation and eſteem may be gained by an inſight into the ſecrets of nature, as by being entruſted with thoſe of princes, or dignified with the ſplendideſt marks of their favour.

BUT though I readily confeſs thus much, and though perhaps I may be thought to have had, I know not by what fate, as great a ſhare of that perfumed ſmoak, applauſe, as (at leaſt) ſome of thoſe, which among the writers, that are now alive, your friend ſeems moſt to envy for it; yet I ſhall not ſcruple to tell you, partly from obſervation of what has happened to others, and partly too upon ſome little experience of my own, that neither is it ſo eaſy, as your friend ſeems to believe it, to get by the ſtudy of nature a ſure and laſting reputation, neither ought the expectation of it, in reaſon, make men undervalue the ſtudy of divinity. Nor would it here avail to object (by way of prevention) that the difficulties and impediments of acquiring and ſecuring reputation lie as well in the way of divines as philoſophers, ſince this objection has been already conſidered at the beginning of this ſecond part of our preſent tract. Beſides that the progreſs of our diſcourſe will ſhew, that the naturaliſt, aſpiring to fame, is liable to ſome inconveniences, which are either not at all, or not near equally incident to the divine. Wherefore, without ſtaying to take any further notice of this preventive allegation, I ſhall proceed to make good the firſt part of the aſſertion, that preceded it; which that I may the more fully do, give me leave (after having premiſed, that a man muſt either be a writer, or forbear to print what he knows;) to propoſe to you the following conſiderations.

AND firſt, if your *Phyſeophilus* ſhould think to ſecure a great reputation, by forbearing to couch any of his thoughts or experiments in writing, he may thereby find himſelf not a little miſtaken. For if once he have gained a repute (upon what account ſoever) of knowing ſome things, that may be uſeful to others, or of which ſtudious men are wont to be very deſirous, he will not avoid the viſits and queſtions of the curious. Or, if he ſhould affect a ſolitude, and be content to hide himſelf, that he may hide the things he knows; yet he will not eſcape the ſolicitations, that will be made him by letters. And if theſe ways of tempting him to diſcloſe himſelf, prevail not at all with him to do ſo, he will provoke the perſons, that have employed them; who finding themſelves diſobliged, by being defeated of their deſires, if not alſo their expectations, will for the moſt part endeavour to revenge themſelves on him, by giving him the character of an uncourteous and ill-natured perſon; and will endeavour, perhaps, ſucceſsfully enough, to decry his parts, by ſuggeſting, that his affected concealments proceed but from a conſcientiouſneſs, that the things he is preſumed to poſſeſs, are but ſuch, as, if they ſhould begin to be known, would ceaſe to be valued.

YOU will ſay (perchance,) that ſo much reſervedneſs is a fault: nor ſhall I diſpute it with you, whether it be or not; but if he be open and communicative in diſcourſe to thoſe

those strangers, that come to pump him, such is the disingenuous temper of too many, that he will be in great danger of having his notions or experiments arrogated by those, to whom he imparts them, or at least, by others, to whom those may (though perchance designlesly) happen to discourse of them. And then, if either *Physeophilus*, or any of his friends, that know him to be author of what is thus usurped, should mention him as such, the usurpers and their friends would presently become his enemies; and, to secure their own reputation, will be solicitous to lessen and blemish his. And if you should now tell me, that your friend might here take a middle way, as that, which in most cases is thought to be the best, by discoursing at such a rate of his discoveries, as may somewhat gratify those, that have a curiosity to learn them, and yet not speak so clearly, as to divest himself of his propriety in them; I should reply, that neither is this expedient a sure one, nor free from inconveniencies. For most men are so self-opiniated, that they will easily believe themselves masters of things, if they do but half understand them. And however, though the persons, to whom the discourse was immediately made, should not have too great an opinion of themselves, no more than too great a sagacity; yet they may easily, by repeating what they heard and observed, give some more piercing wit a hint sufficient to enable him to make out the whole notion, or the discovery, which he will then without scruple, and without almost any possibility of being disproved, assume for his own. But if it happen, (as it often will in extemporaneous discourse) that a philosopher be not rightly understood, either because he has not the leisure, no more than a design, to explain himself fully, or because the persons he converses with, bring not a competent capacity and attention; he then runs a greater danger than before. For the vanity most men take in being known to have conversed with eminent philosophers, makes them very forward to repeat what they heard such a famous wit say; and oftentimes being secure of not being contradicted, ignorantly to mis-recite it, or wittingly to wrest it in favour of the opinion they would countenance by it. So that, whereas by the formerly mentioned frankness of discourse, he is only in danger to have the truths he discovered arrogated by others, this reservedness exposes him to have opinions and errors, that he never dreamed of, fathered on him. And when a man's opinions, or discoveries, come once to be publickly discoursed of, without being proposed by himself, or some friend well instructed by him, he knows not what errors or extravagancies may be imputed to him (and that without a moral possibility left to most men to discern them,) by the mistake of the weak, or the disingenuity of the partial, or the artifices of the malicious. And even the greatness of a man's reputation does sometimes give such countenance to vain reports and surmises, as by degrees to shake, if not ruin it. As we see, that Frier *Bacon*, and *Trithemius*, and *Paracelsus*, who, for their times, were knowing, as well as famous men, had such feats ascribed to them, as by appearing fabulous to most of the judicious, have tempted many to think, that all the great things, that were said of them, were so too.

THESE are some of the inconveniencies, that a naturalist may be liable to, if he forbear the communicating of his thoughts and discoveries himself: but if *Physeophilus* should, to shun these, aspire to fame by the usual way of writing books, he may indeed avoid these, but perhaps, not without running into other inconveniencies and hazards, very little inferior to them.

FIRST then, we may consider, that whether a man writes in a systematical way, as they have done, who have published entire bodies of natural philosophy, or methodical treatises of some considerable part of it; or whether he write in a more loose and unconfined way, of any particular subject, that belongs to physicks; whichsoever, I

say,

ſay, of theſe two ways of writing books he ſhall make choice of, he will find it liable to inconvenience enough.

For if he write ſyſtematically, firſt, he will be obliged, that he may leave nothing neceſſary undelivered, to ſay divers things, that have been ſaid (perhaps many times) by others already, which cannot but be unpleaſant, not only to the reader, but (if he be ingenious) to the writer. Next, there are ſo many things in nature, whereof we know little or nothing, and ſo many more, of which we do not know enough, that our ſyſtematical writer, though we ſhould grant him to be very learned, muſt needs, either leave divers things, that belong to his theme, untreated of, or diſcourſe of them ſlightly, and oftentimes (in likelihood) erroneouſly. So that in this kind of books there is always much ſaid, that the reader did know, and commonly not a little, that the writer does not know. And to this, I muſt add, in the third place, that natural philoſophy, being ſo vaſt and pregnant a ſubject, that (eſpecially in ſo inquiſitive an age as this) almoſt every day diſcovers ſome new thing or other about it, it is ſcarce poſſible for a method, that is adapted but to what is already known, to continue long the moſt proper; as the ſame clothes will not long fit a child, whoſe age will make him quickly out-grow them. And therefore ſucceeding writers will have a fair pretence to compile new ſyſtems, that may be more adequate to philoſophy, improved ſince the publication of the former. And though there were little of new to be added, and it were more eaſy to alter, than to mend the method of our ſuppoſed author; yet novelty itſelf is a thing ſo pleaſing and inviting to the generality of men, that it often recommends things, that have nothing elſe to recommend them; and we may apply to a great many other things, what, I remember, a famous courtier of my acquaintance uſed to ſay of miſtreſſes, that another was preferable to a better, (the better being but the ſame.)

But now, if, declining the ſyſtematical way, one ſhould chooſe the other of writing looſe tracts and diſcourſes, he may indeed avoid ſome of the lately mentioned inconveniencies, but will ſcarce avoid the being plundered by ſyſtematical writers: for theſe will be apt to cull out thoſe things, that they like beſt, and inſert them in their methodical books, (perhaps much curtailed, or otherwiſe injured in the repeating,) and will place them, not as their own author did, where they may beſt confirm or adorn his diſcourſe, and be illuſtrated or upheld by it; but where it may beſt ſerve the turn of the compiler: and theſe methodical books promiſe ſo much more compendious a way, than others, to the attainment of the ſciences they treat of, that though really for the moſt part they prove greater helps to the memory, than the underſtanding; yet moſt readers being, for want of judgment, or of patience, of another mind, they are willing to take it for granted, that in former writers, if there have been any thing conſiderable, it has been all carefully extracted, as well as orderly digeſted by the later compilers: and though I take this to be a very erroneous and prejudicial conceit, yet it obtains ſo much, that as goldſmiths, that only give ſhape and luſtre to gold, are far more eſteemed, and in a better condition, than miners, who find the ore in the bowels of the earth, and with great pains and induſtry dig it up, and refine it into metal; ſo thoſe, that with great ſtudy and toil, ſucceſsfully penetrate into the hidden receſſes of nature, and diſcover latent truths, are uſually leſs regarded, or taken notice of, by the generality of men, than thoſe, who by plauſible methods, and a neat ſtile, reduce the truths, that others have found out, into ſyſtems of a taking order, and a convenient bulk.

I conſider in the ſecond place, that as the method of the books one writes, ſo the bulk of them may prove prejudicial to the naturaliſt, that aſpires to fame: for if he write large books, it is odds but that he will write in them many things unaccurate, if

not

not impertinent, or that he will be obliged to repeat many things, that others have faid before; and if he write but fmall tracts, as is the cuftom of the judicioufeft authors, who have no mind to publifh but what is new and confiderable, as their excellency will make them to be the fooner difperfed, fo the fmallnefs of the bulk will endanger them to be quickly loft, as experience fhews us of divers excellent little tracts, which, though publifhed not many years ago, are already out of print, (as they fpeak) and not to be met with, fave by chance, in ftationers fhops. So that thefe writings (which deferve a better fate) come, after a while, either to be loft (which is the cafe of divers,) or to have their memory preferved only in the larger volume of fome compiler, whofe induftry is only preferable to his judgment; it being obfervable, that (by I know not what unlucky fate) very few (for I do not fay none) that addict themfelves to make collections out of others, have the judgment to cull out the choiceft things in them; and the fmall tracts, we are fpeaking of, being preferved but in fuch a quoter or abridger, will run a very great danger of being conveyed to pofterity but under fuch a reprefentation, as it pleafes the compiler.

AND this (that I may proceed to my third confideration) may make the naturalift's fame very uncertain, not only becaufe of the want of judgment, that (as I newly faid) is too often obfervable in compilers, whereby they frequently leave far better things than they take, but for the want of fkill to underftand the author they cite and epitomize, or candor to do him right, For fometimes men's phyfical opinions, and feveral paffages of their writings, are fo mifreprefented by miftake or defign, efpecially if thofe, that recite their opinions be not of them, that men are made to teach or deliver things quite differing from their fenfe, and perhaps quite contrary to it; of which I myfelf have had fome unwelcome experience, a learned writer pretending, I know not how often, that I afferted an opinion, about which I did exprefsly ἐπέχειν. And another noted writer having (not out of defign, but unacquaintednefs with mechanicks, and the fubject I writ of,) given me commendations for having, by a new experiment, proved a thing, the quite contrary whereof I intended thereby to evince, and am not alone miftaken, if I did not do it. Other naturalifts I have met with, whofe writings compilers, have traduced out of hatred to their perfons, or their religion; as if truth could in nothing be a friend to one, that is the traducer's enemy; or as if a man, that falls into an error in religion, could not light upon a good notion in philofophy, in fpite of all the truths we owe to *Ariftotle*, *Epicurus*, and the other heathen philofophers, Nay, fome there are, that will fet themfelves to decry a man's writings, not becaufe they are directly his enemies, but becaufe he is efteemed by theirs; as you may remember an inftance in a fervant of yours, who had divers things written againft him upon this very account. Nor is it only by the citations of profeffed adverfaries, or opponents, that a worthy writer's reputation may be prejudiced, fince it is not unfrequently fo by thofe, that mention him with an encomium, and feem difpofed to honour him. For I have obferved it to be the trick of certain writers, to name an author with much compliment, only for fome one or few of the leaft confiderable things they borrow of him; by which artifice they endeavour to conceal their being plagiaries of more and better; which yet is more excufable than the practice of fome, who proceed to that pitch of difingenuity, that they will rail at an author, to whom indeed they owe too much, that they may not be thought to be beholden to him.

BUT (4.) I muft add, that befides thefe dangers, that a naturalift's reputation with pofterity may run through the ignorance or preverfenefs of men, it is liable to divers other hazards, from the very nature both of men, of opinions and of things.

FOR, as men's geniuffes and inclinations are naturally various in reference to ftudies, one man paffionately affecting one fort of them, and another being fond of quite differing ones; so those inclinations are oftentimes variously and generally determined by external and accidental causes. As when some great monarch happens to be a great patron, or a despiser, and perhaps adversary of this or that kind of learning; and when some one man has gained much applause for this or that kind of study; imitation or emulation oftentimes makes many others addict themselves to it. Thus though *Rome*, under the consuls, was iconsiderable for learning, yet the reputation of *Cicero*, and favour of *Augustus*, brought learning into request there; where the small countenance it met with amongst most of the succeeding emperors, kept it far inferior to what it had been among the Greeks about *Alexander*'s age. And the age of the same *Augustus* was ennobled with store of poets, not only by the countenance, which he and *Mæcenas* afforded them, but probably also by the examples they gave to, and the emulation they excited in one another. And after the decay of the Roman empire, in the fourth century, natural philosophy and the mathematicks being very little valued, and less understood, by reason that men's studies were by the genius of those ages applied to other subjects, every hundred years scarce produced one improver, (not to say one eminent cultivator) either of mathematicks or of physicks: by which you may see, how little certainty there is, that, because a man is skilled in natural philosophy, and that science is now in request, his reputation shall be as great as now, when perhaps the science itself will be grown out of repute.

But besides the contingencies, that may happen to a naturalist's fame upon this account, that the science he cultivates is, as well as others, subject to wanes and eclipses in the general esteem of men; there is another uncertainty arising from the vicissitudes, that are to be met with in the estimates men make of differing hypotheses, sects, and ways of philosophizing about the same science, and particularly about natural philosophy. For during those learned times, when physicks first and most flourished among the Grecians, *Democritus*, *Leucippus*, *Epicurus*, *Anaxagoras*, *Plato*, and almost all the naturalists, that preceded *Aristotle*, were Corpuscularians, endeavouring, though not all by the same way, to give an account of the phænomena of nature, and even of qualities themselves, by the bigness, shape, motion, &c. of corpuscles, or the minutest active parts of matter: whereas *Aristotle*, having attempted to deduce the phænomena from the four first qualities, the four elements, and some few other barren hypotheses, ascribing what could not be explicated by them, (and consequently far the greatest part of nature's phænomena) to substantial forms and occult qualities; (principles, that are readily named, but scarce so much as pretended to be understood,) and having upon these slight and narrow principles reduced physicks into a kind of system, which the judicious modesty of the Corpuscularians had made them backward to do; the reputation, that his great pupil *Alexander*, as well as his learning gave him; the easiness of the way he proposed to the attainment of natural philosophy; the good luck his writings had to survive those of *Democritus*, and almost all the rest of the Corpuscularians, when *Charles* the Great began to establish learning in *Europe*: these, I say, and some other lucky accidents, that concurred, did for about seven or eight hundred years together, make the Corpuscularian philosophy not only be justled, but even exploded out of the schools by the Peripatetick; which in our times is, by very many, upon the revival of the Corpuscularian philosophy, rejected, and, by more than a few derided as precarious, unintelligible, and useless. And to give an instance in a particular thing, (which, though formerly named, deserves to be again mentioned to our present purpose,) *Aristotle*, himself somewhere confesses, (not to say brags) that the Greek philosophers, his predecessors, did unanimously teach, that the world was (I say not created, but) made, and yet he, almost by his single authority, and the subtile arguments (as some have been pleased to think them,) that he employed, (though divers of them were borrowed of *Ocellus Lucanus*,) was able for many ages to introduce into the ſchools of philoſophers that irreligious and ill-grounded opinion of the eternity of the world, which afterwards the Chriſtian doctrine made men begin to queſtion, and which now, both that and right reaſon have perſuaded moſt men to reject.

AND this invites me to conſider farther, than the preſent ſucceſs of the opinions, that your *Phyſeophilus* befriends, ought not to make him ſo ſure, as he thinks he is, that the ſame opinions will be always in the ſame, or a greater vogue, and have the ſame advantages, in point of general eſteem, that they now have, over their corrivals. For, opinions ſeem to have their fatal ſeaſons and viciſſitudes, as well as other things; as may appear, not only by the examples of it newly given, but alſo by the hypotheſis of the earth's motion, which having been in great requeſt before *Pythogaras*, (who yet is commonly thought the inventor of it,) had its reputation much encreaſed by the ſuffrage of the famous ſect of the Pythagoreans, (whom *Ariſtotle* himſelf takes notice of as the patrons of that opinion;) and yet afterwards for near 2000 years it was laughed at, as not only falſe, but ridiculous. After all which time, this ſo long antiquated opinion being revived by *Copernicus*, has in a little time made ſo great a progreſs among the modern aſtronomers and philoſophers, that if it go on to prevail at the ſame rate, the motion of the earth will be acknowledged by all its mathematical inhabitants. But though it be often the fate of an oppreſſed truth, to have at length a reſurrection, yet it is not always its peculiar privilege; for obſolete errors are ſometimes revived, as well as diſcredited truths: ſo that the general diſrepute of an opinion in one age will not give us an abſolute ſecurity, that it will not be in as general requeſt in another, in which it may perhaps, not only revive, but reign.

NOR is it only in the credit of men's opinions about philoſophical matters, that we may obſerve an inconſtancy and viciſſitude, but in the very way and method of philoſophizing, for *Democritus, Plato, Pythagoras*, and others, who were of the more ſincere and ingenious cultivators of phyſicks among the Greeks, exerciſed themſelves chiefly either in making particular experiments and obſervations, as *Democritus* did in his manifold diſſections of animals; or elſe applied the mathematicks to the explicating of a particular phænomenon of nature, as may appear (not to mention what *Hero* teaches in his Pneumaticks,) by the accounts, *Democritus*, *Plato*, and others, give of fire and other elements, from the figure and motion of the corpuſcles they conſiſt of. And although this way of philoſophizing were ſo much in requeſt before *Ariſtotle*, that (albeit he unluckily brought in another, yet) there are manifeſt and conſiderable footſteps of it to be met with in ſome of his writings, (and particularly in his books of animals, and his mechanical queſtions;) yet the ſcholaſtick followers of *Ariſtotle* did, for many ages, neglect the way of philoſophizing of the antients, and (to the great prejudice of learning) introduced every where, inſtead of it, a quite contrary way of writing. For, not only they laid aſide the mathematicks, (of which they were for the moſt part very ignorant,) but inſtead of giving us intelligible and explicite (if not accurate) accounts of particular ſubjects, grounded upon a diſtinct and heedful conſideration of them, they contented themſelves with hotly diſputing, in general, certain unneceſſary, or at leaſt unimportant queſtions about the objects of phyſicks, about *Materia Prima*, ſubſtantial forms, privation, place, generation, corruption, and other ſuch general things, with which when they had quite tired themſelves and their readers, they uſually remained utter ſtrangers to the particular productions of that nature, about which they had ſo much wrangled, and were not able to give a man ſo much true and uſeful information about particular bodies, as even the meaneſt mechanicks, ſuch as mine-diggers, butchers, ſmiths, and even dairy-maids, could do. Which made their philoſophy

philosophy appear so imperfect and useless, not only to the generality of men, but to the more elevated and philosophical wits, that our great *Verulam* attempted with much skill and industry, (and not without some indignation) to restore the more modest and useful way practised by the antients, of enquiring into particular bodies, without hastening to make systems, into the request it formerly had; wherein the admirable industry of two of our *London* physicians, *Gilbert* and *Harvey*, has not a little assisted him. And I need not tell you, that since him, *Des Cartes*, *Gassendus*, and others, having taken in the application of geometrical theorems, for the explication of physical problems; he, and they, and other restorers of natural philosophy, have brought the experimental and mathematical way of enquiring into nature, into at least as high and growing an esteem, as ever it possessed when it was most in vogue among the naturalists, that preceded *Aristotle*.

To the considerations I have hitherto deduced, which (perhaps) might alone suffice for my purpose, I shall yet subjoin one, that I take to be of greater weight than any of them, for the manifesting, how difficult it is to be sure, that the physical opinions, which at present procure a champion or promoter of them veneration, shall be still in request. For besides that inconstant fate of applauded opinions, which may be imputed to the inconstancy of men, there is a greater danger, that threatens the aspirer's reputation from the very nature of things: for the most general principles of all, viz. the figure, bigness, motion, and other mechanical affections of the small parts of matter, being (as your friend believes) sufficiently and clearly established already; he must expect to raise his reputation from subordinate hypotheses and theories; and in these I shall not scruple to say, that it is extremely difficult, even for those, that are more exercised than he in framing them, and in making of experiments to have so reaching and attentive a prospect of all things fit to be known, as not to be liable to have their doctrine made doubtful, or disproved by something, that he did not discover, or that after-times may. This, I doubt not, but you would easily be prevailed with to allow, if I had leisure and conveniency to transmit to you my sceptical naturalist. And without having recourse to that tract, it may possibly suffice, that we consider, that one of the conditions of a good * hypothesis is, that it fairly comport not only with all other truths, but with all other phænomena of nature, as well as those it is framed to explicate. For this being granted, (which cannot be denied,) he, that establishes a theory, which he expects shall be acquiesced in by all succeeding times, and make him famous in them, must not only have a care, that none of the phænomena of nature, that are already taken notice of, do contradict his hypothesis at the present, but that no phænomena, that may be hereafter discovered, shall do it for the future. And I very much question, whether *Physeophilus* do know, or, upon no greater a number and variety of experiments than most men build upon, can know, how incompleat the history of nature we yet have, is, and how difficult it is to build an accurate hypothesis upon an incompleat history of the phænomena it is to be fitted to; especially considering, that (as I was saying) many things may be discovered in after-times by industry or chance, which are not now so much as dreamed of, and which may yet overthrow doctrines speciously enough accommodated to the observations, that have been hitherto made.

THOSE antient philosophers, that thought the torrid zone to be uninhabitable, did not establish their opinion upon wild reasonings; and as it continued uncontrouled for many ages, so perhaps it would have always done, if the discoveries made by modern navigations had not manifested it to be erroneous. The solidity of the celestial orbs was, for divers centuries above 1000 years, the general opinion of astronomers and philosophers; and yet in the last age, and in ours, the free trajection, that has been

* See the requisites of a good hypothesis.

obſerved in the motion of ſome comets, from one of the ſuppoſed orbs to another, and the intricate motions in the planet *Mars*, (obſerved by *Kepler* and others, to be ſometimes nearer, as well as ſometimes remoter from the earth than is the ſun;) theſe, I ſay, and other phænomena undiſcovered by the antients, have made even *Tycho*, as well as moſt of the recent aſtronomers, exchange the too long received opinion of ſolid orbs for the more warrantable belief of a fluid æther. And though the celeſtial part of the world, by reaſon of its remoteneſs from us, be the moſt unlikely of any other to afford us the means of over-throwing old theories by new diſcoveries; yet even in that we may take notice of divers inſtances to our preſent purpoſe, though I ſhall here name but this one, viz. That, after the Ptolemaick number and order of the planets had paſt uncontradicted for very many ages; and even the Tychonians and Copernicans, (however they did, by their differing hypotheſes, diſſent from the Ptolemaick ſyſtem (as to the order) did (yet) acquieſce in it as to the number of the planets; by the happy diſcoveries, made by *Galilæo* of the Satellites of *Jupiter*, and by the excellent *Hugenius*, of the new planet about *Saturn*, (which I think I had the luck to be the firſt, that obſerved and ſhewed diſbelievers of it in *England)* the aſtronomers of all perſuaſions are brought to add to the old ſeptenary number of the planets, and take in five others, that their predeceſſors did not dream of. That the chyle prepared in the ſtomach paſſed through the meſaraick veins to the liver, and ſo to the heart, was for many ages the unanimous opinion, not only of phyſicians, but anatomiſts, whoſe numerous diſſections did not tempt them to queſtion it; and yet, ſince the caſual, though lucky, diſcoveries made of the milky veſſels in the thorax by the dextrous *Pecquet*, thoſe, that have had with you and me the curioſity to make the requiſite experiments, are generally convinced, that, at leaſt, a good part of the chyle goes from the ſtomach to the heart, without paſſing through the meſaraick veins, or coming at all to the liver.

It were eaſy to multiply inſtances of this kind, but I rather chuſe to add, that it is not only about the qualities, and other attributes of things, but about their cauſes alſo, that new, and oftentimes accidental diſcoveries may deſtroy the credit of long and generally approved opinions. That quick-lime exceedingly heats the water, that is poured on to quench it, on the account of *Antiperiſtaſis*, has been very long and univerſally received by the ſchool-philoſophers, where it is the grand and uſual argument, urged to eſtabliſh *Antiperiſtaſis* *; and yet I preſume you have taken notice, that this proof is made wholly ineffectual in the judgment of many of the virtuoſi, by ſome contrary experiments of mine, and particularly that of exciting in quick-lime full as great an effervescence by the affuſion of hot water inſtead of cold. So it has been generally believed, that in the congelation of water, that liquor is condenſed into a narrower room; whereas our late experiments † have ſatisfied moſt of the curious, that ice is water expanded, or, if you pleaſe, that ice takes up more room than the water did, whilſt it remained unfrozen. And whereas the notion of nature's abhorrence of a vacuum has not only, ever ſince *Ariſtotle*'s time, made a great noiſe in the ſchools, but ſeems to be confirmable by a multitude of phænomena; the experiments of *Torricellius* and ſome of ‡ ours, evidencing, that the air has a great weight and a ſtrong ſpring, have, I think, perſuaded almoſt all, that have impartially conſidered them, that, whether there be or be not ſuch a thing, as they call *fuga vacui*, yet ſuction, and the aſcenſion of water in pumps, and thoſe other phænomena, that are generally aſcribed to it, may be very well explicated without it, and are, indeed, cauſed by the weight of the atmoſphere, and the elaſtical power of the air.

* See this ſubject handled at large in an appendix to the author's *Examen* of *Antiperiſtaſis*.
† In the hiſtory of cold.
‡ Now publiſhed in the book of new phyſico-mechanical experiments.

AND

AND this puts me in mind to take notice, that even practical inventions, where one would think the matter of fact to be evident, may, by undreamed-of discoveries be brought to lose the general reputation they had for compleatness in their kind. For to endear the invention of sucking pumps, and of syphons, it has been generally presumed, that by means of either of these, water and any other liquor may, *ob fugam vacui*, be raised to what height one pleases; and accordingly ways have been proposed by famous authors, to convey water from one side of an high mountain to the other: whereas, first, the unexpected disappointments, that were met with by some pump-makers, and afterwards experiments purposely made, sufficiently evince, that neither a pump nor a siphon will raise water to above 35 foot, or thereabouts, nor quick-silver to so many inches.

AND as to the invention of weather-glasses, which has been so much, and justly applauded and used, as it has been generally received for the truest standard of the heat and cold of the weather; so it seems to be liable to no suspicion of deceiving us: for not only it is evident, that in winter, when the air is very cold, the water rises much higher than in summer, and other seasons, when it is not so; but if you but apply your warm hand to the bubble at the top, the water will be visibly depressed by the rarified air, which upon the removal of the hand returning to its former coldness, the water will forthwith as manifestly ascend again. * And yet by finding, that, as the atmosphere has a considerable weight, so this weight is not always the same, but varies much, and that, as far as I can yet discover, uncertainly enough; I have had the luck to satisfy many of the curious, that these open thermometers are not to be safely relied on, since in them the liquor is made to rise and fall, not only, as men have hitherto supposed, by the cold and heat of the ambient air, but (as I have shewn by divers new experiments) according to the varying gravity of the atmosphere; which variation has not only a sensible, but a very considerable influence upon the weather-glass. To these instances I shall annex only one more, from which we may learn, that notwithstanding a very heedful survey of all, that at present a man can take notice of, or well suspect, that he ought to take into his consideration, the case may be such, that having devised an instrument, he may use it many years with good success; and yet, unless he were able to live very many more, he shall not be sure to outlive the danger of finding the same instrument (though the sense as well conditioned as ever) fallacious: as he, that first applied a magnetick needle to the finding of the meridian line, might very probably conclude, that his needle pointing directly N. and S. or declining from it just two or three, or some other determinate number of degrees, he had discovered a certain and ready way, without the help of sun or stars, or astronomical instruments, to describe a meridian line, and if he lived but an ordinary number of years after his observation, he might probably have found his instrument not deceitful; which yet it may now be, the magnetick needle, not only declining in many places from the true points of N. and S. but (as later discoveries inform us) varying in tract of time its declination in the self same place.

THE considerations hither to proposed might easily enough be increased by more of the same tendency, especially if I thought fit to borrow from a discourse (of mine) purposely written about the partiality and uncertainty of fame; but instead of adding to their number, I should think myself obliged to excuse my having already mentioned so many, and insisted so much upon them, if I did not vehemently suspect, that in your *Physeophilus*, (as well as in many other modern naturalists) scarce any thing does more contributes to an undervaluation of the study of divinity, than, that being eagerly ambitious of a certain, as well as a posthume fame, he is confident, that physiology

* See a Tract on this Subject, premised by the Author to his Book of Cold.

fiology will help to it; and therefore, the defign of his difcourfe made me think it expedient to fpend fome time to manifeft, "that it is far lefs eafy than he thinks, to be "as fure, that he fhall have the praifes of future ages, as that (though he have them) "he fhall not hear them."

THE paft confiderations have, I prefume, convinced you, that it is no fuch eafy matter for a naturalift to acquire a great reputation, and be fure it will prove a lafting one. Wherefore, that I may alfo confirm the fecond part of what formerly I propofed, I now proceed to fhow, that, though the cafe were otherwife, yet he would have no reafon to flight the ftudy of divinity.

1. FOR, in the firft place, nothing hinders, but that a man, who values and enquires into the myfteries of religion, may attain to an eminent degree in the knowledge of thofe of nature. For frequently men of great parts may fuccefsfully apply themfelves to more than one ftudy; and few of them have their thoughts and hours fo much engroffed by that one fubject or employment, but that if they have great inclinations, as well as fitnefs for the ftudy of nature, they will find time, not only to cultivate it, but to excel in it, You need not be told, that *Copernicus*, to whom our late philofophers owe fo much, was a churchman; that his champion *Lansbergius* was a minifter, and that *Gaffendus* himfelf was a doctor of divinity. Among the Jefuits you know, that *Clavius*, and divers others, have as profperoufly addicted themfelves to mathematicks as divinity. And as to phyficks, not only *Scheiner*, *Aquilonius*, *Kircher*, *Schottus*, *Zucchius*, and others, have very laudably cultivated the optical and fome other parts of philofophy; but *Ricciolus* himfelf, the learned compiler of that voluminous and judicious work of the *Almageftum novum*, wherein he has inferted divers accurate obfervations of his own, is not only a divine, but a profeffor of divinity. And without going out of our own country, I could, if I durft for fear of offending the modefty of thofe I fhould name, or injuring the merit of thofe I fhould omit; I could, I fay, if it were not for this, among our Englifh ecclefiafticks name you divers, who though they apply themfelves fo much to the ftudy of the fcripture, as to be not only folid divines, but excellent preachers, have yet been fo happily converfant with nature, that, if they had lived in the learned times of the Greeks, they would have rivalled, if not eclipfed, fome of them, *Pythagoras* and *Euclid*; others of them, *Anaxagoras* and *Epicurus*; and fome of them, even *Archimedes* and *Democritus* themfelves.

AND certainly, provided there be curiofity and induftry enough employed in the ftudy of nature, it is not neceffary, that the knowledge of nature fhould be the ultimate end of that ftudy; a fondnefs of the object being required only in order to the engaging the mind to fuch a ferious application, as a higher aim may fufficiently invite us to; and will rather promote than difcourage. *David* became no lefs fkilful in mufick, than thofe, that were addicted to it only to pleafe themfelves in it; though we may reafonably fuppofe, that fo pious an author of pfalms and inftruments afpired to an excellency in that delightful fcience, that he might apply and prefer it to the fervice of the temple, and promote the celebration of God's praifes with it. And as experience has manifefted, that the heathen philofophers, that courted moral virtue for herfelf, did not raife it to that pitch, to which it was advanced by the heroick practices of thofe true Chriftians, that in the higheft exercife of virtue had a religious aim at the pleafing and enjoying of God; fo I fee not, why natural knowledge muft be more profperoufly cultivated by thofe felfifh naturalifts, that aim but at the pleafing of themfelves in the attainment of that knowledge, than thofe religious naturalifts, who are invited to attention and induftry, not only by the pleafantnefs of the knowledge itfelf, but by a higher and more engaging confideration; namely, that by the difcoveries they

Amos vi. 5.

hey make in the book of nature, both themſelves and others may be excited and qualified the better to admire and praiſe the author, whoſe goodneſs does ſo well match the wiſdom they celebrate, that he declares in his word, that "thoſe, that honour "him, he will honour." 1 Sam. ii. 30.

AND as a man, that is not in love with a fair lady, but has only a reſpect for her, may have as true and perfect, though not as diſcompoſing an idea of her face, as the moſt paſſionate Inamorato; ſo I ſee not, why a religious and inquiſitive contemplator of nature may not be liable to give a good account of her, without preferring her ſo far to all other objects of his ſtudy, as to make her his miſtreſs, and perhaps too his idol.

II. AND now I proceed to conſider in the ſecond place, that matters of divinity may, as well as thoſe of philoſophy, afford a reputation to him, that diſcovers, or illuſtrates them. For though the fundamental articles of Chriſtian religion be, as I have formerly declared, little leſs evident than important; yet there are many other points in divinity, and paſſages in the ſcripture, which (for reaſons, that I have elſewhere mentioned) are exceeding hard to be cleared, and do not only poſe ordinary readers, and the common ſort of ſcholars, but will ſufficiently exerciſe the abilities of a great wit, and give him opportunity enough to manifeſt, that he is one. For divers of the points I ſpeak of, are much benighted upon the ſcore of the ſublimity of the things they treat of; ſuch as are the nature, attributes, and decrees of God, which cannot be eaſy to the dim underſtandings of us, that are but men: and many other particulars, that are not abſtruſe in their own nature, are yet made obſcure to us by our ignorance, (or at leaſt imperfect knowledge,) of the diſuſed languages, wherein they are delivered, and the great remoteneſs of the ages when, and the countries where, the things recorded were done or ſaid. So that oftentimes a man may need and ſhow as great learning and judgment to diſpel the darkneſs, wherein time has involved things, as that, which nature has caſt on them: and in effect we ſee, that St. *Auguſtin*, St. *Hierom*, *Origen*, and others of the fathers, have acquired no leſs a reputation, than *Empedocles*, *Anaxagoras*, or *Zeno*; and *Grotius*, *Salmaſius*, Mr. *Mede*, Dr. *Hamond*, and ſome other critical expounders of difficult texts of ſcripture, have thereby got as much credit, as *Fracaſtorius* by his book *De Sympathia & Antipathia*; *Levinus Lemnius* by his *De occultis rerum Miraculis*; or *Cardanus* (and his adverſary *Scaliger)* by what they writ *De Subtilitate*; or even *Fernelius* himſelf by his book *De Abditis rerum Cauſis*. And it will contribute to the credit, which theological diſcoveries and illuſtrations may procure a man, that the importance of the ſubjects, and the earneſtneſs, wherewith men are wont to buſy themſelves about them, ſome upon the ſcore of piety, and others upon that of intereſt, ſome to learn truths, and others to defend what they have long or publickly taught for truth, does make greater numbers of men take notice of ſuch matters, and concern themſelves far more about them, than about almoſt any other things, and eſpecially far more, than about matters purely philoſophical, which but few are wont to think themſelves fit to judge of, and concerned to trouble themſelves about. And accordingly we ſee, that the writings of *Socinus Calvin*, *Bellarmine*, *Padre Paulo*, *Arminius*, *&c.* are more famous, and more ſtudied, than thoſe of *Teleſius*, *Campanella*, *Severinus Danus*, *Magnenus*, and divers other innovators in natural philoſophy. And *Eraſtus*, though a very learned phyſician, is much leſs famons for all his elaborate diſputations againſt *Paracelſus*, than for the little tract againſt particular forms of church-government. And I preſume you have taken notice, as well as I, that there are ſcarce any five new controverſies in all phyſicks, that are known to, and hotly contended for by ſo many, as are the five articles of the Remonſtrants.

III. My ſecond conſideration being thus diſpatched, it remains, that I tell you in the third place, that ſuppoſing, but not granting, that to proſecute the ſtudy of divinity, one muſt of neceſſity neglect the acquiſt of reputation; yet this inconvenience itſelf ought not to deter us from the duty they would diſſuade. For in all deliberations, wherein any thing is propoſed to be quitted or declined, to obey or pleaſe God; methinks, we may fitly apply that of the prophet to the Jewiſh king, who being perſuaded (to expreſs his concern for God's glory) to decline the aſſiſtance of an idolatrous army of *Iſraelites*, and objecting, that by complying with the advice given him, he ſhould loſe a ſum of money, amounting to no leſs than the hire of a potent army; received
2 Chron. xxv. 9. from the prophet this briſk, but rational anſwer, "The Lord is able to give thee far "more than this." The apoſtle *Paul*, who had been traduced, reviled, buffetted, ſcourged, impriſoned, ſhip-wrecked, and ſtoned for his zeal to propagate the truths, whoſe ſtudy I plead for; after he had once had a glimpſe of that great recompence of reward, that is reſerved for us in heaven, ſcruples not to pronounce, that he finds upon caſting up the account (for he uſes the arithmetical λογίζομαι) "that the ſufferings of
Rom. viii. 18. "this preſent time are not worthy to be compared with the glory, that is to be revealed "in us." And if all, that the perſecuted Chriſtians of his time could ſuffer were
Luke xxiii. 15. not ſuitable (for ſo I remember the ſame Greek word to ſignify elſewhere) or proportionable to that glory; it will ſure far outweigh what we can now forego or decline for it; the loſs of an advantage, and much more the bare miſſing of it, being uſually but a negative affliction, in compariſon of the actual ſufferance of evil. Chriſt did not only tell his diſciples, that he, who ſhould give the leaſt of his followers ſo much as a cup of cold water upon the ſcore of their relation to him, ſhould not be unrewarded; but when the ſame perſons aſked him, what ſhould be done to them, who had left all to follow him, he preſently allots them thrones, as much over-valuing that all they had loſt, as an ordinary recompence may exceed a cup of cold water. And indeed God's goodneſs is ſo great, and his treaſure ſo unexhauſted, that as he is forward to recompence even the leaſt ſervices, that can be done him, ſo he is able to give the greateſt a proportionable reward, *Solomon* had an opportunity, ſuch as never any mortal had, (that we know of,) either before or ſince, of ſatisfying his deſires,
1 Kings iii. 5. whether of fame, or any other thing, that he could wiſh; "Aſk what I ſhall give "thee," was the proffer made him by him, that could give all things worth receiving; and yet the wiſdom even of *Solomon*'s choice, approved by God himſelf, conſiſted in declining the moſt ambitious things of this life for thoſe things, that might the better qualify him to ſerve and pleaſe God. And to give you an example in a greater than
Phil. ii. 6. *Solomon*, we may conſider, that he, "who being in the form of God, thought it not "robbery to be equal with God;" and who, by leaving heaven, did, to dwell on earth, quit more than any inhabitant of the earth can gain in heaven, and denied more to become capable of being tempted, than he did when he was tempted with an offer of all the kingdoms of the world, and the glory of them: this Saviour, I ſay, is
Heb. xii. 2. ſaid in ſcripture to have, "for the joy, that was ſet before him, endured the croſs, "and deſpiſed the ſhame;" as if heaven had been a ſufficient recompence for even his renouncing honours, and embracing torments.

HE, that declines the acquiſt of the applauſe of men for the contemplation of the truths of God, does but forbear to gather that, whilſt it is immature which, by waiting God's time, he will more ſeaſonably gather when it is full ripe, and wholeſome, and ſweet. That immarceſcible crown, as St. *Peter* calls it, which Goſpel promiſes
Rom. ii. 7. to them, "who, by patient continuance in well-doing, ſeek for glory and honour;" will make a rich amends for the declining of a fading wreath here upon earth, where reputation

reputation is oftentimes as undeservedly acquired, as lost; whereas in heaven, the very having celestial honours argues a title to them. And since it is our Saviour's reasoning, that his disciples ought to rejoice when their reputation is pursued by calumny, as well as their lives by persecution, "because their reward is great in "heaven," we may justly infer, that the grounded expectation of so illustrious a condition may bring us more content, even when it is not attended with a present applause, than this applause can give those, who want that comfortable expectation. So that, upon the whole matter, we have no reason to despond, or to complain of the study of theology, for but making us decline an empty and transitory fame for a solid and eternal glory. Matth. v. 11, 12.

The CONCLUSION.

BY this time, Sir, I have said as much as I think fit (and therefore, I hope, more than upon your single account was necessary) to manifest, that *Physeophilus* had no just cause to undervalue the study of divinity nor our friend the doctor, for addicting himself to it. I hope you have not forgotten what I expresly enough declared at the beginning of this letter, that both your friend and you admitting the holy scriptures, I know myself thereby to be warranted to draw proofs from their authority. And if I need not remind you of this, perhaps I need not tell you by way of apology, that I am not so unacquainted with the laws of discoursing, but that, if I had been to argue with Atheists or Scepticks, I should have forborn to make use of divers of the arguments I have employed, as fetched from unconceded topicks, and substituted others for such, as yet, I think, it very allowable for me to urge, when I deal with a person, that, as your friend, does only undervalue the study of the scriptures, not reject their authority. And if the prolixity I have been guilty of already, did forbid me to encrease it by apologies not absolutely necessary, I should perchance, rather think myself obliged to excuse the plainness of the stile of this discourse; which both upon the subject's score, and yours, may seem to challenge a richer dress. But the matter is very serious, and you are a philosopher, and when the things we treat of are highly important, I think truths clearly made out to be the most persuasive pieces of oratory. And a discourse of this nature is more likely to prove effectual on intelligent perusers, by having the reasons it presents perspicuously proposed, and unprejudicedly entertained, than by their being pathetically urged, or curiously adorned. And I have the rather forborn expressions, that might seem more proper to move, than to convince; because I foresee, I may very shortly have occasion to employ some of the former sort in another letter to a friend of yours and mine, who will, I doubt, make you a sharer in the trouble of reading it. But writing this for you, and *Physeophilus*, I was far more solicitous to give the arguments I employ a good than a bright gloss. For even when we would excite devotion, if it be in rational men, the most effectual pieces of oratory are those, which like burning-glasses inflame, by nothing but numerous and united beams of light. If this letter prove so happy as to give you any satisfaction, it will thereby bring me a great one. For prizing you as I do, I cannot but wish to see you esteem those things now, which I am confident we shall always have cause to esteem; and then most, when the light of glory shall have made us better judges of the true worth of things. And it would extremely trouble me to ſee you a diſeſteemer of thoſe divine things, which as long as a man undervalues, the poſſeſſion of heaven itſelf would not make him happy. And therefore, if the bleſſing of him, whoſe glory is aimed at in it, make the ſucceſs of this paper anſwerable to the wiſhes, the importance of the ſubject will make the ſervice done you by it, ſuitable to the deſires of,

SIR,

Your moſt faithful,

moſt affectionate,

and moſt humble ſervant.

ABOUT

ABOUT THE EXCELLENCY AND GROUNDS OF THE MECHANICAL HYPOTHESIS; SOME CONSIDERATIONS, Occaſionally propoſed to a FRIEND.

The Publiſher's Advertiſement.

THE following paper having been but occaſionally and haſtily penned, long after what the author had written (by way of dialogue) about the requiſites of a good hypotheſis, it was intended, that if it came forth at all, it ſhould do ſo as an appendix to that diſcourſe; becauſe, though one part of it does little more than name ſome of the heads treated of in the dialogue, yet, according to the exigency of the occaſion, the other part contains ſeveral things either pretermitted, or but more lightly touched on in this diſcourſe. But, although the author's deſign were to reſerve theſe thoughts, as a kind of paralipomena to his dialogue; yet, ſince he is not willing to let that, at leaſt quickly, come abroad, and theſe are fallen into my hands; I will make bold, with his good leave, to annex them to the foregoing treatiſe, not only to compleat the bulk of the book, but becauſe of ſome affinity between them, ſince both aim at manifeſting the excellency of the ſtudies they would recommend. And perhaps it will not be unwelcome to ſome of the curious to find, that our noble author in the ſame book, wherein he prefers the ſtudy of divine things to that of natural ones, does himſelf prefer the mechanical principles before all other hypotheſes about natural things; they being in their own nature ſo accommodate, to make conſidering men underſtand, rather than diſpute of, the effects of nature.

OF THE EXCELLENCY and GROUNDS OF THE CORPUSCULAR or MECHANICAL PHILOSOPHY.

THE importance of the queſtion, you propoſe, would oblige me to refer you to "the dialogue about a good hypotheſis," and ſome other papers of that kind, where you may find my thoughts about the advantages of the mechanical hypotheſis ſomewhat amply ſet down, and diſcourſed of. But, ſince your deſires confine me to deliver in few words, not what I believe reſolvedly, but what I think may be probably ſaid for the preference or the pre eminence of the corpuſcular philoſophy above *Ariſtotle*'s, or that of the chemiſts, you muſt be content to receive from me, without any preamble, or exact method, or ample diſcourſes, or any other thing, that may coſt many words, a ſuccinct mention of ſome of the chief advantages of the hypotheſis we incline to. And I the rather comply, on this occaſion, with your curioſity, becauſe I have often obſerved you to be alarmed and diſquieted, when you hear of any book, that pretends to uphold, or repair the decaying philoſophy of the ſchools, or ſome bold chymiſt, that arrogates to thoſe of his ſect the title of philoſophers, and pretends to build wholly upon experience, to which he would have all other naturaliſts thought ſtrangers. That therefore you may not be ſo tempted to deſpond, by the confidence or reputation of thoſe writers, that do ſome of them applaud, and others cenſure, what, I fear, they do not underſtand, (as when the Peripateticks cry up ſubſtantial forms, and the chemiſts, mechanical explications) of nature's phænomena, I will propoſe ſome conſiderations, that, I hope, will not only keep you kind to the philoſophy you have embraced, but perhaps, (by ſome conſiderations, which you have not yet met with,) make you think it probable, that the new attempts you hear of from time to time, will not overthrow the corpuſcularian philoſophy, but either be foiled by it, or found reconcilable to it.

BUT when I ſpeak of the corpuſcular or mechanical philoſophy, I am far from meaning with the Epicureans, that atoms, meeting together by chance in an infinite vacuum, are able of themſelves to produce the world, and all its phænomena; nor with ſome modern philoſophers, that, ſuppoſing God to have put into the whole maſs of matter ſuch an invariable quantity of motion, he needed do no more to make the world, the material parts being able by their own unguided motions, to caſt themſelves into ſuch a ſyſtem (as we call by that name:) but I plead only for ſuch a philoſophy, as reaches but to things purely corporeal, and diſtinguiſhing between the firſt original of things, and the ſubſequent courſe of nature, teaches, concerning the former, not only that God gave motion to matter, but that in the beginning he ſo guided the various motions of the parts of it, as to contrive them into the world he deſigned they ſhould compoſe, (furniſhed with the ſeminal principles and ſtructures, or models of living creatures,) and eſtabliſhed thoſe rules of motion, and that order amongſt things corporeal, which we are wont to call the laws of nature. And having told this as to the former, it may be allowed as to the latter to teach, that the univerſe being once framed by God, and the laws of motion being ſettled and all upheld by his inceſſant concourſe and general providence, the phænomena of the world thus conſtituted are phyſically produced by the mechanical affections of the parts of matter, and what they operate upon one another according to mechanical laws. And now having ſhewn what kind of corpuſcular philoſophy it is, that I ſpeak of, I proceed to the particulars, that I thought the moſt proper to recommend it.

I. THE firſt thing, that I ſhall mention to this purpoſe, is the intelligibleneſs or clearneſs of mechanical principles and explications. I need not tell you, that among the Peripateticks, the diſputes are many and intricate about matter, privation, ſubſtantial forms, and their eduction, &c. And the chemiſts are ſufficiently puzzled, (as I have elſewhere ſhewn,) to give ſuch definitions and accounts of their hypoſtatical principles, as are reconcileable to one another, and even to ſome obvious phænomena. And much more dark and intricate are their doctrines about the Archeus, Aſtral Beings, Gas, Blaſs, and other odd notions, which perhaps have in part occaſioned the darkneſs and ambiguity of their expreſſions, that could not be very clear, when their conceptions were far from being ſo. And if the principles of the Ariſtotelians and Spagyriſts are thus obſcure, it is not to be expected, the explications, that are made by the help only of ſuch principles ſhould be clear. And indeed many of them are either ſo general and ſlight, or otherwiſe ſo unſatisfactory, that granting their principles, it is very hard to underſtand or admit their applica ions of them to particular phænomena. And even in ſome of the more ingenious and ſubtle of the peripatetick diſcourſes upon their ſuperficial and narrow theories, methinks, the authors have better plaid the part of painters than philoſophers, and have only had the ſkill, like drawers of landſkips, to make men fancy they ſee caſtles and towns, and other ſtructures, that appear ſolid and magnificent, and to reach to a large extent, when the whole piece is ſuperficial, and made up of colours and art, and compriſed within a frame perhaps ſcarce a yard long. But to come now to the corpuſcular philoſophy, men do ſo eaſily underſtand one another's meaning, when they talk of local motion, reſt, bigneſs, ſhape, order, ſituation, and contexture of material ſubſtances; and theſe principles do afford ſuch clear accounts of thoſe things, that are rightly deduced from them only, that even thoſe Peripateticks or chymiſts, that maintain other principles, acquieſce in the explications made by theſe, when they can be had, and ſeek not any further, though perhaps the effect be ſo admirable, as would make it paſs for that of a hidden form, or occult quality. Thoſe very Ariſtotelians, that believe the celeſtial bodies to be moved by intelligences, have no recourſe to any peculiar agency of theirs to account for eclipſes. And we laugh at thoſe Eaſt-Indians, that to this day go out in multitudes, with ſome inſtruments, that may relieve the diſtreſſed luminary, whoſe loſs of light they fancy to proceed from ſome fainting fit, out of which it muſt be rouzed. For no intelligent man, whether chemiſt or Peripatetic, flies to his peculiar principles, after he is informed, that the moon is eclipſed by the interpoſition of the earth betwixt her and it, and the ſun by that of the moon betwixt him and the earth. And when we ſee the image of a man caſt into the air by a concave ſpherical looking-glaſs, though moſt men are amazed at it, and ſome ſuſpect it to be no leſs than an effect of witchcraft, yet he, that is ſkilled enough in catoptricks, will, without conſulting *Ariſtotle*, or *Paracelſus*, or flying to hypoſtatical principles and ſubſtantial forms, be ſatisfied, that the phænomenon is produced by the beams of light reflected, and thereby made convergent according to optical, and conſequently mathematical laws.

BUT I muſt not now repeat what I elſewhere ſay, to ſhew, that the corpuſcular principles have been declined by philoſophers of different ſects, not becauſe they think not our explications clear, if not much more ſo, than their own; but becauſe they imagine,

gine, that the applications of them can be made but to few things, and consequently are insufficient.

II. In the next place I observe, that there cannot be fewer principles than the two grand ones of mechanical philosophy, matter and motion. For, matter alone, unless it be moved, is altogether unactive; and whilst all the parts of the body continue in one state without any motion at all, that body will not exercise any action, nor suffer any alteration itself, though it may perhaps modify the action of other bodies, that move against it.

III. Nor can we conceive any principles more primary, than matter and motion. For, either both of them were immediately created by God, or, (to add that for their sakes, that would have matter to be unproduced,) if matter be eternal, motion must either be produced by some immaterial supernatural agent, or it must immediately flow by way of emanation from the nature of the matter it appertains to.

IV. Neither can there be any physical principles more simple than matter and motion; neither of them being resoluble into any things, whereof it may be truly, or so much as tolerably said to be compounded.

V. The next thing I shall name to recommend the corpuscular principle, is their great comprehensiveness. I consider then, that the genuine and necessary effect of the sufficiently strong motion of one part of matter against another, is, either to drive it on in its intire bulk, or else to break or divide it into particles of determinate motion, figure, size, posture, rest, order or texture. The two first of these, for instance, are each of them capable of numerous varieties. For the figure of a portion of matter may either be one of the five regular figures treated of by geometricians, or some determinate species of solid figures, as that of a cone, cylinder, &c. or irregular, though not perhaps anonymous, as the grains of sand, hoops, feathers, branches, forks, files, &c. And as the figure, so the motion of one of these particles may be exceedingly diversified, not only by the determination to this or that part of the world, but by several other things, as particularly by the almost infinitely varying degrees of celerity, by the manner of its progression with, or without rotation, and other modifying circumstances; and more yet, by the line, wherein it moves, as (besides streight) circular, eliptical, parabolical, hyperbolical, spiral, and I know not how many others. For as later geometricians have shewn, that those crooked lines may be compounded of several motions, (that is, traced by a body, whose motion is mixed of, and results from, two or more simpler motions,) so how many more curves may, or rather may not be made by new compositions and decompositions of motion, is no easy task to determine.

Now, since a single particle of matter, by virtue of two only of the mechanical affections, that belong to it, be diversifiable so many ways; how vast a number of variations may we suppose capable of being produced by the compositions and decompositions of myriads of single invisible corpuscles, that may be contained and contexed in one small body, and each of them be embued with more than two or three of the fertile catholick principles above-mentioned? Especially since the aggregate of those corpuscles may be farther diversified by the texture resulting from their convention into a body, which, as so made up, has its own bigness, and shape, and pores, (perhaps very many and various) and has also many capacities of acting and suffering upon the score of the place it holds among other bodies in a world constituted as ours is: so that, when I consider the almost innumerable diversifications, that compositions and decompositions may make of a small number, not perhaps exceeding twenty of distinct things, I am apt to look upon those, who think the mechanical principles may ferve indeed to give an account of the phænomena of this or that particular part of natural philofophy, as ftaticks, hydroftaticks, the theory of the planetary motions, &c. but can never be applied to all the phænomena of things corporeal; I am apt, I fay, to look upon thofe, otherwife learned men, as I would do upon him, that fhould affirm, that by putting together the letters of the alphabet, one may indeed make up all the words to be found in one book, as in *Euclid*, or *Virgil*; or in one language, as Latin, or Englifh; but that they can by no means fuffice to fupply words to all the books of a great library, much lefs to all the languages in the world.

AND whereas there is another fort of philofophers, that, obferving the great efficacy of the bignefs, and fhape, and fituation, and motion, and connexion in engines, are willing to allow, that thofe mechanical principles may have a great ftroke in the operations of bodies of a fenfible bulk, and manifeft mechanifm, and therefore may be ufefully employed in accounting for the effects and phænomena of fuch bodies, who yet will not admit, that thefe principles can be applied to the hidden tranfactions, that pafs among the minute particles of bodies; and therefore think it neceffary to refer thefe to what they call nature, fubftantial forms, real qualities, and the like unmechanical principles and agents.

BUT this is not neceffary; for both the mechanical affections of matter are to be found, and the laws of motion take place, not only in the great maffes, and the middle fized lumps, but in the fmalleft fragments of matter; and a leffer portion of it being as well a body as a greater, muft, as neceffarily as it, have its determinate bulk and figure: and he, that looks upon fand in a good microfcope, will eafily perceive, that each minute grain of it has as well its own fize and fhape, as a rock or mountain. And when we let fall a great ftone and a pebble from the top of a high building, we find not, but that the latter as well as the former moves conformably to the laws of acceleration in heavy bodies defcending. And the rules of motion are obferved, not only in cannon bullets, but in fmall fhot; and the one ftrikes down a bird according to the fame laws, that the other batters down a wall. And though nature (or rather its divine author) be wont to work with much finer materials, and employ more curious contrivances than art, (whence the ftructure even of the rareft watch is incomparably inferior to that of a human body;) yet an artift himfelf, according to the quantity of the matter he employs, the exigency of the defign he undertakes, and the bignefs and fhape of the inftruments he makes ufe of, is able to make pieces of work of the fame nature or kind of extremely differing bulk, where yet the like, though not equal art and contrivance, and oftentimes motion too, may be obferved: as a fmith, who with a hammer, and other large inftruments, can, out of maffes of iron, forge great bars or wedges, and make thofe ftrong and heavy chains, that were employed to load malefactors, and even to fecure ftreets and gates, may, with leffer inftruments, make fmaller nails and filings, almoft as minute as duft; and may yet, with finer tools, make links of a ftrange flendernefs and lightnefs, infomuch, that good authors tell us of a chain of divers links, that was faftened to a flea, and could be moved by it; and if I mifremember not, I faw fomething like this, befides other inftances, that I beheld with pleafure, of the littlenefs, that art can give to fuch pieces of work, as are ufually made of a confiderable bignefs. And therefore to fay, that though in natural bodies, whofe bulk is manifeft and their ftructure vifible, the mechanical principles may be ufefully admitted, that are not to be extended to fuch portions of matter, whofe parts and texture are invifible; may perhaps look to fome, as if a man fhould allow, that the laws of mechanifm may take place in a town clock, but cannot in a pocket-watch; or, (to give you an inftance, mixed of natural and artificial,) as if, becaufe the terraqueous globe is a vaſt magnetical body of ſeven or eight thouſand miles in diameter, one ſhould affirm, that magnetical laws are not to be expected to be of force in a ſpherical piece of loadſtone, that is not perhaps an inch long: and yet experience ſhews us, that notwithſtanding the ineſtimable diſproportion betwixt theſe two globes, the terrella, as well as the earth, hath its poles, æquator, and meridians, and in divers other magnetical properties, emulates the terreſtrial globe.

THEY, that, to ſolve the phænomena of nature, have recourſe to agents, which, though they involve no ſelf-repugnancy in their very notions, as many of the judicious think ſubſtantial forms and real qualities to do, yet are ſuch, that we conceive not, how they operate to bring effects to paſs: theſe, I ſay, when they tell us of ſuch indeterminate agents, as the ſoul of the world, the univerſal ſpirit, the plaſtic power, and the like; though they may in certain caſes tell us ſome things, yet they tell us nothing, that will ſatisfy the curioſity of an inquiſitive perſon, who ſeeks not ſo much to know, what is the general agent, that produces a phænomenon, as, by what means, and after what manner, the phænomenon is produced. The famous *Sennertus*, and ſome other learned phyſicians, tell us of diſeaſes, which proceed from incantation; but ſure it is but a ſlight account, that a ſober phyſician, that comes to viſit a patient reported to be bewitched, receives of the ſtrange ſymptoms he meets with, and would have an account of, if he be coldly anſwered, that it is a witch, or the devil, that produces them; and he will never ſit down with ſo ſhort an account, if he can by any means reduce thoſe extravagant ſymptoms to any more known and ſtated diſeaſes, as epilepſies, convulſions, hyſterical fits, &c. and, if he cannot, he will confeſs his knowledge of this diſtemper to come far ſhort of what might be expected and attained in other diſeaſes, wherein he thinks himſelf bound to ſearch into the nature of the morbific matter, and will not be ſatisfied, till he can, probably at leaſt, deduce from that, and the ſtructure of an human body, and other concurring phyſical cauſes, the phænomena of the malady. And it would be but little ſatisfaction to one, that deſires to underſtand the cauſes of what occurs to obſervation in a watch, and how it comes to point at, and ſtrike the hours, to be told, that it was ſuch a watch-maker that ſo contrived it; or to him, that would know the true cauſe of an eccho, to be anſwered, that it is a man, a vault, or a wood, that makes it.

AND now at length I come to conſider that, which I obſerve the moſt to alienate other ſects from the mechanical philoſophy; namely, that they think it pretends to have principles ſo univerſal and ſo mathematical, that no other phyſical hypotheſis can comport with it, or be tolerated by it.

BUT this I look upon as an eaſy, indeed, but an important miſtake; becauſe by this very thing, that the mechanical principles are ſo univerſal, and therefore applicable to ſo many things, they are rather fitted to include, than neceſſitated to exclude, any other hypotheſis, that is founded in nature, as far as it is ſo. And ſuch hypotheſes, if prudently conſidered by a ſkilful and moderate perſon, who is rather diſpoſed to unite ſects than multiply them, will be found, as far as they have truth in them, to be either legitimately (though perhaps not immediately) deducible from the mechanical principles, or fairly reconcileable to them. For, ſuch hypotheſes will probably attempt to account for the phænomena of nature, either by the help of a determinate number of material ingredients, ſuch as the *tria prima* of the chemiſts, by participation whereof other bodies obtain their qualities; or elſe by introducing ſome general agents, as the Platonic ſoul of the world, or the univerſal ſpirit, aſſerted by ſome ſpagyriſts; or by both theſe ways together.

NOW, to diſpatch firſt thoſe, that I named in the ſecond place; I conſider, that the chief thing, that inquiſitive naturaliſts ſhould look after in the explicating of difficult

ficult phænomena, is not so much what the agent is or does, as, what changes are made in the patient, to bring it to exhibit the phænomena, that are proposed; and by what means, and after what manner, those changes are effected. So that the mechanical philosopher being satisfied, that one part of matter can act upon another but by virtue of local motion, or the effects and consequences of local motion, he considers, that as if the proposed agent be not intelligible and physical, it can never physically explain the phænomena; so, if it be intelligible and physical, it will be reducible to matter, and some or other of those only catholick affections of matter, already often mentioned. And the indefinite divisibility of matter, the wonderful efficacy of motion, and the almost infinite variety of coalitions and structures, that may be made of minute and insensible corpuscles, being duly weighed, I see not, why a philosopher should think it impossible, to make out, by their help, the mechanical possibility of any corporeal agent, how subtil, or diffused, or active soever it be, that can be solidly proved to be really existent in nature, by what name soever it be called or disguised. And though the Cartesians be mechanical philosophers, yet, according to them, their *Materia Subtilis*, which the very name declares to be a corporeal substance, is, for aught I know, little (if it be at all) less diffused through the universe, or less active in it than the universal spirit of some spagyrists, not to say, the *Anima Mundi* of the Platonists. But this upon the by; after which I proceed, and shall venture to add, that whatever be the physical agent, whether, it be inanimate or living, purely corporeal, or united to an intellectual substance, the above mentioned changes, that are wrought in the body, that is made to exhibit the phænomena, may be effected by the same or the like means, or after the same or the like manner; as for instance, if corn be reduced to meal, the materials and shape of the milstones, and their peculiar motion and adaptation, will be much of the same kind; and (though they should not, yet) to be sure the grains of corn will suffer a various contrition and comminution in their passage to the form of meal; whether the corn be ground by a water-mill or a wind-mill, or a horse mill, or a hand-mill; that is, by a mill, whose stones are turned by inanimate, by brute, or by rational agents. And, if an angel himself should work a real change in the nature of a body, it is scarce conceivable to us men, how he could do it without the assistance of local motion; since, if nothing were displaced, or otherwise moved than before, (the like happening also to all external bodies to which it related,) it is hardly conceivable, how it should be in itself other, than just what it was before.

BUT to come now to the other sort of hypothesis formerly mentioned; if the chemists, or others, that would deduce a compleat natural philosophy from salt, sulphur, and mercury, or any other set number of ingredients of things, would well consider, what they undertake, they might easily discover, that the material parts of bodies, as such, can reach but to a small part of the phænomena of nature, whilst these ingredients are considered but as quiescent things, and therefore they would find themselves necessitated to suppose them to be active; and that things purely corporeal cannot be but by means of local motion, and the effects, that may result from that, accompanying variously shaped, sized, and aggregated parts of matter: so that the chemist and other materialists, if I may so call them, must (as indeed they are wont to do) leave the greatest part of the phænomena of the universe unexplicated by the help of the ingredients (be they fewer or more than three) of bodies, without taking in the mechanical, and more comprehensive affections of matter, especially local motion. I willingly grant, that salt, sulphur, and mercury, or some substances analogous to them, are to be obtained by the action of the fire, from a very great many dissipable bodies here below; nor would I deny, that in explicating divers of the phænomena of ſuch bodies, it may be of uſe to a ſkilful naturaliſt to know and conſider, that this or that ingredient, as ſulphur, for inſtance, does abound in the body propoſed, whence it may be probably argued, that the qualities, that uſually accompany that principle, when predominant, may be alſo, upon its ſcore, found in the body, that ſo plentifully partakes of it. But not to mention, what I have elſewhere ſhewn, that there are many phænomena, to whoſe explication this knowledge will contribute very little or nothing at all; I ſhall only here obſerve, that, though chemical explications be ſometimes the moſt obvious and ready, yet they are not the moſt fundamental and ſatisfactory: for, the chemical ingredient itſelf, whether ſulphur or any other, muſt owe its nature and other qualities to the union of inſenſible particles in a convenient ſize, ſhape, motion or reſt, and contexture; all which are but mechanical affections of convening corpuſcles. And this may be illuſtrated by what happens in artificial fireworks. For, though in moſt of thoſe many differing ſorts that are made, either for the uſe of war, or for recreation, gunpowder be a main ingredient, and divers of the phænomena may be derived from the greater or leſſer meaſure, wherein the compoſitions partake of it; yet, beſides that there may be fire-works made without gunpowder, (as appears by thoſe made of old by the Greeks and Romans,) gun-powder itſelf owes its aptneſs to be fired and exploded to the mechanical contexture of more ſimple portions of matter, nitre, charcoal, and ſulphur; and ſulphur itſelf, though it be by many chemiſts miſtaken for an hypoſtatical principle, owes its inflammability to the convention of yet more ſimple and primary corpuſcles; ſince chemiſts confeſs, that it has an inflammable ingredient, and experience ſhews, that it very much abounds with an acid and uninflammable ſalt, and is not quite devoid of terreſtreity. I know it may be here alledged, that the productions of chemical analyſes are ſimple bodies, and upon that account irreſoluble. But, that divers ſubſtances, which chemiſts are pleaſed to call the ſalts, or ſulphurs, or mercuries of the bodies, that afforded them, are not ſimple and homogeneous, has elſewhere been ſufficiently proved; nor is their not being eaſily diſſipable, or reſoluble, a clear proof of their not being made up of more primitive portions of matter. For, compounded, and even decompounded bodies, may be as difficultly reſoluble, as moſt of thoſe, that chemiſts obtain by what they call their analyſis by the fire; witneſs common green glaſs, which is far more durable and irreſoluble than many of thoſe, that paſs for hypoſtatical ſubſtances. And we ſee, that ſome amels will be ſeveral times even vitrified in the fire, without loſing their nature, or oftentimes ſo much as their colour; and yet amel is manifeſtly, not only a compounded, but a decompounded body, conſiſting of ſalt and powder of pebbles or ſand, and calcined tin, and, if the amel be not white, uſually of ſome tinging metal or mineral. But how indeſtructible ſoever the chemical principles be ſuppoſed, divers of the operations aſcribed to them will never be well made out, without the help of local motion, (and that diverſified too;) without which, we can little better give an account of the phænomena of many bodies, by knowing what ingredients compoſe them, than we can explain the operations of a watch, by knowing of how many, and of what metals the balance, the wheels, the chain, and other parts are made; or than we can derive the operations of a wind-mill from the bare knowledge, that it is made up of wood, and ſtone, and canvas, and iron. And here let me add, that it would not at all overthrow the Corpuſcularian hypotheſis, though either by more exquiſite purifications, or by ſome other operations, than the uſual analyſis of the fire, it ſhould be made appear, that the material principles, or elements of mixed bodies, ſhould not be the *tria prima* of the vulgar chemiſts, but either ſubſtances of another nature, or elſe fewer, or more in number; as would be, if that were true, which ſome ſpagyriſts affirm, (but I could never find,) that from all ſorts

of

of mixed bodies, five, and but five, differing ſimilar ſubſtances can be ſeparated: or, as if it were true, that the Helmontians had ſuch a reſolving menſtruum as the Alkaheſt of their maſter; by which he affirms, that he could reduce ſtones into ſalt of the ſame weight with the mineral, and bring both that ſalt, and all other kind of mixed and tangible bodies, into inſipid water. For, whatever be the number or qualities of the chemical principles, if they be really exiſtent in nature, it may very poſſibly be ſhewn, that they may be made up of inſenſible corpuſcles of determinate bulks and ſhapes; and by the various coalitions and contextures of ſuch corpuſcles, not only three or five, but many more material ingredients, may be compoſed or made to reſult. But, though the Alkaheſtical reductions newly mentioned ſhould be admitted, yet the mechanical principles might well be accommodated even to them. For the ſolidity, taſte, &c. of ſalt, may be fairly accounted for, by the ſtiffneſs, ſharpneſs, and other mechanical affections of the minute particles, whereof ſalts conſiſt; and if, by a farther action of the alkaheſt, the ſalt, or any other ſolid body, be reduced into inſipid water, this alſo may be explicated by the ſame principles, ſuppoſing a farther comminution of the parts, and ſuch an attrition, as wears off the edges and points, that enabled them to ſtrike briſkly the organ of taſte: for, as to fluidity and firmneſs, thoſe mainly depend upon two of our grand principles, motion and reſt. And I have elſewhere ſhewn, by ſeveral proofs, that the agitation of reſt, and the looſer contact, or cloſer cohæſion, of the particles, is able to make the ſame portion of matter, at one time a firm, and at another time a fluid body. So that, though the further ſagacity and induſtry of chemiſts (which I would by no means diſcourage) ſhould be able to obtain from mixed bodies homogeneous ſubſtances, differing in number, or nature, or both, from their vulgar ſalt, ſulphur, and mercury; yet the corpuſcular philoſophy is ſo general and fertile, as to be fairly reconcilable to ſuch a diſcovery; and alſo ſo uſeful, that theſe new material principles will, as well as the old *tria prima*, ſtand in need of the more catholick principles of the Corpuſcularians, eſpecially local motion. And indeed, whatever elements or ingredients men have (that I know of) pitched upon, yet if they take not in the mechanical affections of matter, their principles have been ſo deficient, that I have uſually obſerved, that the materialiſts, without at all excepting the chemiſts, do not only, as I was ſaying, leave many things unexplained, to which their narrow principles will not extend; but, even in the particulars, they preſume to give an account of, they either content themſelves to aſſign ſuch common and indefinite cauſes, as are too general to ſignify much towards an inquiſitive man's ſatisfaction; or if they venture to give particular cauſes, they aſſign precarious or falſe ones, and liable to be eaſily diſproved by circumſtances, or inſtances, whereto their doctrine will not agree, as I have often elſewhere had occaſion to ſhew. And yet the chemiſts need not be frighted from acknowledging the prerogative of the mechanical philoſophy, ſince that may be reconcileable with the truth of their own principles, as far as theſe agree with the phænomena they are applied to. For theſe more confined hypotheſes may be ſubordinated to thoſe more general and fertile principles, and there can be no ingredient aſſigned, that has a real exiſtence in nature, that may not be derived either immediately, or by a row of decompoſitions, from the univerſal matter, modified by its mechanical affections. For if, with the ſame bricks, diverſly put together and ranged, ſeveral walls, houſes, furnaces, and other ſtructures, as vaults, bridges, pyramids, &c. may be built, merely by a various contrivement of parts of the ſame kind; how much more may great variety of ingredients be produced by, or, according to the inſtitution of nature, reſult from the various coalitions and contextures of corpuſcles, that need not be ſuppoſed, like bricks, all of the ſame, or near the ſame ſize and ſhape, but may have amongſt them, both of the one and the other, as great a variety as need be wiſhed for, and indeed a greater than can eaſily be ſo much as imagined? And the primary and minute concretions, that belong to theſe ingredients, may, without oppoſition from the mechanical philoſophy, be ſuppoſed to have their particles ſo minute and ſtrongly coherent, that nature of herſelf does ſcarce ever tear them aſunder; as we ſee, that mercury and gold may be ſucceſſively made to put on a multitude of diſguiſes, and yet ſo retain their nature, as to be reducible to their priſtine forms. And you know, I lately told you, that common glaſs and good amels, though both of them but factitious bodies, and not only mixed, but decompounded concretions, have yet their component parts ſo ſtrictly united by the ſkill of illiterate tradeſmen, as to maintain their union in the vitrifying violence of the fire. Nor do we find, that common glaſs will be wrought upon by aqua fortis, or aqua regis, though the former of them will diſſolve mercury, and the latter gold.

FROM the foregoing diſcourſe it may (probably at leaſt) reſult, that if, beſides rational ſouls, there are any immaterial ſubſtances (ſuch as the heavenly intelligences, and the ſubſtantial forms of the Ariſtotelians,) that regularly are to be numbered among natural agents, their way of working being unknown to us, they can but help to conſtitute and effect things, but will very little help us to conceive how things are effected; ſo that by whatever principles natural things be conſtituted, it is by the mechanical principles, that their phænomena muſt be clearly explicated. As for inſtance, though we ſhould grant the Ariſtotelians, that the planets are made of a quinteſſential matter, and moved by angels, or immaterial intelligences; yet, to explain the ſtations, progreſſions, and retrogradations, and other phænomena of the planets, we muſt have recourſe either to eccentricks, epicycles, &c. or to motions made in elliptical or other peculiar lines; and, in a word, to theories, wherein the motion and figure, ſituation, and other mathematical or mechanical affections of bodies are mainly employed. But if the principles propoſed be corporeal things, they will be then fairly reducible, or reconcilable, to the mechanical principles; theſe being ſo general and pregnant, that among things corporeal, there is nothing real, (and I meddle not with chimerical beings, ſuch as ſome of *Paracelſus*'s,) that may not be derived from, or be brought to, a ſubordination to ſuch comprehenſive principles. And when the chemiſts ſhall ſhew, that mixed bodies owe their qualities to the predominancy of this or that of their three grand ingredients, the Corpuſcularians will ſhew, that the very qualities of this, or that ingredient, flow from its peculiar texture, and the mechanical affections of thoſe corpuſcles it is made up of. And to affirm, that, becauſe the furnaces of chemiſts afford a great number of uncommon productions and phænomena, there are bodies or operations amongſt things purely corporeal, that cannot be derived from, or reconciled to, the comprehenſive and pregnant principles of the mechanical philoſophy, is, as if, becauſe there are a great number and variety of anthems, hymns, pavins, threnodies, courants, gavots, branles, ſarabands, jigs, and other (grave and ſprightly) tunes to be met with in the books and practiſes of muſicians, one ſhould maintain, that there are in them a great many tunes, or at leaſt, notes, that have no dependence on the ſcale of muſick; or, as if, becauſe, beſides rhombuſſes, rhomboids, trapeziums, ſquares, pentagons, chiliagons, myriagons, and innumerable other polygons, regular, and irregular, one ſhould preſume to affirm, that there are among them ſome rectilinear figures, that are not reducible to triangles, or have affections, that will overthrow what *Euclid* has taught of triangles and polygons.

TO what has been ſaid I ſhall add but one thing more; that as, according to what I formerly intimated, mechanical principles and explications are for their clearneſs preferred, even by materialiſts themſelves, to others, in the caſes where they can be had; ſo, the ſagacity and induſtry of modern naturaliſts and mathematicians having happily

pily applied them to ſeveral of thoſe difficult phænomena, (in hydoſtaticks, the practical part of opticks, gunnery, &c.) that before were, or might be referred to occult qualities; it is probable, that when this philoſophy is deeplier ſearched into, and farther improved, it will be found applicable to the ſolution of more and more of the phænomena of nature. And on this occaſion let me obſerve, that it is not always neceſſary, though it be always deſirable, that he, that propounds an hypotheſis in aſtronomy, chemiſtry, anatomy, or other part of phyſicks, be able *à priori*, to prove his hypotheſis to be true, or demonſtratively to ſhew, that the other hypotheſes propoſed about the ſame ſubject muſt be falſe. For as, if I miſtake not, *Plato* ſaid, that 'Ο Θεὸς
the world was God's epiſtle written to mankind, and might have added, conſonantly γεωμετρεῖ
to another ſaying of his, it was written in mathematical letters: ſo, in the phyſical explications of the parts and ſyſtem of the world, methinks, there is ſomewhat like what happens, when men conjecturally frame ſeveral keys to enable us to underſtand a letter written in cyphers. For though one man by his ſagacity have found out the right key, it will be very difficult for him, either to prove otherwiſe than by trial, that this or that word is not ſuch, as it is gueſſed to be by others, according to their keys; or to evince, *à priori*, that theirs are to be rejected, and his to be preferred; yet, if due trial being made, the key he propoſes, ſhall be found ſo agreeable to the characters of the letter, as to enable one to underſtand them, and make a coherent ſenſe of them, its ſuitableneſs to what it ſhould decypher, is, without either confutations, or extraneous poſitive proofs, ſufficient to make it be accepted as the right key of that cypher. And ſo, in phyſical hypotheſes, there are ſome, that, without noiſe, or falling foul upon others, peaceably obtain diſcerning men's approbation only by their fitneſs to ſolve the phænomena, for which they were deviſed, without croſſing any known obſervation or law of nature. And therefore, if the mechanical philoſophy go on to explicate things corporeal at the rate it has of late years proceeded at, it is ſcarce to be doubted, but that, in time, unprejudiced perſons will think it ſufficiently recommended by its conſiſtency with itſelf, and its applicableneſs to ſo many phænomena of nature.

A RECAPITULATION.

PERCEIVING, upon a review of the foregoing paper, that the difficulty and importance of the ſubject, has ſeduced me to ſpend many more words about it, than I at firſt deſigned; it will not now be amiſs to give you this ſhort ſummary of what came into my mind, to recommend to you the mechanical philoſophy, and obviate your fears of ſeeing it ſupplanted; having firſt premiſed once for all, that preſuppoſing the creation and general providence of God, I pretend to treat but of things corporeal, and do abſtract in this paper from immaterial Beings, (which otherwiſe I very willingly admit,) and all agents and operations miraculous or ſupernatural.

I. OF the principles of things corporeal, none can be more few, without being inſufficient, or more primary, than matter and motion.

II. THE natural and genuine effect of variouſly determined motion in portions of matter is, to divide it into parts of differing ſizes, and ſhapes, and to put them into different motions; and the conſequences, that flow from theſe, in a world framed as ours is, are, as to the ſeparate fragments, poſture, order, and ſituation, and, as to the conventions of many of them, peculiar compoſitions and contextures.

3 III. THE

III. THE parts of matter endowed with theſe catholick affections are, by various aſſociations, reduced to natural bodies of ſeveral kinds, according to the plenty of the matter, and the various compoſitions and decompoſitions of the principles; which all ſuppoſe the common matter they diverſify: and theſe ſeveral kinds of bodies, by virtue of their motion, reſt, and other mechanical affections, which fit them to act on, and ſuffer from one another, become endowed with ſeveral kinds of qualities, (whereof ſome are called manifeſt, and ſome occult,) and thoſe, that act upon the peculiarly framed organs of ſenſe, whoſe perceptions, by the animadverſive faculty of the ſoul, are ſenſations.

IV. THESE principles, matter, motion, (to which reſt is related) bigneſs, ſhape, poſture, order, texture, being ſo ſimple, clear, and comprehenſive, are applicable to all the real phænomena of nature, which ſeem not explicable by any other not conſiſtent with ours. For, if recourſe be had to an immaterial principle or agent, it may be ſuch an one, as is not intelligible; and however it will not enable us to explain the phænomena, becauſe its way of working upon things material, would probably be more difficult to be phyſically made out, than a mechanical account of the phænomena. And, notwithſtanding the immateriality of a created agent, we cannot conceive, how it ſhould produce changes in a body, without the help of mechanical principles, eſpecially local motion; and accordingly we find not, that the reaſonable ſoul in man is able to produce what changes it pleaſes in the body, but is confined to ſuch, as it may produce by determining, or guiding the motions of the ſpirits, and other parts of the body, ſubſervient to voluntary motion.

V. AND if the agents, or active principles reſorted to, be not immaterial, but of a corporeal nature, they muſt either in effect be the ſame with the corporeal principles above-named; or, becauſe of the great univerſality and ſimplicity of ours, the new ones propoſed, muſt be leſs general than they, and conſequently capable of being ſubordinate, or reduced to ours, which by various compoſitions may afford matter to ſeveral hypotheſes, and by ſeveral coalitions afford minute concretions exceedingly numerous and durable, and conſequently fit to become the elementary ingredients of more compounded bodies, being in moſt trials ſimilar, and as it were the radical parts, which may, after ſeveral manners, be diverſified; as in Latin, the themes are by prepoſitions, terminations, &c. and in Hebrew, the roots by the hæemantic letters. So that the fear, that ſo much of a new phyſical hypotheſis, as is true, will overthrow, or make uſeleſs the mechanical principles, is, as if one ſhould fear, that there will be a language propoſed, that is diſcordant from, or not reducible to, the letters of the alphabet.

T R A C T S.

CONTAINING

I. Sufpicions about fome Hidden Qualities of the AIR; with an Appendix touching CELESTIAL MAGNETS, and fome other Particulars.

II. Animadverfions upon Mr. HOBBES's PROBLEMATA DE VACUO.

III. A Difcourfe of the Caufe of Attraction by SUCTION.

P R E F A C E.

AMONG other papers, that I defigned to contribute towards the natural hiftory of the air, I began fome years ago to fet down a collection of fome new or lefs heeded obfervations and experiments relating to the caufes and effects of changes in the air, which I referred to feveral heads, as to the air's heat, coldnefs, moifture, drynefs, diaphaneity, opacity, confiftence, feveral faltneffes and other titles; the laft of which was of the occult qualities of the air, fuppofing there be any fuch. And though afterwards I was, by ficknefs and other impediments, diverted from proceeding in that collection, and induced to lay afide fome of the obfervations I had provided, and employ in other treatifes, fuch as were proper to them; yet as to the title, that contained fufpicions about fome hidden qualities of the air, the poffibility, if not likelihood, that either the matters of fact, or the intimations delivered in them, might afford hints not ufelefs to the fagacious and inquifitive, perfuaded me to let it efcape the fate of its companions, though poffibly, if I had more confulted my own reputation, I fhould leaft of all have fuffered this title to appear, there being none of the reft, that was not lefs conjectural. But it being thought unfit, that any thing fhould perifh, that related to fo confiderable and uncommon a fubject, as that of this title, I was content to caft the collected experiments into the following effay, for the reafons expreffed at the beginning and clofe of the enfuing paper. Which, it was hoped, may be the better underftood, and lefs liable to have its defign miftaken, by being ufhered in by this advertifement about the occafion of it.

OBSERVATIONS about the GROWTH OF METALS in their ORE, expofed to the AIR.

IT is altogether unneceffary to my prefent purpofe, to examine, whether metals and minerals, as if they were a kind of fubterraneal plants, do properly grow as vegetables do. For this enquiry belongs to another place, but not to this, where the reference made in a future page of the following paper does not oblige me to fpeak of the growth of metals in any other than a lax and popular fenfe, in which a metal

metal may be ſaid to grow, if a portion of matter being aſſigned, wherein as yet men can find either no metal, as gold or tin, or but ſuch a quantity of it; this being expoſed to the air, will after a time either afford ſome metal, where there appeared none before, or a greater proportion of metal than it had before.

OBSERVATIONS of this kind requiring length of time, as well as reſidence near places abounding with minerals, I have little or no opportunity to make any of them myſelf, at leaſt with the warineſs, that to me ſeems due to obſervations, that I think not eaſy to be well made. And therefore I muſt content myſelf to ſet down what I have been able to learn by converſing with mineraliſts and travellers, and to add ſome particulars, that I met with in authors of good credit.

OBSERVATIONS about the GROWTH of TIN.

AN ancient owner of mines, being aſked by me, whether he could, otherwiſe than upon the conjectures of vulgar tradition, prove, that minerals grow, even after the veins have been dug? anſwered affirmatively; and being deſired to let me know his proofs, he gave me theſe that follow.

HE told me, that not far from his houſe there was a tin-mine, which the old diggers affirmed to have been left off, ſome ſaid eighty, ſome an hundred and twenty years ago, becauſe they had by their waſhing and vanning ſeparated all the ore from the reſt of the earth, and yet of late years they found it ſo richly impregnated with metalline particles, that it was wrought over again with very good profit, and preferred to ſome other mines, that were actually wrought, and had never been ſo robbed. And when I objected, that probably this might proceed from the lazineſs and unſkilfulneſs of workmen in thoſe times, who left in the earth the tin, that was lately ſeparated, and might then have been ſo; I was anſwered, that it was a known thing in the country, that in thoſe times the mine-men were more careful and laborious, to ſeparate the metalline part from the reſt of the ore, than now they are.

HE alſo affirmed to me, that in his own time ſome tenants and neighbours of his (employed by him) having got all the ore they could out of a great quantity of ſtuff, dug out of a tin-mine, they laid the remains in great heaps, expoſed to the air, and within twenty and thirty years after, found them ſo richly impregnated, that they wrought them over again with good benefit.

AND laſtly, he aſſured me, that in a work of his own, wherein he had exerciſed his ſkill and experience, (which is ſaid to be very great) to ſeparate all the particles of tin from the terreſtrial ſubſtances, that were dug up with it out of the vein, he cauſed dams to be made to ſtop the earthy ſubſtance, which the ſtream waſhed away from the ore, giving paſſage to the water; after it had let fall this ſubſtance, which lying in heaps expoſed to the air, within ten or twelve years, and ſometimes much leſs, he examined this, or that heap, and found it to contain ſuch ſtore of metalline particles, as invited him to work it again, and do it with profit. And yet this gentlemen was ſo dextrous at ſeparating the metalline from the other parts of tin-ore, that I could (not without wonder) ſee, what ſmall corpuſcles he would, to ſatisfy my curioſity, ſever from vaſt quantities (in proportion) of earthy and other mineral ſtuff.

RELATIONS agreeable to theſe I received from another very ingenious gentleman, that was converſant with tin-mines, and lived not far from more than one of them.

I was the more ſolicitous to procure an information about the growth of this metal, becauſe the bulk of that, which is uſed in *Europe*, being found in *England*, I have met with little or no mention of the growth of it in outlandiſh writers.

OBSER-

OBSERVATIONS about the GROWTH of LEAD.

AS for the growth of lead in the ore expofed to the air, I remember, I enquired about it of a perfon of quality, who had a patent for divers leaden mines, that were fuppofed to contain filver, and wrought fome of them himfelf at no fmall charge, yet not without profit; and, as I remember, he anfwered me, that the lead-ore, that had been wrought and laid in heaps, did, in tract of time, grow impregnated with metal again, and, as experience manifefted, became worth working a fecond time. And indeed fome mineralifts deliver it as a general obfervation, that the growth and renafcence of metals is more manifeft in lead than in any other of them. *Fefſularum mons in Hetruria*, fays *Boccatius Certardus*, who delivers it as a moft approved truth, *Florentiæ Civitati imminens, lapides plumbarios habet, qui, ſi excidantur, brevi temporis ſpatio novis incrementis inſtaurantur.* J. Gerhard. *in decade quæſtionum, pag. m* 22.

Tu ſubtilius ne quæras (fays *Agricola*, fpeaking of the growth of mines in general) *ſed tantummodo refer animum ad cuniculos, & conſidera, eos adeo interdum memoriâ hominum in anguſtum veniſſe, ut aliqua ſui parte nullum aut admodum difficilum præbeant tranſitum, cum eos ſatis late agere ſoleant foſſores, ne tranſituros impediant. In tales autem anguſtias ſunt adducti propter accretionem materiæ, ex qua lapes eſt factus.*

BUT whether this increment of lead is obfervable in all mines of that metal, I was induced to doubt by the anfwer given me by a gentleman, whofe houfe was feated near feveral lead-mines, and who was himfelf owner of one or two, which he yet caufes to be wrought: for this gentleman, though a chemift, affured me, that in the country, where he lives, which is divided by the fea from that of the perfon above-mentioned, he never obferved the lead-ore to encreafe, either out of the veins or in them; but that in fome places, whence ore had been dug thirty or forty, if not fifty years before, he perceived not on the fides of the paffages, whence the ore had been dug, that any other had grown in its place, or that the paffages, though narrow before, were fenfibly ftraightened, much lefs blocked up.

AND indeed, if there were no other arguments in the cafe, the ftraightning of the ancient paffages in procefs of time would not convince me. For, when I confider, that the foils, that abound with metals, do ufually alfo abound with waters, which are commonly imbibed by the neighbouring earth; and when I confider too, that water is fomewhat expanded by being turned into ice, and that this expanfion is made, (as I have often tried) though flowly, yet with an exceeding great force, by which it often ftretches or breaks the veffels that contain it: when I confider thefe things, I fay, I am apt to fufpect, that fometimes the encreafing narrownefs of the fubterraneal paffages in mines may proceed from this, that the foil, that invirons them, if they lie not deep, may have the water, imbibed by them, frozen in fharp winters. By which glaciation, the moiftened portion of the foil muft forcibly endeavour to expand itfelf, and actually do fo in the parts contiguous to the paffage, fince there it finds no refiftance: and though the expanfion made in one year or two be but fmall, and therefore not obferved; yet, in a fucceffion of many winters, it may by degrees grow to be very confiderable. But this fufpicion I fuggeft not, that I would deny the growth of minerals, but to recommend this argument for it to further confideration. And yet I take this to be a better proof, than what is much relied on by fome writers of metals, who urge, that in churches, and other magnificent buildings, that are leaded over, the metalline roofs, in a long tract of years, grow far more ponderous, infomuch, that oftentimes there is a neceffity to remove them, and exchange them for brafs ones. For though this plaufible argument be urged by feveral writers, and among them by the learned *Jo. Gerhardus*,

hardus, pag. m. 22; yet I fear they proceed upon a miſtake. For having had ſome occaſion to obſerve and enquire after this kind of lead, I ſoon ſuſpected, that the increment of weight, (which ſometimes may indeed be very great) was no clear proof of the real growth of the metal itſelf. For in that, which I had occaſion to conſider, the additional weight, as well as bulk, ſeemed to proceed from acetous or other ſaline corpuſcles of the timber of thoſe buildings, which by degrees exhaling and corroding, that ſide of the lead which they faſtened on, turned it with themſelves into a kind of Ceruſſe: which ſuſpicion I ſhall briefly make probably by noting, 1. That I have found by trial purpoſely made, that woods afford an acid, though not merely acid, liquor, capable of corroding lead. 2. That it is known, that lead turned into Ceruſſe increaſes notably in weight, ſome ſay, (for I had not opportunity to try it) about ſix or ſeven in the hundred. 3. That from the ſheets of lead, that have very long covered churches, and the like buildings, there is often obtained by ſcraping, a good proportion of white lead, which I have known much preferred, by an eminent artiſt, to common Ceruſſe, when a white pigment was to be employed. And, by the way, men's finding this Ceruſſe not on that ſide of the lead, that is expoſed to the outward air, (where I ſcarce ever obſerved any) but on the inſide, that regards the timber and other wooden work, may diſabuſe thoſe, that fancied this ceruſſe to be a part of the lead calcined by the beams of the ſun, that ſtrike immediately upon the metal. And if to this it be added, that by diſtillation and otherwiſe I have found cauſe to ſuſpect, that alabaſter and white marble may emit ſpirituous parts, that will invade lead, it may be doubted, whether what *Galen* relates of the great intumeſcence of leaden bands or faſtenings, wherewith the feet of ſtatues were faſtened to their pedeſtals, be a ſure argument of the real growth of that metal in the air.

BUT I begin to digreſs, and ſeemingly to the prejudice of the particular ſcope of this paper; but yet not to that of one of the main ſcopes of all my phyſical writings, the diſquiſition and advancement of truth.

OBSERVATIONS about the GROWTH of IRON.

I DID not find in one of our chief mines of iron, that there was any notice taken of the growth of that metal; but in another place or two, ſome, that deal in iron-ore, informed me, that they believe it grows, and may be regenerated; and upon that account one of them ſet up a work, contiguous to ſome land of mine, to melt over again the remainder of ore, that had been already wrought (at a great diſtance from that place) and had for ſome ages lain in heaps expoſed to the free air; but with what ſucceſs this chargeable attempt has been made, I am not yet informed.

BUT of the growth of iron in the iſland of *Ilva* or *Elva*, in the Tyrrhene ſea, not far from the coaſt of *Tuſcany*, not only ancient authors, as *Pliny* and *Strabo*, take ſpecial notice, but modern mineraliſts of very good credit, as *Falopius* and *Cæſalpinus*, particularly atteſt the ſame thing; of whom the latter ſpeaks thus: *Vena ferri copioſiſſima eſt in Italia, ob eam nobilitata, Ilva, Tyrreni maris inſula, incredibili copia etiam noſtris temporibus eam gignens: nam terra, quæ eruitur, dum vena effoditur, tota procedente tempore in venam convertitur.* *Lib.* III. *cap.* 6.

AND the experienced *Agricola* gives us the like account of a place in his country, *Germany*, *In Lygiis*, ſays he, *ad Sagam oppidum in pratis eruitur ferrum, foſſis ad altitudinem bipedaneam actis. Id decennio renatum denuo foditur, non aliter ac Ilvæ ferrum.* *Agric. de Vi & Nat. Met. lib.* II. *cap.* 15.

THE learned *Johan. Gerhardus*, out of a book, which he calls *Conciones Metallicæ*; I ſuppoſe he means the High Dutch Sermons of *Matheſius*, (whoſe language I underſtand

derstand not) has this notable paſſage to our preſent purpoſe: *Relatum mihi eſt a metallico foſſore, ad ferrarias, quæ non longé Ambergâ diſtant, terram inanem cum ferri minera erutam, quam vocant* den Gummer, *mixtam cum recrementis ferri, quæ appellatur* der Sinder, *congeſtam in cumulos, inſtar magni cujuſdam valli, ſolibus pluviiſque exponi, & decimo quinto anno denuo excoqui, eliquarique ferrum tantæ tenacitatis, ut ſolæ laminæ inde procudantur.*

J. Gerhard. Profeſſor Tubingenſis. Decad. Quæſt. Phyſico-chymicarum, pag. m. 18.

OBSERVATIONS about the GROWTH of SILVER.

OF the growth, as is ſuppoſed, of ſilver in the form of trees or glaſs or other vegetables, I have met with ſome inſtances among mineraliſts, and I have elſewhere mentioned, that an acquaintance of mine ſhewed me a ſtone, wherein he affirmed the ſilver, I ſaw in it, to have increaſed ſince he had it. But for certain reaſons, none of theſe relations ſeem to me very proper to my preſent purpoſe; in order to which, I ſhall therefore ſet down only one inſtance, which I lately met with in a French collection of voyages, publiſhed by a perſon of great curioſity and induſtry, from whoſe civility I received the book. For there, in an account given by a gentleman of his country of a late voyage he made to *Peru*, wherein he viſited the famous ſilver-mines of *Potoſi*, I found a paſſage, which ſpeaks to this ſenſe: *Le meilleur argent, &c.* i. e. The beſt ſilver in all the *Indies*, and the pureſt, is that of the mines of *Potoſi*, the chief have been found in the mountain of *Aranzaſſe:* and (ſome lines being interpoſed) it is added, that they draw this metal even from the mineral earths, that were in times paſt thrown aſide, when the ground was open, and the grooves and ſhafts, that are in the mountains, were made; it having been obſerved, that in theſe recrements, metal had been formed afreſh ſince thoſe times, which ſufficiently ſhews the propenſity of the ſoil to the production of this metal; yet it is true, that theſe impregnated earths yield not ſo much as the ordinary ore, which is found in veins betwixt the rocks

Voyage du Sieur au Peru, pag. 15.

OBSERVATIONS about the GROWTH of GOLD.

AS for the growth of gold, the enquiries I have yet made among travellers give me no great ſatisfaction about it, and though I have ſpoken with ſeveral, that have been at the coaſt of *Guinea*, and in *Congo*, and other parts of *Afric*, where much gold is to be had; yet I could not learn by them, that they, or any acquaintance of theirs among the natives, had ſeen any mines or veins of gold, (which yet divers authors affirm to be found in more than one kingdom of *Æthiopia*, and in ſome other African Countries.) And having afterwards met with a learned traveller, that had carefully viſited the famous gold-mines of *Cremnitz* in *Hungary*, he anſwered me, that he did not learn from the miners, whether or no the ores of gold, &c. did really grow or were regenerated in tract of time, by being expoſed to the air, or upon any other account; but the grand overſeer, who was lord of part of the ſoil, told him, that he thought the whole mountain to abound with particles of gold, and therefore was wont, when the diggers had almoſt exhauſted the vein, to caſt in ſtore of earth, and dig up other neighbouring places, which, being kept there as in a conſervatory, would afterwards afford gold, as the mine did before.

AND, if a late German profeſſor of phyſick do not miſinform us, his country affords an eminent inſtance of the growth or regeneration of gold. *Nam Corbachi,* ſays

 he,

he, *quæ est civitas Westphaliæ, sub ditione comitis de Isenborg & Waldeck, aurum excoquitur ex cumulis congestis, ita ut singulis quadrienniis iterum elaboretur cumulus unus, semper se restaurante naturâ, &c.*

Johan. Gerhardus in Decade Quæstionum, pag. m. 19.

POSTSCRIPT.

SINCE the setting down of the foregoing observations, I casually met with a curious book of travels, lately made by the very ingenious Dr. *Edward Brown*, and finding in page 100, a couple of relations, that seem pertinently referable, the one to a passage above-cited out of *Agricola*, in the notes about the growth of lead, and the other to the present title about the growth of gold; I thought fit to annex them in the learned author's own words, viz.

"1. SOME passages in this mine cut through the rock, and long disused, have grown "up again: and I observed the sides of some, which had been formerly wide enough "to carry their ore through, to approach each other, so as we passed with difficulty. "This happens most in moist places; the passages unite not from the top to the bot-"tom, but from one to another.

"2. THE common yellow earth of the country near *Cremnitz*, especially of the "hills towards the west, although not esteemed ore, affords some gold: and in one "place, I saw a great part of an hill digged away, which hath been cast into the "works, washed and wrought in the same manner as pounded ore, with considerable "profit."

THE fore-going observations about the growth of gold and other metals are not all, that I might, perhaps without being blamed for it, have referred to that title. But all my papers, wherein other observations of this kind were set down, are not now at hand, and divers other instances, that I have met with among writers, of the growth of metals, (taking that expression in the sense I formerly declared) do not seem to me so pertinent in this place, because the improving ores were not exposed, nor perchance accessible to the air. And even as to the instances, that I have now mentioned, till severer observations have been made, to determine, whether it be partly the contact or the operation of the air, or some internal disposition, analogous to a metalline seed or ferment, that causes this metalline increment, I dare not be positive; though I thought the interest of the air in this effect might make it pardonable, to add on this occasion to the history of nature some particulars, of which the cause conjecturally proposed may be probably enough to countenance a suspicion, till further experience have more clearly instructed us.

To what has been said of the growth of metals in the air, I add some instances of the growth of fossile salts, and of some other minerals: but, besides that these belong to the paper about the saltness of the air; what has been already said may suffice for the present occasion.

POSTSCRIPT.

AFTER what I writ in a part of the following discourse, having an opportunity to look again upon the marchasite there mentioned to have been hermetically sealed up after its surface had been freed from the grains of vitriolate salt, that adhered to it, I preceived, that notwithstanding the glass had been so closely stopped, yet there plainly appeared from the outside of the mass some grains of an efflorescence, whose colour between blue and green, argued it to be of a vitriolate nature. If this be seconded with other trials made with the like success, it may suggest new thoughts about the growth of metals and minerals, especially with reference to the air.

S U S-

SUSPICIONS

ABOUT

Some HIDDEN QUALITIES in the AIR.

BESIDES the four firſt qualities of the air, (heat, cold, dryneſs and moiſture) *that are known even to the vulgar*; and thoſe more unobvious, that philoſophers and chemiſts have diſcovered, ſuch as gravity, ſpringineſs, the power of refracting the beams of light, &c. I have often ſuſpected, that there may be in the air ſome yet more latent qualities or powers differing enough from all theſe, and principally due to the ſubſtantial parts or ingredients, whereof it conſiſts. And to this conjecture I have been led, partly (though not only, or perhaps chiefly) by conſidering the conſtitution of that air we live and breathe in, which, to avoid ambiguities, I elſewhere call Atmoſpherical air. For this is not, as many imagine, a ſimple and elementary body, but a confuſed aggregate of effluviums from ſuch differing bodies, that, though they all agree in conſtituting, by their minuteneſs and various motions, one great maſs of fluid matter, yet perhaps there is ſcarce a more heterogeneous body in the world.

AND as by air I underſtand not, (as the Peripateticks are wont to do) a mere elementary body; ſo when I ſpeak of the qualities of the air, I would not be thought to mean ſuch naked and abſtracted beings (as the ſchools often tell us of,) but ſuch as they call qualities *in concreto*, namely, corpuſcles endued with qualities, or capable of producing them in the ſubjects they invade and abound in.

I have elſewhere ſhewn it to be highly probable, that, beſides thoſe vapours and exhalations, which by the heat of the ſun are elevated into the air, and there afford matter to ſome meteors, as clouds, rain, parhelions and rain-bows, there are, at leaſt at ſome times, and in ſome places, ſtore of effluviums emitted from the ſubterraneal parts of the terreſtrial globe; and it is no leſs probable, (from what I have there and elſewhere delivered) that in the ſubterraneal regions there are many bodies, ſome fluid and ſome conſiſtent, which, though of an operative nature, and like, upon occaſion, to emit ſteams, ſeldom or never appear upon the ſurface of the earth, ſo that many of them have not ſo much as names aſſigned them even by the mineraliſts. Now, among this multitude and variety of bodies, that lie buried out of our ſight, who can tell, but that there may be ſome, if not many, of a nature very differing from thoſe we are hitherto familiarly acquainted with; and that, as divers wonderful and peculiar operations of the loadſtone, (though a mineral many ages ago famous among philoſophers and phyſicians) were not diſcovered till of later ages, wherein its nobler virtues have been diſcloſed; ſo there may be other ſubterraneous bodies, that are endowed with conſiderable powers, which, if they were known, be found very differing from thoſe of the foſſiles we are wont to deal with?

In a paper about ſubterraneal ſteams.

I alſo further conſider, that (as I have elſewhere endeavoured to make it probable) the ſun and planets (to ſay nothing of the fixed ſtars) may have influences here below diſtinct from their heat and light. On which ſuppoſition it ſeems not abſurd to me to ſuſpect, that the ſubtil, but corporeal, emanations even of theſe bodies may (ſometimes at leaſt) reach to our air, and mingle with thoſe of our globe in that great receptacle or rendeſvous of celeſtial and terreſtrial effluviums, the atmoſphere. And if

this

this ſuſpicion be not groundleſs, the very ſmall knowledge we have of the ſtructure and conſtitution of globes, ſo many thouſands or hundreds of thouſands of miles remote from us, and the great ignorance we muſt be in of the nature of the particular bodies, that may be preſumed to be contained in thoſe globes, (as minerals and other bodies are in the earth) which in many things appear of kin to thoſe that we inhabit, (as with excellent teleſcopes I have often with attention and pleaſure obſerved, particularly in the moon) this great imperfection, I ſay, of our knowledge may keep it from being unreaſonable to imagine, that ſome, if not many, of thoſe bodies and their effluxions, may be of a nature quite differing from thoſe we take notice of here about us, and conſequently may operate after a very differing and peculiar manner.

AND though the chief of the heteroclite effluviums, that endow the air with hidden qualities, may probably proceed from beneath the ſurface of the earth, and from the celeſtial bodies; yet I would not deny, but that, eſpecially at ſome times, and in ſome places, the air may derive multitudes of efficacious particles from its own operations, acting as a fluid ſubſtance upon that vaſt number and variety of bodies, that are immediately expoſed to it. For, though, by reaſon of its great thinneſs, and of its being in its uſual ſtate devoid both of taſte and ſmell, it ſeems wholly unfit to be a menſtruum; yet I am not ſure but it may have a diſſolving, or at leaſt a conſuming, power on many bodies, eſpecially ſuch as are peculiarly diſpoſed to admit its operations.

FOR I conſider, that the air has a great advantage by the vaſt quantity of it, that may come to work in proportion to the bodies that are expoſed to it: and I have long thought, that, in divers caſes, the quantity of a menſtruum may much more conſiderably compenſate its want of ſtrength, than chemiſts are commonly aware of, (as there may be occaſion elſewhere to exemplify.) And there are liquors, which paſs for inſipid, (and are therefore thought to be altogether unfit to be ſolvents,) which, though they have their active parts too thinly diſperſed to be able preſently to make ſenſible impreſſions upon our organs of taſting, yet are not quite deſtitute of corpuſcles fit to act as a ſolvent; eſpecially if they have time enough to make with the other parts of the fluid ſuch numerous and various motions, as muſt bring, now ſome of them, and then others, to hit againſt the body expoſed to them. Which may be illuſtrated by the ruſt like to verdigreaſe, which we have obſerved in copper, that has been long expoſed to the air, whoſe ſaline particles, little by little, do, in tract of time, faſten themſelves in ſuch numbers to the ſurface of the metal as to corrode it, and produce that efforeſcence coloured like verdigreaſe which you know is a factitious body, wont to be made of the ſame metal, corroded by the ſharp corpuſcles of vinegar, or of the huſks of grapes: beſides, that by the power, which mercury has to diſſolve gold and ſilver, it appears, that it is not always neceſſary for the making a fluid fit to be a diſſolvent, that it ſhould affect the taſte. And as to thoſe bodies, on which the aerial menſtruum can, though but ſlowly, work, the greateſt quantity of it may bring it this advantage, that, whereas even the ſtrongeſt menſtruums, if they bear no great proportion in bulk to the bodies they are to work on, are eaſily glutted, and being unable to take up any more, are fain to leave the reſt of the body undiſſolved, our aerial menſtruum bears ſo vaſt a proportion to the bodies expoſed to it, that when one portion of it has impregnated itſelf as much as it is able, there may ſtill come freſh and freſh to work further on the remaining part of the expoſed body.

BESIDES the ſaline and ſulphureous particles, that, at leaſt in ſome places, may (as I have elſewhere ſhewn) impregnate the air, and give it a greater affinity to chemical menſtruums more ſtrictly ſo called; I am not averſe from thinking, that the air, merely as a fluid body, that conſiſts of corpuſcles of differing ſizes and ſolidities reſtleſly and very variouſly moved, may upon the account of theſe corpuſcles be ſtill reſolving, or preying

preying upon the particles of the bodies, that are expoſed to their action. For many of thoſe aerial corpuſcles, ſome hitting and ſome rubbing themſelves every minute againſt thoſe particles of expoſed bodies, that chance to lie in their way, may well, by thoſe numerous occurſions and affrictions, ſtrike off and carry along with them now ſome, and then others of thoſe particles; as you ſee it happens in water, which, as ſoft and fluid as it is, wears out ſuch hard and ſolid bodies as ſtones themſelves, if it often enough meet them in its paſſage, according to the known ſaying,

Gutta cavat lapidem non vi, ſed ſæpe cadendo.

And though the aerial corpuſcles be very minute, and the bodies expoſed to them oftentimes large and ſeemingly ſolid; yet this needs not make you reject our ſuppoſition, becauſe it is not upon the whole body at once, that, according to us, the aerial corpuſcles endeavour to work, but upon the ſuperficial particles, which may often be more minute than thoſe corpuſcles; as you will the more eaſily believe, if you firſt obſerve with a good microſcope, how many extant particles may be met with on the ſurface of bodies, that to the naked eye ſeem very ſmooth, and even of thoſe, that are poliſhed by art with tripoli or puttee; and then conſider, that one of theſe protuberances, being yet manifeſtly viſible, may well be preſumed to conſiſt of a multitude of leſſer particles, divers of which may very well be as minute as thoſe aerial corpuſcles, that ſucceſſively hit againſt them, and endeavour to carry them along with themſelves. And this may be illuſtrated by a familiar inſtance. For if you take a lump of loaf ſugar, or even of a much ſolider and harder body, ſal gemmæ, and caſt it into common water, though this liquor is inſipid, and the motions of its corpuſcles but very languid; yet theſe corpuſcles are capable to looſen and carry off the ſuperficial particles of ſugar or ſalt, that chance to lye in their way, and freſh corpuſcles of water ſtill ſucceeding to work upon the remaining particles of the expoſed body, that ſtands in their way, the whole lump is, by and little, diſſolved, and ceaſes to appear to the eye a thing diſtinct from the liquor.

SOME things, that have occurred to me, have made me ſuſpect, that it is not impoſſible, but that ſome bodies may receive a diſpoſition to volatility, and conſequently to paſs into the air by the action either of the ſun-beams, in the form of ſun-beams, or of ſome ſubſtance, that once iſſued out of the ſun, and reached unto the air. For there may be certain bodies for the moſt part in the form of liquors, which though they paſs off from ſome peculiarly diſpoſed bodies, may during their ſtay or contact produce in them a great and ſtrange aptneſs to be volatized. In favour of which conjecture, I might here alledge both the effects, which the Paracelſians and Helmontians aſcribe to the Alkaheſt, of volatizing even fixed and ponderous bodies barely by being often abſtracted from them, and ſome other things, which I ſhall now leave unmentioned, becauſe you may find them in my notes about Volatility and Fixity.

BUT whatever become of this conjecture, it is conſonant to experience, that, either upon the above recited accounts, or alſo ſome others, thoſe parts of the atmoſphere, which, in a ſtricter ſenſe, may be called the air, are, at leaſt, in ſome places, ſo intermixed with particles of differing kinds, that among that great number of various ſorts of them, it is very likely that there ſhould be ſome of an uncommon and unobſerved nature. And I could countenance what has been ſaid by the waſting of odorous bodies, and eſpecially camphire, and by repreſenting, that I have obſerved ſome ſolid bodies actually cold, when their ſuperficial parts were newly taken off, to emit, though inviſibly, ſuch copious ſteams into the air, as to grow continually and manifeſtly lighter upon the balance, ſo as to ſuffer a notable decrement of weight in a minute of an hour. But the mention I make of ſuch things in another paper, diſſuades me from in-

ſiſting

ſiſting on them here, where it will be ſeaſonable to reſume the diſcourſe, which the mention of the diſſolving power, that may be gueſſed to be in the air, has for ſome pages interrupted, and to tell you, that thoſe propounded, before I entered upon the digreſſion, are the two main conſiderations *à priori* (as they ſpeak) whereon I have grounded my ſurmiſe, which being propoſed but as a ſuſpicion, I preſume it will not be expected, that the argument *à poſteriori*, which I ſhall bring to countenance it, ſhould be more than conjectures, much leſs, that they ſhould be demonſtrations. And therefore I ſhall venture to lay before you ſome few phænomena, which ſeem to be at leaſt as probably referable to ſome latent quality in the air, as to any other cauſe I yet know. Upon which ſcore ſuch phænomena may be allowed to be pleaded in favour of our ſuſpicion, until ſome other certain cauſe of them ſhall be ſatisfactorily aſſigned.

HAVING premiſed thus much to keep you from looking for ſtronger proofs than I think my taſk obliges me to give; the firſt phænomenon, I ſhall propoſe, ſhall be the appearing or growth of ſome ſalts in certain bodies, which we obſerved to afford them either not at all, or at leaſt nothing near in ſuch plenty, or ſo ſoon, unleſs they be expoſed to the air. Of ſuch a phænomenon as this, that is not ſo much as mentioned by vulgar philoſophers, and very rarely, if at all, to be met with in the laboratories of chymiſts, you will not, I ſuppoſe, wonder, that I do not preſent you many examples, and ſome few I am able to name. For I remember, that ſuſpecting a ſolid marchaſite, hard as ſtone, to be fit to be made an inſtance for my purpoſe, I cauſed it to be broken, that the internal more ſhining parts might be expoſed to the air; but, though this were done in a room, where a good fire was uſually kept, ſo that the marchaſite was not only ſheltered from the rain, but kept in a dry air, yet after a while I diſcovered upon the gliſtering parts an effloreſcence of a vitriolate nature.

AND afterwards meeting with a ponderous and dark coloured mineral, and which, at the firſt breaking, diſcovered to the eye no appearance of any ſalt, nor ſo much as any ſhining marchaſitical particles, we found nevertheleſs, that a good quantity of theſe hard and heavy bodies, being kept expoſed to the air, even in a room, that preſerved them from the rain, though probably they had lain many ages entire in the hill, wherein they were found under ground; yet in not many months, by the operation of the air upon them, they were, in great part, crumbled to powder exceeding rich in copperas. Nay, I remember, that having for curioſity ſake, laid up ſome of theſe ſtones in a room, where I conſtantly kept fire, and in the drawer of a cabinet, which I did not often take out to give them freſh air, ſome, if not moſt of them, were notwithſtanding covered with a copious effloreſcence, which by its conſpicuous colour between blue and green, by its taſte, and by its fitneſs to make in a trice an inky mixture with infuſion of galls, ſufficiently manifeſted itſelf to be vitriol; whoſe growth by the help of the contact of the air is the more conſiderable, becauſe it is not a mere acid ſalt, but abounds in ſulphureous and combuſtible parts, which I have divers times been able, by methods elſewhere mentioned actually to ſeparate or obtain from common vitriol without the addition of any combuſtible body, and ſometimes without any additament at all. It was alſo uncommon, that our blackiſh minerals required no longer time, nor no rain, to make them afford their vitriolate effloreſcences: for I remember, I kept many of thoſe marchaſites, both glittering ones and others, of which they make and ſell great quantities of vitriol at *Deptford*, without perceiving in them a change, that came any thing near to what I have recited. And I obſerved thoſe, whoſe trade it is to make vitriol, to be often obliged to let their vitriol-ſtones, as they call them, lie half a year, or even eighteen months, or two years expoſed, not only to the open air, but to the rain and ſun, to be able to obtain from them their vitriolate parts.

THAT

THAT also the earth or ore of allum, being robbed of its salt, will in tract of time recover it by being exposed to the air, we are assured by the experienced *Agricola*, where, having delivered the way of making allum, he subjoins this advertisement: *Terra aluminosa, quæ in castellis diluta, postquam effluxit, superfuit egesta & coacervata quotidie, rursus magis & magis fit aluminosa, non aliter atque terra, ex qua halinitrum fuit confectum, suo succo plenior fit; quare denuo in castella conjicitur, & aquæ affusæ ea percolantur.*

I have likewise observed, as you also perchance have done, that some kind of lime in old walls and moist places has gained in length of time a copious efflorescence, very much of nitrous nature; as I was convinced by having obtained salt-petre from it by barely dissolving it in common water, and evaporating the filtrated solution: and, that in calcined vitriol, whose saline parts have been driven away by the violence of the fire, particles of fresh salt may be found, after it has lain a competent time in the air, I shall before long have occasion to inform you.

BUT in the mean time, (to deal ingenuously with you,) I shall confess to you, that though these and the like observations have satisfied learned men, without having been called in question, and consequently have, at least, probability enough to ground our suspicion upon; yet I, that am more concerned for the discovery of a truth than the reputation of a paradox, propose the argument drawn from the foregoing observations, but as a probationer. For it yet seems to me somewhat doubtful, whether the salts, that appear in the forementioned cases, are really produced by the operation of the air working as an agent, or also concurring as an ingredient; or whether these saline substances be not the production of some internal thing, that is analagous to a seminal principle, which makes in these bodies a kind of maturation of some parts, which being once ripened, and perhaps assisted by the moisture of the air, disclose themselves in the form of saline concretions; as in the feculent or tartareous parts of many wines, there will in tract of time be generated or produced store of corpuscles of a saline nature, that produce the acid taste we find in tartar, especially, that of rhenish wine. It may also be suspected, that the formerly mentioned salts found in marchasites, in nitrous and aluminous earths, &c. are made by the saline particles of the like nature, that among multitudes of other kinds swim in the air, and are attracted by the congenerous particles, that yet remain in the terrestrial bodies that are, as it were, the wombs of such minerals, (as I have elsewhere shewn, that the spirit of nitre will, with fixed nitre and some other alkalies, compose salt-petre;) or else, that these ærial salts, if I may so call them, assisted by the moisture of the air, do soften and open, and almost corrode or dissolve the more terrestrial substance of these wombs, and thereby solicit out and somewhat extricate the latent saline particles, and, by their union with them, compose those emerging bodies, that resemble vitriol, allum, &c.

BUT not only to suggest these scruples, as if I had a mind they should but trouble you, and keep you irresolute, I shall propound something towards the removal of them; namely, that a convenient quantity of nitrous earth, or that other of those substances, which you would examine, be kept in a close vessel to which the air has not access, for at least as long time as has been observed to be sufficient to impregnate the like substance, or rather a portion of the same parcel, that was chosen to be included: for if the body, that was kept close, have either gained no salt at all, or very much less in proportion to its bulk than that, which was kept exposed, we may thence estimate what is to be ascribed to the air in the production of nitre or other saline concretions. And, because I have observed none of these bodies, that would so soon, and so manifestly even to the eye, disclose a saline substance, as the blackish vitriol-ore, I lately told you I kept in a drawer of my cabinet; I judged, that a very fit subject, wherewith to try, what maturation, or time, when the air was ſecluded, would perform towards the deciding of our difficulty: and accordingly having taken ſome fragments of it, which we had carefully freed from the adhering vitriolate effloreſcence, by whoſe plenty we are aſſured, that is was very well diſpoſed to be wrought on by the air, we put of theſe fragments of differing ſizes into two conveniently ſhaped glaſſes, which being hermetically ſealed were ordered to be carried away, and kept in fixed places; by which means it was expected, that, even without opening the glaſſes, we ſhould be able eaſily to ſee by the changed colour of the ſuperficial parts, whether any vitriolate effloreſcence were produced; but, through the negligence or miſtake of thoſe, to whom the care was recommended, the experiment was never brought to an iſſue; and though I afterwards got more of the mineral, and made a ſecond trial of the ſame, I have not yet been informed of the event.

BUT, Sir, though, until the ſucceſs of ſome trial be known, I dare not too confidently pronounce about the production or regeneration of ſalts in bodies, that have been robbed of them, and aſcribe it wholly to the air; yet, when I conſider the ſeveral and great effects of the air upon divers other bodies, I think it not raſh to conjecture, in the mean time, that the operations of the air may have a conſiderable ſhare in theſe phænomena, and ſo that there may be latent qualities in the air, in the ſenſe I declared above, where I told you, that when I ſpeak of theſe qualities, I look upon them *in concreto*, (as they phraſe it,) together with the ſubſtances or corporeal effluvia they reſide in: and of theſe ærial qualities, taken in this ſenſe, I ſhall now proceed to mention ſome other inſtances.

THE difficulty we find of keeping flame and fire alive, though but for a little time, without air, makes me ſometimes prone to ſuſpect, that there may be diſperſed through the reſt of the atmoſphere ſome odd ſubſtance, either of a ſolar, or aſtral, or ſome other exotic nature, on whoſe account the air is ſo neceſſary to the ſubſiſtence of flame; which neceſſity I have found to be greater, and leſs dependent upon the manifeſt attributes of the air, than naturaliſts ſeem to have obſerved. For I have found by trials purpoſely made, that a ſmall flame of a lamp, though fed perhaps with a ſubtil thin oyl, would in a large capacious glaſs-receiver expire, for want of air, in a far leſs time than one would believe. And it will not much leſſen the difficulty to alledge, that either the groſs fuliginous ſmoak did in a cloſe veſſel ſtifle the flame, or, that the preſſure of the air is requiſite to impel up the aliment into the wick: for, to obviate theſe objections, I have in a larger receiver imployed a very ſmall wick with ſuch rectified ſpirit of wine, as would in the free air burn totally away; and yet, when a very ſmall lamp, furniſhed (as I was ſaying) with a very ſlender wick, was made to burn, and, filled with this liquor, was put lighted into a large receiver, that little flame, though it emitted no viſible ſmoak at all, would uſually expire within about one minute of an hour, and, not ſeldom, in a leſs time; and this, though the wick was not ſo much as ſinged by the flame: nor indeed is a wick neceſſary for the experiment, ſince highly rectified ſpirit of wine will in the free air flame away well without it. And indeed it ſeems to deſerve our wonder, what that ſhould be in the air, which enabling it to keep flame alive, does yet, by being conſumed or depraved, ſo ſuddenly render the air unfit to make flame ſubſiſt. And it ſeems by the ſudden waſting or ſpoiling of this fine ſubject, whatever it be, that the bulk of it is but very ſmall in proportion to the air it impregnates with its virtue. For after the extinction of the flame, the air in the receiver was not viſibly altered, and, for aught I could perceive by the ways of judging I had then at hand, the air retained either all, or at leaſt far the greateſt part of its elaſticity, which I take to be its moſt genuine and diſtinguiſhing property.

AND this undeſtroyed ſpringineſs of the air ſeems to make the neceſſity of freſh air to the life of hot animals, (few of which, as far as I can gueſs after many trials, would be able to live two minutes of an hour, if they were totally and all at once deprived of air,) ſuggeſt a great ſuſpicion of ſome vital ſubſtance, if I may ſo call it, diffuſed through the air, whether it be a volatile nitre, or (rather) ſome yet anonimous ſubſtance, ſydereal or ſubterraneal, but not improbably of kin to that, which I lately noted to be ſo neceſſary to the maintenance of other flames.

I know not, whether you will think it pertinent to our preſent diſcourſe, that I obſerve to you, that by keeping putrifying bodies in glaſſes, which by *Hermes* his ſeal were ſecured from the contact of the external air, I have not been able to produce any inſect, or other living creature, though ſometimes I have kept animal ſubſtances, and even blood ſo included, for many months, and one or two of them for a longer time; and though all theſe ſubſtances had a manifeſt change made in their conſiſtence whilſt they remained ſealed up.

ON this occaſion I ſhall add an odd obſervation, that I met with in a little diſſertation *de admirandis Hungariæ aquis*, written by an anonymous, but ingenious nobleman of that country, where, ſpeaking of the native ſalt, that abounds in their regions, he ſays, that in the chief mine (by them called *Deſienſis)* of *Tranſylvania*, there was, a few years before he writ, a great oak, like a huge beam, dug out of the middle of the ſalt; but, though it was ſo hard, that it would not eaſily be wrought upon by iron tools, yet being expoſed to the air out of the mine, it became ſo rotten, as he expreſſes it, that in four days it was eaſy to be broken, and crumbled between one's fingers. And of that corruptive or diſſolutive power of air near thoſe mines, the ſame author mentions other inſtances.

HAVING found an antimonial preparation to procure vomits, in a caſe where I did not at all expect it, I was afterwards curious to enquire of ſome phyſicians and chymiſts, that were of my acquaintance, whether they had not taken notice, that Antimonium Diaphoreticum, which, as its name imports, is wont to work by ſweat or tranſpiration, would not become vomitive, if it were not kept from the air? To which one phyſician, that was a learned man, aſſured me, it would, as he had found by particular trials: and the like anſwer has been given me by more than one. And I find, that the experienced *Zwelfer* himſelf does ſomewhere give a caution againſt letting the air have acceſs to theſe antimonial medicines, leſt it ſhould render them, as he ſays it will, in tract of time, not only emetic, but diſpoſed to produce heart-burnings, (as they call them,) faintings, and other bad ſymptoms. And I learned by enquiry, from a very ingenious doctor of phyſic, that, having carefully prepared Antimonium Diaphoreticum, he gave many doſes whilſt it was freſh and kept ſtopped in a glaſs, (without finding, that in any patient it procured ſo much as one vomit,) but having kept a parcel of the ſelf-ſame remedy for a pretty while in a glaſs only covered looſely with a paper, the medicine, vitiated by the air, proved emetic (ſtrongly enough) to thoſe, who neither by conſtitution, or foulneſs of ſtomach, or on any other diſcernible account, were more than others, that had taken it diſpoſed to vomit. By which obſervations, and from what I formerly told you of the ſalt-petre obtainable from quicklime, a man partial to the air would be made forward to tell you, that this looks, as if either there were in the air a ſubſtance diſpoſed to be aſſimilated by all kinds of bodies, or that the air is ſo vaſt and rich a rendezvouz of innumerable ſeminal corpuſcles, and other analogous particles, that almoſt any body long expoſed to it may there meet with particles of kin to it, and fit to repair its wrongs and loſſes, and reſtore it to its natural condition. But without taking any further notice of this odd ſurmize, I will

 proceed

ject, wherewith to try, what maturation, or time, when the air was fecluded, would perform towards the deciding of our difficulty: and accordingly having taken fome fragments of it, which we had carefully freed from the adhering vitriolate efflorefcence, by whofe plenty we are affured, that is was very well difpofed to be wrought on by the air, we put of thefe fragments of differing fizes into two conveniently fhaped glaffes, which being hermetically fealed were ordered to be carried away, and kept in fixed places; by which means it was expected, that, even without opening the glaffes, we fhould be able eafily to fee by the changed colour of the fuperficial parts, whether any vitriolate efflorefcence were produced; but, through the negligence or miftake of thofe, to whom the care was recommended, the experiment was never brought to an iffue; and though I afterwards got more of the mineral, and made a fecond trial of the fame, I have not yet been informed of the event.

BUT, Sir, though, until the fuccefs of fome trial be known, I dare not too confidently pronounce about the production or regeneration of falts in bodies, that have been robbed of them, and afcribe it wholly to the air; yet, when I confider the feveral and great effects of the air upon divers other bodies, I think it not rafh to conjecture, in the mean time, that the operations of the air may have a confiderable fhare in thefe phænomena, and fo that there may be latent qualities in the air, in the fenfe I declared above, where I told you, that when I fpeak of thefe qualities, I look upon them *in concreto*, (as they phrafe it,) together with the fubftances or corporeal effluvia they refide in: and of thefe ærial qualities, taken in this fenfe, I fhall now proceed to mention fome other inftances.

THE difficulty we find of keeping flame and fire alive, though but for a little time, without air, makes me fometimes prone to fufpect, that there may be difperfed through the reft of the atmofphere fome odd fubftance, either of a folar, or aftral, or fome other exotic nature, on whofe account the air is fo neceffary to the fubfiftence of flame; which neceffity I have found to be greater, and lefs dependent upon the manifeft attributes of the air, than naturalifts feem to have obferved. For I have found by trials purpofely made, that a fmall flame of a lamp, though fed perhaps with a fubtil thin oyl, would in a large capacious glafs-receiver expire, for want of air, in a far lefs time than one would believe. And it will not much leffen the difficulty to alledge, that either the grofs fuliginous fmoak did in a clofe veffel ftifle the flame, or, that the preffure of the air is requifite to impel up the aliment into the wick: for, to obviate thefe objections, I have in a larger receiver imployed a very fmall wick with fuch rectified fpirit of wine, as would in the free air burn totally away; and yet, when a very fmall lamp, furnifhed (as I was faying) with a very flender wick, was made to burn, and, filled with this liquor, was put lighted into a large receiver, that little flame, though it emitted no vifible fmoak at all, would ufually expire within about one minute of an hour, and, not feldom, in a lefs time; and this, though the wick was not fo much as finged by the flame: nor indeed is a wick neceffary for the experiment, fince highly rectified fpirit of wine will in the free air flame away well without it. And indeed it feems to deferve our wonder, what that fhould be in the air, which enabling it to keep flame alive, does yet, by being confumed or depraved, fo fuddenly render the air unfit to make flame fubfift. And it feems by the fudden wafting or fpoiling of this fine fubject, whatever it be, that the bulk of it is but very fmall in proportion to the air it impregnates with its virtue. For after the extinction of the flame, the air in the receiver was not vifibly altered, and, for aught I could perceive by the ways of judging I had then at hand, the air retained either all, or at leaft far the greateft part of its elafticity, which I take to be its moft genuine and diftinguifhing property.

AND

AND this undeftroyed fpringinefs of the air feems to make the neceffity of frefh air to the life of hot animals, (few of which, as far as I can guefs after many trials, would be able to live two minutes of an hour, if they were totally and all at once deprived of air,) fuggeft a great fufpicion of fome vital fubftance, if I may fo call it, diffufed through the air, whether it be a volatile nitre, or (rather) fome yet anonimous fubftance, fydereal or fubterraneal, but not improbably of kin to that, which I lately noted to be fo neceffary to the maintenance of other flames.

I know not, whether you will think it pertinent to our prefent difcourfe, that I obferve to you, that by keeping putrifying bodies in glaffes, which by *Hermes* his feal were fecured from the contact of the external air, I have not been able to produce any infect, or other living creature, though fometimes I have kept animal fubftances, and even blood fo included, for many months, and one or two of them for a longer time; and though all thefe fubftances had a manifeft change made in their confiftence whilft they remained fealed up.

ON this occafion I fhall add an odd obfervation, that I met with in a little differtation *de admirandis Hungariæ aquis*, written by an anonymous, but ingenious nobleman of that country, where, fpeaking of the native falt, that abounds in their regions, he fays, that in the chief mine (by them called *Defienfis)* of *Tranfylvania*, there was, a few years before he writ, a great oak, like a huge beam, dug out of the middle of the falt; but, though it was fo hard, that it would not eafily be wrought upon by iron tools, yet being expofed to the air out of the mine, it became fo rotten, as he expreffes it, that in four days it was eafy to be broken, and crumbled between one's fingers. And of that corruptive or diffolutive power of air near thofe mines, the fame author mentions other inftances.

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THE ingenious Monſieur *de Rochefort*, in the handſome account he gives of the apple, or fruit of the tree junipa, whoſe juice is employed by the Indians to black their ſkins, that they may look the more terrible to their enemies, obſerves, that, though the ſtain, or, as he ſpeaks, the tincture of this fruit cannot be waſhed out with ſoap, yet, within nine or ten days, it will vaniſh of itſelf; which would make one ſuſpect, that there may be in the air ſome ſecret powerful ſubſtance, that makes it a menſtruum of more efficacy than ſoap itſelf to obliterate ſtains. I remember, I have ſeen this fruit, but not whilſt it was ſucculent enough to have a trial made with it; which I was therefore troubled at, becauſe the author does not clearly expreſs, whether this diſappearing of the tincture happens indifferently to the bodies it chances to ſtain, or only is obſerved on the ſkins of men. For, as in the former caſe, it will afford an inſtance pertinent to our preſent purpoſe; ſo in the latter I ſhould ſuſpect, that the vaniſhing of the tincture may be due, not ſo much to the operation of the air upon it, as to the ſweat and exhalations of a human body, which abounding with volatile ſalt, may either deſtroy or carry off with them, the coloured particles they meet with in their paſſages.

I have ſometimes, not altogether without wonder, obſerved the excellency of the better ſort of Damaſco-ſteel, (for I ſpeak not of all that goes under that name,) in compariſon of ordinary ſteel. And, beſides what I have elſewhere taken notice of concerning it, there is one phænomenon, which though I am not ſure it belongs to the latent qualities of the air, yet, becauſe it may well do ſo, and I am unwilling it ſhould be loſt, I will here tell you, that having enquired of an eminent and experienced artificer, (whom I long ſince employed in ſome difficult experiments,) about the properties of Damaſco-ſteel, this honeſt and ſober man averred to me, that when he made inſtruments of it, and gave them the true temper, which is ſomewhat differing from that of other ſteel, he generally obſerved, that though, when raſors or other inſtruments made of it were newly forged, they would be ſometimes no whit better, and ſometimes leſs good, than thoſe made of other ſteel; yet when they had been kept a year or two or three in the air, though nothing elſe were done to improve them, they would be found much to ſurpaſs other inſtruments of the ſame kind, and what themſelves were before; inſomuch, that ſome of them have been laid aſide at firſt, as no way anſwering the great expectation conceived of them, which after two or three years were found to ſurpaſs it; of which alſo I am now making a trial. I have ſeveral times made a ſubſtance, that conſiſts chiefly of a metalline body, and is of a texture cloſe enough to lie for many hours undiſſolved in a corroſive menſtruum; and yet this ſubſtance, that was fixed enough to endure the being melted by the fire without loſing its colour, would, when I had purpoſely expoſed it to the air, be diſcoloured in a very ſhort time, and have its ſuperficial parts turned almoſt black.

AND this brings into my mind that very pretty obſervation, that has been newly made in *Italy* by an ingenious man, who took notice, that, if after the opening of a vein, the blood be kept till it be concreted, and have excluded the ſuperficial ſerum, though the lower part be uſually of a dark and blackiſh colour, in compariſon of the ſuperficial parts, and therefore be counted far more feculent; yet, if the lump, or clot of blood be broken, and the internal, and dark coloured parts of the blood be expoſed to the air, it will after a time (for it is not ſaid how long) be wrought on by the contact of the air, that the ſuperficial part of the blood will appear as florid, as the lately mentioned upper part (ſuppoſed to be, as it were, the flower of the blood,) did ſeem before. And this obſervation I found to hold in the blood of ſome beaſts, whereon

whereon I tried it, in which I found it to succeed in much fewer minutes, than the Italian virtuoso's experiment on human blood would make me expect.

ON the other side I have often prepared a substance, whose effect appears quite contrary to this. For, though this factitious concrete, whilst kept to the fire, or very carefully preserved from the air, be of a red colour, almost like the common opacous bloodstone of the shops; yet, if I broke it, and left the lumps, or fragments of it, a little while in the air, it would in a short time (sometimes perhaps, not amounting to a quarter of an hour) it would, I say, have its superficial part turned of a dark colour, very little, and sometimes scarce at all, short of blackness.

A very inquisitive person of my acquaintance, having occasion to make, by distillation, a medicine of his own devising, chanced to observe this odd property in it, that, at that time of the year, if it were kept stopped, it would be coagulated almost like oil of anniseeds in cold weather; yet, if the stopple were taken out, and so access were for a while given to the air, it would turn to a liquor, and the vessel being again stopped, it would, though more slowly, recoagulate. The hints, that I guessed, might be given by such a phænomenon, making me desirous to know something of it more than barely by relation, I expressed rather a curiosity than diffidence about it; and the maker of it telling me, he thought, he had in a small vial about a spoonful of this medicine left in a neighbouring chamber, I desired his leave to consider it myself, which request being presently complied with, I found it, when he brought it into the room which I stayed in, not liquid but consistent, though of but a slight and soft contexture. And having taken out the cork, and set the vial in a window, which (if I well remember) was open, though the season, which was winter, was cold, yet in a little time, that I stayed talking with the chemist, I found, that the so lately coagulated substance was almost all become fluid. And another time, when the season was less cold, having occasion to be where the vial was kept well stopped, and casting my eyes on it, I perceived the included substance to be coagulated much like oil of anniseeds. And this substance having, as the maker assured me, nothing at all of mineral in it, nor any chemical salt, it consisting only of two simple bodies, the one of a vegetable, and the other of an animal substance, distilled together, I scarce doubt but you will think with me, that these contrary operations of the air, which seems to have a power in some circumstances to coagulate such a body, and yet to dissolve and make it fluid, when fresh and fresh parts are allowed access to it, may deserve to be further reflected on, in reference (among other things) to the opportune operations, the inspired air may have on the consistence and motion of the circulating blood, and to the discharge of the fuliginous recrements to be separated from the blood in its passage through the lungs.

THERE are two other phænomena, that seemed favourable to our suspicion, that there are anonymous substances and qualities in the air, which ought not to be altogether prætermitted on this occasion; though, because to speak fully of them would require far more time than I can now spare, I shall speak of them but succinctly.

THE latter of these two phænomena is the growth or appearing production of metals or minerals dug out of the earth, and exposed to the air. And this, though it be the last of the two, I mention first, because it seems expedient, lest it should prove too long an interruption to our discourse, to postpone the observations, and annex them to the end of this paper; only intimating to you now, that the caution I formerly interposed about the regeneration of salts in nitrous, and other earths, may, for greater security, be applied, *mutatis mutandis*, to that production of metalline and mineral bodies we are speaking of.

THE

THE other of the two phænomena, I lately promifed to mention, is afforded me by thofe various and odd difeafes, that at fome times, and in fome places, happen to invade, and deftroy numbers of beafts, fometimes of one particular kind, and fometimes of another. Of this we have many inftances in the books of approved authors, both phyficians and others; and I have myfelf obferved fome notable examples of it. But yet I fhould not mention it as a ground of fufpicion, that there may be, in fome times and places, unknown effluvia and powers in the air, but, that I diftinguifh thefe from thofe difeafes of animals, that proceed, as the rot in fheep often does, from the exorbitancy of the feafons, the immoderatenefs of cold, heat, or any other manifeft quality in the air. And you will eafily perceive, that fome of thefe examples probably argue, that the fubterraneal parts do fometimes (efpecially after earthquakes, or unufual cleavings of the ground) fend up into the air peculiar kinds of venomous exhalations, that produce new and mortal difeafes in animals of fuch a fpecies, and not in thofe of another, and in this or that particular place, and not elfewhere: of which we have an eminent inftance in that odd plague or murrain of the year 1514, which, *Fernelius* tells us, invaded none but cats. And even in animals of the fame fpecies, fometimes one fort have been incomparably more obnoxious to the plague than another; as *Dionyfius Halicarnaffeus* mentions a plague, that attacked none but maids; whereas, the peftilence, that raged in the time of *Gentilis* a famed phyfician, killed few women, and fcarce any but lufty men. And fo *Boterus* mentions a great plague, that affaulted almoft only the younger fort of perfons, few paft thirty years of age being attacked by it: which laft obfervation has been alfo made by feveral later phyficians. To which may be added, what learned men of that faculty have noted at feveral times concerning plagues, that particularly invaded thofe of this or that nation, though confufedly mingled with other people; as *Cardan* fpeaks of a plague at *Bafil*, with which only the Switzers, and not the Italians, French, or Germans, were infected. And *Johannes Utenhovius* takes notice of a cruel plague at *Copenhagen*, which, though it raged among the Danes, fpared both the Englifh, Dutch, and Germans, though they freely entered infected houfes, and were not careful to fhun the fick. In reciting of which inftances I would not be underftood, as if I imputed thefe effects merely to noxious fubterraneal fumes; for I am far from denying, that the peculiar conftitutions of men's bodies are likely to have a great intereft in them: but yet it feems lefs probable, that the peftilent venom diffufed through the air fhould owe its enormous and fatal efficacy to the excefs of the manifeft qualities of the air, than to the peculiar nature of the peftilential poifon fent up into the air from under-ground, which when it is by dilution or diffipation enervated, or by its progrefs paft beyond the air we breathe in, or rendered ineffectual by fubterraneal, or other corpufcles, of a contrary quality, the plague, which it, as a con-caufe, produced, either quite ceafes, or degenerates into fomewhat elfe. But I have not time to countenance this conjecture, much lefs to confider, whether fome of thofe difeafes, that are wont to be called new, which either did begin to appear, or at leaft to be rife, within thefe two or three centuries, as the *Sudor Anglicus* in the fifteenth century, the fcurvy, and the *Morbus Hungaricus*, the *Lues Moraviæ*, *Novus Morbus Luneburgenfis*, and fome others, in the laft century of all, may be in part caufed by the exotic fteams this difcourfe treats of. But this confideration I willingly refign to phyficians.

AND now, if the two forementioned fufpicions, the one about fubterraneal, the other about fidereal, effluviums, fhall prove to be well grounded, they may lead us to other fufpicions and further thoughts about things of no mean confequence; three of which I fhall venture to make mention of in this place.

I. FOR

I. FOR we may hence be awakened to confider, whether divers changes of temperature and conftitution in the air, not only as to manifeft qualities, but as to the more latent ones, may not fometimes in part, if not chiefly, be derived from the paucity or plenty, and peculiar nature of one or both of thefe forts of effluviums. And in particular, we find in the moft approved writers fuch ftrange phænomena to have feveral times happened in great plagues and contagious difeafes, fomented and communicated, nay (as many eminent phyficians believed) begun, by fome latent peftiferous, or other malignant, diathefis or conftitution of the air, as have obliged many of the learnedeft of them to have recourfe to the immediate operation of the angels, or of the power and wrath of God himfelf, or at leaft to fome unaccountable influence of the ftars; none of the folutions of which difficulties feem preferable to what may be gathered from our conjecture; fince of phyfical agents, of which we know nothing fo much, as that they are to us invifible and probably of a heteroclite nature, it need be no great wonder, that the operation fhould alfo be abftrufe, and the effects uncommon. And on this occafion it may be confidered, that there are clearer inducements to perfwade us, that another quality of the atmofphere, its gravity, may be altered by unfeen effluviums, afcending from the fubterraneous regions of our globe; and we have often perceived by the mercurial barofcope the weight of the air to be notably encreafed, when we could not perceive in the air, nor furface of earth, any caufe, to which we could afcribe fo notable a change And this gives me a rife to add, that I have fometimes allowed myfelf to doubt, whether even the fun itfelf may not now and then alter the gravity of the atmofphere otherwife than by its beams of heat. And I remember, I defired fome virtuofi of my acquaintance to affift me in the enquiry, whether any of the fpots, that appear about the fun, may not, upon their fudden diffolution, have fome of their difcuffed and difperfed matter thrown off, as far as to our atmofphere, and, that copioufly enough to produce fome fenfibly alterations in it, at leaft as to gravity.

II. ANOTHER thing, that our two forementioned fufpicions, if allowed of, will fuggeft, is, that it may not feem altogether improbable, that fome bodies, we are converfant with, may have a peculiar difpofition and fitnefs to be wrought on by, or to be affociated with, fome of thofe exotic effluvia, that are emitted by unknown bodies lodged under ground, or that proceed from this or that planet. For what we call fympathies and antipathies depending indeed on the peculiar textures and other modifications of the bodies, between whom thefe friendfhips and hoftilities are faid to be exercifed, I fee not, why it fhould be impoffible, that there be a cognation betwixt a body of a congruous or convenient texture, (efpecially as to the fhape and fize of its pores,) and the effluviums of any other body, whether fubterraneal or fidereal. We fee, that convex burning-glaffes, by virtue of their figure and the difpofition of their pores, are fitted to be pervaded by the beams of light and to refract them, and thereby to kindle combuftible matter; and the fame beams of the fun will impart a lucidnefs to the Bolonian ftone. And as for fubterraneal bodies, I elfewhere * mention two minerals, which being prepared, (as I there intimate,) the fteams of the one, afcending without adventitious heat, and wandering through the air, will not fenfibly work on other bodies; but if they meet with that, which we prepared, they will immediately have an operation on it, whofe effect will be both manifeft and lafting.

III. I now pafs on to the other thing, that the two formerly mentioned fufpicions may fuggeft, which is, that if they be granted to be well founded, we may be allowed to confider, whether among the bodies we are acquainted with here below, there may not be found fome, that may be receptacles, if not alfo attractives, of the fidereal, and other exotic effluviums, that rove up and down in our air.

* See the Experiment in the difcourfe of the Determinate Nature of Effluviums.

SOME

Some of the myſterious writers about the philoſophers-ſtone ſpeak great things of the excellency of what they call their philoſophical magnet, which, they ſeem to ſay, attracts and (in their phraſe) corporifies the univerſal ſpirit, or (as ſome ſpeak) the ſpirit of the world. But theſe things being abſtruſities, which the writers of them profeſſed to be written for, and to be underſtood only by the ſons of art; I, who freely acknowledge I cannot clearly apprehend them, ſhall leave them in their own worth as I found them, and only, for brevity ſake, make uſe of the received word of a magnet, which I may do in my own ſenſe, without avowing the received doctrine of attraction. For by ſuch a magnet as I here propoſe to ſpeak of, I mean not a body, that can properly attract our foreign effluviums; but ſuch an one, as is fitted to detain and join with them, when by virtue of the various motions, that belong to the air as a fluid, they happened to accoſt the magnet. Which may be illuſtrated by the known way of making oil of tartar (as the chemiſts call it) *per deliquium*. For, though the ſpagyriſts and others ſuppoſe, that the fiery ſalts draw to it the aqueous vapours, yet indeed it does but arreſt, and embody with ſuch of thoſe, that wander through the air, as chance in their paſſage to accoſt it.

And, without receding from the Corpuſcularian principles, we may allow ſome of the bodies, we ſpeak of, a greater reſemblance to magnets, than what I have been mentioning. For not only ſuch a magnet may upon the bare account of adheſion by juxta-poſition, or contact, detain the effluviums, that would glide along it, but theſe may be the more firmly arreſted by a kind of precipitating faculty, that the magnet may have in reference to ſuch effluviums; which, if I had time, I could illuſtrate by ſome inſtances; nay, I dare not deny it to be poſſible, but, that in ſome circumſtances of time, or place, one of our magnets may, as it were, fetch in ſuch ſteams, as would indeed paſs near it, but would not otherwiſe come to touch it. On which occaſion I remember, I have in certain caſes been able to make ſome bodies, not all of them electrical, attract (as they ſpeak) without being excited by rubbing, &c. far leſs light bodies, than the effluviums we are ſpeaking of.

But this it may ſuffice to have glanced at, it not being here my purpoſe to meddle with the myſtical theories of the chemiſts; but rather to intimate, that, without adopting, or rejecting them, one may diſcourſe like a naturaliſt about magnets of celeſtial, and other emanations, that appear not to have been conſidered, not to ſay thought of, either by the ſcholaſtick, or even the mechanical philoſophers.

Of CELESTIAL and AERIAL MAGNETS.

If now, upon what I have granted in the cloſe of the paſt diſcourſe, you ſhould urge the queſtion further, and preſs me to declare, whether, as I think it no impoſſible thing, that nature ſhould make, ſo I think it no unpracticable or hopeleſs thing, that men ſhould find, or art ſhould prepare, uſeful magnets of the exotic effluviums of the lower region of the earth, or the upper of the world: it would much diſtreſs me to give any other anſwer, than that I think it extremely difficult, and not abſolutely impoſſible; and therefore I would not diſcourage any curious or induſtrious man from attempting to ſatisfy himſelf by experiments, becauſe even a ſeemingly ſlight diſcovery in a thing of this nature may be of no ſmall uſe in the inveſtigation of the nature of the air, eſpecially in ſome particular places, and of the correſpondency, which, by the intervention of the air, the ſuperficial part of the terreſtrial globe may have both with the ſubterraneal regions of the earth, and the celeſtial ones of the univerſe. Some of the things I have tried or ſeen relating to this diſcovery, I muſt, for certain reaſons, leave here unmentioned; and only advertiſe you, that ſeveral bodies, which experience has aſſured us do imbibe or retain ſomething from the air, as ſome calcined minerals, ſome marchaſites, ſome ſalts, as well factitious as natural, &c. may be fit to be often expoſed to it, and then weighed again, and further diligently examined, whether that, which makes the increment of weight, be a mere imbibed moiſture, or alſo ſomewhat elſe; and likewiſe, whether it be ſeparable from the body or not, or however have endowed it with any conſiderably quality; and if you chance to meet with a good magnet, you may then vary experiments with it, by expoſing it long to the air in regions differing much in climate, or ſoil, or both, by expoſing it by day only, or by night, at ſeveral ſeaſons of the year, in ſeveral temperatures of the air, at ſeveral conſiderable aſpects of the ſtars and planets, by making it more or leſs frequently part with what it has gained from the air; and in ſhort, by having regard to variety of circumſtances, which your curioſity and ſagacity may ſuggeſt. For, by thus diverſifying the experiment many ways, you may perhaps, by one or other of them, make ſome unexpected and yet important diſcovery of what effluviums the air, in particular places and times, abounds with, or wants, and perchance too, of ſome correſpondency between the terreſtrial and etherial globes of the world.

I ſhall neither be ſurprized, nor quarrel with you, if you tell me, that theſe are extravagant thoughts; but if I had been fortunate in preſerving all, that trial, obſervation, or other productions of ſome curioſity, I once had for ſuch enquiries, procured me, you would not, perhaps, think me ſo very extravagant. But though I muſt not here make any further mention of them, and ſhall only take notice of one body, namely vitriol, whether crude, or unripe, and (as chemiſts ſpeak) embrionated, or ſpagyrically prepared; yet ſome phænomena of theſe vitriolate ſubſtances may for the preſent, I hope, ſomewhat moderate your cenſure for my putting you upon obſervations, that I fear you yourſelf will judge unpromiſing, and leſs favourable perſons than you, would think phantaſtical. And to let you ſee by a pregnant inſtance, that the air may not only have a notable operation upon vitriol, and that, after a ſtrong fire could work no farther on it, but that this operation was conſiderably diverſified by circumſtances; I ſhall begin what I have to alledge, with what the experienced *Zwelſer* occaſionally obſerved, and relates to uſher in a caution about a chemical preparation of vitriol: For, having informed his reader, that the colcothar, that is made by a ſtrong diſtillation, is not corroſive, he denies, that (to uſe his own words) *ſtatim à diſtillatione ſal ex eodem, affusâ aquâ, elici queat*; *ſed tum prius*, (continues he,) *ubi aliquandiu aeri expoſitum fuerit*; *tunc enim ſal præbet quandoque candidum, quandoque purpureum, aſpectu pulcherrimum, quod aliquando in copia acquiſivi, & penes me aſſervo, quandoque etiam nitroſum.*

WHICH teſtimony of this candid ſpagyriſt has much the more weight with me, becauſe I find, what he affirms of the ſaltleſneſs of newly and ſtrongly calcined vitriol to be very agreeable to ſome of my experiments about colcothar of blue (venereal) vitriol; which ſalt or mineral (I mean vitriol) is ſo odd a concrete, that I have thought fit more than once to recommend the making experiments about it to ſeveral curious perſons, that had better opportunity to continue them than I, whoſe reſidence was not ſo fixed. And I remember, that one of theſe, a perſon induſtrious and verſed in chemical operations, gave me this account, that not only he had differing kinds of ſalts from colcothar expoſed to the air for many months, and robbed at convenient times of what it had acquired, but that in tract of time he found it ſo altered, that he obtained from it a pretty quantity of true running mercury.

AND now, to refume and conclude what I was faying about colcothar, there are two or three things I would propofe to be obferved by you, or any virtuofo, that would affift me in thefe trials about this odd *calcinatum*, (for to call it *terra damnata* were to injure it.)

THE firft is, to take notice of fome circumftances, that moft obfervers would overlook; fuch as (befides the nature of the foil) the temperature of the air, the month of of the year, and the winds, the weight of the atmofphere, the fpots of the fun, if any be, the moon's age, and her place in the Zodiac, and the principal afpects of the planets, and the other chief ftars. For, though it be a boldnefs to affirm, that any, or perhaps all of thefe together, will have any intereft in the production of the falt or other fubftance, to be made or difclofed in the colcothar; yet in things new and exorbitant, it may be fometimes rafh and peremptory to deny, even fuch things as cannot, without rafhnefs, be pofitively afferted; and in our cafe, the fmall trouble of taking notice of circumftances will be richly paid by the leaft difcovery made in things fo abftrufe and confiderable. And as we cannot yet knowingly pronounce, fo much as negatively, whether the libration of the moon, and the motion of the fun (and perhaps of fome of the other planets) about their own centers, and confequently their obverting feveral parts of their bodies to us, may have an operation upon our atmofphere; fo, for aught I know, there may be in thofe vaft internal parts of the earth, whofe thin cruft only has been here and there dug into by men, confiderable maffes of matter, that may have periodical revolutions, or accenfions, or euftations, or fermentations, or, in fhort, fome other notable commotions, whofe effluvia and effects may have operations, yet unobferved, on the atmofphere, and on fome particular bodies expofed to it; though thefe periods may be perhaps either altogether irregular, or have fome kind of regularity differing from what one would expect. As we fee, that the fea has thofe grand intumefcencies, we call fpring tides, not every day, nor at any conftant day of the month or week, but about the full and new moon; and thefe fpring tides are moft notably heightened, not every month, but twice a year, at or about the vernal and autumnal equinoxes; which obfervations have not been near fo ancient and known, as the daily ebbing and flowing of the fea. The Etefians of the ancients I fhall not now infift on, nor the obfervations, that I think I elfewhere mentioned of the elder inhabitants of the *Caribbee iflands*, who, when the Europeans firft reforted thither, were wont to have hurricanes but once in feven years; afterwards they were molefted with them but once in three years; and of late they are troubled with them almoft every year. And a phyfician, that lived there, told me, that he had fcarce ever obferved them to come but within the compafs of two months joining to one another. In which inftances, and divers others, that may be noted of what changes happened to great quantities of matter, nature feems to affect fomething of periodical, but not in a way, that appears too regular.

ONE may add on this occafion, that memorable paffage related by the learned *Varenius** of thofe hot fprings in *Germany*, that he calls *Thermæ Piperinæ*, of which he affirms in more than one place, that they have this peculiarity, that they annually begin and ceafe to flow at certain times; the former about the third of *May*, and the latter near the middle of *September*, at which time they are wont to reft till the following fpring. But though, for aught I know, our geographer's obfervation will hold in hot fpring; yet it muft not be extended to all, at leaft, if we admit that, which is related by the accurate *Johannes de Laet*, (I fuppofe out of *Ximenes*, or the famous conqueror of *Mexico*, *Cortes*,) who tells us, that in the Mexican province, *Xilotepec fons celebratur qui quatuor continuis annis fcaturit, deinde quatuor fequentibus deficit, & rur-* *Amer, lib. V. cap. 7.*

* *Varenius, lib. 1. Geograph. Univerfc. Thermæ omnes ferè quas novimus fine ceffatione fluunt, exceptis Piperinis Germaniæ, &c.*

fus

ſus ad priorem modum erumpit, &, quod mirabile, pluviis diebus, parcius, quum ſudum eſt tempus & aridum, copioſius, exuberat.

BUT this is not a place to enlarge upon the grounds of my ſuſpecting, there may be ſome periodical motions and commotions within the terreſtrial globe; what has been mentioned being only to invite you to take notice of circumſtances in your obſervations of colcothar, ſome of which may, with the more ſhew of probability, be kept expoſed for a long time, becauſe that bars of windows, and other erected irons, I have found to acquire, in tract of time, from the effluvia of the earth, a ſettled magnetiſm.

THE other main thing I would recommend, is, that notice be taken not only of the kind of vitriol, the colcothar is made of; (for I generally uſed blue Dantzick vitriol) as martial vitriol, Hungarian vitriol, Roman vitriol, &c. to which I have, for curioſity, added vitriol made by ourſelves of the ſolution of the more ſaline parts of marchaſites in water, without the uſual additament of iron, or copper; but alſo, to what degree the calcination is made, and how far the calcined matter is freed from the ſalt by water. For theſe circumſtances, at leaſt in ſome places, may be of moment, and perhaps may afford us good hints of the conſtitution of the atmoſphere in particular parts, as well as of the beſt preparation of colcothar for detaining the exotic effluviums. And I would the rather have experiments tried again in other places with colcothar not calcined to the utmoſt, nor yet ſo exquiſitely edulcorated, but that ſome ſaline particles ſhould be left in it for future encreaſe; becauſe I have more than once purpoſely tried in vain, that the *Caput Mortuum* of blue vitriol, whereof the oil and other parts had been driven off with a violent and laſting fire, would not, when freſh, impart any ſaltneſs to the water; nor do I think, that out of ſome ounces purpoſely edulcorated I obtained one grain of ſalt. And this ſaltleſs colcothar being expoſed, ſome by me, and ſome by a friend, that had conveniency in another place not far off, to the air, ſome for many weeks and ſome for divers months, we did not find it to have manifeſtly encreaſed in weight, or to have acquired any ſenſible ſaltneſs, which, ſuppoſing the vitriol to have nothing extraordinary, gave me the ſtronger ſuſpicion of ſome peculiarity in the air of that part of *London*, where the trials had been made, at leaſt, during thoſe times, wherein we made them; becauſe not only former experience, made here in *England*, had aſſured me, that ſome colcothars will gain no deſpicable acceſſion of weight by being expoſed to the air; but accidentally complaining of my lately mentioned diſappointment to an ingenious traveller, that had, in divers countries, been curious to examine their vitriols, he aſſured me, that, though he uſually dulcified his colcothar very well, yet within four or five weeks he found it conſiderably impregnated by the air it was expoſed to.

IT remains, that I add one intimation more about vitriol, which is, that I have found it to have ſo great a correſpondency with the air, that it would not be amiſs to try, not only colcothar of differing vitriols (whether barely made the common way, or without any metalline addition to the vitriol ſtones or ore,) but other preparations of vitriol too, ſuch as expoſing vitriol, only calcined to whiteneſs by the ſun-beams, or further to an higher colour by a gentle heat, or throughly calcined, and then impregnated with a little of its own oil. For ſuch vitriolate ſubſtances as theſe the air may work upon, nay even liquid preparations of vitriol may be peculiarly affected by the air, and thereby perhaps be uſeful to diſcover the preſent conſtitution, or foretel ſome approaching changes of it. Of the uſe of which conjecture, namely the peculiar action of the air on ſome vitriolate liquors, I remember I ſhewed ſome virtuoſi a new inſtance in an experiment, whereof this was the ſum:

[I elſewhere mention a compoſition, that I deviſed, to make with ſublimate, copper, and ſpirit of ſalt, a liquor of a green exceeding lovely. But in the deſcription of it I

 mentioned

mentioned not (having no need to do it there) a circumſtance as odd, as the liquor itſelf was grateful. For the air has ſo much intereſt in the production of this green, that when you have made the ſolution of the copper and mercury with the ſpirit of ſalt, that ſolution will not be green, nor ſo much as greeniſh, as long as you keep it ſtopped in the bolt-head, or ſuch like glaſs, wherein it is made. But if you pour it out into a vial, which, by not being ſtopped, leaves it expoſed to the air, it will after a while ſooner or later attain that delightful green, that ſo much endears it to the beholder's eye. This appeared ſo odd an experiment to the virtuoſi, to whom I firſt related it, that thoſe, that could not gueſs by what means I attained it, could ſcarce believe it. But that troubled not me, who, to ſatisfy myſelf not only of the truth of the experiment, but that it was not ſo contingent as many others, repeated it ſeveral times, and found the ſolution, till the air made it floriſh, to be of a muddy reddiſh colour, quite differing from green. So that I remember, that having once kept ſome of the liquor in the ſame glaſs egg, wherein the ſolution had been made, it looked like very dirty water, whilſt the other part of the ſame ſolution, having been expoſed to the air, emulated the colour of an emerald. In which change it is remarkable, that to clarify this liquor and give it a tranſparent greenneſs, I perceived not, that any precipitation of foul matter was made, to which the alteration could be aſcribed; and yet to make it the more probable, that this change proceeded not from a ſubſidence made of ſome opacating matter effected by ſome days reſt, I kept ſome of the ſolution ſealed up in a fine vial ſeveral months, without finding it at the end of that time other than a dark or muddy liquor, which in ſhort time it ceaſed to be, when, the hermetic ſeal being broken off, the air was permitted to work upon it. And this I further obſerved in our various experiments on this liqnor, that, according to the quality of the matter and other circumſtances, the greenneſs was not attained to but at certain periods of time, now and then diſcloſing itſelf within two or three days, and ſometimes not before nine or ten.]

WITH how little confidence of ſucceſs trials, that have the aims of theſe I have been ſpeaking of, are to be attempted, not only conſideration but experience have made me ſenſible. But yet I would not diſcourage men's curioſity from venturing even upon ſlight probabilities, where the nobleneſs of the ſubjects and ſcope may make even ſmall attainments very deſirable. And till trial have been made on occaſions of great moment, it is not eaſy to be ſatisfied, that men have not been wanting to themſelves; which I ſhall only illuſtrate by propoſing what, I preſume, will not need, that I ſhould make an application of it. Thoſe adventurous navigators, that have made voyages for diſcovery in unknown ſeas, when they firſt diſcerned ſomething obſcure near the horizon, at a great diſtance off, have often doubted, whether what they had ſo imperfect a ſight of, were a cloud, or an iſland, or a mountain: but though ſometimes it were more likely to be the former, as that, which more frequently occurred, than the latter; yet they judged it adviſable to ſteer towards it, till they had a clearer proſpect of it: for if it were a deluding meteor, they would not however ſuſtain ſo great a loſs in that of a little labour, as, in caſe it were a country, they would in the loſs of what might prove a rich diſcovery: and if they deſiſted too ſoon from their curioſity, they could not rationally ſatisfy themſelves, whether they ſlighted a cloud, or neglected a country.

Some

Some additional EXPERIMENTS

RELATING TO THE

SUSPICIONS about the HIDDEN QUALITIES of the AIR.

THE eſſay about Suſpicions of ſome Hidden Qualities of the Air having been detained ſomewhat long at the preſs, that it might come abroad accompanied with the other tracts deſigned to attend it, whilſt I was rumaging among ſeveral papers to look for ſome other things, I met now and then with an experiment or obſervation, that ſeemed to relate to ſome of the things delivered in that tract; and though they be in themſelves of no great moment, I am content to annex them to the reſt, becauſe, as in that company they may ſignify ſomewhat, ſo I am unwilling, that any matter of fact, relating to ſuch a ſubject, ſhould periſh to ſave the labour of tranſcribing.

EXPERIMENT I.

HAVING occaſion to dulcify ſome calx of Dantzick vitriol, from which the oil had been a good while before diſtilled; water was put upon two large portions of it, that the liquor might be impregnated with the vitriolate particles remaining in the calx; the water put upon one of theſe portions was, ſoon after it was ſufficiently impregnated, filtrated and gently abſtracted, by which means it afforded many drachms of a kind of ſalt of vitriol, that ſeemed to differ very little from the vitriol, that had been calcined: but the water, that was put upon the other portion of calcined vitriol, was in a wide-mouthed veſſel left in the air for a month or ſix weeks; after which time, when it came to be abſtracted after the manner formerly recited, it afforded many drachms of a ſalt, that did not then, nor long after, look at all like common vitriol, or like the other, but ſhot white almoſt like ſalt-petre, or ſome other untincted ſalt. Whether this experiment will conſtantly ſucceed, and at other ſeaſons of the year than that it was made in, which was ſummer, I had not the opportunity to make a full trial, though I endeavoured it. But that the air may have a great ſtroke in varying the ſalts obtainable from calcined vitriol, ſeemed the more probable, becauſe we had ſome colcothar, that had lain many months, if not ſome years, in the air, but in a place ſheltered from the rain; and having cauſed a lixivium to be made of it, to try what ſort or plenty of ſaline particles it would yield, we found, when the ſuperfluous moiſture was exhaled, that they began to ſhoot into ſalt far more white than vitriol, and very differing from it in its figure and way of concretion.

EXPERIMENT II.

WE took colcothar of venereal vitriol carefully dulcified, and leaving it in my ſtudy in the months of *January* and *February*, by weighing it carefully before an ounce of it was expoſed to the air, and after it had continued there ſome weeks, we found it to have encreaſed in weight four grains and about a quarter, beſides ſome little duſt, that ſtuck to the glaſs. This was made at Oxford.

THIS ſlight experiment is here mentioned, that, being compared with the next enſuing trial, it may appear, that the difference of airs, ſeaſons, calces of vitriol, or other circumſtances, may produce a notable diſparity in the increment of weight, the expoſed bodies gain in the air.

EXPERIMENT III.

We put eight ounces of outlandiſh vitriol, calcined to a deep redneſs, into a ſomewhat broad and flat metalline veſſel, and ſet it by upon a ſhelf, in a ſtudy, that was ſeldom frequented; and at the ſame time, that we might obſerve what increment would be gained by expoſing to the air a larger ſuperficies of the powder, in reference to the bulk, we put into another metalline veſſel, ſmaller than the other, only two ounces of colcothar, and ſet it on the ſame ſhelf with the other, this was done at the vernal equinox, (the twelfth of *March*;) on the twenty-fifth of *June* we weighed theſe powders again, and found the eight ounces to have gained one drachm and ſeventeen grains; but the two ounces had acquired the ſame weight within a grain: then putting them back into their former veſſels, we left them in the ſame place as formerly, till the twenty-fourth of *Auguſt*, when we found cauſe to ſuppoſe, that the greater parcel of colcothar had met with ſome miſchance, either by mice or otherwiſe; but the leſſer parcel weighed twenty-ſix grains heavier than it did in *June*, amounting now to two ounces, one dram, forty-two grains, having increaſed, in leſs than ſix months, above an hundred grains, and conſequently above a tenth part of its firſt weight.

No trial was made to diſcover, what this acquired ſubſtance may be, that we might not diſturb the intended proſecution of the experiment.

EXPERIMENT IV.

Because in moſt of the experiments of ſubſtances expoſed to be impregnated by the air, or detain its ſaline or other exotic particles, we employed bodies prepared and much altered by the previous operation of the fire; we thought fit to make ſome trials with bodies unchanged by the fire; and to this purpoſe we took a marchaſite, which was partly of a ſhining and partly of a darkiſh colour, and which ſeemed well-diſpoſed to afford vitriol: of this we took ſeveral ſmaller lumps, that amounted to two ounces; theſe were kept in a room, where they were freely acceſſible to the air, which, by reaſon that the houſe, that was ſeated in the country, ſtood high, was eſteemed to be very pure. After the marchaſites had been kept in this room ſomewhat leſs than ſeven weeks, we weighed them again in the ſame balance, and found the two ounces to have gained above twelve grains in weight.

EXPERIMENT V.

The experiment uſed at the latter end of our paper, about celeſtial and aerial magnets, ſeeming to ſome virtuoſi very ſtrange, and the way, that I employed in making that liquor, that turns green in the air, being ſomewhat troubleſome, I remember I thought fit to try, upon the ſame ground, a way of producing the ſame phænomenon more eaſy and more expeditious. And though perhaps this way will not ſucceed ſo conſtantly, nor always ſo well as the other, yet, for its eaſineſs and cheapneſs, it will not probably be unwelcome to thoſe, that are deſirous to ſee the odd phænomenon.

We took then, more than once, filings of clean crude copper, and having put on them a convenient quantity of good ſpirit of ſalt, we ſuffered the menſtruum in heat (which need not be very great) to work upon the metal, which it uſually does ſlowly, and not like aqua fortis. When the liquor had by this operation acquired a thick and muddy colour, we decanted it into a clean glaſs with a wide mouth, which being left for a competent time in the open air, the expoſed liquor came to be of a fair green, though it did not appear, that any thing was precipitated at the bottom, to make it clear.

EXPE-

EXPERIMENT VI.

PERHAPS it may not be impertinent to add, that I once or twice obſerved the fumes of a ſharp liquor to work more quickly or manifeſtly on a certain metal ſuſtained in the air, than did the menſtruum itſelf, that emitted thoſe fumes on thoſe parts of the metal, that it covered. And this brings into my mind, that, aſking divers queſtions of a chemiſt, that had been in *Hungary*, and other parts, purpoſely to ſee the mines; he anſwered me, among other things, that, as to the ladders and other wooden work employed in one or more of the deep Hungarian mines, thoſe, that were in the upper part of the grooves, any thing near the external air, would, by the fretting exhalations, be rendered unſerviceable, in not many months; whereas thoſe ladders, and pieces of timber, &c. that were employed in the lower part of the mine, would hold good for two or three times as long.

EXPERIMENT VII.

WE took, about the bigneſs of a nutmeg, of a certain ſoft but conſiſtent body, that we had cauſed to be chemically prepared, and which, in the free air, would continually emit a thick ſmoke: this being put into a vial, and placed in a middle-ſized receiver in our engine, continued for ſome time to afford manifeſt fumes, whilſt the exhauſtion was making; till at length, the air having been more and more pumped out, the viſible aſcenſion of fumes out of the vial quite ceaſed; and the matter having remained ſome time in this ſtate, the ſmoking ſubſtance was ſo altered, that it would not emit fumes, not only when the air was let into the receiver, but not in a pretty while after the vial was taken out of it, till it had been removed to the window, where the wind blowing in freſh and freſh air, it began to ſmoke as formerly.

THE other phænomena of this experiment belong not to this place; but there are two, which will not be impertinent here, and the latter of them may deſerve a ſerious reflection.

THE firſt of them was, that the ſubſtance hitherto mentioned had been kept in a large glaſs, wherein it had been diſtilled at leaſt five or ſix weeks, and yet would ſmoke very plentifully upon the contact of the air, and be kept from ſmoking, though the chemical receiver were ſtopped but with a piece of paper.

THE ſecond was, that, when the vial was put unſtopped in the receiver, and the receiver cloſe luted on, though no exhauſtion were made, yet the white fumes did very quickly ceaſe to aſcend into the receiver, as if this ſmoke participated of the nature of flame, and preſently glutted the air, or otherwiſe made it unfit (and yet without diminution of its gravity) to raiſe the body, that ſhould aſcend.

ANIMAD-

ANIMADVERSIONS

UPON

Mr. HOBBES's PROBLEMATA DE VACUO.

PREFACE.

UPON the coming abroad of Mr. *Hobbes*'s *Problemata Phyſica*, finding them in the hands of an ingenious perſon, that intended to write a cenſure of them, which ſeveral employments, private and publick, have it ſeems, hindered him to do, I began, as is uſual on ſuch occaſions, to turn over the leaves of the book, to ſee what particular things it treated of. This I had not long done, before I found, by obvious paſſages in the third chapter, or dialogue, as well as by the title, which was *Problemata de Vacuo*, that I was particularly concerned in it; upon which I deſired the poſſeſſor of the book, who readily conſented, to leave me to examine that dialogue, on which condition I would leave him to deal with all the reſt of the book. Nor did I look upon the reflections I meant to make, as repugnant to the reſolutions I had taken againſt writing books of controverſy, ſince the explications, Mr. *Hobbes* gave of his problems, ſeemed to contain but ſome variations of, or an appendix to, his tract *De Natura Aeris*, which, being one of the two firſt pieces that were publiſhed againſt what I had written, was one of thoſe, that I had expreſly reſerved myſelf the liberty to anſwer. But the animadverſions I firſt made upon Mr. *Hobbes*'s Problems *de Vacuo*, having been caſually miſlaid ere they were finiſhed; before I had occaſion to reſume my taſk, there paſſed time euough to let me perceive, that his doctrine, which it will eaſily be thought, that the vacuiſts diſapproved, was not much reliſhed by moſt of the Pleniſts themſelves, the moderneſt Peripateticks, and the Carteſians; each of them maintaining the fullneſs of the world, upon their own grounds, which are differing enough from thoſe of our author, the natural indiſpoſition I have to polemical diſcourſes, eaſily perſuaded me to let alone a controverſy, that did appear needful. And I had ſtill perſiſted in my ſilence, if Mr. *Hobbes* had not, as it were, ſummoned me to break it by publiſhing again his explications, which in my Examen of his dialogue *De Natura Aeris*, I had ſhewn to be erroneous.

AND I did not grow at all more ſatisfied, to find him ſo conſtant, as well as ſtiff an adverſary to interſperſed vacuities, by comparing what he maintains in his dialogue *De Vacuo*, with ſome things, that he teaches, eſpecially concerning God, the cauſe of motion, and the imperviouſneſs of glaſs, in ſome other of his writings, that are publiſhed in the ſame volume with it. For ſince he aſſerts, that there is a God, and owns Him to be the Creator of the World; and ſince, on the other ſide, the penetration of dimenſions is confeſſed to be impoſſible, and he denies, that there is any vacuum in the univerſe; it ſeems difficult to conceive, how in a world, that is already perfectly full of body, a corporeal Deity ſuch as he maintains in his *Append. ad Leviath. cap.* 3. can have that acceſs, even to the minute parts of the mundane matter, that ſeems requiſite to the attributes and operations, that belong to the Deity, in reference to the world. But I leave divines to conſider, what influence the conjunction of Mr. *Hobbes*'s two opinions, the corporeity of the Deity, and the perfect plenitude of the world, may have on theology. And perhaps, I ſhould not in a phyſical diſcourſe have taken any notice of the propoſed difficulty, but that, to prevent an imputation on the ſtudy of nature's nature's works, (as if it taught us rather to degrade, than admire their author,) it ſeemed not amiſs to hint (*in tranſitu*) that Mr. *Hobbes*'s groſs conception of a corporeal God is not only unwarranted by ſound philoſophy, but ill-befriended even by his own.

MY adverſary having propoſed his problems by way of dialogue between *A.* and *B*; it will not I preſume, be wondered at, that I have given the ſame form to my animadverſions; which come forth no earlier, becauſe I had divers other treatiſes, that I was more concerned for, to publiſh before them.

BUT, becauſe it will probably be demanded, why, in a tract, that is but ſhort, my animadverſions ſhould take up ſo much room? it will be requiſite, that I here give an account of the bulk of this treatiſe.

AND firſt, having found, that there was not any one problem, in whoſe explication, as propoſed by Mr. *Hobbes*, I ſaw cauſe to acquieſce, I was induced, for the reader's eaſe, and that I might be ſure to do my adverſary no wrong, to tranſcribe his whole dialogue, bating ſome few tranſitions, and other clauſes not needful to be transferred hither,

NEXT, I was not willing to imitate Mr. *Hobbes*, who recites in the dialogue we are conſidering the ſame experiments, that he had already mentioned in his tract *De Natura Aeris*, without adding as his own (that I remember) any new one to them. But my unwillingneſs to tire the reader with bare repetitions of the arguments I employed in my Examen of that tract, invited me to endeavour to make him ſome amends for the exerciſe of his patience by inſerting, as occaſion was offered, five or ſix new experiments, that will not perhaps be ſo eaſily made by every reader, that will be able (now that I have perſpicuouſly propoſed them) to underſtand them.

AND laſtly, ſince Mr. *Hobbes* has not been content to manage himſelf and his way of treating of phyſical matters, but has been pleaſed to ſpeak very ſlightingly of experimentarian philoſophers (as he ſtiles them) in general, and, which is worſe, to diſparage the making of elaborate experiments; * I judged the thing, he ſeemed to aim at, ſo prejudicial to true and uſeful philoſophy, that I thought it might do ſome ſervice to the leſs knowing, and leſs wary ſort of readers, if I tried to make his own explications enervate his authority, and by a ſomewhat particular Examen of the ſolutions he has given of the problems I am concerned in, ſhew, that it is much more eaſy to undervalue a frequent recourſe to experiments, than truly to explicate the phænomena of nature without them. And ſince our author, ſpeaking of his Problemata Phyſica, (which is but a ſmall book) ſcruples not to tell his majeſty, to whom he dedicates them, that he has therein compriſed (to ſpeak in his own terms) the greateſt and moſt probable part of his phyſical meditations; and ſince by the alterations, he has made in what he formerly writ about the phænomena of my engine, he ſeems to have deſigned to give it a more advantageous form; I conceive, that by theſe ſelected ſolutions of his, one may, without doing him the leaſt injuſtice, make an eſtimate of his

* *Credo*, (ſays Mr. *Hobbes* in his *Dialogus Phyſicus*:) *Nam motus hic Reſtitutionis Hobbii eſt, & ab illo primo & ſolo explicatus in Lib. de corpore, cap.* 21. *Art.* 1. *Sine qua Hypotheſi, quantuſcunque labor, ars, ſumptus, ad rerum Naturalium inviſibiles cauſas inveniendas adhibeatur, fruſtra erit.* And ſpeaking of the Gentlemen (to whom it were not here proper for me to give Epithets) that uſed to meet at *Greſham-College*, and are known by the Name of the *Royal Society*, he thus treats them and their way of inquiring into Nature: *Conveniant, ſtudia conferant, Experimenta faciant quantum volunt, niſi & Principiis utantur meis, nihil proficient.*

A. *Fateris ergo nihil hactenus à Collegis tuis promotam eſſe ſcientiam Cauſarum Naturalium, niſi quod unus eorum Machinam invenerit, quâ motus excitari Aeris poſſit talis, ut partes Sphæræ ſimul undiquaque tendant ad Centrum, & ut Hypotheſes Hobbianæ, antè quidem ſatis probabiles, hinc reddantur probabiliores.*

B. *Nec fateri pudet, nam eſt aliquid prodire tenus, ſi non datur altra.*

A *Quid tenus? quorſum autem tantus apparatus & ſumptus Machinarum factu difficilium, ut catenus tantum prodiretis, quantum ante prodierat Hobbius? Cur non inde potiùs incepiſtis, ubi ille deſiit? Cur principiis ab illo poſitis non eſtis uſi? Cumque Ariſtoteles recte dixit, ignorato motu ignorari Naturam, &c.*

—— *Ad Cauſas autem, propter quas proficere ne paululum quidem potuiſtis, nec poteritis, accedant etiam aliæ, ut odium Hobbii, &c.*

way of discoursing about natural things. And though I would not interest the credit of experimentarian philosophers in no considerabler a paper than this; yet if Mr. *Hobbes*'s explication and mine be attentively compared, it will not, I hope, by them be found, that the way of philosophising he employs is much to be preferred before that, which he undervalues.

ANIMADVERSIONS, &c.

A. MAY, one, without too bold an inquisitiveness, ask, what book you are reading so attentively?

B. You will easily believe you may, when I shall have answered you, that it was Mr. *Hobbes*'s lately published tract of physical problems, which I was perusing.

A. WHAT progress have you made in it?

B. I was finishing the third dialogue or chapter, when you came in, and finding myself, though not named, yet particularly concerned, I was perusing it with that attention, which it seems you take notice of.

A. DIVERS of your experiments are so expressly mentioned there, that one need not be skilled in decyphering to perceive, that you are interested in that chapter; and therefore seeing you have heedfully read it over, pray give me leave to ask your judgment, both of Mr. *Hobbes*'s opinion, and his reasonings about a vacuum.

B. CONCERNING his opinion, I am sorry I cannot now satisfy your curiosity, having long since taken, and ever since kept a resolution to decline, at least, until a time, that is not yet come, the declaring myself either for or against the Plenists. But as to the other part of your question, which is about Mr. *Hobbes*'s arguments for the absolute plenitude of the world, I shall not scruple readily to answer, that his ratiocinations seem to me far short of that cogency, which the noise he would make in the world, and the way, wherein he treats both ancient and modern philosophers, that dissent from him, may warrant us to expect.

A. You will allow me the freedom to tell you, that, to convince me, that your resentment of his explicating divers of the phænomena of your pneumatick engine otherwise than you have been wont to do, (and perhaps in terms, that might well have been more civil) has had no share in dictating this judgment of yours; the best way will be, that entering for a while into the party of the vacuists, you answer the arguments he alledges in his chapter to confute them.

B. HAVING always, as you know, forborn to declare myself either way in this controversy, I shall not tye myself strictly to the principles and notions of the vacuists, nor, though but for a while, oppose myself to those of the Plenists: but so far I shall comply with your commands, as either upon the doctrine of the vacuists, or upon other grounds, to consider, whether this dialogue of Mr. *Hobbes* have cogently proved his, and the schools assertion, *Non dari vacuum*; and whether he has rightly explained some phænomena of nature, which he undertakes to give an account of, and especially some produced in our engine, whereof he takes upon him to render the genuine causes. And this last inquiry is that, which I chiefly design.

A. BY this I perceive, that if you can make out your own explications of your adversary's problems *de vacuo*, and shew them to be preferable to his, you will think you have done your work; and that it is but your secondary scope to shew, that in Mr. *Hobbes*'s way of solving them, he gives the vacuists an advantage against him, though not against the Plenists in general.

B. You

B. YOU do not miſtake my meaning, and therefore without any further preamble, let us now proceeed to the particular phænomena conſidered by Mr. *Hobbes*; the firſt of which is an experiment propoſed by me in the one and thirtieth of the phyſico-mechanical experiments concerning the adheſion of two flat and poliſhed marbles, which I endeavoured to ſolve by the preſſure of the air. And this experiment Mr. *Hobbes* thinks ſo convincing an one to prove the plenitude of the world, that, though he tells us he has many cogent arguments to make it out, yet he mentions but this one, becauſe that, he ſays, ſuffices.

A. THE confidence he thereby expreſſes of the great force of the argument does the leſs move me, becauſe I remember, that formerly in his elements of philoſophy he thought it ſufficient to employ one argument to evince the plenitude of the world, and for that one he pitched upon the vulgar experiment of a gardener's watering-pot: but whether he were wrought upon by the objections made to his inference from that phænomenon in your examen of his dialogue *De Natura Aeris*, or by ſome other conſiderations, I will not pretend to divine. But I plainly perceive, he now prefers the experiment of the cohering marbles.

B. OF which it will not be amiſs, though the paſſage be ſomewhat long, to read you his whole diſcourſe out of the book I have in my hand.

A. IT is fit, that you, who for my ſake are content to take the pains of anſwering what he ſays, ſhould be eaſed of the trouble of reading it, which I will therefore, with your leave, take upon me. His diſcourſe then about the marbles is this:

A. AD *probandam univerſi plenitudinem, nullum noſtin' argumentum cogens?*

B. IMO *multa: unum autem ſufficit ex eo ſumptum, quod duo corpora plana, ſi ſe mutuò ſecundùm amborum planitiem communem tangant, non facile in inſtante divelli poſſunt; ſucceſſive verò facillimè. Non dico, impoſſible eſſe duo duriſſima marmora ita cohærentia divellere, ſed difficile; & vim poſtulare tantam, quanta ſufficit ad duritiem lapidis ſuperandam. Siquidem verò majore vi ad ſeparationem opus ſit quàm illa, quâ moventur ſeparata, id ſignum eſt, non dari vacuum.*

A. ASSERTIONES *illæ demonſtratione indigent. Primò autem oſtende, quomodo ex duorum duriſſimorum corporum, conjunctorum ad ſuperficies exquiſite læves, diremptione difficili, ſequatur plenitudo mundi?*

A. SI *duo plana, dura, polita corpora (ut marmora) collocentur unum ſupra alterum, ita ut eorum ſuperficies ſe mutuò per omnia puncta exactè, quantum fieri poteſt, contingant, illa ſine magna difficultate ita divelli non poſſunt, ut eodem inſtante per omnia puncta dirimantur. Veruntamen marmora eadem, ſi communis eorum ſuperficies ad horizontem erigatur, aut non valde inclinetur, alterum ab altero facillime (ut ſcis) etiam ſolo pondere dilabentur. Nonne cauſa hujus rei hæc eſt, quod labenti marmori ſuccedit aer, & relictum locum ſemper implet?*

A. CERTISSIME. *Quid ergo?*

B. QUANDO *verò eadem uno inſtante divellere conaris, nonne multo major vis adhibenda eſt; quam ob cauſam?*

A. EGO, *& mecum (puto) omnes cauſam ſtatuunt, quod ſpatium totum inter duo illa marmora divulſa ſimul uno inſtante implere aer non poteſt, quantacunque celeritate fiat divulſio.*

B. AN *qui ſpatia in aere dari vacua contendunt, in illo aere ſolo dari negant, qui marmora illa conjuncta circumdat?*

A. MINIME, *ſed ubique interſperſa.*

B. DUM *ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per conſequens vacuum exprimunt, vacuum faciunt locum per revulſionem relictum; nulla ergo ſeparationis erit difficultas, ſaltem non major, quàm eſt difficultas corpora eadem movendi in aere.*

aere, postquam separata fuerint. Itàque quoniam, concesso vacuo, difficultas marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse vacuum.

A. RECTE *quidem illud infers. Mundi autem plenitudine supposita, quomodo demonstrabis possibile omnino esse, ut divellantur ?*

B. COGITA *primo corpus aliquod ductile, nec nimis durum, ut ceram, in duas partes distrahi, quæ tamen partes non minus exacte in communi plano se mutuo tangunt quàm lævissima marmora. Jam quo pacto distrahatur cera, consideremus. Nonne perpetuo attenuatur, donec in filum evadat tenuissimum, & omni dato crasso tenuius, & sic tandem divellitur? Eodem modo etiam durissima columna in duas partes distrabetur, si vim tantam adhibeas, quanta sufficit ad resistentiam duritici superandam, Sicut enim in cera partes primò extimæ distrabuntur, in quarum locum succedit aer; ita etiam in corpore quantumlibet duro aer locum subit partium extimarum, quæ primæ vulsionis viribus dirumpuntur. Vis autem, quæ superat resistentiam partium extimarum duri, facilè superabit resistentiam reliquarum. Nam resistentia prima est à toto duro, reliquarum verò semper à residuo.*

A. ITA *quidem videtur consideranti, quàm corpora quædam, præsertim verò durissima fragilia sint.*

DOES this ratiocination seem to you as cogent, as it did to the proposer of it?

B. YOU will quickly think it does not; and perhaps you would think it should not, if you please to consider with me some of the reflections, that the reading of it suggested to me.

AND first, without declaring for the vacuist's opinion, I must profess myself unsatisfied with Mr. *Hobbes*'s way of arguing against them: for, where he says, *Dum ergo illi, qui marmor unum ab altero revellentes aerem comprimunt, & per consequens vacuum exprimunt, vacuum faciunt locum per revulsionem relictum; nulla ergo separationis erit difficultas, saltem non major, quàm est difficultas corpora eadem movendi in aere, postquam separata fuerint. Itaque quoniam, concesso vacuo, difficultas marmora illa dirimendi nulla est, sequitur per difficultatis experientiam, nullum esse vaccuum.* Methinks he expresses himself but obscurely, and leaves his readers to guess, what the word *dum* refers to. But that, which seems to be his drift in this passage, is, that, since the vacuists allow interspersed vacuities, not only in the air, that surrounds the conjoined marbles, but in the rest of the ambient air, there is no reason, why there should be any difficulty in separating the marbles, or at least any greater difficulty than in moving the marbles in that air after their separation. But, not to consider, whether his adversaries will not accuse his phrase of squeezing out a vacuum, as if it were a body, they will easily answer, that notwithstanding the vacuities they admit in the ambient air, a manifest reason may be given in their hypothesis of our finding a difficulty in the divulsion of the marbles. For, the vacuities they admit, being but intersperfed, and very small, and the corpuscles of the atmosphere being, according to them, endowed with gravity, there lean so many upon the upper surface of the uppermost marble, that that stone cannot be at once perpendicularly drawn up from the lower marble contiguous to it, without a force capable to surmount the weight of the aerial corpuscles, that lean upon it. And this weight has already so constipated the neighbouring parts of the ambient air, that he, that would perpendicularly raise the upper marble, from the lower, shall need a considerable force to make the revulsion, and compel the already contiguous parts of the incumbent air to a subingression into the pores or intervals, intercepted between them. For the conatus of him, that endeavours to remove the upper marble, whilst the lower surface of it is fenced from the pressure of the atmosphere, by the contact of the lower marble, which suffers no air to come in between them, is not assisted by the weight or pressure of the atmosphere, which, when the marbles are once separated, pressing as strongly against the undermost surface of the upper marble, as the incumbent atmoſpherical pillar does againſt the upper ſurface of the ſame marble, the hand, that endeavours to raiſe it in the free air, has no other reſiſtance, than that ſmall one of the marble's own weight to ſurmount.

A. BUT what ſay you to the reaſon, that Mr. *Hobbes*, and, as he thinks, all others give of the difficulty of the often-mentioned divulſion? namely, *Quòd ſpatium totum inter duo illa marmora divulſa ſimul uno inſtante implere aer non poteſt, quantacunque celeritate fiat divulſio.*

B. I ſay, that, for aught I know, the pleniſts may give a more plauſible account of this experiment, than Mr. *Hobbes* has here done; and therefore, abſtracting from the two oppoſite hypotheſes, I ſhall further ſay, that the genuine cauſe of the phænomenon ſeems to be that, which I have already aſſigned; and that difficulty of raiſing the upper ſtone, that accompanies the air's not being able to come in all at once, to poſſeſs the ſpace left between the ſurfaces of the two marbles upon their ſeparation, proceeds from hence, that, till that ſpace be filled with the atmoſpherical air, the hand of him, that would lift up the ſuperior marble, cannot be fully aſſiſted by the preſſure of the air againſt the lower ſurface of that marble.

A. THIS is a paradox, and therefore I ſhall deſire to know on what you ground it.

B. THOUGH I mention it but as a conjecture propoſed *ex abundanti*, yet I ſhall on this occaſion countenance it with two things; the firſt, that, ſince I declare not for the hypotheſis of the Pleniſts, as it is maintained by Mr. *Hobbes*, I am not bound to allow, what the common explication, adopted by my adverſary, ſuppoſes; namely, that either nature abhors a vacuum (as the ſchools would have it,) or that there could be no divulſion of the marbles, unleſs, at the ſame time, the air were admitted into the room, that divulſion makes for it. And a vacuiſt may tell you, that, provided the ſtrength employed to draw up the ſuperior marble be great enough to ſurmount the weight of the aerial corpuſcles accumulated upon it, the divulſion would enſue, though, by divine Omnipotence, no air, or other body, ſhould be permitted to fill the room made for it by the divulſion; and that the air's ruſhing into that ſpace does not neceſſarily accompany, but in order of nature and time follow upon, a ſeparation of the marbles, the air, that ſurrounded their contiguous ſurfaces, being by the weight of the collaterally ſuperior air, impelled into the room newly made by the divulſion. But I ſhall rather countenance what you call my paradox, by an experiment I purpoſely made in our pneumatical receiver, where, having accommodated two flat and poliſhed marbles, ſo that the lower being fixed, the upper might be laid upon it, and drawn up again as there ſhould be occaſion, I found, that if, when the receiver was well exhauſted, the upper marble was, by a certain contrivance, laid flat upon the lower, they would not then cohere as formerly, but be with great eaſe ſeparated, though it did not, by any phænomenon appear, that any air could come to ruſh in, to poſſeſs the place given it, by the receſs of the upper marble, whoſe very eaſy avulſion is as eaſily explicable by our hypotheſis; ſince the preſſure of that little air, that remained in the receiver, being too faint to make any at all conſiderable reſiſtance to the avulſion of the upper marble, the hand, that drew it up, had very little more than the ſingle weight of the ſtone to ſurmount.

A. AN Anti-pleniſt had expected, that you would have obſerved, that the difficult ſeparation of the marbles in the open air does rather prove, that there may be a vaccum, than that there can be none. For in caſe the air can ſucceed as faſt at the ſides, as the divulſion is made, a vacuiſt may demand, whence comes the difficulty of the ſeparation? And if the air cannot fill the whole room made for it by the ſeparated marbles, at the ſame inſtant they are forced aſunder, how is a vacuum avoided for that time,

how

how small soever, that is necessary for the air to pass from the edges to the middle of the room newly made?

B. WHAT the Plenists will say to your argument, I leave them to consider; but I presume, they will be able to give a more plausible account of the phænomenon we are treating of, than is given by Mr. *Hobbes.*

A. WHAT induces you to dislike his explication of it?

B. Two things; the one, that I think the cause he assigns improbable; and the other, that I think another, that is better, has been assigned already.

AND first, whereas Mr. *Hobbes* requires to the divulsion of the marbles a force great enough to surmount the hardness of the stone, this is asserted gratis, which it should not be; since it seems very unlikely, that the weight of so few pounds, as will suffice to separate two coherent marbles of about an inch, for instance, in diameter, should be able to surmount the hardness of such solid stones, as we usually employ in this experiment. And though it be generally judged more easy to bend, if it may be, or break a broader piece of marble *cæteris paribus*, than a much narrower; yet, whereas neither I, nor any else that I know, nor I believe Mr. *Hobbes*, ever observed any difference in the resistance of marbles to separation from the greater or lesser thickness of the stones; I find by constant experience, that, *cæteris paribus*, the broadness of the coherent marble does exceedingly encrease the difficulty of disjoining them: insomuch, that, whereas not many pounds, as I was saying, would separate marbles of an inch, or a lesser diameter; when I encreased their diameter to about four inches, if I misremember not, there were several men, that successively tried to pull them asunder, without being able by their utmost force to effect it.

A. BUT what say you to the illustration, that Mr. *Hobbes*, upon the supposition of the world's plenitude, gives of our phænomenon, by drawing asunder the opposite parts of a piece of wax?

B. To me it seems an instance improper enough. For first, the parts, that are to be divided in the wax, are of a soft and yielding consistence, and according to him, of a ductile, or, if you please, of a tractile nature, and not, as the parts of the coherent marbles, very solid and hard. Next, the parts of the wax do not stick together barely by a superficial contact of two smooth planes, as do the marbles we are speaking of; but have their parts implicated, and as it were intangled with one another. And therefore, they are far from a disposition to slide off, like the marbles, from one another, in how commodious a posture soever you place them. Besides, it is manifest, that the air has opportunity to succeed in the places successively deserted by the receding parts of the attenuated wax; but it is neither manifest, nor as yet well proved, by Mr. *Hobbes*, that the air does after the same manner succeed between the two marbles, which, as I lately noted, are not forced asunder after such a way, but are, as himself speaks, severed in all their points at the same instant.

A. I know, you forget not what he says, of the dividing of a hard column into two parts, by a force sufficient to overcome the resistance of its hardness.

B. HE does not here either affirm, that he, or any he can trust, has seen the thing done; nor does he give us any such account of the way wherein the pillar is to be broken, whether in an erected, inclined, or horizontal posture; nor describe the particular circumstances, that were fit to be mentioned in order to the solution of the phænomenon. Wherefore, till I be better informed of the matter of fact, I can scarce look upon what Mr. *Hobbes* says of the pillar, as other than his conjecture, which now I shall the rather pass by, not only because the case is differing from that of our polished marbles, which are actually distinct bodies, and only contiguous in one commissure; but also, because I would hasten to the second reason of my dislike of Mr. *Hobbes's*

Hobbes's explication of our phænomenon, which is, that a better has been given already, from the pressure of the atmosphere upon all the superficial parts of the upper marble, save those, that touch the plane of the lower.

A. You would have put fair for convincing Mr. *Hobbes* himself, at least would have put him to unusual shifts, if you had succeeded in the attempt you made, among other of your physico-mechanical experiments, to disjoin two coherent marbles, by suspending them horizontally in your pneumatical receiver, and pumping out the air, that invironed them; for, from your failing in that attempt, though you rendered a not improbable reason of it, Mr. *Hobbes* took occasion, in his dialogue *De Natura Aeris*, to speak in so high a strain as this: *Nihil isthic erat, quod ageret pondus; experimento hoc excogitari contra opinionem eorum, qui vacuum asserunt, aliud argumentum fortius aut evidentius non potuit. Nam si duorum cohærentium alterutrum secundùm eam viam, in qua jacent ipsæ contiguæ superficies, propulsum esset, facile separarentur, Aere proximo in locum relictum successivè semper influente; sed illa ita divellere, ut simul totum amitterent contactum, impossibile est, mundo pleno. Oporteret enim aut motum fieri ab uno termino ad alium in instante, aut duo corpora eodem tempore in eodem esse loco: quorum utrumvis dicere, est absurdum.*

B. You may remenber, that where I relate that experiment, I expressed a hope, that, when I should be better accommodated than I then was, I might attempt the trial with prosperous success; and accordingly afterwards, having got a lesser engine than that I used before, wherewith the air might be better pumped out, and longer kept out, I cheerfully repeated the trial. To shew then, that when two coherent marbles are sustained horizontally in the air, the cause, why they are not to be forced asunder, if they have two or three inches in diameter, without the help of a considerable weight, is the pressure I was lately mentioning of the ambient air; I caused two such coherent marbles to be suspended in a large receiver, with a weight at the lowermost, that might help to keep them steady, but was very inconsiderable to that, which their cohesion might have surmounted; then causing the air to be pumped by degrees out of the receiver, for a good while the marbles stuck close together, because, during that time, the air could not be so far pumped out, but that there remained enough to sustain the small weight, that endeavoured their divulsion: but when the air was further pumped out, at length the spring of the little, but not a little expanded air, that remained, being grown too weak to sustain the lower marble and its small clog, they did, as I expected, drop off.

A. This will not agree over-well with the confident and triumphant expressions just now recited.

B. I never envied Mr. *Hobbes*'s forwardness to triumph, and am content, his conjectures be recommended by the confidence, that accompanies them, if mine be by the success, that follows them. But to confirm the explication given by me of our phænomenon, I shall add, that as the last mentioned trial, which I had several times occasion to repeat, shews, that the cohesion of our two contiguous marbles would cease upon the withdrawing of the pressure of the atmosphere; so by another experiment I made, it appears, that the supervening of that pressure sufficed to cause that cohesion. For, in prosecution of one of the lately mentioned trials, having found, that when the receiver was well exhausted, two marbles, though considerably broad, being laid upon one another after the requisite manner, their adhesion was, if any at all, so weak, that the uppermost would be easily drawn up from off the other; we laid them again one upon the other, and then letting the external air flow into the receiver, we found, according to expectation, that the marbles now cohered well, and we could not raise the uppermost, but accompanied with the lowermost. But I am sensible, I have detained you too long upon the single experiment of the marbles: and though I hope

hope the ſtreſs Mr. *Hobbes* lays on it, will plead my excuſe, yet, to make your patience ſome amends, I ſhall be the more brief in the other particulars, that remain to be conſidered in his dialogue *De Vacuo*. And it will not be difficult for me to keep my promiſe without injuring my cauſe, ſince almoſt all theſe particulars being but the ſame, which he has already alledged in his dialogue *De Natura Aeris*, and I ſoon after anſwered in my Examen of that dialogue, I ſhall need but to refer you to the paſſages, where you may find theſe allegations examined, only ſubjoining here ſome reflections upon thoſe few and ſlight things, that he has added in his problems *De Vacuo*.

A. I may then, I ſuppoſe, read to you the next paſſage to that long one, you have hitherto been conſidering, and it is this: *Ad vacuum nunc revertor: Quas cauſas ſine ſuppoſitione vacui redditurus es illorum effectuum, qui oſtenduntur per Machinam illam, quæ eſt in Collegio* Greſhamenſi?

B. *Machina illa*—

A. STOP here, I beſeech you a little, that, before we go any further, I may take notice to you of a couple of things, that will concern our ſubſequent diſcourſe,

WHEREOF the firſt is, that it appears by Mr. *Hobbes*'s Dialogue about the Air, that the explications he there gave of ſome of the phænomena of the Machina Boyliana, were directed partly againſt the virtuoſi, that have ſince been honoured with the title of the Royal Society, and partly againſt the author of that engine, as if the main thing therein deſigned were to prove a vacuum. And ſince he now repeats the ſame explications, I think it neceſſary to ſay again, that if he either takes the Society or me for profeſſed vacuiſts, he miſtakes, and ſhoots beſides the mark; for, neither they nor I have ever yet declared either for or againſt a vacuum.

AND the other thing I would obſerve to you, is, that Mr. *Hobbes* ſeems not to have rightly underſtood, or at leaſt not to have ſufficiently heeded in what chiefly conſiſts the advantage, which the vacuiſts may make of our engine againſt him: for, whereas in divers places he is very ſolicitous to prove, that the cavity of a pneumatical receiver is not altogether empty, the vacuiſts may tell him, that ſince he aſſerts the abſolute plenitude of the world, he muſt, as indeed he does, reject not only great vacuities, but alſo thoſe very ſmall and interſperſed ones, that they ſuppoſe to be intercepted between the ſolid corpuſcles of other bodies, particularly of the air: ſo that it would not confute them to prove, that in our receiver, when moſt diligently exhauſted, there is not one great and abſolute vacuity, or, as they ſpeak, a *vacuum coacervatum*, ſince ſmaller and diſſeminated vacuities would ſerve their turn. And therefore they may think their pretenſions highly favoured, as by ſeveral particular effects, ſo by this general phænomena of our engine, that it appears by ſeveral circumſtances, that the common or atmoſpherical air, which, before the pump is ſet to work, poſſeſſed the whole cavity of our receiver, far the greateſt part is by the intervention of the pump made to paſs out the cavity into the open air, without being able, at leaſt for a little while, to get in again; and yet it does not appear by any thing alledged by Mr. *Hobbes*, that any other body ſucceeds to fill adequately the places deſerted by ſuch a multitude of aerial corpuſcles.

A. IF I gueſs aright, by thoſe words, (viz. "it appears not by any thing alledged " by Mr. *Hobbes*,") you deſign to intimate, that you would not in general prejudice the pleniſts.

B. YOUR conjecture was well founded: for I think divers of them, and particularly the Carteſians, who ſuppoſe a ſubtile matter or æther fine enough to permeate glaſs, though our common air cannot do it, have not near ſo difficult a taſk to avoid the arguments the vacuiſts may draw from our engine, as Mr. *Hobbes*, who, without having recourſe to the poroſity of glaſs, which indeed is impervious to common air, ſtrives

to

to ſolve the phænomena, and prove our receiver to be always perfectly full, and therefore as full at any one time as at any other of common or atmoſpherical air, as far as we can judge of his opinion by the tendency or import of his explications.

A. Yet, if I were rightly informed of an experiment of yours, Mr. *Hobbes* may be thereby reduced either to paſs over to the vacuiſts, or to acknowledge ſome ætherial or other matter more ſubtil than air, and capable of paſſing through the pores of glaſs; and therefore, to ſhew yourſelf impartial between the vacuiſts and their adverſaries in this controverſy, I hope you will not refuſe to gratify the pleniſts by giving your friends a more particular account of the experiment.

B. I know which you mean, and remember it very well. For, though I long ſince deviſed it, yet having but the other day had occaſion to peruſe the relation I writ down of one of the beſt trials, I think I can repeat it, almoſt in the very words, which if I miſtake not, were theſe:

There was taken a bubble of thin white glaſs, about the bigneſs of a nutmeg, with a very ſlender ſtem, of about four or five inches long, and of the bigneſs of a crow's-quill. The end of the quill being held in the flame of a lamp blown with a pair of bellows, was readily and well ſealed up, and preſently the globous part of the glaſs, being held by the ſtem, was kept turning in the flame, until it was red hot and ready to melt: then being a little removed from the flame, as the included air began to loſe of its agitation and ſpring, the external air manifeſtly and conſiderable preſſed in one of the ſides of the bubble. But the glaſs being again, before the cold could crack it, held as before in the flame, the rarified air diſtended and plumped up the bubble; which being the ſecond time removed from the flame, was the ſecond time compreſſed; and, being the third time brough back to the flame, ſwelled as before, and removed, was again compreſſed, (either this time or the laſt by two diſtinct cavities;) until at length, having ſatisfied ourſelves, that the included air was capable of being condenſed or dilated without the ingreſs or egreſs of air (properly ſo called) we held the bubble ſo long in the flame, ſtrengthened by nimble blaſts, that not only it had its ſides plumped up, but a hole violently broken in it by the over-rarified air, which together with the former watchfulneſs, we employed from time to time to diſcern, if it were any where cracked or perforated, ſatisfied us, that it was until then entire.

A. I confeſs, I did not readily conceive before, how you could, (as I was told you had,) make a ſolid veſſel, wherein there was no danger of the air's getting in or out, whoſe cavity ſhould be ſtill poſſeſſed with the ſame air, and yet the veſſel be made by turns bigger and leſſer. And, though I preſently thought upon a well ſtopped bladder, yet I well foreſaw, that a diſtruſtful adverſary might make ſome objections, which are by your way of proceeding obviated; and the experiment agrees with your doctrine in ſhewing, how impervious we may well think your thick pneumatick receivers are to common air, ſince a thin glaſs bubble, when its pores were opened or relaxed by flame, would not give paſſage to the ſpringy particles of the air, though violently agitated; for if thoſe particles could have got out of the pores, they never would have broke the bubble, as at length a more violent degree of heat made them do; nor probably would the compreſſion, that afterwards enſued, of the bubble by the ambient air, be checked near ſo ſoon, if thoſe ſpringy corpuſcles had not remained within to make the reſiſtance. Methinks, one may hence draw a new proof of what I remember you elſewhere teach, that the ſpring of the air may be much ſtrengthened by heat. For, in our caſe, the ſpring of the air was thereby enabled to expand the compreſſed glaſs, it was impriſoned in, in ſpite of the reſiſting preſſure of the external air; and yet, that this preſſure was conſiderable, appears by this, that the weight of ſo ſmall a column of atmoſpherical air, as could bear upon the bubble, was able to preſs in the heated glaſs, in ſpite of the reſiſtance of its tenacity and arched figure.

B. YET that, which I mainly designed in this experiment, was, (if I were able) to shew and prove at once, by an instance not liable to the ordinary exceptions, the true nature of rarefaction and condensation, at least of the air. For to say nothing of the Peripatetick rarefaction and condensation, strictly so called, which I scruple not to declare, I think to be physically inconceptible or impossible; it is plain by our experiment, that, when the bubble, after the glass had been first thrust in towards the center, was expanded again by heat, the included air possessed more room than before, and yet it could perfectly fill no more room than formerly, each aerial particle taking up, both before and after the heating of the bubble, a portion of space adequate to its own bulk; so that in the cavity of the expanded bubble we must admit either vacuities interspersed between the corpuscles of the air, or that some fine particles of the flame, or other subtil matter, came in to fill up those intervals, which matter must have entered the cavity of the glass at its pores: and afterwards, when the red-hot bubble was removed from the flame, it is evident, that since the grosser particles of the air could not get through the glass, which they were not able to do, even when vehemently agitated by an ambient flame, the compression of the bubble, and the condensation of the air, which was necessarily consequent upon it, could not, supposing the plenitude of the world, be performed without squeezing out some of the subtile matter contained in the cavity of the bubble, whence it could not issue but at the pores of the glass. But I will no longer detain you from Mr. *Hobbes*'s explications of the *Machina Boyliana*; to the first of which you may now, if you please, advance.

A. THE passage I was going to read, when you interrupted me, was this:

B. MACHINA *illa eosdem effectus producit, quos produceret in loco non magno magnus inclusus ventus.*

A. QUOMODO *ingreditur istuc ventus? Machinam nosti cylindrum esse cavum, aeneum, in quem protruditur cylindrus alius solidus ligneis, corio tectus, (quem suctorem dicunt) ita exquisitè congruens, ut ne minimus quidem aer inter corium & aes intrare (ut putant) possit.*

B. SCIO, *& quó suctor facilius intrudi possit, foramen quoddam est in superiori parte cylindri, per quod aer (qui suctoris ingressum alioqui impedire possit) emittatur. Quod foramen aperire possunt, & claudere, quoties usus postulat. Est etiam in cylindri cavi recessu summo datus aditus aeri in globum concacum vitreum, quem etiam aditum claviculâ obturare & aperire possunt, quoties volunt. Denique in globo vitreo summo relinquitur foramen satis amplum, (claviculâ item claudendum & recludendum) ut in illum, quae volunt, immittere possint, experiendi causâ,*

B. THE imaginary wind, to which Mr. *Hobbes* here ascribes the effects of our engine, he formerly had recourse to in the thirteenth page of his Dialogue; and I have sufficiently answered that passage of it in a part of my Examen, to which I therefore refer you.

A. I presume, you did not overlook the comparison Mr. *Hobbes* annexes to what I last read out of the problems, since he liked the conceit so well, that we meet with it in his Dialogue *De Natura Aeris*. The words (as you see) are these: *Tota denique machina non multum differt, si naturam ejus spectes, à sclopeto ex sambuco, quo pueri se delectant, imitantes sclopetos militum, nisi quòd major sit, & majori arte fabricatus, & pluris constet.*

B. I could scarce, for the reason you give, avoid taking notice of it. And if Mr. *Hobbes* intended it for a piece of raillery, I willingly let it pass, and could more easily forgive him a more considerable attempt than this, to be revenged on an engine, that has destroyed several of his opinions: but if he seriously meant to make a physical comparison, I think he made a very improper one. For not to urge, that one may well doubt how he knows, that in the enclosed cavity of his pot-gun, there is a very vehement

vehement wind, ſince that does not neceſſarily follow from the compreſſion of the encluded air: in Mr. *Hobbes*'s inſtrument, the air, being forcibly compreſſed, has an endeavour to expand itſelf, and when it is able to ſurmount the reſiſtance of its priſon, that part, that is firſt disjoined, is forcibly thrown downwards; whereas in our engine it appears by the paſſage lately cited of our Examen, that the air is not compreſſed, but expanded, in our receiver, and if an intercourſe be opened, or the veſſel be not ſtrong enough, the outward air violently ruſhes in; and if the receiver chance to break, the fragments of the glaſs are not thrown outwards, but forcibly inwards.

A. So that, whether or no Mr. *Hobbes* could have pitched upon a compariſon more ſuitable to his intentions, he might eaſily have employed one more ſuitable to the phænomena.

B. I preſume, you will judge it the leſs agreeable to the phænomena, if I here ſubjoin an experiment, that poſſibly you will not diſlike; which I deviſed to ſhew, not only that in our exhauſted receivers there is no ſuch ſtrong endeavour outwards, as moſt of Mr. *Hobbes*'s explications of the things, that happen in them are built upon, but that the weight of the atmoſpherical air, when it is not reſiſted by the counterpreſſure of any internal air, is able to perform what a weight of many pounds would not ſuffice to do.

A. I ſhall the more willingly learn an experiment to this purpoſe, becauſe in your receivers the rigidity of the glaſs keeps us from ſeeing, by any manifeſt change of its figure, whether, if it could yield without breaking, it would not be preſſed in, as your hypotheſis requires.

B. THE deſires to obviate that very difficulty, for their ſatisfaction, that had not yet penetrated the grounds of our hypotheſis, made me think of employing, inſtead of a receiver, of glaſs, one of a ſtiff and tough, but yet ſomewhat flexible, metal. And accordingly, having provided a new pewter porringer, and whelmed it upſide down upon an iron plate, faſtened to the upper end of our pneumatical pump, we carefully faſtened, by cement, the orifice to the plate; and though the inverted veſſel, by reaſon of its ſtiffneſs and thickneſs, and the convexity of its ſuperficies, were ſtrong enough to have ſupported a great weight without changing its figure; yet, as ſoon as, by an exſuction or two, the remaining part of the included air was brought to ſuch a degree of expanſion, that its weakened ſpring was able to afford but little aſſiſtance to the tenacity and firmneſs of the metal, the weight of the pillar of the incumbent atmoſphere (which, by reaſon of the breadth of the veſſel, was conſiderably wide alſo) did preſently and notably depreſs the upper part of the porringer, both leſſening its capacity and changing its figure; ſo that, inſtead of the convex ſurface, the receiver had before, it came to a concave one, which new figure was ſomewhat, though not much, increaſed by the further withdrawing of the included and already rarified air. The experiment ſucceeded alſo with another common porringer of the ſame metal. But in ſuch kind of veſſels, made purpoſely of iron plates, it will ſometimes ſucceed and ſometimes not, according to the diameter of the veſſel, and the thickneſs of the plate, which was ſometimes ſtrong enough, and ſometimes too weak to reſiſt the preſſure of the incumbent air, And ſometimes I found alſo, that the veſſel would be thruſt in, not at the top but ſide-ways, in caſe that ſide were the only part, that were made too thin to reſiſt the preſſure of the ambient; which phænomenon I therefore take notice of, that you may ſee, that that powerful preſſure may be exerciſed laterally as well as perpendicularly.

PERHAPS this experiment, and that I lately recited of an hermetically ſealed bubble, by their fitneſs to diſprove Mr. *Hobbes*'s doctrine, may do ſomewhat towards the letting him ſee, that he might have ſpared that not over-modeſt and wary expreſſion, where, ſpeaking of the gentlemen, that meet at *Greſham-college*, (of whom I pretend not to be one of the chief) he is pleaſed to ſay, *Experimenta faciant quantum volunt, niſi principiis utantur [illegible] meis, nihil proficient.* But let us, if you pleaſe, paſs on to what he further alledges to prove, that the ſpace in the exhauſted receiver, which the vacuiſts ſuppoſe to be partly empty, is full of air. *Video* (ſays A.) *ſi ſuctor trudatur uſque ad fundum cylindri aenei, obturenturque foramina, ſecuturum eſſe, dum ſuctor retrabitur, locum in cylindro cavo relictum fore vacuum. Nam ut in locum ejus ſuccedat aer, eſt impoſſibile.* To which B. anſwers, *Credo equidem, ſuctorem cum cylindri cavi ſuperficie ſatis arctè cohaerere ad excludendum ſtramen & plumam, non autem aërem neque aquam. Cogita enim, quod non ita accuratè congruerent, quin undiquaque interſtitium relinqueretur, quantum tenuiſſimi capilli capax eſſet. Retracto ergo ſuctore, tantum impelleretur aëris, quamtam viribus illis conveniret, quibus aer propter ſuctoris retractionem reprimitur, idque ſine omni difficultate ſenſibili. Quanto autem interſtitium illud minus eſſet, tantum ingrederetur aer velocius: vel ſi contactus ſit, ſed non per omnia puncta, etiam tunc intrabit aer, modò ſuctor majore vi retrahatur. Poſtremò etſi contactus ubique exactiſſimus ſit, vi tamen ſatis auctâ per cochleam ferream, tum corium cedet, tum ipſum aes; atque ita quoque ingredietur aer. Credin' tu, poſſibile eſſe duas ſuperficies ita exactè componere, ut has compoſitas eſſe ſupponunt illi; aut corium ita durum eſſe, ut aeri, qui cochleae ope incutitur, nihil omnino cedat? Corium, quanquam optimum, admittit aquam, ut ipſe ſcis, ſi fortè feciſti unquam iter vento & pluvia ὑόμενος καὶ ἀήμενος. Itaque dubitare non potes, quin retractus ſuctor tantum aeris in cylindrum adeoque in ipſum recipiens incutiat, quantum ſufficit ad locum ſemper relictum* perfectè *implendum. Effectus ergo, qui oritur à retractione ſuctoris, alius non eſt quàm ventus (inquam) vehementiſſimus, qui ingreditur undiquaque inter ſuctoris ſuperficiem convexam, & cylindri aenei concavum, proceditque (versâ claviculâ) in cavitatem globi vitrei, ſive (ut vocatur) recipientis.*

The ſubſtance of this ratiocination having been already propoſed Mr. *Hobbes*, in his dialogue of the air, the eleventh page, I long ſince anſwered it in my Examen; and therefore I ſhall only now take notice *in tranſitu* of ſome ſlight, whether additions or variations, that occur in what you have been reading. And, firſt, I ſee no probability in what he *gratis* aſſerts, that ſo thick a cylinder of braſs, as made the chief part of the pump of our engine, ſhould yield to the ſucker, that was moved up and down in it, though by the help of an iron rack. And whereas he adds, that the leather, that ſurrounds the more ſolid part of the ſucker, would yield to ſuch a force; it ſeems, that that compreſſion of the leather ſhould, by thruſting the ſolid parts into the pores, make the leather rather leſs, than more fit to give paſſage to the air. Nor would it however follow, notwithſtanding Mr. *Hobbes*'s example, that, becauſe a body admits water, it muſt be pervious to air; for I have ſeveral times, by ways elſewhere taught, made water penetrate the pores of bladders, and yet bladders reſiſt the paſſage of the air ſo well, that even when air included in them was ſufficiently rarified by heat, or by our engine, it was neceſſary for the air to break them before it could get out; which would not have been, if it could have eſcaped through their pores. What Mr. *Hobbes* inculcates here again concerning his *ventus vehementiſſimus*, you will find anſwered in the place of my Examen I lately directed you to.

A. We may then proceed to Mr. *Hobbes*'s next explication, which he propoſes in theſe terms:

A. Causam *video nunc unius ex machinae mirabilibus, nimirum cur ſuctor, poſtquam eſt aliquatenus retractus & deinde amiſſus, ſubitò recurrit ad cylindri ſummitatem. Nam aer, qui vi magna fuit impulſus, rurſus per repercuſſionem ad externa vi eadem revertitur.*

B. Atque *hoc quidem argumenti ſatis eſt, etiam ſolum, quòd locus à ſuctore relictus non eſt vacuus. Quid enim aut attrahere aut impellere ſuctorem potuit ad locum illum, unde*

 retractus

retractus erat, si cylindrus fuisset vacuus? Nam ut aeris pondus aliquod id efficere potuisset, falsum esse satis supra demonstravi ab eo, quod aer in aere gravitare non potest. Nosti etiam, quod cum è recipiente aerem omnem (ut illi loquuntur) exegerint, possunt tamen trans vitrum id quod intus fit videre, & sonum, si quis fiat, inde audire. Id quod solum, etsi nullum alliud argumentum esset (sunt autem multa,) ad probandum, nullum *esse in recipiente vacuum, abundè sufficit.*

B. HERE are several things joined together, which the author had before separately alledged in his often-mentioned dialogue. The first is, the cause he assigns of the ascension of the sucker, forcibly depressed to the bottom of the exhausted cylinder, and then let alone by him that pumped; to which might be added, that this ascension succeeded, when the sucker was clogged with an hundred pound weight. This explication of Mr. *Hobbes* you will find examined in my discourse. And as to his denying, that the weight or pressure of the air could drive up the sucker in that phænomenon, because the air does not weight in air, we may see the contrary largely proved in divers places of my Examen, and more particularly and expresly in the first pages of the third chapter. And whereas he says, in the last place, that the visibility of bodies included in our receivers, and the propagation of sound, (which, by the way, is not to be understood of all sound, that may be heard, though made in the exhausted receiver,) are alone sufficient arguments to prove no vacuum; I have considered that passage in the answer I made to the like allegation, in a part of the Examen; and shall only observe here, that, since the vacuists can prove, that much of the air is pumped out of the exhausted receiver, and will pretend, that, notwithstanding many interspersed vacuities, there may be in the receiver corporeal substance enough to transmit light and stronger sounds, Mr. *Hobbes* has not performed what he pretended, if he have but barely proved, that there may be substances capable of conveying light and sound in the cavity of our receiver, since he triumphantly asserts, *nullum esse in recipienti vacuum.* But we may leave Mr. *Hobbes* and his adversaries to dispute out this point, and go on to the next passage.

A. WHICH follows in these words:

Ad illud autem, quod si vesica aliquatenus inflata in recipiente includatur, paulo post per exuctionem aeris inflatur vehementiùs, & dirumpitur, quid respondes?

B. *Motus partium aeris undiquaque concurrentium velocissimus & per concursum in spatiis brevissimis numeroque infinitis gyrationis velocissimae vesicam in locis innumerabilibus simul & vi magna, instar totidem terebrarum, penetrat, praesertim si vesica, antequam immittatur, quò magis resistat aliquatenus inflata sit. Postquam autem aer penetrans semel ingressus est, facile cogitare potes, quo pacto deinceps vesicam tendet, & tandem rumpet. Verùm si antequam rumpatur, versâ claviculâ, aer externus admittatur, videbis vesicam propter vehementiam motus temperatum diminutâ tensione rugosiorem. Nam id quoque observatum est. Jam si haec, quam dixi, causa minùs tibi videatur verisimilis, vide an tu aut alius quicunque imaginari potest, quo pacto vesica distendi & rumpi possit à viribus vacui, id est, nihili.*

B. THIS explication Mr. *Hobbes* gives us in the 19th page of his dialogue *De Natura Aeris*, and you may find it at large confuted in the latter part of the third chapter of my Examen. Nor does, what he here says in the close about the *vires vacui* or *nihili*, deserve to detain us, since there is no reason at all, that the vacuists should ascribe to nothing a power of breaking a bladder, of whose rupture the spring of the included air supplies them so easily with a sufficient cause.

AFTER what Mr. *Hobbes* has said of the breaking of a bladder, he proceeds to an experiment, which he judges of affinity with it, and his academian having proposed this question:

Unde

Unde fit, ut animalia tam cito, nimirum ſpatio quatuor minutorum horae, in recipiente interficiantur?

For anſwer to it our author ſays:

B. *Nonne animalia ſic incluſa inſugunt in pulmones aerem vehementiſſimè motum? quo motu neceſſe eſt, ut tranſitus ſanguinis ab uno ad alterum cordis ventriculum interceptus, non multò pòſt ſiſtatur. Ceſſatio autem ſanguinis mors eſt. Poſſunt tamen animalia ceſſante ſanguine reviviſcere, ſi aer externus ſatis maturè intromittatur, vel ipſa in aerem temperatum, antequam refrixerit ſanguis, extrahantur.*

This explication is not probable enough, to oblige me to add any thing about it to what I have ſaid in my Examen; eſpecially the moſt vehement motion, aſcribed to the air in the receiver, having been before proved to be an imaginary thing. You may therefore, if you pleaſe, take notice of the next explication.

[*Idem aer* (ſays he) *in recipiente carbones ardentes extinguit, ſed & illi, ſi, dum ſatis calidi ſunt, eximantur, relucebunt. Notiſſimum eſt, quòd in ſodinis carbonum terreorum (cujus rei experimentum ipſe vidi) ſaepiſſime è lateribus foveae ventus quidam undiquaque exit, qui foſſores interficit, ignemque extinguit, qui tamen reviviſcunt, ſi ſatis cito ad aerem liberum extrahantur.*]

This compariſon, which Mr. *Hobbes* here ſummarily makes, he more fully diſplayed in his dialogue *De Natura Aeris*, and I conſidered, what he there alledged, in my Examen. And though I will not contradict Mr. *Hobbes* in what he hiſtorically aſſerts in this paſſage; yet I cannot but ſomewhat doubt, whether he mingles not his conjecture with the bare matter of fact. For, though I have with ſome curioſity viſited mines in more places than one, and propoſed queſtions to men, that have been converſant in other mines, both elſewhere and in *England* (and particularly in *Derbyſhire* where Mr. *Hobbes* lived long;) yet I could never find, that any ſuch odd and vehement wind, as Mr. *Hobbes* aſcribes the phænomenon to, had been by them obſerved to kill the diggers, and extinguiſh well-lighted coals themſelves: and indeed, it ſeems more likely, that the damp, by its tenacity or ſome peculiarly malign quality, did the miſchief, than a wind, of which I found not any notice taken; eſpecially ſince we ſee, what vehement winds men will be able to endure for a long time, without being near killed by them; and that it ſeems very odd, that a wind, that Mr. *Hobbes* does not obſerve to have blown away the coals, that were let down, ſhould be able (inſtead of kindling them more fiercely) to blow them out.

A. The laſt experiment of your engine, that your adverſary mentions in theſe problems, is delivered in this paſſage:

A. *Si phialam aquae in recipiens demiſeris, exuсto aere bullire videbis aquam. Quid ad hoc reſpondebis?*

B. *Credo ſanè in tanta aeris motitatione ſaltaturam eſſe aquam, ſed ut caleſiat, nondum audivi. Sed imaginabile non eſt*, ſaltationem illam à vacuo naſci poſſe.

B. This phænomenon he likewiſe took notice of, and attempted to explicate in his above-mentioned dialogue, which gave me occaſion, to ſhew, how unlikely it is, that the vehement motion of the air ſhould be the cauſe of it; but he here tells us, that it is not imaginable, that this dancing of the water (as he is pleaſed to call it) proceeds from a vacuum, nor do I know any man, that ever pretended, that a vacuum was the efficient cauſe of it. But the vacuiſts perhaps will tell him, that, though the bubbling of the water be not an effect of a vacuum, it may be a proof of it againſt him; for they will tell him, that it has been formerly proved, that a great part of the atmoſpherical air is by pumping removed out of our exhauſted receiver, and conſequently can no more, as formerly, preſs upon the ſurface of the water. Nor does Mr. *Hobbes* ſhew what ſucceeds in the room of it; and therefore it will be allowable, for them to conclude againſt him (though not perhaps againſt the Carteſians) that there are a great many interſperſed vacuities left in the receiver, which are the occaſion, though not the proper efficient cauſe of the phænomenon. For they will ſay, that the ſpringy particles of the yet included air, having room to unbend themſelves in the ſpaces deſerted by the air, that was pumped out, the aerial and ſpringy corpuſcles, that lay concealed in the pores of the water, being now freed from the wonted preſſure, that kept them coiled up in the liquor, expanded themſelves into numerous bubbles, which becauſe of their comparative lightneſs, are extruded by the water, and many of them appear to have riſen from the bottom of it. And Mr. *Hobbes*'s vehement wind, to produce the ſeveral circumſtances of this experiment, muſt be a laſting one. For, after the agitation of the pump has been quite left off, provided the external air be kept from getting in, the bubbles will ſometimes continue to riſe for an hour after. And that, which agrees very well with our explication, and very ill with that of Mr. *Hobbes*'s, is, that, when by having continued to pump a competent time, the water has been freed from the aerial particles, that lurked in it before, though one continue to pump as luſtily as he did, yet the water will not at all be covered with bubbles, as it was, the air that produced them, being ſpent; though, according to Mr. *Hobbes*'s explications, the wind in the receiver continuing, the dance of the water ſhould continue too.

A. I eaſily gueſs, by what you have ſaid already, what you may ſay of that epiphomena, wherewith Mr. *Hobbes* (in his 18th page) concludes the explications of the phænomena of your engine. [*Spero jam te certum eſſe*, ſays he, *nullum eſſe machinae illius phænomenon, quo demonſtrari poteſt ullum in univerſo locum dari corpore omni vacuum.*]

B. If you gueſſed aright, you gueſſed, that I would ſay, that as to the phænomena of my engine, my buſineſs was to prove, that he had not ſubſtituted good explications of them in the place of mine, which he was pleaſed to reject. And as for the proving a vacuum by the phænomena of my engine, though I declared, that was not the thing intended, yet I ſhall not wonder, that the vacuiſts ſhould think thoſe phænomena give them an advantage againſt Mr. *Hobbes.* For, though in the paſſage recited by you he ſpeak more cautiouſly than he is wont to do, yet, by what you may have already obſerved in his argumentations, the way he takes to ſolve the phænomena of our engine, is by contending, that our receiver, when we ſay it is almoſt exhauſted, is as full as ever (for he will have it perfectly full,) of common air; which is a conceit ſo contrary to I know not how many phænomena, that I do not remember I have met with or heard of any naturaliſt, whether vacuiſt or pleniſt, that having read my phyſico-mechanical experiments and his dialogue, has embraced his opinion.

A. After what you have ſaid, I will not trouble you with what he ſubjoins about vacuum in general, where having made his academian ſay, [*Mundum ſcis finitum eſſe, & per conſequens vacuum eſſe oportere totum illud ſpatium, quod eſt extra mundum infinitum. Quid impedit, quo minus vacuum illud cum aere mundano permiſceatur?*] he anſwers: *De rebus tranſmundanis nihil ſcio.* For I know, that it concerns not you to take notice of it. But poſſibly the vacuiſts will think he fathers upon them an impropriety they would not be guilty of, making them ſpeak, as if they thought, the *ultra mundan vacuum* were a real ſubſtance, that might be brought into this world, and mingled with our air. And ſince, for aught I know, Mr. *Hobbes* might have ſpared this paſſage, if he had not deſigned it ſhould introduce the ſlighting anſwer he makes to it; I ſhall add, that by the account Mr. *Hobbes* has given of ſeveral phænomena within the world, it is poſſible, that the vacuiſts may believe his profeſſion of knowing nothing of things beyond it.

After

After the *Experimenta Boyliana* (as your other adversary calls them;) Mr. *Hobbes* proceeds to the Torricellian experiment, of which he thus discourses:

A. *Quid de experimento censes Torricelliano, probante vacuum per argentum vivum hoc modo: est in seq. figura ad* A, *pelvis, sive aliud vas, & in eo argentum vivum usque ad* B; *est autem* C D *tubus vitreus concavus repletus quoque argento vivo. Hunc tubum si digito obturaveris, erexerisque in vase* A, *manumque abstuleris, descendet argentum vivum à* C; *verùm non effundetur totum in pelvim, sed sistetur in distantia quadam, puta in* D. *Nonne ergo necessarium est, ut pars tubi inter* C *&* D *sit vacua? non enim puto negabis, quin superficies tubi concava & argenti vivi convexa se mutuo exquisitissimè contingant.*

B. *Ego neque nego contactum, neque vim consequentiae intelligo.*

By which passage it seems, that he still persists in the solution of this experiment, which he gave in his dialogue *De Natura Aeris*, and formerly did, for the main, either propose, or adopt, in his elements of philosophy.

B. This opinion or explication of Mr. *Hobbes* I have, as far as concerns me, considered in my Examen, to which it may well suffice me to refer you. But yet let me take notice of what he now alledges:

B. *Si quis* (says he) *in argentum vivum, quod in vase est, vesicam immerserit inflatam, nonne illa amotâ manu emerget?*

A. *Ita certè, etsi esset vesica ferrea vel ex materia quacunque praeter aurum.*

B. *Vides igitur ab aere penetrari posse argentum vivum.*

A. *Etiam, & quidem illâ ipsâ vi, quam à pondere accipit argenti vivi.*

I confess this allegation did a little surprize me: it concerned Mr. *Hobbes* to prove, that as much air, as was displaced by the descending mercury, did at the orifice of the tube, immersed in stagnant mercury, invisibly ascend to the upper part of the pipe. To prove this he tells us, that a bladder full of air being depressed in quicksilver, will, when the hand, that depressed it, is removed, be squeezed up by the very weight of the mercury, whence it follows, that air may penetrate quicksilver. But I know not, who ever denied, that air invironed with quicksilver may thereby be squeezed upwards; but, since even very small bubbles of air may be seen to move in their passage through mercury, I see not, how this example will at all help the proposer of it. For it is by mere accident, that the air included in the bladder comes to be buoyed up, because the bladder itself is so; and if it were filled with water instead of air, or with stone instead of water, it would nevertheless emerge, as himself confesses it would do, if it were made of iron, or of any matter besides gold, because all other bodies are lighter in specie than quicksilver. But since the immersion of the bladder is manifest enough to the sight, I see not how it will serve Mr, *Hobbes*'s turn, who is to prove, that the air gets into the Torricellian tube invisibly; since it is plain, that even heedful observations can make our eyes discover no such trajection of the air; which (to add that enforcement of our argument) must not only pass unseen through the sustained quicksilver, but must likewise unperceivedly dive, in spite of its comparative lightness, beneath the surface of the ponderous stagnant mercury, to get in at the orifice of the erected tube. But let us, if you please, hear the rest of his discourse about this experiment.

A. Though it be somewhat prolix, yet, according to my custom hitherto, I will give it you verbatim.

B. Simul *atque argentum vivum descenderit ad* D, *altius erit in vase* A *quàm antè nimirum plus erit argenti vivi in vase quàm erat ante descensum, tanto quantum capit pars tubi* C, D. *Tanto quoque minus erit aeris extra tubum quàm ante erat. Ille autem aer, qui ab argento vivo loco suo extrusus est, (suppositâ universi plenitudine) quò abire potest nisi ad eum locum, qui in tubo inter* C *&* D *à descensu argenti vivi relinquebatur? sed quâ, inquies, viâ in illum locum successurus est? Quà nisi per ipsum corpus argenti vivi aerem urgentis?*

Sicut

Sicut enim omne grave liquidum, sui ipsius pondere, aerem, quem descendendo premit, ascendere cogit (si via alia non detur) per suum ipsius corpus; ita quoque aerem quem premit ascendendo, (si via alia non detur) per suum ipsius corpus transire cogit. Manifestum igitur est, suppositâ mundi plenitudine posse aerem externum ab ipsa gravitate argenti vivi cogi in locum illum inter C & D. *Itaque phaenomenon illud necessitatem vacui non demonstrat. Quoniam autem corpus argenti vivi penetrationi, quae fit ab aere, non nihil resistit, & ascensioni argenti vivi in vase* A *resistit aer; quando illae duae resistentiae aequales erunt, tunc in tubo sistetur alicubi argentum vivum; atque ibi est* D.

B. IN answer to this explication I have in my Examen proposed divers things, which you may there meet with: and indeed his explication has appeared so improbable to those, that have written of this experiment, that I have not found it embraced by any of them, though, when divers of them opposed it, the phænomena of our engine were not yet divulged. Not then needlesly to repeat what has been said already, I shall on this occasion only add one experiment, that I afterwards made, and it was this: having made the Torricellian experiment (in a straight tube) after the ordinary way, we took a little piece of a fine bladder, and raising the pipe a little in the stagnant mercury, but not so high as the surface of it, the piece of bladder was dexterously conveyed in the quicksilver, so as to be applied by one's finger to the immersed orifice of the pipe, without letting the air get into the cavity of it; then the bladder was tied very straight and carefully to the lower end of the pipe, whose orifice, as we said, it covered before, and then the pipe being slowly lifted out of the stagnant mercury, the impendent quicksilver appeared to lean but very lightly upon the bladder, being so near an exact æquilibrium with the atmospherical air, that, if the tube were but a very little inclined, whereby the gravitation of the quicksilver, being not so perpendicular, came to be somewhat lessened, the bladder would immediately be driven into the orifice of the tube, and to the eye, placed without, appear to have acquired a concave superficies instead of the convex it had before. And when the tube was re-erected, the bladder would no longer appear sucked in, but be again somewhat protuberant. And if, when the mercury in the pipe was made to descend a little below its station into the stagnant mercury, if, I say, at that nick of time, the piece of bladder were nimbly and dexterously applied, as before, to the immersed orifice, and fastened to the sides of the pipe, upon the lifting the instrument out of the stagnant mercury, the cylinder of that liquor being now somewhat short of its due height, was no longer able fully to counterpoise the weight of the atmospherical air, which consequently, though the glass were held in an erected posture, would press up the bladder into the orifice of the pipe, and both make and maintain there a cavity sensible both to the touch and the eye.

A. WHAT did you mainly drive at in this experiment?

B. To satisfy some ingenious men, that were more diffident of, than skilful in hydrostaticks, that the pressure of the external air is capable of sustaining a cylinder of twenty-nine or thirty inches of mercury; and upon a small lessening of the gravitation of that ponderous liquor, to press it up higher into the tube. But a farther use may be made of it against Mr. *Hobbes*'s pretension. For, when the tube is again erected, the mercury will subside as low as at first, and leave as great a space as formerly was left deserted at the top; into which, how the air should get to fill it, will not appear easy to them, that, like you and me, know by many trials, that a bladder will rather be burst by air than grant it passage. And if it should be pretended, either, that some air from without had yet got through the bladder, or, that the air, that they may presume to have been just before included between the bladder and the mercury, made its way from the lower part of the instrument to the upper; it is obvious to anſwer, that it is no way likely, that it ſhould paſs all along the cylinder unſeen by us; ſince when there are really any aerial bubbles, though ſmaller than pins heads, they are eaſily diſcernible. And in our caſe, there is no ſuch reſiſtance of the air to the aſcenſion of the ſtagnant mercury, as Mr. *Hobbes* pretends in the Torricellian experiment made the uſual way.

A. BUT, whatever becomes of Mr. *Hobbes*'s explication of the phænomenon; yet may not one ſtill ſay, that it affords no advantage to the vacuiſts againſt him?

B. WHETHER or no it do againſt other Pleniſts, I ſhall not now conſider; but I doubt, the vacuiſts will tell Mr. *Hobbes*, that he is fain in two places of the explication, we have read, to ſuppoſe the plenitude of the world, that is, to beg the thing in queſtion, which it is not to be preſumed they will allow.

A. BUT may not Mr. *Hobbes* ſay, that it is as lawful for him to ſuppoſe a plenum, as for them to ſuppoſe a vacuum.

B. I think he may juſtly ſay ſo; but it is like they will reply, that, in their way of explicating the Torricellian experiment, they do not ſuppoſe a vacuum as to air, but prove it. For they ſhew a great ſpace, that having been juſt before filled with quickſilver, is now deſerted by it, though it appeared not, that any air ſucceeded in its room; but rather, that the upper end of the tube, is either totally, or near totally ſo devoid of air, that the quickſilver may without reſiſtance, by barely inclining the tube, be made to fill it to the very top: whereas, Mr. *Hobbes* is fain to have recourſe to that, which he knows they deny, the plenitude of the world, not proving by any ſenſible phænomena, that there did get in, through the quickſilver, air enough to fill the deſerted part of the tube, but only concluding, that ſo much air muſt have got in there, becauſe, the world being full, it could find no room any where elſe; which the vacuiſts will take for no proof at all, and the Carteſians, though Pleniſts, who admit an etherial matter capable of paſſing through the pores of glaſs, will, I doubt, look upon but as an improper explication.

A. I remember on this occaſion another experiment of yours, that ſeems unfavourable enough to Mr. *Hobbes*'s explication; and you will perhaps call it to mind, when I tell you, that it was made in a bended pipe almoſt filled with quickſilver.

B. To ſee, whether we underſtand one another, I will briefly deſcribe the inſtrument I think you mean. We took a cylindrical pipe of glaſs, cloſed at the upper end, and of that length, that being dexterouſly bent at ſome inches from the bottom, the ſhorter leg was made as parallel, as we could, to the longer: in this glaſs we found an expedient, (for it is not eaſy to do,) to make the Torricellian experiment, the quickſilver in the ſhorter leg ſerving inſtead of the ſtagnant quickſilver in the uſual baroſcope, and the quickſilver in the longer leg reaching above that in the ſhorter, about eight or nine and twenty inches. Then, by another artifice, the ſhorter leg, into which the mercury did not riſe within an inch of the top, was ſo ordered, that it could in a trice be hermetically ſealed, without diſordering the quickſilver. And this is the inſtrument, that I gueſs you mean.

A. IT is ſo, and I remember, that it is the ſame with that, which in the paradox about ſuction you call, whilſt the ſhorter leg remains unſealed, a travelling baroſcope. But when I ſaw you make the experiment, that leg was hermetically ſealed, an inch of air in its natural or uſual conſiſtence being left in the upper part of it, to which air you outwardly applied a pair of heated tongs.

B. YET that, which I chiefly aimed at in the trial, was not the phænomenon I perceive you mean; for my deſign was, by breaking the ice for them, to encourage ſome, that may have more ſkill and accommodation than I then had, to make an attempt, that I did not find to have been made by any; namely, to reduce the expanſive force of heat in every way included air, if not in ſome other bodies alſo, to ſome kind of meaſure, and, if it were poſſible, to determine it by weight. And I preſumed, that, at leaſt, the event of my trial would much confirm ſeveral explications of mine, by ſhewing, that heat is able, as long as it laſts, very conſiderably to encreaſe the ſpring or preſſing power of the air. And in this conjecture I was not miſtaken; for, having ſhut up, after the manner newly recited, a determinate quantity of uncompreſſed air, which (in the experiment you ſaw) was about one inch; we warily held a pair of heated tongs near the outſide of the glaſs, (without making it touch the inſtrument, for fear of breaking it,) whereby the air being agitated, was enabled to expand itſelf to double its former dimenſions, and conſequently had its ſpring ſo ſtrengthened by heat, that it was able to raiſe all the quickſilver in the longer leg, and keep up, or ſuſtain, a mercurial cylinder of about nine and twenty inches high, when, by its expanſion, it would, if it had not been for the heat, have loſt half the force of its elaſticity. But whatever I deſign in this experiment, pray tell me, what uſe you would make of it againſt Mr. *Hobbes*.

A. I believe, he will find it very difficult to ſhew, what keeps the mercury ſuſpended in the longer leg of the travelling baroſcope, when the ſhorter leg is unſtopped, at which it may run out; ſince this inſtrument may, as I have tried, be carried to diſtant places, where it cannot with probability be pretended, that any air has been diſplaced by the fall of the quickſilver in the longer leg, which perhaps fell long before above a mile off. And when the ſhorter leg is ſealed, it will be very hard for Mr. *Hobbes* to ſhew there the odd motions of the air, to which he aſcribes the Torricellian experiment. For, if you warily incline the inſtrument, the quickſilver will riſe to the top of the longer leg, and immediately ſubſide, when the inſtrument is again erected, and yet no air appears to paſs through the quickſilver interpoſed between the ends of the longer and the ſhorter leg. But that, which I would chiefly take notice of in the experiment, is, that upon the external application of a hot body to the ſhorter leg of the baroſcope, when it was ſealed up, the included air was expanded from one inch to two, and ſo raiſed the whole cylinder of mercury in the longer leg, and, whilſt the heat continued undiminiſhed, kept it from ſubſiding again. For, if the air were able to get unſeen through the body of the quickſilver, why had it not been much more able, when rarified by heat, to paſs through the quickſilver, than for want of doing ſo to raiſe and ſuſtain ſo weighty a cylinder of mercury? I ſhall not ſtay to enquire on this occaſion, how Mr. *Hobbes* will, according to his hypotheſis, explicate the rarefaction of the air to double its former dimenſions, and the condenſation of it again; eſpecially ſince, aſſerting that part of the upper leg, that is unfilled with the quickſilver, to be perfectly full of air, he affirms that, which I doubt he cannot prove, and which may very probably be diſproved by the experiment you mention in the diſcourſe about ſuction, where you ſhew, to another purpoſe, that in a travelling baroſcope, whoſe ſhorter leg is ſealed, if the end of the longer leg be opened, whereby it comes indeed to be filled with air, the preſſure of that air will enable the ſubjacent mercury notably to compreſs the air included in the ſhorter leg.

B. I leave Mr. *Hobbes* to conſider what you have objected againſt his explication of the Torricellian experiment; to which I ſhall add nothing, though, perhaps, I could add much, becauſe I think it may be well ſpared, and our conference has laſted long already.

A. I will then proceed to the laſt experiment recited by Mr. *Hobbes* in his *Problemata de Vacuo.*

A. *Si phialam, collum habentem longiuſculum, eandémque omni corpore praeter aerem vacuam ore ſugas, continuoque phialae os aquae immergas, videbis aquam aliquouſque aſcen-*

dere in phialam. Quî fieri hoc poteſt, niſi factum ſit vacuum ab exuctione aeris, in cujus locum poſſit aqua illa aſcendere?

B. *Conceſſo vacuo, oportet quaedam loca vacua fuiſſe in illo aere, etiam qui erat intra phialam ante ſuctionem. Cur ergo non aſcendebat aqua ad ea implenda abſque ſuctione? is qui ſugit phialam, neque in ventrem quicquam, neque in pulmones, neque in os è phiala exugit. Quid ergo agit? Aerem commovet, & in partibus ejus conatum ſugendo efficit per os exeundi, & non admittendo, conatum redeundi. Ab his conatibus contrariis componitur circumitio intra phialam, & conatus exeundi quaquaverſum. Itaque phialae ore aquae immerſo, aer in ſubjectam aquam penetrat è phiala egrediens, & tantundem aquae in phialam cogit.*

Praeterea vis illa magna ſuctionis facit, ut ſugentis labra cum collo phialae aliquando arctiſſimè cohaereant propter contactum exquiſitiſſimum.

B. As to the firſt clauſe of Mr. *Hobbes*'s account of our phænomenon, the vacuiſts will eaſily anſwer his queſtion, by acknowledging, that there were indeed interſperſed vacuities in the air contained in the vial before the ſuction; but they will add, there was no reaſon, why the water ſhould aſcend to fill them, becauſe, being a heavy body, it cannot riſe of itſelf, but muſt be raiſed by ſome prevalent weight or preſſure, which then was wanting. Beſides, that there being interſperſed vacuities as well in the reſt of the air, that was very near the water, as in that contained in the vial, there was no reaſon, why the water ſhould aſcend to fill the vacuities of one portion of air rather than thoſe of another. But when once by ſuction a great many of the aerial corpuſcles were made to paſs out of the vial, the ſpring of the remaining air being weakened, whilſt the preſſure of the ambient air, which depends upon its conſtant gravity, is undiminiſhed, the ſpring of the internal becomes unable to reſiſt the weight of the external air, which is therefore able to impel the interpoſed water, with ſome violence, into the cavity of the glaſs, until the air, remaining in that cavity, being reduced almoſt to its uſual denſity, is able, by its ſpring, and the weight of the water got up into the vial, to hinder any more water from being impelled up. For, as to what Mr. *Hobbes* affirms, that, *Is qui ſugit phialam neque in ventrem quicquam, neque in pulmones, neque in os quicquam exugit*; how it will agree with what he elſewhere delivers about ſuction, I leave him to conſider. But I confeſs, I cannot but wonder at his confidence, that can poſitively aſſert a thing ſo repugnant to the common ſentiments of men of all opinions, without offering any proof for it. But I ſuppoſe they, that are by trial acquainted with ſucking, and have felt the air come in at their mouths, will prefer their own experience to his authority. And as to what he adds that the perſon, that ſucks, agitates the air, and turns it within the vial into a kind of circulating wind, that endeavours every where to get out; I wiſh, he had ſhewn us by what means a man, that ſucks, makes this odd commotion of the air; eſpecially in ſuch vials as I uſe to employ about the experiment, the orifice of whoſe neck is ſometimes leſs than a pin's head.

A. THAT there may be really air extracted by ſuction out of a glaſs, methinks you might argue from an experiment I ſaw you make with a receiver, which was exhauſted by your pump, and conſequently by ſuction. For I remember, when you had counterpoiſed it with very good ſcales, and afterwards by turning a ſtop-cock, let in the outward air, there ruſhed in as much air to fill the ſpace, that had been deſerted by the air pumped out, as weighed ſome ſcruples (conſiſting of twenty grains apiece) though the receiver were not of the largeſt ſize.

B. You did well to add that clauſe; for, the *Magdeburgic* experiment, mentioned by the induſtrious *Schottus*, having been made with a vaſt receiver, the re-admitted air amounted to a whole ounce and ſome drachms. But to return to Mr. *Hobbes*. I fear not,

not, that he will perſuade you, that have ſeen the experiment he recites, that as ſoon as the neck of the vial is unſtopped under water, the air, that whirled about before, makes a ſally out, and forces in as much water. For, if the orifice be any thing large, you will, inſtead of feeling an endeavour to thruſt away your finger, that ſtopped it, find the pulp of your finger ſo thruſt inward, that a *Peripatetick* would affirm, that he felt it ſucked in. And that intruſion may be the reaſon, why the lip of him, that ſucks, is oftentimes ſtrongly faſtened to the orifice of the vial's neck, which Mr. *Hobbes* aſcribes to a moſt exquiſite contact, but without clearly telling us, how that extraordinary contact is effected. And when your finger is removed, inſtead of perceiving any air go out of the vial through the water, (which, if any ſuch thing happen, you will eaſily diſcover by the bubbles,) you ſhall ſee the water briſkly ſpring up in a ſlender ſtream to the top of the vial, which it could not do, if the cavity were already full of air. And to let you ſee, that when the air does really paſs in or out of the vial immerſed under water, it is very eaſy to perceive its motions, if you dip the neck of vial in water, and then apply to the globulous part of it either your warm hands or any other competent heat, the internal air being rarified; you ſhall ſee a portion of it, anſwerable to the degree of heat you applied, manifeſtly paſs through the water in ſucceſſive bubbles, whilſt yet you ſhall not ſee any water get into the vial to ſupply the place deſerted by that air. And if, when you have (as you may do by the help of ſucking) filled the neck and part of the belly of the vial with water, you immerſe the orifice into ſtagnant water, and apply warm hands to the globulous part as before, you will find the water in the vial to be driven out, before any bubbles paſs out of the vial into the ſurrounding water; which ſhews, that the air is not ſo forward to dive under the water, (and much leſs under ſo ponderous a liquor as quickſilver,) as Mr. *Hobbes* has ſuppoſed.

A. THAT it is the preſſure of the external air, that (ſurmounting the ſpring of the internal) drives up the water into the vial we have been ſpeaking of, does, I confeſs, follow upon your hypotheſis: but an experimentarian philoſopher, as Mr. *Hobbes* calls you among others, may poſſibly be furniſhed with an experiment to confirm this to the eye.

B. You bring into my mind what I once deviſed to confirm my hypotheſis about ſuction, but found a while ſince, that I had omitted it in my diſcourſe about that ſubject. And therefore I ſhall now repeat to you the ſubſtance at leaſt of the memorial, that was written of that experiment, by which the great intereſt of the weight of the atmoſpherical air in ſuction will appear, and in which alſo ſome things will occur, that will not well agree with Mr. *Hobbes*'s explication, and prevent ſome of his allegations againſt mine.

A. HAVING not yet met with an experiment of this nature, ſuch an one, as you ſpeak of, will be welcome to me.

B. WE took a glaſs bubble, whoſe long ſtem was both very ſlender and very cylindrical; then by applying to the outſide of the ball or globulous part a convenient heat, we expelled ſo much of the air, as that, when the end of the pipe was dipped in water, and the inward air had time to recover its former coolneſs, the water aſcended either to the top of the pipe or very near it. This done, we gently and warily rarified the air in the cavity of the bubble, 'till by its expanſion it had driven out almoſt all the water, that had got up into the ſtem, that ſo it might attain, as near as could be, to that degree of heat and meaſure of expanſion, that it had when the water began to riſe in it. And we were careful to leave two or three drops of water unexpelled at the bottom of the pipe, that we might be ſure, that none of the included air was by this ſecond rarefaction driven out at the orifice of it; as the depreſſion of the water ſo low aſſured us, on the other

other ſide, that the included air wanted nothing conſiderable of the expanſion it had when the water began to aſcend into the pipe. Whilſt the air was in this rarified ſtate, we preſently removed the little inſtrument out of the ſtagnant water into ſtagnant quickſilver, which in a ſhort time began to riſe in the pipe. Now, if the aſcenſion of the liquor were the effect of nature's abhorrence of a vacuum; or of ſome internal principle of motion; or of the compreſſion and propagated pulſion of the outward air by that, which had been expelled, why ſhould not the mercury have aſcended to the top of the pipe, as the water did before? But *de facto* it did not aſcend half, or perhaps a quarter ſo far; and if the pipe had been long enough, as well as it was ſlender enough, I queſtion, whether the mercury would have aſcended (in proportion to the length of the ſtem) half ſo high as it did.

Now of this experiment, which we tried more than once, I ſee not, for the reaſon lately expreſſed, how any good account will be given without our hypotheſis, but according to that it is clear.

A. I think I perceive, why you ſay ſo; for the aſcenſion of liquors being an effect of the prevalency of the external air's preſſure againſt the reſiſtance it meets with in the cavity of the inſtrument, and the quickſilver being bulk for bulk many times heavier than water, the ſame ſurpluſage of preſſure, that was able to impel up water to the top of the pipe, ought not to be able to impel up the quickſilver to any thing near that height. And if it be here objected, as it very plauſibly may be, that the raiſed cylinder of mercury was much longer than it ought to have been in reference to a cylinder of water, the proportion in gravity between thoſe two liquors (which is almoſt that of fourteen to one) being conſidered; I anſwer, that when the cylinder of water reached to the pipe, the air poſſeſſed no more than the cavity of the globulous part of the inſtrument, being very little aſſiſted to dilate itſelf by ſo light a cylinder as that of water: but when the quickſilver came to be impelled into the inſtrument by the weight of the external air, that pondrous body did not ſtop its aſcent as ſoon as it came to be equiponderant to the formerly expelled cylinder of water; becauſe, to attain that height, it reached but a little way into the pipe, and left all the reſt of the cavity of the pipe to be filled with part of that air, which formerly was all ſhut up in the cavity of the bubble; by which means the air, included in the whole inſtrument, muſt needs be in a ſtate of expanſion, and thereby have its ſpring weakened, and conſequently diſabled to reſiſt the preſſure of the external air, as much as the ſame included air did before, when it was leſs rarified; on which account, the undiminiſhed weight or preſſure of the external air was able to raiſe the quickſilver higher and higher, till it had obtained that height, at which the preſſure, compounded of the weight of the mercurial cylinder, and the ſpring of the internal air (now leſs rarified than before,) was equivalent to the preſſure of the atmoſphere or external air.

B. You have given the very explication I was about to propoſe; wherefore I ſhall only add, that, to confirm this experiment by a kind of inverſion of it, we drove by heat a little air out of the bubble, and dipped the open end of the pipe into quickſilver, which by this means we made to aſcend, till it had filled about a fourth part, or leſs, of the pipe, when that was held erected. Then carefully removing it without letting fall any quickſilver, or letting in any air, we held the orifice of the pipe a little under the ſurface of a glaſs full of water, and applying a moderate heat to the outſide of the ball, we warily expelled the quickſilver, yet leaving a little of it to make it ſure, that no air was driven out with it; then ſuffering the included air to cool, the external air was found able to make the water not only aſcend to the very top of the pipe, and thence ſpread itſelf a little into the cavity of the ball, but to carry up before it the quickſilver, that had remained unexpelled at the bottom of the ſtem. And if in making

ing the experiment we had firſt raiſed, as we ſometimes did, a greater quantity of quickſilver, and afterwards drove it out, the quantity of water, that would be impelled into the cavity of the pipe and ball, would be accordingly increaſed.

A. IN this experiment it is manifeſt, that ſomething is driven out of the cavity of the glaſs, before the water or quickſilver begins to aſcend in it: and here alſo we ſee not, that the air can paſs through the pores of quickſilver or water, but that it drives them on before it, without ſenſibly mixing with them. In this experiment there appears not at all any circular wind, as Mr. *Hobbes* fancies in the ſucked vial we are diſputing of, nor any tendency outwards of the included air upon the account of ſuch a wind; but, inſtead of theſe things, that the aſcenſion of the liquors into the cavity of the pipe depends upon the external air, preſſing up the liquors into that cavity, may be argued by this, that the ſame weight of the atmoſphere impelled up into the pipe ſo much more of the lighter liquor, water, than of the heavier liquor, mercury.

B. You have ſaid enough on this experiment; but it is not the only one I have to oppoſe to Mr. *Hobbes*'s explication: for, that there is no need of the ſallying of air out of a vial, to make the atmoſpherical air preſs againſt a body, that cloſes the orifice of it, when the preſſure of the internal air is much weakened; I have had occaſion to ſhew ſome virtuoſi, by ſucking out, with the help of an inſtrument, a conſiderable portion of the air contained in a glaſs; for having then, inſtead of unſtopping the orifice under water, nimbly applied a flat body to it, the external air preſſed that body ſo forcibly againſt it, as to keep it faſtened and ſuſpended, though it were clogged with a weight of many ounces.

A. ANOTHER experiment of yours Mr. *Hobbes*'s explication brings into my mind, by which it appears, that, if there be ſuch a circular wind, as he pretends, produced by ſuction in the cavity of the vial, it muſt needs be ſtrangely laſting. For I have ſeen more than once, that, when you have by an inſtrument ſucked much of the air out of a vial, and afterwards carefully cloſed it, though you kept the ſlender neck of it ſtopped a long time, perhaps for ſome weeks or months, yet when it was opened under water, a conſiderable quantity of the liquor would be briſkly impelled up into the neck and belly of the vial. So that, though I will not be ſo pleaſant with Mr. *Hobbes*, as to remind you on this occaſion of thoſe writers of natural magick, that teach us to ſhut up articulate ſounds in a veſſel, which being tranſported to a diſtant place, and opened there, will render the words, that are committed to it; yet I muſt needs ſay, that ſo laſting a circular wind, as, according to Mr. *Hobbes*, your experiments exhibited, may well deſerve our wonder.

B. YOUR admiration would perchance increaſe, if I ſhould aſſure you, that, having with the ſun-beams produced ſmoke in one of thoſe well-ſtopped vials, this circular wind did not at all appear to blow it about, but ſuffered it to riſe, as it would have done if the included air had been very calm. And now I ſhall add but one experiment more, which will not be liable to ſome of the things, as invalid as they are, which Mr. *Hobbes* has alledged in his account of the vial, and which will let you ſee, that the weight of the atmoſpherical air is a very conſiderable thing; and which may alſo incline you to think, that, whilſt Mr. *Hobbes* does not admit a ſubtiler matter, than common air, to paſs through the pores of cloſe and ſolid bodies, the air, he has recourſe to will ſometimes come too late to prevent a vacuum. The experiment, which was partly accidental, I lately found regiſtered to this ſenſe, if not in theſe words: [Having, to make ſome diſcovery of the weight of the air, and for other purpoſes, cauſed an æolipile, very light, conſidering its bulk, to be made by a famous artiſt, I had occaſion to put it ſo often into the fire for ſeveral trials, that at length the copper ſcaled off by degrees, and left the veſſel much thinner than when it firſt came out of the artificer's tificer's hands; and a good while after, this change in the inſtrument being not in my thoughts, I had occaſion to employ it, as formerly, to weigh how many grains it would contain of the air at ſuch a determinate conſtitution of the atmoſphere, as was to be met with, where I then chanced to be. For the making this experiment the more exactly, the air was, by a ſtrong but warily applied fire, ſo carefully driven away, that, when clapping a piece of ſealingwax to the pin-hole, at which it had been forced out, we hindered any communication betwixt the cavity of the inſtrument and the external air, we ſuppoſed the æolipile to be very well exhauſted, and therefore laid it by, that, when it ſhould be grown cold we might, by opening the orifice with a pin, again let in the outward air, and obſerve the encreaſe of weight, that would thereupon enſue: but the inſtrument, that, as I was ſaying, was grown thin, had been ſo diligently freed from air, that the very little that remained, and was kept by the wax from receiving any aſſiſtance from without, being unable, by its ſpring, to aſſiſt the æolipile to ſupport the weight of the ambient air; this external fluid did, by its weight, preſs againſt it ſo ſtrongly, that it compreſſed it, and thruſt it ſo conſiderably inwards, and in more than one place ſo changed its figure, that, when I ſhewed it to the virtuoſi, that were aſſembled at *Greſham-college*, they were pleaſed to command it of me to be kept in their repoſitory, where I preſume it is ſtill to be ſeen.

OF THE CAUSE OF ATTRACTION by SUCTION, A PARADOX.

PREFACE.

HAVING, about twelve years ago, ſummarily expreſſed and publiſhed my opinion of the cauſe of ſuction, and a while before, or after, brought to the Royal Society the glaſs inſtrument I employed to make it out; I deſiſted for ſome time to add any thing about a problem, that I had but occaſionally handled: only, becauſe the inſtrument, I mentioned in my Examen of Mr. *Hobbes*'s opinion, and afterwards uſed at *Greſham-college*, was difficult enough to be well made, and not to be procured ready made, I did, for the ſake of ſome virtuoſi, that were curious of ſuch things, deviſe a ſlight and eaſy made inſtrument, deſcribed in the following tract, chap. iv. in which the chief phænomena, I ſhewed before the Society, were eaſily producible. But afterwards the miſtakes and erroneous opinions, that in print, as well as in diſcourſe, I met with, even among the learned men, about ſuction, and the curioſity of an ingenious perſon, engaged me to reſume that ſubject and treat of it, as if I had never before meddled with it, for the reaſon intimated in the beginning of the enſuing paper. And finding, upon the review of my latter Animadverſions on Mr. *Hobbes*'s *Problemata de Vacuo*, that ſome paſſages of this tract are referred to there, I ſaw myſelf thereby little leſs than engaged to annex that diſcourſe to thoſe animadverſions.

madverſions. And this I the rather conſented to, becauſe it contains ſome experiments, that I have not elſewhere met with, which, together with ſome other parts of that eſſay, may, I hope, prove of ſome uſe to illuſtrate and confirm our doctrine about the weight and ſpring of the air, and ſupply the leſs experienced than ingenious friends to our hypotheſis, with more grounds of anſwering the latter objections of ſome learned men, againſt whoſe endeavours I perceive it will be uſeful to employ variety of experiments and other proofs, to evince the ſame truth; that ſome or other of theſe may meet with thoſe arguments or evaſions, with which they ſtrive to elude the force of the reſt.

The title of the following eſſay may ſufficiently keep the reader from expecting to find any other kind of attraction diſcourſed of, than that which is made by ſuction. But yet thus much I ſhall here intimate in general, that I have found by trials purpoſely made, that the examples of ſuction are not the only noted ones of attraction, that may be reduced to pulſion.

OF THE

CAUSE of ATTRACTION by SUCTION.

CHAP. I.

I MIGHT, Sir, ſave myſelf ſome trouble in giving you that account you deſire of me about ſuction, by referring you to a paſſage in my Examen, I long ſince writ, of Mr. *Hobbes's Dialogus Phyſicus de Natura Aeris*, if I knew you had thoſe two books lying by you. But becauſe I ſuſpect, that my Examen may not be in your hands, ſince it is almoſt out of print, and has not for ſome years been in my own; and becauſe I do not ſo well remember, after ſo long a time, the particulars, that I writ there about ſuction, as I do in general, that the hypotheſis I propoſed, was very incidentally and briefly diſcourſed of, upon an occaſion miniſtered by a wrong explication given of ſuction by Mr. *Hobbes*, I ſhall here decline referring you to what I there writ; and propoſing to you thoſe thoughts about ſuction, that I remember I there pointed at, I ſhall annex ſome things to illuſtrate and confirm them, that would not have been ſo proper for me to have inſiſted on in a ſhort, and but occaſional excurſion.

And I ſhould immediately proceed to what you expect from me, but, that ſuction being generally looked upon as a kind of attraction, it will be requiſite for me to premiſe ſomething about attraction itſelf. For, beſides that the cauſe of it, which I here diſpute not of, is obſcure, the very nature and notion of it is wont by naturaliſts to be either left untouched, or but very darkly delivered, and therefore will not be unfit to be here ſomewhat explained.

How general and ancient ſoever the common opinion may be, that attraction is a kind of motion quite differing from pulſion, if not alſo oppoſite to it; yet I confeſs, I concur in opinion, though not altogether upon the ſame grounds, with ſome modern naturaliſts, that think attraction a ſpecies of pulſion. And at leaſt among inanimate bodies I have not yet obſerved any thing, that convinces me, that attraction cannot be reduced to pulſion; for, theſe two ſeem to me to be but extrinſical denominations of the ſame local motion, in which, if a moved body precede the movent, or tend to acquire a greater diſtance from it, we call it pulſion; and if, upon the ſcore of the motion, the same body follow the movent or approach to it, we call it attraction. But this difference may consist but in an accidental respect, which does not physically alter the nature of the motion, but is founded upon the respect, which the line, wherein the motion is made, happens to have to the situation of the movent. And that, which seems to me to have been the chief cause of men's mistaking attraction for a motion opposite to pulsion, is, that they have looked upon both the moving and moved bodies in too popular and superficial a manner, and considered in the movent rather the situation of the conspicuous and more bulky part of the animal or other agent, than the situation of that part of the animal, or instrument, that does immediately impress that motion upon the mobile.

For those, that attentively heed this, may easily take notice, that some part of that body, or of the instrument, which by reason of their conjunction in this operation is to be looked on but as making one with it, is really placed behind some part of the body to be drawn, and therefore cannot move outwards itself without thrusting that body forward. This will be easily understood, if we consider, what happens when a man draws a chain after him; for though his body do precede the chain, yet his finger or some other part of the hand, wherewith he draws it, has some part or other, that reaches behind the fore part of the first link, and the hinder part of this link comes behind the anteriour part of the second link; and so each link has one of its parts placed behind some part of the link next after it, till you come to the last link of all. And so, as the finger, that is in the first link, cannot move forwards, but it must thrust on that link, by this series of trusions the whole chain is moved forwards; and if any other body be drawn by that chain, you may perceive, that some part of the last link comes behind some part of that body, or of some intervening body, which, by its cohesion with it, ought in our present case to be considered as part of it. And thus attraction seems to be but a species of pulsion, and usually belongs to that kind of it, which, for distinction's sake, is called trusion, by which we understand that kind of pulsion, wherein the movent goes along with the moved body, without quitting it, whilst the progress lasts; as it happens, when a gardener drives his wheel-barrow before him without letting go his hold of it.

But I must not here dissemble a difficulty, that I foresee may be speciously urged against this account of attraction. For it may be said, that there are attractions, where it cannot be pretended, that any part of the attrahent comes behind the attracted body; as in magnetical and electrical attractions, and in that, which is made of water, when it is drawn up into springs and pumps.

I need not tell you, that you know so well, as that partly the Cartesians, and partly other modern philosophers, have recourse on this occasion, either to screwed particles and other magnetical emissions, to explicate phænomena of this kind. And, according to such hypothesis, one may say, that many of these magnetical and electrical effluvia come behind some parts of the attracted bodies, or at least of the little solid particles, that are, as it were, the walls of their pores, or procure some discussion of the air, that may make it thrust the moveable towards the loadstone or amber, &c. But if there were none of these, nor any other subtil agents, that cause this motion by a real, though unperceived pulsion; I should make a distinction betwixt other attractions and these, which I should then stile attraction by invisibles. But, whether there be really any such in nature, and why I scruple to admit things so hard to be conceived, may be elsewhere considered. And you will, I presume, the freelier allow me this liberty, if (since in this place it is proper to do it,) I shew you, that in the last of the instances I formerly objected, (that of the drawing up of water into the barrel of a syringe,) there is no attraction of the liquor made by the external air. I say then, that by the ascending rammer, as a part of which I here

here consider the obtuse end, plug or sucker, there is no attraction made of the contiguous and subjacent water, but only there is room made for it, to rise into, without being exposed to the pressure of the superior air. For, if we suppose the whole rammer to be by divine omnipotence annihilated, and consequently uncapable of exercising any attraction; yet, provided the superior air were kept off from the water by any other way, as well as it was by the rammer, the liquor would as well ascend into the cavity of the barrel; since (as I have elsewhere abandantly proved) the surface of the terraqueous globe being continually pressed on by the incumbent air or atmosphere, the water must be, by that pressure, impelled into any cavity here below, where there is no air to resist it; as by our supposition there is not in the barrel of our syringe, when the rammer, or whatever else was in it, had been annihilated. Which reasoning may be sufficiently confirmed by an experiment, whereby I have more than once shewn some curious persons, that, if the external air, and consequently its pressure, be withdrawn from about the syringe, one may pull up the sucker as much as he pleases, without drawing up after it the subjacent water. In short, let us suppose, that a man standing in an inner room does by his utmost resistance keep shut a door, that is neither locked nor latched, against another, who with equal force endeavours to thrust it open; in this case, as if one should forcibly pull away the first man, it could not be said, that this man, by his recess from the door he endeavoured to press outwards, did truly and properly draw in his antagonist, though upon that recess the coming in of his antagonist would presently ensue; so it cannot properly be said, that by the ascent of the rammer, which displaces the superior air, either the rammer itself, or the expelled air, does properly attract the subjacent water, though the ingress of that liquor into the barrel does thereupon necessarily ensue. And that, as the comparison supposes, there is a pressure of the superior air against the upper part of the sucker, you may easily perceive, if having well stopped the lower orifice of the syringe with your finger, you forcibly draw up the sucker to the top of the barrel. For if then you let go the rammer, you will find it impelled downwards by the incumbent air with a notable force.

CHAP. II.

HAVING thus premised something in general about the nature of attraction, as far as it is necessary for my present design; it will be now seasonable to proceed to the consideration of that kind of attraction, that is employed to raise liquors, and is by a distinct name called suction.

ABOUT the cause of this there is great contention between the New Philosophers, as they are stiled, and the Peripateticks. For the followers of *Aristotle*, and many learned men, that in other things dissent from him, ascribe the ascension of liquors upon suction to nature's abhorrence of a vacuum. For, say they, when a man dips one end of a straw, or reed into stagnant water, and sucks at the other end, the air contained in the cavity of the reed passes into that of his lungs, and consequently the reed would be left empty, if no other body succeed in the place it deserts; but there are only (that they take notice of) two bodies, that can succeed, the air and the (grosser liquor) the water; and the air cannot do it, because of the interposition of the water, that denies it access to the immersed orifice of the reed, and therefore it must be the water itself, which accordingly does ascend to prevent a vacuum detested by nature.

BUT many of the modern philosophers, and generally all the Corpuscularians, look upon this *Fuga Vacui* as but an imaginary cause of suction, though they do it upon very differing grounds. For, the atomists, that willingly admit of vacuities, properly so called, both within and without our world, cannot think, that nature hates or fears a vacuum, and declines her usual course to prevent it: And the Cartesians, though they do, as well as the Peripateticks, deny, that there is a vacuum, yet since they affirm not only, that there is none *in rerum natura*, but that there can be none, because what others call an empty space having three dimensions, hath all, that they think belonging to the essence of a body, they will not grant nature to be so indiscreet, as to strain herself to prevent the making of a thing, that is impossible to be made.

THE Peripatetick opinion about the cause of suction, though commonly defended by the schools, as well modern as ancient, supposes in nature such an abhorrence of a vacuum, as neither has been well proved, nor does well agree with the lately discovered phænomenon of suction. For, according to their hypothesis, water and other liquors should ascend upon suction to any height to prevent a vacuum, which yet is not agreeable to experience. For I have carefully tried, that by pumping with a pump far more stanch than those, that are usually made, and indeed as well closed as we could possibly bring it to be, we could not by all our endeavours, raise water by suction to above 33 1-half foot. The Torricellian experiment shews, that the weight of the air is able to sustain, and some of our experiments shew, it is able to raise a mercurial cylinder equal in weight to as high a cylinder of water, as we were able to raise by pumping. For mercury being near 14 times as heavy as water of the same bulk, if the weight of the air be equivalent to that of a mercurial cylinder of 29 or 30 inches, it must be able to counterpoise a cylinder of water near fourteen times as long, that is, from thirty-four to near thirty-six foot. And very disagreeable to the common hypothesis, but consonant to ours, is the experiment, that I have more than once tried, and I think elsewhere delivered, namely; that, if you take a glass pipe of about three foot long, and, dipping one end of it in water, suck at the other, the water will be suddenly made to flow briskly into your mouth. But, if instead of water you dip the lower end into quicksilver, though you suck as strongly as ever you can, provided, that in this case, as in the former, you hold the pipe upright, you will never be able to suck up the quicksilver near so high as your mouth; so that if the water ascended upon suction to the top of the same pipe, because else there would have been a vacuum left in the cavity of it, why should not we conclude, that, when we have sucked up the quicksilver as strongly as we can, as much of the upper part of the tube, as is deserted by the air, and yet not filled by the mercury, admits, in part at least, a vacuum, (as to air) of which consequently nature cannot reasonably be supposed to have so great and unlimited an abhorrency, as the Peripateticks and their adherents presume. Yet I will not determine, whether there be any more than many little vacuities, or spaces devoid of air, in the cavity, so called, of the pipe unfilled by the mercury, (so that the whole cavity is not one entire empty space; it being sufficient for my purpose, that my experiment affords a good argument *ad hominem* against the Peripateticks, and warrants us to seek for some other cause than the *fuga vacui*, why a much stronger suction, than that, which made water ascend with ease into the sucker's mouth, will not also raise quicksilver to the same height or near it.

See Cont. of Phys. Mech. Exp. the 15th Exp.

THOSE modern philosophers, that admit not the *fuga vacui* to be the cause of the raising of liquors in suction, do generally enough agree in referring it to the action of the sucker's thorax. For, when a man endeavours to suck up a liquor, he does by means of the muscles enlarge the cavity of his chest, which he cannot do, but at the same time he must thrust away those parts of the ambient air, that were contiguous to his chest, and the displaced air does, according to some learned men, (therein, if I mistake not, followers of *Gassendus*,) compress the contiguous air, and that the next to to it, and ſo outwards, till the preſſure, ſucceſſively paſſing from one part of the air to the other, arrive at the ſurface of the liquor; and all other places being as to ſenſe full, the impelled air cannot find place, but by thruſting the water into the room made for it in the pipe, by the receſs of the air, that paſſed into the ſucker's lungs. And they differed not much from this explication, that, without taking in the compreſſion of the ambient air made by the thorax, refer the phænomenon to the propagated motion or impulſe, that is impreſſed on the air diſplaced by the thorax in its dilatation, and yet unable to move in a world perfectly filled, as they ſuppoſe ours to be, unleſs the liquor be impelled into as much of the cavity of the pipe, as faſt as it is deſerted by the air, that is ſaid to be ſucked up. But though I readily confeſs this explication to be ingenious, and ſuch as I wonder not they ſhould acquieſce in, who are acquainted but with the long known, and obvious phænomena of ſuction; and though I am not ſure, but that in the moſt familiar caſes the cauſes aſſigned by them may contribute to the effect; yet, preſerving for *Carteſius* and *Gaſſendus* the reſpect I willingly pay ſuch great philoſophers, I muſt take the liberty to tell you, that I cannot acquieſce in their theory. For I think, that the cauſe of ſuction they aſſign, is in many caſes not neceſſary, in others not ſufficient. And firſt, as to the condenſation of the air by the dilatation of the ſucker's cheſt; when I conſider the extent of the ambient air, and how ſmall a compreſſion no greater an expanſion than that of the thorax is like to make, I can ſcarce think ſo ſlight a condenſation of the free air can have ſo conſiderable an operation on the ſurface of the liquor to be raiſed, as the hypotheſis I examine requires: and that this impulſe of the air by a ſucker's dilated thorax, though it be wont to accompany the aſcenſion of the water procured by ſuction, yet is not of abſolute neceſſity to it, will, I preſume, be eaſily granted, if it can be made out, that even a propagated pulſion, abſtracted from any condenſation of air, is not ſo neceſſarily the cauſe of it, but that the effect may be produced without it. For ſuppoſe, that by divine omnipotence ſo much air, as is diſplaced by the thorax, were annihilated; yet I ſee not, why the aſcenſion of the liqour ſhould not enſue. For, when a man begins to ſuck, there is an æquilibrium, or rather æquipollency between the preſſure, which the air, contained in the pipe, (which is ſhut up with the preſſure of the atmoſphere upon it,) has, by virtue of its ſpring, upon that part of the ſurface of the water, that is environed by the ſides of the pipe, and the preſſure, which the atmoſpherical air has, by virtue of its weight, upon all the reſt of the ſurface of the ſtagnant water; ſo that, when by the dilatation of the ſucker's thorax, the air within the cavity of the pipe comes to be rarified, and conſequently loſe of its ſpring, the weight of the external air continuing in the mean time the ſame, it muſt neceſſarily happen, that the ſpring of the internal air will be too weak to compreſs any longer the gravitation of the external, and conſequently, that part of the ſurface of the ſtagnant water, that is included in the pipe, being leſs preſſed upon, than all the other parts of the ſame ſurfaces muſt neceſſarily give way, where it can leaſt reſiſt, and conſequently be impelled up into the pipe, where the air, having had its ſpring weakened by expanſion, is no longer able to reſiſt, as it did before. This may be illuſtrated by ſomewhat varying an inſtance already given, and conceiving, that within a chamber three men thruſt all together with their utmoſt force againſt a door, (which we ſuppoſe to have neither bolt nor latch) to keep it ſhut, at the ſame time the three other men have juſt equal ſtrength, and employ their force to thruſt it open. For though, whilſt their oppoſite endeavours are equal, the door will continue to be kept ſhut, yet if one of the three men, within the room, ſhould go away, there will need no new force, nor other acceſſion of ſtrength to the three men, to make them prevail and thruſt open the door againſt

againſt the reſiſtance of thoſe, that endeavoured to keep it ſhut, who are now but two.

AND here (upon the by) you may take notice, that, to raiſe water in ſuction, there is no neceſſity of any rarified and forcibly ſtretched rope, as it were, of the air, to draw up the ſubjacent water into the pipe, ſince the bare debilitation of the ſpring of the included air may very well ſerve the turn. And though, if we ſhould ſuppoſe the air within the pipe to be quite annihilated, it could not be pretended (ſince it would not have ſo much as exiſtence) that it exerciſes an attractive power; yet in this caſe the water would aſcend into the pipe, without the aſſiſtance of nature's imaginary abhorrence of a vacuum, but by a mechanical neceſſity, plainly ariſing from this, that there would be a preſſure of the incumbent atmoſphere upon the reſt of the ſurface of the ſtagnant water, and no preſſure at all upon that part of the ſurface, that is within the pipe, where conſequently there could be no reſiſtance made to the aſcenſion of the water, every where elſe ſtrongly urged by the weight of the incumbent air.

I ſhall add on this occaſion, that, to ſhew ſome inquiſitive men, that the weak reſiſtance within a veſſel, that had but one orifice expoſed to the water, may much more contribute to the aſcenſion of that liquor into the veſſel, than either the compreſſion, or the continued or reflected impulſe of the external air; I thought fit to produce a phænomenon, which by the beholders was without ſcruple judged an effect of ſuction, and yet could not be aſcribed to the cauſe of ſuction, aſſigned by either of the ſects of philoſophers I diſſent from. The experiment was this: by a way, elſewhere delivered, the long neck of a glaſs bubble was ſealed up, and almoſt all the air had been by heat driven out of the whole cavity of the bubble or vial, and then the glaſs was laid aſide for ſome hours, or as long as we pleaſed; afterwards the ſealed apex of the neck was broken off under water. I demand now of a Peripatetic, whether the liquor ought to be ſucked or drawn into the cavity of the glaſs, and why? If he ſays, as queſtionleſs he will, that the water would be attracted to hinder a vacuum, he would thereby acknowledge, that, till the glaſs was unſtopped under water, there was ſome empty ſpace in it; for, till the ſealed end was broken off, the water could not get in, and therefore, if the *fuga vacui* had any thing to do in the aſcenſion, the liquor muſt riſe, not to prevent an empty ſpace, but to fill one, that was made before. Nor does our experiment much more favour the other philoſophers I diſſent from; for in it there is no dilatation made of the ſides of the glaſs, as in ordinary ſuction there is made of the thorax, but only there is ſo much air driven out of the cavity of the bubble, into whoſe room ſince neither common air nor water is permitted to ſucceed, it appears not, how the propagated and returning impulſe, or the circle of motion, as to common air and water, does here take place. And then I demand, what becomes of the air, that has been by heat driven out, and is by the hermetical ſeal kept out of the cavity of the bubble? If it be ſaid, that it diffuſes itſelf into the ambient air, and mingles with it, that will be granted, which I contended for, that ſo little air, as is uſually diſplaced in ſuction, cannot make any conſiderable compreſſion of the free ambient air; for, what can one cubic inch of air, which is ſometimes more than one of our glaſſes contains, do, to the condenſation ſo much as of all the air in the chamber, when the expelled corpuſcles are evenly diſtributed among thoſe of the ambient? And how comes this inconſiderable condenſation to have ſo great an effect in every part of the room, as to be able there to impel into the glaſs as much water in extent, as the whole air, that was driven out of the cavity of it? But if it be ſaid, that the expelled air condenſed only the contiguous or very neighbouring air, it is eaſy to anſwer, that it is no way probable, that the expelled particles of the air ſhould not, by the differing motions of the ambient air, be quickly made to mingle with it, but ſhould rather wait (which, if it did, we ſometimes made it do for many hours)

hours) till the veſſels, whence it was driven out, were unſtopped again. But, though this could probably be pretended, it cannot truly be aſſerted. For if you carry the ſealed glaſs quite out of the room or houſe, and unſtop it at ſome other place, though two or three miles diſtant; the aſcenſion of the water will (as I found by trial) nevertheleſs enſue; in which caſe I preſume, it will not be ſaid, that the air, that was expelled out of the glaſs, and condenſed the contiguous or near contiguous air, attended the bubble in all its motions, and was ready at hand to impel in the water, as ſoon as the ſealed apex of the vial was broken off. But I doubt not, but moſt of the embracers of the opinion I oppoſe, being learned and ingenious perſons, if they had been acquainted with theſe and the like phænomena, would rather have changed their opinion about ſuction, than have gone about to defend it by ſuch evaſions, which I ſhould not have thought worth propoſing, if I had not met with objections of this nature publickly maintained by a learned writer, on occaſion of the air's ruſhing into the exhauſted Magdenburgic engine. But as in our experiment theſe objections have no place, ſo in our hypotheſis the explication is very eaſy, as will anon be intimated.

CHAP. III.

HAVING thus ſhewn, that the aſcenſion of water upon ſuction may be cauſed otherwiſe than by the condenſation or the propagated pulſion of air contiguous to the ſucker's thorax, and thruſt out of place by it; it remains, that I ſhew, (which was one of the two things I chiefly intended) that there may be caſes, wherein the cauſe, aſſigned in the hypotheſis I am examining, will not have place. But this will be better underſtood, if, before I proceed to the proof of it, I propoſe to you the thoughts, I had many years ſince, and do ſtill retain, about the cauſe of the aſcenſion of liquors in ſuction.

To clear the way to the right underſtanding of the enſuing diſcourſe, it will not be amiſs here to premiſe a ſummary intimation of ſome things, that are ſuppoſed in our hypotheſis.

WE ſuppoſe then firſt, without diſputing either the exiſtence or the nature of elementary air, that the common air we breathe in, and which I often call atmoſpherical air, abounds with corpuſcles not devoid of weight, and endowed with elaſticity or ſpringineſs, whereby the lower parts, compreſſed by the weight of the upper, inceſſantly endeavour to expand themſelves, by which expanſion, and in proportion to it, the ſpring of the air is weakened, (as other ſprings are wont to be) the more they are permitted to ſtretch themſelves.

NEXT, we ſuppoſe, that the terraqueous globe, being environed with this gravitating and ſpringy air, has its ſurface and the bodies placed on it, preſſed by as much of the atmoſphere, as either perpendicularly leans on them, or can otherwiſe come to bear upon them. And this preſſure is, by the Torricellian and other experiments, found to be equivalent to a perpendicularly erected cylinder of about twenty-nine or thirty inches of quickſilver, (for the height is differing, as the gravity of the atmoſphere happens to be various.)

LASTLY, we ſuppoſe, that air being contained in a pipe or other hollow body, that has but one orifice open to the free air, if this orifice be hermetically ſealed, or otherwiſe (as with the mouth of one, that ſucks) cloſed, the now included air, whilſt it continues without any farther expanſion, will have an elaſticity equivalent to the weight of as much of the outward air, as did before preſs againſt it. For if the weight of the atmoſphere, to which it was then expoſed, had been able to compreſs it further, it would have done ſo, and then the cloſing of the orifice, at which the internal and external air communicated, as it fenced the included air from the preſſure of the incumbent, ſo it hindred the ſame included air from expanding itſelf; ſo that, as it was ſhut up with the preſſure of the atmoſphere upon it, that is in a ſtate of as great compreſſion, as the weight of the atmoſphere could bring it to, ſo, being ſhut up, and thereby kept from weakening that preſſure by expanſion, it muſt retain a ſpringineſs equipollent to the preſſure it was expoſed to before, which (as I juſt now noted) was as great, as the weight of the incumbent pillar of the atmoſphere could make it. But if as was ſaid in the firſt ſuppoſition, the included air ſhould come to be dilated or expanded, the ſpring being then unbent, its ſpring, like that of other elaſtical bodies, would be debilitated anſwerably to that expanſion.

To me then it ſeems, that, ſpeaking in general, liquors are upon ſuction raiſed into the cavities of pipes and other hollow bodies, when, and ſo far as there is a leſs preſſure on the ſurface of the liquor, in the cavity, that on the ſurface of the external liquor, that ſurrounds the pipe, whether that preſſure on thoſe parts of the external liquor, that are from time to time impelled up into the orifice of the pipe, proceed from the weight of the atmoſphere, or the propagated compreſſion, or impulſe of ſome parts of the air, or the ſpring of the air, or ſome other cauſe, as the preſſure of ſome other body quite diſtinct from air.

UPON the general view of this hypotheſis, it ſeems very conſonant to the mechanical principles. For, if there be on the differing parts of the ſurface of a fluid body unequal preſſures, it is plain, as well by the nature of the thing, as by what has been demonſtrated by *Archimedes*, and his commentators, that the greater force will prevail againſt the leſſer, and that that part of the water's ſurface muſt give way, where it is leaſt preſſed. So that that, wherein the hypotheſis I venture to propoſe to you, differs from that, which I diſſent from, is not, that mine is leſs mechanical; but partly in this, that, whereas the hypotheſis, I queſtion, ſuppoſes a neceſſity of the protruſion or impulſe of the air, mine does not require that ſuppoſition, but, being more general, reaches to other ways of procuring the aſcenſion of liquors, without raiſing them by the impulſe of the air; and partly, and indeed chiefly, in that the hypotheſis, I decline, makes the cauſe of the aſcenſion of liquors to be only the increaſed preſſure of the air external to the pipe; and I chiefly make it to depend upon the diminiſhed preſſure of the air within the pipe, on the ſcore of the expanſion it is brought to by ſuction.

To proceed now to ſome experiments, that I made in favour of this hypotheſis, I ſhall begin with that which follows:

WE took a glaſs-pipe bended like a ſyphon, but ſo, that the ſhorter leg was as parallel to the longer, as we could get it made, and was hermetically ſealed at the end: into this ſyphon we made a ſhift (for it is not very eaſy) to convey water, ſo that the crooked part being held downwards, the liquor reached to the ſame height in both the legs, and yet there was about an inch and a half of uncompreſſed air ſhut up in the ſhorter leg. This little inſtrument (for it was but about fifteen inches long) being thus prepared, it is plain, that according to the hypotheſis I diſſent from, there is no reaſon, why the water ſhould aſcend upon ſuction. For, though we ſhould admit, that the external air were conſiderably compreſſed, or received a notable impulſe, when the ſucker's cheſt is enlarged; yet in our caſe, that compreſſion, or protruſion, will not reach the ſurface of the water in the ſhorter leg, becauſe it is there fenced from the action of the external air by the ſides of the glaſs, and the hermetical ſeal of the top, and yet, if one ſucked ſtrongly at the open orifice in the longer leg, the water in the ſhorter would be depreſſed; and that in the longer aſcended at one ſuck about an inch and half: of which the reaſon is clear in our hypotheſis. For, the ſpring of the included air, together with the weight of the water in the ſhorter leg, and the preſſure of the atmoſpherical air, aſſiſted by the weight of the liquor in the longer leg,

counter-

counter-balanced one another before the suction began: But when afterwards upon suction, the air in the longer leg came to be dilated, and thereby weakened, it was rendered unable to resist the undiminished pressure of the air included in the shorter leg, which consequently expanding itself by virtue of its elasticity, depressed the contiguous water, and made it proportionably rise in the opposite leg, till, by the expansion, its spring being more and more weakened, it arrived at an equipollency with the gravitation or pressure of the atmosphere. Which last clause contains the reason, why, when the person, that sucked, had raised the water in the longer leg, less than three inches higher by repeated endeavours to suck, and that without once suffering the water to fall back again, he was not able to elevate the water in the longer, so much as three inches above its first station. And if in the shorter leg, there was but an inch and a quarter of space left for the air unfilled by the water, by divers skilfully reiterated acts of suction, he could not raise the liquor in the longer leg above two inches; because, by that time, the air included in the shorter leg, had, by expanding itself further and further, proportionably weakened its spring, till at length it became as rarified, as was the air in the cavity of the longer leg, and consequently was able to thrust away the water with no more force than the air in the long leg was able to resist. And by the recited trial it appeared, that the rarefaction usually made of air by suction is not near so great, as one would expect, probably, because by the dilatation of the lungs, the air being still shut up, is but moderately rarified, and the air in the longer leg, can by them, be brought to no greater degree of rarity, than that of the air within the chest. For, whereas the included air in our instrument was not expanded, by my estimate, at one suck, to above the double of its former dimensions, and by divers successive sucks was expanded but from one inch and an half, to less than four inches and an half, if the suction could have been conveniently made with a great and staunch syringe, the rarefaction of the air would probably have been far greater; since in our pneumatick engine air may, without heat, and by a kind of suction, be brought to possess many hundreds of times the space it took up before. From this rarefaction of the air in both the legs of our instrument proceeds another phænomenon, readily explicable by our hypothesis. For if, when the water was impelled up as high as the suction could raise it, the instrument were taken from the sucker's mouth, the elevated water would with violence return to its wonted station. For, the air, in both the legs of the instrument, having by the suction lost much of the spring, and so of its power of pressing; when once the orifice of the longer leg was left open, the atmospherical air came again to gravitate upon the water in that leg, and the air, included in the other leg, having its spring debilitated by the precedent expansion, was not able to hinder the external air from violently repelling the elevated water, till the included air was thrust into the space it possessed before the suction; in which space it had density and elasticity enough to resist the pressure, that the external air exercised against it through the interposed water.

But our hypothesis about the cause of suction would not need to be sollicitously proved to you by other ways, if you had seen what I have sometimes been able to do in our pneumatick engine. For, there we found, by trials purposely devised, and carefully made, that a good syringe being so conveyed into our receiver, that the open orifice of the pipe or lower part was kept under water; if the engine were exhausted, though the handle of the syringe were drawn up, the water would not follow it, which yet it would do, if the external air were let in again. The reason of which is plain in our hypothesis; for the air, that should have pressed upon the surface of the stagnant water, having been pumped out, there was nothing to impel up the water into the deserted cavity of the syringe, as there was, when the receiver was filled with air.

CHAP. IV.

BUT becauſe ſuch a conveniency as our engine, and the apparatus neceſſary for ſuch trials, are not eaſily procurable, I ſhall endeavour to confirm our hypotheſis about ſuction, by ſubjoining ſome experiments, that may be tried without the help of that engine, for the making out theſe three things:

I. THAT a liquor may be raiſed by ſuction, when the preſſure of the air, neither as it has weight nor elaſticity, is the cauſe of the elevation.

II. THAT the weight of the atmoſpherical air is ſufficient to raiſe up liquors in ſuction.

III. THAT, in ſome caſes, ſuction will not be made, as, according to the hypotheſis I diſſent from, it ſhould, although there be a dilatation of the ſucker's thorax, and no danger of a vacuum, though the liquor ſhould aſcend.

AND firſt, to ſhew, how much the riſing of liquors in ſuction depends upon the weight or preſſure of the impellent body, and how little neceſſity there is, where that preſſure is not wanting, that, in the place deſerted by the liquor, that is ſucked there ſhould ſucceed air, or ſome other viſible body, as the Peripatetic ſchools would have it; to ſhew this, I ſay, I thought on the following experiments. We took a glaſs pipe, fit to have the Torricellian experiment made with it, but a good deal longer than was neceſſary for that uſe: this pipe being hermetically ſealed at one end, the other end was ſo bent, as to be reflected upwards, and make as it were the ſhorter leg of the ſyphon as parallel as we could to the longer, ſo that the tube now was ſhaped like an inverted ſyphon, with legs of a very unequal length. This tube, notwithſtanding its inconvenient figure, we made a ſhift (for it is not eaſily done) to fill with mercury, when it was in an inclined poſture, and then erecting it, the mercury ſubſided in the longer leg, as in the Torricellian experiment, and attained to between two foot and a quarter and two foot and an half above the ſurface of the mercury in the ſhorter leg, which in this inſtrument anſwers to the ſtagnant mercury in an ordinary barometer, from which to diſtinguiſh it, I have elſewhere called this ſyphon, furniſhed with mercury, a travelling baroſcope, becauſe it may be ſafely carried from place to place. Out of the ſhorter leg of this tube, we warily took as much mercury, as was thought convenient for what we had further to do; and this we did by ſuch a way, as to hinder any air from getting into the deſerted cavity of the longer leg, by which means the mercurial cylinder (eſtimated as I lately mentioned) retained the ſame height above the ſtagnant mercury in the ſhorter: the upper and cloſed part of this travelling baroſcope, you will eaſily grant to have been free from common air, not only for other reaſons, that have been given elſewhere, but particularly for this, that if you gently incline the inſtrument, the quickſilver will aſcend to the top of the tube; which you know it could not do, if the place, formerly deſerted by it, were poſſeſſed by the air, which, by its ſpring, would hinder the aſcenſion of the mercury (as is eaſy to be tried.) The inſtrument having been thus fitted, I cauſed one of the by-ſtanders to ſuck at the ſhorter leg, whereupon (as I expected) there preſently enſued an aſcenſion of four or five inches of mercury in that leg, and a proportionable ſubſidence of the mercury in the longer, and yet in this caſe the raiſing of the mercury cannot be pretended to proceed from the preſſure of the air. For the weight of the atmoſphere is fenced off by that, which cloſes the upper end of the longer tube, and the ſpring of the air has here nothing to do, ſince, as we have lately ſhewn, the ſpace deſerted by the mercury is not poſſeſſed by the included air, and the pulſion or condenſation of the air, ſuppoſed, by divers modern philoſophers, to be made by the dilatation of the ſucker's cheſt,

cheſt, and to preſs upon the ſurface of the liquor, that are to be ſucked up: this, I ſay, cannot here be pretended, in regard the ſurface of the liquor in the longer leg is every way fenced from the preſſure of the ambient air. So that it remains, that the cauſe, which raiſed the quickſilver in the ſhorter leg, upon the newly recited ſuction, was the weight of the collaterally ſuperior quickſilver in the longer leg, which being (at the beginning of the ſuction) equivalent to the weight of the atmoſphere, there is a plain reaſon, why the ſtagnant mercury, in the ſhorter leg, ſhould be raiſed ſome inches by ſuction; as mercury, ſtagnant in an open veſſel, will be raiſed by the weight of the atmoſphere, when the ſuction is made in the open air. For in both caſes, there is a pipe, that reaches to the ſtagnant mercury, and a competent weight to impel it into the pipe; when the air in the cavity of the pipe has its ſpring weakened by the dilatation, that accompanied ſuction.

THE ſecond point formerly propoſed, which is, that the weight of the air is ſufficient to raiſe liquors in ſuction, may not be ill proved by arguments, legitimately drawn from the Torricellian experiment itſelf, and much more clearly by the firſt and fifteenth of our continued phyſico-mechanical experiments. And therefore I ſhall only here take notice of a phænomenon, that may be exhibited by the travelling baroſcope, which, though it be much inferior to the experiments newly referred to, may be of ſome uſe on the preſent occaſion.

HAVING then provided an inſtrument like the travelling baroſcope, mentioned under the former head, but whoſe legs were not ſo unequally long, and having in it made the Torricellian experiment, after the manner lately deſcribed, we ordered the matter ſo, that there remained in the ſhorter leg the length of divers inches unfilled with ſtagnant mercury. Then I cauſed one, verſed in what he was to do, ſo to raiſe the quickſilver by ſuction to the open orifice of the ſhorter leg, that the orifice being ſeaſonably and dexterouſly cloſed, the mercury continued to fill that leg, as long as we thought fit; and then, having put a mark to the ſurface of the mercury in the longer leg, we unſtopped the orifice of the ſhorter; whereupon the mercury, that before filled it, was depreſſed, till the ſame liquor in the longer leg was raiſed five inches or more above the mark, and continued at that height. I ſaid, that the mercury, that had been raiſed by ſuction, was depreſſed, rather than that it ſubſided, becauſe its own weight could not here make it fall, ſince a mercurial cylinder of five inches was far from being able to raiſe ſo tall a cylinder of mercury, as made a counterpoiſe in the longer leg; and therefore the depreſſion we ſpeak of, is to be referred to the gravitation of the atmoſpherical air upon the ſurface of the mercury in the ſhorter leg: and I ſee no cauſe to doubt, but that, if we could have procured an inſtrument, into whoſe ſhorter leg a mercurial cylinder of many inches higher could have been ſucked up, it would, by this contrivance, have appeared, that the preſſure of the atmoſphere would eaſily impel up a far taller cylinder of mercury, than it did in our recited experiment.

THAT this is no groundleſs conjecture, may appear probable by the experiment you will preſently meet with. For, if the gravity of an incumbent pillar of the atmoſphere be able to compreſs a parcel of included air, as much as a mercurial cylinder, equivalent in weight to between thirty and five and thirty foot of water, is able to condenſe it, it cannot well be denied, that the ſame atmoſpherical cylinder may be able, by its weight, to raiſe and counter-balance eight or nine and twenty inches of quickſilver, or an equivalent pillar of water in tubes, where the reſiſtance of theſe two liquors, to be raiſed and ſuſtained by the air, depends only upon their own unaſſiſted gravity.

To confirm our doctrine of the gravitation of the atmoſphere upon the ſurface of the liquors expoſed to it, I will ſubjoin an experiment, that I deviſed to ſhew, that the incumbent air, in its natural or usual state, would compress other air not rarified, but in the like natural state, as much as a cylinder of eight or nine and twenty inches of mercury would condense or compress it.

IN order to the making of this, I must put you in mind of what I have shewn elsewhere * at large, and shall further confirm by one of the experiments, that follows the next; namely, that about twenty-nine or thirty inches of quicksilver will compress air, that being in its natural or usual state (as to rarity and density) has been shut up in the shorter leg of our travelling or syphon-like baroscope, into half the room, that included air possessed before. This premised, I pass on to my experiment, which was this:

WE provided a travelling baroscope, wherein the mercury in the longer leg was kept suspended by the counterpoise of the air, that gravitated on the surface of the mercury in the shorter leg, which we had so ordered, that it reached not by about two inches to the top of the shorter leg. Then making a mark at the place, where the stagnant mercury rested, it was manifest, according to our hypothesis, that the air in the upper part of the shorter leg was in its natural state, or of the same degree of density with the outward air, with which it freely communicated at the open orifice of the the shorter leg; so that this stagnant air was equally pressed upon by the weight of the collaterally superior cylinder of mercury in the longer leg, and the equivalent weight of a directly incumbent pillar of the atmosphere. Things being in this posture, the upper part of the shorter leg, which had been before purposely drawn out to an almost capillary smallness, was hermetically sealed, which, though the instrument was kept erected, was so nimbly done by reason of the slenderness of the pipe, that the included air did not appear to be sensibly heated, though for greater caution we staid a while from proceeding, that, if any rarefaction had been produced in the air, it might have time to lose it again. This done, we opened the lower end of the longer leg, (which had been so ordered before, that we could easily do it, and without concussion of the vessel,) by which means the atmospherical air, gaining access to the mercury included in the longer leg, did, as I expected, by its gravitation upon it, so compress the air included in the shorter leg, that, according to the estimate we made with the help of a ruler, (for by reason of the conical figure of the upper part of the glass we could not take precise measures,) it was thrust into near half the room it took up before, and consequently, according to what I put you lately in mind of, endured a compression like that, which a mercurial cylinder of about twenty nine inches would have given it.

THIS experiment, as to the main of it, was for greater caution made the second time with much the like success; and though it had been more easy to measure the condensation of the air, if, instead of drawing out and sealing up the shorter leg of the instrument, we had contented ourselves to close it some other way; yet we rather chose to employ *Hermes*'s seal, lest, if any other course had been taken, it might be pretended, that some of the included air, when it began to be compressed, might escape out at the not perfectly and strongly closed orifice of the leg, wherein it was imprisoned.

To make it yet further appear, how much the ascension of liquors by suction depends upon pressure, rather than upon nature's imaginary abhorrence of a vacuum, or the propagated pulsion of the air; I will subjoin an instance, wherein that presumed abhorrence cannot be pretended. This experiment was thus made:

A glass-siphon, like those lately described, with one leg far longer than the other, was hermetically sealed at the shorter leg, and then by degrees there was put in, at the orifice

* See the Author's Defence of the Doctrine touching the Spring and Weight of the Air, against *Fr. Linus*, ch. v.

orifice of the longer leg, as much quicksilver, as by its weight sufficed to compress the air in the shorter leg into about half the room it possessed before; so that, according to the Peripatetick doctrine, the air must be in a state of preternatural condensation, and, that to a far greater degree, than (as I have tried) it is usually brought to by cold, intense enough to freeze water. Then measuring the heighth of the quicksilver in the longer tube above the superficies of that in the shorter, we found it not exceed thirty inches. Now, if liquors did rise in suction *ob fugam vacui*, there is no reason, why this quicksilver in the longer part of the siphon should not easily ascend upon suction, at least till the air in the shorter leg had regained its former dimensions, since it cannot in this place be pretended, that, if the mercury should ascend, there would be any danger of a vacuum in the shorter leg of the tube, in regard, that the contiguous included air is ready at hand to succeed, as fast as the mercury subsides in the shorter leg of the siphon. Nor can it be pretended, that, to fill the place deserted by the quicksilver, the included air must suffer a preternatural rarefaction or descension; since it is plain in our case, that on the contrary, as long as the air continues in the state, whereto the weight of the quicksilver has reduced it, it is kept in a violent state of compression; since in the shorter leg it was in its natural state, when the mercury, poured into the longer leg, did by its weight thrust it into about half the room it took up before. And yet, having caused several persons, one of them versed in sucking, to suck divers times as strongly as they could, they were neither of them able, not so much as for a minute of an hour, to raise the mercury in the longer leg, and make it subside in the shorter for more than about an inch at most. And yet to shew you, that the experiment was not favourably tried for me, the height of the mercurial cylinder in the longer leg above the surface of that in the shorter leg was, when the suction was tried, an inch or two shorter than thirty inches, and the compressed air in the shorter leg was so far from having been by the exsuction expanded beyond its natural and first dimensions, that it did not, when the contiguous mercury stood as low as we could make it subside, regain so much as one half of the space it had lost by the precedent compression, and consequently was in a preternatural state of condensation, when it had been freed from that state as far as suction would do it. Whence it seems evident, that it was not *ob fugam vacui*, that the quicksilver did upon suction ascend one inch; for, upon the same score it ought to have ascended two, or perhaps more inches, since there was no danger, that by such an ascension any vacuum should be produced or left in the shorter leg of the siphon; whereas, according to our hypothesis, a clear cause of the phænomenon is assignable. For, before the suction was begun, there was an æquilibrium, or equipollency, between the weight of the superior quicksilver in the longer leg, and a spring of the compressed air included in the shorter leg; but when the experimentor began to suck, his chest being widened, part of the air included in the upper part of the longer leg passed into it, and that, which remained, had by that expansion its pressure so weakened, that the air in the shorter leg, finding no longer the former resistance, was able by its own spring to expand itself, and consequently to depress the contiguous mercury in the same shorter leg, and raise it as much in the longer.

BUT here a hydrostatician, that heedfully marks this experiment, may discern a difficulty, that may perhaps somewhat perplex him, and seems to overthrow our explication of the phænomenon. For he may object, that if the compressed air in the shorter leg had a spring equipollent to the weight of the mercury in the longer leg, it appears not, why the mercury should not be sucked up in this instrument, as well as in the free air; since, according to me, the pressure of the included air upon the subjacent mercury must be equivalent to the weight of the atmosphere, and yet experience shews,

ſhews, that the weight of the atmoſphere will, upon ſuction, raiſe quickſilver to the height of ſeveral inches.

To clear this difficulty, and ſhew, that, though it be conſiderable, it is not at all inſuperable, be pleaſed to conſider with me, that I make indeed the ſpring of the compreſſed air to be equipollent to the weight of the compreſſing mercury, and I have a manifeſt reaſon to do it; becauſe, if the ſpring of the air were not equipollent to that weight, the mercury muſt neceſſarily compreſs the air farther, which it is granted *de facto* not to do. But then I conſider, that in our caſe there ought to be a great deal of difference between the operation of the ſpring of the included air and the weight of the atmoſphere, after ſuction has been once begun. For the weight of the atmoſphere, that impels up mercury and other liquors, when the ſuction is made in the open air, continues ſtill the ſame; but the force or preſſure of the included air is equal to the counterpreſſure of the mercury, no longer than the firſt moment of the ſuction; after which, the force of the impriſoned air ſtill decreaſes more and more, ſince this compreſſed air, being further and further expanded, muſt needs have its ſpring proportionably weakened; ſo that it need be no wonder, that the mercury was not ſucked up any more than we have related; for there was nothing to make it aſcend to a greater height, than that, at which the debilitated ſpring of the (included but) expanded air was brought to an equipollency with the undiminiſhed, and indeed ſomewhat encreaſed weight of the mercurial cylinder in the longer leg, and the preſſure of the aerial cylinder in the ſame leg, leſſened by the action of him, that ſucked. For whereas, when the orifice of this leg ſtood open, the mercury was preſſed on by a cylinder of the atmoſpherical air, equivalent to about thirty inches of quickſilver; by the mouth and action of him that ſucked, the tube was freed from the external air, and by the dilatation of his thorax, the neighbouring air, that had a free paſſage through his wind-pipe to it, was proportionably expanded, and had its ſpring and preſſure weakened: by which means, the compreſſed air in the ſhorter leg of the ſyphon was enabled to impel up the mercury, until the lately mentioned equilibrium or equipollency was attained. And I muſt here take notice, that, as the quickſilver was raiſed by ſuction but a little way, ſo the cylinder, that was raiſed, was a very long one; whereas, when mercury is ſucked up in the free air, it is ſeldom raiſed to half that length; though, as I noted before, the impellent cauſe, which is the weight of the atmoſphere, continued ſtill the ſame, whereas in our ſyphon, when the mercury was ſucked up but an inch, the compreſſed air, poſſeſſing double the ſpace it did before, had by this expanſion already loſt a very conſiderable part of its former ſpring and preſſure.

I ſhould here conclude this diſcourſe, but that I remember a phænomenon of our pneumatick engine, which to divers learned men, eſpecially Ariſtotelians, ſeemed ſo much to argue, that ſuction is made either by a *fuga vacui*, or ſome internal principle, that divers years ago I thought fit to ſet down another account of it, and lately meeting with that account among other papers, I ſhall ſubjoin it juſt as I found it, by way of appendix to the foregoing Tract.

AMONG the more familiar phænomena of the *Machina Boyliana*, as they now call it, none leaves ſo much ſcruple in the minds of ſome ſorts of men, as this, that when one's finger is laid cloſe upon the orifice of the little pipe, by which the air is wont to paſs from the receiver into the exhauſted cylinder, the pulp of the finger is made to enter a good way into the cavity of the pipe, which doth not happen without a conſiderable ſenſe of pain in the lower part of the finger. For moſt of thoſe, that are ſtrangers to hydroſtaticks, eſpecially if they be prepoſſeſſed with the opinions generally received, both in the Peripatetick and other ſchools, perſuade themſelves, that they feel the newly mentioned and painful protuberance of the pulp of the finger, to be effected, not by preſſure, as we would have it, but diſtinctly by attraction.

To this we are wont to anſwer, that common air being a body not devoid of weight, the phænomenon is clearly explicable by the preſſure of it: for, when the finger is firſt laid upon the orifice of the pipe, no pain nor ſwelling is produced, becauſe the air, which is in the pipe, preſſes as well againſt that part of the finger, which covereth the orifice, as the ambient air doth againſt the other parts of the ſame finger. But when by pumping, the air in the pipe, or the moſt part of it, is made to paſs out of the pipe into the exhauſted cylinder, then there is nothing left in the pipe, whoſe preſſure can any thing near countervail the undiminiſhed preſſure of the external air on the other parts of the finger; and conſequently, that air thruſts the moſt yielding and fleſhy part of the finger, which is the pulp, into that place, where its preſſure is unreſiſted, that is, into the cavity of the pipe, where this forcible intruſion cauſeth a pain in thoſe tender parts of the finger.

To give ſome viſible illuſtration of what we have been ſaying, as well as for other purpoſes, I thought on the following experiment.

We took a glaſs pipe of a convenient length, and open at both ends, whoſe cavity was near about an inch in diameter, (ſuch a determinate breadth being convenient, though not neceſſary;) to one of the ends of this pipe we cauſed to be firmly tied on a piece of very fine bladder, that had been ruffled and oiled, to make it both very limber and unapt to admit water; and care was taken, that the piece of bladder tied on ſhould be large enough, not only to cover the orifice, but to hang looſe ſomewhat beneath it.

This done, we put the covered end of the pipe into a glaſs body, or cucurbit, purpoſely made more than ordinarily tall, and the pipe being held in ſuch manner, as that the end of it reached almoſt, but not quite, to the bottom of the glaſs body, we cauſed water to be poured, both into this veſſel, and into the pipe (at its upper orifice, which was left open) that the water might aſcend equally enough, both without and within the pipe. And when the glaſs body was full of water, and the ſame liquor was level to it, or a little higher within the pipe, the bladder at the lower orifice was kept plump, becauſe the water within the pipe did, by its weight, preſs as forcibly downwards, as the external water in the large glaſs endeavoured to preſs it inwards and upwards.

All this being done, we cauſed part of the water in the pipe to be taken out of it, (which may be done either by putting in and drawing out a piece of ſpunge or of linnen, or more expeditiouſly by ſucking up part of the water with a ſmaller pipe to be immediately after laid aſide;) upon which removal of part of the internal water, that, which remained in the pipe, being no longer able, by reaſon of its want of weight, to preſs againſt the inſide of the bladder near as forcibly as it did before, the external water, whoſe weight was not leſſened, preſſed the ſides and bottom of the bladder, whereto it was contiguous into the cavity of the pipe, and thruſted it up therein ſo ſtrongly, that the diſtended bladder made a kind of either thimble or hemiſphere within the pipe. So that here we have a protuberance, like that above-mentioned of the finger, effected by pulſion, not attraction; and in a caſe, where there can be no juſt pretence of having recourſe to nature's abhorrence of a vacuum, ſince the upper orifice of the pipe being left wide open, the air may paſs in and out without reſiſtance.

The like ſwelling of the bladder in the pipe we could procure without taking out any of the internal liquor, by thruſting the pipe deeper into the water; for then the external liquor having, by reaſon of its increaſe of depth, a greater preſſure on the outſide

outside of the bladder, than the internal liquor had on the inside of it, the bladder must yield to the stronger pressure, and consequently be impelled up.

If the bladder lying loose at the lower end of the pipe, the upper end were carefully closed with one's thumb, that the upper air might not get out, until the experimentor thought fit, and if the thus closed pipe were thrust almost to the bottom of the water, the bladder would not be protuberant inwards, as formerly; because the included air, by virtue of its spring, resisted from within the pressure of the external water against the outside of the bladder: but if the thumb, that stopped the pipe's upper orifice, were removed, the formerly compressed air having liberty to expand itself, and its elasticity being weakened thereby, the external water would with suddenness and noise enough, not to be unpleasant to the spectators, drive up the bladder into the cavity of the pipe, and keep it there very protuberant.

To obviate an objection, that I foresaw might be brought in by persons not well versed in hydrostaticks, I caused the pipe fore-mentioned, or such another, to be so bent near the lower end, as that the orifice of it stood quite on one side, and the parts of the pipe made an angle as near to a right one, as he, that blew it, could bring it to. This lower orifice being fitted with a bladder and the pipe, with its contained liquor, being thrust under water after the former manner, the lateral pressure of the water forced the bladder into the short and horizontal leg, and made it protuberate there, as it had done when the pipe was straight.

Lastly, that the experiment might appear not to be confined to one liquor; instead of water, we put into the unbent pipe, as much red wine (whose colour would make it conspicuous) as was requisite to keep the bladder somewhat swelling outwards, when it was somewhat near the bottom of the water; and then it was manifest, that, according as we had foreseen, the superficies of the red liquor in the pipe was a good deal higher than that of the external water, and if the depth of both liquors were proportionably lessened, the difference of height betwixt the two surfaces would indeed, as it ought to happen, decrease, but still the surface of the wine would be the higher of the two, because, being lighter in specie than the common water, the æquilibrium between the pressures of the two liquors upon the bladder would not be maintained, unless a greater height of wine compensated its defect of specifick gravity. And if the pipe was thrust deeper into the water, then the bladder would be made protuberant inwards, as when the pipe had water in it. By which it appears, that these phænomena, without recourse to attraction, may be explicated barely by the laws of the æquilibrium of liquors.

NEW

NEW EXPERIMENTS

ABOUT THE

PRESERVATION of BODIES in VACUO BOYLIANO.

PREFACE.

MY willingness to make the bulk of the papers about the hidden qualities of the air less inconsiderable, by things, that were of affinity to the subject, inducing me to tumble over some of my *adversaria*, I met among them with divers loose notes, or short memorials of some experiments I made several years ago (and some of a fresher date) about the preservation of bodies by excluding the air. Wherefore I was easily persuaded to subjoin these to the additional experiments last recited. For it seems not yet clear, by what manifest quality the exclusion of the air should so much contribute to keep from putrefaction variety of bodies, that are usually found very much disposed to it. And therefore, till the cause of this preservation be further penetrated, it may not be altogether impertinent to mention some experiments relating to it. And though these be only such, as come now to hand, and were most of them set down rather as notes than relations; yet being faithfully registered, and most of them having been made in *Vacuo Boyliano* (as they call it) they will probably be new, and so perhaps not altogether useless to naturalists, who may vary them, and requite me for them, by trying the same experiments, I made by the removal of the air, by the bare exclusion of adventitious air. For sometimes through haste I did not, and sometimes for want of conveniency I could not try, whether the same phænomena would appear, if the same bodies were shut up with air in them, provided they were diligently kept from all commerce with the air about them.

EXPERIMENT I.

A PIECE of roasted rabbet, being exactly closed up in an exhausted receiver the sixth of *November*, was two months, and some few days after taken out, without appearing to be corrupted, or sensibly altered in colour, taste, or smell.

EXPERIMENT II.

A SMALL glass-receiver, being half filled with pieces of white-bread, (part crust and part crumb) was exhausted, and secured the eleventh of *March*: the receiver being opened the first of *April*, part of the bread was shaken out, and appeared not to have been considerably, if at all sensibly, impaired in that time, save, that the outside of some pieces of crumb seemed to be a little, and but a little, less soft and white than before. There appeared no drops, or the least dew on the inside of the glass. The remaining bread was again secured soon after.

THE eighteenth of *April*, the bread was taken out again, and tasted much as it did the last time, the crust being also soft, and no drops of water appearing on the inside of the glass.

EXPERIMENT III.

THIS day (being the ninth of *March*) I opened a small exhausted and secured receiver, wherein, about the ninth of *December*, that is, about three mouths ago, we had

had included ſome milk: upon opening an acceſs to the air, we found the milk well coloured, and turned partly into a kind of whey, and partly into a kind of ſoft curd. The taſte was not offenſive, only a little ſouriſh like whey, and the ſmell was not at all ſtinking, but ſomewhat like that of ſouriſh milk.

EXPERIMENT IV.

THE violet-leaves, that were put up, and freed and ſecured from air the fifth of *March*, being this day opened, (*April* the ſeventh) appeared not to have changed their ſhape, or colour, or conſiſtence: for, as for their odour, it could not be well judged of, becauſe he that included them had, for his own eaſe, contrary to my expreſs direction, cruſhed many of them together in thruſting them down; and by ſuch a violation of their texture, it is natural for violets to loſe their fragrancy, and acquire an earthly ſmell.

EXPERIMENT V.

HAVING carefully placed ſome violets in an exhauſted receiver, of a convenient ſize and bigneſs, and ſecured it from immediate commerce with the external air; the ſeventh month after we looked upon them again, and found they were not putrified, or reſolved into any mucilaginous ſubſtance, but kept their ſhape entire, ſome of them retaining their colour, but more of them having ſo loſt it, as to look like white violets.

EXPERIMENT VI.

NOVEMBER the fifth, we conveyed into a convenient ſhaped receiver ſome ounces of ſheep's-blood, taken from an animal, that had been killed that afternoon. And after the exhauſtion of the air, during which ſtore of bubbles were generated in the liquor, that made it ſwell notably, the included blood was kept in a place, (whoſe warmth we judged equal to that of a digeſtive furnace) for twenty days; for one or two of the firſt of which, the blood ſeem to continue fluid, and of a florid colour, which afterwards degenerated into one, that tended more to blackneſs. On the twenty-fifth of *November*, we came to let in the external air, and found it to ruſh into the receiver, and the glaſs containing the blood, being held in a lightſome place, the moſt part of the bottom of it ſeemed to be thinly overlaid with a coagulated ſubſtance, of a higher colour than that, which ſwam above it, which yet, though it appeared dark, and almoſt blackiſh in the glaſs, whilſt it was looked on in the bulk, yet, if it was ſhook, thoſe parts of it, that fell down along the inſide of the glaſs, appeared of a deep, but fair colour. But whilſt the blood continued in the glaſs, it was ſuppoſed not to ſtink, ſince, even when it was poured out, though its ſmell ſeemed to me (whoſe organs of ſmelling are tender) to have I know not what, that was offenſive, yet to others it ſeemed to ſmell but as the blood of a newly killed dog.

EXPERIMENT VII.

SOME cream being put up and ſecured the ſeventeenth of *March*, in an exhauſted receiver, did this day appear to be more thick, and almoſt butter-like at the top (whoſe ſuperficies ſeemed rugged) than otherwiſe; and afterwards by being well ſhaken together in the not inconveniently ſhaped glaſs, was eaſily enough reduced to butter, whoſe butter-milk, by the judgment of thoſe, who were more uſed to deal in it than I, appeared not differing from ordinary butter-milk. And I found it had, like that, a grateful ſourneſs. The butter was judged to be a little ſourer than ordinary, but was not, as they ſpeak, made.

[IN the entry of this experiment, blanks were left for the years; but the tenor of the words, and design of the experiment, and other circumstances, assure me, that the cream continued a year in the vessel.]

EXPERIMENT VIII.

FEBRUARY the eighteenth, we looked again upon three vials, that had been exhausted and secured the fifteenth of *September* last; the one of these had in it some slices of roasted beef, and the other some shivers of white bread, and the last some thin pieces of cheese; all which seemed to be free from putrefaction, and looked much as they did, when they were first put up: wherefore we thought not fit to let the air into the receiver, but left them as they were, to lengthen the designed trial.

EXPERIMENT IX.

FEBRUARY the eighteenth, there was a fourth vial, wherein, about six months before, viz. *August* the twelfth, had been inclosed and secured some July flowers and a rose; and yet these being kept in the same place with the rest, though they seemed a little moist, retained their shape and colour, especially the rose, which looked fresh enough to seem to have been gathered but lately.

N. B. THAT we observed not in any of these four receivers any great drops, or so much as dew in the upper parts, viz. those, that were situated above the included matter.

EXPERIMENT X.

JUNE the fourth, we left some strawberries in an exhausted receiver, and coming to look upon them after the beginning of *November*, we found them to be discoloured, but not altered in shape, nor affording any sign of corruption, by being at all mouldy; wherefore we thought fit to leave them still in the receiver for further trial.

EXPERIMENT XI.

MAY the second, 1669, a piece of roasted beef, secured *September* the fifteenth, appeared to be not at all altered: as did likewise a piece of cheese secured in another receiver; and some pieces of a French rose the same day (*September* the fifteenth) secured in a third.

N. B. THE flowers sealed up *August* the twelfth, 1668, being this day looked upon, appeared fresh, and consequently did so, after having been kept eight months and an half.

EXPERIMENT XII.

THERE was taken beer of eight shillings a barrel, of a year old, near a pint of which, *June* the seventeenth, was put into a convenient shaped glass, and it was afterwards exhausted and secured from the air; the most part of the month of *August* proved extraordinarily hot. Towards the latter end, there was, at several times, great thunder, which made the beer in our cellar, and in most of those of the neighbourhood, turn sour. The first of *September* the beer was opened, but did not seem to have degenerated into any sourness.

EXPERIMENT XIII.

BEING desirous to try, whether the thunder would have such effect upon ale, exactly stopped in glass vessels, as it often has on that liquor in the ordinary wooden casks, I caused some ale, moderately strong, to be put into a conveniently shaped receiver; and had included ſome milk: upon opening an acceſs to the air, we found the milk well coloured, and turned partly into a kind of whey, and partly into a kind of ſoft curd. The taſte was not offenſive, only a little ſouriſh like whey, and the ſmell was not at all ſtinking, but ſomewhat like that of ſouriſh milk.

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Having carefully placed ſome violets in an exhauſted receiver, of a convenient ſize and bigneſs, and ſecured it from immediate commerce with the external air; the ſeventh month after we looked upon them again, and found they were not putrified, or reſolved into any mucilaginous ſubſtance, but kept their ſhape entire, ſome of them retaining their colour, but more of them having ſo loſt it, as to look like white violets.

EXPERIMENT VI.

November the fifth, we conveyed into a convenient ſhaped receiver ſome ounces of ſheep's-blood, taken from an animal, that had been killed that afternoon. And after the exhauſtion of the air, during which ſtore of bubbles were generated in the liquor, that made it ſwell notably, the included blood was kept in a place, (whoſe warmth we judged equal to that of a digeſtive furnace) for twenty days; for one or two of the firſt of which, the blood ſeem to continue fluid, and of a florid colour, which afterwards degenerated into one, that tended more to blackneſs. On the twenty-fifth of *November*, we came to let in the external air, and found it to ruſh into the receiver, and the glaſs containing the blood, being held in a lightſome place, the moſt part of the bottom of it ſeemed to be thinly overlaid with a coagulated ſubſtance, of a higher colour than that, which ſwam above it, which yet, though it appeared dark, and almoſt blackiſh in the glaſs, whilſt it was looked on in the bulk, yet, if it was ſhook, thoſe parts of it, that fell down along the inſide of the glaſs, appeared of a deep, but fair colour. But whilſt the blood continued in the glaſs, it was ſuppoſed not to ſtink, ſince, even when it was poured out, though its ſmell ſeemed to me (whoſe organs of ſmelling are tender) to have I know not what, that was offenſive, yet to others it ſeemed to ſmell but as the blood of a newly killed dog.

EXPERIMENT VII.

Some cream being put up and ſecured the ſeventeenth of *March*, in an exhauſted receiver, did this day appear to be more thick, and almoſt butter-like at the top (whoſe ſuperficies ſeemed rugged) than otherwiſe; and afterwards by being well ſhaken together in the not inconveniently ſhaped glaſs, was eaſily enough reduced to butter, whoſe butter-milk, by the judgment of thoſe, who were more uſed to deal in it than I, appeared not differing from ordinary butter-milk. And I found it had, like that, a grateful ſourneſs. The butter was judged to be a little ſourer than ordinary, but was not, as they ſpeak, made.

3 [In

[IN the entry of this experiment, blanks were left for the years; but the tenor of the words, and design of the experiment, and other circumstances, assure me, that the cream continued a year in the vessel.]

EXPERIMENT VIII.

FEBRUARY the eighteenth, we looked again upon three vials, that had been exhausted and secured the fifteenth of *September* last; the one of these had in it some slices of roasted beef, and the other some shivers of white bread, and the last some thin pieces of cheese; all which seemed to be free from putrefaction, and looked much as they did, when they were first put up: wherefore we thought not fit to let the air into the receiver, but left them as they were, to lengthen the designed trial.

EXPERIMENT IX.

FEBRUARY the eighteenth, there was a fourth vial, wherein, about six months before, viz. *August* the twelfth, had been inclosed and secured some July flowers and a rose; and yet these being kept in the same place with the rest, though they seemed a little moist, retained their shape and colour, especially the rose, which looked fresh enough to seem to have been gathered but lately.

N. B. THAT we observed not in any of these four receivers any great drops, or so much as dew in the upper parts, viz. those, that were situated above the included matter.

EXPERIMENT X.

JUNE the fourth, we left some strawberries in an exhausted receiver, and coming to look upon them after the beginning of *November*, we found them to be discoloured, but not altered in shape, nor affording any sign of corruption, by being at all mouldy; wherefore we thought fit to leave them still in the receiver for further trial.

EXPERIMENT XI.

MAY the second, 1669, a piece of roasted beef, secured *September* the fifteenth, appeared to be not at all altered: as did likewise a piece of cheese secured in another receiver; and some pieces of a French rose the same day (*September* the fifteenth) secured in a third.

N. B. THE flowers sealed up *August* the twelfth, 1668, being this day looked upon, appeared fresh, and consequently did so, after having been kept eight months and an half.

EXPERIMENT XII.

THERE was taken beer of eight shillings a barrel, of a year old, near a pint of which, *June* the seventeenth, was put into a convenient shaped glass, and it was afterwards exhausted and secured from the air; the most part of the month of *August* proved extraordinarily hot. Towards the latter end, there was, at several times, great thunder, which made the beer in our cellar, and in most of those of the neighbourhood, turn sour. The first of *September* the beer was opened, but did not seem to have degenerated into any sourness.

EXPERIMENT XIII.

BEING desirous to try, whether the thunder would have such effect upon ale, exactly stopped in glass vessels, as it often has on that liquor in the ordinary wooden casks, I caused some ale, moderately strong, to be put into a conveniently shaped receiver; and having exhausted the air, and secured a glass vessel, it was put into a quiet, but not cool, place: last week, which was about six weeks after the liquor had been inclosed, there happening some very loud thunder, and our beer, though the cask was kept in a good cellar, being generally noted to have been turned sour after this thunder; I staid yet a day or two longer, that the operation upon our included liquor might be the more certain and manifest; and then, permitting an access to the outward air, we took out the ale, and found it to be good drink, and not at all soured.

COMPARE this with the wish made in the Essay of the great efficacy of effluviums, chap. V. that such an experiment should be tried.

EXPERIMENT XIV.

SEPTEMBER the twenty-first, 1670, some blackberries, included in an exhausted receiver, were opened *June* the twentieth, 1673, and were found free from all mouldiness and ill-scent; only there was found some liquor, that was sour, which being taken out, the berries were secured again.

[AT the same time, was another parcel of the same berries exactly closed up in a receiver, whence the air was not pumped, to try what difference in the event would appear by this variation. But, coming in *October* the eleventh, 1673, to look upon the glass, we found it cracked, and the fruit all covered over with a thick mould. Nor was this the only vessel, wherein trials, made to preserve fruits, without any exhaustion of the air, miscarried.]

OCTOBER the eleventh, 1674, the same berries, being looked upon, appeared to have their colour altered, and much less black than before; but did not appear putrefied by either loss of shape, or by any stinking smell, nor was the least mouldiness observed to be on them, though they had been kept in the same receiver above four years.

THAT *fructus horarii*, especially so tender and juicy ones, should, without any additament, be preserved from putrefaction so many times longer than otherwise they would have lasted; as it is more than would be expected, so it may give hopes, that both odd and useful things of this kind, may be this way performed.

POSTSCRIPT.

THE foregoing experiments, as the memorials themselves declare, were all of them made *in vacuo Boyliano*, nor did I intend to set down any other: but meeting, among those memorials, with a short account of a couple of trials made without the help of our pneumatic engine, I was induced to annex them, because many may make the like, that will not be able to make such as have been hitherto recited. And these two requiring no peculiarly shaped vessels, it it thought, it may prove of some oeconomical, as well as physical use; if it be shewn by experience, that liquors hermetically sealed the ordinary way in common bolt-heads, may be kept from souring very much beyond their usual time of lasting.

JUNE the fourteenth, we put a convenient quantity of good ale into a bolt-head, and sealed it up hermetically; the next year, on the fifth of *July*, we broke off the seal, and found the liquor very good, and without any sensible sourness. The next day it was sealed up again, and set by for thirteen months, at which time the neck of the glass being broken, the ale was found pretty sour, and therefore the trial was prosecuted no farther: so that, though this liquor would not by this way of preservation be kept from souring so long as the wine, to be mentioned in the following experiment, yet even a small quantity of it was preserved good at the least above a year, which is very much longer than ale is wont to keep from souring.

JUNE the fourteenth, 1670, in a large bolt-head was hermetically ſealed up about a pint, by gueſs, of French claret wine, which, when we came to look upon, *July* the fifth, 1671, appeared very clear and high coloured and had depoſited ſtore of feces at the bottom of the glaſs, but faſtened no tartar, that we could perceive to the ſides. Upon the breaking of the ſealed end of the glaſs, the byſtanders thought, that there was an eruption of included air or ſteams, and, above the ſurface of the wine, there appeared, to a pretty height, a certain white ſmoke almoſt like a miſt, and then gradually vaniſhed: the wine continued well-taſted, and was a little rough upon the tongue, but not at all ſour,

THE bolt-head was ſealed up again *July* the ſixth, 1671, and ſo ſet by till *Auguſt* the fifth, 1672, at which time it was opened again, and then the wine did ſtill taſte very well.

JUNE the twenty-ſixth, 1673, the bolt-head, with the ſame claret wine, was opened, and was found very good, and was ſealed up again.

OCTOBER the eleventh, 1674, the ſame claret wine was opened again, and appeared of a good colour, not ſour, but ſeemed ſomewhat leſs ſpirituous than other good claret wine, perhaps becauſe of the cold weather.

THIS, and the foregoing trial about the preſervation of ale, were made in Mr. *Oldenburg*'s houſe, and preſence.

An Account of the Two Sorts of the HELMONTIAN LAUDANUM, together with the Way of the Noble Baron *F. M. Van Helmont*, (Son to the famous *Johannes Baptiſta*) of Preparing his LAUDANUM.

Firſt publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. cvii. p. 147, for *October* 26, 1674.

AS for the Helmontian Laudanum, you may uſe your own liberty in ſuſpecting, the receipts, that go about of it. For the name itſelf ſeems ambiguous to me, who am well informed, that there are two ſorts of the Helmontian Laudanum; the one uſed by the elder *Helmont*, the other by his ſon. The former was as a great ſecret communicated to me by an expert chemiſt, ſent by a German prince to compliment *Johannes Baptiſta Van Helmont*, ſome of whoſe manuſcripts (one of which periſhed in the fire of *London*,) he procured, together with a way of making his Laudanum, which, having received from him fourteen or fifteen years ago, I carefully prepared, and thought my labour ſo well recompenſed by the extraordinary operations it had, not ſo much in my hands, as thoſe of learned phyſicians and others, to whom I preſented portions of it, that I ſhould have thought the chemiſt a benefactor to phyſic, if he would have made publick, or permitted me to publiſh the way of making ſo ſucceſsful a medicine. And though the acceſs to my laboratory was ſo free to ingenious men, who knew ſuch a medicine to be preparing there, that ſome of them might eaſily ſuppoſe themſelves maſters of the ſecret; yet my juſtice to the communicator, who made a great and deſerved benefit of the laudanum, made me take that care to conceal ſome circumſtances, that men may eaſily be much more confident than ſure, that they have

have the right way of making the medicine. Which because I durst not communicate, meeting two years ago with that obliging and very ingenious person, *F. M.* baron *Van Helmont*, son to the famous *Johannes Baptista*, I obtained from him, by word of mouth, some directions about the laudanum he uses, which though he confessed, and I soon perceived to be differing from his father's, yet he seemed to think it not inferior and more parable. But he having, for a certain reason, imparted to me his process only by word of mouth; lest it should slip out of my memory, I soon after committed it to writing, as the particulars I gathered from his writing occurred to me; and at the next season caused the medicine to be prepared in my laboratory, where the progress was often watched in my absence by a very learned and industrious London doctor, who having, at my request, made many trials with it, and in some cases, where other laudanums had been found unavailable, both uses it, and commends it, more than I could expect from so wary and judicious a man. This medicine being somewhat more cheap and easy to be made than the elder *Helmont*'s, the experience of its efficacy made me desire of the younger a permission to communicate it for the publick good, and to prevent those spurious receipts, that go about of the Helmontian laudanum: which request of mine being almost as soon granted as made, I think myself bound both to his own readiness, to oblige the publick, and to acquaint them with his way of making so a considerable medicine, as I practised it; though if I had received his directions in writing, they might have been more full and methodical. But though I perceived, that he sometimes a little varies his preparations; yet that laudanum proving very successful, that was made according to the annexed paper, I think it will not be amiss to keep to that: which I wish could have been published, before the season of the quinces were so far advanced. And I shall the more hope it may come abroad before it be quite too late, if you please to afford it room in the papers, wherewith I am informed you intend this week to gratify the curious:

Laudanum Helmontii Junioris.

TAKE of opium a quarter of a pound, and of the juice of quinces four pound at least **; the opium being cut into very thin slices, and then as it were minced, to reduce it into smaller parts, is to be put into, and well mixed with, the liquor, (first made lukewarm) and fermented with a moderate heat for eight or ten days, rather more than less; then filter* it, and having infused in it of cinnamon, nutmeg and cloves, of each an ounce ||, let them stand three or four days more; if it be a full week, it may be so much the better; then filter § the liquor once more, having let it boil a whalm or two after the spices have been put in: this being done evaporate away the superfluous water to the consistence of an extract, or to what consistence you please.

LASTLY, incorporate very well with it two, or at most three ounces of the best saffron reduced to fine † powder.

ACCORDING to the consistence you desire to have your medicine of, you may order it so, as either to make it up into a mass of pills (in which form I have caused it to be given,) or keep it in a liquid form; but in this later case the evaporation must have been made more sparingly, that after the putting in of the ‡ saffron it may not grow too thick. In this form the dose may be from five or six drops to ten or fewer, according to circumstances; and of the pills a somewhat less quantity is required.

** (For near five pound would perhaps do better.) * (Which circumstance the author often omits, though I do not.) || (The author sometimes uses half an ounce more of each spice.) § Or strain it well through a canvass-bag. † (Sometimes the author instead of the powder makes use of as much extract as can be obtained from that quantity of saffron. ‡ (Or its extract.)

Some

Some CONSIDERATIONS ABOUT THE RECONCILEABLENESS of REASON and RELIGION.

By *T. E.* a LAY-MAN.

To which is annexed by the PUBLISHER,

A DISCOURSE of Mr. *BOYLE*, ABOUT THE POSSIBILITY of the RESURRECTION.

The Publisher to the Reader.

THESE considerations about religion and reafon, delivered by a perfon of an excellent genius and ability to confider the nature of the things he is wont to difcourfe upon, being fallen into my hands, nor being forbidden to publifh them; I thought the fubject fo weighty, and the way of handling it both fo difcrete and folid, that I could not forbear recommending it to the prefs, being fully perfuaded, the publick in general, as well as all perfons in particular, that are concerned for the fafety both of reafon and religion, and confequently for their dignity as they are men, and their noblenefs as they are Chriftians, will find fufficient caufe to be pleafed with the publication of it. To which I have nothing to add but that, whereas at the beginning of the following difcourfe there is mention made of its being to confift of two parts; one, to fhew, that a Chriftian need not lay afide his reafon; and the other, that he is not commanded to do fo: the author thought fit to keep that paper, which concerned the latter, from now accompanying the former, which feems the moft feafonable, and likelieft to make impreffions on that fort of perfons, whom he chiefly defigns to perfuade.

The PREFACE.

IT is the juft grief, and frequent complaint of thofe, that take to heart the concerns of religion, that they fee it now more furioufly affaulted and ftudioufly undermined than ever, not only by the vicious lives of men, but by their licentious difcourfes. I know, there have been vices in the world, as long as there have been men; and it is an obfervation as old as *Solomon*, *Ecclef.* vii. 10. that men are apt to look upon their own times as worfe than thofe, that preceded them. And becaufe I remember too, that in reciting this complaint he difapproves it; I fhall not difpute, whether other ages have been lefs faulty than this we live in: but this I think I may fay with as much truth as grief, that among us here in *England*, the times, to which our memory can reach, have been lefs guilty, than the prefent time is, of a fpreading and bold profanenefs. For though many allowed themfelves to court gold, and cups, and miftreffes,

little

little less than now they do; yet these were still acknowledged to be faults even by those, that committed them, and the precepts and the counsels of religion were neglected or disobeyed, but not their authority thrown off or affronted; men retaining yet such a kind of respect for her, as the elder son in the parable did for his father, when, receiving a command from him to go and work in his vineyard, he answered, "I go, Sir," though he went not, *Matth.* xxi. 30. But now too many of the vicious do not only scandalously violate the laws of religion, but question the truth, and despise the very name of it. They rather chuse to imitate the rebels in the other parable, and say of religion what they did of their lawful king, when they insolently declared, "That they would not have him to reign over them," *Luke* xix. 14. They seek not to hide their sins, like *Adam*, but think either to cover or protect all others, by that greatest of all impiety; and instead of cheating conscience into silence, (as sinners, not impudent, are wont to do,) by deceitful promises of repenting hereafter of their sins, they endeavour to stifle or depose it, by maintaining, that repentance is a weakness of mind, and conscience ought not to be looked on as the vicegerent of a deity, whose very existence or providence they dispute.

And that, which more troubled me, and made me most apprehend the spreading of this impiety, was, that it was propagated in a new way, that made me fear, the arguments not only of vulgar preachers, but even of learned divines themselves, would be much less fit than formerly to give a check to its progress. For, till of late, the generality of our infidels did, either as philologers, question the historical part of the scriptures, and perhaps cavil at some of the doctrines; or, if they employed philosophical arguments, as *Pomponatius* and *Vaninus* did, they borrowed them from *Aristotle*, or the Peripatetick school. And against both these sorts of adversaries, the learneder champions of the Christian religion, such as *Vives*, *Mornay*, and *Grotius*, had furnished divines with good and proper weapons. For, the historical part of the scriptures, and especially the miracles, were strongly confirmable by competent testimonies, and other moral proofs, sufficient in their kind. And *Aristotle* being himself a dark and dubious writer, and his followers being on that account divided into sects and parties, which for the most part had nothing to alledge but his single authority, it was not difficult to answer the arguments drawn from the Peripatetick philosophy; and, if that could not have been done, it had not been difficult to reject the doctrines themselves as false or precarious. But our new libertines take another and shorter way, (though I hope it will not be a more prosperous one,) to undermine religion. For, not troubling themselves to examine the historical or doctrinal parts of Christian theology, in such a way as Jews, Pagans, Mahometans, would do; these deny those very principles of natural theology, wherein the Christian, and those other differing religions agree, and which are supposed in almost all religions, that pretend to revelation, namely, the existence and providence of a Deity, and a future state (after this life is ended.) For, these libertines own themselves to be so upon the account of the Epicurean, or other mechanical principles of philosophy; and, therefore, to press them with the authorities wont to be employed by preachers, is improper, since they are so far from paying any respect to the venerable fathers of the church, that they slight the generality of the heathen philosophers themselves, judging no writers worthy of name, but those, that, like *Leucippus*, *Democritus*, *Epicurus*, &c. explicate things by matter and local motion; and therefore it is not to be expected, that they should reverence any more the Peripatetick arguments of *Scotus* or *Aquinas*, than the homilies of St. *Augustine*, or St. *Chrysostom*; and to give *Aristotle* himself the title of the philosopher, were enough to make some of them conclude the ascriber were no philosopher. And this, by the way, may excuse me for not having brought into the following papers the sentences of the fathers

thers or the moralifts, or the authority of *Ariftotle*, or any of the fchool philofophers, which I fhould have declined to employ, though my frequent removes from place to place, when I was writing thefe papers, had not denied me the convenience of a library.

THINGS being at this pafs, though the title of this difcourfe acknowledges the author of it to be a layman; yet I fhall not beg pardon for the enfuing papers as for an intrenchment upon the ecclefiafticks. For befides that, though I know fome functions, yet I know no truths of religion, that have the peculiarity of the fhew bread under the law, *Matth.* xii. 4. with which it was lawful only for the priefts to meddle; I will not fo far miftruft the charity of churchmen, as not to fuppofe, that they will rather thank than blame any man, that being not altogether a ftranger to this warfare, offers them his affiftance againft the common enemy in fo important a quarrel, and fo great a danger. The fathers, and other divines, being wont to compare the church militant to a fhip, it will not be an improper extenfion of the comparifon, to fay, that, when the veffel is threatened with fhipwreck, or boarded by pirates, it may be the duty, not only of profeffed feamen, but any private paffenger, to lend his helping hand in that common danger. And I wifh I were as fure, that my endeavours will prove fuccefsful, as I am, that fuch churchmen, as I moft efteem, will think them neither needlefs nor unfeafonable. Nay, perhaps my being a fecular perfon may the better qualify me to work on thofe I am to deal with, and may make my arguments, though not more folid in themfelves, yet more prevalent with men, that ufually (though how juftly, let them confider) have a particular pique at the clergy, and look with prejudice upon whatever is taught by men, whofe intereft is advantaged by having what they teach believed. And I was the more invited not to be a mere fpectator, or a lazy deplorer of the danger I faw religion in, becaufe it feemed not unlikely, that philofophical infidels, as they would be thought, would be lefs tractable to divines, though never fo good humanifts and antiquaries, than to a perfon, that reafons with them upon their own grounds, and difcourfes with them in their own way, having had a fomewhat more than ordinary curiofity to acquaint himfelf with the Epicurean or Cartefian principles, and exercife himfelf in that philofophy, which is very converfant with things corporeal, and ftrives to explain them by matter and motion, and fhakes off all authority (at leaft that is not infallible.) Upon fuch confiderations as thefe, I complied with an occafion I had of folemnly afking reafon the queftion, that *Jofhua* once afked the angel, that appeared to him in the plains of *Jericho*, " Art thou for us, or for our adverfaries?" *Jofh.* v. 14; and of committing to paper thofe thoughts, that fhould occur to me on that fubject. And this I the rather did, that I might thereby, as well contribute to my own fatisfaction, as to that of my friends. For, as I think, that there is nothing, that belongs to this life, that fo much deferves our ferious care, as what will become of us when we are paft it; fo I think, that he, who takes a refolution, either to embrace or reject fo important a thing, as religion, without ferioufly examining, why he does it, may happen to make a good choice, but can be but a bad choofer. And, that I might not exclude, by too early a method, thofe things, that, for aught I knew, might hereafter be pertinent and ufeful, I threw my reflections into one book, as into a repofitory, to be kept there only as a heap of differing materials, that, if they appeared worth it, they might be afterwards reviewed, and forted, and drawn into an orderly difcourfe. But, before I began to do what I intended, a fucceffion of accidents (wherewith it would not be proper to trouble the reader,) quite diverted me to employments of a very diftant nature; fo that thefe papers, being thrown by, did, for divers years, lie neglected, with many others, till at length the perfon, for whofe perufal I, in the firft place, defigned them, joined with fome other intelligent friends, to urge me to fend them abroad, though I was not in a condition to give them the finifhing ſtrokes, or ſo much as to fill up ſeveral of the blanks, my haſte had made me leave to be ſupplied when I ſhould be at leiſure. And indeed, notwithſtanding the juſt averſeneſs I had from letting a piece ſo incomplete and uncorrected appear in this critical age; yet the hopes, they confidently gave me, that this piece, ſuch as it is, might not be unacceptable nor uſeleſs, were not, I confeſs, altogether groundleſs.

Novelty being a thing very acceptable in this age, and particularly to the perſons I am to deal with, to whom perhaps it is none of the leaſt endearments of their errors, I deſpair not, that it will ſomewhat recommend theſe papers, to which I deſigned to commit not tranſcripts of what I thought they may have already met with in authors, but ſuch conſiderations, as a ſerious attention, and the nature of the things I treated of, ſuggeſted to me; ſo that moſt of the things will perhaps be thought new; and ſome few things coincident with what they may have elſewhere met with, may poſſibly appear rather to have been ſuggeſted by conſidering the ſame ſubjects, to other authors and to me, than to have been borrowed by me of them. But ſome few things, I confeſs, I employ, that were commonly enough employed before, and, I hope, I may, in that, have done religion, no diſſervice; for having taken notice, that ſome of the more familiar arguments had a real force in them, but had been ſo unwarily propoſed, as to be liable to exceptions, that had diſcredited them; I made it my care, by propoſing them more cautiouſly, to prevent ſuch objections, which alone kept their force from being apparent.

I was not unmindful of the great diſadvantage this tract was likely to undergo, partly for want of a more curious method, and partly becauſe my other occaſions required, that if I publiſhed it at all, it muſt be left to come abroad unpoliſhed and unfiniſhed. But though this inconvenience had like to have ſuppreſſed this diſcourſe; yet the force of it was much weakened by this conſideration, that this immethodical way of writing would beſt comply with what was deſigned and pretended in this paper, which was, not to write a compleat treatiſe of the ſubject of it, but only to ſuggeſt about it ſome of thoſe many conſiderations, that (queſtionleſs) might have occurred to (what I do not pretend to) an enlightened and penetrating intellect. And the loadſtone, divers of whoſe phænomena are mentioned in the body of this little tract, ſuggeſted ſomewhat to me in reference to the publication of it, by exciting in me a hope, that, if this diſcourſe have any thing near as much truth, as I endeavoured to furniſh it with, that truth will have its operation upon ſincere lovers of it notwithſtanding the want of regularity in the method; as a good loadſtone will not, by being rough and rudely ſhaped, be hindered from exerciſing its attractive and directive powers upon ſteel and iron.

As for the ſtile, I was rather ſhy than ambitious of bringing in the thorns of the ſchool-men, or the flowers of rhetorick; for, the latter, though they had, of their own accord, ſprung up under my pen, I ſhould have thought improper to be employed in ſo ſerious and philoſophical a ſubject: and as to the former, I declined them, in complaiſance to the humour of my infidels, who are generally ſo prejudiced againſt the ſchool-men, that ſcarce any thing can be preſented them with more diſadvantage than in a ſcholaſtick dreſs; and a demonſtration will ſcarce paſs for a good argument with ſome of them, if it be formed into a ſyllogiſm in mode and figure. That therefore, which I chiefly aimed at in my expreſſions, was ſignificancy and clearneſs, that my reader might ſee, that I was willing to make him judge of the ſtrength of my arguments, and would not put him to the trouble of divining in what it lay, nor inveigle him by ornaments of ſpeech, to think it greater than it was. I was alſo led by my reaſon, as well as by my inclination, to be careful not to rail at my infidels: and though I have ſome cauſe to think, that many of them had their underſtandings debauched by their lives, and were ſeduced from the church, not by *Diagoras* or *Pyrrho*, but by *Bacchus*

 and

and *Venus*; yet I treat them, as ſuppoſing them to be what they would be thought, friends to philoſophy: and being but a layman, I did not think myſelf obliged to talk to them, as out of a pulpit, and threaten them with damnation, unleſs they believed me, but choſe to diſcourſe to them rather as to erring virtuoſi, than wicked wretches.

THIS moderation, that I have uſed towards them, will, I hope, induce them to grant me two or three reaſonable requeſts; whereof the firſt ſhall be, that they would not make a final judgment of theſe papers, till they have peruſed them quite through: eſpecially having in their eye what is declared in the preamble, where both the deſign and ſcope of the whole diſcourſe, and what it does not pretend to, is expreſſed. The next thing I am to requeſt of them, and my readers, is, that they would not have the meaner thoughts of my arguments, for not being propoſed with the confidence, wherewith many writers are wont to recommend weaker proofs. For I wrote to intelligent men, and, in the judgment of ſuch, I never obſerved, that a demonſtration ceaſed to be thought one, for being modeſtly propoſed; but I have often known a good argument loſe of its credit by the invidious title of a demonſtration. And I muſt further beg my readers, to eſtimate my deſign in theſe papers, by the title of them, in which I do not pretend to make religion trample upon reaſon, but only to ſhew the reconcileableneſs of the one to the other, and the friendly agreement between them. I am a perſon, who looking upon it as my honour and happineſs to be both a man, and a Chriſtian, would neither write nor believe any thing, that might miſbecome me in either of thoſe two capacities. I am not a Chriſtian, becauſe it is the religion of my country, and my friends; nor, becauſe I am a ſtranger to the principles, either of the atomical, or the mechanical philoſophy. I admit no man's opinions in the whole lump, and have not ſcrupled, on occaſion, to own diſſents from the generality of learned men, whether philoſophers or divines: and when I chuſe to travel in the beaten road, it is not, becauſe I find it is the road, but becauſe I judge it is the way. Poſſibly I ſhould have much fewer adverſaries, if all thoſe, that yet are ſo, had as attentively and impartially conſidered the points in controverſy, as I have endeavour to do. They would then, it is like, have ſeen, that the queſtion I handle, is not, whether rational beings ought to avoid unreaſonable aſſents, but whether, when the hiſtorical and other moral proofs clearly ſway the ſcales in favour of Chriſtianity, we ought to fly from the difficulties, that attend the granting of a Deity and Providence, to hypotheſes, whether Epicurean, or others, that are themſelves incumbered with confounding difficulties: on which account I conceive, that the queſtion between them and me is not, whether they, or I, ought to ſubmit to reaſon (for we both agree in thinking ourſelves bound to that;) but whether they or I ſubmit to reaſon the fullieſt informed, and leaſt biaſſed by ſenſuality, vanity, or ſecular intereſt.

I reverence and cheriſh reaſon as much, I hope, as any of them; but I would have reaſon practice ingenuity as well as curioſity, and both induſtriouſly pry into things within her ſphere, and frankly acknowledge, (what no philoſopher, that conſiders, will deny,) that there are ſome things beyond it. And in theſe it is, that I think it as well her duty to admit revelation, as her happineſs to have it propoſed to her; and, even as to revelations themſelves, I allow reaſon to judge of them, before ſhe judges by them. The following papers will, I hope, manifeſt, that the main difference betwixt my adverſaries and me is, that they judge upon particular difficulties and objections, and I upon the whole matter. And to conclude; as I make uſe of my watch to eſtimate time, when ever the ſun is abſent or clouded, but when he ſhines clearly forth, I ſcruple not to correct and adjuſt my watch by his beams caſt on a dial; ſo, wherever no better light is to be had, I eſtimate truth by my own reaſon; but were divine revelation can be consulted, I willingly submit my fallible reason to the sure informations afforded by celestial light.

I should here put an end to this long preface, but that, to the things, which have been said concerning what I have written of my own, I see it is requisite, that I add a few words about what I quote from other writers; especially because in this very preface I mention my having intended to entertain my friend with my own thoughts. Of the citations therefore, that my reader will meet with in the following papers, I have this account to give him: 1. That I had written the considerations and distinctions, to which they are annexed, before I met with these cited passages, which I afterwards inserted in the margent, and other vacant places of my epistle. 2. That these passages are not borrowed from books, that treat of the truth of the Christian religion, or of Christian theology at all, but are taken from authors, that write of philosophical subjects, and are by me applied to mine, which are usually very distant from theirs. 3. If you then ask me, why I make use of their authority, and did not content myself with my own ratiocinations? I have this to answer; that my design being to convince another, who had no reason to look upon my authority, and whom I had cause to suspect to have entertained some prejudices against any reasons, that should come from one, that confessedly aimed at the defending of the Christian religion, I thought it very proper and expedient to let him see, that divers of the same things (for substance) that I delivered in favour of that religion, had been taught as philosophical truths by men, that were not professed divines, and were philosophers, and such strict naturalists, too, as to be extraordinarily careful, not to take any thing into their philosophy upon the account of revelation. And on this occasion let me observe to you, that there are some arguments, which being clearly built upon sense, or evident experiments, need borrow no assistance from the refutation of any of the proposers or approvers and may, I think, be fitly enough compared to arrows shot out of a cross-bow, and bullets shot out of a gun, which have the same strength, and pierce equally, whether they be discharged by a child, or a strong man. But then, there are other ratiocinations, which either do, or are supposed to depend, in some measure, upon the judgment and skill of those, that make the observations, whereon they are grounded, and their ability to discern truth from counterfeits, and solid things from those, that are but superficial ones: and these may be compared to arrows shot out of a long-bow, which make much the greater impression, by being shot by a strong and skilful archer. And therefore when we question, what doctrines ought, or ought not to be thought reasonable, it does not a little facilitate a proposition's appearing (not contrary, but) consonant to reason, that it is looked upon as such by those, that are acknowledged the masters of that faculty.

Some

Some CONSIDERATIONS ABOUT THE RECONCILEABLENESS OF REASON and RELIGION.

PART I.

AS to what you write in your friend's name, near the bottom of the firſt page of your letter, perhaps I ſhall not miſtake, if I gueſs, that, when he ſeems but to propoſe a queſtion, he means an objection; and covertly intimates, that I, among many others, am reduced to that paſs, that to embrace our religion, we muſt renounce our reaſon; and conſequently, that to be a Chriſtian, one muſt ceaſe to be a man, and much more, leave off being a philoſopher.

WHAT liberal conceſſions ſoever ſome others have been pleaſed to make on ſuch an occaſion as this, they do not concern me; who, being aſked but my own opinion, do not think myſelf reſponſible for that of others. And therefore, that I may frame my anſwer ſo, as to meet both with the obvious ſenſe of the queſtion, and the intimated meaning of him, that propoſes it, I ſhall roundly make a negative reply, and ſay, "that I do not "think, that a Chriſtian, to be truly ſo, is obliged to forego his reaſon; either by deny"ing the dictates of right reaſon, or by laying aſide the uſe of it."

I doubt not, but this anſwer is differing enough from what your friend expects; and perhaps thoſe grants, that have been made by the indulgence, or inadvertency of many perſons, eminent for being pious or learned, may make you yourſelf ſtartle at this declaration: and therefore, though you will not, I know, expect an anſwer to what objections your friend may make, ſince he has expreſſed but what he thinks ought to be a Chriſtian's opinion, not what he has to object againſt what is ſo; yet, to ſatisfy thoſe ſcruples, that you yourſelf may retain, I ſhall endeavour (but with the brevity, that becomes a letter) to acquaint you by themſelves, with ſome of the poſitive inducements, that have led me to this opinion, and interweave ſome others, in anſwering the chief objections, that I think likely to be made againſt it.

AND this preamble, ſhort as it is, will, I hope, ſerve to keep you from miſtaking my deſign; which, as you may gather from what I have intimated, is not to give you the poſitive proofs of the Chriſtian religion (which is not here to be expected from a bare defendant,) but to give you ſome ſpecimens of ſuch general conſiderations, as may probably ſhew, that the matter (or eſſential doctrines) peculiar to the Chriſtian religion is not ſo repugnant to the principles of true natural philoſophy, as that to believe them, a man muſt ceaſe to act like a rational man, any more than he would be obliged to do by embracing other religions, or even the tenets, that have been held without diſparagement to their intellectuals, by the mere philoſophers themſelves; which laſt clauſe I add, becauſe, I preſume, you do not expect, that I ſhould be ſo ſolicitous to vindicate the Chriſtian's belief of a Deity from being irrational; ſince, beſides that, perhaps your friend would think himſelf affronted to be dealt with as an Atheiſt, without having profeſſed himſelf one, the acknowledgement of a Deity blemiſhes the Chriſtian's reaſon no more, than it does that of men of all religions, not to ſay of all mankind;

and imports no other contradiction to reafon, than what has been judged to be none at all by the greateft, if not by all, of the philofophers, that were famed for being guided by reafon (without revelation.) And I fhall venture to add (upon the by) that, as I do not, for my own part, think the Atheift's philofophical objections (if your friend had produced them) to be near fo confiderable for weight or number, as not only thofe few, that deny a God, but many of thofe, that believe one, are wont to think; fo the Chriftian is not reduced, as is imagined, to make the Being of a Deity a mere poftulatum; fince, befides the philofophical arguments he can alledge in common with the beft champions for a Deity, he has a peculiar hiftorical proof, that may fuffice; the miracles performed by Chrift and his followers being fuch, that if the matter of fact can be (as it may be) well evinced, they will not only prove the reft of the Chriftian religion, but in the firft place, that there muft be a God to be the Author of them.

BUT though of the two things, which my defign obliges me to endeavour the making good of, the moft natural order feems to be, that I fhould firft fhew, that no precepts of Chriftianity do command a man to lay afide his reafon in matters of religion; and then, that there is nothing in the nature of the Chriftian doctrine itfelf, that makes a man need to do fo; yet I think it not amifs in treating of thefe two fubjects to invert the order, and firft confider that difficulty, which is the principal, and which your friend and you jointly defire to have my thoughts of; namely, "Whether there be a neceffity for a Chriftian to deny his reafon?" And then we fhall proceed to examine, whether, though he need not difclaim his reafon, it be neverthelefs his duty fo to do?

SECTION I.

TO proceed then to the confiderations, that make up the former part of this epiftle; I fhall, in the firft place, diftinguifh betwixt that, which the Chriftian religion itfelf teaches, and that, which is taught by this or that church, or fect of Chriftians, and much more by this or that particular divine or fchool-man.

I need not perfuade you, who cannot but know it fo well already, that there are many things taught about the attributes and decrees of God, the myfteries of the trinity, and incarnation, and divers other theological fubjects, about which not only private Chriftians, but churches of Chriftians do not at all agree. There are too many men, whofe ambition, or boldnefs, or felf-conceit, or intereft, leads them to obtrude upon others, as parts of religion, things, that are not only ftrangers, but oftentimes enemies to it. And there are others, who, out of an indifcrete devotion, are fo folicitous to encreafe the number, and the wonderfulnefs of myfteries, that, to hear them propofe and difcourfe of things, one would judge, that they think it is the office of faith, not to elevate, but to trample upon reafon; and that things are then fitteft to be believed, when they are not clearly to be proved or underftood. And indeed, when, on the one fide, I confider the charitable defign of the gofpel, and the candid fimplicity, that fhines in what it propofes, or commands; and on the other fide, what ftrange and wild fpeculations and inferences have been fathered upon it, not only in the metaphyfical writings of fome fchool-men, but in the articles of faith of fome churches; I cannot but think, that if all thefe doctrines are parts of the Chriftian religion, the apoftles, if they were now alive, would be at beft but *Catechumeni*; and I doubt not, but many of the nice points, that are now much valued and urged by fome, would be as well difapproved by St. *Paul*, as by *Ariftotle*; and fhould be as little entertained by an orthodox divine, as a rigid philofopher. I do not therefore allow all that for gofpel, which is taught for fuch in a preacher's pulpit, or even a profeffor's chair. And therefore, if fcholaftick writers, of what church foever, take the liberty of impofing upon the Chriftian

Chriſtian religion, their metaphyſical ſpeculations, or any other merely human doctrines, as matters of faith, I who, not without ſome examination, think metaphyſicks themſelves not to have been for the moſt part over-well underſtood, and applied, ſhall make bold to leave all ſuch private doctrines to be defended by their own broachers or abettors; and ſhall deny, that it will follow, that in caſe of this multitude of placets, which ſome bold men have been pleaſed to adopt into the catalogue of Chriſtian verities, any, or all, ſhould be found inconſiſtent with right reaſon, the Chriſtian religion muſt be ſo too. For by that name I underſtand only that ſyſtem of revealed truths, that are clearly delivered in the ſcriptures; or by legitimate and manifeſt conſequences deduced thence. And by this one declaration, ſo many unneceſſary, and perhaps hurtful retainers to Chriſtianity will be at once thrown off, that I doubt not, but if you conſider the matter aright, you will eaſily diſcern, that by this firſt diſtinction I have much leſſened the work, that is to be done by thoſe, that are to follow it.

SECTION II.

IN the next place, among the things, that ſeem not rational in religion, I make a great difference between thoſe, in which uninlightened reaſon is manifeſtly a competent judge, and thoſe, which natural reaſon itſelf may diſcern to be out of its ſphere.

You will allow me, that natural theology is ſufficient to evince the exiſtence of the deity; and we know, that many of the old philoſophers, that were unaſſiſted by revelation, were, by the force of reaſon, led to diſcover and confeſs a God, that is, a being ſupremely perfect; under which notion, divers of them expreſsly repreſent him. Now, if there be ſuch a being, it is but reaſonable to conceive, that there may be many things relating to his nature, his will, and his management of things, that are without the ſphere of mere or unaſſiſted reaſon. For, if his attributes and perfections be not fully comprehenſible to our reaſon, we can have but inadequate conceptions of them; and ſince God is a Being, *toto cœlo*, as they ſpeak, differing from all other beings, there may be ſome things in his nature, and in the manner of his exiſtence, which is without all example, or perfect analogy, in inferior beings. For we ſee, that even in man himſelf, the co-exiſtence and intimate union of the ſoul and body, that is, an immaterial and a corporeal ſubſtance, is without all preſident or parallel in nature. And though the truth of this union may be proved; yet, the manner of it was never yet, nor perhaps ever will be, in this life clearly underſtood, (to which purpoſe I ſhall elſewhere ſay more.) Moreover, if we ſuppoſe God to be omnipotent, (that is, to be able to do whatever involves no contradiction, that it ſhould be done,) we muſt allow him to be able to do many things, that no other agent can afford us any examples of, and ſome of them perhaps, ſuch as we, who are but finite, and are wont to judge of things by analogy, cannot conceive how they can be performed. Of the laſt ſort of things may be the recollecting a ſufficient quantity of the ſcattered matter of a dead human body, and the contriving of it ſo, that (whether alone, or with ſome addition of other particles) upon a re-conjunction with the ſoul, it may again conſtitute a living man, and ſo effect that wonder we call the reſurrection. Of the latter ſort, is the creation of matter out of nothing, and much more the like production of thoſe rational and intelligent beings, human ſouls. For as for angels (good or bad) I doubt, whether mere philoſophy can evince their exiſtence, though I think it may the poſſibility thereof. And ſince we allow the Deity a wiſdom equal to this boundleſs power, it is but reaſonable to conceive, that theſe unlimited attributes conſpiring, may produce contrivances and frame deſigns, which we men muſt be unable

able (at leaft of ourfelves) fufficiently to underftand, and to reach to the bottom of. And by this way of arguing, it may be made to appear, that there may be many things relating to the Deity above the reach of unenlightened human reafon. Not that I affirm all thefe things to be, in their own nature, incomprehenfible to us, (though fome of them may be fo,) when they are once propofed; but that reafon, by its own light, could not difcover them particularly, and therefore it muft owe its knowledge of them to divine revelation. And if God vouchfafes to difclofe thofe things to us, fince not only he muft needs know about his own nature, attributes, &c. what we cannot poffibly know unlefs he tells us, and fince we know, that whatever he tells us is infallibly true, we have abundant reafon to believe rather what he declares to us concerning himfelf and divine things, than what we fhould conclude or guefs about them, by analogy to things of a nature infinitely diftant from his, or by maxims framed according to the nature of inferior beings. If therefore he clearly reveal to us, that there is in the Godhead, three diftinct perfons, and yet that God is one, we, that think ourfelves bound to believe God's teftimony in all other cafes, ought fure not to difbelieve it concerning himfelf, but to acknowledge, that in an unparalleled and incomprehenfible Being, there may be a manner of exiftence not to be paralleled in any other being, though it fhould never be underftood by us men, who cannot clearly comprehend, how in ourfelves two fuch diftant natures, as that of a grofs body, and an immaterial fpirit, fhould be united, fo as to make up one man. In fuch cafes therefore, as we are now fpeaking of, there muft indeed be fomething, that looks like captivating one's reafon, but it is a fubmiffion, that reafon itfelf obliges us to make; and he, that in fuch points as thefe, believes rather what the divine writings teach him, than what he would think, if they had never informed him, does not renounce or enflave his reafon, but fuffers it to be pupil to an omnifcient and infallible inftructor, who can teach him fuch things, as neither his own mere reafon, nor any others could ever have difcovered to him.

I thought to have here difmiffed this propofition, but I muft not omit to give it a confirmation afforded me by chance, (or rather providence:) for, fince I writ the laft paragraph, refuming a philofophical enquiry, I met, in profecuting it, with a couple of teftimonies of the truth of what I was lately telling you, which are given, not by divines or fchool-men, but by a couple of famous mathematicians, that have both led the way to many of the modern philofophers, to fhake off the reverence wont to be borne to the authority of great names, and have advanced reafon in a few years, more than fuch as *Vaninus* and *Pomponatius* would do in many ages; and have always boldly, and fometimes fuccefsfully, attempted to explain intelligibly thofe things, which others fcrupled not, either openly or tacitly, to confefs inexplicable.

THE firft of thefe teftimonies I met with in a little French treatife put out by fome mathematician, who, though he conceals his name, appears, by his way of writing, to be a great virtuofo, and takes upon him to give his readers in French the new thoughts of *Galilæo*, by making that the title of his book. This writer then fpeaking of a paradox (which I but recite) of *Galilæo*'s, that makes a point equal to a circle, adds, *& per confequent l'on peut dire*, i. e. and confequently one may fay, that all circles are equal between themfelves, fince each of them is equal to a point. For though the imagination be over-powered by this idea, or notion, yet reafon will fuffer itfelf to be perfuaded of it. I know (continues he) divers other excellent perfons, (befides *Galilæo*) who conclude the fame thing by other ways; but all are conftrained to acknowledge, that indivifible and infinite are things, that do fo fwallow up the mind of man, that he fcarce knows what to pitch on, when he contemplates them. For it will follow, from *Galilæo*'s fpeculation, &c. which paffage I have cited, to fhew you, that

Pag. 22, 23.

Galilæo

Galilæo is not the only philosopher and mathematician, who has confessed his reason quite passed about the attributes of what is infinite.

THE other testimony I mentioned to you, is that of the excellent *Des Cartes*, in the second part of his principles of philosophy, where, speaking of the circle to be made by matter moving through places still lesser and lesser, he has this ingenious acknowledgment; *fatendum tamen est* (says he) *in motu isto aliquid reperiri, quod mens quidem nostra percipit esse verum, sed tamen quo pacto fiat non comprehendit, nempe divisionem quarundam particularum materiæ in infinitum, sive indefinitam, atque in tot partes ut nulla cogitatione determinare possimus tam exiguam, quin intelligamus ipsam in alias adhuc minores reipsa esse divisam.* And in the close of the next paragraph, he gives this for a reason, why, though we cannot comprehend this indefinite division, yet we ought not to doubt of the truth of it, that we discern it to be of that kind of things, that cannot be comprized by our minds, as being but finite.

IF then such bold and piercing wits, and such excellent mathematicians, are forced to confess, that not only their own reason, but that of mankind, may be passed and non-plussed about quantity, which is an object of contemplation natural, nay, mathematical, and which is the subject of the rigid demonstrations of pure mathematicks; why should we think it unfit to be believed, and to be acknowledged, that in the attributes of God, who is essentially an infinite Being, and an *ens singularissimum*, and in divers other divine things, of which we can have no knowledge without revelation, there should be some things, that our finite understandings cannot, especially in this life, clearly comprehend.

SECTION III.

TO this consideration, I shall, for affinity's sake, subjoin another, which I leave to your liberty to look upon as a distinct one, or as an enlargement and application of the former.

I consider then, that there is a great difference between a doctrine's being repugnant to the general and well-weighed rules or dictates of reason, in the forming of which rules, it may be supposed to have been duly considered; and its disagreeing with axioms, at the establishment whereof the doctrine in question was probably never thought on. There are several rules, that pass current, even among the most learned men, and which are indeed of very great use, when restrained to those things whence they took their rise, and others of the like nature; which yet ought not to overthrow those divine doctrines, that seem not consonant to them. For the framers of these rules having generally built them upon the observations they had made of natural and moral things, since (as we lately argued) reason itself cannot but acknowledge, there are some things out of its sphere, we must not think it impossible, that there may be rules, which will hold in all inferior beings for which they were made; and yet not reach to that infinite and most singular Being, called God, and to some divine matters, which were not taken into consideration, when those rules were framed. And indeed, if we consider God as the author of the universe, and the free establisher of the laws of motion, whose general concourse is necessary to the conservation and efficacy of every particular physical agent, we cannot but acknowledge, that, by with-holding his concourse, or changing these laws of motion, which depend perfectly upon his will, he may invalidate most, if not all the axioms and theorems of natural philosophy: these supposing the course of nature, and especially the established laws of motion among the parts of the universal matter, as those upon which all the phænomena depend. It is a rule in natural philosophy, that *causæ necessariæ semper agunt quantum possunt*; but it will not follow from thence, that the fire must necessarily burn *Daniel*'s three companions, or their clothes, that were cast by the Babylonian king's command into the midst of a burning fiery furnace, when the author of nature was pleased to withdraw his concourse to the operation of the flames, or supernaturally to defend against them the bodies, that were exposed to them. That men once truly dead cannot be brought to life again, hath been in all ages the doctrine of mere philosophers; but though this be true, according to the course of nature, yet it will not follow, but that the contrary may be true, if God interpose either to recal the departed soul, and re-conjoin it to the body, if the organization of this be not too much vitiated, or by so altering the fabrick of the matter, whereof the carcase consists, as to restore it to a fitness for the exercise of the functions of life. Agreeably to this, let me observe to you, that, though it be unreasonable to believe a miraculous effect, when attributed only to a mere physical agent; yet the same thing may reasonably be believed, when ascribed to God, or to agents assisted with his absolute or supernatural power. That a man born blind should, in a trice, recover his sight, upon the application of clay and spittle, would justly appear incredible, if the cure were ascribed to one, that acted as a mere man; but it will not follow, that it ought to be incredible, that the Son of God would work it. And the like may be said of all the miracles performed by Christ, and those apostles and other disciples of his, that acted by virtue of a divine power and commission. For in all these, and the like cases, it suffices not to make one's belief irrational, that the things believed are impossible to be true, according to the course of nature; but it must be shewn, either that they are impossible, even to the power of God, to which they are ascribed, or that the records, we have of them, are not sufficient to beget belief in the nature of a testimony; which latter objection against these relations is foreign to our present discourse. And as the rules about the power of agents will not all of them hold in God, so I might shew the like, if I had time, concerning some of his other attributes; insomuch, that even in point of justice, wherein we think we may freeliest make estimates of what may or may not be done, there may be some cases, wherein God's supreme dominion, as maker and governor of the world, places him above some of those rules; I say, some, for I say not above all those rules of justice, which oblige all inferior beings, without excepting the greatest and most absolute monarchs themselves. I will not give examples of his power of pardoning or remitting penalties, which is but a relaxing of his own right; but will rather give an instance in his power of afflicting and exterminating men, without any provocation given him by them. I will not here enter upon the controversy, *de jure Dei in creaturas*, upon what it is founded, and how far it reaches. For, without making myself a party in that quarrel, I think, I may safely say, that God, by his right of dominion, might, without any violation of the laws of justice, have destroyed, and even annihilated *Adam* and *Eve*, before they had eaten of the forbidden fruit, or had been commanded to abstain from it. For man being as much and as entirely God's workmanship as any of the other creatures, unless God had obliged himself by some promise or pact, to limit the exercise of his absolute dominion over him, God was no more bound to preserve *Adam* and *Eve* long alive, than he was to preserve a lamb, or a pidgeon; and therefore, as we allow, that he might justly recal the lives he had given those innocent creatures, when he pleased, (as actually he often ordered them to be killed, and burned in sacrifice to him:) so he might, for the declaration of his power to the angels, or for other reasons, have suddenly taken away the lives of *Adam* and *Eve*, though they had never offended him. And upon the same grounds he might, without injustice, have annihilated, I say not, damned their souls; he being no more bound to continue existence to a nobler, than a less noble creature; as he is no more bound to keep an eagle, than an oyster always alive. I know, there is a difference betwixt God's resuming a being he lent *Adam*, and his doing the same to inferior creatures: but that disparity, if it concern any of his attributes, will concern some other than his justice; which allowed him to resume, at pleasure, the being he had only lent them, or lay any affliction on them, that were lesser than that good could countervail. But, mentioning this instance only occasionally, I shall not prosecute it any further, but rather mind you of the result of this and the foregoing consideration; which is, that divinely revealed truths may seem to be repugnant to the dictates of reason, when they do but seem to be so: nor does Christianity oblige us to question such rules, as to the cases they were framed for, but the application of them to the nature of God, who has already been truly said to be *ens singularissimum*, and to his absolute power and will; so that we do not reject the rules we speak of, but rather limit them; and when we have restrained them to their due bounds, we may safely admit them.

FROM men's not taking notice of, or not pondering this necessary limitation of many axioms delivered in general terms, seems to have proceeded a great error, which has made so many learned men presume to say, that this or that thing is true in philosophy, but false in divinity, or on the contrary: as for instance, that a virgin, continuing such, may have a child, is looked upon as an article, which theology asserts to be true, and philosophy pronounces impossible. But the objection is grounded upon a mistake, which might have been prevented by wording the propositions more warily and fully. For though we grant, that physically speaking, it is false, that a virgin can bring forth a child; yet that signifies no more, than that, according to the course of nature, such a thing cannot come to pass; but speaking absolutely and indefinitely, without confining the effect to mere physical agents, it may safely be denied, that philosophy pronounces it impossible, that a virgin should be a mother. For why should the author of nature be confined to the ways of working of dependent and finite agents? And to apply the answer to the divines, that hold the opinion I oppose, I shall demand, why God may not out of the substance of a woman form a man, without the help of a man, as well as at the beginning of the substance of a man he formed a woman without the concurrence of a woman? And so that iron, being a body far heavier (*in specie*, as they speak,) will, if upheld by no other body, sink in water, is a truth in natural philosophy; but since physicks themselves lead men to the acknowledgment of a God, it is not repugnant to reason, that, if God please to interpose his power, he may (as in *Elisha*'s case) make iron swim, either by with-holding his concourse to the agents, whatever they be, that cause gravity in bodies, or perhaps by other ways unknown to us; since a vigorous loadstone may, as I have more than once tried, keep a piece of iron, which it touches not, swimming in the air, though this thin body must contribute far less, than water would, to the sustaining it aloft.

THAT strict philosopher *Des Cartes*, who has with great wit and no less applause attempted to carry the mechanical powers of matters higher than any of the modern philosophers; this naturalist, I say, that ascribes so great a power to matter and motion, was so far from thinking, that what was impossible to them, must be so to God too, that, though he were urged by a learned adversary with an argument, as likely as any to give him a strong temptation to limit the omnipotence of God; yet even on this occasion he scruples not to make this ingenious and wary acknowledgment, and that in a private letter; "For my part, says he, I think we ought never to say of any Vol. II.
"thing, that it is impossible to God. For all, that is true and good, being depen- Letter 6.
"dent on his all-mightiness, I dare not so much as say, that God cannot make a
"mountain without a valley, or cannot make it true, that one and two shall not make
"three; but I say only, that he has given me a soul of such a nature, that I cannot

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" conceive a mountain without a valley, nor that the aggregate of one and of two " ſhall not make three, &c. and I ſay only, that ſuch things imply a contradiction in " my conception." And conſonantly to this, in his Principles of Philoſophy he gives, on a certain occaſion, this uſeful caution,—*Quod ut ſatis tutò & ſine errandi periculo aggrediamur, eâ nobis cautelâ eſt utendum, ut ſemper quàm maxime recordemur, & Deum autorem rerum eſſe infinitum, & nos omnino finitos.*

Parte Prima. Art. 24.

SECTION IV.

IN the next place, I think we ought to diſtinguiſh between reaſon conſidered in itſelf, and reaſon conſidered in the exerciſe of it, by this or that philoſopher, or by this or that man, or by this or that company, or ſociety of men, whether all of one ſect or of more.

IF you will allow me to borrow a ſchool phraſe, I ſhall expreſs this more ſhortly by ſaying, I diſtinguiſh between reaſon *in abſtracto*, and *in concreto*. To clear this matter, we may conſider, that whatever you make the faculty of reaſon to be in itſelf, yet the ratiocinations it produces are made by men, either ſingly reaſoning, or concurring in the ſame ratiocinations and opinions; and conſequently, if theſe men do not make the beſt uſe of their reaſoning faculty, it will not be neceſſary, that what thwarts their ratiocinations, muſt likewiſe thwart the principles or the dictates of right reaſon. For man having a will and affections as well as an intellect, though our dijudications and tenents ought indeed (in matters ſpeculative) to be made and pitched upon by our unbiaſſed underſtandings; yet really our intellectual weakneſſes, or our prejudices, or prepoſſeſſion by cuſtom, education, &c. our intereſt, paſſions, vices, and I know not how many other things, have ſo great and ſwaying an influence on them, that there are very few concluſions, that we make, or opinions, that we eſpouſe, that are ſo much the pure reſults of our reaſon, that no perſonal diſability, prejudice, or fault, has any intereſt in them.

About the Diverſity of Religion.

THIS I have elſewhere more amply diſcourſed of on another occaſion; wherefore I ſhall now add but this, that the diſtinction, I have been propoſing, (does if I miſtake not) reach a great deal further than you may be aware of. For not only whole ſects, whether in religion or philoſophy, are in many caſes ſubject to prepoſſeſſions, envy, ambition, intereſt, and other miſleading things, as well as ſingle perſons; but, which is more conſiderable to our preſent purpoſe, the very body of mankind may be embued with prejudices, and errors, and that from their childhood, and ſome alſo even from their birth, by which means they continue undiſcerned, and conſequently unreformed.

THIS you will think an accuſation as bold as high; but to let you ſee, that the philoſophers, you moſt reſpect, have made the ſame obſervation, though not applied to the ſame caſe, I muſt put you in mind, that Monſieur *Des Cartes* begins his principles of philoſophy with taking notice, that, becauſe we are born children, we make divers unright judgments of things, which afterwards are wont to continue with us all our lives, and prove radicated prejudices, that miſlead our judgments on ſo many occaſions, that he elſewhere tells us, he found no other way to ſecure himſelf from their influence, but once in his life ſolemnly to doubt of the truth of all, that he had till then believed, in order to the re-examining of his former dijudications. But I remember, our illuſtrious *Verulam* warrants a yet further prejudice againſt many things, that are wont to be looked on as the ſuggeſtions of reaſon. For having told us, that the mind of man is beſieged with four differing kinds of idols or phantaſms, when he comes to enumerate them, he teaches, that there are not only ſuch, as men get by converſation and diſcourſe one with another, and ſuch as proceed from the divers hypotheſes thefes or theories and opinions of philofophers, and from the perverfe ways of demonftration, and likewife fuch as are perfonal to this or that man, proceeding from his education, temperament, ftudies, &c. but fuch as he calls *idola tribus*, becaufe they are founded in humane nature itfelf, and in the very tribe or nation of mankind; and of thefe he particularly difcourfes of feven or eight; as, that the intellect of man has an innate propenfity to fuppofe in things a greater order and equality than it finds, and that being unable to reft or acquiefce, it does always tend further and further; to which he adds divers other innate prejudices of mankind, which he folicitoufly as well as judicioufly endeavours to remove.

Now, if not only fingle philofophers, and particular fects, but the whole body of mankind be fubject to be fwayed by innate and unheeded prejudices and proclivities to errors about matters, that are neither divine, nor moral, nor political, but phyfical, where the attainment of truth is exceeding pleafant to human nature, and is not attended with confequences diftafteful to it: why may not we juftly fufpect not only this or that philofopher, or particular fect; but the generality of men, of having fome fecret propenfities to err about divine things, and indifpofitions to admit truths, which not only detect the weakneffes of our nature, and our perfonal difabilities, and thereby offend or mortify our pride and our ambition, but fhine into the mind with fo clear, as well as pure and chafte a light, as is proper both to difcover to ourfelves and others our vices and faults, and oftentimes to crofs our defigns and interefts?

AND to this purpofe we may take notice, that divers of thofe very idols, which my lord *Bacon* obferves to befiege, or pervert men's judgments in reference to things natural, may probably have the fame kind of influence (and that much ftronger) on the minds of men in reference to fupernatural things. Thus he takes notice, that if fome things have once pleafed the underftanding, it is apt to draw all others to comport with, and give fuffrage to them, though perhaps the inducements to the contrary belief be either more numerous or more weighty. He obferves alfo, that man is apt to look upon his fenfes and other perceptions as the meafures of things, and alfo that the underftanding of man is not fincerely difpofed to receive the light of truth, but receives an infufion as it were of adventitious colours, (that difguife the light) from the will and affections, which makes him fooner believe thofe things, that he is defirous fhould be true, and reject many others upon accounts, that do no way infer their being falfe. Now if we apply thefe things to divine truths (to which it were well they were lefs juftly applicable) and confider, that in our youth we generally converfe but with things corporeal, and are fwayed by affections, that have them for their objects, we fhall not much wonder, that men fhould be very prone, either to frame fuch notions of divine things, as they were wont to have about others of a far different and meaner nature; or elfe to reject them for not being analogous to thofe things, which they have been ufed to employ for the meafures of truth and falfity. And if we confider the inbred pride of man, which is fuch, that if we believe the facred ftory, even *Adam* in paradife affected to be like God, knowing good and evil; we fhall not fo much marvel, that almoft every man in particular makes the notions he has entertained already, and his fenfes, his inclinations, and his interefts, the ftandards, by which he eftimates and judges of all others things, whether natural or revealed. And as *Heraclitus* juftly complained, that every man fought the knowledge of natural things in the microcofm, that is, himfelf, and not in the macrocofm, the world; fo we may juftly complain, that men feek all the knowledge, they care to find, or will admit, either in thefe little worlds themfelves, or from that great world, the univerfe; but not from the omnifcient author of them both. And laftly, if even in purely phyfical things, where one would not think it likely, that rational beings fhould feek truth with any other defigns than of finding and enjoying it,

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our understandings are so universally biassed, and imposed upon by our wills and affections; how can we admire, especially if we admit the fall of our first parents, that our passions and interests, and oftentimes our vices, should pervert our intellects about those revealed truths; divers of which we discern to be above our comprehensions, and more of which we find to be directly contrary to our inclinations?

SECTION V.

AND now it will be seasonable for me to tell you, that I think, there may be a great difference betwixt a thing's being contrary to right reason, or so much as to any true philosophy, and its being contrary to the received opinions of philosophers, or to the principles or conclusions of this or that sect of them.

FOR here I may justly apply to my present purpose what *Clemens Alexandrinus* judiciously said on another occasion, that philosophy was neither Peripatetical, nor Stoical, nor Epicurean, but whatsoever among all those several parties was fit to be approved.

AND indeed, if we survey the hypotheses and opinions of the several sects of philosophers, especially in those points, wherein they hold things repugnant to theological truths, we shall find many of them so slightly grounded, and so disagreeing among themselves, that a severe and inquisitive examiner would see little cause to admit them upon the bare account of his being a philosopher, though he did not see any to reject them upon the account of his being a Christian. And in particular, as to the Peripateticks, who by invading all the schools of *Europe* (and some in *Asia* and *Africk*) have made their sect almost Catholick, and have produced divers of the famous questioners of Christianity in the last age, and the first of this; the world begins to be apace undeceived, as to many of their doctrines, which were as confidently taught and believed for many ages, as those, that are repugnant to our religion; and there is now scarce any of the modern philosophers, that allow themselves the free use of their reason, who believes any longer, that there is an element of fire lodged under the supposed sphere of the moon; that heaven consists of solid orbs; that all celestial bodies are ingenerable and incorruptible; that the heart, rather than the brain, is the origine of nerves; that the torrid zone is uninhabitable; and I know not how many other doctrines of the Aristotelians, which our Corpuscularian philosophers think so little worth being believed, that they would censure him, that should now think them worthy to be solicitously confuted; upon which score I presume you will allow me to leave those, and divers others, as weak Peripatetick conceits, to fall by their own groundlessness.

BUT you will tell me, that the Epicureans, and the Somatici, that will allow nothing but body in the world, nor no author of it but chance, are more formidable enemies to religion than the Aristotelians. And indeed I am apt to think they are so, but they may well be so, without deserving to have any of their sects looked upon as philosophy itself, there being none of them, that I know of, that maintain any opinion inconsistent with Christianity, that I think may not be made appear to be also repugnant to reason, or at least not demonstrable by it. You will not expect I should descend to particulars, especially having expresly discoursed against the Epicurean hypotheses of the origin of the world in another paper; and therefore, I shall observe to you in general, that the Cartesian philosophers, who lay aside all supernatural revelation in their inquiries into natural things, do yet both think, and, as to the two first of them, very plausibly prove, the three grand principles of *Epicurus*, that the little bodies he calls atoms are indivisible, that they all have their motion from themselves, and, that there is a vacuum in *rerum naturâ*, to be as repugnant to mere reason, as the Epicureans think the notion of an incorporeal substance, or the creation of the world, or the immortality of the soul. And as for the new Somatici, such as Mr. *Hobbes* (and some few others) by what

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I have yet seen of his, I am not much tempted to forsake any thing, that I looked upon as a truth before, even in natural philosophy itself, upon the score of what he (though never so confidently) delivers, by which hitherto I see not, that he hath made any great discovery either of new truths, or old errors. An honourable member of the Royal Society hath elsewhere purposely shewn, how ill he has proved his own opinions about the air, and some other physical subjects, and how ill he has understood and opposed those of his adversary. But to give you in this place a specimen, how little their repugnancy to his principles of natural philosophy ought to affright us from those theological doctrines they contradict, I shall here examine the fundamental maxim of his whole physicks, that nothing is removed but by a body contiguous and moved; it having been already shewn (by the gentleman newly mentioned) that, as to the next to it, which is, that there is no vacuum, whether it be true or no, he has not proved it.

[IF no body can possibly be moved, but by a body contiguous and moved, as Mr. *Hobbes* teaches; I demand, how there comes to be local motion in the world? For, either all the portions of matter, that composed the universe, have motion belonging to their nature, which the Epicureans affirmed for their atoms; or some parts of matter have this motive power, and some have not; or else none of them have it, but all of them are naturally devoid of motion. If it be granted, that motion does naturally belong to all parts of matter, the dispute is at an end, the concession quite overthrowing the hypothesis. If it be said, that naturally some portions of matter have motion, and others not, then the assertion will not be universally true: for though it may hold in the parts, that are naturally moveless, or quiescent, yet it will not do so in the others, there being nothing, that may shew a necessity, why a body, to which motion is natural should not be capable of moving, without being put into motion by another contiguous and moved. And if there be no body, to which motion is natural, but every body needs an outward movent, it may well be demanded, how there comes to be any thing locally moved in the world? which yet constant and obvious experience demonstrates, and Mr. *Hobbes* himself cannot deny. For if no part of matter have any motion but what it must owe to another, that is contiguous to it, and being itself in motion, impels it; and if there be nothing but matter in the world, how can there come to be any motion amongst bodies, since they neither have it upon the score of their own nature, nor can receive it from external agents? If Mr. *Hobbes* should reply, that the motion is impressed upon any of the parts of the matter by God, he will say that, which I most readily grant to be true, but will not serve his turn, if he would speak congruously to his own hypothesis. For I demand, whether this supreme Being, that the assertion has recourse to, be a corporeal or an incorporeal substance? If it be the latter, and yet be the efficient cause of motion in bodies, then it will not be universally true, that whatsoever body is moved, is so by a body contiguous and moved. For, in our supposition, the bodies, that God moves, either immediately, or by the intervention of any other immaterial being, are not moved by a body contiguous, but by an incorporeal spirit. But because Mr. *Hobbes*, in some writings of his, is believed to think the very notion of an immaterial substance to be absurd, and to involve a contradiction; and because it may be subsumed, that if God be not an immaterial substance, he must by consequence be a material and corporeal one, there being no *medium negationis*, or third substance, that is none of those two. I answer, that, if this be said, and so that Mr. *Hobbes*'s deity be a corporeal one, the same difficulty will recur, that I urged before. For this body will not, by Mr. *Hobbes*'s calling or thinking it divine, cease to be a true body; and consequently a portion of divine matter will not be able to move a portion of our mundane matter, without it be itself contiguous and moved; which it cannot be, but by another portion of divine matter, so qualified to impress a motion, nor this again, but by another portion.

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AND befides that it will breed a ftrange confufion, in rendering the phyfical caufes of things, unlefs an expedient be found, to teach us how to diftinguifh accurately the mundane bodies from the divine, (which will perhaps prove no eafy tafk;) I fee not yet, how this corporeal deity will make good the hypothefis I examine. For I demand, how this divine matter comes to have this local motion, that is afcribed to it? If it be anfwered, that it hath it from its own nature, without any other caufe, fince the Epicureans affirm the fame of their atoms, or merely mundane matter, I demand, how the truth of Mr. *Hobbes*'s opinion will appear to me, to whom it feems as likely by the phænomena of nature, that occur, that mundane matter fhould have a congenit motion, as that any thing, that is corporeal, can be God, and capable of moving it; which to be, it muft, for aught we know, have its fubfiftence divided into as many minute parts, as there are corpufcles and particles in the world, that move feparately from their neighbouring ones. And, to draw towards a conclufion, I fay, that thefe minute divine bodies, that thus moved thofe portions of mundane matter, concerning which Mr. *Hobbes* denies, that they can be moved but by bodies contiguous and moved, thefe divine fubftances, I fay, are, according to the late fuppofition, true bodies, and yet are moved themfelves, not by bodies, contiguous and moved, but by a motion, which muft be innate, derived or flowing from their very effence or nature, fince no fuch body is pretended to have a being, as cannot be referred as a portion, either to the mundane, or the divine matter. In fhort, fince local motion is to be found in one, if not in both, of thefe two matters, it muft be natural to (at leaft fome parts of) one of them in Mr. *Hobbes*'s hypothefis; for, though he fhould grant an immaterial being, yet it could not produce a motion in any body, fince, according to him, no body can be moved, but by another body contiguous and moved.]

As then to this grand pofition of Mr. *Hobbes*, though, if it were cautioufly propofed, as it is by *Des Cartes*, it may perhaps be fafely admitted, becaufe *Cartefius* acknowledges the firft impulfe, that fet matter a moving, and the confervation of motion once begun, to come from God; yet, as it is crudely propofed by the favourers of Mr. *Hobbes*, I am fo far from feeing any fuch cogent proof for it, as were to be wifhed for a principle, on which he builds fo much, (and which yet is not at all evident by its own light,) that I fee no competent reafon to admit it.

I expect your friend fhould here oppofe to what I have been faying, that formerly recited fentence, that is fo commonly employed in the fchools, as well of divines as of philofophers: that fuch or fuch an opinion is true in divinity, but falfe in philofophy; or, on the contrary, philofophically true, but theologically falfe.

UPON what warrant thofe, that are wont to employ fuch expreffions, ground their practice, I leave to them to make out; but as to the objection itfelf, as it fuppofes thefe ways of fpeaking to be well grounded, give me leave to confider that philofophy may fignify two things, which I take to be very differing.

FOR, firft, it is moft commonly employed to fignify a fyftem, or body of the opinions, and other doctrines of the parcicular fect of thofe philofophers, that make ufe of the word. As when an Ariftotelian talks of philofophy, he ufually means the Peripatetick, as an Epicurean does the Atomical, or a Platonift the Platonick.

BUT we may alfo, in a more general, and no lefs juft acception of the term, underftand by philofophy, a comprehenfion of all thofe truths or doctrines, which the natural reafon of man, freed from prejudices and partiality, and affifted by learning, attention, exercife, experiments, &c. can manifeftly make out, or, by neceffary confequence deduce from clear and certain principles.

THIS being briefly premifed, I muft, in the next place, put you in mind of what I formerly obferved to you, that many opinions are maintained by this, or that fect of Chriftians, Chriſtians, or perhaps by the divinity-ſchools of more than one or two ſects, which either do not at all belong to the Chriſtian religion, or, at leaſt, ought not to be looked upon as parts of it, but upon ſuppoſition, that the philoſophical principles and ratiocinations, upon which, and not upon expreſs or mere revelation, they are preſumed to be founded, are agreeable to right reaſon.

AND having premiſed theſe two things, I now anſwer more directly to the objection; that, if philoſophy be taken in the firſt ſenſe above-mentioned, it's teaching things repugnant to theology, eſpecially taking this word in the more large and vulgar ſenſe of it, will not cogently conclude any thing againſt the Chriſtian religion. But, if philoſophy be taken in the latter ſenſe for true philoſophy, and divinity only for a ſyſtem of thoſe articles, that are clearly revealed as truths in the ſcriptures; I ſhall not allow any thing to be falſe in philoſophy ſo underſtood, that is true in divinity ſo explained, till I ſee ſome clearer proof of it, than I have yet met with. I have had occaſion, in the foregoing diſcourſe, to ſay ſomething, that may be applied to the point under debate; and in the following part of this letter, I ſhall have occaſion to touch upon it again: and therefore I ſhall now ſay but this in ſhort, that it is not likely, that God, being the author of reaſon as well as revelation, ſhould make it men's duty to believe as true, that, which there is juſt reaſon to reject as falſe.

THERE is indeed a ſenſe, wherein the phraſes, I diſapprove, may be tolerated. For if by ſaying, that ſuch a thing is true in divinity, but falſe in philoſophy, it were meant, that if the doctrine were propoſed to a mere philoſopher, to be judged of according to the principles of his ſect, or at moſt, according to what he, being ſuppoſed not to have heard of the Chriſtian religion, or had it duly propoſed to him, would reject it, the phraſe might be allowed, or at leaſt indulged. But then we muſt conſider, that the reaſon, why ſuch a philoſopher would reject the articles of Chriſtian faith, would not be, becauſe they could by no mediums be poſſibly proved, but becauſe theſe doctrines, being founded upon a revelation, which he is preſumed either not to have heard of, or not to have had ſufficiently propoſed to him, he muſt, as a rational man, refuſe to believe them upon the ſcore of their proofleſneſs. And the ſame philoſopher, ſuppoſing him to be a true one, though he will be very wary, how he admits any thing as true, that is not proved, if it fall properly under the cognizance of philoſophy; yet he will be as wary, how he pronounces things to be falſe or impoſſible, in matters, which he diſcerns to be beyond the reach of mere natural reaſon, eſpecially if ſober and learned men do very confidently pretend to know ſomething of thoſe matters by divine revelation, which though he will not eaſily believe to be a true one, yet he will admit, in caſe it ſhould be proved true, to be a fit medium to evince truths, which, upon the account of mere natural light, he could not diſcover or embrace. To be ſhort, ſuch a philoſopher would indeed reject ſome of the articles of our faith hypothetically, i. e. upon ſuppoſition that he need employ no other touch-ſtone to examine them by, than the principles and dictates of natural philoſophy, that he is acquainted with (upon which ſcore I ſhall hereafter ſhew, that divers ſtrange chemical experiments, and other diſcoveries would alſo be rejected;) but yet he would not pronounce them falſe, but upon ſuppoſition, that the arguments, by which they lay claim to divine revelation, are incompetent in their kind. For as he will not eaſily believe any thing within the ſphere of nature, that agrees not with the eſtabliſhed laws of it; ſo he will not eaſily adventure to pronounce one way or other in matters, that are beyond the ſphere of nature: he will indeed, as he juſtly may, expect as full a proof of the divine teſtimony, that is pretended, as the nature of the thing requires and allows; but he will not be backward to acknowledge, that God, to whom that teſtimony is aſcribed, is able to know and to do many more things, than we can explicate how he can discover, or imagine how any physical agent can perform.

Princip. Philos. part. prima. Artic. 25. [SINCE I proposed to you this fifth consideration, I happened to light on a passage in *Des Cartes*'s principles, which affords of what I have been discovering the suffrage of a philosopher, that is wont to be accused of excluding theology too scrupulously out of his philosophy. His words are so full to my present purpose, that I need not, to accommodate them to it, alter one of them, and therefore shall transcribe them just as they lie: *Si forté nobis Deus de seipso, vel aliis, aliquid revelet, quod naturales ingenii nostri vires excedat, qualia sunt mysteria incarnationis & trinitatis, non recusabimus illa credere, quamvis non clarè intelligamus, nec ullo modo mirabimur, multa esse tum in immensa ejus natura, tum etiam in rebus ab eo creatis, quæ captum nostrum excedant.*]

AND let me add on this occasion, that whereas the main scruples, that are said to be suggested by philosophy against some mysterious articles of religion, are grounded upon this, that the modus, as they speak of those things, is not clearly conceivable, or at least, is very hardly explicable; these objections are not always so weighty, as perhaps, by the confidence, wherewith they are urged, you many think them. For, whereas I observed to you already, that there are divers things maintained by school divines, which are not contained in the scripture, that observation is chiefly applicable to the things we are considering; since in several of these nice points, the scripture affirms only the thing, and the schoolmen are pleased to add the modus: and as by their unwarrantable boldness, the school divines determine many things without book; so the scruples and objections, that are made against what the scripture really delivers, are usually grounded upon the erroneous or precarious assertions of the school philosophers, who often give the title of metaphysical truths to conceits, that do very little deserve that name, and to which a rigid philosopher would perhaps think, that of sublime nonsense more proper. But of this I elsewhere say enough, and therefore shall now proceed to the consideration I chiefly intended, viz. that from hence, that the modus of a revealed truth is either very hard, or not at all explicable, it will not necessarily follow, that the thing itself is irrational, provided the positive proofs of its truth be sufficient in their kind. For even in natural things philosophers themselves do and must admit several things, whereof they cannot clearly explicate or perhaps conceive the modus. I will not here mention the origin of substantial forms as an instance in this kind, because, though it may be a fit one as to the Peripatetick philosophy, yet not admitting, that there are any such beings, I will take no farther notice of them; especially because, for a clear instance to our present purpose, we need go no further than ourselves, and consider the union of the soul and body in man. For who can physically explain, both how an immaterial substance should be able to guide or determine, and excite the motions of a body, and yet not be able to produce motion in it (as by dead palsies, great faintnesses, &c. it appears the soul cannot,) and, which is far more difficult, how an incorporeal substance should receive such impressions from the motions of a body, as to be thereby affected with real pain and pleasure; to which I elsewhere add some other properties of this union, which, though not taken notice of, are perhaps no less difficult to be conceived and accounted for. For how can we comprehend, that there should be naturally such an intimate union betwixt two such distant substances, as an (incorporeal) spirit and a body, as that the former may not, when it pleases, quit the latter, which cannot possibly have any strings or chains, that can tie, or fasten to it that, which has no body, on which they may take hold. And I there shew, that it is full as difficult, physically to explicate, how these so differing beings come to be united, as how they are kept from parting at pleasure, both the one and the other being to be resolved into the mere appointment of God. And if to avoid the abstruseness of the modus of of this conjunction betwixt the rational ſoul and the human body, it be ſaid, as it is by the Epicureans, that the former is but a certain contexture of the finer and moſt ſubtle parts of the latter, the formerly propoſed abſtruſeneſs of the union betwixt the ſoul and the body will indeed be ſhifted off; but it will be by a doctrine, that will not much relieve us. For thoſe, that will allow no ſoul in man but what is corporeal, have a modus to explain, that I doubt they will always leave a riddle. For of ſuch I deſire, that they would explain to me, (who know no effects, that matter can produce, but by local motion and reſt, and the conſequences of it,) how mere matter, (let them ſuppoſe it as fine as they pleaſe, and contrive it as well as they can) can make ſyllogiſms, and have conceptions of univerſals, and invent ſpeculative ſciences and demonſtrations, and in a word do all thoſe things, which are done by man, and by no other animal; and he, that ſhall intelligibly explicate to me the modus of matter's framing theories and ratiocinations, will, I confeſs, not only inſtruct me, but ſurprize me too.

AND now give me leave to make this ſhort reflection on what has been ſaid in this ſection, compared with what formerly I ſaid in the firſt ſection; that if on the one hand we lay aſide all the irrational opinions, that the ſchoolmen and other bold writers have unwarrantably fathered on Chriſtian religion, and on the other hand all the erroneous conceits repugnant to Chriſtianity, which the ſchoolmen and others have prooflesly fathered upon philoſophy, the ſeeming contradictions betwixt ſolid divinity and true philoſophy will appear to be but few, as I think the real ones will be found to be none at all.

SECTION VI.

THE next conſideration I ſhall propoſe, is, that a thing may, if ſingly or preciſely conſidered, appear unreaſonable, which yet may be very credible, if conſidered as a part of, or a manifeſt conſequence from a doctrine, that is highly ſo.

OF this I could give you more inſtances in ſeveral arts and ſciences, than I think fit to be here ſpecified; and therefore I ſhall content myſelf to mention three or four.

WHEN aſtronomers tell us, that the ſun, which ſeems not to us a foot broad, nor conſiderably bigger than the moon, is above a hundred and threeſcore times bigger than the whole globe of the earth, which yet is forty times greater than the moon; the thing thus nakedly propoſed ſeems very incredible. But yet, becauſe aſtronomers very ſkilful in their art have, by finding the ſemidiameter of the earth, and obſerving the parallaxes of the planets, concluded the proportion of theſe three bodies to be ſuch as has been mentioned, or thereabout, even learned and judicious men of all ſorts, (philoſophers, divines, and others,) think it not credulity to admit what they affirm.

So the relations of earthquakes, that have reached divers hundreds of miles; of eruptions of fire, that have at once overflown and burned vaſt ſcopes of land; of the blowing up of mountains by their own fires; of the caſting up of new iſlands in the ſea itſelf, and other prodigies of too unqueſtionable truth; (for I know what work ignorance and ſuperſtition have made about other prodigies:) if they were atteſted but by ſlight and ordinary witneſs, they would be judged incredible, but we ſcruple not to believe them, when the relations are atteſted with ſuch circumſtances, as make the teſtimony as ſtrong, as the things atteſted are ſtrange.

IF ever you have conſidered, what *Clavius*, and divers other geometricians teach upon the ſixteenth propoſition of the third book of *Euclid*, (which contains a theorem about the tangent, and the circumference of a circle,) you cannot but have taken notice, that there are ſcarce greater paradoxes delivered by philoſophers or divines, than you will find aſſerted by geometricians themſelves. And though of late the learned Jeſuit

Tacquet, and fome rigid mathematicians, have queftioned divers of thofe things, yet even what fome of thefe fevere examiners confefs to be geometrically demonftrable from that propofition, contains things fo ftrange, that philofophers themfelves, that are not well acquainted with that propofition and its corollaries, can fcarce look upon them as other than incomprehenfible, or at leaft incredible, things; which yet, as improbable as they are confidered in themfelves, even rigid demonftrators refufe not to admit, becaufe they are legitimately deducible from an acknowledged truth.

AND fo alfo among the magnetical phænomena there are divers things, which, being nakedly propofed, muft feem altogether unfit to be believed, as indeed having nothing like them in all nature; whereas thofe, that are verfed in magnetick philofophy, even before they have made particular trials of them, will look upon them as credible, becaufe, how great paradoxes foever they may feem to others, they are confonant and confequent to the doctrine of magnetifm, whofe grand axioms (from what caufe foever magnetifms are to be derived) are fufficiently manifeft; and therefore a magnetical philofopher would not, though an ordinary philofopher would, think it unreafonable to believe, that one part of the fame loadftone fhould draw a needle to it, and the other part drive the fame needle from it; and that the needle in a feaman's compafs, after having been carried may hundred leagues (through differing climates, and in ftormy weather) without varying its declination, may, upon a fudden, without any manifeft caufe, point at fome part of the horizon feveral whole degrees diftant from that, which it pointed to before. To which might here be added divers other fcarce credible things, which either others or I have tried about magnetical bodies; but I fhall hereafter have occafion to take notice of fome of them in a fitter place.

WHEREFORE, when fomething delivered in, or clearly deduced from fcripture, is objected againft, as a thing, which it is not reafonable to believe, we muft not only confider, whether, if it were not delivered in that book, we fhould upon its own fingle account think it fit or unworthy to be believed; but whether or no it is fo improbable, that it is more fit to be believed, that all the proofs, that can be brought for the authority of the fcripture, are to be rejected, than that this thing, which comes manifeftly recommended to our belief by that authority, is worthy to be admitted: I fay, "manifeftly recommended by that authority," becaufe that, if the thing be not clearly delivered in fcripture, or be not clearly and cogently deduced thence, fo far as that clearnefs is wanting, fo far the thing itfelf wants of the full authority of the fcripture, to impofe it on our affent.

[PERHAPS it will procure what I have faid the better reception, if I add a couple of teftimonies not of any modern bigots, no, nor of any devout fathers of the church; but of two modern authors of fects, and who in their kinds have been thought extremely fubtle reafoners, and no lefs rigid exacters of reafon in whatever they admitted.

THE firft paffage I fhall alledge, is the confeffion of *Socinus*, who in his fecond epiftle to *Andreas Dudithius*, fpeaks thus: *Jam verò ut rem in pauca conferam, quod ad meas aliorúmve opiniones, quæ novitatis præ fe ferunt fpeciem, attinet, mihi ita videtur; fi detur, fcripturam facram ejus effe authoritatis, ut nullo modo ei contradici poffit, ac de interpretatione illius omnis duntaxat fit fcrupulus*, (which he allows) *nihil, utut verifimile aut ratione conclufum videatur, afferri contra eas poffit, quod ullarum fit virium, quotiefcunque illæ fententiis atque verbis illius libri aut rationibus liquidò inde deductis probatæ atque affertæ fuerint.* Which confeffion of *Socinus* is furpaffed by that of his champion *Smalcius*, to be produced elfewhere in this paper. The other paffage I met with in the excellent Monfieur *Des Cartes*'s principles of philofophy, where difcourfing of the either infinite or indefinite divifion of the particles of matter, which is neceffary to make them fill exactly all the differingly figured fpaces, through which various motions

Part II. Artic. 34, 35.

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do ſometimes make them paſs; he confeſſes, as he well may, that the point is exceedingly abſtruſe, and yet concludes: *Et quamvis quomodo fiat indefinita iſta diviſio, cogitatione comprehendere nequeamus, non ideo tamen debemus dubitare, quin fiat, quia clarè percipimus illam neceſſario ſequi ex natura materiæ nobis evidentiſſimè cognitâ,* &c.]

AND in this place it may be ſeaſonable, as well as pertinent, to take notice of three or four particulars, which, though they be in ſome meaſure implied in the former general conſideration, yet deſerve to be diſtinctly inculcated here, both for their importance, and becauſe they may as well be deduced as corollaries from the foregoing diſcourſe, as be confirmed by the proofs I ſhall add to each of them. Of theſe the firſt ſhall be this, that we muſt not preſently conclude a thing to be contrary to reaſon, becauſe learned men profeſs or even complain, that they are not able clearly to comprehend it, provided there be competent proof, that it is true, and the thing be primary or heteroclite.

FOR it is not always neceſſary to the making the belief of a thing rational, that we have ſuch a comprehenſion of the thing believed as may be had, and juſtly required in ordinary caſes; ſince we may be ſure of the truth of a thing, not only by arguments ſuggeſted by the nature of the thing itſelf clearly underſtood by us, but by the external teſtimony of ſuch a witneſs, as we know will not deceive us, and cannot (at leaſt in our caſe) be reaſonably ſuſpected to be himſelf deceived. And therefore it may in ſome caſes ſuffice to make our belief rational, that we clearly diſcern ſufficient reaſon to believe, that a thing is true, whether that reaſon ſpring from the evidence and cogency of the extrinſick motives we have to believe, or from the proofs ſuggeſted to us by what we know of the thing believed, nay, though there be ſomething in the nature of that thing, which does puzzle and poſe our underſtanding.

THAT many things, that are very hard, and require a great attention, and a good judgment to be made out, may yet be true, will be manifeſt from what I ſhall, within a page or two, note about divers geometrical demonſtrations, which require, beſides a good ſtock of knowledge in thoſe mattters, an almoſt invincible patience, to carry ſo many things along in one's mind, and go through with them. That alſo there are other things, which, though they be as manifeſtly exiſtent, as thoſe newly mentioned can be demonſtratively true, are yet of ſo abſtruſe a kind, that it is exceeding difficult to frame clear and ſatisfactory notions of their nature, we might learn, if we were inquiſitive enough, even from ſome of the moſt obvious things; ſuch as, for inſtance, matter and time: As to the former whereof, (matter,) though the world and our own bodies be made of it, yet the ideas, that are wont to be framed of it, even by the greateſt clerks, are incumbered with too great difficulties (ſome of which I elſewhere mention) to be eaſily acquieſced in by conſidering men. And as for the latter, (time,) though that juſtly celebrated ſaying of *Auguſtine, Si nemo ex me quærat, quid ſit tempus, ſcio; ſi quærenti explicare velim, neſcio;* ſeem in the firſt part of it to own a knowledge of what time is, yet by the latter part, (wherein he confeſſes he cannot declare what it is,) I am not only allowed to believe, that he could not propoſe an intelligible idea of it, but invited to think, that, in the firſt part of the ſentence, he only meant, that when he did not attentively conſider the nature of it, he thought he underſtood it, or that he knew, that there is ſuch a thing as time, though he could no explain what it is.

AND indeed, though time be that, which all men allow to be, yet, if *per impoſſibile* (as the ſchools ſpeak) a man could have no other notion or proof of time and eternity, (even ſuch eternity as muſt be conceded to ſomething,) than what he could collect from the beſt deſcriptions of its nature and properties, that are wont to be given; I ſcarce doubt, but he would look upon it as an unintelligible thing, and incumbered with too many difficulties to be fit to be admitted into a wiſe man's belief. And this perhaps you will grant me, if you have ever put yourfelf to the penance of perufing thofe confounding difputes and fpeculations about time and eternity, that partly in *Ariftotle* and his commentators, and partly among the fchoolmen, and others, are to be met with upon thefe abftrufe fubjects. And no wonder, fince the learned *Gaffendus* and his followers have very plaufibly (if not folidly) fhewn, that duration (and time is but duration meafured) is neither a fubftance nor an accident, which they alfo hold of fpace; about which the altercations among philofophers and fchoolmen are but little, if at all, inferior to thofe about time. And I the rather choofe to mention thefe inftances of time and fpace, becaufe they agree very well with what I intimated by the expreffion of primary or heteroclite things.

To which may be referred fome of thofe things, that are called fpiritual or fupernatural, about which the fame confiderations may have place, efpecially by reafon of this affinity between them, that when we treat of either, fome proofs may in certain cafes be fufficient, in fpite of fuch objections, as in other (and more ordinary cafes) fhould invalidate arguments feemingly as ftrong as thofe proofs.

If it be here objected, that I am too bold in venturing, without the precedence or authority of learned men, to introduce fo great a difference betwixt other things, and thofe, which I call primary and heteroclite; I anfwer, that I fhall not folicitoufly enquire, whether any others have had the fame thoughts, that I propofed; fince, whether they be new or no, they ought not to be rejected, if they be rational.

And I have this inducement to fuppofe, that there ought to be in fome cafes a great difference between them and other things, and confequently between the judgments we make of the ways of arguing about them, and about other things; fo that they are exceeding difficult, to be clearly conceived and explicated by our imperfect faculties, and by that difficulty, apt to make what men fay of them, though true, to be lefs fatisfactory and acquiefced in, than things not more true or rational, fuggefted upon enquiries about fubjects more familiar, or which are, at leaft, more proportionate to our faculties: for thofe abftrufe things, of which we have been fpeaking, being fuch, as either have no proper and clear genus, by the help of which they may be comprehended, or have not any thing in nature, that is (fufficiently, like them) by a refemblance to which we may conceive them; or being perhaps, both primary and heteroclite too, as not being derived from the common phyfical caufes of other things, and having a nature widely differing from the reft of things; it is no wonder, that our limited and imperfect underftandings fhould not be able to reach to a full and clear comprehenfion of them; but fhould be fwallowed up with the fcruples and difficulties, that may be fuggefted by a bold and nice enquiry into things, to which there feems to belong, in fome refpect or other, a kind of infinity.

Upon thefe, and other confiderations of kin to them, I count it not irrational to think, that things primary and heteroclite, as alfo by a parity of reafon, fome things immaterial and fupernatural, may be fufficiently proved in their kind, if there be fuch a pofitive proof of them, as would be competent and fatisfactory, in cafe there were no confiderable objections made againft the thing proved (efpecially fuppofing, that the afferted doctrine be not incumbered with much greater inconveniencies than the contrary doctrine, or than any other, propofed concerning that fubject:) nay, I know not, why we may not, in judging of primary, and of immaterial things, fafely enough prefer that opinion, which has the more cogent pofitive proofs, though it feem liable to fomewhat the greater inconveniencies; becaufe, in fuch cafes, our underftanding is gratified with what it moft requires in all cafes, that is, competent pofitive inducements to affent; and it is not confounded by the objections, becaufe a difability

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to anſwer them directly, and fully, may very well proceed, either from the too abſtruſe nature of the thing, or the limitedneſs and weakneſs of our human intellects.

AND thus we may render a reaſon, why, when we diſcourſe of ſuch uncommon matters, we may ſometimes reaſonably acquieſce in proofs, in ſpight of ſuch objections, as in ordinary caſes would be prevailing ones. For the things, about which theſe proofs are converſant, being primary or heteroclite, or of as abſtruſe a nature, as if they were ſo, it too often happens, that, what opinion ſoever we chooſe about them, we muſt admit ſomething, that is incumbered with great difficulties, and therefore will be liable to great objections, that perhaps will never be directly and ſatisfactorily anſwered. And ſince it may fare thus with us, where two oppoſite opinions are contradictory, we may conclude, that thoſe difficulties will not cogently evince the falſity of a theological opinion, which are but ſuch, that the ſame, or as great, may be objected againſt another, that either is manifeſtly or confeſſedly a truth, or which muſt neceſſarily be admitted to be one, if the contrary theological tenet be ſuppoſed not to be one.

2. ANOTHER corollary, that may be drawn from the diſcourſe, that afforded us the former, may be this; that it may not be unreaſonable to believe a thing, though its proof be very difficult to be underſtood. To manifeſt this, I ſhall need no other argument, than what may be afforded by divers geometrical and other mathematical demonſtrations; ſome of which are fetched, by intermediate concluſions, from principles ſo very remote, and require ſo long a ſeries of mediums to be employed about them, that not only a man, that were of *Pilate*'s temper, who having aſked him, that could beſt tell him, what is truth? would not ſtay a while to be ſatisfied about his enquiry, would, before he reaches half way to the end of the demonſtration, or perhaps of the lemmas, be quite diſcouraged from proceeding any further; but even ſedulous and heedful peruſers do find themſelves oftentimes unable to carry along ſuch a chain of inferences in their minds, as clearly to diſcern, whether the whole ratiocination be coherent, and all the particulars have their due ſtrength and connection. And if you pleaſe to make a trial upon ſome of the demonſtrations of *Vitellio*, or even of *Clavius*, that I can direct you to, I doubt they will put you to the full exerciſe of your patience, and quite tire your attention: and though the modern algebraiſts, by their excellent way of expreſſing quantities by ſymbols, have ſo abridged geometrical and arithmetical demonſtrations, that, by the help of ſpecies, it is ſometimes eaſy to demonſtrate, that in a line, which in the ordinary way would require a whole page, (as our moſt learned friend Dr. *Ward* has ingeniouſly ſhewn, by giving the demonſtrations of about twenty of Mr. *Hobbes*'s theorems, in leſs than ſo many lines;) yet ſome demonſtrable truths are ſo abſtruſe, that, even in the ſymbolical way, men need more attention to diſcern them, than moſt men would employ in any ſpeculation whatſoever. And *Des Cartes* himſelf, as famous and expert a maſter as he was in this way, confeſſes, in a letter to one of his friends, that the ſolution of a problem in *Pappus* coſt him no leſs than ſix weeks ſtudy; though now, moſt mathematical demonſtrations do indeed ſeem far ſhorter than they are, becauſe that *Euclid*'s elements being generally received among mathematicians, all his propoſitions are ſo many lemmata, which need be but referred to in the margin, being known and demonſtrated already. By all which it may appear, that, granting ſome theological truths to be complained of by many, as things ſo myſterious and abſtruſe, that they cannot readily diſcern the force of thoſe proofs, that *Des Cartes*, and other ſubtile ſpeculators, have propoſed to evince them; yet if other learned men, that are competent eſtimators, and are accuſtomed to bring much patience and attention to the diſcernment of difficult and important truths, profeſs themſelves ſatisfied with them, the probations may yet be cogent, notwithſtanding the

difficulty

you will grant me, if you have ever put yourfelf to the penance of perufing thofe confounding difputes and fpeculations about time and eternity, that partly in *Ariftotle* and his commentators, and partly among the fchoolmen, and others, are to be met with upon thefe abftrufe fubjects. And no wonder, fince the learned *Gaffendus* and his followers have very plaufibly (if not folidly) fhewn, that duration (and time is but duration meafured) is neither a fubftance nor an accident, which they alfo hold of fpace; about which the altercations among philofophers and fchoolmen are but little, if at all, inferior to thofe about time. And I the rather choofe to mention thefe inftances of time and fpace, becaufe they agree very well with what I intimated by the expreffion of primary or heteroclite things.

To which may be referred fome of thofe things, that are called fpiritual or fupernatural, about which the fame confiderations may have place, efpecially by reafon of this affinity between them, that when we treat of either, fome proofs may in certain cafes be fufficient, in fpite of fuch objections, as in other (and more ordinary cafes) fhould invalidate arguments feemingly as ftrong as thofe proofs.

IF it be here objected, that I am too bold in venturing, without the precedence or authority of learned men, to introduce fo great a difference betwixt other things, and thofe, which I call primary and heteroclite; I anfwer, that I fhall not folicitoufly enquire, whether any others have had the fame thoughts, that I propofed; fince, whether they be new or no, they ought not to be rejected, if they be rational.

AND I have this inducement to fuppofe, that there ought to be in fome cafes a great difference between them and other things, and confequently between the judgments we make of the ways of arguing about them, and about other things; fo that they are exceeding difficult, to be clearly conceived and explicated by our imperfect faculties, and by that difficulty, apt to make what men fay of them, though true, to be lefs fatisfactory and acquiefced in, than things not more true or rational, fuggefted upon enquiries about fubjects more familiar, or which are, at leaft, more proportionate to our faculties: for thofe abftrufe things, of which we have been fpeaking, being fuch, as either have no proper and clear genus, by the help of which they may be comprehended, or have not any thing in nature, that is (fufficiently, like them) by a refemblance to which we may conceive them; or being perhaps, both primary and heteroclite too, as not being derived from the common phyfical caufes of other things, and having a nature widely differing from the reft of things; it is no wonder, that our limited and imperfect underftandings fhould not be able to reach to a full and clear comprehenfion of them; but fhould be fwallowed up with the fcruples and difficulties, that may be fuggefted by a bold and nice enquiry into things, to which there feems to belong, in fome refpect or other, a kind of infinity.

UPON thefe, and other confiderations of kin to them, I count it not irrational to think, that things primary and heteroclite, as alfo by a parity of reafon, fome things immaterial and fupernatural, may be fufficiently proved in their kind, if there be fuch a pofitive proof of them, as would be competent and fatisfactory, in cafe there were no confiderable objections made againft the thing proved (efpecially fuppofing, that the afferted doctrine be not incumbered with much greater inconveniencies than the contrary doctrine, or than any other, propofed concerning that fubject:) nay, I know not, why we may not, in judging of primary, and of immaterial things, fafely enough prefer that opinion, which has the more cogent pofitive proofs, though it feem liable to fomewhat the greater inconveniencies; becaufe, in fuch cafes, our underftanding is gratified with what it moft requires in all cafes, that is, competent pofitive inducements to affent; and it is not confounded by the objections, becaufe a difability

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difficulty to have their strength apprehended. For, if such a difficulty ought to pass for a mark, that a ratiocination is not valid, no reasonings will be found fitter to be rejected or distrusted, than many of those, whose cogency has procured such a repute to mathematical demonstrations.

3. IT may also be deduced from the foregoing discourse, that it is not always against reason to embrace an opinion, which may be incumbered with a great difficulty, or liable to an objection not easy to be solved; especially if the subject be such, that other opinions about it avoid not either the same inconveniencies, or as great ones. The first part of what is said in this consideration will often follow from the supposition made in the precedent discourse. For those things, that render a doctrine or assertion difficult to be conceived and explained, will easily supply the adversaries of it with objections against it.

AND as for the latter, viz. the clause, which takes notice, that the consideration, to which it is annexed, will chiefly take place in that sort of opinions, that are specified in it; it will need but little of distinct proof.

FOR it is manifest enough, that if the subject or object, about which the opinion proposed is conversant, be such, that not only the contradictory opinion, but others also, are obnoxious either to the same inconveniencies, or to others, that are equal or greater; the difficulties, that are urged against a theological doctrine, may (as hath been shewn already in the first corollary,) be rationally enough attributed, not to the unreasonableness of the opinion, but to somewhat else.

THE last consectary, that (as I intimated) may be deduced from the precedent discourse, is, that it is not always unreasonable to believe something theological for a truth, which (I do not say, is truly inconsistent with, but) we do not clearly discern to comport very well with something else, that we also take for a truth, or perhaps, that is one indeed; if the theological tenet be sufficiently proved in its kind, and be of that sort of things, that we have been of late, and are yet discoursing of.

THE generality of our philosophers, as well as divines, believe, that God has a foreknowledge of all future contingencies; and yet how a certain prescience can consist with the freewill of man, (which yet is generally granted him, in things merely moral or civil,) is so difficult to discern, that the Socinians are wont to deny such things, as depend upon the will of free agents, to be the proper objects of omniscience; and the head of the Remonstrants, though a very subtile writer, confesses, that he knows not, how clearly to make out the consistency of God's prescience, and man's freedom; both which he yet confesses to be truths, being compelled to acknowledge the former, (for the latter is evident,) as well by the infiniteness, that must be ascribed to God's perfections, as by the prophetick predictions, whereby such contingent events have been actually foretold. And the reconcilement of these truths is not a difficulty peculiar to the Christian religion, but concerns speculative men in all religions, who acknowledge the Deity to be infinitely perfect, and allow man, as they do, to be a free agent.

[BUT I have made this section so prolix already, that I must not enlarge on this third particular. And therefore I shall shut it up with an acknowledgment of *Des Cartes*, which may be applied not only to it, but to almost all, that has been discoursed in this section, and indeed to a great part of this letter. He then in an epistle, that came not forth, till some years after the writer's death, speaks thus to the philosophical adversary, to whom it is addressed: "As I have often said, when the question "is about things that relate to God, or to what is infinite, we must not consider, "what we can comprehend of them, (since we know, that they ought not to be comprehended by us) but only what we can conceive of them, or can attain to by any "certain reason or argument."]

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SECTION

SECTION VII.

AND now it is time to advance to one of the main confiderations I had to propofe to you, concerning the fubject of this letter, and it is this; that when we are to judge, whether a thing be contrary to reafon or not, there is a great deal of difference, whether we take reafon for the faculty furnifhed only with its own innate principle, and fuch notions, as are generally obvious, (nay, and if you pleafe, with this or that philofophical theory;) or for the faculty illuminated by divine revelation, efpecially that, which is contained in the books commonly called the Scripture.

To clear and enforce this the better, I fhall invite you to take notice with me of the two following particulars.

WE may then in the firft place confider, that even in things merely natural, men do not think it at all irrational, to believe divers fuch things upon extrinfecal proofs, efpecially the teftimony of the fkilful, as, if it were not for that teftimony, a man, though born with good parts, and poffibly very learned in the Peripatetick, or fome other particular philofophy, would look upon as irrational to be believed, and contrary to the laws of nature.

OF this I fhall give you fome inftances in the phænomena of the loadftone, and particularly fuch as thefe; that the loadftone, though (as was above intimated) with one part it will draw, yet with another the fame ftone will repel the fame point of the fame excited needle; and yet at the fame time be fit to attract either point of another needle, that never came near a loadftone before: that though it be the loadftone, that imparts an attractive virtue to the iron, yet when the loadftone is capped, as they called it, and fo a piece of iron (and confequently a diftance) is interpofed betwixt the ftone and the weight to be raifed, it will take up by many times more, than if it be itfelf applied immediately thereunto, infomuch that *Merfennus* relates, that (if there be no miftake,) he had a loadftone, that of itfelf would take up but half an ounce of iron, which when armed (or capped) would lift up ten pounds, which, fays he, exceeded the former weight three hundred and twenty times: that a mariner's needle, being once touched with a vigorous loadftone, will afterwards, when freely poifed, turn itfelf north and fouth; and if it be by force made to regard the eaft and weft, or any other points of the compafs, as foon as it is left at liberty, it will of itfelf return to its former pofition: that a loadftone floating on water will as well come to, and follow a piece of iron, that is kept from advancing towards it, as, when itfelf is fixed, and the iron at liberty, it will draw that metal to it: that without any fenfible alteration in the agent or the patient, the loadftone will in a trice communicate all its virtues to a piece of fteel, and enable that to communicate them to another piece of the fame metal: that if a loadftone, having been marked at one end, be cut longwife according to its axis, and one fegment be freely fufpended over the other, the halves of the marked end, that touched one another before, will not now lie together, but the lower will drive away the upper; and that, which regarded the north in the marked end of the intire loadftone, will join with that extreme of the lower half, which in the intire ftone regarded the fouth: that (as appears by this laft named property) there are the fame magnetical qualities in the feparated parts of a magnet, as in the intire ftone; and if it be cut, or even rudely broken into a great many parts or fragments, every one of thefe portions, though perhaps not fo big as a corn of wheat, will, if I may fo fpeak, fet up for itfelf, and have its own northern and fouthern poles, and become a little magnet *fui juris*, or independent upon the ftone, from which it was fevered, and from all its other parts: that if a loadftone be fkilfully made fpherical, this little magnetick

In his little tract *De Magnetis Proprietatibus*. p. m. 350.

netick globe, very fitly by our *Gilbert* called a terrella, will not only, being freely placed, turn north and fouth, and retain that pofition, but have its poles, its meridians, its æquator, &c. upon good grounds defignable upon it, as they are upon the great globe of the earth. And this will hold, whether the terrella be great or fmall.

I might not only much encreafe the number of thefe odd magnetical phænomena, but add others about other fubjects; but thefe may fuffice to fuggeft to us this reflection, that there is no doubt to be made, but that a man, who never had the opportunity to fee or hear of magnetical experiments, would look upon thefe as contrary to the principles of nature, and therefore to the dictates of reafon, as accordingly fome learned Ariftotelians, to whom I had occafion to propofe fome of them, rejected them as incredible. And I doubt not, but I could frame as plaufible arguments from the mere axioms of philofophers, and the doctrine of philofophick fchools againft fome magnetical phænomena, which experience hath fatisfied me of, as are wont to be drawn from the fame topicks againft the myfterious articles of faith; fince among the ftrange properties of the loadftone there are fome, which are not only admirable and ftupendous, but feem repugnant to the dictates of the received philofophy and the courfe of nature. For whereas natural bodies, how fubtile foever, require fome particular difpofitions in the medium, through which their corpufcles are to be diffufed, or their actions tranfmitted; fo that light itfelf, whether it be a moft fubtile body, or a naked quality, is refifted by all opacous mediums, and the very effluvia of amber and other electricks will not permeate the thinneft glafs, or even a fheet of fine paper; yet the loadftone readily performing his operations through all kind of mediums, without excepting glafs itfelf.

If the poles of two magnetick needles do both of them regard the north, another philofopher would conclude them to have a fympathy, at leaft to be unlikely to difagree: and yet, if he bring thefe extremes of the fame denomination within the reach of one another, one will prefently drive away the other, as if there were a powerful antipathy between them.

A fomewhat long needle being placed horizontally, and exactly poifed upon the point of a pin, if you gently touch one end with the pole of a vigorous magnet, that end fhall manifeftly dip or ftoop, though you often take it off the pin, and put it on again. And this inclination of the needle will continue many years, and yet there is not only no other fenfible change made in the metal by the contact of the loadftone; but one end has required a durable preponderancy, though the other be not lighter, nor the whole needle heavier than before. And the inclination of the magnetick needle may be by another touch of the loadftone taken away, without leffening the weight of the part, that is deprived of it.

The operation, that, in a trice, the loadftone has on a mariner's needle, though it makes no fenfible change in it, or weakens the loadftone itfelf, will not be loft, though you carry it as far as the fouthern hemifphere: but it will not be the fame in all places, but in fome, the magnetick needle will point directly at the north, in others, it will deviate or decline fome degrees towards the eaft or the weft: and, which feems yet more ftrange, the fame needle in the fame place will not always regard the fame point of the compafs, but, looked on at diftant times, may vary from the true meridian, fometimes to the weft, and afterwards to the eaft.

All the communicable virtues of the magnet may be imparted to iron, without any actual contact of the two bodies, but barely by approaching in a convenient way the iron to the loadftone for a few moments. And the metal may likewife be deprived of thofe virtues in a trice, without any immediate contact by the fame, or another loadftone.

If

If you mark one end of a rod, or other oblong piece of iron, that never came near a magnet, and hold it perpendicularly, you may at pleaſure, and in the hundredth part of a minute, make it become the north or ſouth pole of a magnetical body. For if, when it is held upright, you apply to the bottom of it the north extreme of an excited and well-poiſed needle, the lower end of the iron will drive away that extreme, which yet will be drawn by the upper end of the ſame iron. And if, by inverting, you make this lower end the uppermoſt, it will not attract, but repel the ſame lilly or north point of the needle, juſt under which it is to be perpendicularly held.

Though *vis unita fortior* be a received rule among naturaliſts; yet oftentimes, if a magnet be cut into pieces, theſe will take up, and ſuſtain much more iron, than the entire ſtone was able to do.

If, of two good loadſtones, the former be much bigger, and on that account ſtronger than the other, the greater will draw a piece of iron, and retain it much more ſtrongly than the leſſer; and yet, when the iron ſticks faſt to the greater and ſtronger loadſtone, the leſſer and weaker may draw the iron from it, and take it quite away.

These phænomena, (to mention now no more,) are ſo repugnant to the common ſentiments of naturaliſts, and the ordinary courſe of things, that, if, antecedently to any teſtimony of experience, theſe magnetical properties had been propoſed to *Ariſtotle* himſelf, he would probably have judged them fictitious things, as repugnant to the laws of nature: nevertheleſs, though it ſeems incredible, that the bare touch of a loadſtone ſhould impart to the mariner's needle a property, which, (as far as we know) nothing in the whole world, that is not magnetical, can communicate or poſſeſs; and ſhould operate (as men ſuppoſe) upon it at three or four thouſand leagues diſtance; yet this is believed by the Peripateticks themſelves upon the teſtimony of thoſe navigators, that have ſailed to the *Eaſt* and *Weſt-Indies*; and divers even of the more rigid of the modern philoſophers believe more than this, upon the teſtimony of *Gilbert*, *Cabæus*, *Kircherus*, and other learned magnetical writers, who have affirmed theſe things; moſt of which I can alſo aver to you upon my own knowledge.

Thus the habitableneſs of the torrid zone, though (as I lately noted) upon probable grounds denied by *Ariſtotle*, and the generality of philoſophers for many ages; yet not only that, but its populouſneſs, is now confidently believed by the Peripatetick ſchoolmen themſelves, who never were there.

And though *Ptolomy*, and ſome other eminent aſtronomers, did with great care and ſkill, and by the help of geometry, as well as obſervations, frame a theory of the planets ſo plauſibly contrived, that moſt of the ſucceeding mathematicians for twelve or fourteen ages acquieſced in it; yet almoſt all the modern philoſophers and aſtronomers, that have ſearched into theſe matters, with a readineſs to believe their eyes, and allow their reaſon to act freely, have been forced, if not to reject the whole theory, yet, at leaſt to alter it quite, as to the number and order of the planets, though theſe laſt named innovations are ſometimes ſolely, and always mainly, built upon the phænomena, diſcovered to us by two or three pieces of glaſs placed in a long hollow cane, and honoured with the name of a teleſcope.

The laſt of the two things, I invited you to conſider with me, is this, that when we are to judge, which of two diſagreeing opinions is moſt rational, i. e. to be judged moſt agreeable to right reaſon, we ought to give ſentence, not for that, which the faculty, furniſhed only with ſuch and ſuch notions, whether vulgar, or borrowed from this or that ſect of philoſophers, would prefer, but that, which is preferred by the faculty, furniſhed, either with all the evidence requiſite or advantageous to make it give a right judgment in the caſe lying before it, or, when that cannot be had, with the beſt and fulleſt informations, that it can procure.

THIS is ſo evident by its own light, that your friend might look upon it as an affront to his judgment, if I ſhould go about ſolicitouſly to prove it. And therefore I ſhall only advertiſe you, that, provided the information be ſuch, as a man has juſt cauſe to believe, and perceives, that he clearly underſtands, it will not alter the caſe, whether he have it by reaſon, as that is taken for the faculty furniſhed but with its inbred notions, and the more common obſervations, or by ſome philoſophical theory, or by experiments purpoſely deviſed, or by teſtimony human or divine, which laſt we call revelation. For all theſe are but differing ways of informing the underſtanding, and of ſignifying to it the ſame thing; as the ſight and the touch may aſſure a man, that a body is ſmooth or rough, or in motion or at reſt; (and in ſome other inſtances, ſeveral ſenſes diſcover to us the ſame object, which is therefore called *objectum commune*;) and provided theſe informations have the conditions lately intimated, which way ſoever the underſtanding receives them, it may ſafely reaſon and build opinions upon them.

ASTRONOMERS have within theſe hundred years obſerved, that a ſtar hath appeared among the fixed ones for ſome time, and having afterwards diſappeared, has yet ſome years after that ſhewed itſelf again. And though, as to this ſurprizing phænomenon, our experimental philoſophers could have contributed nothing to the producing it, and though it is quite out of all the received ſyſtems of the heavens, that aſtronomers have hitherto delivered; yet the ſtar itſelf may be a true celeſtial light, and may allow us to philoſophize upon it, and draw inferences from the diſcoveries it makes us; as well as we can from the phænomena of thoſe ſtars, that are not extraordinary, and of thoſe falling ſtars, that are within our own ken and region.

THAT the ſupernatural things, ſaid to be performed by witches and evil ſpirits, might, if true, ſupply us with hypotheſes and mediums, whereby to conſtitute and prove theories, as well as the phænomena of mere nature, ſeems tacitly indeed, but yet ſufficiently, to be acknowledged, by thoſe modern naturaliſts, that care not to take any other way to decline the conſequences, that may be drawn from ſuch relations, than ſolicitouſly to ſhew, that the relations themſelves are all, as I fear moſt of them are, falſe, and occaſioned by the credulity or impoſture of men.

BUT not to do any more than glance at theſe matters, let us proceed upon what is more unqueſtionable, and conſider, that, ſince even our moſt critical philoſophers do admit many of the aſtoniſhing attributes of magnetick bodies, which themſelves never had occaſion to ſee, upon the teſtimony of *Gilbert*, and others, who never were able to give the true cauſes of them; becauſe they look upon thoſe relators as honeſt men, and judicious enough not to be impoſed upon as to the matter of fact; ſince, I ſay, ſuch amazing things are believed by ſuch ſevere naturaliſts, upon the authority of men, who did not know the intimate nature of magnetick bodies; and ſince theſe ſtrange phænomena are not only aſſented to, as true, by the philoſophers we ſpeak of, but many philoſophical conſequences are without hæſitancy deduced from them, without any blemiſh to the judgment of thoſe, that give their aſſent both to the things and the inferences; why ſhould it be contrary to reaſon to believe the teſtimony of God, either about his nature, which he can beſt, and he alone can fully know, or about the things, which either he himſelf has done, as the creation of the world, and of man; or which he means to do, as the deſtroying the world, (whether the whole world, or our great vortex only, I diſpute not) and the raiſing both of good and bad men to life again, to receive rewards and puniſhments, according to their demerits. For methinks that 1 John v. 9. apoſtle argues very well, who ſays, "if we receive the teſtimony of men, the teſti-"mony of God is greater;" eſpecially about ſuch things concerning his own nature, will, and purpofes, as it is evident, that reafon, by its own unaffifted light, cannot give us the knowledge of.

So that we Chriftians, in affenting to doctrines upon the account of revelation, need not, nor do not, reject the authority of reafon, but only appeal from reafon to itfelf, i. e. from reafon, as it is more flightly, to its dictates, as it is more fully informed. Of which two forts of dictates there is nothing more rational, than to prefer the latter to the former.

AND for my part, I am apt to think, that, if what has been reprefented in this fection, were duly confidered, this alone would very much contribute to prevent or anfwer moft of the objections, that make fuch of the queftioners of religion, as are not refolutely vicious, entertain fuch hard thoughts of fome articles of the Chriftian faith, as if they were directly repugnant to reafon. For, as we were obferving, that is not to be looked on as the judgment of reafon, that is pronounced even by a rational man, according to a fet of notions, though the inferences from thefe would be rational, in cafe there were nothing elfe fit to be taken into confideration by him, that judges; but that is rather to be looked upon as the judgment of reafon, which takes in the moft information procurable, that is pertinent to the things under confideration. And therefore men, though otherwife learned and witty, fhew themfelves not equal eftimators of the cafe of thofe, that believe the articles we fpeak of, when they pronounce them to affent irrationally, becaufe the things they affent to cannot be demonftrated or maintained by mere natural reafon, and would probably be rejected by *Democritus*, *Epicurus*, *Ariftotle*, or any other of the ancient philofophers, to whom they fhould be nakedly propofed, and whofe judgment fhould be defired about them. For, although this allegation would fignify much, if we pretended to prove what we believe only by arguments drawn from the nature of the thing affented to; yet it will not fignify much in our cafe, wherein we pretend to prove what we believe, chiefly by divine teftimony, and therefore ought not to be concluded guilty of an irrational affent, unlefs it can be fhewn, either that divine teftimony is not duly challenged by us for the main of our religion; or that in the particular articles we father fomething on that teftimony, which is not contained in it, or rightly deducible from it. And to put us upon the proving our particular articles of faith, fufficiently delivered in the fcriptures, and not knowable without revelation, by arguments merely natural, without taking notice of thofe we can bring for the proof of that revelation, on whofe account we embrace thofe articles, is to challenge a man to a duel, upon condition he fhall make no ufe of his beft weapons; and is as unreafonable, as if a fchoolman fhould challenge your friend to prove, that the torrid zone is inhabited, againft the reafons, that the Ariftotelians are wont to give to prove it uninhabitable, without allowing him to make ufe of the teftimony of navigators, who affure us of the conftant breezes, that daily ventilate the air, and qualify that heat, which otherwife would not be fupported, and who furnifh us with thofe other circumftances, whereon to build our proofs, which we, that were never there, can have but by relation.

AND indeed, the limitations, that Chriftian religion puts to fome of the dictates of philofophy, which were wont to be admitted in a more general and unreftrained fenfe, and the doctrines about God and the foul, &c. that it fuperadds to thofe, which the light of nature might lead men to about the fame fubjects; though to fome they may feem injurious to philofophy and reafon, are as little unkind to either, as is the gardener to a crab-ftock, or fome fuch other wild plant, when by cutting off fome of the branches, and by making a flit in the bark, that he may graft on it a pare-main, or fome other choice apples, by this feemingly hard ufage he brings it to bear much nobler fruit, than, if left to its own natural condition, it ever would have done.

I know

I know not, whether to all, that hath been faid in this fection, I may not add thus much further, that it fometimes happens, that thofe very things, which at firft were propofed to the underftanding, and believed upon the fcore of revelation, are afterward affented to by it upon the account of mere reafon. To which purpofe I confider, that not any of the ancient philofophers, nay, as far as I have read, even of thofe, that believed God to be the author of the world, dreamed, that he created matter of nothing, but only formed the world out of præ-exiftent matter; whereas Chriftian divines ufually teach, as an article of faith, that befides what they call a mediate creation, as when fifhes were made out of the water, or *Adam*'s body was made out of the earth, there was an immediate production of matter itfelf out of nothing.

SECTION VIII.

AFTER what has been hitherto difcourfed, it may be feafonable to confider, what kind of probation, or what degree of evidence may reafonably be thought fufficient, to make the Chriftian religion thought fit to be embraced.

PERHAPS I fhall not need to tell you, that, befides the demonftrations wont to be treated of in vulgar logick, there are among philofophers three diftinct, whether kinds or degrees, of demonftration. For there is a metaphyfical demonftration, as we may call that, where the conclufion is manifeftly built on thofe general metaphyfical axioms, that can never be other than true; fuch as *nihil poteft fimul effe & non effe*; *non entis nullæ funt proprietates reales*, &c. There are alfo phyfical demonftrations, where the conclufion is evidently deduced from phyfical principles; fuch as are *ex nihilo nihil fit: Nulla fubftantia in nihilum redigitur*, &c. which are not fo abfolutely certain as the former, becaufe, if there be a God, he may (at leaft for aught we know) be able to create and annihilate fubftances; and yet are held unqueftionable by the ancient naturalifts, who ftill fuppofe them in their theories. And laftly, there are moral demonftrations, fuch as thofe, where the conclufion is built, either upon fome one fuch proof cogent in its kind, or fome concurrence of probabilities, that it cannot be but allowed, fuppofing the truth of the moft received rules of prudence and principles of practical philofophy.

AND this third kind of probation, though it come behind the two others in certainty, yet it is the fureft guide, which the actions of men, though not their contemplations, have regularly allowed them to follow. And the conclufions of a moral demonftration are the fureft, that men afpire to, not only in the conduct of private men's affairs, but in the government of ftates, and even of the greateft monarchies and empires. And this is confiderable in moral demonftrations, that fuch may confift, and be, as it were, made up of particulars, that are each of them but probable; of which, the laws eftablifhed by God himfelf among his own people, as well as the practice of our courts of juftice here in *England*, afford us a manifeft inftance in the cafe of murder, and fome other criminal caufes. For, though the teftimony of a fingle witnefs fhall not fuffice to prove the accufed party guilty of murder; yet the teftimony of two witneffes, though but of equal credit, that is, a fecond teftimony added to the firft, though of itfelf never a whit more credible than the former, fhall ordinarily fuffice to prove a man guilty; becaufe it is thought reafonable to fuppofe, that, though each teftimony fingle be but probable, yet a concurrence of fuch probabilities, (which ought in reafon to be attributed to the truth of what they jointly tend to prove) may well amount to a moral certainty, *i. e.* fuch a certainty, as may warrant the judge to proceed to the fentence of death againft the indicted party.

To apply thefe things now to the Chriftian religion: if you confider, with how much approbation from difcerning men, that judicious obfervation of *Ariftotle* has been entertained, where he ſays, that it is as unſkilful and improper a thing, to require mathematical demonſtrations in moral affairs, as to take up with moral arguments in matters mathematical; you will not deny, but that thoſe articles of the Chriſtian religion, that can be proved by a moral, though not by a metaphyſical or phyſical demonſtration, may, without any blemiſh to a man's reaſon, be aſſented to; and that conſequently (by virtue of the foregoing conſiderations) thoſe other articles of the Chriſtian faith, that are clearly and legitimately deducible from the ſo demonſtrated truths, may likewiſe, without diſparagement, be aſſented to.

WE may alſo here conſider further, that the chuſing, or refuſing to embrace the Chriſtian religion, which is not propoſed to us only as a ſyſtem of ſpeculative doctrines, but alſo as a body of laws; according to which, it teaches us, that God commands us to worſhip him, and regulate our lives; the embracing, I ſay, or not embracing this religion, is an act of human choice, and therefore ought to be determined according to the dictates of prudence. Now, though in matters, that very much import us, we may wiſh for and endeavour after ſuch reaſons, whereby to determine our reſolves, as may amount to moral demonſtrations; yet prudence will not always require, that we ſhould refuſe to act upon arguments of a leſs cogency, than moral demonſtrations. For oftentimes, in human affairs, it ſo falls out, that divers hazards, or other inconveniencies, will attend whatever reſolution we take; and in that caſe, all that prudence requires, or can enable us to do, is, to take that reſolution, which upon the whole matter ſeems to be preferable to any other; though that, which is thus preferred, may perhaps be liable to ſome objection, that cannot be directly anſwered, but only obliquely, by the preponderancy of the arguments, that perſuade the choice, againſt which the objection is made.

BUT here perhaps you will tell me, that the ſafeſt way, in caſe of ſuch importance, is to ſuſpend an action, that is every way attended with difficulties, and to forbear either embracing or rejecting the Chriſtian religion, till the truth or falſeneſs of it come to appear evident and unqueſtionable.

To which I anſwer, that indeed in matters of bare ſpeculation, about which our underſtandings only need to be converſant, the ſuſpenſion of aſſent is not only practicable, but uſually the ſafeſt way; but *Des Cartes* himſelf, who has been the greateſt example and inculcator of this ſuſpenſion, declares, that he would have it practiſed only about human ſpeculations, not about human actions; *ſed hæc interim dubitatio ad ſolam* contemplationem *veritatis reſtringenda; non quantum ad uſum vitæ: quia perſæpe rerum agendarum occaſio præteriret, antequam nos dubiis noſtris exolvere poſſemus. Non raro quod tantum eſt veriſimile cogimur amplecti, vel etiam interdùm, etſi è duobus unum altero veriſimiliùs non appareat, alterutrum tamen eligere.* And in ſome of his other writings he ſpeaks ſo much to ſhew, that it is unreaſonable to expect in matters, where embracing or rejecting a courſe, that requires practice, is neceſſary, ſuch a certainty, as he judges neceſſary to make a true philoſopher acquieſce in reference to propoſitions about ſpeculative matters, that I find by one of his letters, that he was vehemently accuſed for having taught, that men need not have as ſure grounds for chuſing virtuous and avoiding vicious courſes, as for determining about things merely notional.

AND here let me obſerve to you the difference, that I take notice of in the caſes, where we are put upon deliberating, whether we will chuſe or refuſe a thing propoſed. For it may be propounded to us, either as a proffer, on whoſe acceptance an advantage may be hoped, or as a duty, which, beſides the advantage it promiſes to the performance, has a penalty annexed to the non-performance, or as an only expedient to avoid a great miſchief, or obtain a great good.

THUS,

Thus, when in the *Theatrum Chymicum* some of its chief authors, as *Lully*, *Geber*, *Artephius*, who pretend to have been *adepti*, i. e. possessors of the elixir, very earnestly exhort their readers to apply themselves to so noble and useful a study as alchymy, (by the help of which, the last named *Artephius* is said to have lived 1000 years,) they make but a proposition of the first sort. For though a prosperous attempt to make the philosophers stone (supposing there be such a thing) would possess a man of an inestimable treasure; yet, if he either refuse to believe these writers, or if he do believe them, refuses to take the pains required of him, that would follow their counsel, he can only miss of the wealth, &c. they would make him hope for, but is really never a whit the poorer, or in a worse condition, than if they had not endeavoured to engage him.

But if an absolute sovereign commands something to be done by his subjects; and to enforce his command, does not only propose great recompenses to those, that shall perform what it prescribed, but threatens heavy penalties to the disobedient; this will belong to the second sort of cases above mentioned, in which, as it is evident, a man has not the same latitude allowed him as in the first.

But if we suppose, that a man by a translation of very peccant matter has got a spreading gangrene in his arm, and a skilful surgeon tell him, that, if he will part with his arm, he may be recovered, and save his life, which else he will certainly lose; this case will belong to the last sort above-mentioned; the patient's parting with his arm being the only remedy of the gangrene, and expedient to save his life, and recover his health. And here also it is manifest, that there are far stronger motives, than those mentioned in the first case, to make a positive and timely resolution.

To bring this home to our subject, I need but mind you, that the Christian doctrine does not only promise a heaven to sincere believers, but theatens no less than a hell to the refractory.

Deutr. xi. 26, 27, 28. The voice of *Moses* to the *Jews* is this, "Behold, I set before you this day a blessing and a curse; a blessing, if ye obey the commandments of the Lord your God, which I command you this day; and a curse, if ye will not obey the commandment of the Lord your God, but turn aside out of the way, which I command you this day."

And the commission, that Christ gave his apostles, to preach the gospel, runs thus: Mark xvi. 15, 16. "Go ye into all the world, and preach the gospel to every creature," i. e. to all mankind; "he that believeth, and is baptized, shall be saved; but he, that believeth not, shall be damned."

By this you may perceive, that as far as there is either truth or probability in the Christian religion, so far forth he, that refuses to become a disciple to it, runs a venture, not only to lose the greatest blessings, that men can hope, but to fall eternally into the greatest miseries that they can fear. And indeed our case, in reference to the Christian religion may not only be referred to the second sort of cases lately mentioned, but to the third sort too. For as the language of the author of the Christian religion was to John viii. 24. his auditors. "If ye believe not, that I am he (the Messias) ye shall die in your sins;" so of the two greatest heralds of it, the one tells the Jews, that neither is there salvation in any other: for, Acts iv. 12. "there is no other name under heaven given among men, whereby we must be saved:" And the other tells the Thessalonians, that the 2 Thess. i. 7, 8, 9. "Lord Jesus shall be revealed from heaven with his mighty angels in flaming fire, taking vengeance on them, that know not God, and obey not the gospel of our Lord Jesus Christ; who shall be punished with everlasting destruction from the presence of the Lord, and from the glory of his power."

By all this it appears, that the Christian religion is not proposed barely as a proffer of heaven in case men embrace it, but as a law, that men should embrace it upon the greatest

greateſt penalty, and as the only expedient and remedy to attain eternal happineſs, and eſcape endleſs miſery; ſo that the forbearing to ſubmit our necks to the yoke of Chriſt being as well a ruinous courſe, as to reject it, that, which reaſon here puts us upon, is, not ſo much to conſider, whether or no the arguments for the Chriſtian religion be demonſtrations, and will enable a man to anſwer directly all objections and ſcruples; (for there are divers courſes, that prudence may enjoin a man to ſteer, whilſt philoſophy ſuggeſts ſpeculative doubts about the grounds of ſuch reſolutions;) but whether it be more likely to be true, than not to be true, or rather, whether it be not more adviſeable to perform the conditions it requires upon a probable expectation of obtaining the bleſſings it promiſes, than by refuſing it to run a probable hazard of incurring ſuch great and endleſs miſeries, as it peremptorily threatens.

IT will perhaps be ſaid, that this is a hard caſe. But that is an allegation I am not here to conſider; ſince it properly belongs to the doctrine about the providence of God, who being the only Author and abſolute Lord of the creatures, who can receive neither laws nor benefits from them, that can oblige him to them; has a right to preſcribe them what laws he thinks fit, that are not impoſſible for them to obey, and to puniſh their diſobedience to ſuch laws; and much more has a right to annex what conditions he pleaſes, to that ineſtimable felicity he holds forth; the proffer of it upon any terms being a free act of his mere goodneſs, and the value of it incomparably ſurpaſſing whatever we men can do or ſuffer to obtain it; eſpecially conſidering, that, as he might enforce his commands, as ſovereigns commonly do, by threatning penalties to the diſobedient, without propoſing rewards to the performers; ſo he has given men ſuch probable arguments to ground their expectations on, that they will be ſelf-condemned, if they reject the religion he propoſes, and yet maintain it to be decent (if I may ſo ſpeak) for him to crown their faith with unvaluable bleſſings. But, as I was ſaying, the direct and full anſwer to this allegation belongs not to this place, where it may ſuffice to ſay, that, whether the caſe be hard or no, yet this is the caſe. And therefore, though the proofs of the Chriſtian religion did not amount (which yet I do not grant) to moral demonſtrations, a man may act rationally in embracing that religion, if all things conſidered, it appear more likely to be true, than not to be true.

AND I ſhall by and by ſhew you, that this is not the only caſe, where prudence puts us upon making reſolutions, notwithſtanding contrary doubts.

I know the harſhneſs of the caſe is by moſt men made to conſiſt in this, that for a religion, whereof the truth ſuppoſed in its promiſes and threats is not demonſtratively proved, we muſt reſign up our pleaſures, and ſometimes undergo conſiderable hardſhips and loſſes, and conſequently we muſt quit what is certain, for what is uncertain. I have in another paper had occaſion to ſay ſomething elſe to this objection, than what (to avoid repetition) ſhall make up my preſent anſwer, which conſiſts of two parts.

THE firſt whereof is, that what we are to give up to become Chriſtians, is not really ſo valuable in itſelf, as the objecters think, and that it is of ſcarce any value at all, if compared to the goods we may acquire by parting with them. For alas! what is it, that Chriſtianity requires us to forego, but ſmall petty enjoyments? which thoſe, that have had the moſt of, have found them, and pronounced them unſatisfactory, whilſt they poſſeſſed them, and which manifeſt experience ſhews to be no leſs tranſitory, than they have been declared empty, ſince a thouſand accidents may take them from us, and death will infallibly, after a ſhort time (which can be but a moment compared to eternity) take us from them. And if it be ſaid, that theſe enjoyments, ſuch as they are, are at leaſt the only happineſs, that we can make ourſelves ſure of; I muſt freely profeſs, that I think it therefore the more reaſonable to part with them, if it be neceſſary, upon the hopes, that Christian religion gives us. For (especially if a man behold those things, not only with a philosophical eye, that can look through them, but with a Christian eye, that can look beyond them,) if there can be no greater happiness, I do not think so poor a thing, as men call happiness, worth being greedily desired; and if there be such a transcendent happiness as Christianity holds forth, I am sure, that deserves to be the object of my ambition. So that either the meanness of worldly happiness will make me think it no great misery to want it, or the excellency of heavenly felicity will make me think it great wisdom to part with earthly for it.

AND now, in the second part of my answer, I must invite you to consider with me, that Christian religion requires not of us actions more imprudent, than divers others, that are generally looked upon as complying with the dictates of prudence, and some of them practised, by great politicians themselves, in the weighty affairs of state.

YOU know, what a common practice it is, in great storms at sea, for the merchants themselves to throw over-board their goods, and, perhaps too, their victuals, (as in *Paul's* case) though they be sure to lose what they cast away, and are not certain, either that this loss will save the ship, or that the ship may not be saved without it. The wisest, and even the worldliest men, whether princes or private persons, think themselves never more so, than when they toil, and lay out their care and time, and usually deny themselves many things, to provide advantagiously for children, which they have but a woman's word for, and consequently a bare moral probability to assure them to be theirs.

IN the small pox many physicians are for bleeding, and others (as most of our English practitioners) are very much against it. Supposing then (which is no very rare case) that a person invaded by that disease be told by one of his physicians, that, unless nature be eased of part of her burthen by phlebotomy, she will never be able to overcome the disease; and on the contrary, the other assures him, that, if by exhausting the treasure of life, (the blood) he further weakens nature, which is but too weak already, the disease must needs overcome her: what can a prudent man do in this case, where he can take no resolution, against which probable arguments that are not directly and fully to be answered, may not be opposed, and where yet the suspension of his resolution may be as ruinous, as the venturing to take either of those he is invited to?

AND in the formerly mentioned case, a man, that has a spreading gangrene in his arm, if he consents, that it be cut off, which prudence often requires that he should do, he is certain to lose one of his usefullest limbs, and is not certain, but that he may save his life without that loss, nor that he shall save it by that loss.

AND to give you an instance or two of a more publick nature; how many examples does history afford us of famous generals and other great commanders, who have ventured their forces and their lives to seize upon places promised to be betrayed to them by those they had corrupted with money; though the ground, upon which they run this hazard, be the engagement of some, who, if they were not traytors, that could falsify their faith, would never have been bribed to make so criminal and ignominious an engagement? How often have the greatest politicians either resolved to enter into a war, or taken courses, that they foresee will end in a war, upon the informations they receive from those they have corrupted in other prince's councils; though, to believe such intelligencers, those, who venture so much upon their informations, must suppose them faithless and perfidious men?

IT were not difficult, to add other instances to the same purpose, by which, joined with what has been above discoursed, it may appear, that a man need not renounce or lay aside his reason to resolve to fulfil the conditions of the gospel, though the arguments for it were none of them demonstative ones. For, so much as a probability of obtaining by it ſuch ineſtimable bleſſings, as it propoſes, and little more than a bare probability of incurring, by rejecting it, ſuch unſpeakable miſeries, as it threatens, may rationally induce a man to reſolve upon fulfiling its reaſonable conditions, and his prudence may very well be juſtified, if it do but appear, that (1.) It is more probable, that ſome religion ſhould be true, than that ſo many well atteſted miracles alledged by the ancient Chriſtians ſhould be falſe; and that God, who is the author of the world, and of men, (for ſo much I think may be phyſically proved) ſhould leave man, whom he has ſo fitted, and by benefits and internal laws obliged to worſhip him, without any expreſs direction how to do it: and that (2.) If there be any true religion, the Chriſtian is the moſt likely to be that, in regard not only of the excellency of its doctrine and promiſes, but of the prophecies and miracles, that bore witneſs to it, the records of which were made by honeſt plain men, who taught and practiſed the ſtricteſt virtue, and who knew their religion condemned lying, freely joined their doctrines and narratives with their blood: the truth of which was ſo manifeſt in the times, when they were ſaid to be done, that the evidence ſeemed abundantly ſufficient to convert whole nations, and among them many conſiderable and prudent perſons, who had great opportunity, as well as concern, to examine the truth of them, and who were by their intereſt and education ſo indiſpoſed to embrace Chriſtianity, that, to make a ſincere profeſſion of it, they muſt neceſſarily relinquiſh both their former religion, and their former vices, and venturouſly expoſe for it not only their fortunes, but their lives.

If it be here objected, that it is very harſh, if not unreaſonable, to exact, upon ſo great penalty as damnation, ſo firm an aſſent, as is requiſite to faith, to ſuch doctrines, as are either obſcurely delivered, or have not their truth demonſtratively made out: I anſwer, that whatever others may think, I don't believe, that there is any degree of faith abſolutely neceſſary to ſalvation, that is not ſuitable to the evidence, that men may have of it, if they be not wanting to themſelves through lazineſs, prejudices, vice, paſſion, intereſt, or ſome other culpable defect. For conſidering, that God is juſt, and gracious, and has been pleaſed to promulgate the goſpel, that men, whom it ſuppoſes to act as ſuch (that is, as rational creatures) ſhould be brought to ſalvation by it; I ſee no juſt cauſe to think, that he intends to make any thing abſolutely neceſſary to ſalvation, that they may not ſo far clearly underſtand as they are commanded diſtinctly and explicitely to believe it; and what is not ſo delivered, I ſhould, for that very reaſon, unwillingly admit to be neceſſary to ſalvation: and you may here remember, that I formerly told you, I was far from thinking all the tenents either of the ſchools, or of particular churches, to be ſo much as Chriſtian verities, and therefore am very unlike to allow them here to be fundamental and neceſſary ones; and I take it to be almoſt as great as common a miſtake, that all the doctrines, that concern fundamental articles, muſt be fundamental too; as if, becauſe the head is a noble part of the body, and eſſential to life, therefore all the hair, that grows upon it, muſt be thought ſuch too. But then, as to the abſolute firmneſs of aſſent, that is ſuppoſed to be exacted by Chriſtianity to the articles it delivers, I am not ſure, that it is ſo neceſſary in all caſes to true and ſaving faith, as very many take it to be. For firſt, the ſcripture itſelf tells us, that ſome of the truths it reveals, are unfathomable myſteries, and ſome other points are δυσνόητα, hard to be underſtood; and it is unreaſonable to ſuppoſe, that the higheſt firmneſs of aſſent is to be given to ſuch articles, or to thoſe parts of them, as their obſcurity keeps us from having ſo much reaſon to think, that we clearly underſtand them, as we have to ſuppoſe we underſtand thoſe, that are far more plainly revealed. And, ſecondly, to ſpeak more generally, it is harſh to ſay, that the ſame degree of faith is neceſſary to all perſons, ſince men's natural capacities and diſpoſitions, and their education, and the opportunities they have had of being informed, do very much, yet perhaps not culpably, difpofe fome more than others to be diffident, and apt to hefitate, and frame doubts. And the fame arguments may appear evident enough to one man, to make it his duty to believe firmly what they perfuade, which in another, naturally more fceptical, or better acquainted with the difficulties and objections urged by the oppofite party, may leave fome doubts and fcruples excufable enough. And when either the doctrine itfelf is not clearly delivered, or the proofs of it, that a man could yet meet with, are not fully cogent; for that man, not to give fuch truths the fame degree of affent, that demonftration may produce, is not, as many interpret it, an affront to the veracity of God, fince he may be heartily difpofed and ready to believe all, that fhall appear to him to be revealed by God, and only doubts, whether the thing propofed be indeed revealed by him, or whether the diffident party rightly uderftands the fenfe of thefe words, wherein the revelation is contained; which is not to diftruft God, but himfelf: and that in fome cafes, a degree of faith, not exempt from doubts, may, through God's goodnefs, be accepted, we may learn from hence, that the apoftles themfelves, who were fo much in Chrift's favour, made it their prayer to him, that he would increafe their faith: and he, that begged, that if he could do any thing for his fon, and cried out, "Lord, I believe, help thou my unbelief," was fo far accepted by that merciful high prieft, who is apt to be touched with the fenfe of our infirmities, that his requeft was granted, though it could not be fo but by having a miracle done in his favour. The difciples diftreffed by a ftorm, and crying to their mafter, as thinking themfelves upon the very point of perifhing, were faved by him, at the fame time, when he gave them the epithet, "of men of little faith:" and at another time, *Peter* walking upon the fea, though he had loft a degree of that faith, that made him firft engage upon that adventure, and was reproved for it by Chrift, was yet refcued from that finking condition, which both he and his faith were in. And we are all told, in the gofpel, of a faith, which, though no bigger than a grain of muftard-feed, may enable a man to remove mountains: and though this paffage fpeaks not primarily of juftifying faith, yet ftill it may ferve to fhew, that degrees of affent, far fhort of the greateft, may be fo far accepted by God, as to be owned by miraculous exertions of his power. For the faith then, that is made a neceffary condition under the gofpel, as the genuine fruit and fcope of it is obedience; fo it is not indifpenfably fuch a faith, as excludes doubts but refufals. And though the affent be not fo ftrong, as may be produced by a demonftration; yet it may be gracioufly accepted, if it be but ftrong enough to produce obedience. And accordingly, whereas *Paul*, in one place declares, "that in Chrift Jefus "neither circumcifion availeth any thing, nor uncircumcifion, but faith operative "through love;" we may learn his meaning from a parallel place, where varying the words, and not the fenfe, of the latter part of the fentence, he fays, "that in Chrift "Jefus, neither circumcifion availeth any thing, nor uncircumcifion, but the keeping "of the commandments of God." I readily grant, that attainment of a higher degree of faith is always a bleffing, and cannot be fufficiently prized, without being fincerely aimed at; but there are in fome virtues and graces degrees, which though to reach be a great happinefs, yet it is but the endeavouring after them, that is an indifpenfable duty. Likewife it is true, that the firmnefs of affent to divine verities does, in fome regard, bring much honour to God; as it is faid of the father of the faithful, (who in reference to the promife made him of *Ifaac*, did not confider his own age, nor *Sarah*'s long barrennefs, fo as to entertain any diffidence of what God had told him) that being mighty in faith, he gave glory to God: but it is true too, that in another refpect a practical affent built upon a lefs undoubted evidence may have its preheminence; for when Chrift now rifen from the dead, had faid to the diftruftful *Didymus*, "*Thomas*, becaufe thou has feen me, thou haft believed;" he immediately adds, "But

"But blessed (that is, peculiarly and preferably blessed) are those, that have not seen, "and yet have believed." And indeed he does not a little honour God (in that sense, wherein mortals may be said to honour him) who is so willing to obey and serve him, and so ambitious to be in an estate, where he may always do so, that upon what he yet discerns to be but a probability of the Christian religion's being the most acceptable to God, he embraces it with all its difficulties, and dangers, and upon this score venturously resolves to submit, if need be, to a present and actual dereliction of all his sins and lusts, and perhaps his interest and his life too, upon a comparatively uncertain expectation of living with him hereafter.

The Conclusion of the FIRST PART.

AND here I will put a period to my answer to your friend's question in one of the two senses of it, and so to the first part of this discourse. Against all which perhaps your friend will object, that at this rate of arguing for the Christian religion, one may apologize for any opinion, and reconcile the most unreasonable ones to right reason. But it is not difficult for me to reply, that this objection is grounded either upon a mistake of the design of this letter, or upon the overlooking of what is supposed in it. For I do not pretend, that the considerations hitherto alledged should pass for demonstrations of the truth of Christianity, which is to be proved by the excellency of the doctrines it teaches, and that of the rewards it promises, (both which are worthy of God,) and by divers other arguments, especially the divine miracles, that attest it: but that, which I was here to do, was, not to lay down the grounds, why I received the Christian religion, but to return an answer, backed with reasons, to the question, that was proposed: "whether I did not think, that a Christian, to continue "such, must deny or lay aside his reason?" The sum of the answer it this, that the doctrines really proposed by the Christian religion, seeming to me to be by proper arguments sufficiently proved in their kind, so as that the proofs of it, whether they be demonstrative or no, are sufficient, (the nature of the things to be proved, considered) to justify a rational and prudent man's embracing it; this religion, I say, seeming to me to have such positive proofs for it, I do not think, that the objections, that are said to be drawn from reason against it, do really prove the belief of it to be inconsistent with right reason, and do outweigh the argument alledgable in that religion's behalf. To propose some of the general grounds of this answer of mine, was the design of the considerations hitherto discoursed of; which (as I hinted to you at the beginning) could be no other than general, unless you had mentioned to me some of your friend's particular objections, which when he tells you, you will perhaps find, that I have already given you the grounds of answering them. And though to propose arguments to evince positively the truth of our religion, after the example of the excellent *Grotius*, and some other very learned writers, be not, as you see, either my task or my design; yet if you attentively consider, what I write in that short discourse, wherein I manage but that seemingly popular argument for Christianity, that is drawn from the miracles, that are said to attest it, you will perchance be invited to think, that when all the other proofs of it are taken in, a man may, without renouncing or affronting his reason, be a Christian.

BUT to proceed to the more considerable part of what I presumed your friend will object, I answer, that the considerations I have alledged in the behalf of some mysteries of the Christian religion, will not be equally applicable to the most absurd or unreasonable opinions. For these considerations are offered as apologies for Christian doctrines, but upon two or all of these three suppositions. The first, that the truth of the main religion, of which such doctrines make a part, is so far positively proved

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by real and uncontrouled miracles, and other competent arguments, that nothing, but the manifeſt and irreconcileable repugnancy of it's doctrines to right reaſon, ought to hinder us from believing them. The ſecond, that divers of the things, at which reaſonable men are wont to take exception, are ſuch, as reaſon itſelf may diſcern to be very difficult, or perhaps impoſſible for us to underſtand perfectly by our own natural light. And the third, that ſome things in Chriſtianity, which many men think contrary to reaſon, are, at moſt, but contrary to it, as it is incompetently informed and aſſiſted, but not when it is more fully inſtructed, and particularly when it is enlightened and aſſiſted by divine revelation. And as I think theſe three ſuppoſitions are not juſtly applicable, (I ſay not, as the objection does, to the moſt abſurd, or unreaſonable opinions, but) to any other religion than the true, which is the Chriſtian; ſo the laſt of theſe ſuppoſitions prompts me to take notice to you, that, though we ought to be exceeding wary, how we admit what pretends to be ſupernaturally revealed; yet if it be attended with ſufficient evidence of its being ſo, we do very much wrong and prejudice ourſelves, if, out of an unreaſonable jealouſy, or to acquire or maintain the repute of being wiſer than others, we ſhut our eyes againſt the light it offers. For beſides that a man may as well err, by rejecting, or ignoring the truth, as by miſtaking a falſhood for it; I conſider, that thoſe men, that have an inſtrument of knowledge, which other men either have not, or, (which is as bad) refuſe to employ, have a very great advantage above others towards the acquiring of truth, and with far leſs parts than they, may diſcover divers things, which the others, with all their pride and induſtry, ſhall never attain to. As when *Galileo* alone among the modern aſtronomers was maſter of a teleſcope, it was eaſy for him to make noble diſcoveries in heaven of things, to which not only *Ptolomy*, *Alphonſus*, and *Tycho*, but even his maſters, *Ariſtarchus Samius* and *Copernicus* themſelves, never dreamed of, and which other aſtronomers cannot ſee but by making uſe of the ſame kind of inſtrument. And on this occaſion let me carry the compariſon, ſuggeſted by the teleſcope, a little further, and take notice, that if men having heard, that there were four planets moving about *Jupiter*, and that *Venus* is an opacous body, and ſometimes horned like the moon, had reſolved to examine theſe things by their naked eyes, as by the proper organs of ſight, without employing the teleſcope, by which they might ſuſpect, that *Galileo* might put ſome optical deluſion upon them; they would perhaps have aſſembled in great multitudes to gaze at *Venus* and *Jupiter*, that (ſince *plus vident oculi quam oculus)* the number of eyes might make amends for their dimneſs. This attempt not ſucceeding, they would perhaps chooſe out ſome of the youngeſt and ſharpeſt ſighted men, that by their piercing eyes that may be diſcovered, which ordinary ones could not reach. And this expedient not ſucceeding neither, they would perhaps diet their ſtar-gazers, and preſcribe them the inward uſe of fennel, and eye-bright, and externally apply collyriums and eye-waters, and thoſe to as little purpoſe as the reſt. With ſuch a pity, mixed with indignation, as *Galileo* would probably have looked on ſuch vain and fruitleſs attempts with, may a judicious Chriſtian, that upon a due examination admits the truth of the ſcriptures, look upon the preſumptuous and vain endeavours of thoſe men, who, by the goodneſs of their natural parts, or by the improvements of them, or by the number of thoſe, that conſpire in the ſame ſearch, think, with the bare eye of reaſon to make as great diſcoveries of heavenly truths, as a perſon aſſiſted by the revelations, contained in the ſcripture, can with great eaſe and ſatisfactorineſs attain. To which let me add this further improvement of the compariſon, that as a ſkilful aſtronomer will indeed, firſt ſeverely examine, whether the teleſcope be an inſtrument fit to be truſted and not likely to impoſe upon him; but being once reſolved of that, will confidently believe the diſcoveries it makes him, however contrary to the received theories of the celeſtial bodies,

bodies, and to what he himself believed before, and would still, if the telescope did not otherwise inform him, continue to believe; so a well qualified inquirer into religions, though he will be very wary, upon what terms he admits scripture, yet if he once be fully satisfied, that he ought to admit it, he will not scruple to receive upon its authority, whatever supernatural truths it clearly discloses to him; though perhaps, contrary to the opinions he formerly held, and which, if the scripture did not teach him otherwise, he would yet assent to. And as the galaxy, and other whitish parts of the sky, were by *Aristotle* and his followers, and many other philosophers, who looked on them only with their naked eyes, for many ages reputed to be but meteors; but to those, that look on them with an eye assisted by the telescope, they plainly appear true constellations made up of a multitude of bright, though little, stars; so there are theological doctrines, which to philosophers, and others, that look on them with the naked eye of natural reason, seem to be but light and fantastical things; which yet, when reason, assisted and heightened by revelation, comes to contemplate, it manifestly sees them to be true and celestial lights, which only their sublimity keeps concealed from our weak, (naked) eyes.

SOME PHYSICO-THEOLOGICAL CONSIDERATIONS ABOUT THE POSSIBILITY of the RESURRECTION.

The PREFACE.

WHILST the Considerations about Religion and Reason, (to which the following essay is annexed) were not yet come from the press, the learned publisher of them falling one day into discourse with me about the design they aimed at, and some of the points they treated of, and particularly the resurrection; our discourse occasioned my letting him know, that I had long since had thoughts, and perhaps imparted some of them to my friends, about such subjects; and that in particular about the resurrection I had yet by me a manuscript, wherein divers years ago I had endeavoured to shew, that the philosophical difficulties, urged against the possibility of the resurrection, were nothing near so insuperable, as they are by some pretended, and by others granted to be. Upon this notice, the curiosity he expressed to see this essay, engaged me quickly to bring it him; though my being ready to go from *London* made me do it without staying to look it over myself; much less, to add what since occurred to me about the things treated of in it. But notwithstanding its imperfections, and my unwillingness to let it go abroad; especially without some papers, that should have preceded it, the learned peruser would not be denied leave to send it, in my absence, unaltered to the press, and join it to the tract he expected thence; positively affirming, that I ought no longer to stifle a discourse, that he judged very seasonable, and thought likely to do good. In which conjecture, if he do not prove mistaken, I hope some more ingenious than religious men, seeing what

what can be eaſily ſaid by ſo incompetent a pen as mine, for one of the moſt oppoſed doctrines of Chriſtianity, will hereby be made leſs forward to condemn all thoſe for deſerters of reaſon, that ſubmit to revelation. And I ſhall hope too, on the other ſide, that ſome more religious, than, in this matter, well informed men, will be induced to think, that what they call the new philoſophy may furniſh us with ſome new weapons for the defence of our ancienteſt creed; and that corpuſcularian principles may not only be admitted without Epicurean errors, but be employed againſt them.

Some Phyſico-Theological CONSIDERATIONS, &c.

THE queſtion about which my thoughts are deſired being this; "whether to "believe the reſurrection of the dead, which the Chriſtian religion teaches, be "not to believe an impoſſibility?" I ſhall, before I proceed any farther, crave leave to ſtate the queſtion ſomewhat more clearly and diſtinctly; that, being freed from ambiguities, you may the better know in what ſenſe I underſtand it in my anſwer; in the returning whereof, your friend need not deſire me to inſiſt but upon my own thoughts, unleſs he could do me the favour to direct me to ſome author, which I have not yet ſeen, that has expreſly treated, upon philoſophical grounds, of the queſtion he propoſes.

First then I take it for granted, that he does not mean, whether the reſurrection is a thing knowable, or directly provable by the mere light of nature. For, if God had not, in the ſcripture, poſitively revealed his purpoſe of raiſing the dead, I confeſs, I ſhould not have thought of any ſuch thing; neither do I know, how to prove that it will be, but by flying, not only to the veracity, but the power of God; who having declared, that he will raiſe the dead, and being an almighty agent, I have reaſon to believe, that he will not fail to perform what he has foretold.

Nor do I (ſecondly) underſtand the queſtion to be, whether the reſurrection be poſſible to be effected by merely phyſical agents and means. For that it is not to be brought to paſs according to the common courſe of nature, I preſume; after the univerſal experience of many ages, which have afforded us no inſtances of it. And though perhaps in ſpeculation it ſeems not abſolutely repugnant to reaſon, that the ſcattered parts of a dead body might be reconjoined, ſoon after the death of the man; yet I think you will eaſily grant it to be morally impoſſible, that this ſhould happen to any one perſon, and much more, that it may, nay, that it will happen to all the perſons of mankind at the world's end: ſo that when I treat of the poſſibility of the general reſurrection, I take it for granted, that God has been pleaſed to promiſe and declare, that there ſhall be one, and that it ſhall be effected, not by, or according to the ordinary courſe of nature, but by his own power. On which occaſion, I remember, that when our Saviour, treating of the reſurrection, ſilenced the Sadducees, that denied it, he conjoins, as the cauſes of their error, the two things I have pointed at in this obſervation, and in the firſt, that preceded it: "You err, ſays he, not knowing the "ſcriptures, nor the power of God." And when an angel would aſſure the bleſſed virgin, that ſhe ſhould bear a child without the intervention of a man, (which was a caſe ſomewhat akin to ours, ſince it was a production of a human body out of a ſmall portion of human ſubſtance in a ſupernatural way,) he concludes his ſpeech by telling her, "That nothing ſhall prove impoſſible to God."

In the third place, I ſuppoſe, that the article of the reſurrection, taught by the Chriſtian religion, is not here meant by the propoſer in ſuch a latitude, as to comprize all,

all, that any particular church or ſect of Chriſtians, much leſs any private doctor or other writer, hath taught about the reſurrection; but only what is plainly taught about it in the holy ſcriptures themſelves. And therefore if beſides what is there ſo delivered, the propoſer hath met with any thing, that he judges to be impoſſible in it's own nature, he hath my free conſent to deal with the authors and abettors of ſuch unreaſonable opinions, (which I declare myſelf to be not only unconcerned to defend, but ſufficiently diſpoſed to reject,) as raſhneſſes unfriendly to the growth of Chriſtianity.

4. AND now, that I may yet further clear the way for the diſcourſe, that is to follow, and obviate ſome objections and ſcruples, which I think it is better ſeaſonably to prevent, than ſolemnly to anſwer; I ſhall deſire your leave to lay down in this place a couple of conſiderations; of which I ſhall begin with this, that it is no ſuch eaſy way, as at firſt it ſeems, to determine, what is abſolutely neceſſary and but ſufficient to make a portion of matter, conſidered at differing times or places, to be fit to be reputed the ſame body.

THAT the generality of men do in vulgar ſpeech allow themſelves a great latitude about this affair, will be eaſily granted by him, that obſerves the received forms of ſpeaking. Thus *Rome* is ſaid to be the ſame city, though it hath been ſo often taken and ruined by the barbarians and others, that perhaps ſcarce any of the firſt houſes have been left ſtanding, and at leaſt very few remain in compariſon of thoſe, that have been demoliſhed, and have had others, built in their ſtead. Thus an univerſity is ſaid to be the ſame, though ſome colleges fall to ruin, and new ones are built; and though once in an age all the perſons, that compoſed it, deceaſe, and are ſucceeded by others. Thus the *Thames* is ſaid to be the ſame river, that it was in the time of our forefathers, though indeed the water, that now runs under *London* Bridge, is not the ſame, that ran there an hour ago, and is quite other than that, which will run there an hour hence. And ſo the flame of a candle is ſaid to be the ſame for many hours together, though it indeed be every minute a new body, and the kindled particles, that compoſe it at any time aſſigned, are continually putting off the form of flame, and are repaired by a ſucceſſion of like ones.

NOR is it by the vulgar only, that the notion of identity has been uneaſy to be penetrated. For it ſeems, that even the ancient philoſophers have been puzzled about it; witneſs their diſputes, whether the ſhip of *Theſeus* were the ſame after it had (like that of Sir *Francis Drake*) been ſo patched up from time to time to preſerve it as a monument, that ſcarce any plank remained of the former ſhip, new timber having been ſubſtituted in the place of any part, that in length of time rotted. And even in metaphyſicks themſelves, I think it no eaſy taſk to eſtabliſh a true and adequate notion of identity, and clearly determine, what is the true principle of individuation. And at all this I do not much wonder; for almoſt every man, that thinks, conceives in his mind this or that quality or relation, or aggregate of qualities, to be that, which is eſſential to ſuch a body, and proper to give it ſuch a denomination; whereby it comes to paſs, that, as one man chiefly reſpects this thing, and another that in a body, that bears ſuch a name; ſo one man may eaſily look upon a body, as the ſame, becauſe it retains what he chiefly conſidered in it, whilſt another thinks it to be changed into a new body, becauſe it has loſt that, which he thought was the denominating quality or attribute. Thus philoſophers and phyſicians diſagree about water and ice, ſome taking the latter to be but the former diſguiſed, becauſe they are both of them cold and ſimple bodies, and the latter eaſily reducible to the former, by being freed from the exceſſive and adventitious degree of coldneſs; whilſt others, looking upon fluidity as eſſential to water, think ice upon the ſcore of its ſolidity to be a diſtinct ſpecies of bodies. And ſo Peripateticks and chemiſts often diſagree about the aſhes and calces of burnt bodies; the firſt referring them

them to earth, becauſe of their permanency and fixedneſs, and divers of the Spagyriſts, taking them to be bodies *ſui generis*, becauſe common aſhes uſually contain a cauſtick ſalt, whereas earth ought to be inſipid: and the like may be ſaid of ſome wood-aſhes and lime-ſtone, and even coral, which, when well calcined and recent, have a biting taſte, beſides that ſome of them, that are inſipid, may be reduced into metals, as may be eaſily enough tried in the calces of lead and copper.

These difficulties about the notion of identity I have therefore taken notice of, that we may not think it ſtrange, that among the ancient Hebrews and Greeks, whoſe languages were ſo remote in ſeveral regards from ours, the familiar expreſſions employed about the ſameneſs of a body ſhould not be ſo preciſe as were requiſite for their turn, who maintain the reſurrection in the moſt rigid ſenſe. And this leads me from the firſt of my two conſiderations to the ſecond.

That, then, it is not repugnant or unconſonant to the holy ſcripture, to ſuppoſe, that a comparatively ſmall quantity of the matter of a body, being encreaſed either by aſſimilation or other convenient appoſition of aptly diſpoſed matter, may bear the name of the former body, I think I may reaſonably gather from the three following expreſſions, I meet with in the Old and New Teſtament.

For firſt, St. *Paul* in the 15th chapter of his firſt epiſtle to the *Corinthians*, where he profeſſedly treats of the reſurrection, and anſwers this queſtion; "But ſome men "will ſay, how are the dead raiſed up? and with what body do they come?" ver 35: he more than once explains the matter by the ſimilitude of ſowing, and tells them, ver 37. "That which thou ſoweſt, thou ſoweſt not that body, that ſhall be: but bare "grain, it may chance of wheat, or of ſome other grain. Adding, that God gives "this ſeed a body as he thought fit, to each ſeed its own body, ver. 38." Now. if we conſider the multitude of grains of corn, that may in a good ſoil grow out of one; inſomuch, that our Saviour ſpeaking, in the parable *de Agro Dominico*, of a whole field, tells us, that the grain may well bear a hundred for one; we cannot but think, that the portion of the matter of the ſeed, that is in each of the grains (not to reckon what may be contained in the roots, ſtalk, and chaff,) muſt be very ſmall.

I will not now conſider, whether this text juſtifies the ſuppoſition of a plaſtick power in ſome part of the matter of a deceaſed body; whereby, being divinely excited, it may be enabled to take to its ſelf freſh matter, and ſo ſubdue and faſhion it, as thence ſufficiently to repair or augment itſelf; though the compariſon ſeveral times employed by St. *Paul* ſeems to favour ſuch an hypotheſis. Nor will I examine, what may be argued from conſidering that leaven, though at firſt not differing from other dough, is by a light change of qualities, that it acquires by time, enable to work upon and ferment a great proportion of other dough. Nor yet will I here debate, what may be ſaid in favour of this conjecture from thoſe chemical experiments, by which *Kircherus*, a *Polonian* phyſician in *Quercetanus* and others, are affirmed to have, by a gentle heat, been able to re-produce, in well-cloſed vials, the perfect ideas of plants deſtroyed by the fire: I will not, I ſay, in this place enter upon a diſquiſition of any of theſe things, both becauſe I want time to go thorough with it, and becauſe, the reſuſcitation, ſuppoſing the matter of fact, may give no ſmall countenance to our cauſe; yet I do not either abſolutely need it, or perhaps fully acquieſce in all the circumſtances and inferences, that ſeem to belong to it. But one thing there is, that I muſt not leave unmentioned in this place; becauſe I received it, ſoon after the trial was made, from two eminent perſons of my acquaintance, men of great veracity, as well as judgment; whereof one made the experiment, and the other ſaw it made in his own garden, where the trier of the experiment (for he was ſo modeſt, that he would not confeſs himſelf to be the author of it,) took ſome aſhes of a plant, juſt like our Engliſh red poppy; and

and having ſowed theſe alkaliſate aſhes in my friend's garden, they did, ſooner than was expected, produce certain plants larger and fairer than any of that kind, that had been ſeen in thoſe parts. Which ſeems to argue, that, in the ſaline and earthy, *i. e.* the fixed particles of a vegetable, that has been diſſipated and deſtroyed by the violence of the fire, there may remain a plaſtick power, inabling them to contrive diſpoſed matter, ſo as to re-produce ſuch a body, as was formerly deſtroyed. But to this plaſtick power, reſiding in any portion of the deſtroyed body itſelf, it will not perhaps be neceſſary to have recourſe; ſince an external and omnipotent agent can, without it, perform all that I need contend for: as I think I might gather from that other expreſſion of holy ſcripture, that I meet with in the ſecond chapter of *Geneſis*, where it is ſaid, "That the Lord God cauſed a deep ſleep to fall upon *Adam*, and he ſlept; and "he took one of his ribs, and cloſed up the fleſh inſtead thereof. And the rib, which "the Lord God had taken from man, made he a woman, and brought her unto the "man, ver. 21, 22." For, ſince it cannot be pretended, that either the whole, or any conſiderable portion of *Eve*'s body was taken out of *Adam*'s, which was deprived but of a rib; ſince it cannot be probably affirmed, that this rib had any ſpermatick faculty, both becauſe the text aſſigns the formation of the woman to God, and becauſe the ſeminal principles in animals require the commixture of male and female, the latter of which the text ſuppoſes not to have been then made; why may I not conclude, that, if it pleaſe God, by his immediate operation, to take a portion of the matter of a human body, and add to it a far greater quantity, either of newly created, or of pre-exiſtent matter; the new body ſo framed may, congruouſly enough to ſcripture-expreſſions, be reputed to be made of the former body. And accordingly *Adam* (ver. 23.) gives the reaſon why, he called his wife *Iſha*, which our tranſlation renders woman; becauſe ſhe was taken out of *Iſh*, which in our verſion is rendered man.

The other text, that I conſider, to my preſent purpoſe, is the myſtical reſurrection deſcribed in *Ezekiel*'s viſion, where all, that remain of the dead men, that were to riſe up an army of living men, was a valley full of dry bones, which being by the divine Power approached to one another, and made to join together in a convenient manner, were afterwards by the ſupernatural appoſition of either newly created, or extrinſecally ſupplied matter, furniſhed with ſinews, (by which I ſuppoſe is meant not only nerves, Ezekiel xxxvii.
but veſſels, tendons, ligaments, &c.) and fleſh covered with ſkins; and laſt of all, a Ver. 7, 8.
vivifying ſpirit was conveyed into them, that made them ſtand upon their feet alive, an Ver. 9, 10.
exceeding great army. Whence I gather, that it is not unconſonant to the expreſſions of ſcripture, to ſay, that a portion of the matter of a dead body, being united with a far greater portion of matter furniſhed from without by God himſelf, and completed into a human body, may be reputed the ſame man, that was dead before. Which may appear, both by the tenor of the viſion, and particularly from the expreſſion ſet down in the tenth verſe, where God, calling for the enlivening ſpirit, names the compleated, but not yet revived bodies, *theſe ſlain*, as if he now counted them the ſame, that had formerly been killed.

These preliminary conſiderations being thus laid down, we may now proceed to examine more cloſely thoſe difficulties, which are ſaid to demonſtrate the impoſſibility of the reſurrection; the ſubſtance of which difficulties may be compriſed in this objection.

When a man is once really dead, divers of the parts of the body will, according to the courſe of nature, reſolve themſelves into multitudes of ſteams, that wander to and fro in the air; and the remaining parts, that are either liquid or ſoft, undergo ſo great a corruption and change, that it is not poſſible ſo many ſcattered parts ſhould be again brought together, and re-united after the ſame manner wherein they exiſted in a human body, whilſt it was yet alive. And much more impoſſible it is to effect this re-union, if if the body have been, as it often happens, devoured by wild beasts or fishes; since in this case, though the scattered particles of the cadaver might be recovered as particles of matter, yet having already passed into the substance of other animals, they are quite transmuted, as being informed by the new form of the beast or fish that devoured them, and of which they now make a substantial part.

And yet far more impossible will this redintegration be, if we put the case, that the dead body be devoured by Canibals; for then the same flesh belonging successively to two differing persons, it is impossible that both should have it restored to them at once, or that any footsteps should remain of the relation it had to the first possessor.

In answer to this (indeed weighty) objection, I have several things to offer.

And first, I consider, that a human body is not as a statue of brass or marble, that may continue, as to sense, whole ages in a permanent state; but is in a perpetual flux, or changing condition, since it grows in all its parts, and all its dimensions, from a corpusculum, no bigger than an insect, to the full stature of man; which in many persons, that are tall and fat, may amount to a vast bulk, which could not happen but by a constant apposition and assimilation of new parts to the primitive ones of the little embryo; and since men, as other animals, grow but to a certain pitch, and till a certain age, (unless perhaps it be the crocodile, which some affirm to grow always till death,) and therefore must discharge a great part of what they eat and drink by insensible transpiration, which *Sanctorius*'s Statical Experiments, as well as mine, assure me to be scarce credibly great, as to men and some other animals, both hot and cold; it will follow, that, in no very great compass of time, a great part of the substance of a human body must be changed: and yet it is considerable, that the bones are of a stable and lasting texture, as I found not only by some chemical trials, but by the sculls and bones of men, whom history records to have been killed an exceeding long time ago, of which note we may hereafter make use.

Secondly, I consider, that there is no determinate bulk or size, that is necessary to make a human body pass for the same, and that a very small portion of matter will sometimes serve the turn; as an embryo, for instance, in the womb; a new-born babe, a man at his full stature, and a decrepit man of perhaps an hundred years old, notwithstanding the vast difference of their sizes, are still reputed to be the same person; as is evident, by the custom of crowning kings and emperors in the mother's belly, and by putting murderers, &c. to death in their old age, for crimes committed in their youth; and if a very tall and unweildy fat man should, as it sometimes happens, be reduced by a consumption to a skeleton, as they speak, yet none would deny, that this wasted man were the same with him, that had once so enormously big a body.

I consider also, that a body may either consist of, or abound with such corpuscles, as may be variously associated with those of other bodies, and exceedingly disguised with those mixtures, and yet retain their own nature; of this we have divers instances in metalline bodies: thus gold, for example, when dissolved in aqua regis, passes for a liquor, and when dexterously coagulated, it appears a salt or vitriol: by another operation, I have taken pleasure to make it part of the fuel of a flame: being dexterously conjoined to another mineral, it may be reduced to glass: being well precipitated with mercury, it makes a glorious transparent powder: being precipitated with spirit of urine, or oil of tartar *per deliquium*, it makes a fulminating calx, that goes off very easily, yet is far stronger than gun-powder: being precipitated with a certain other alkali, the fire turns it to a fixed and purple calx. And yet in spite of all these, and divers other disguises, the gold retains its nature; as may be evinced by chemical operations, especially by reductions. Mercury also is a greater Proteus than gold, sometimes putting on the form of a vapour; sometimes appearing in that of an almost insipid

ſipid water; ſometimes aſſuming, in that condition, the form of a red powder; ſometimes that of a white one, and of a yellow one, or of a chryſtalline ſalt, of a malleable metal; of what not? And yet all theſe are various dreſſes of the ſame quickſilver, which a ſkilful artiſt may eaſily make it put off, and re-appear in its native ſhape.

And though it be true, that inſtances of the permanence of corpuſcles, that paſs under ſucceſſive diſguiſes, may be much eaſier found among metals and minerals, than vegetables and animals; yet there are ſome to be met with among theſe: for, not to mention *Hippocrates* his affirmation about purging a child with the milk of an animal, that had taken *Elaterium*, (if I miſ remember not the drug,) not to mention this, I ſay; I remember, that when I once paſſed a ſpring in *Savoy*, I obſerved, that all the butter, that was made in ſome places, taſted ſo rank of a certain weed, that at that time of the year abounds there in the fields, that it made ſtrangers much nauſeate the butter, which otherwiſe was very good. If it be conſidered, how many, if I may ſo call them, elaborate alterations the rank corpuſcles of this weed muſt have undergone in the various digeſtions of the cow's ſtomach, heart, breaſts, &c. and that afterwards, two ſeparations, at leaſt, were ſuperadded, the one of the cream from the reſt of the milk, and the others of the unctuous parts of the cream from the ſerum or butter-milk; it will ſcarce be denied, but that vegetable corpuſcles may, by aſſociation, paſs through divers diſguiſes, without loſing their nature; eſpecially conſidering, that the eſſential attributes of ſuch corpuſcles may remain undeſtroyed, though no ſenſible quality ſurvive to make proof of it; as in our newly mentioned example the offenſive taſte did. And beſides what we commonly obſerve on the ſea-coaſt, of the fiſhy taſte of thoſe ſea-birds, that feed only upon ſea-fiſh, I have purpoſely enquired of an obſerving man, that lived upon a part of the Iriſh coaſt, where the cuſtom is to fatten their hogs with a ſhell-fiſh, which that place very much abounds with, about the taſte of their pork: to which he anſwered me, that the fleſh had ſo ſtrong and rank a taſte of the fiſh, that ſtrangers could not endure to eat it. There is a certain fruit in *America*, very well known to our Engliſh planters, which many of them call the prickle-pear, whoſe very red juice being eaten with the pulp of the fruit, whereof it is a part, doth ſo well make its way through the divers ſtrainers and digeſtions of the body, that it makes the urine red enough to perſuade thoſe, that are unacquainted with this property, that they piſs blood; as I have been ſeveral times aſſured by unſuſpected eye-witneſſes. But more odd is that, which is related by a learned man, that ſpent ſeveral years upon the Dutch and Engliſh plantations in the *Charibbe-Iſlands*, who ſpeaking of a fruit, (which I remember I have ſeen, but had not the liberty to make trial of it,) called janipa, or junipa, growing in ſeveral of thoſe iſlands, he tells us, among other things, that *au temps*, &c. which is, at the ſeaſon, when this fruit falls from the tree, the hogs, that feed on it, have both their fleſh and fat of a violent colour, as experience witneſſeth, (which colour is the ſame, that the juice dies;) and the like happens to the fleſh of parrots and other birds, that feed upon it. I ſhall by and by give you an inſtance of a vegetable ſubſtance, which, though torn in pieces by very corroſive liquors, and ſo diſguiſed as to leave no ſuſpicion of what it was, does thereby not only loſe its nature, but is in an immediate capacity of re-appearing cloathed even with the ſenſible qualities of it, as colour, taſte, and ſmell.

Having thus ſhewn, that the particles of a body may retain their nature under various diſguiſes, I now proceed to add, that they may be ſtripped of thoſe diſguiſes, or, to ſpeak without a metaphor, be extricated from thoſe compoſitions, wherein they are diſguiſed, and that ſometimes by ſuch ways, as thoſe, that are ſtrangers to the nicer operations of nature, would never have thought upon, nor will not perhaps judge probable, when propoſed. It is not unknown to expert chemiſts, that in deſpite of all the

the various shapes, which that Proteus, mercury, may be made to appear in, as of a cristalline sublimate, a red precipitate, a yellow turbith, a vapor, a clear water, a cinaber, &c. a skilful method of reduction will quickly free it from all, that made it impose upon our senses, and re-appear in the form of plain running mercury. And though vitrification be looked upon by chemists, as the ultimate action of the fire, and powerfulest way of making inseparable conjunctions of bodies; yet even out of glass of lead, for instance, (made of sand, and the ashes of a metal,) though the transmutation seems so great, that the dark and flexible metal is turned into a very transparent and brittle mass; yet even from this have we recovered opacous and malleable lead. And though there be several ways, besides precipitations, of divorcing substances, that seem very strictly, if not unseparably united; (which though I may, perhaps, have practised, it is not now convenient I should discourse of;) yet, by precipitation alone, if a man have the skill to choose proper precipitants, several separations may not only be made, but be easily and thoroughly made, that every one would not think of: for, it is not necessary, that in all precipitations, as is observed in most of the vulgar ones, the precipitant body should indeed make a separation of the dissolved body from the mass, or bulk of that liquor, or other adjunct, whereto it was before united, but should not be able to perform this without associating its own corpuscles with those of the body it should rescue, and so make in some sense a new and further composition. For, that some bodies may precipitate others, without uniting themselves with them, is easily proved by the experiment of refiners, separating silver from copper; for, the mixture being dissolved in aqua fortis, if the solution be afterwards diluted, by adding fifteen or twenty times as much common water, and you put into this liquor a copper-plate, you shall quickly see the silver begin to adhere to the plate, not in the form of a calx, as when gold is precipitated to make *aurum fulminaus*, or tin-glass to make a fine white powder for a *Fucus*; but in the form of a shining metalline substance, that needs no farther reduction to be employed as good silver. And by a proper precipitant, I remember, I have also in a trice (perhaps in a minute of an hour) reduced a pretty quantity of well disguised mercury into running quicksilver. And if one can well appropriate the precipitants to the bodies they are to recover, very slight and unpromising agents may perform great matters in a short time; as you may guess by the experiment I lately promised you: which is this, that, if you take a piece of camphire, and let it lie a-while upon oil of vitriol, shaking them now and then, it will be so corroded by the oil, as totally to disappear therein, without retaining so much as its smell, or any manifest quality, whereby one may suspect there is camphire in that mixture; and yet, that a vegetable substance, thus swallowed up, and changed by one of the most fretting and destroying substances, that is yet known in the world, should not only retain the essential qualities of its nature, but be restorable to its obvious and sensible ones, in a minute, and that by so unpromising a medium as common water, you will readily grant, if you pour the dissolved camphire into a large proportion of that liquor, to whose upper parts it will immediately emerge white, brittle, strong-scented, and inflammable camphire, as before.

One main consideration I must add to the foregoing ones, namely, that body and body being but a parcel, and a parcel of universal matter mechanically different; either parcel may successively put on forms in a way of circulation, if I may so speark, till it return to the form, whence the reckoning was begun, having only its mechanical affections altered.

That all bodies agree in one common matter, the schools themselves teach, making what they call the *materia prima* to be the common basis of them all, and their specifick differences to spring from their particular forms: and since the true notion of body consists

conſiſts either alone in its extenſion, or in that, and impenetrability together, it will follow, that the differences, which make the varieties of bodies we ſee, muſt not proceed from the nature of matter, of which, as ſuch, we have but one uniform conception; but from certain attributes; ſuch as motion, ſize, poſition, &c. that we are wont to call mechanical affections. To this it will be congruous, that a determinate portion of matter being given, if we ſuppoſe, that an intelligent and otherwiſe duly qualified agent do watch this portion of matter in its whole progreſs, through the various forms it is made to put on, till it come to the end of its courſe, or ſeries of changes; if I ſay, we ſuppoſe this, and withal, that this intelligent agent lay hold of this portion of matter cloathed in its ultimate form, and extricating it from any other parcels of matter, wherewith it may be mingled, make it exchange its laſt mechanical affections for thoſe, which it had, when the agent firſt began to watch it; in ſuch caſe, I ſay, this portion of matter, how many changes and diſguiſes ſoever it may have undergone in the mean time, will return to be what it was; and if it were before part of another body to be re-produced, it will become capable of having the ſame relation to it, that formerly it had.

To explain my meaning by a groſs example; ſuppoſe a man cut a large globe, or ſphere, of ſoft wax, in two equal parts or hemiſpheres, and of the one make cones, cylinders, rings, ſcrews, &c. and kneading the other with dough, make an appearance of pie-cruſt, cakes, vermicelli, (as the Italians call paſte, ſqueezed through a perforated plate into the form of little worms,) wafers, biſkets, &c. it is plain, that a man may by diſſolution, and other ways, ſeparate the wax from the dough or paſte, and reduce it in a mould to the ſelf-ſame hemiſphere of wax it was before, and ſo he may deſtory all, that made the other part of the wax paſs for ſeveral bodies, as cones, or cylinders, or rings, &c. and may reduce it in a mould to one diſtinct ſemi-globe, fit to be re-conjoined to the other, and ſo to re-compoſe ſuch a ſphere of wax, as they conſtituted, before the biſection was made. And to give you an example to the ſame purpoſe, in a caſe, that ſeems much more difficult; if you look upon precipitate, carefully made *per ſe*, you would think, that art has made a body extremely different from the common mercury; this being conſiſtent like a powder, very red in colour, and purgative, and for the moſt part vomitive in operation, though you give but four or five grains of it; and yet, if you but preſs this powder with a due heat, by putting the component particles into a new and fit motion, you may re-unite them together, ſo as to re-obtain, or re-produce the ſame running mercury you had, before the precipitate *per ſe* was made of it.

HERE I muſt beg your leave to recommend more fully to your thoughts that, which, ſoon after the beginning of this diſcourſe, I did (purpoſely) but touch upon, and invite you to conſider with me, that the Chriſtian doctrine doth not aſcribe the reſurrection to nature, or any created agent, but to the peculiar and immediate operation of God, who has declared, that, before the very laſt judgment, he will raiſe the dead. Wherefore, when I lately mentioned ſome chemical ways of recovering bodies from their various diſguiſes, I was far from any deſire it ſhould be imagined, that ſuch ways were the only, or the beſt, that can poſſibly be employed to ſuch an end. For, as the generality of men, without excepting philoſophers themſelves, would not have believed or thought, that, by eaſy chemical ways, bodies, that are reputed to have paſſed into a quite other nature, ſhould be reduced or reſtored to their former condition; ſo, till chemiſtry, and other parts of true natural philoſophy, be more throughly underſtood, and farther promoted, it is probable, that we can ſcarce now imagine, what expedients to re-produce bodies a further diſcovery of the myſteries of art and nature may lead us mortals to. And much leſs can our dim and narrow knowledge determine, what means,

even

even phyſical ones, the moſt wiſe author of nature, and abſolute governor of the world, is able to employ to bring the reſurrection to paſs, ſince it is a part of the imperfection of inferior natures to have but an imperfect apprehenſion of the powers of one, that is incomparably ſuperior to them. And even among us, a child, though endowed with a reaſonable ſoul, cannot conceive, how a geometrician can meaſure inacceſſible heights and diſtances, and much leſs how a coſmographer can determine the whole compaſs of the earth and ſea, or an aſtronomer inveſtigate, how far it is from hence to the moon, and tell many years before, what day and hour, and to what degree, ſhe will be eclipſed. And indeed in the *Indies*, not only children, but rational illiterate men, could not perceive, how it was poſſible for the Europeans to converſe with one another by the help of a piece of paper, at an hundred miles diſtance, and in a moment produce thunder and lightening, and kill men a great way off, as they ſaw gunners and muſketeers do, and much leſs foretel an eclipſe of the moon, as *Columbus* did to his great advantage; which things made the Indians, even the chiefeſt of them, look upon the Spaniards, as perſons of a more than human nature. Now among thoſe, that have a true notion of a Deity, which is a Being both omnipotent and omniſcient; that he can do all, and more than all, that is poſſible to be performed by any way of diſpoſing of matter and motion, is a truth, that will be readily acknowledged, ſince he was able at firſt to produce the world, and contrive ſome part of the univerſal matter of it into the bodies of the firſt man and woman. And that his power extends to the re-union of a ſoul and body, that have been ſeparated by death, we may learn from the experiments God has been pleaſed to give of it both in the Old Teſtament and the New, eſpecially in the raiſing again to life *Lazarus* and Chriſt; of the latter of which particularly, we have proofs cogent enough to ſatisfy any unprejudiced perſon, that deſires but competent arguments to convince him. And that the miraculous power of God will be, as well as his veracity is, engaged in raiſing up the dead, and may ſuffice, if it be ſo, we may not difficultly gather from that excellent admonition of our Saviour to the Sadducees, where he tells them, (as I elſewhere noted) that the two cauſes of their errors are, their not knowing the ſcriptures, wherein God hath declared, he will raiſe the dead; nor the power of God, by which he is able to effect it. But the engagement of God's omnipotence is alſo in that place clearly intimated by St. *Paul*, *Acts* xxvi. 8. where he aſks king *Agrippa*, and his other auditors, why they ſhould think it a thing not to be believed, (ἄπιστον,) that God ſhould raiſe the dead. And the ſame truth is yet more fully expreſſed by the ſame apoſtle, where ſpeaking of Chriſt returning in the glory and power of his father, to judge all mankind, after he has ſaid, that this divine judge ſhall transform, or transfigure (μετασχηματίζειν) our vile bodies (ſpeaking of his own, and thoſe of other ſaints,) to ſubjoin the account on which this ſhall be done, he adds, "That it will be according to the powerful working, (ἐνέργειαν) whereby he is able even to ſubdue all things to himſelf, *Phil.* iii 21."

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A Conjecture concerning the Bladders of Air, that are found in Fishes, communicated by *A. J.* and illustrated by an Experiment.

First published in the Philosophical Transactions, No. CXIV. p. 310, for *May* 24, 1675.

REFLECTING on that question, whether liquids gravitate upon bodies immersed or not? I came to a resolution in my own thoughts, that they do gravitate; and one of the greatest instances, that did occur to me was, that a bubble of air, rising from the bottom, does dilate itself all the way to the top; which is caused by the lessening of the weight or pressure of the incumbent water, the nearer it is to the top. Upon consideration of that instance, the following conjecture presented itself to my thoughts; that fishes, by reason of the bladder of air, that is within them, can sustain or keep themselves in any depth of water. For the air in that bladder is like the bubble, more or less compressed, according to the depth the fish swims at, and takes up more or less space; and consequently the body of the fish, part of whose bulk this bladder is, is greater or less according to the several depths, and yet retains the same weight.

weight. The rule *de insidentibus humido* is, that a body, that is heavier than so much water, as is equal in quantity to the bulk of it, will sink; a body, that is lighter, will swim; a body of equal weight will rest in any part of the water.

Now by this rule, if the fish in the middle region of the water be of equal weight to the water, that is, commensurate to the bulk of it, the fish will rest there without any tendency upwards or downwards: and if the fish be deeper in the water, the bulk of the fish becoming less by the compression of the bladder, and yet retaining the same weight, it will sink and rest at the bottom: and on the other side, if the fish be higher than that middle region, the air dilating itself, and the bulk of the fish consequently encreasing, but not the weight, the fish will rise upwards, and rest at the top of the water.

PERHAPS the fish by some action can emit air out of this bladder, and afterwards out of its body, and also, when there is not enough, take in air and convey it to this bladder; and then it will not be wondered, that there should be always a fit proportion of air in the bodies of all fishes, to serve their use, according to the depth of water they are bred and live in: perhaps by some muscle the fish can contract this bladder beyond the pressure of the weight of water: perhaps the fish can by its sides, or some other defence, keep off the pressure of the water, and give the air leave to dilate itself. In these cases the fish will be helped in all intermediate distances, and may rise or sink from any region of the water without moving one finn.

IT were worth observing, what fishes want bladders, and if the bladders of several fishes are not of different shapes or bigness, and how they are in sea-fishes, that live in great depths, and whether any amphibious creatures have them, or any thing analogous; as the lungs may be, or other cavities. By an inquiry into these, and other particulars, this conjecture may be either fortified or refuted.

[So far this conjecture: in reference to which, when it was propounded to the honourable *Robert Boyle*, he, reflecting upon the manner how a fish comes to rise or sink in water, soon bethought himself of an experiment probably to determine, whether a fish makes those motions by constructing or expanding himself? the experiment by him suggested was; to take a bolthead with a wide neck, and having filled it almost full with water, to put into it some live fish of a convenient size, that is, the biggest, that can be got in, as a roch, perch, or the like; and then to draw out the neck of the bolthead as slender as you can; and to fill that also with water: whereupon the fish lying at a certain depth in the water of the glass, if upon his sinking you perceive the water at the slender top does subside, you may infer, he contracts himself, and if, upon his rising, the water be also raised, you may conclude, he dilates himself.]

A New ESSAY INSTRUMENT, together with the USES thereof.

First publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. CXV. p. 329, for *June* 21, 1675.

SECTION I.

Shewing the occaſion of making this new eſſay inſtrument, together with the hydroſtatical principles it is founded on.

TO give you now a more explicite and particular account, than I had then time to do, of the inſtrument, which you ſaw tried at the Royal Society, I ſhall inform you, on what grounds I deviſed it, and then annex ſome obſervations about the fabrick and the uſes of it.

You may remember, that many years ago I ſhewed you a little glaſs inſtrument, conſiſting of a bubble, furniſhed with a long and ſlender ſtem, which was to be put into ſeveral liquors, to compare and eſtimate their ſpecifick gravities, and which I made uſe of to ſome purpoſes, for which it is not, that I know, as yet employed. But afterwards conſidering this little inſtrument ſomewhat more attentively, I thought the application of it might eaſily be, as it were, inverted, and that, whereas it was employed but to diſcover the differing gravities of ſeveral liquors, by its various degrees of immerſion in them, it might be employed to diſcover the ſpecifick gravities of ſeveral appended ſolids, by its being more or leſs depreſſed by them in the ſame liquor. For it is clearly deducible from the grounds of the hydroſtaticks, that any ſolid body, heavier than water, looſes in the water as much of the weight it had in the air, as water of equal bulk to the immerſed ſolid would weigh in the air; and conſequently, ſince gold is by far the moſt ponderous of metals, a piece of gold, and one of equal weight of copper, braſs, or any other metal, being propoſed, the gold muſt be leſs in bulk, than the copper or braſs. And by this means, if both of them be weighed in the water, the gold muſt looſe in that liquor leſs of its former weight than the braſs or copper; becauſe the baſer metal, as well as the gold, grows lighter by the weight of a bulk of water equal to it; and the baſer metal being the more voluminous, the correſpondent water muſt weigh more than that, which is equal to the gold.

THIS hydroſtatical principle may be evidently proved from what has been demonſtrated in a mathematical way, by the moſt ſubtile Archimedes *de inſidentibus humido*, and his commentators; and thoſe, that are either unacquainted with, or diſtruſtful of ſuch ratiocinations, may find the principle made out in a phyſical and experimental way in another paper. Whence I concluded, that I might ſafely infer, that the floating inſtrument abovementioned would be made to ſink deeper by an ounce, for inſtance, of gold hanging at it under water, than by an ounce of braſs or any other metal, which by reaſon of its greater bulk than gold, looſing more of its weight by the immerſion, muſt needs retain leſs, and ſo have leſs power to depreſs the inſtrument it was faſtened to. Which concluſion you will eaſily believe the event did upon trial exactly juſtify; and I preſume you will as little doubt, that the concluſion will alſo hold (though the diſparity be not ſo great and conſpicuous) in reference to other metals, as lead and tin, that differ in ſpecifick gravity.

V. Hydroſt. Paradox.

To give at once an inſtance of the truth and uſe of this notion, I was induced to fit the inſtrument, that was grounded on it, for the examination of guineas, which are

 by

by far the moſt uſual gold-coins, that paſs in *England*. And though the exactneſs and diligence of our ingenious friend Mr. *Slingſby* allows us to expect, that no injury, that care and ſkill can prevent, ſhall be done to that coin; yet becauſe ſome goldſmiths and others retain fears of being deceived by the fraudulent and ſubtile artifices of falſe coiners, I thought it might not be amiſs to furniſh them with an eaſy and practical way of diſtinguiſhing a true guinea from a counterfeit. And though I hope I need not tell you, that I look not upon the inſtrument I ſhewed you at *Greſham-College*, as capable of examining gold and other metals with as much nicety, as by other methods one may hydroſtatically do; yet this little trifle may on ſome occaſions be preferable, ſince the inſtrument, which is not dear, being once fitted, there is no need to have either exact ſcales, or ſkill in hydroſtaticks, or any knowledge of arithmetick, and yet the difference of a true guinea from a counterfeit will not only be ſufficiently, but conſpicuouſly made to appear, and the operation will be much ſooner performed than in the other way, and very much ſooner and cheaper than by the methods commonly employed by goldſmiths and refiners. For, in our way the coin is not defaced or injured by cutting, punching, &c. nor is there any need of touch-ſtones, or aqua-fortis, and yet the trial is ſo quickly made, that perhaps near twenty guineas may be examined, one by one, in about a quarter of an hour: I ſay, one by one, becauſe, that if the inſtrument be deſigned and fitted for ſuch a purpoſe, many guineas may be tried at once. But whether the goldſmiths will make uſe of this way, I leave them to determine; it being ſufficient for me, to have gratified ſuch virtuoſi, eſpecially the diſciples of *Vulcan*, as have given occaſion to expect this trifle will be acceptable unto them; and to add this inſtance to thoſe I have elſewhere given by way of proof, that by the knowledge of cauſes men may employ exceedingly differing means to produce the ſame effects (as in our caſe, gold, that chemiſts and ſay-maſters are fain to examine by the fire, we examine by water) and, that philoſophical truths, and particularly hydroſtatical ones, are not lightly to be deſpiſed, as airy and empty ſpeculations, ſince they may be ſometimes applied to practical uſes, to which at firſt ſight they ſeem to have no relation at all.

SECTION II.

Deſcribing the conſtruction of this inſtrument.

I PROCEED now to the conſtruction of the inſtrument itſelf, in which are to be conſidered the matter and the form.

The matter may be glaſs, copper, ſilver, or almoſt any other ſolid body, that is, or may be made, fit to float in the water with a guinea hanging at it, and of a texture cloſe enough to keep out the water. For, if any of that ſhould, by ſoaking or otherwiſe, get in, it may alter the gravity of the inſtrument, and render it deceitful.

My firſt trials were made with bubbles of glaſs, furniſhed with ſlender ſtems, hermetically ſealed at the top; and theſe, when one can procure an artificer, that can blow them well, are both the gentileſt and the cheapeſt, and for ſome of the uſes, that may hereafter be mentioned, they are almoſt the only ones, that can be fitly employed. But beſides that it is not eaſy to meet with artificers, that can give glaſs the right bigneſs and ſhape, thoſe as all other inſtruments of glaſs, being very frail and ſubject to be broken; the ſafeſt way and more durable is, to make them of ſome metal, eſpecially either copper or ſilver, (of which the former is far more cheap, and the other more gentile, but either will ſerve well;) in regard they are leſs heavy, and, being more ſtiff, will maintain their figure better than gold or lead. Copper and ſilver will alſo ſuffer themſelves to be beaten into plates thin and yet ſtrong enough, and are not ſo ſubject to ruſt, as iron and ſteel. But in ſome caſes, eſpecially in want of metal in inſtruments,

inſtruments, we may make uſe of well ſeaſoned wood, laid over with ſome china varniſh, or ſome other, that is very cloſe.

As to the form of the inſtrument, it conſiſts of three parts; the ball or globulous part; the ſtem or pipe; and that, which holds the coin.

The ball or round part conſiſts of two thin concave plates of copper, or other metal, exactly ſodered together in the middle; and at the diſtanteſt parts from the commiſſure there ought to be left two oppoſite holes, one in each plate, for the two other parts of the inſtrument. This middle part, though for brevity ſake we name it the ball, ſhould not be exactly round, but, for the conveniency of ſwimming, of an almoſt eliptical or oval form, or rather ſomewhat inclining to that of a very deep double convex glaſs; or it may be of any other ſhape, that ſhall be found fitteſt to make the inſtrument keep its erect poſture ſteadily in the water. The bigneſs of it muſt be ſomewhat greater or leſs, as the plate is made thicker or thinner. But the general rule for its capacity is, that it ſhould contain as much air, as may ſerve to keep the whole inſtrument, when furniſhed, if need be, with its ballaſt and clogged with a guinea, from ſinking beneath the top of the ſtem, which ſtem is the next part to be taken notice of.

If the inſtrument be to have its ballaſt (if I may ſo call it) within its cavity, it will be convenient, if not neceſſary, that it ſhould be hollow, like a pipe, exactly cloſed at the upper end; but where the ballaſt is to be placed without, the pipe ſhould be made ſolid, as of a piece of wire, or a little cylinder of ſome lighter matter, that will not ſoak in water: but, whether it be hollow or no, it ought to be made very ſlender, that the different depreſſions of the inſtrument in the water may be the more notable. And for the ſame reaſon it ought not to be too ſhort, eſpecially if it be to be applied to other uſes than the examining of guineas.

The inſtrument, I moſt uſe merely for guineas, hath its ball about the bigneſs of a ſmall hen-egg, or rather leſs, and the pipe between four and five inches long, being ſodered on to the ball at the uppermoſt of the two holes abovementioned; at the undermoſt of which is inſerted and ſodered the undermoſt part of the inſtrument, which I call the ſcrew, or the ſtirrup, becauſe ſometimes it is made of a piece of wire, that a little beneath the bottom of the ball is bent round, ſo as to ſtand horizontally, that the guinea being laid on it, it may be ſupported by it, as the foot is by a ſtirrup; and in this way a piece of coin is the moſt readily put on and taken off. But the more ſecure way is, inſtead of the bent wire, to employ a very ſhort piece of braſs with a broad ſlit in it, capable of receiving the edge of the guinea, which with one turn or two of a ſmall and ſlight lateral ſcrew may be kept faſt in it, and readily, the operation being ended, taken out again.

If you deſire to examine not only guineas, but greater gold coins and metalline mixtures, it would be convenient, that the undermoſt ſtem and the ſcrew be made by itſelf, that it may be at pleaſure thruſt upon the ſtem and taken off again. For by this means, if the ball of the inſtrument be made large enough, you may have room to put on, as occaſion ſhall require, one, two, or three flat and round pieces of copper, lead, &c. with each of them a hole in the middle, fitted to the ſize of the ſtem, ſo that they may be put on as near the lower part of the ball, as you think fit, and then the ſcrew may be thruſt on after them, not only to take hold of the coin or metalline mixture to be examined, but to ſupport the plate, if need be; and by a variety of ſuch plates, which may be taken off and put on at pleaſure, the ſame inſtrument if (as I was ſaying) the ball be competently large, may be adjuſted ſometimes to a guinea, ſometimes to a coin of gold or ſilver, or to a metalline mixture twice or thrice as heavy as a guinea in the air.

The

THE inſtrument being made of a convenient bigneſs and ſhape; to adjuſt it for the uſe of examining guineas, you muſt by the help of the ſtirrup or ſcrew, hang, at the bottom of it, a piece of that coin, which you know to be genuine, and having carefully ſtopped the orifice of the ſtem, if it be a pipe, (that no water may get in at it,) immerſe the inſtrument leiſurely and perpendicularly into a veſſel full of clean water, until it be depreſſed almoſt to the top of the ſtem, and then letting it alone, if being ſettled it continue in the ſame ſtation and poſture, your work is done, but if it ſink quite under water, you muſt lighten it either with a file, or by ſcraping or grating off a little of the ballaſt-plate abovementioned; or, if you have put any weight into the cavity to poiſe it, by taking out ſome of that, until you have made it light enough: but if, when you leave the inſtrument to itſelf, it emerge, you muſt then add a little weight to it, either by putting into the ſtem, if it be hollow, ſome duſt ſhot, filings of lead, or ſome other minute and heavy body, or elſe by putting on the ſhort ſtem abovementioned, that comes out beneath the ball, a flat, round, and perforated piece of lead, of weight, ſufficient to enable the guinea to depreſs the weight, as low as it is deſired: which being done, a mark is to be made juſt at the place, where the ſurface of the water touches the ſtem, and then taking out your inſtrument, ſubſtitute in the place of your guinea a little round plate of braſs, of the ſame weight, or a grain or two heavier, in the air; and putting the inſtrument into the water as before, ſuffer it to ſettle, and make another mark at the interſection of the ſtem and the horizontal ſurface of the water.

ABOUT this way of adjuſting our inſtrument, the following particulars may be noted:

IF a ſcrew be employed to ſuſtain the guinea, the coin ought to be ſo placed, that one half, according to the eſtimate of the eye, may be on the right hand, and the other on the left hand of the ſcrew; that the inſtrument being depreſſed may continue in an erected poſture, and not ſwerve to an inclined.

THOUGH, when the ſtem is hollow, and the inſtrument too light, it may ſeem the better to add quickſilver than any other weight, becauſe of its fluidneſs, and great ſpecifick gravity; yet, unleſs the inſtrument be of glaſs, it is not ſafe to employ mercury, becauſe it is apt to diſſolve the ſoder.

IF the marks be made of a white colour, they will be ſo much the more conſpicuous: and theſe marks may be made, if the pipe be hollow, by making round impreſſions with a ſmall file, and encompaſſing them with little circles of fine wire of ſilver, gold, &c. And, if the ſtem be ſolid, it may then be either quite perforated at the requiſite places, and have the holes filled with chawed maſtic, or ſome ſuch white ſubſtance, that diſſolves not in water, or elſe have little holes, that pierce not quite through, ſtuck into it; and theſe may likewiſe, be filled with the ſame ſubſtance, which, if further diſtinction be deſired, may have ſome parts of it differingly coloured, before they be employed.

IT will be requiſite to employ in adjuſting the inſtrument one of the heavieſt guineas you can get, to depreſs the inſtrument as low as it is like to be by any piece of that coin, leſt otherwiſe meeting with one conſiderably heavier than that you made uſe of, the inſtrument may be thereby made to ſink to the very bottom of the water.

THE reaſon why it is above preſcribed, that the inſtrument be immerſed almoſt, not quite, to the apex of the ſtem, is, becauſe I have found, that guineas are not all preciſely of the ſame weight, nor all waters neither; and therefore it is ſafeſt, to leave a ſmall part of the ſtem, as an eighth, or, in longer inſtruments, a quarter of an inch, extant above the water, that we may ſecure the inſtrument from being by a heavier guinea made quite to ſink.

I foresee, it may be hence objected, that these contingencies may make our instrument useless: to which it is not difficult to answer, that, though some guineas weigh a grain or two more than others, it is not that will frustrate the use of our instrument, and less will the difference of our waters do it, since (as I have observed in another paper, where I mention some trials of this kind) having examined and compared together the specifick gravities of (common) pump-water, Thames-water, and rain-water, I found the difference far more inconsiderable, than one would have thought, and consequently unable to keep hydrostatical trials of metals from being accurate enough for practice, and more exact than those troublesome and chargeable ones, that are commonly relied on.

These answers to the recited objections will be made good by this, that it is not a doubtful or inconsiderable difference, that appears upon the differing depressions of the instrument, that are made by a true guinea, and by a piece of brass or of copper, of the same weight with it in the air. For, in the instrument lately described, though smaller than most, that I have employed, the distance betwixt the mark, to which the gold, and that, to which the other metal, though copper, depressed it, was, by measure, about an inch and three quarters; so, that it is not every small variation of circumstances, that can make it doubtful to him, that employs our instrument, whether a guinea be true or counterfeit.

But philosophical candor forbids me to conceal, that there may, (though it is like there very seldom will,) happen a case, wherein, though the principle, our instrument is framed on, will hold good, yet the practical application may be unsecure. For, if a falsifier of money have the skill, by washing or otherwise, to take off much of the quantity or substance of the guinea, without altering or impairing either the figure or the stamp, the piece of coin will not be able to depress our instrument to the usual mark, and may thereby make it be judged counterfeit, when it is indeed but too light.

But on this occasion it is to be considered, that neither the touch-stone, nor aqua-fortis, nor antimony, nor the cupel, can shew us, whether a piece of coin proposed have its just weight, but only, whether the metal be true gold; and therefore our instrument need not pretend to do more, than discover the genuiness of the metal: but whether the coin have the just weight the law requires, is to be judged by the balance; as each single piece is wont to be in most of the gold coins of *Europe*, and is in *England*, in reference to angels and twenty-shilling pieces, and all the other coins of broad gold, as they are now called. And yet it may be further considered, that our instrument does more than it need pretend to: for, without a pair of scales, it presently shews, that the proposed guinea, if it be not counterfeit, is otherwise abused; and though it does not clearly determine, whether that likewise proceed from the want of specifick gravity in the metal, or from the coins having been washed, or otherwise fraudulently lessened; yet it probably resolves the doubt, because, if the want of weight appear by the instrument to be very great, as it usually does, where the piece has been robbed of some of its substance, (especially if it be so much, as is reported, of some guineas, that of late are said to have been found wanting to the value of near four shillings;) it is a strong presumption, that it is rather washed, &c. than counterfeited. For men will scarce venture their lives to steal but three or four grains from a true guinea, and much less from a false one. And they, that counterfeit, are not wont to be so sparing as to make their coins too light. However, our instrument will in these cases be sure to prompt him, that uses it, to employ the balance, which will presently assist him to resolve his doubt. For, if the suspected coin have in the air its due weight, it will argue, that the greatest lightness of it in the water proceeds from the metal's not being true gold, or, at least, of its not being of the requisite fineness; and, if it want much

much of its due weight in the air, it is very probable, for the reason above-intimated, that it is washed, &c. rather, than of another metal than gold; and however may be lawfully refused to be taken in payments, and perhaps afford a just ground of questioning him, that utters it. And if one would, for curiosity, be further satisfied, whether the metal be gold or no, one may add to the coin (as will be hereafter taught) as much sterling-gold, as will make it, in the air, of the weight of a guinea, and then examining it by the weight in the water, he will presently discover, whether it be gold or not.

THERE comes into my thoughts another possible way of counterfeiting guineas; but because it is very likely, that coiners will not light upon it, and it cannot be practised on any of the guineas already coined, the fear of teaching bad men a skill, that probably they will not otherwise acquire, makes me forbear to mention it, though the fraud may be quickly discovered, sometimes by the bare eye, and always by our instruments and the balance; whereof publick advertisements may be given, if there shall appear need of it.

AND now I have this to add about the construction of this instrument, that perhaps it would not be very difficult to propose a much more accurate and elaborate contrivance, if it were thought fit to propound any, that would require an extraordinary skill in the artificer to make it, and some considerable skill or dexterity in the person, that is to use it: but the slight construction, hitherto described, seemed to suit better with my principal aim, which was, to propose at present an instrument, as simple, cheap, and easy, to be employed and kept in order, as I could well examine guineas with; little doubting, but that the principle, upon which this is framed, being well understood and considered, will, if it be found useful, be further improved by new applications and more artificial contrivances.

Explications of the figures.

IN fig. 1. A B, the stem or pipe.

C E, the two parts of the ball sodered together.

B C D E, the ball itself.

F, the screw.

G, the stirrup, somewhat represented out of its place.

H, the mark to which a copper-plate, of equal weight in the air with the guinea, depresses the instrument.

I, the mark to which a true guinea sinks it.

FIG. 2. is the screw by itself, to be put upon, or taken from the (short) undermost stem of the instrument.

FIG. 3. the perforated plates of lead or other metal, to be put on as ballast upon the undermost stem.

FIG. 4. the undermost stem, with a perforated ballast-plate put upon it.

FIG. 5. the stirrup, that may be employed instead of a screw.

FIG. 6. A B C, the glass-instrument.

D D D, the coin hanging at the bottom of it, and supported by four horse-hairs, or slender strings of silk.

FIG. 7. the undermost stem of the glass-instrument, to which, being streight and solid, a screw is fastened on with horse-hair or otherwise.

FIG. 8. A B C D, the small glass-instrument for estimating the specifique gravity of liquors, (of which an account may be expected in our next.)

E E, the quicksilver and water, that is employed as ballast to sink it in an erected posture.

SECTION III.

Representing the uses of this instrument, as relating to metals.

THREE is in the nature of the thing such a connection between the fabrick and use of our instrument, that I could not well describe it without plainly intimating the principal uses of it. Wherefore I shall here but summarily repeat those, that are delivered already, and make a more explicit mention of those few, that have been either omitted, or but lightly touched.

USE I.

The first use, and that, which was mainly intended, is, easily and cheaply to discriminate true guineas from counterfeit, without defacing, or any ways injuring the coin. But of this use I have spoken largely enough already, and therefore shall advance to the next.

USE II.

Any other kind of gold-coin, that is near about the weight of a guinea, may be examined by our instrument after the manner above delivered; but more easily, if it want of the weight of a guinea, than if it exceed it. For in case it be heavier, as is a twenty-shilling piece of broad gold, the ballast, whether internal or external, of the instrument must be taken off, that so heavey a coin may not quite sink it; whereas, if the coin proposed be lighter than a guinea, one may add as much gold (of the same alloy) beaten into thin plates, as, with the coin proposed, will make up in the air the weight of a guinea. For then this aggregate, being examined, as if it were a guinea, will discover in the water, whether the coin be right or counterfeit. I shall add, that if the piece, to be examined, be not much heavier than a guinea, it may be convenient to pass a very small perforated plate of copper or lead over the upper stem (or pipe,) so as to make it rest upon the ball before the instrument is adjusted. For, by this means, nothing need be altered beneath the ball; and such pieces of metal (of which several differingly heavy may be easily provided) being thin and light, will not (as trial has shewn) make the instrument top heavy, though one of them be placed above the center of gravity, and may be very readily taken off, and (if need be) scraped or filed to lighten the instrument, when an extraordinarily heavy guinea, or a coin somewhat more weighty than a guinea, is to be examined.

But to return to what I was saying about adding a weight of gold to a piece of proposed coin; in order to this use it will be necessary, that the slit or aperture at the bottom of the instrument, which is to be shut and opened by the lateral screw, be made (as it easily may without inconvenience) wide enough to receive double the thickness of a guinea, that so different coins, as *English*, *French*, *Spanish*, &c. and the grain-weights, necessary to bring them to the weight required (in the air,) may be securely fastened to the instrument by the screw.

If the ball be large, and the pipe well proportioned to it, coins, that do not much exceed the weight of a guinea, may be examined without much altering the weight of our instrument, provided it be at first adjusted so, as that a guinea will not depress it so far as not to leave a considerable part of the pipe above water, that the coin heavier than a guinea may not be able to draw it quite under water.

According to the method above described, may half guineas be examined. For, if the instrument be good, it will shew a manifest difference, if instead of an entire guinea, you fasten in the screw a half guinea, that you know to be true, and that, which is fuspected to be counterfeit; adding a grain-weight or two of gold, in case the proposed coin needs it; I say, a grain-weight of gold, because, if it be of brass, of which the grain-weights, commonly used, are made, it will loose in water more than it should of the weight it had in the air; and therefore it will be useful to such, as intend to try several sorts of English coins, as angels, two and twenty shilling pieces, double guineas, &c. to have by them a numerous set of grains, (about whose shape, by the way, one need not be curious, that not being material) made of a thin plate of sterling gold.

USE III.

If the instrument be skilfully fitted for such a purpose, it may be made to serve to examine some sorts of white money less heavy than half crowns. And because it may be useful to know in general, what coins may, and what may not, be examined by this or that particular instrument proposed, I shall here add a general way, that is not difficult for finding this out; namely, first by weighing the piece of gold or silver in the air, and afterwards in the water, and substracting the latter from the former, to obtain the difference of the two weights: and next, by weighing also in the air and in water a piece of copper, or brass, if this be the likeliest to be employed in counterfeiting the coin, and observing likewise the difference between those weights. For, the lesser of these differences being substracted from the greater, the remains will shew, how much the true piece of coin will outweigh the other in the water; and consequently if so many grains, as this residue amounts to, being added to the weight of the lighter metal, do make a sufficiently manifest depression of it below the mark it would stay at without that addition, one may probably conclude, that the difference between a true and counterfeit piece of coin proposed will be discoverable by the instrument.

The cheapness of these slight instruments being considered, it may be expedient for goldsmiths and others, that have frequent occasions to examine various sorts of coin, to have a several instrument adjusted for each of them, to save themselves some pain and trouble. But if the ball be made large, and fitted with a stem slender and long enough, one may quickly by changing the ballast-plates, as occasion requires, fit the same instrument to examine coins of differing metals, and of very differing weights. For one of these, made of copper, serves me to examine both guineas and crown-pieces of silver, and half crowns too; and it may be easily made to serve also for divers foreign coins.

USE IV.

It is a great complaint of pewterers, that the tin they buy of the miners or merchants, is often adulterated with lead, as they find to their prejudice, when they have made vessels of it. And many others, that are buyers, complain much more of divers pewterers for putting too much lead into their pewter, because lead is by many times cheaper than tin, On these accounts, I shall add, to the other use of our instrument, something, that relates to tin and pewter. Though I must take notice, that some tin may perhaps be found a little heavier in specie than ordinary, although no fraud intervene; because I have observed some tin (as I elsewhere relate) to contain some, though but a very little, proportion of gold or silver. But this being no usual case, I shall proceed to say, that the pewterer may judge, whether the miner or merchant have deceived him; if, taking a piece of tin, that he knows to be pure, and is of a convenient weight, he observes, how much it depresses the pipe, and then makes the like observation with an equal piece of the tin suspected to have lead or some other metal in it. For if this depresses the instrument much lower than the other, it will justify the suspicion; since as gold, being the heaviest of metals, cannot be allayed by any other, that will not depreſs our inſtrument leſs than gold can do; ſo tin, being the lighteſt of metals, cannot be mixed with any other, that will not ſink it lower than unmixed tin, (ſtill ſuppoſing the weights to be the ſame in the air.)

And as for the buyers of pewter, it will be eaſy for them (if they think it worth while) to find by our inſtrument, if there be too much, or but enough of lead mixed with the tin in an aſſigned portion of pewter of convenient weight to be examined by it. For, having once obſerved, how much the inſtrument is depreſſed by a piece of two, three, or four drams, or even an ounce weight of pewter, which is known to be good, and to contain ſuch a proportion of lead in reference to the tin, if you load the inſtrument with an equally heavy piece of any other maſs of pewter propounded, if the inſtrument ſink deeper, it will be a ſign, that the former proportion of lead may be very probably argued to exceed in the mixture; I ſay, probably, becauſe perhaps it is poſſible to embaſe pewter by mixing not only lead, but other mineral ſubſtances, whoſe ſpecifick gravity is not well known: but yet I ſay very probably, becauſe the addition of too much lead is the moſt gainful way of adulterating pewter. And the other things, that ſome employ, as regulus of antimony, tin-glaſs, copper, and ſpeltar, are ſeldom uſed in great quantities; and if I thought it worth the while, I could facilitate the diſcovery even of theſe by adding, what I have obſerved of their differing ſpecifick gravities, and ſome other things, that I think fitter to be here omitted than to have time and words ſpent upon them.

USE V.

The laſt uſe, I ſhall now mention of our inſtrument, in reference to metals, is, that it may aſſiſt us to eſtimate the quality of metalline mixtures, whether in coins or other maſſes, and to gueſs at the proportion of the ingredients, that compoſe them. For, ſince we have formerly ſeen, that the ſame inſtrument, employed to examine guineas, ſerved alſo for crown-pieces of ſilver, that wanted of an ounce leſs than a twentieth part of that weight, it will be eaſily granted, that the ſame inſtrument, and more eaſily, that a larger one, may be ſo fitted, as to help goldſmiths, chemiſts, and others, that are not acquainted with hydroſtaticks, to make ſuch an eſtimate, as will not much deceive them, of the fineneſs of gold and its differing allays with ſilver, or ſome its other determinate metal.

In order to this, the inſtrument may be fitted to ſink to the tip of the pipe, with ſome determinate weight of the fineſt gold, as of 24 carats, as they call that, which is moſt pure and fine. But it will be convenient, that this metal in the air be juſt an ounce, or half an ounce, or ſome ſuch determinate weight, that is commodiouſly diviſible into many aliquot parts. Then you may make a mixture, that contains a known proportion of the metal wherewith you allay the gold; as if it hold 19 or 15 parts of gold, and one of ſilver; and, letting the inſtrument ſettle in the water, mark the place, where the ſurface of the water cuts the ſtem or pipe. And then putting in another mixture, wherein the ſilver has a new and greater proportion to the gold; as if the former be an eighteenth or a fourteenth part of the latter, you may obſerve, how much leſs than before this depreſſes the inſtrument, and ſo you may proceed with as many mixtures or degrees of allays, as you think fit, or can be diſtinguiſhed conveniently on the ſtem; being always careful, that, whatever be the proportion of the two ingredients, the weight of the maſs in the air be juſt the ſame with that of the pure gold, which we have lately ſuppoſed to be one ounce, or half an ounce.

By the ſame method may be examined the differing alloys of pure ſilver upon the admixture of ſuch and ſuch determinate proportions of copper, or any other metal, lighter in ſpecie than ſilver; and by the ſame way, with a ſlight variation, it will not

 be

be difficult to eſtimate, how much divers coins, whether of ſilver or gold, are more or leſs embaſed by the known ignobler metal, that is mixed in the piece propoſed.

AND though this way of determining the alloys of metals, be not ſo exact, as is poſſible to be propoſed by the help of hydroſtaticks and calculation; yet it may be very uſeful to chemiſts, goldſmiths, refiners, and others, that are unacquainted with hydroſtatical matters, to make without trouble or ſupputation eſtimates, that will not much deceive them, and perhaps will come nearer the truth, not only than the eſtimates wont to be made by the touchſtone, but perhaps too than ſome of thoſe, that divers make with trouble, and inconvenience, and charge. And indeed I was chiefly invited to communicate this trifle, and ſpend ſo many words about it, by the requeſt of ſome ingenious diſciples of *Vulcan*, who thought they perceived, that by this way they could oftentimes make better eſtimates of the ſucceſs of their graduating, and ſome other operations upon metals, than otherwiſe they they ſhould be able; this way greatly accommodating them by this particular advantage, that they may from time to time try the degrees of purity, and ſome other conſiderable alterations of their mixtures, without at all deſtroying or injuring them, though they have not yet attained the pitch they aim at and expect; whereas, if they happen to be too forward, as often they are, in examining the productions of their labours by the cupel or ſevere cementations, what they would try may be deſtroyed or ſpoiled in its way to a perfection, which otherwiſe, in their opinion, it might in due time be brought to.

PERHAPS it may not be amiſs, on this occaſion, to add, as an improvement of this fifth uſe of our inſtrument, that it may be employed to examine other mixtures beſides allayed coins, and that if the inſtrument be adjuſted to an ounce, for inſtance, of pure copper, it may help men to make an eſtimate of the alloy of tin, or the quantity of it, that is oftentimes added to copper, to make differing ſorts of bell-metal, and of thoſe metalline ſpecula, whether plain or concave, that are called ſteel glaſſes, as alſo of ſoders conſiſting of certain proportions of ſilver and braſs, or copper; in all which, and divers others, the diſcovery of the proportion of the ingredients may, on ſome occaſions, be uſeful to tradeſmen, as well as deſirable by virtuoſi. And though I have obſerved, that by mixture, tin and copper acquire a ſpecifick gravity ſomewhat differing from what their ingredients promiſe; yet, ſince the inſtrument is to be fitted for ſuch eſtimates, not by calculation, but by trials, the eſtimates may be made near enough to the truth.

NEW EXPERIMENTS

ABOUT THE

Weakened SPRING, and ſome unobſerved Effects of the AIR.

First publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. CXX. p. 467, for *December* 27, 1675.

AS for the not yet communicated trials, that I made in proſecuting my deſign of diſcovering or obſerving ſome latent qualities of the air, I will not deny you ſome of them, as imperfect as yet they are, but will venture to ſend them you, as my notes or my memory ſuggeſt them to me; not only, becauſe without

without being complicated they may be fit enough to countenance ſuſpicions (for you know, that I do not call them ſo much as opinions,) but for a weightier inducement, to be told you at the end of this paper.

The two chief things aimed at in the imperfect attempts I now ſend you, were to diſcover; firſt, whether, as ſome corroſions of bodies do in cloſe veſſels increaſe the ſpring of the air (as I long ſince noted them to do,) ſo ſome other corroſions may not, by a contrary, or ſome other way, weaken the ſpring of the air; and next, whether in ſome ſolutions and precipitations the air on the account of ſome unobſerved quality may not be found to produce ſome phænomena not yet taken notice of.

In order to each of theſe inquiries, I ſhall mention a few trials, though without curiouſly ſorting them, becauſe ſometimes in the ſame experiment both thoſe attempts were jointly proſecuted.

You may remember, that in ſome of my formerly publiſhed trials I acquainted you with an odd phænomenon of the change of colour producible in ſolutions of copper by the operation of the air: I ſhall now add, what further phænomena my memory or notes ſupply me with, about the ſubject of that and the like experiments.

EXPERIMENT I.

We took filings of crude copper, and put them into a chryſtalline glaſs of a conical ſhape, into which we poured ſome ſtrong ſpirit of ſalt, (that was fitted for our peculiar purpoſe) to the height of about a finger's breadth above the filings; and then cloſing the veſſel with a glaſs-ſtopple exquiſitely fitted to it, we ſuffered it to continue unmoved in a window for ſome days, until the liquor had both obtained a high and darkiſh brown colour by the ſolution of ſome of the copper, and loſt that colour again, growing clear like common water, (which is itſelf a ſomewhat odd phænomenon;) and then taking out the ſtopple, (without ſhaking the liquor) and thereby giving acceſs to the outward air, we perceived, (as we had conjectured) that the upper ſurface of the liquor did in a few minutes re-acquire a darkiſh brown colour, which penetrating deeper and deeper, at the end of about a quarter of an hour the whole body of the liquor appeared to be likewiſe tinged. The conical glaſs being again well ſtopped, the menſtruum did again in very few days let fall, or otherwiſe looſe its tincture; which, the ſtopple being taken out, it re-gained as before. Nor were theſe two the only trials I made with the like ſucceſs for the main; but afterwards being deſirous by a further trial to reſolve a doubt I had, I kept the glaſs yet longer in the ſame place with the ſame filings and menſtruum in it for (if I miſ-remember not) a month or two together; but obſerved not, that the liquor would any more grow clear.

EXPERIMENT II.

Having taken ſuch a glaſs, as is mentioned in the firſt experiment, wherein the liquor was grown clearer than is uſual, and had probably been ſo a good while before (for the veſſel, having been hid by others, which ſtood before it, had been for ſome weeks forgotten;) we took out the ſtopple, and left it open for about half an hour, but did not perceive the liquor to have acquired any colour ſo much as at the top. Whether this proceeded from the long debarring of commerce with the freſh air, or from ſome other cauſe, being unable to wait the event as long as would perhaps be requiſite, I thought fit to try, whether the air had already had ſome operation upon the liquor, though it did not yet appear; and accordingly putting in the ſtopple, I left the veſſel cloſed for two or three hours, and at my return to viſit it, I perceived, that

that it had acquired a faint colour tending to a green; wherefore taking out the ſtopple again, I opened its commerce with the outward air, leaving the glaſs unſtopped for 20 or 24 hours, but found, that in all that time it had not regained its wonted dark colour, but was only arrived at a green, deep enough, but not true, nor very tranſparent.

This obſervation being made in the ſame veſſel, that had been formerly employed, ſuggeſted to us an enquiry, whether the advanced time of the year, which was the middle of *October*, might not have an intereſt in the ſlow and imperfect ſucceſs of this trial.

EXPERIMENT III.

Some ſtrong ſpirit of ſalt having been kept upon filings of copper, until the ſolution was come to be of a dark brown colour, about three ſpoonfuls of it by gueſs was put into a receiver, that might hold eight or ten times as much: being kept in vacuo (if the time be rightly remembered) about half a year, it retained its colour, but the veſſel being opened and the external air permitted a free acceſs to it, the ſolution in about an hour was turned into a fine tranſparent green, though no precipitation of any muddy ſubſtance appeared by any ſediment to be made.

EXPERIMENT IV.

In one of that ſort of conical glaſſes, that has been already more than once deſcribed, we had put upon ſome filings of copper, a convenient quantity of our ſpirit of ſalt; and though we obſerved, that for a great while it would not part with its deep and ſomewhat muddy tincture; yet we left it in the window for many weeks longer, and at length, towards the latter end of *December*, we found it to have loſt its tincture, ſo much, that the liquor appeared like common water. Upon which obſervation, though the time of the year were unpromiſing, I thought fit to try, whether the air in that ſeaſon would not have ſome, though perhaps but a ſlow operation on the ſaline ſpirit, and accordingly taking out the glaſs ſtopple to give free acceſs to the outward air, we obſerved, that in ſome hours its operation on the liquor was ſcarce ſenſible, but within about 24 hours the menſtruum had acquired not juſt its former colour, but a ſomewhat faint and moderately tranſparent green: ſo that this tincted menſtruum, as it had been very ſlow in loſing its colour, ſo it did but ſlowly and imperfectly re-acquire it.

" I have not in the foregoing experiments made mention of any phænomena of " them relating to the Spring of the included Air, becauſe I do not remember, that " they were ſuch, as invited me to draw any poſitive concluſion from them, and my " ſilence on this occaſion may be the more allowable, becauſe the way of further mak" ing ſuch obſervations may be ſufficiently deduced from the enſuing trials; in reciting " of which I alter very little, and in ſome of them not at all, the expreſſions I find " them regiſtered in, though more than once the phænomena, that relate to the air's " elaſtic power, be mingled in the ſame experiment with the mention of its operations " upon colours.

" The ſpring of the air and its variations, by the ways now known to many of the " curious, being things, that manifeſtly appear to have a notable intereſt in divers " phænomena of nature, whoſe cauſes, if not themſelves alſo, were unknown to " former philoſophers; it ſeemed an attempt, though not very promiſing, yet worth " the making, to try, whether the ſpring of the air, which may divers ways, as by " heat, compreſſion, &c. be increaſed, may not by ſome other way than cold and di-

" latation

" latation be weakened: and having often found menſtruums, that corrode metals, " ſo as to produce bubbles to invigorate the ſtrength of the ſpring of the air included " in the veſſels, wherein the ſolution was made, I thought fit to try, whether in ſome " metalline diſſolutions, wherein I had obſerved, that few or no viſible bubbles at all " were produced, the ſpring of the neighbouring and included air would not be debi- " litated; and in order to this were made the following trials."

EXPERIMENT V.

[We took ſome filings of copper, and putting them together with a mercurial gage * in a conical glaſs fitted with an exactly ground-ſtopple of the ſame matter, (which was chryſtalline) we poured on the filings, as much rectified ſpirit of fermented urine made *per ſe*, as ſufficed to ſwim an inch or better above them; then carefully ſtopping the glaſs, coming to look on it many hours after, we perceived, that the mercury in the ſealed leg was conſiderably depreſſed, and gently drawing out the ſtopple to let in the outward air, we perceived that acceſs to have a manifeſt effect upon the mercury.]

But this will be better underſtood by the more circumſtantial experiment, that enſues.

EXPERIMENT VI.

[We took a cryſtal glaſs of an almoſt conical ſhape, and capable of containing between five and ſix ounces of water, and furniſhed with a ſtopple of the ſame matter, that by grinding was exactly fitted to it. Into this we put a convenient quantity of clean filings of good copper, on which we poured as much ſtrong ſpirit of fermented, or rather, putrified urine, as ſerved to ſwim about an inch above the copper, and having let down a mercurial gage, ſo that it leaned upon the bottom and ſide of the glaſs, we cloſed it very well with a ſtopple, and ſet it in a quiet and well enlightened place, having taken good notice at what mark the quickſilver reſted in the open leg of the gage. This done, we let the menſtruum alone to work upon the filings; which it did, as we foreſaw, ſomewhat ſlowly and very calmly, without producing any noiſe or ſenſible bubbles, acquiring by degrees a very pleaſant blue colour, and the glaſs being kept quiet in the ſame place for two or three days longer, the liquor, as I conjectured would happen, began to loſe of the intenſneſs of its colour, which by degrees grew fainter and fainter, until at the end of three or four days the liquor was grown very pale, and left me little doubt but that, if I would have ſtaid ſome days longer, it would have loſt the remaining eye of blue, and have looked almoſt like common water. But being unwilling to tarry ſo long, I took out the ſtopple, that the air without the glaſs might have acceſs to that within; and leaving the vial in the ſame place and poſture, my expectation was ſomewhat anſwered by finding, that within four or five minutes, if not leſs, the upper part of the liquor, that was contiguous to the air, had acquired a fine blue colour, which deſcending deeper and deeper, before the end of the tenth minute had diffuſed itſelf, but ſomewhat weakened, through the liquor, whoſe colour was ſuffered, to deepen for a while longer; ſo that in leſs than a quarter of an hour from the firſt unſtopping of the vial, the liquor was grown to be throughout of a rich ceruleous colour, which grew almoſt too opacous within a few minutes longer: when carefully cloſing the vial again with the ſame ſtopple as before, we ſet it aſide in the ſame place, where, the included air being denied all commerce with the external, the liquor began again, within two or three days, to loſe of its colour, and, to be ſhort, afforded me the opportunity of making a ſecond experiment, much like the former. And the like ſucceſs I had, for the main, in a trial or two made in another glaſs

See the reference in the foregoing experiment.

* About ſuch glaſſes, ſee Experiment XVII. in the continuation of our New Phyſico-Mechanical Experiments.

glaſs with another portion of the ſame ſpirit of urine, put upon the filings of copper; ſo that the experiment was, in all, made divers times, as well when I was not, as when I was alone: and particularly, once to be ſure, that the diurnal air as ſuch had not any great intereſt in the phænomenon, I made the trial ſucceſsfully about nine a clock at night, in the preſence of ſo well known a witneſs, as the learned ſecretary of the Royal-Society.

One circumſtance I forgot to take notice of, which was, that in moſt of theſe experiments I forbore to ſhake the glaſs, leſt it ſhould be ſuſpected, that the agitation of the liquor might have raiſed ſome little fine powder, that might have been ſuppoſed to have been precipitated out of the tincture, and, being thus mingled with the liquor again, reſtore it to its former colour; but in truth, I did not perceive any ſuch powder to be precipitated. And though, to obviate the objection, I forbore to ſhake the vial; yet I juſtly ſuppoſed, that if, by the agitation of the liquor, more parts of it ſhould be quickly expoſed to the action of the air, the coloration would be haſtened, which upon trial appeared to be true.

EXPERIMENT VII.

[Experience has made me think it likely, that ſtrong ſpirit of ſal armoniac, made without quick-lime, would operate more nimbly and more powerfully on that metal than our ſpirit of urine had done; we took ſuch a conical glaſs, as has been lately deſcribed, and covering the bottom of it with a convenient quantity of filings of good copper, we poured on them as much ſtrong ſpirit of ſal armoniac, as ſerved to ſwim about a finger's breadth above them; and, having let down ſuch a mercurial gage, as is formerly mentioned, ſo that it leaned upon the bottom and ſide of the glaſs, we cloſed it very well with a ſtopple, and ſet it in a quiet and well enlightened place, having taken good notice at what mark the quickſilver reſted in the open leg of the gage: this done, we let the menſtruum alone to work upon the filings, which it did, as we foreſaw, ſomewhat ſlowly and very calmly, without producing any noiſe or ſenſible bubbles, acquiring by degrees a very pleaſant blue colour, and afforded us alſo the phænomenon we chiefly looked after; which was, that repairing from time to time to the window to ſee what paſſed, we perceived, that for two or three days together the mercury in the ſealed leg of the gage did, though very ſlowly, deſcend, until it appeared to be near a quarter of an inch lower than at firſt; and probably the depreſſion might have been greater, if ſome indiſcreet body or other had not, by tampering with the glaſs, diſturbed the experiment; whoſe event yet ſeemed ſufficiently to argue, that the ſpring of the air, contained in the cavity of the glaſs, and communicating with that in the open leg of the gage or ſyphon, was weakened in compariſon of that in the cloſed leg, which by the hermetic ſeal on one ſide, and the quickſilver on the other ſide, was kept from ſuch communication.

And becauſe I thought it might be ſuſpected, that the phænomenon might be referrable to ſome inequality in the preſſure of the air, occaſioned by the greater operation of the heat of the day on the more impriſoned air of the gage, than on that more immediately included in the cavity of the vial; I was careful to obſerve, whether the depreſſion did not continue at differing times of the day, and found it to do ſo, as well at night, as at noon, though at this laſt named time, the ſun ſhined hot upon the place and veſſels too.

This experiment was made, in all, four or five times, though not always with equal, yet ſtill with ſome ſucceſs, the mercury in the ſealed leg of the gage being ſometimes more and ſometimes leſs, but always manifeſtly depreſſed; which phænomenon was confirmed by the obſervation we more than once made of the ſudden return of the quickfilver to its former ftation, upon the unftopping of the glafs, to give free admiffion to the outward air.

EXPERIMENT VIII.

CONSIDERING, whilft I was about thefe trials, that fpirit of vinegar, though in working upon coral and fome other bodies, it not only produces ftore of bubbles, but alfo, as I have elfewhere delivered *, a fomewhat odd kind of elaftical fubftance, yet being put upon minium it was wont, in my obfervation, to work calmly and without producing froth; I thought fit to make trial, whether this calm and filent folution of minium would be accompanied with a permanent change of the air's fpring: the event I find thus fet down:

[A pretty quantity of fpirit of vinegar being put upon minium in a conical glafs, furnifhed with a glafs ftopple and a mercurial gage, continued divers days without any fenfible depreffion of the mercury in either leg, nor did any change appear in the gage, upon the removal of the ftopple, though it was evident by the great fweetnefs acquired, that it had made a folution of a great portion of the minium.] But to return to our trials upon copper.

EXPERIMENT IX.

[WE took fome filings of copper, and in a vial capable of holding fome two or three ounces of water, we poured on them ftrong fpirit of fal armoniac, made without quicklime, till the liquor reached near an inch above them. This was done about the twentieth of *Auguft*, on the *Friday* before noon, and the following *Monday*, prefently after dinner, it had acquired a deep blue tincture, and loft again fo much of it, that it was pale, almoft like common water: then, to fatisfy a virtuofo, I unftopped the vial, defiring him to place his eye level with the furface of the liquor, which, in a minute of an hour, or lefs, appeared, to his furprife and wonder, to have acquired a deep blue tincture, that reached downwards to the thicknefs of the back of a knife, the whole liquor becoming of the like colour in four or five minutes more, and the glafs being prefently ftopped again, and left where it was before, appeared not at the end of nine days, to have loft its tincture; though now and then, within that time, it feemed manifeftly paler than when the vial was ftopped.]

" NONE of the former trials with fpirit of fal armoniac having been made in an her-
" metically fealed glafs, it will not be amifs both to diverfify and to confirm our ex-
" periments, by fetting down the fuccefs of one made in fuch a veffel."

EXPERIMENT X.

[WE took a round vial, holding about eight ounces of water, and having put into it filings of copper and a mercurial gage, we poured on the metal ftrong fpirit of fal armoniac, till it reached to a good height in the vial, which then being hermetically fealed up, was fet by in a fouth-window, where it quickly acquired a deep blue tincture: there

* To the better underftanding of this, the enfuing trial may much conduce; and therefore is tranfcribed out of another paper, to which it properly belongs.

A mercurial gage having been put into a conical glafs whofe bottom was covered with beaten coral, fome fpirit of vinegar was poured in, and then the glafs ftopple, which was very well ground, clofing the neck exactly, we obferved, that upon the working of the menftruum on the coral, ftore of bubbles were for a good while produced, which fucceffively broke in the cavity of the veffel, and their acceffion fo conftipated the air, that they compreffed the air imprifoned in the clofed leg of the gage three marks or divifions, which I gueffed to amount to about the third part of the extent it had before: but fome hours after the corrofion had ceafed, the compreffion made by this newly generated air grew manifeftly fainter, and the imprifoned gage air drove down the mercury again, till it was depreffed within one divifion of its firft ftation; and thereabouts, or little lower, continued five or fix days; fo that in this operation there feemed to have been a double compreffive power exercifed; the one tranfient, by the brifk agitation of vapours or exhalations, and the other durable, from the aerial and fpringy particles, either produced or extricated by the action of the fpirit of vinegar upon the coral.

there it ſtood about twelve days, before that tincture, which decayed but ſlowly, did, little by little, grow ſo diluted, that the liquor was pale and almoſt like water: during this ſtay of the glaſs in the window, the mercury in the open leg appeared to be impelled up, and when after nine o'clock at night, (which time I choſe to try, whether the nocturnal air, as nocturnal, would have any thing to do with the phænomenon,) the hermetic ſeal was broken off; immediately upon which there was produced a noiſe, and the mercury in the ſhorter and cloſed leg was briſkly impelled up, by our gueſs, near three eights of an inch, and though the orifice, at which the air had acceſs, was ſcarce wide enough to admit a middle-ſized pea, yet, within a minute and half, the ſurface of the liquor being held between the eye and the candle, appeared to have acquired a very lovely and fair colour, which reached downwards a quarter of an inch; ſo that the vial ſeemed to contain two very differing liquors ſwimming on one another, and the coloration piercing deeper and deeper within five minutes in all, the whole liquor had attained a rich blue colour.]

"WITH this experiment I ſhall conclude this paper: for though I made ſeveral " other trials, with the ſame deſign, that I made the foregoing ones, as with ſpirit of " nitie, and minium, ſpirit of vinegar and copper; yet a preſent want of time hin- " ders me from troubling you with them, which I the rather forbear to do, becauſe I " fear, they would prove leſs ſatisfactory than thoſe I have ſet down, which them- " ſelves muſt, to a leſs diſcerning eye than yours, appear very imperfect, notwith- " ſtanding that prolixity in reciting ſome of them, which I was obliged to by my not " yet knowing, in ſuch odd attempts, what circumſtances might ſafely be omitted. " But ſuch as they are, I ſend them you, who, by your diffuſed correſpondency, have " great opportunity to get them made, if you think them worth it, by curious per- " ſons in ſeveral countries, various manners, and differing ſeaſons of the year: and " however the things I ſend you be but trifles, yet their novelty may, perhaps, excite " the induſtry of others, and give riſe to further enquiries."

An EXPERIMENTAL DISCOURSE

OF

QUICKSILVER growing hot with GOLD.

Firſt publiſhed in the PHILOSOPHICAL TRANSACTIONS, No. CXXII. p. 515, for *February* 21, 1675-6.

The Introduction of the Publiſher.

THOUGH the following diſcourſe was by the author of it made part of a ſhort Examen of the ſuppoſed ſympathy between gold and quickſilver, (which itſelf belongs to another treatiſe;) yet the worthineſs of the ſubject, and the great curioſity, that is obſerved among many virtuoſi, (not only chemiſts, but others,) about mercurial preparations and experiments, made me think I might do them an acceptable piece of ſervice, if I could prevail with the author to ſever them from the papers, whereto he he had annexed them, (but to which they seemed not absolutely necessary) though upon the conditions he judged requisite to insist on.

AND since I venture to impart before the time these things unto the curious, I hope and desire, they will be so equitable, as to indemnify me to the author, and not fruitlessly endeavour to put a person, that has already given so many proofs of his propensity to gratify ingenious men, upon making unseasonable answers to any verbal or epistolary questions about things, wherein some considerations, that he thinks are not to be dispensed with by him, do as yet injoin him silence.

Now, to gratify the curious among strangers, as well as those of our own nation, the publisher was not unwilling to give this discourse in Latin, as the author hath been pleased to impart it in English.

Follows the DISCOURSE itself.

Of mercury growing hot with gold.

1. —BUT that what I have hitherto said, may not be drawn to the disparagement or discouragement of those Spagyrists, that possess or aspire to the nobler arcana of gold and mercury, I must mind you to take notice, that what I have objected against the supposed sympathy of gold and quicksilver, is spoken only of common mercury, that being it, whose sympathy with gold is wont to be celebrated. And though perhaps, a good part of the things I have alledged will be found applicable even to true running mercuries; yet I would not be thought to deny, that there may be a quicksilver more subtile and penetrant than that which is common, and that those chemists, that ground the sympathy of gold and mercury upon the operations of a more philosophical mercury, may likewise argue for it more speciously, than vulgar mercury will enable them to do. And to let you see on this occasion, that I am not unkind to the chemists, I will annex part of a paper, written to a friend to give him my opinion about mercury's incalescence with gold.

2. —AND now I shall abruptly begin this section with the consideration of a problem much agitated among the curious, especially those, that pretend, whether truly or vainly, to have more than ordinary insight into chemistry: among whom I find it hotly disputed, whether or no there

De Mercurio cum auro incalescente.

—VERùM *enimverò, ne quae hactenus disserui eò torqueantur, ac si laudes animosque viris illis Spagyricis demere velimus, qui nobiliora auri & argenti vivi arcana possident ambiuntve, monendus es mihi, ut advertas, me quod contra suppositam auri & mercurii sympathiam objeci, de vulgari duntaxat mercurio dictum velle, cum ille sit, cujus cum auro sympathia celebrari sueverit. Et quamvis forté magna á me dictorum pars, consultâ experientiâ, ad nativum etiam mercurium currentem extendi possit; non tamen censere lectorem velim, negare me, dari argentum vivum posse vulgari subtilius & penetrantius, istosque chymicos, qui auri & mercurii sympathium niti volunt mercurii magis philosophici operationibus, contendere etiam pro ea multò speciosius posse, quám si vulgaris duntaxat mercurius adhibeatur. Atque ut hac occasione testatum faciam, me viros chymiae addictos neutiquam aversari; subjungam hîc scripti mei partem, ad amicum quendam idcircò exarati, ut meam ipsi de mercurii cum auro incalescentia opinionem depromerem.*

—NUNC *verò abruptè sectionem hanc ordiar problematis cujusdam discussione, quod diu multumque inter curiosos fuit agitatum, eos imprimis, qui, sive verè sive falsò obtendunt, se intimiores, quám vulgo concessum est, chymiae recessus adiisse: inter quos id calidè disceptari reperio, utrum ejusmodi detur* there be any ſuch thing, as a mercury, that will heat with gold, that is, which by being barely mingled with that metal reduced to fine parts, will, without the help of external heat, produce upon the commixture of thoſe two bodies very ſenſible heat.

tur mercurius, qui incaleſcat cum auro, id eſt, qui, dum nudè metallo iſti, ad minutas admodùm partes redacto, commiſcetur, citra externi caloris adminiculum, factâ ſolummodò duorum illorum corporum cramate, ſenſibilem valdè calorem pariat.

3. THE affirmative of this queſtion is poſitively aſſerted by ſome writers and others, that pretend to the tranſmutation of metals: for, among theſe, I have met with ſome, that aſcribe this virtue of incaleſcence with gold to the mercuries extracted, as they ſuppoſe, from ſome complete metals, which are therefore in their phraſe ſtiled *mercurii corporum*, or the mercuries of the metalline bodies.

HUJUS *quaeſtionis affirmaiivam mordicùs tenent nonnulli authores, aliique, qui metallorum tranſmutationem ſibi vendicant; inter hos quippe nonnullos videre mihi licuit, qui hanc incaleſcendi cum auro virtutem mercuriis adſcribunt, ex perfectis quibuſdam corporibus, ut autumant, elicitis; quos idcircò* mercurios, corporum, *ſive mercurios corporum metallicorum, nuncupare ſolent.*

4. BUT the negative part of the queſtion is more generally maintained, being not only embraced by far the greateſt number of philoſophers and phyſicians, but aſſented to by many of the more learned Spagyriſts themſelves, eſpecially the modern, divers of whom have reckoned this ſort of mercuries among the chimæras and *non-entia* of bragging chemiſts. And I have the leſs wondered to find many learned men ſo averſe from believing this incaleſcence of mercury and gold, becauſe, having purpoſely enquired of ſeveral prying alchemiſts, that have ſpent much labour, and many trials, to find out things of this kind, and have, of late years, travelled into many parts of *Europe*, to pry into the ſecrets of other ſeekers of metalline tranſmutations, they have apart ingenuouſly confeſſed to me, that they never actually ſaw any incaleſcent mercury, though they ſometimes heard it boaſted of by all alchemiſts, whoſe bold pretenſions had the leſs weight with me in this matter, becauſe I had long taken notice, how great a confidence, fraud, or ignorance (for I would not think all thoſe cheats, that are miſtaken,) can give to ſome of that ſort of men, that I am ſpeaking of. Inſomuch that one of them having impoſed upon an honeſt chemiſt, well known, and much employed, with a pretended incaleſcent mercury, they had the confidence to bring it

AT *negativam tuentur multò plures, iique non modò philoſophi & medici, ſed & ex ipſis Spagyricis doctrinâ clariores, imprimis, ex neotericis & modernis, quorum non pauci hanc mercuriorum familiam chimaeris & nonentibus grandiloquentium chemiſtarum accenſent. Atque eò minùs mirabar, complures viros doctos adeò eſſe ab hoc mercurii cum auro incaleſcentiae aſſenſu alienos, quia, conſultò quaeſiti à me plures ex alchymiſtis ſagacioribus, qui multum impenderant operae, plurimaque experimenta peregerant ad hujus generis arcana depromanda, quique per aliquot annos noviſſimos varias* Europæ *partes permearant, ut aliorum, qui tranſmutationes metallicas veſtigant, ſecreta rimarentur, illi, inquam, ſinguli ſeorſim à me rogati ingenue apud me faſſi ſunt, ſe revera nunquam incaleſcentem ullum mercurium vidiſſe, licèt id quandoque jactatum ab alchymiſtis audiviſſent; quorum jactabundi obtentus eò minùs apud me in hoc negotio valebant, quòd à longo jam tempore notaveram, quantam fraus vel ignorantia (non enim omnes illos haberi impoſtores velim, qui hallucinantur) in nonnullis hujuſmodi, de quibus loquor, viris fiduciam parere poſſint; quae ſanè tanta erat, ut illi, cùm eorum unus, bonae frugis chymiſtam, multis notum multiſque operam ſuam locantem, ſuppoſito mercurio incaleſcente feſellerat, eò fiduciae aſpirerentur, ut apud memet ſe ſiſterent, de experimento illo me convicturi. At, re, ut par erat, exploratâ, nullum* it me to convince me of the experiment; but upon due trial, I found not any ſenſible degree of that great heat, that was promiſed. Which miſcarriage was vainly pretended to be ſalved by I know not what unſatisfactory excuſes.

5. BUT, notwithſtanding all this, having, for the reaſons I have long ſince expreſſed in other papers (and for ſome other conſiderations, that I have not judged fit to mention) looked upon mercury as a body, which is not neceſſarily ſo homogeneous, as it is ſuppoſed, the opinion I moſt liked of was that of a poſſibility of an incaleſcent mercury. For, notwithſtanding the vulgarly ſuppoſed ſimular nature of quickſilver, which I willingly confeſs to be great enough to be admirable, it was yet congruous to my principles, that a liquor, which in weight, colour, total volatility, &c. was anſwerable to all the eſſential properties for which a body is called mercury, might yet have an internal conſtitution of parts, that might make it in ſome unobſerved things conſiderably differing from common mercury. And, among theſe differing qualites, I did not know but one might well be, that of growing hot with gold. And this opinion I judged the more reaſonable, becauſe, having deviſed two ways (unpractiſed, that I know of, by any chymiſt) the one, to diſcover, whether a clean and carefully diſtilled mercury might not be a compounded body, and have in it parts, that were not mercurial; and the other, out of ſuch a fine diſtilled mercury to ſeparate parts, and that in no deſpicable number, that are plainly heterogeneous; I found, upon trial, that both the methods I had thought on would ſucceed, which warranted me to think it poſſible, that a mercury very fine and clean, and even purged by ſublimations and diſtillations, may, by art, have been made to aſſume and incorporate with it a multitude of heterogeneous corpuſcles, not to be diſcovered, much leſs ſeparated, (as thoſe of tin, lead, &c. may be) but by a ſkilful artiſt.

nullum percepi ſenſibilem illius caloris gradum, quem promiſerant.

VERÙM *enimverò, his omnibus nequicquam obſtantibus, cùm ex rationibus dudum in alio ſcripto à me expoſitis, aliiſque de cauſis hìc non memorandis, argentum vivum corpus reputem, quod non neceſſariò tam ſit homogeneum, ac paſſim habetur; illa mihi opinio prae caeteris allubuit, quae mercurii incaleſcentis poſſibilitatem adſtruit. Etenim, non obſtante vulgò ſuppoſitâ mercurii (ut ſic dicam) ſimilaritate, quàm adeò eximiam eſſe puto, ut parere admirationem poſſit, meis tamen principiis conſonum erat, liquorem quendam, qui pondere, colore, totali volatilitate, &c. omnes referebat proprietates eſſentiales, quarum reſpectu corpus aliquod mercurii nomine venit, habere tamen poſſe internam ejuſmodi partium diatheſin, quae in nonnullis hactenus non obſervatis inſignem illi à mercurio vulgari diſcrepantiam conciliare queat: atque has inter qualitates differentes neſciebam, annon ea recenſeri meritò poſſet, quâ incaleſcit cum auro commixtus. Atque hanc opinionem rationi eò magis conſentaneam arbitrabar, quòd, excogitatâ à me duplici methodo (hactenus à chymicorum nullo, quòd ſciam, in praxin verſâ,) unâ quidem, ut manifeſtum redderem, eſſetne purus curatéque diſtillatus mercurius corpus compoſitum, parteſque contineret non mercuriales; alterâ verò, ex purificato ejuſmodi & diſtillato mercurio partes ſeparandi non paucas manifeſtò heterogeneas; experiundo comperi, utramque illam methodum à me inventam ſucceſſu gaudere: id quod auctoramentum mihi haud leve erat, ut poſſibile exiſtimarem, mercurium valdè defaecatum, quin & per ſublimationes & diſtillationes repurgatum, arte poſſe eò redigi, ut aſſumat ſecúmque conflet heterogeneorum corpuſculorum multitudinem, quae nonniſi à perito artis filio detegi, multò minus ſegregari queant (ut fieri de ſtanneis, plumbeis, &c. corpuſculis poteſt.)*

6. Hoc

6. THIS, in the general, may ſuffice to make me ſuſpend my judgment about the problem formerly propoſed, and to engage me to make trials, whether ſome of theſe heterogeneous particles, that I found reducible with mercury into a laſting mercurial flux, might not ſo alter it, as to diſpoſe it to heat with gold. But this was not ſufficient to determine me to an aſſent; for to oblige me to admit incaleſcent mercuries, it ought not to ſuffice, that it is poſſible, or even probable, that there may be ſuch, but there was neceſſary ſome poſitive proof, that there are ſuch; and that alſo, through God's bleſſing, my trials afforded me about the year 1652.

7. SOME years after I was in poſſeſſion of this mercury, I found in ſome of their books, that chemiſts call philoſophers, ſome dark paſſages, whence I then gueſſed their knowledge of it, or of ſome other very like it; and in one of them I found, though not all in the very ſame place, an allegorical deſcription of it, the greateſt part of which was not very difficult for me to underſtand; but not finding there any notice taken of the property of this mercury to grow hot with gold, I was induced to ſuſpect, that either they had not the knowledge of it, or judged it unfit to be ſpoken of. But you will, I ſuppoſe, expect from me rather narratives than conjectures. And, indeed, it is but reaſonable, that, having but mentioned to you a phænomenon, whoſe credibility is by many denied, I ſhould take notice of ſome circumſtances fit to bring credit to it. And I ſhall the leſs grudge the pains of ſetting down ſeveral particular phænomena, becauſe I preſume you have not met with them, and becauſe alſo it may gratify ſome of your chemical friends, who may have or diſcover ſome noble mercury, by helping them to examine it, and to try, whether it reſembles ours.

8. THAT I might not then be impoſed on by others, I ſeveral times made trial of our mercury, when I was all alone. For when no body was by me, nor probably dreamed of what I was doing, I took to one

Hoc *generatim ſuffecerit, ut meum de problemate ſuprà propoſito judicium ſuſpendam, & ad experimenta ſumenda properem, quibus palàm reddatur, annon aliquae ex particulis illis heterogeneis, quas cum mercurio in durabilem fluxum mercurialem reduci poſſe deprehendi, ita alterare eum poſſint, ut ad incaleſcendum cum auro ipſum diſponant. At non erat hoc ſatis ad eliciendum à me aſſenſum; ut enim ad mercurios incaleſcentes admittendum adducerer, ſufficere non debebat, poſſibiles eos eſſe, vel etiam probabiles, ſed reverâ tales dari manifeſtâ probatione erat evincendum: & hoc ipſum quoque, favente Deo, experimenta mea, anno* 1652. *circiter, comprobarunt.*

POST *aliquot ab eo tempore annos, quo mercurium hujuſmodi jam poſſidebam, in quibuſdam ex eorum, quos turba chymica philoſophos nuncupat, libris obſcura quaedam loca inveniebam, unde tunc eorum de ipſo, vel alio aliquo perquam ei ſimili, cognitionem conjectabam; atque in ipſorum uno reperiebam (non tamen rem totam in uno planè eodemque loco) deſcriptionem ejus allegoricam, cujus pars maxima adeò difficilis intellectu mihi non erat: at cùm nihil ibi notatum viderem de illa mercurii hujus proprietate, quâ calorem cum auro acquirit, in ſuſpicionem incidi, eos vel cognitione illius fuiſſe deſtitutos, vel eam ſilentio premendam cenſuiſſe. At tu ſine dubio facti potiùs narrationes, quàm conjecturas à me exſpectas. Et ſanè aequum omninò fuerit, ut, cùm mentionem duntaxat fecerim phaenomeni, cujus à multis negatur credibilitas, circumſtantias nonnullas annotem, quae fidem ei conciliare valeant. Atque eò minùs laborem detrectabo particularia aliquot phaenomena hîc tradendi, tum quòd ea tibi non occurriſſe autumem, tum quòd ea grata fore putem quibuſdam amicis tuis chymicis, nobilem quendam vel jam poſſidentibus vel paraturis mercurium, ut ſcil. hoc qualicunque ſcripto noſtro ad eum examinandum, &, an referat noſtrum, experiundum, juventur.*

ITAQUE, *ne mihi imponerent alii, pluries mercurium noſtrum, quando ſolus eram, explorabam. Etenim quando nemo mihi aderat, neque quiſquam per ſomnium quid agerem conjiceret, ſumebam unam partem illius mercurii,* ad one part of the mercury, ſometimes half the weight and ſometimes an equal weight of refined gold reduced to a calx or ſubtle powder. This I put into the palm of my left hand, and putting the mercury upon it, ſtirred it and preſſed it a little with the finger of my right hand, by which the two ingredients were eaſily mingled, and grew not only ſenſibly, but conſiderably hot, and that ſo nimbly, that the incaleſcence did ſometimes come to its height in about a minute of an hour by a minute-clock. I found the experiment ſucceed, whether I took altogether, or but half as much gold as mercury; but the effect ſeemed to be much greater when they were employed in equal weight. And, to obviate a ſuſpicion, which, though improbable, might poſſibly ariſe, as if the immediate contact of the ingredients and the ſkin produced a ſenſe of heat, which was not due to the action of the metals upon one another; I had the curioſity to keep the mixture in a paper, and found not its interpoſition to hinder me from feeling the incaleſcence, though it much abated the degree of my ſenſe of it.

9. I tried alſo the ſame mercury with refined ſilver reduced to a very fine powder; but I could not perceive any heat or warmth at all; though, I am apt to think, that if I had had a ſufficient quantity of leaf-ſilver to have made the experiment with, I ſhould, after ſometime, have produced an incaleſcence, though much inferior to what the ſame quantity of mercury would produce with gold; but this only upon the by. I ſhall now add, that that to the end I might not be thought to impoſe upon myſelf, I did not make trial in my own hand, when it was in different tempers, as to heat and cold, but I did it in the hands of others, who were not a little ſurpriſed and pleaſed at the event. And this I did more than once or twice; by which means I had, and ſtill have, divers witneſſes of the truth of the experiment, whereof ſome are noted perſons, and eſpecially him, to whom I laſt ſhewed it,

ad auri, in calcem vel pollinem redacti, pondus quandoque dimidium, quandoque aequale. Hoc polline volae manus ſiniſtrae immiſſo, & mercurio ſuperinfuſo, utrumque ſimul agitabam, premebamque nonnihil digito manûs dextrae; quâ ratione duo haec ingredientia facilè commixta, non modò ad ſenſum ſed inſigniter incaleſcebant, idque adeò properè ut incaleſcentia interdum unius horae circiter minuto, indicante idipſum horologio minutis inſtructo, ad ἀκμὲν *perveniret. Succedebat hoc experimentum, ſive aequalem ſumerem ſive dimidiam auri quantitatem; effectus tamen multò videbatur inſignior, quando aequali pondere adhibebantur. Atque, ut ſuſpicioni, quae, licèt improbabilis, ſubnaſci tamen poſſet, occurrerem, immediatum ſcil. ingredientium & cutis contactum producere poſſe ſenſum caloris, qui non debeatur metallorum in ſe invicem actioni, curioſitate ducebar mixturam hanc in charta ſervandi; quo facto, interpoſitionem ejus nequaquam impedire incaleſcentiae ſenſum comperiebam, quanquam, ex natura rei, intenſiorem illius gradum remitteret.*

PORRÒ *mercurium eundem cum repurgato argento ad ſubtilem valdè pulverem redacto, exploravi; at nullum omnino calorem percipere potui; quanquam eò ferar, ut exiſtimem, ſi ſufficiens argenti foliati quantitas, ad peragendum experimentum, mihi ſuppetiiſſet, me poſt aliquot temporis ſpatium incaleſcentiam ſuſcitaturum fuiſſe, quamvis multò inferiorem eo, quem eadem mercurii quantitas cum auro produceret: at hoc nonniſi in tranſitu. Adjiciam nunc, me, ne mihimet impoſuiſſe cenſerer, non tantùm rem hanc exploraſſe in manu mea, quando variè erat pro caloris & frigoris ratione temperata, ſed & in manibus aliorum, quos non parùm attonitos habebat, juvabatque eventus. Atque hoc ipſum pluries quàm ſemel biſve feci; unde mihi teſtes ſuppetunt experimenti veritatis aſſertores, probatae fidei viri, quorum unus erat eruditus Societatis Regiae ſecretarius, quem, exhibitis ei ingredientibus, rogabam, ut ſuiſmet manibus experimentum caperet;*

it, which you will eafily believe, when I tell you it is the learned fecretary of the Royal Society; to whom having given the ingredients, I defired him to make the experiment in and with his own hands, in which it proved fuccefsful within fomewhat lefs than a minute of an hour *.

10. AND that, which makes this incalefcence the more confiderable is, that being willing to hufband my mercury, a great part of which had been, as I gueffed, ftolen from me before I employed it, I made thefe trials but with a drachm at a time, which fcarce amounts in quantity to the bignefs of half a middle-fized bean; whereas, if I could have made the experiment with a fpoonful or two of quickfilver, and a due proportion of gold, it is probable the heat would have been intenfe enough, not only to burn one's hand, but perchance to crack a glafs vial; fince I have fometimes had of this mercury fo fubtle, that when I employed but a drachm at a time, the heat made me willing to put it haftily out of my hand.

11. THESE things being matters of fact, I fcruple not to deliver them; but I would much fcruple to determine thence, whether thofe, that are *mercurii corporum*, and were made, as chemifts prefume, by extraction only from metals and minerals, will each of them grow hot with gold, as, if I much miftake not, I found antimonial mercury to do. And much lefs would I affirm, that every metalline mercury (though never fo difpofed to incalefcence) or even that of filver or gold itfelf, is the fame with that, which the chryfopæan writers mean by their philofophic mercury, or is near fo noble as this. Nay, I would not fo much as affirm, that every mercury, obtained by extraction even from the perfect metals themfelves, muft needs be more noble and fit (as alchemifts fpeak) for the philofophic work, than that, which may with fkill and pains be at length obtained

peret; in quibus & optatum fucceffum minori quàm unius minuti fpatio fortiebatur †.

ATQUE, *quod incalefcentiam hanc infigniorem reddit, eft, quòd, cùm parcè uti mercurio meo cuperem, quippe cujus magna pars (ut conjicio) furrepta mihi fuerat, priufquam eum adhiberem, experimenta fingula nonnifi cum una drachma peragebam, quae vix fabae mediocris dimidiae magnitudinem aequat, cùm fi copia mihi fuiffet capiendi experimentum cum cochleari uno alteróve mercurii pleno, fupparique quantitate auri, probabile fit, calorem inde oriturum fuiffe fatis intenfum, ut non modò ureret manum, fed forfan & in phiala vitrea rimas ageret; quandoquidem interdum hujus generis mercurium habui adeò fubtilem, ut, adhibente me fingulis vicibus nonnifi drachmam unam, calor me adegerit, ut properè è manibus mixturam deponerem.*

HÆC, *cùm fint res facti, tradere non dubito; at valdè ambigerem exinde determinare, num, qui appellantur mercurii corporum, paranturque, ut jactant chymici, fola extractione ex metallis & foffilibus, eorum quilibet calorem acquirat cum auro, quemadmodum, ni multum fallor, mercurium antimonialem acquirere comperi. Multóque minus affirmarem, quemvis mercurium metallicum (quantumcumque ad incalefcentiam difpofitum,) quin & mercurium argenti aurive ipfius eundem effe cum eo, quem fcriptores chryfopaei per mercurium fuum philofophicum intelligunt, vel praeftantiâ fuâ ad hunc accedere. Quin imò, ne quidem affererem, quemlibet mercurium, extractione etiam ab ipfis perfectis metallis impetratum, nobiliorem effe oportere, & (ut loquuntur alchymiftae) ad philofophicam operationem magis idoneum, quàm illum, qui, peritiâ & induftriâ comite, obtineri tandem poteft à mercurio vulgari, à partibus fuis recre-*

* Since this was written, the noble and judicious prefident of the Royal Society, the lord vifcount *Brouncker*, made the fame experiment with fome of the fame mercury, in his own hand with good fuccefs.

† *Ex quo tempore hoc literis fuit confignatum, illuftriffimus & judiciofiffimus Regiae Societatis praefes, Dom. Vicecomes* Brouncker, *idem experimentum fuâ cum ejufdem mercurii portione manu cum fucceffu peregit.*

tained from common mercury ſkilfully freed from its recrementitious and heterogeneous parts, and richly impregnated with the ſubtle and active ones of congruous metals or minerals. Theſe and the like points I ſhould, as I was ſaying, much ſcruple at offering to determine in this place, where what I deſigned to deliver was hiſtorical, though I have not thought it impertinent to glance at the points lately mentioned, becauſe thoſe glances may intimate things conducive to the better underſtanding of what I have ſaid, and have to ſay in this paper.

recrementitiis heterogeneiſq purgato; ſubtilibuſque & efficacibus metallorum mineraliumve congruorum partibus uberrime faeto: haec, inquam, & ſimilia hoc loco affirmare admodum vererer; cùm hîc nonniſi ea tradere inſtituerim, quae ad rei hiſtoriam faciunt; quanquam praeter rem non exiſtimaverim, jamjam indigitatos rei hujus apices innuere, quòd ſtricturae iſtae ea poſſint lectori ingerere, quae ad meliorem tum dictorum tum dicendorum intelligentiam conducere queant.

12. I doubt not but what I have related and hinted has given you a curioſity to know ſomewhat further of this mercury: and I confeſs, that if there be any truth in what ſome of the moſt approved Spagyriſts have delivered about a ſolvent of gold, that ſeems of kin, and perhaps is not much nobler than one, that I had; it ſeems allowable to expect, that even ours ſhould be of more than ordinary uſe, both in phyſick and alchemy. But the misfortune I had to have loſt a conſiderable quantity of it, being afterwards increaſed by the almoſt ſudden death of the only operator I truſted in the making of it; I was altogether diſcouraged from repeating ſuch a troubleſome preparation, eſpecially being diverted by buſineſs, removes, ſickneſs, and more pleaſing ſtudies. And though I have not forgot ſome not deſpicable trials, that I made with our mercury, yet ſince they are not neceſſary to the queſtion, that occaſioned this paper, I ſhall paſs them over in ſilence, and only obſerve ſome few things I had almoſt forgot to tell you; namely, firſt, that whereas it is uſual to take four, five, or ſix, nay eight or ten parts of common quickſilver, to make an amalgame with one of gold, even when both are heated by the fire; I found our mercury ſo congruous to that metal, that it would preſently embody with no leſs than an equal weight of it, and produce a pretty hard amalgame or mixture, in which the mercury was ſo diffuſed, that the gold had quite loſt its colour. Secondly, I ſhall add

NON *dubito, quin hactenus à me enarrata indigitataque curioſitatem in te pepererint, aliquid amplius de hoc mercurio cognoſcendi: & fateor, ſi quid veri ſubeſt ei, quod quidam ex probatiſſimis ſpagyricis de quodam auri diſſolvente, quod affine videtur noſtro, nec eo fortè multò eſt nobilius, tradiderunt; exſpectare fas fuerit, ipſiſſimum hoc noſtrum in inſignem, cum in medicina, tum in alchymia, uſum cedere poſſe. Verùm cùm infortunium illud, quo inſigniori quantitate ejus ſui privatus, ſtipatum fuerit ſubitâ morte operatoris unici, cui in eo parando penitùs fidebam, mentem planè alienam ab iteranda tam moleſta praeparatione ſenſi; maximè cùm occupationes, migrationes, adverſa valetudo, ſtudiaque gratiora aliorſùm me traherent; & licèt experimenta quaedam non ſpernenda, cum mercurio noſtro peracta, memoriâ mea non exciderint; cùm tamen ad quaeſtionem illam, quae ſcriptum hoc peperit, non ſint neceſſaria, ſilentio ea involvam, paucula duntaxat annotaturus, quae commemorare propemodum fuiſſem oblitus. Quorum primum eſt, quòd, cùm ſolenne ſit capere mercurii vulgaris partes quatuor, 5 vel 6, imò 8 vel 10, ad amalgama faciendum cum una parte auri, etiam tum, quando utrumque incaluit igne; ego adeò congruum deprehenderim cum metallo illo mercurium noſtrum, ut non minus quàm aequale illius pondus intimè ſtatim pervaderet, ſatiſque durum amalgama cramâve produceret, in quo adeò diffuſus erat mercurius, ut aurum colorem ſuum penitus amitteret. Secundum eſt, (quod hactenus obſervatum fuiſſe haud putem,) vim ſcil. hancce, aurum* add what, for aught I know, has not been yet obſerved, that this power of penetrating gold and growing hot with it, is ſo inherent, not to ſay radicated, in our mercury, that after it had been diſtilled from gold again and again, I found it to retain that property. And, laſtly, whereas it may be ſuſpected, that this faculty may be quickly loſt, (as that of the prepared Bononian ſtone to receive light, has been complained of as not durable) I found by trial, that a ſingle drachm of mercury, made after a certain manner, did, the third or fourth year after I had laid it by, grow ſo hot with gold, that I feared it would have burnt my hand.

THUS far the author to his friend: but when he ſent me the paper, he accompanied it with the following lines;

13. I have little at preſent to ſay to you about the papers, which this ſheet accompanies, ſave that one of the chief reaſons, that makes me backward to have the foregoing obſervations communicated to the curious, is, that I fear, we may thereby procure divers queries and perhaps requeſts, (relating to this mercury) which I would by all means avoid, for divers reaſons, and particularly for this, that a great weakneſs of that part diſables me to write with my own hand, and I know, you will not think it fit I ſhould, about ſuch a ſubject, employ that of an amanuenſis. And therefore I cannot conſent, this paper ſhould go out of your hands, unleſs you can think on ſome likely courſe to ſecure me from trouble, and from the unwelcome neceſſity of diſobliging ſome, whilſt I endeavour to gratify others. If this precaution be uſed, I may ſafely learn, by means of your diffuſed acquaintance, what thoſe, that are ſkilful and judicious enough to deſerve to be much conſidered in ſuch an affair, will think of our mercury, and whether, in caſe they have an eſteem of it approaching to that of divers eminent chemiſts (ſome of which importune me to impart it;) they judge the good, that the preparations of it (ſuch as precipitats and turbiths of divers kinds, mercurius dul-

aurum penetrandi, cûmque eo incaleſcendi, mordicùs adeò inhaerere mercurio noſtro, ne dicam ita in eo radicatam eſſe, ut poſtquam iterum atque iterum ab auro eſſet diſtillatus, proprietatis illius tenacem eum deprehenderim. Et denique, cùm ſuſpicio inceſſere lectorem poſſit, facultatem hanc citò deperdi, (ut de praeparato ad hauriendam lucem lapide Bononienſi queruntur authores) experiundo didici, unicam drachmam mercurii, certo modo parati, poſt tertium quartúmve à quo ſepoſuerum annum adeò cum auro incaluiſſe, ut ne adureret manum meam, timerem.

HACTENUS *author noſter ad amicum ſuum: ſed cùm mihi chartas illas mitteret, voluit eas ſequenti mantiſſâ locupletare;*

NON *diu te morabor diſſerendo de chartis hîc junctis: dicam ſolummodò, unam ex praecipuis rationibus, quae in vulgandis praegreſſis obſervationibus cunctabundum me faciunt, hanc eſſe, quòd vereor, nos hoc ipſo variis circa mercurium hunc quaeſtionibus & fortè ſollicitationibus anſam daturos, quas omni ſtudio praecavere velim, cum ob alias, tum hanc ob cauſam, quòd magna manuum mearum debilitas me impedit, quò minus meamet manu id conſignarè literis valeam, quod conſcribi amanuenſis operâ conſultum haud judicaveris. Proindéque concedere haud poſſum, ſcriptum hoc è manibus tuis dimitti, niſi rationem ſuggeras probabilem, quâ ſecurum me praeſtes à moleſtia, atque ab ingrata neceſſitate repulſam dandi nonnullis, dum aliis obſecundare ſtudeo, Hac cautelâ ſi utaris, potero ampliſſimae tuae conſuetudinis beneficio citra moleſtiam edoceri, quid ii, qui tantâ peritiâ tantoque judicio valent, ut in hoc negotio magni fieri mereantur, de mercurio noſtro ſentiant; adhaec utrum, ſi aeſtimationem de eo foveant illi ſupparem, quam praecellentium chymicorum complures (quorum nonnulli me urgent ad eum communicandum) prae ſe ferunt, veriſimile cenſeant, utilitatem, quam praeparationes ipſius (cuju modi ſunt praecipitata & turbithi diverſorum generum, mercurius dulcis, cinnabaris ex antimonio & auro cum parata, &c.) afferre poſſint rei* cis, cinaber made of the sulphur of antimony, and with gold, &c.) may do in physick, is likely much to exceed the political inconveniencies, that may ensue, if it should prove to be of the best kind, and fall in ill hands. The knowledge of the opinions of the wise and skilful about this case will be requisite to assist me to take right measures in an affair of this nature. And, till I receive this information, I am obliged to silence.

14. ONLY, in the mean while, I shall, for the sake of the enquiries into the mercurial arcana, make bold to add a secret, which, I think, will to divers philalethists and other students of the chemical philosophers books seem a paradox, if not an untruth; namely, that a mercury, qualified to heat with gold, and perhaps with other powders, may be made by more ways than one or two; experience having assured me (whatever authorities or theories may be urged to the contrary) that such a mercury may be (I say not, easily or speedily, but successfully) prepared, not only by employing antimony and solid metals, as mars, but without any such metal at all, or so much as antimony itself.

15. HERE I purposed to conclude: but, because I am, as you know, very averse (which I declare myself to be on this occasion also) from making any promise to the publick, I think fit in this place to give you an advertisement, and obviate a scruple. I shall therefore admonish those inquisitive Spagyrists, that may be desirous to try, whether their purified mercury be incalescent, that they be not too hasty to conclude it is not so; nor to reject it, unless they have made the trial with gold duly prepared. For I have found, that my mercury did not grow hot with the smallest filings of gold I could make (though indeed within a few hours after it did, without the help of fire, imbody with it into a hard amalgama,) which argued, that the corpuscles of the metal were not yet small enough to be suddenly penetrated by the quicksilver: nor will every calx of gold serve our turn, as I have found by employing,

medicæ, longè superaturum esse incommoda illa politica, quae nascitura forent, si fortè de praestantissima esset indole, atque in maleferiatas manus incideret. Sapientum & peritorum hoc in casu opiniones cognoscere, necessarium mihi fuerit, ut recto tramite in istiusmodi negotio incedere mihi detur. Atque, donec edoctus id fuero, silentii sacra colere teneor.

INTERIM *in eorum gratiam, qui arcana mercurialia scrutantur, subjungere ausim secretum aliquod, quod philalethis compluribus, aliisque, qui chymicorum philosophorum libris meditandis incumbent, paradoxum, quin & falsum fortè videbitur: mercurium scil. ad incalescendum cum auro aliisve pulveribus idoneum, modis uno binóve pluribus parari posse; cùm per experientiam certò mihi constet, (quicquid in contrarium obtendant authoritates & theoriae) talem mercurium posse, (non dicam facilè properéve, sed cum successu) parari, non modò antimonium solidòque metalla, putà martem, &c. adhibendo, sed citra ullius omnino metalli, quin vel ipsius antimonii, usum.*

Hîc *statueram finem huic sermoni imponere: at cùm aegerrimè, ut nosti, tum aliàs, tum hac imprimis occasione, promissi fidem publico obstringam, visum est mihi hoc loco monitum aliquod suggerere, & scrupulo cuidam obviam ire. Prius quod attinet, curiosos illos Spagyricos, quos fortè tentandi cupido incesserit, sitne purgatus ipsorum mercurius incalescendi qualitate instructus, monebo, ne nimis festinantur concludant ipsum eâ praeditum non esse, néve eum rejiciant, nisi experimentum fecerint cum auro ritè praeparato. Comperi quippe, mercurium meum non incalescere cum ramentis auri, omnium quas conficere poteram minimis, (quanquam reverâ intra paucas exinde horas, sine ignis adminiculo, cum ipso in durum amalgama conflaretur;) quod argumento erat, metalli illius corpuscula necdum exigua satis fuisse, ut properè à mercurio penetrarentur: neque quaevis auri calx rem nostram conficiet; ut comperi, dum perquàm subtilem spongiosamque calcem, modo non vulgari* employing, without fuccefs, a very fine and fpongy calx made after an uncommon way, the golden particles having, as it feemed, fome extremely fine, though unobferved duft of the additament fticking to them, which hindered the adhæfion of the mercurial ones. Now, the calx of gold, that I moft ufed, as finding it ftill to do well, was that made by quartation*, as alchemifts call it. But becaufe it is not fo eafy, as even chemifts, that have not tried, imagine, to make good calces of gold, and that in the way newly mentioned, there needs fufion of gold and of filver (for which many chemifts want conveniencies,) and they are often impofed on by common refiners, who here ufually fell in wires fuch filver for fine (which indeed it is comparitively,) as I have found not to be without mixture; I fhall add, that by making an amalgama, the common way, with pure gold and vulgar mercury, and diffolving the mercury in good aqua-fortis, there will remain a powder, which, being well wafhed with fair water to dulcify it, and kept a while in a moderate fire to dry it thoroughly without melting it, will become a calx, which I have more than once ufed with our mercury with good fuccefs. It is true, both in this way and in that (by quartation) aqua-fortis, which is a corrofive liquor, is employed to bring the gold to powder, and therefore in a diffident mind fome fufpicion may arife, that the incalefcence may proceed only from the action of the acid particles of the menftruum, which yet adhering to the corpufcles of the gold works upon the quickfilver, as aqua-fortis is known to do: but, to omit thofe anfwers, that cannot be given in few words, after I have taken notice, that, if the effect depends not on our mercury (as prepared) but only on the calx, it appears not, why this fhould not grow hot with common mercury, as well as with ours; I fhall need to add, for the removal of

* That is, by melting together one part of fine gold, and three or four parts of cuppelled filver, and then putting the mafs, wherein the metals are mixed, almoft *per minima*, into purified aqua-fortis, which diffolving the filver only, leaves the gold in the form of a fine calx.

gari paratam, citra fucceffum adhibui, in qua, ut videtur, apprimè tenuis fenfumque fugiens additamenti pulvis adhaerebat particulis aureis, & mercurialium adhaefionem praepediebat. Jam verò calx auri, quâ plerumque utebar fucceffu ejus inductus, illa erat, quae quartationis† (ut vocant) beneficio paratur. At quia non adeò facilè eft, ut ipfi chymici, qui manum operi non admoverunt, fibi imaginantur, bonae notae calces auri parare, cumque in methodo jamjam memorata requiratur fufio auri & argenti (cujus peragendae commoditate non pauci chymici deftituuntur,) cùm etiam crebrò à vulgaribus metallorum purgatoribus fallantur, qui hîc paffim, filorum formâ, ejufmodi argentum pro puro venditant (quale, comparatè loquendo, reverâ eft,) quod non effe mixturae expers deprehendi; adjiciam, quòd, dum communi more amalgama conficitur cum auro puro & mercurio vulgari, mercuriufque diffolvitur bonâ aquâ forti, remanfurus fit pulvis, qui cum aqua pura, ad conciliandam ei, quam vocant, dulcedinem, probè elotus, & aliquandiu in temperato igne, ad eum penitus exiccandum citra fufionem, affervatus, talem calcem praebebit, quâ pluries cum mercurio noftro feliciter ufus fui. Fateor equidem, tum in hac methodo, tum in illa, quae inftituitur per quartationem, adhiberi aquam fortem, liquorem fcil. corrofivum, ad aurum in pulverem redigendum, unde fcrutanti genio fuboriri fufpicio poterit, incalefcentiam illam foli actioni acidarum particularum menftrui acceptam ferendam effe, quod haerens etiamnum auri corpufculis, in mercurium operetur, folenni aquae fortis more. Verùm, (ut eas refponfiones fileam, quae paucis tradi non poffunt,) poftquam notavi, quòd, fi effectus hic non dependet à mercurio noftro (ritò praeparato,) fed à fola calce, non pateat, quare haec non incalefcat aequè cum mercurio vulgari ac noftro; opus haud fuerit, aliud quicquam an fcrupulum hunc eximendum, quàm obviam hoc experimentum, quod fequiter, quodque bis térve à me paractum fuit, adjicere: fumpfi,

† *Hoc eft, per fufionem conflando unam partem auri puri, & tres quatuórve partes argenti cupellati, ut vocant, & tunc immittendo maffam, in qua metalla mifcentur quafi per minima, in purgatam aquam fortem, quae folum argentum diffolvens, aurum in forma calcis relinquit.*

of this subtile scruple, no more than this plain experiment, (which I twice or thrice made,) namely, that taking, instead of a calx of gold, a competent number of leaves of gold, such as book-binders and the apothecaries use, this gold, that was without the help of salts reduced by beating to a sufficient thinness (insomuch that seventy odd leaves did not weigh a scruple,) I found (more than once) upon putting two or three times the weight of our mercury to them, that a smart heat was presently produced in my hand.

sumpsi, inquam, calcis auri loco, sufficientem numerum foliorum auri, qualibus utuntur bibliopegi & aurifabri; hoc aurum, quod citra salium opem tundendo redactum erat ad tenuitatem sufficientem (adeò ut ultra septuaginta folia vix unius scrupuli pondus aequarent) hoc, inquam, aurum comperi (unâ vice pluries,) cùm binum trinumve mercurii nostri pondus ipsi commiscerem, insignem in manu mea calorem mox peperisse.

EXPERIMENTS, NOTES, &c. about the mechanical Origin or Production of divers particular QUALITIES: Among which is inserted, a Discourse of the Imperfection of the Chemist's Doctrine of QUALITIES; together with some Reflections upon the Hypothesis of ALKALI and ACIDUM.

The Publisher to the Reader.

TO keep the reader from being at all surprized at the date of the title-page, I must inform him, that a good part of the ensuing tracts were printed off, and in my custody the last year; and the rest had come out with them divers months ago, if the noble author had not been hindered from committing them to the press by the desire and hope of being able in a short time to send them abroad more numerous, and by his being hindered to do so, partly by remove, partly by the want of some papers, that were oddly lost or spoiled, and partly by the sickness of himself, and divers of his near relations. And some of these impediments do yet suppress what the author intended should have made a part of the book, which now he suffers to be published without them, though divers of his papers about some other particular qualities have been written so long ago, as to have lain for many years neglected among other of his old writings: which that he may have both leisure and health to review and fit for publication, is the ardent wish of the sincere lovers of real knowledge, who have reason to look on it as no mean proof of his constant kindness to experimental philosophy, that in these tracts he perseveres in his course of freely and candidly communicating his experiments and observations to the publick, notwithstanding the liberty, that hath been too boldly taken to mention them as their own by later writers; as particularly by the compiler of the treatise, entitled *Polygraphice*, who in two chapters hath allowed himself to present his reader with above fifty experiments, taken out of our author's book of colours, without owning any of them to him, or so much as naming him or his book in either of those chapters, nor, that I remember, in any of the others. Nor did I think this practice justified by the confession made in the preface, importing, that the compiler had taken the particulars he delivered from the writings of others. For this general

general and perfunctory acknowledgement neither doth right to particular authors, nor, by naming them, enables the reader to know, whether the things delivered come from persons fit to be credited or not; and therefore, since it is but too likely, that such concealment of the names, if not usurpation of the labours of the benefactors to philosophy, will prove much more forbidding to many others to impart their experiments, than as yet they have to our generous author; it seems to be the interest of the commonwealth of learning openly to discountenance so discouraging a practice, and to shew, that they do not think it fit, that possessors of useful pieces of knowledge should be strongly tempted to envy them to the publick, to the end only that a few compilers should not be put upon so reasonable and easy a work, as by a few words or names to shew themselves just, if not grateful.

BUT not to keep the reader any longer from the perusal of these tracts themselves, I shall conclude with intimating only, that what our author saith in one of them concerning the insufficiency of the chemical hypothesis for explaining the effects of nature, is not at all intended by him to derogate from the sober professors of chemistry, or to discourage them from useful chemical operations; forasmuch as I had the satisfaction, some years since, to see in the author's hands a discourse of his about the Usefulness of Chemistry for the Advancement of Natural Philosophy; with which also it is hoped he will ere long gratify the publick.

ADVERTISEMENTS relating to the following TREATISE.

TO obviate some misapprehensions, that may arise concerning the ensuing notes about particular qualities, it may not be improper to add something in this place to what has been said in another paper * in reference to those notes, and consequently to premise to the particular experiments some few general advertisements about them.

AND I. we may consider, that there may be three differing ways of treating historically of particular qualities. For either one may in a full and methodical history prosecute the phænomena; or one may make a collection of various experiments and observations, whence may be gathered divers phænomena to illustrate several, but not all of the heads or parts of such an ample or methodical history; or, in the third place, one may in a more confined way content one's self to deliver such experiments and observations of the productions, or the destruction or change of this or that quality, as being duly reasoned on, may suffice to shew, wherein the nature of that quality doth consist, especially in opposition to those erroneous conceits, that have been entertained about it. Of the first of these three ways of treating of a quality I pretend not to have given any complete example; but you will find, that I have begun such histories in my specimens about fluidity and firmness, and in the experiments, observations, &c. that I have put together about cold. The second sort of historical writings I have given an instance of in my experiments about colours; but in these ensuing notes, the occasion I had to make them, having obliged me chiefly to have an eye to the disproval of the errors of the peripateticks and the chemists about them, I hope I shall not be thought to have fallen very short in my attempt, if I have (here and there) performed, what may be required in the third way of writing historically of a quality; my present design being chiefly to give an intelligent and historical account of the possible mechanical origination, not of the various phænomena of the particular qualities succinctly mentioned in these notes; though my secondary end being to become a benefactor to the history

* See Tracts about Cosmical Qualities, &c. to which is prefixed an Introduction to the History of Particular Qualities.

hiſtory of qualities by providing materials for myſelf or better architects, I have not ſcrupled to add to thoſe, that tend more directly to diſcover the nature or eſſence of the quality treated of, and to derive it from mechanical principles, ſome others (which happened to come in my way) that acquaint us but with ſome of the leſs luciferous phænomena.

II. THAT you may not miſtake what is driven at in many of the experiments and reaſonings delivered or propoſed in the enſuing notes about particular qualities, I muſt deſire you to take notice with me, what it is, that I pretend to offer you ſome proofs of. For if I took upon me to demonſtrate, that the qualities of bodies cannot proceed from (what the ſchools call) ſubſtantial forms, or from any other cauſes but mechanical, it might be reaſonably enough expected, that my argument ſhould directly exclude them all. But ſince, in my explications of qualities, I pretend only, that they may be explicated by mechanical principles, without enquiring, whether they are explicable by any other; that, which I need to prove, is, not that mechanical principles are the neceſſary and only things, whereby qualities may be explained, but that probably they will be found ſufficient for their explication. And ſince theſe are confeſſedly more manifeſt and more intelligible, than ſubſtantial forms and other ſcholaſtic entities (if I may ſo call them) it is obvious, what the conſequence will be of our not being obliged to have recourſe to things, whoſe exiſtence is very diſputable, and their nature very obſcure.

THERE are ſeveral ways, that may be employed, ſome on one occaſion, and ſome on another, either more directly to reduce qualities (as well as divers other things in nature) to mechanical principles; or, by ſhewing the inſufficiency of the Peripatetick and cheminal theories of qualities, to recommend the Corpuſcularian doctrine of them.

FOR further illuſtration of this point, I ſhall add on this occaſion, that there are three diſtinct ſorts of experiments (beſides other proofs) that may be reaſonably employed, (though they be not equally efficacious) when we treat of the origin of qualities. For ſome inſtances may be brought to ſhew, that the propoſed quality may be mechanically introduced into a portion of matter, where it was not before. Other inſtances there may be to ſhew, that by the ſame means the quality may be notably varied as to degrees, or other not eſſential attributes. And by ſome inſtances alſo it may appear, that the quality is mechanically expelled from, or aboliſhed in, a portion of matter, that was endowed with it before. Sometimes alſo by the ſame operation the former quality it deſtroyed, and a new one is produced. And each of theſe kinds of inſtances may be uſefully employed in our notes about particular qualities, For as to the firſt of them, there will be ſcarce any difficulty. And as to the ſecond, ſince the permanent degrees, as well as other attributes of qualities are ſaid to flow from (and do indeed depend upon) the ſame principles, that the quality itſelf does; if, eſpecially in bodies inanimate, a change barely mechanical does notably and permanently alter the degree or other conſiderable attribute; it will afford, though not a clear proof, yet a probable preſumption, that the principles, whereon the quality itſelf depends; are mechanical. And laſtly, if, by a bare mechanical change of the internal diſpoſition and ſtructure of a body, a permanent quality, confeſſed to flow from its ſubſtantial form, or inward principle, be aboliſhed, and, perhaps, alſo immediately ſucceeded by a new quality mechanically producible; if, I ſay, this come to paſs in a body inanimate, eſpecially, if it be alſo, as to ſenſe ſimilar, ſuch a phænomenon will not a little favour that hypotheſis, which teaches, that theſe qualities depend upon certain contextures, and other mechanical affections of the ſmall parts of the bodies, that are endowed with them, and conſequently may be aboliſhed when that neceſſary modification is deſtroyed. This is thus

thus briefly premised to shew the pertinency of alledging differing kinds of experiments and phænomena, in favour of the corpuscular hypothesis about qualities.

WHAT has been thus laid down, may, I hope, facilitate and shorten most of the remaining work of this preamble, which is to shew, though but very briefly, that there may be several ways, not impertinently employable to recommend the corpuscularian doctrine of qualities.

FOR first, it may sometimes be shewn, that a substantial form cannot be pretended to be the necessary principle of this or that quality; as will, for instance, hereafter be made manifest in the asperity and smoothness of bodies, and in the magnetical virtue, residing in a piece of iron, that has been impregnated by a loadstone. It is true, that the force of such instances is indirect, and that they do not expresly prove the hypothesis, in whose favour they are alledged; but yet they may do it good service, by disproving the grounds and conclusions of the adversaries, and so (by removing prejudices) making way for the better entertainment of the truth.

SECONDLY, we may sometimes obtain the same, or the like quality, by artificial and sometimes even temporary compositions, which, being but factitious bodies, are by learned adversaries confessed, not to have substantial forms, and can indeed reasonably be presumed to have but resulting temperaments: as will be hereafter exemplified in the production of green by compounding blue and yellow, and in the electrical faculty of glass; and in the temporary whiteness produced by beating clear oil and fair water into an ointment, and by beating water into a froth, and, more permanently, in making coral white by flawing it with heat; and in divers other particulars, that will more properly be elsewhere mentioned.

THIRDLY then, in some cases the quality proposed may be either introduced, or varied, or destroyed, in an inanimate body, when no change appears to be made in the body, except what is mechanical, and what might be produced in it, supposing such a parcel of matter were artificially framed and constituted as the body is, though without any substantial form, or other such like internal principle. So when a piece of glass, or of clarified rosin, is, by being beaten to powder, deprived of its transparency, and made white, there appears no change to be made in the pulverized body, but a comminution of it into a multitude of corpuscles, that by their number, and the various situations of their surfaces are fitted copiously to reflect the sincere light several ways, or give some peculiar modification to its rays; and hinder that free passage of the beams of light, that is requisite to transparency.

FOURTHLY, as in the cases belonging to the foregoing number there appears not to intervene in the patient or subject of the change, any thing but a mechanical alteration of the mechanical structure or constitution; so in some other cases it appears not, that the agent, whether natural or factitious, operates on the patient, otherwise than mechanically, employing only such a way of acting, as may proceed from the mechanism of the matter, which itself consists of, and that of the body it acts upon. As when goldsmiths burnish a plate or vessel of silver, that having been lately boiled looked white before, though they deprive it of the greatest part of its colour, and give it a new power of reflecting the beams of light and visible objects, in the manner proper to specular bodies; yet all this is done by the intervention of a burnishing tool, which often is but a piece of steel or iron conveniently shaped; and all that this burnisher does, is but to depress the little prominencies of the silver, and reduce them, and the little cavites of it, to one physically level or plain superficies. And so when a hammer striking often on a nail, makes the head of it grow hot, the hammer is but a purely mechanical agent, and works by local motion. And when by striking a lump of glass, it breaks it into a multitude of small parts, that compose a white powder, it acts as mechanically in the production of that whiteneſs, as it does in driving in a nail to the head. And ſo likewiſe, when the powdered glaſs, or colophony lately mentioned, is, by the fire, from a white and opacous body, reduced into a colourleſs (or a reddiſh) and tranſparent one, it appears not, that the fire, though a natural agent, need work otherwiſe than mechanically, by colliquating the incoherent grains of powder into one maſs; wherein, the ranks of pores not being broken and interrupted as before, the incident beams of light are allowed every way a free paſſage through them.

FIFTHLY, the like phænomena to thoſe of a quality to be explicated, or at leaſt as difficult in the ſame kind, may be produced in bodies and caſes, wherein it is plain we need not recur to ſubſtantial forms. Thus a varying colour, like that, which is admired in a pigeon's neck, may be produced in changeable taffety, by a particular way of ranging and connecting ſilk of ſeveral colours into one piece of ſtuff. Thus we have known opals caſually imitated and almoſt excelled by glaſs, which luckily degenerated in the furnace. And ſomewhat the like changeable and very delightful colour I remember to have introduced into common glaſs, with ſilver, or with gold and mercury. So likewiſe merely by blowing fine cryſtal-glaſs, at the flame of a lamp, to a very extraordinary thinneſs, we have made it to exhibit, and that vividly, all the colours (as they ſpeak) of the rainbow; and this power of pleaſing by diverſifying the light, the glaſs, if well preſerved, may keep for a long time. Thus alſo by barely beating gold into ſuch thin leaves, as artificers and apothecaries are wont to employ, it will be brought to exhibit a green colour, when you hold it againſt the light, whether of the day, or of a good candle; and this kind of greenneſs, as it is permanent in the foliated gold, ſo I have found by trial, that if the ſun-beams, ſomewhat united by a burning-glaſs, be trajected through the expanded leaf, and caſt upon a piece of white paper, they will appear there, as if they had been tinged in their paſſage. Nay, and ſometimes a ſlight and almoſt momentary mechanical change will ſeem to over-rule nature, and introduce into a body the quite oppoſite quality to that ſhe had given it: as when a piece of black horn is, only by being thinly ſcraped with the edge of a knife, or a piece of glaſs, reduced to permanently white ſhavings. And to theſe inſtances of colours, ſome emphatical, and ſome permanent, might be added divers belonging to other qualities, but that I ought not to anticipate what you will elſewhere meet with.

THERE is yet another way of arguing in favour of the Corpuſcularian doctrine of qualities, which, though it do not afford direct proofs of its being the beſt hypotheſis, yet it may much ſtrengthen the arguments drawn from other topicks, and thereby ſerve to recommend the doctrine itſelf. For, the uſe of an hypotheſis being to render an intelligible account of the cauſes of the effects, or phænomena propoſed, without croſſing the laws of nature, or other phænomena; the more numerous, and the more various the particles are, whereof ſome are explicable by the aſſigned hypotheſis, and ſome are agreeable to it, or, at leaſt, are not diſſonant from it, the more valuable is the hypotheſis, and the more likely to be true. For it is much more difficult, to find an hypotheſis, that is not true, which will ſuit with many phænomena, eſpecially, if they be of various kinds, than but with a few. And for this reaſon, I have ſet down among the inſtances belonging to particular qualities, ſome ſuch experiments and obſervations, as we are now ſpeaking of, ſince, although they be not direct proofs of the preferableneſs of our doctrine, yet they may ſerve for confirmation of it; though this be not the only, or perhaps the chief reaſon of their being mentioned. For, whatever they may be as argument, ſince they are matters of fact, I thought it not amiſs to take this occaſion of preſerving them from being loſt; ſince, whether or no they contribute much to the eſtabliſhment of the mechanical doctrine about qualities, they will, at leaſt, contribute to the natural hiſtory of them.

III. I ſhall not trouble the reader with a recital of thoſe unlucky accidents, that have hindered the ſubjects of the following book from being more numerous; and I hope he will the more eaſily excuſe their paucity, if he be advertiſed, that although the particular qualities, about which ſome experiments and notes, by way of ſpecimens, are here preſented, be not near half ſo many as were intended to be treated of; yet I was careful to choose them ſuch as might comprehend in a ſmall number a great variety; there being ſcarce one ſort of qualities, of which there is not an inſtance given in this ſmall book, ſince therein experiments and thoughts are delivered about heat and cold, which are the chief of the four firſt qualities; about taſtes and odours, which are of thoſe, that, being the immediate objects of ſenſe, are wont to be called ſenſible qualities; about volatility and fixity, corroſiveneſs and corroſibility, which, as they are found in bodies purely natural, are referable to thoſe qualities, that many phyſical writers call ſecond qualities, and which yet, as they may be produced and deſtroyed by the chemiſts art, may be ſtiled chemical qualities, and the ſpagyrical ways of introducing, or expelling them, may be referred to chemical operations, of which there is given a more ample ſpecimen in the mechanical account of chemical precipitations. And laſtly, ſome notes are added about magnetiſm and electricity, which are known to belong to the tribe of occult qualities.

IV. If a want of apt coherence, and exact method, be diſcovered in the following eſſays, it is hoped, that defect will be eaſily excuſed by thoſe, that remember and conſider, that theſe papers were originally little better than a kind of rhapſody of experiments, thoughts, and obſervations, occaſionally thrown together by way of annotations upon ſome paſſages of a diſcourſe, (about the differing parts and redintegration of nitre) wherein ſome things were pointed at, relating to the particular qualities, that are here more largely treated of. And though the particulars, that concern ſome of theſe qualities, were afterwards (to ſupply the place of thoſe borrowed by other papers whilſt theſe lay by me) encreaſed in number; yet it was not to be expected, that their acceſſion ſhould as well correct the form as augment the matter of our annotations. And as for the two tracts, that are inſerted among theſe eſſays about qualities; I mean, the diſcourſe of the imperfection of the chemical doctrine of them, and the reflections on the hypotheſis of acidum and alcali, the occaſion of their being made parts of this book, is ſo far expreſſed in the tracts themſelves, that I need not here trouble the reader with a particular account of it.

V. I do not undertake, that all the following accounts of particular qualities would prove to be the very true ones, nor every explication the beſt, that can be deviſed. For beſides that the difficulty of the ſubject, and incompleatneſs of the hiſtory we yet have of qualities, may well deter a man, leſs diffident of his own abilities than I juſtly am, from aſſuming ſo much to himſelf, it is not abſolutely neceſſary to my preſent deſign. For, mechanical explications of natural phænomena do give ſo much more ſatisfaction to ingenious minds, than thoſe, that muſt employ ſubſtantial forms, ſympathy, antipathy, &c. that the more judicious of the vulgar philoſophers themſelves prefer them before all others, when they can be had; (as is elſewhere ſhewn at large,) but then they look upon them either as confined to mechanical engines, or at leaſt, but as reaching to very few of nature's phænomena, and, for that reaſon, unfit to be received as phyſical principles. To remove therefore this grand prejudice and objection, which ſeems to be the chief thing, that has kept off rational inquiries from cloſing with the mechanical philoſophy, it may be very conducive, if not ſufficient, to propoſe ſuch mechanical accounts of particular qualities themſelves, as are intelligible and poſſible, and are agreeable to the phænomena whereto they are applied. And to this it is no more neceſſary, that the account propoſed ſhould be the trueſt and beſt, that can poſſibly be given, than it is to the proving, that a clock is not acted by a vital principle, (as thoſe Chineſes thought, who took the firſt, that was brought them out of *Europe*, for an animal,) but acts as an engine, to do more than aſſign a mechanical ſtructure made up of wheels, a ſpring, a hammer, and other mechanical pieces, that will regularly ſhew and ſtrike the hour, whether this contrivance be, or be not, the very ſame with that of the particular clock propoſed; which may indeed be made to move either with ſprings or weights, and may conſiſt of a greater or leſſer number of wheels, and thoſe differingly ſituated and connected; but for all this variety, it will ſtill be but an engine. I intend not therefore by propoſing the theories and conjectures ventured at in the following papers, to debar myſelf of the liberty either of altering them, or of ſubſtituting others in their places, in caſe a further progreſs in the hiſtory of qualities ſhall ſuggeſt better hypotheſes or explications. And it was but agreeable to this intention of mine, that I ſhould, as I have done, on divers occaſions in the following notes, employ the word *or*, and expreſs myſelf ſomewhat doubtingly, mentioning more than one cauſe of a phænomenon, or reaſon of an opinion, without dogmatically declaring for either; ſince my purpoſe in theſe notes was rather to ſhew, it was not neceſſary to betake ourſelves to the ſcholaſtick or chemical doctrine about qualities, than to act the umpire between the differing hypotheſes of the Corpuſcularians; and, provided I kept myſelf within the bounds of mechanical philoſophy, my deſign allowed me a great latitude in making explications of the phænomena I had occaſion to take notice of.

Of the MECHANICAL ORIGIN OF HEAT AND COLD.

SECTION I.

About the MECHANICAL PRODUCTION *of* COLD.

HEAT and cold being generally looked upon as the moſt active among qualities, from which many other qualities are deducible, and by which many of nature's phænomena, eſpecially among the Peripateticks, are attempted to be explicated; I ſuppoſe it will be very proper to begin with inſtances of them to ſhew, that qualities may be mechanically produced or deſtroyed. A not uſeleſs paraphraſe of which expreſſion may be this, that a portion of matter may come to be endowed with a quality, which it had not before, or to be deprived of one, that it had, or ſometimes to acquire, or loſe a degree of that quality; though on the part of the matter (or, as ſome would ſpeak, of the patient) there do not appear to intervene any more than a change of texture, or ſome other mechanical alteration; and though the agents (on their part) do not appear to act upon it otherwiſe, than after a mechanical manner, that is, by their bigneſs, ſhape, motion, and thoſe other attributes, by virtue whereof mechanical powers and engines perform their operations; and this without having recourſe to the Peripatetick ſubſtantial forms and elements, or the hypoſtatical principles of the chemiſts.

AND having here (as in a proper place) to avoid ambiguity, premiſed once for all this * ſummary declaration of the ſenſe, agreeably whereunto I would have theſe terms underſtood in the following notes about the origin of particular qualities; I proceed now to ſet down ſome few examples of the mechanical production of cold and heat, beginning with thoſe, that relate to the former, becauſe, by reaſon of their paucity, they will be quickly diſpatched. And I hope I ſhall not need to make an apology for mentioning no greater number; ſince I ſcarce remember to have met with any inſtances of this kind in any of the claſſick writers of natural philoſophy.

EXPERIMENT I.

MY firſt experiment is afforded me by the diſſolution of ſal armoniac, which I have ſomewhat wondered, that chemiſts having often occaſion to purify that ſalt by the help of water, ſhould not have, long ſince, and publickly, taken notice of. For, if you put into three or four times its weight of water, a pound, or but half a pound (or even leſs) of powdered ſal armoniac, and ſtir it about to haſten the diſſolution, there will be produced in the mixture a very intenſe degree of coldneſs, ſuch as will not be only very ſenſible to his hand, that holds the glaſs whilſt the diſſolution is making, but will very manifeſtly diſcover itſelf by its operation upon a thermoſcope. Nay, I have more than once, by wetting the outſide of the glaſs, where the diſſolution was making, and nimbly ſtirring the mixture, turned that externally adhering water into real ice, (that was ſcraped off with a knife) in leſs than a minute of an hour. And this thus generated cold continued conſiderably intenſe, whilſt the action of diſſolution laſted; but afterwards by degrees abated, and within a very few hours ceaſed. The particular phænomena I have noted in the experiments, and the practical uſes, that may be made of it, I reſerve for another place †, the knowledge of them being not neceſſary in this, where what I have already related, may ſuffice for my preſent argument.

AND to ſhew, that not only a far more intenſe degree of cold may emerge in this mixture, than was to be found in either of the ingredients before they were mingled, but a conſiderable coldneſs may be begun to be produced between bodies, that were neither of them actually cold before they were put together, I will ſubjoin a tranſcript of what I find to this purpoſe among my *adverſaria*.

EXPERIMENT II.

[I remember, that once I had a mind to try, whether the coldneſs produced upon the ſolution of beaten ſal armoniac in water might not be more probably referred to ſome change of texture or motion reſulting from the action of the liquor upon the ſalt, than to any infrigidation of the water made by the ſudden diſperſion of ſo many ſaline grains of powder, which, by reaſon of their ſolidity, may be ſuſpected to be actually more cold than the water they are put into; I therefore provided a glaſs full of that liquor, and having brought it to ſuch a temper, that its warmth made the ſpirit of wine in the ſealed weather-glaſs, manifeſtly, though not nimbly, aſcend; I took out the thermoſcope, and laid it in powdered ſal armoniac, warmed beforehand; ſo that the tincted liquor was made to aſcend much nimblier by the ſalt than juſt before by the water; and having preſently removed the inſtrument into that liquor again, and poured the ſomewhat warm ſal armoniac into the ſame, I found, as I imagined, that within a ſpace of time, which I gueſſed to be about half a minute or leſs, the ſpirit of wine began haſtily to ſubſide, and within a few minutes fell above a whole diviſion and a quarter

* See more of this in the preamble.

† Divers of the phænomena, &c. of this experiment were afterwards printed, Numb. 15. of the *Philoſophical Tranſactions*.

quarter below the mark at which it ſtood in the water, before that liquor or the ſalt were warmed. Nor did the ſpirit in a great while re-aſcend to the height, which it had, when the water was cold.

THE ſame experiment, being at another time reiterated, was tried with the like ſucceſs; which ſecond may therefore ſerve for a confirmation of the firſt.]

EXPERIMENT III.

HAVING a mind likewiſe to ſhew ſome ingenious men, how much the production of heat and cold depends upon texture and other mechanical affections, I thought fit to make again a ſal armoniac by a way I formerly publiſhed, that I might be ſure to know what ingredients I employed, and ſhew their effects, as well before conjunction as after it. I took then ſpirit of ſalt, and ſpirit of fermented, or rather putrified urine; and having put a ſealed weather-glaſs into an open veſſel, where one of them was poured in, I put the other, by degrees, to it, and obſerved, that as, upon their mingling, they made a great noiſe with many bubbles, ſo, in this conflict, they loſt their former coldneſs, and impelled up the ſpirit of wine in the ſealed thermoſcope: then ſlowly evaporating the ſuperfluous moiſture, I obtained a fine ſort of ſal armoniac, for the moſt part figured not unlike the other, when being diſſolved and filtrated, it is warily coagulated. This new ſalt being gently dried, I put into a wide glaſs of water, wherein I had before placed a ſealed weather-glaſs, that the included ſpirit might acquire the temper of the ambient liquor, and having ſtirred this ſalt in the water, though I took it then off the mantle-tree of a chimney, that had had fire in it divers hours before, it did, as I expected, make the tincted ſpirit haſtily ſubſide, and fall conſiderably low.

EXPERIMENT IV.

SINCE, if two bodies, upon their mixture, acquire a greater degree of cold than either of them had before, there is a production of this additional degree of that quality, it will be proper to add, on this occaſion, the enſuing experiment.

WE took a competent quantity of acid ſpirit diſtilled from roch-allom, (that, though rectified, was but weak,) which, in the ſpirit of that ſalt, is not ſtrange. Of this we put into a wide-mouthed glaſs (that was not great) more than was ſufficient to cover the globulous part of a good ſealed thermoſcope, and then ſuffering the inſtrument to ſtay a pretty while in the liquor, that the ſpirit of wine might be cooled, as much as the ambient was, we put in, little by little, ſome volatile ſalt ſublimed from ſal armoniac and a fixed alcali, and notwithſtanding the very numerous (but not great) bubbles, and the noiſe and froth that were produced, as is uſual upon the re-action of acids and alcalies, the tincted ſpirit in the weather-glaſs, after having continued a good while at a ſtand, began a little to deſcend, and continued (though but very ſlowly) to do ſo, till the ſpirit of allom was glutted with the volatile ſalt; and this deſcent of the tincted liquor in the inſtrument being meaſured, appeared to be about an inch (for it manifeſtly exceeded ſeven eighths.) By comparing this experiment with the firſt part of the foregoing, we may gather, that when volatile and urinous ſalts or ſpirits (for the ſaline particles appear ſometimes in a dry, and ſometimes in a liquid form) tumultuate upon their being mixed with acids, neither the heat nor the cold, that enſues, is produced by a conflict with the acids preciſely as it is acid, ſince we have ſeen, that an urinous ſpirit produced an actual heat with ſpirit of ſalt, and the diſtilled ſalt of ſal armoniac, which is alſo urinous, with the acid ſpirit of roch-allom, produces not a true efferveſcence, but a manifeſt coldneſs: as the ſame ſalt alſo did in a trial of another ſort, which was this.

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EXPERIMENT V.

WE took one part of oil of vitriol, and ſhaking it into twelve parts of water we made a mixture, that at firſt was ſenſibly warm: then ſuffering this to cool, we put a ſufficient quantity of it into a wide-mouthed glaſs, and then we put a good thermoſcope hermetically ſealed, above whoſe ball the compounded liquor reached a pretty way. After ſome time had been allowed, that the liquor in the thermometer might acquire the temper of the ambient; we put in, by degrees, as much volatile ſalt of ſal armoniac, as would ſerve to ſatiate the acid ſpirits of the mixture: for, though theſe two made a notable conflict with tumult, noiſe, and froth, yet it was but a cold ebullition (if I may ſo ſtile it,) for the ſpirit in the thermoſcope deſcended about an inch beneath the mark it reſted at, when the ſeeming effervеſcence began.

EXPERIMENT VI.

IT is known, that ſalt-petre being put into common water produces a ſenſible coldneſs in it, as it alſo does in many other liquors: But that the ſame ſalt put into a liquor of another conſtitution may have a quite differing effect, I have convinced ſome inquiſitive perſons, by mingling eight ounces of fine ſalt-petre, powdered, with ſix ounces of oil of vitriol: for by that commixture with a ſalt, that was not only actually, but, as to many other bodies, potentially cold, the oil of vitriol, that was ſenſibly cold before, quickly conceived a conſiderable degree of heat, whoſe effects alſo became viſible in the copious fumes, that were emitted by the incaleſcent mixture.

EXPERIMENT VII.

THIS brings into my mind, that though gunpowder ſeems to be of ſo igneous a nature, that, when it is put upon a coal, it is turned preſently into a flame capable of promoting the deflagration of the charcoal, and kindling divers bodies it meets with in its way; yet if ſome ounces of gunpowder, reduced to powder, be thrown into four or five times as much water, it will very manifeſtly impart a coldneſs to it, as experience made with, as well as without, a ſealed thermoſcope has aſſured me.

THIS and the foregoing experiment do readily ſuggeſt an enquiry into the nature of the coldneſs, which philoſophers are wont to oppoſe to that, which immediately, and upon the firſt contact, affect the organs of ſenſe, and which therefore they call actual or formal.

THE ſucceſs of this experiment upon a ſecond trial ſerved to confirm it, which is the more ſtrange, becauſe I have found, that a ſmall quantity of oil of vitriol, not beforehand mingled with water, would produce a notable heat in its conflict with a ſmall portion of juſt ſuch ſalt as I employed before (both the parcels having been, if I well remember, taken out of the ſame glaſs.) And this heat did, upon trial made with the former thermoſcope, make the tincted ſpirit aſcend much further than the lately recited experiment made it ſubſide.

A Digreſſion about POTENTIAL COLDNESS.

POTENTIAL coldneſs has been generally looked upon, and that partly perhaps upon the ſcore of its very name, as ſo abſtruſe a quality, that it is not only rational, but neceſſary to derive it from the ſubſtantial forms of bodies. But, I confeſs, I ſee no neceſſity of believing it not to be referrible to mechanical principles. For, as to the chief inſtances of potential coldneſs, which are taken from the effects of ſome medicines

medicines and aliments in the bodies of men, it may be ſaid, without improbability, that the produced refrigeration proceeds chiefly from this, that the potentially cold body is made up of corpuſcles of ſuch ſize, ſhape, &c. that, being reſolved and disjoined by the menſtruum of the ſtomach, or the fluids it may elſewhere meet with, they do ſo aſſociate themſelves with the ſmall parts of the blood, and other liquors, as, by clogging them, or otherwiſe, to leſſen their wonted agitation, and perhaps make them act in a peculiar way, as well as leſs briſkly on the nervous and fibrous parts; and the perception of this imminution (and perhaps change) of motion in the organs of feeling, is that, which, being referred to the body, that produces it, we call it's potential coldneſs. Which quality appears by this account to be, as I was ſaying before, but a relative thing, and is wont to require the diffuſion or diſperſion of the ſmall parts of the corpuſcles of the agent, and their mingling themſelves with the liquors, or the ſmall parts of the body they are to refrigerate. And therefore, if it be granted, that, in agues, there is ſome morbifick matter, of a viſcous or not eaſily diſſipable texture, that is harboured in ſome part of the body, and requires ſuch a time to be made fluid and reſolvable, the cold fits of agues need not be ſo much admired as they uſually are; ſince, though juſt before the fit the ſame parcel of matter, that is to produce it, were actually in the body, yet it was not, by reaſon of its clamminess, actually reſolved into ſmall parts, and mingled with thoſe of the blood, and conſequently could not make ſuch a change in the motion of that liquor, as is felt in the cold fit of an ague; (for, of the further change, that occaſions the hot fit, I am not here to ſpeak.) And in ſome other diſeaſes, a ſmall quantity of matter, being reſolved into minute parts, may be able to produce a great ſenſe of coldneſs in ſome part of a body, which, by reaſon of the ſtructure of that part, may be peculiarly diſpoſed to be affected thereby; as I have known hypochondriac and hyſterical women complain of great degrees of coldneſs, that would ſuddenly invade ſome particular part, chiefly of the head or back, and be for a good while troubleſome there. And that, if a frigorific vapour, or matter, be exeeeding ſubtile, an inconſiderable quantity of it being diſperſed through the blood, may ſuffice to produce a notable refrigeration, I have learned by enquiry into the effects of ſome poiſons; and it is not very material, whether the poiſon, generally ſpeaking, be cold or hot, if it meet with a body diſpoſed to have thoſe affections, that paſs for cold ones produced in it. For I have made a chemical liquor, that was penetrant and fiery enough to the taſte, and had acquired a ſubtlety and briſkneſs from diſtillation, with which I could, almoſt in a trice, giving it but in the quantity of about a drop, caſt an animal into that, which appeared a ſleep; and the like liquor, in a not much greater quantity, being, by I know not whoſe miſtake, applied to the aching tooth of a very ingenious perſon, did preſently, as he ſoon after told me, give him an univerſal refrigeration, and trembling, worſe than the cold paroxiſm of a quartane. And though ſcorpions do ſometimes cauſe, by their ſting, violent heats in the parts they hurt, yet ſometimes alſo the quite contrary happens, and their poiſon proves, in a high degree, potentially cold; as may be learned from the two following obſervations, recorded by eminent phyſicians. * *Famulum habui* (ſaith *Benivenius,*) *qui à ſcorpione ictus, tam ſubito ac tam frigido ſudore toto corpore perfuſus eſt, ut algentiſſimâ nive atque glacie ſeſe opprimi quereretur. Verùm cùm algenti illi ſolam theriacam ex vino potentiore exhibuiſſem, illicò curatus eſt:* thus far he: to whoſe narrative I add this of *Amatus Luſitanus.*

* *Benivien. cap. 56. Abditorum apud Schenk. lib. 7 de venen. observ. 24. Cent. 6. observ.*

VIR *qui à ſcorpione in manus digito punctus fuit, multum dolebat, & refrigeratus totus contremebat, & per corpus dolores, cute totâ quaſi acu punctâ, formicantes patiebatur, &c.*

I cannot

I cannot now ſtay to enquire, whether there may not be in theſe great refrigerations, made by ſo ſmall a quantity of poiſon, ſome ſmall concretions or coagulations made of the minute particles of the blood into little clots, leſs agile and more unweildy than they were, when they moved ſeparately: which may be illuſtrated by the little curdlings, that may be made of the parts of milk, by a very ſmall proportion of runnet, or ſome acid liquor, and the little coagulations made of the ſpirit of wine by that of urine: nor will I now enquire, whether, beſides the retardment of the motion of the blood, ſome poiſons, and other analogous agents, may not give the motion of it a new modification, (as if ſome corpuſcles, that uſually are more whirled or brandiſhed, be put into a more direct motion,) that may give it a peculiar kind of grating, or other action, upon the nervous and fibrous parts of the body. Theſe, I ſay, and other ſuſpicions, that have ſometimes come into my thoughts, I muſt not ſtay to examine; but ſhall now rather offer to conſideration, whether, ſince ſome parts of the human body are very differing from others in their ſtructure and internal conſtitution; and ſince alſo ſome agents may abound in corpuſcles of differing ſhapes, bulks, and motions, the ſame medicine may not, in reference to the ſame human body, be potentially cold, or potentially hot, according as it is applied; or perhaps may, upon one or both of the accounts newly mentioned, be cold, in reference to one part of the body, and hot, in reference to the other, And theſe effects need not be always aſcribed to the mere and immediate action of the corpuſcles of the medicine, but ſometimes to the new quality they acquire in their paſſage, by aſſociating themſelves with the blood, or other fluids of the body, or to the expulſion of ſome calorific or frigorific corpuſcles, or to the diſpoſition they give the part on which they operate, to be more or leſs permeated and agitated than before, by ſome ſubtile æthereal matter, or other efficients of heat or cold. Some of theſe conjectures about the relative nature of potentially cold bodies may be either confirmed or illuſtrated by ſuch inſtances as theſe; that ſpirit of wine, being inwardly taken, is potentially very hot; and yet, being outwardly applied to ſome burns, and ſome hot tumours, does notably abate the heat of the inflamed parts, though the ſame ſpirit, applied even outwardly to a tender eye, will cauſe a great and dolorous agitation in it. And camphire, which in the doſe of leſs than a half, or perhaps a quarter of a ſcruple, has been obſerved to diffuſe a heat through the body, is, with ſucceſs, externally applied by phyſicians and ſurgeons in refrigerating medicines.

BUT I leave the further inquiry into the operations of medicines to phyſicians, who may poſſibly, by what has been ſaid, be aſſiſted to compoſe the differences between ſome famous writers about the temperament of ſome medicines, as mercury, camphire, &c. which ſome will have to be cold, and others maintain to be hot; and ſhall only offer by way of confirming in general, that potential coldneſs is only a relative quality, a few particulars; the firſt whereof is afforded by comparing together the ſixth and the ſeventh experiment before going, (which have occaſioned this digreſſion about potential coldneſs;) ſince by them it ſeems probable, that the ſame thing may have it in reference to one body, and not to another, according to the diſpoſition of the body it operates upon, or that operates upon it. And the fumes of lead have been obſerved ſometimes (for I have not found the effect to ſucceed always) to arreſt the fluidity of mercury, which change is ſuppoſed to be the effect of a potential coldneſs belonging to the chemiſt's Saturn in reference to fluid mercury, though it have not that operation on any other liquor, that we know of.

AND laſtly, (for I would not be too prolix) though nitre and ſal armoniac be both apart and jointly cold in reference to water, and though, however nitre be thoroughly melted in a crucible, it will not take fire of itſelf, yet if, whilſt it is in fuſion, you ſhould by degrees caſt on it ſome powdered ſal armoniac, it will take fire and flaſh vehemently, almoſt as if, ſulphur had been injected.

But our excurſion has, I fear, laſted too long, and therefore I ſhall preſently reenter into the way, and proceed to ſet down ſome trials about cold.

EXPERIMENT VIII.

In the firſt experiment we obſerved, that upon the pouring of water upon ſal armoniac there enſued an intenſe degree of cold; and we have elſewhere recited, that the like effect was produced by putting, inſtead of common water, oil of vitriol to ſal armoniac: but now, to ſhew further, what influence motion and texture may have upon ſuch trials, it may not be amiſs to add the following experiment: to twelve ounces of ſal armoniac we put, by degrees, an equal weight of water, and whilſt the liquor was diſſolving the ſalt, and by that action producing a great coldneſs, we warily poured in twelve ounces alſo of good oil of vitriol; of which new mixture the event was, that a notable degree of heat was quickly produced in the glaſs, wherein the ingredients were confounded, as unlikely as it ſeemed, that, whereas each of the two liquors is wont, with ſal armoniac, to produce an intenſe cold, both of them acting on it together ſhould produce the contrary quality. But the reaſon I had to expect the ſucceſs I met with, was this, that it was probable the heat, ariſing from the mixture of the two liquors, would overpower the coldneſs produceable by the operation of either, or both of them upon the ſalt.

EXPERIMENT IX.

In moſt of the experiments, that we have hitherto propoſed, cold is wont to be regularly produced in a mechanical way; but I ſhall now add, that in ſome ſort of trials I found, that the event was varied by unobſerved circumſtances; ſo that ſometimes manifeſt coldneſs would be produced by mixing two bodies together, which at another time would upon their congreſs diſcloſe a manifeſt heat, and ſometimes again, though more rarely, would have but a very faint and remiſs degree of either.

Of this ſort of experiments, whoſe events I could not confidently undertake for, I found to be, the diſſolution of ſalt of tartar in ſpirit of vinegar, and of ſome other ſalts, that were not acid, in the ſame menſtruum, and even ſpirit of verdigreaſe (made *per ſe)* though a more potent menſtruum than common ſpirit of vinegar would not conſtantly produce near ſuch a heat at the beginning of its operation, as the greatneſs of the ſeeming effervеſcence, then excited, would make one expect, as may appear by the following obſervation tranſeribed *verbatim* out of one of my *Adverſaria.*

[Into eight ounces of ſpirit of verdigreaſe (into which we had put a while before a ſtandard-thermoſcope, to acquire the like temper with the liquor) we put in a widemouthed glaſs two ounces of ſalt of tartar, as faſt as we durſt for fear of making the matter boil over; and though there were a great commotion excited by the action and reaction of the ingredients, which was attended with a copious froth and a hiſſing noiſe; yet it was a pretty while, ere the glaſs was ſenſibly warm on the outſide; but by that time the ſalt was all diſſolved, the liquor in the thermoſcope appeared to be impelled up about three inches and a half.

And yet, if my memory do not much deceive me, I have found, that by mixing ſalt of tartar with another ſalt, the texture of the fixed alkali was ſo altered, that upon the affuſion of ſpirit of verdigreaſe, (made without ſpirit of vinegar and ſpirit of wine) though there enſued a great conflict with noiſe and bubbles, yet, inſtead of an incaleſcence, a conſiderable degree of coldneſs was produced.

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EXPERIMENT X.

It is very probable, that further trials will furnifh us with more inftances, to fhew how the production of cold may, in fome cafes, be effected, varied, or hindered by mechanical circumftances, that are eafily and ufually overlooked. I remember, on this occafion, that though, in the experiment above recited, we obferved, that oil of vitriol and water being firft fhaken together, the volatile falt of fal armoniac being afterwards put to them, produced a fenfible coldnefs; yet I found, that if a little oil of vitriol, and of the volatile falt, were firft put together, though foon after a confiderable proportion of water were added, there would be produced, not a coldnefs, but a manifeft degree of heat, which would impel up the liquor in the thermofcope to the height of fome inches. And I remember too, that though falt of tartar will, as we fhall fee ere long, grow hot in the water, yet having diftilled fome falt of tartar and cinnabar in a ftrong fire, and put the whole *Caput mortuum* into diftilled or rain water, it made indeed a hiffing there, as if it had been quick-lime, but produced no heat, that I could by feeling perceive. I fhall add, that not only, as we have feen already, fome unheeded circumftances may promote or hinder the artificial production of cold by particular agents, but, which will feem more ftrange, fome unobferved, and perhaps hardly obfervable, indifpofition in the patient, may promote or hinder the effects of the grand and catholick efficients of cold, whatever thofe be. This fufpicion I reprefent as a thing, that further experience may poffibly countenance, becaufe I have fometimes found, that the degree of the operation of cold has been much varied by latent circumftances, fome bodies being more wrought upon, and others lefs, than was, upon very probable grounds, expected. And particularly I remember, that though oil of vitriol be one of the firieft liquors, that is yet known, and does perform fome of the operations of fire, itfelf, (as we fhall elfewhere have occafion to fhew) and will thaw ice fooner than fpirit of wine, or any other liquor, as I have tried; yet having put about a pound or more, by our eftimate, of choice rectified oil of vitriol, into a ftrong glafs vial proportionable to it, we found, that, except a little, that was fluid at the top, it was all congealed or coagulated into a mafs like ice, though the glafs ftood in a laboratory, where a fire was conftantly kept not far from it, and where oil of vitriol very feldom, or never, has before, or fince, been obferved to congeal or coagulate fo much as in part. And the oddnefs of our phænomenon was encreafed by this circumftance, that the mafs continued folid a good while after the weather was grown too mild to have fuch operations upon liquors far lefs indifpofed to lofe their fludity by cold, than even common oil of vitriol is. On the other fide I remember, that about two years ago, I expofed fome oil of fweet almonds hermetically fealed up in a glafs bubble, to obferve what condenfation an intenfe cold could make of it, (for though cold expands water, it condenfes common oil;) but the next day I found, to my wonder, that not only the oil remained unfrozen by the fharp froft it had been expofed to, but that it had not its tranfparency troubled, though it is known, that oil will be brought to concrete, and turn opacous by a far lefs degree of cold than is requifite to freeze water; notwithftanding which, this liquor, which was lodged in a glafs, fo thin, that it was blown at the flame of a lamp, continued fluid and diaphanous in very frofty weather, fo long till I loft the expectation of feeing it congealed or concreted. And this brings into my mind, that though camphire be, as I formerly noted, reckoned by many potentially cold, yet we kept fome oil of it, of our making, wherein the whole body of the camphire remained, being only by fome nitrous fpirits reduced to the form of an oil; we kept it, I fay, in fuch intenfe degrees of cold, that would have eafily frozen water, without finding it to lofe its tranfparency, or its fluidity.

AND here I shall put an end to the first section, (containing our notes about cold) the design of which may be not a little promoted by comparing with them the beginning of the ensuing section. For, if it be true, that (as we there shew) the nature of heat consists either only or chiefly in the local motion of the small parts of a body mechanically modified by certain conditions, of which the principal is the vehemency of the various agitations of those insensible parts; and if it be also true, as experience witnesses it to be, that, when the minute parts of a body are in, or arrive at such a state, that they are more slowly or faintly agitated than those of our fingers, or other organs of feeling, we judge them cold: these two things, laid together, seem plainly enough to argue, that a privation or negation of that local motion, that is requisite to constitute heat, may suffice for the denominating a body cold, as coldness is a quality of the object, (which, as it is perceived by the mind, is also an affection of the sentient:) and therefore an imminution of such a degree of former motion, as is necessary to make a body hot as to sense, and which is sufficient to the production of sensible coldness, may be mechanically made, since slowness, as well as swiftness, being a mode of local motion, is a mechanical thing. And though its effect, which is coldness, seem a privation or negation; yet the cause of it may be a positive agent acting mechanically, by clogging the agile calorific particles, or deadning their motion, or perverting their determination, or by some other intelligible way bringing them to a state of coldness, as to sense: I say, coldness as to sense; because as it is a tactile quality, in the popular acception of it, it is relative to our organs of feeling; as we see, that the same lukewarm water will appear hot and cold to the same man's hands, if, when both are plunged into it, one of them shall have been newly held to the fire, and the other be benumbed with frost. And indeed the custom of speaking has introduced an ambiguity into the word cold, which often occasions mistakes, not easily, without much attention, and sometimes circumlocution also, to be avoided; since usually by cold is meant that, which immediately affects the sensory of him, that pronounces a body cold, whereas sometimes it is taken in a more general notion for such a negation or imminution of motion, as though it operates not perceivably on our senses, does yet upon other bodies; and sometimes also it is taken (which is perhaps the more philosophical sense) for a perception, made in and by the mind, of the alteration produced in the corporeal organs by the operation of that, whatever it be, on whose account a body is found to be cold.

BUT the discussion of these points is here purposely omitted, as for other reasons, so principally, because they may be found expresly handled in a fitter place.

SECTION II.

Of the MECHANICAL ORIGIN, *or* PRODUCTION *of* HEAT.

AFTER having dispatched the instances I had to offer of the production of cold, it remains, that I also propose some experiments of heat, which quality will appear the more likely to be mechanically producible, if we consider, the nature of it, which seems to consist mainly, if not only, in that mechanical affection of matter we call local motion mechanically modified, which modification, as far as I have observed, is made up of three conditions.

THE first of these is, that the agitation of the parts be vehement, by which degree of rapidness the motion proper to bodies, that are hot, distinguishes them from bodies, that are barely fluid. For these, as such, require not near so brisk an agitation, as is wont to be necessary to make bodies deserve the name of hot. Thus we see, that the particles

particles of water, in its natural (or uſual) ſtate, move ſo calmly, that we do not feel it at all warm, though it could not be a liquor, unleſs they were in a reſtleſs motion; but when water comes to be actually hot, the motion does manifeſtly and proportionably appear more vehement, ſince it does not only briſkly ſtrike our organs of feeling, but ordinarily produces ſtore of very ſmall bubbles, and will melt butter or coagulated oil caſt upon it, and will afford vapours, that, by the agitation they ſuffer, will be made to aſcend into the air. And if the degree of heat be ſuch, as to make the water boil, then the agitation becomes much more manifeſt by the confuſed motions, and waves, and noiſe, and bubbles, that are excited, and by other obvious effects, and phænomena of the vehement and tumultous motion, which is able to throw up viſibly into the air great ſtore of corpuſcles, in the form of vapours or ſmoke. Thus, in a heated iron, the vehement agitation of the parts may be eaſily inferred, from the motion and hiſſing noiſe it imparts to drops of water, or ſpittle, that fall upon it. For it makes them hiſs and boil, and quickly forces their particles to quit the form of a liquor, and fly into the air in the form of ſteams. And, laſtly, fire, which is the hotteſt body we know, conſiſts of parts ſo vehemently agitated, that they perpetually and ſwiftly fly abroad in ſwarms, and diſſipate or ſhatter all the combuſtible bodies they meet with in their way; fire making ſo fierce a diſſolution, and great a diſperſion of its own fuel, that we may ſee whole piles of ſolid wood (weighing perhaps many hundred pounds) ſo diſſipated, in very few hours, into flame and ſmoke, that, oftentimes, there will not be one pound of aſhes remaining. And this is the firſt condition required to heat.

THE ſecond is this, that the determinations be very various, ſome particles moving towards the right, ſome to the left hand, ſome directly upwards, ſome downwards, and ſome obliquely, &c. This variety of determinations appears to be in hot bodies, both by ſome of the inſtances newly mentioned, and eſpecially that of flame, which is a body; and by the diffuſion, that metals acquire, when they are melted, and by the operations of heat, that are exerciſed by hot bodies upon others, in what poſture or ſituation ſoever the body to be heated be applied to them. As a thoroughly ignited coal will appear every way red, and will melt wax, and kindle brimſtone, whether the body be applied to the upper or to the lower, or to any other part of the burning coal. And congruouſly to this notion, though air and water be moved never ſo vehemently, as in high winds and cataracts; yet we are not to expect, that they ſhould be manifeſtly hot, becauſe the vehemency belongs to the progreſſive motion, of the whole body; notwithſtanding which, the parts it conſiſts of may not be near ſo much quickened in their motions, made according to other determinations, as to become ſenſibly hot. And this conſideration may keep it from ſeeming ſtrange, that, in ſome caſes, where the whole body, though rapidly moved, tends but one way, it is not by that ſwift motion perceived to be made hot.

NAY, though the agitation be very various, as well as vehement, there is yet a third condition required to make it calorific; namely, that the agitated particles, or at leaſt the greateſt number of them, be ſo minute, as to be ſingly inſenſible. For though a heap of ſand, or duſt itſelf, were vehemently and confuſedly agitated by a whirl-wind, the bulk of the grains or corpuſcles, would keep their agitation from being properly heat, though, by their numerous ſtrokes upon a man's face, and the briſk commotion of the ſpirits: and other ſmall particles, that may thence enſue, they may perchance occaſion the production of that quality.

IF ſome attention be employed, in conſidering the formerly propoſed notion of the nature of heat, it may not be difficult to diſcern, that the mechanical production of it may be divers ways effected. For, excepting in ſome few anomalous caſes, (wherein the regular courſe of things happens to be over-ruled,) by whatever ways the inſenſible parts of a body are put into a very confused and vehement agitation, by the same ways heat may be introduced into that body: agreeably to which doctrine, as there are several agents and operations, by which this calorific motion (if I may so call it) may be excited, so there may be several ways of mechanically producing heat, and many experiments may be reduced to almost each of them, chance itself having, in the laboratories of chemists, afforded divers phænomena, referable to any one or other of those heads. Many of the more familiar instances, applicable to our present purpose, have been long since collected by our justly-famous *Verulam*, in his short, but excellent paper *de forma calidi*, wherein (though I do not acquiesce in every thing I meet with there) he seems to have been, at least among the moderns, the person, that has first handled the doctrine of heat like an experimental philosopher. I shall therefore decline accumulating a multitude of instances of the production of heat, and I shall also forbear to insist on such known things, as the incalescence, observable upon the pouring either of oil of vitriol upon salt of tartar, (in the making of tartarum vitriolatum) or of aqua fortis upon silver or quicksilver, (in the dissolution of these metals,) but shall rather chuse to mention some few instances not so notorious as the former, but not so unfit, by their variety, to exemplify several of the differing ways of exciting heat.

AND yet I shall not decline the mention of the most obvious and familiar instance of all, namely, the heat observed in quick-lime, upon the affusion of cold water, because, among learned men, and especially Peripateticks, I find causes to be assigned, that are either justly questionable, or manifestly erroneous. For, as to what is inculcated by the schools, about the incalescence of a mixture of quick-lime and water, by virtue of a supposed Antiperistasis, or invigoration of the internal heat of the lime, by its being invironed by cold water, I have elsewhere shewn, that this is but an imaginary cause, by delivering, upon experiment, (which any man may easily make,) that if, instead of cold water, the liquor be poured on very hot, the ebullition of the lime will not be the less, but rather the greater: and oil of turpentine, which is a lighter, and is looked upon as a subtiler liquor than water, though it be poured quite cold on quick-lime will not, that I have observed, grow so much as sensibly hot with it.

AND now I have mentioned the incalescence of lime, which, though an obvious phænomenon, has exercised the wits of divers philosophers and chemists, I will add two or three observations, in order to an enquiry, that may be some other time made into the genuine causes of it; which are not so easy to be found, as many learned men may, at first sight, imagine. The acute *Helmont* indeed, and his followers, have ingeniously enough attempted to derive the heat under consideration from the conflict of some alcalizate and acid salts, that are to be found in quick-lime, and are dissolved, and so set at liberty, to fight with one another by the water that slakes the lime. But, though we have some manifest marks of an alcalizate salt in lime, yet, that it contains also an acid salt, has not, that I remember, been proved; and if the emerging of heat be a sufficient reason to prove a latent acid salt in lime, I know not, why I may not infer, that the like salt lies concealed in other bodies, which the chemists take to be of the purest or merest sort of alcalies.

EXPERIMENT I.

FOR I have purposely tried, that by putting a pretty quantity of dry salt of tartar in the palm of my hand, and wetting it well in cold water, there has been a very sensible heat produced in the mixture; and when I have made the trial with a more considerable quantity of salt and water in a vial, the heat proved troublesomely intense, and continued to be at least sensible a good while after.

THIS

THIS experiment seems to favour the opinion, that the heat produced in lime, whilst it is quenching, proceeds from the empyreuma, as the chemists call it, or impression left by the violent fire, that was employed to reduce the stone to lime. But if by empyreuma be meant a bare impression made by the fire, it will be more requisite than easy, to declare intelligibly, in what that impression consists, and how it operates to produce such considerable effects. And if the effect be ascribed to swarms of atoms of fire, that remain adherent to the substance of the lime, and are set at liberty to fly away by the liquor, which seems to be argued by the slaking of lime without water, if it be for some time left in the air, whereby the atoms of fire get opportunity to fly away by little and little: if this, I say, be alledged, I will not deny, but there may be a sense, which I cannot explicate in few words, wherein the co-operation of a substantial effluvium, (for so I call it,) of the fire, may be admitted in giving an account of our phænomenon. But the cause formerly assigned, as it is crudely proposed, leaves in my mind some scruples. For it is not so easy to apprehend, that such light and minute bodies, as those of fire, are supposed, should be so long detained, as by this hypothesis they must be allowed to be, in quick-lime, kept in well-stopped vessels, from getting out of so lax and porous a body as lime, especially since we see not a great incalescence or ebullition ensue upon the pouring of water upon minium, or *crocus Martis per se*, though they have been calcined by violent and lasting fires, whose effluviums or emanations appear to adhere to them by the increase of weight, that lead, if not also *Mars*, does manifestly receive from the operation of the fire. To which I shall add, that, whereas one would think, that the igneous atoms should either fly away, or be extinguished by the supervening of water, I know, and elsewhere give account, of an

EXPERIMENT II.

IN which two liquors, whereof one was furnished me by nature, did by being several times separated and reconjoined without additament, at each congress produce a sensible heat.

EXPERIMENT III.

AND an instance of this kind, though not so odd, I purposely sought and found in salt of tartar, from which, after it had been once heated by the affusion of water, we abstracted or evaporated the liquor, without violence of fire, till the salt was again dry; and then putting on water a second time, the same salt grew hot again in the vial, and, if I misremember not, it produced this incalescence the third time, if not the fourth; and might probably have done it oftner, if I had had occasion to prosecute the experiment. Which seems at least to argue, that the great violence of fire is not necessary to impress what passes for an empyreum upon all calcined bodies, that will heat with water.

AND on this occasion I shall venture to add, that I have sometimes doubted, whether the incalescence may not much depend upon the particular disposition of the calcined body, which being deprived of its former moisture, and made more porous by the fire, doth by the help of those igneous effluviums, for the most part of a saline nature, that are dispersed through it, and adhere to it, acquire such a texture, that the water impelled by its own weight, and the pressure of the atmosphere, is able to get into a multitude of its pores at once, and suddenly dissolve the igneous and alcalizate salt it every where meets with there, and briskly disjoin the earthy and solid particles, that were blended with them; which being exceeding numerous, though each of them perhaps be very minute, and moves but a very little way, yet their multitude makes the confused

confused agitation of the whole aggregate of them, and of the particles of the water and salt vehement enough to produce a sensible heat; especially if we admit, that there is such a change made in the pores, as occasions a great increase of this agitation, by the ingress and action of some subtile ethereal matter, from which alone Monsieur *Des Cartes* ingeniously attempts to derive the incalescence of lime and water, as well as that of metals dissolved in corrosive liquors; though as to the phænomena we have been considering, there seems at least to concur a peculiar disposition of body, wherein heat is to be produced to do one or both of these two things, namely, to retain good store of the igneous effluvia, and to be, by their adhesion or some other operation of the fire, reduced to such a texture of its component particles, as to be fit to have them easily penetrated, and briskly, as well as copiously, dissipated, by invading water. And this conjecture (for I propose it as no other) seems favoured by divers phænomena, some whereof I shall now annex. For here it may be observed, that both the dissolved salt of tartar lately mentioned, and the artificial liquor, that grows hot with the natural, re-acquires that disposition to incalescence upon a bare constipation, or closer texture of the parts from the superfluous moisture they were drowned in before; the heat, that brought them to this texture, having been so gentle, that it is no way likely, that the igneous exhalations could themselves produce such a heat, or at least, that they should adhere in such numbers, as must be requisite to such an effect, unless the texture of the salt of tartar, or other body, did peculiarly dispose it to detain them; since

EXPERIMENT IV.

I have found by trial, that sal armoniac dissolved in water, though boiled up with a brisker fire to a dry salt, would, upon its being again dissolved in water, not produce any heat, but a very considerable degree of cold. I shall add, that though one would expect a great cognation between the particles of fire adhering to quick-lime, and those of high rectified spirit of wine, which is of so igneous a nature, as to be totally inflammable; yet I have not found, that the affusion of alkaol of wine upon quick-lime would produce any sensible incalescence, or any visible dissolution or dissipation of the lime, as common water would have done, though it seemed to be greedily enough soaked in by the lumps of lime. And I further tried, that, if on this lime so drenched I poured cold water, there ensued no manifest heat, nor did I so much as find the lump swelled, and thereby broken, till some hours after; which seems to argue, that the texture of the lime was such, as to admit the particles of the spirit of wine into some of its pores, which were either larger or more congruous, without admitting it into the most numerous ones, whereinto the liquor must be received, to be able suddenly to dissipate the corpuscles of lime into their minuter particles, into which (corpuscles) it seems, that the change, that the aqueous particles received by associating with the spirituous ones, made them far less fit to penetrate and move briskly there, than if they had entered alone.

I made also an experiment, that seems to favour our conjecture, by shewing, how much the disposition of lime to incalescence may depend upon an idoneous texture, and the experiment, as I find it, registered in one of my memorials, is this.

EXPERIMENT V.

[UPON quick-lime we put in a retort as much moderately strong spirit of wine, as would drench it, and swim a pretty way above it; and then distilling with a gentle fire, we drew off some spirit of wine much stronger than that, which had been put on, and then the phlegm following it, the fire was encreased, which brought over a good deal of phlegmatick strengthless liquor; by which one would have thought, that the quick-

lime

lime had been ſlacked; but when the remaining matter had been taken out of the retort, and ſuffered to cool, it appeared to have a fiery diſpoſition, that it had not before. For, if any lump of it, as big as a nutmeg, or an almond, was caſt into the water, it would hiſs as if a coal of fire had been plunged into the liquor, which was ſoon thereby ſenſibly heated. Nay, having kept divers lumps of this prepared calx well covered from the air for divers weeks, to try, whether it would retain this property, I found, as I expected, that the calx operated after the ſame manner, if not more powerfully. For ſometimes, eſpecially when it was reduced to ſmall pieces, it would upon its coming into the water make ſuch a briſk noiſe, as might almoſt paſs for a kind of exploſion.]

THESE phænomena ſeem to argue, that the diſpoſition, that lime has to grow hot with water, depends much on ſome peculiar texture, ſince the aqueous parts, that one would think capable of quenching all, or moſt of the atoms of fire, that are ſuppoſed to adhere to quick-lime, did not near ſo much weaken the diſpoſition of it to incaleſcence, as the acceſſion of the ſpirituous corpuſcles and their contexture, with thoſe of the lime, encreaſed that igneous diſpoſition. And that there might intervene ſuch an aſſociation, ſeems to me the more probable, not only becauſe much of the diſtilled liquor was as phlegmatick, as if it had been robbed of its more active parts, but becauſe I have ſometimes had ſpirit of wine come over with quick-lime not in unobſerved ſteams, but white fumes. To which I ſhall add, that beſides, that the taſte, and perhaps odour of the ſpirit of wine, is often manifeſtly changed by a well-made diſtillation from quick-lime; I have ſometimes found that liquor to give the lime a kind of alcalizate penetrancy, not to ſay fierineſs of taſte, that was very briſk and remarkable. But I will not undertake, that every experimenter, nor I myſelf, ſhall always make trials of this kind with the ſame ſucceſs, that I had in thoſe above recited, in regard, that I have found quick-limes to differ much, not only according to the degree of their calcination, and to their recentneſs, but alſo, and that eſpecially, according to the differing natures of the ſtones and other bodies calcined. Which obſervation engages me the more to propoſe what hath been hitherto delivered about quick-lime, as only narratives and a conjecture; which I now perceive has detained us ſo long, that I am obliged to haſten to the remaining experiments, and to be the more ſuccinct in delivering them.

EXPERIMENT VI.

AND it will be convenient to begin with an inſtance or two of the production of heat, wherein there appears not to intervene any thing in the part of the agent or patient, but local motion, and the natural effects of it. And as to this ſort of experiments, a little attention and reflection may make ſome familiar phænomenon appoſite to our preſent purpoſe. When, for example, a ſmith does haſtily hammer a nail, or ſuch like piece of iron, the hammered metal will grow exceeding hot, and yet there appears not any thing to make it ſo, ſave the forcible motion of the hammer, which impreſſes a vehement, and variouſly determined agitation of the ſmall parts of the iron; which being a cold body before, by that ſuperinduced commotion of its ſmall parts, becomes in divers ſenſes hot; firſt, in a more lax acceptation of the word in reference to ſome other bodies, in reſpect of whom it was cold before, and then ſenſibly hot; becauſe this newly gained agitation, ſurpaſſes that of the parts of our fingers. And in this inſtance, it is not to be overlooked, that oftentimes neither the hammer, by which, nor the anvil, on which a cold piece of iron is forged, (for all iron does not require precedent ignition to make it obey the hammer) continue cold, after the operation is ended; which ſhews, that the heat acquired by the forged piece of iron was not communicated by the hammer or anvil as heat, but produced in it by motion, which was great enough to put fo fmall a body, as the piece of iron, into a ftrong and confufed motion of its parts, without being able to have the like operation upon fo much greater maffes of metal, as the hammer and the anvil; though, if the percuffions were often and nimbly renewed, and the hammer were but fmall, this alfo might be heated, (though not fo foon, nor fo much, as the iron;) by which one may alfo take notice, that it is not neceffary, a body fhould be itfelf hot, to be calorifick. And now I fpeak of ftriking an iron with a hammer, I am put in mind of an obfervation, that feems to contradict, but does indeed confirm our theory: namely, that if a fomewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers ftrokes on the head before it grow hot; but when it is driven to the head, fo that it can go no further, a few ftrokes will fuffice to give it a confiderable heat; for whilft, at every blow of the hammer, the nail enters further and further into the wood, the motion, that is produced, is chiefly progreffive, and is of the whole nail tending one way; whereas, when that motion is ftopped, then the impulfe given by the ftroke, being unable either to drive the nail further on, or deftroy its intirenefs, muft be fpent in making a various vehement and inteftine commotion of the parts among themfelves, and in fuch an one we formerly obferved the nature of heat to confift.

EXPERIMENT VII.

In the foregoing experiment, the brifk agitation of the parts of a heated iron was made fenfible to the touch. I fhall now add one of the attempts, that I remember I made, to render it difcoverable to the eye itfelf. In order to this, and that I might alfo fhew, that not only a fenfible, but an intenfe degree of heat, may be produced in a piece of cold iron by local motion, I caufed a bar of that metal to be nimbly hammered by two or three lufty men, accuftomed to manage that inftrument; and thefe ftriking with as much force, and as little intermiffion, as they could, upon the iron, foon brought it to that degree of heat, that not only it was a great deal too hot to be fafely touched, but probably would, according to my defign, have kindled gun-powder, if that, which I was fain to make ufe of, had been of the beft fort: for, to the wonder of the by-ftanders, the iron kindled the fulphur of many of the grains of the corns of powder, and made them turn blue, though I do not well remember, that it made any of them go off.

EXPERIMENT VIII.

Besides the effects of manifeft and violent percuffions, fuch as thofe we have been taking notice of to be made with a hammer, there are among phænomena obvious enough, fome, that fhew the producibleneſs of heat, even in cold iron, by caufing an inteftine commotion of its parts: for we find, that, if a piece of iron, of a convenient fhape and bulk, be nimbly filed with a large rough file, a confiderable degree of heat will be quickly excited in thofe parts of the iron where the file paffes to and fro, the many prominent parts of the inftrument giving a multitude of ftrokes or pufhes to the parts of the iron, that happen to ftand in their way, and thereby making them put the neighbouring parts into a brifk and confufed motion, and fo into a ftate of heat. Nor can it be well objected, that, upon this account, the file itfelf ought to grow as hot as the iron, which yet it will not do; fince, to omit other anfwers, the whole body of the file being moved to and fro, the fame parts, that touch the iron this moment, pafs off the next; and, befides, have leifure to cool themfelves, by communicating their newly received agitation to the air, before they are brought to grate again upon the iron, which, being fuppofed to be held immoveable, receives almoft perpetual fhakes in the fame place.

We

WE find alſo, that attrition, if it be any thing vehement, is wont to produce heat in the ſolideſt bodies; as when the blade of a knife, being nimbly whetted, grows preſently hot. And if, having taken a braſs nail, and driven it as far as you can to the end of the ſtick, to keep it faſt, and gain a handle, you then ſtrongly rub the head to and fro againſt the floor, or a plank of wood, you may quickly find it to have acquired a heat intenſe enough to offend, if not burn one's fingers. And I remember, that going once, in exceeding hot weather, in a coach, which, for certain reaſons, we cauſed to be driven very faſt; the attrition of the nave of the wheel, againſt the axeltree, was ſo vehement, as obliged us to light out of the coach, to ſeek for water to cool the overchafed parts, and ſtop the growing miſchief the exceſſive heat had begun to do.

THE vulgar experiment, of ſtriking fire with a flint and ſteel, ſufficiently declares, what a heat, in a trice, may be produced in cold bodies by percuſſion, or colliſion; the latter of which ſeems but mutual percuſſion.

BUT inſtances of the ſame ſort, with the reſt mentioned in the VI. experiment, being obvious enough, I ſhall forbear to multiply and inſiſt on them.

EXPERIMENT IX.

FOR the ſake of thoſe, that think the attrition of contiguous air is neceſſary to the production of manifeſt heat, I thought, among other things, of the following experiment, and made trial of it.

WE took ſome hard black pitch, and having, in a baſon, porringer, or ſome ſuch veſſel, placed it a convenient diſtance under water, we caſt on it, with a good burning-glaſs, the ſun-beams, in ſuch a manner, that, notwithſtanding the refraction, that they ſuffered in the paſſage through the interpoſed water, the focus fell upon the pitch; wherein it would produce ſometimes bubbles, ſometimes ſmoke, and quickly communicated a degree of heat capable to make pitch melt, if not alſo to boil.

EXPERIMENT X.

THOUGH the firſt and ſecond experiments of Section I. ſhew, that a conſiderable degree of cold is produced by the diſſolution of ſal armoniac in common water; yet, by an additament, though but ſingle, the texture of it may be ſo altered, that, inſtead of cold, a notable degree of heat will be produced, if it be diſſolved in that liquor. For the manifeſtation of which, we deviſed the following experiment.

WE took quick-lime, and ſlaked it in common cold water, that all the igneous, or other particles, to which its power of heating that liquor is aſcribed, might be extracted and imbibed, and ſo the calx freed from them; then, on the remaining powder, freſh water was often poured, that all adhering reliques of ſalt might be waſhed off. After this, the thus dulcified calx, being again well dried, was mingled with an equal weight of powdered ſal armoniac, and having, with a ſtrong fire, melted the maſs, the mixture was poured out; and, being afterwards beaten to powder, having given it a competent time to grow cold, we put two or three ounces of it into a wide-mouthed glaſs; and pouring water upon it, within about a minute of an hour, the mixture grew warm, and quickly attained ſo intenſe a heat, that I could not hold the glaſs in my hand. And though this heat did not long laſt at the ſame height, it continued to be very ſenſible for a conſiderable time after.

EXPERIMENT XI.

TO confirm this experiment, by a notable variation, we took finely powdered ſal armoniac, and filings or ſcales of ſteel, and when they were very diligently mixed, (for that circumſtance ought to be obſerved,) we cauſed them to be gradually ſublimed in a glaſs veſſel, giving a ſmart fire towards the latter end. By this operation, ſo little of the mixture aſcended, that, as we deſired, far the greateſt part of the ſal armoniac ſtaid at the bottom with the metal; then, taking out the caput mortuum, I gave it time thoroughly to cool, but in a glaſs well ſtopped, that it might not imbibe the moiſture of the air, (as it is very apt to do.) And laſtly, though the filings of ſteel, as well as the ſal armoniac, were bodies actually cold, and ſo might be thought likely to increaſe, not check the coldneſs wont to be produced in water by that ſalt; yet, putting the mixture into common water, there enſued, as we expected, an intenſe degree of heat. And I remember, that, having ſublimed the forementioned ſalt in diſtinct veſſels, with the filings of ſteel, and with filings of copper, and, for curioſity-ſake, kept one of the caput mortuums (for I cannot certainly call to mind which of the two it was) divers months, (if I miſtake not, eight or nine,) we at length took it out of the veſſel, wherein it had been kept carefully ſtopped; and, upon trial, were not deceived in having expected, that all that while the diſpoſition to give cold water a notable degree of heat, was preſerved in it.

EXPERIMENT XII.

If experiments were made after the above recited manner with ſal armoniac and other mineral bodies than iron and copper, it is not improbable, that ſome of the emerging phænomena would be found to confirm what has been ſaid of the intereſt of texture, (and ſome few other mechanical affections) in the production of heat and cold. Which conjecture is ſomewhat favoured by the following trial. Three ounces of antimony, and an equal weight of ſal armoniac being diligently powdered and mixed, were, by degrees of fire, ſublimed in a glaſs veſſel, by which operation we obtained three differing ſubſtances, which we cauſed to be ſeparately powdered, when they were taken out of the ſubliming glaſs, leſt the air or time ſhould make any change in them; and having before put the ball of a good ſealed weather-glaſs for a while into water, that the ſpirit of wine might be brought to the temper of the external liquor, we put on a convenient quantity of the powdered Caput mortuum, which amounted to two ounces, and ſeemed to be little other than antimony, which accordingly did ſcarce ſenſibly raiſe the ſpirit of wine in the thermoſcope, though that were a tender one. Then laying aſide that water, and putting the inſtrument into freſh, of the ſame temper, we put to it a very yellow ſublimate, that aſcended higher than the other parts, and ſeemed to conſiſt of the more ſulphureous flowers of the antimony, with a mixture of the more volatile parts of the ſal armoniac. And this ſubſtance made the tincted ſpirit in the thermoſcope deſcend very ſlowly about a quarter of an inch; but when the inſtrument was put into freſh water of the ſame temper, and we had put in ſome of the powder of the lower ſort of ſublimate, which was dark-coloured, though both the antimony and ſal armoniac, it conſiſted of, had been long expoſed to the action of a ſubliming heat; yet the water was thereby ſpeedily and notably cooled, inſomuch, that the ſpirit of wine in the weather-glaſs haſtily deſcended, and continued to ſink, till, by our gueſs, it had fallen not much ſhort of three inches. Of theſe phænomena the ætiology, as ſome moderns call the theory, which propoſes the cauſes of things, is more eaſy to be found by a little conſideration, than to be made out in few words.

We made alſo an experiment like that above recited, by ſubliming three ounces a piece of minium and ſal armoniac; in which trial we found, that though, in the Caput mortuum, the ſalt had notably wrought upon the calx of lead, and was in part aſſociated with it, as appeared by the whiteneſs of the ſaid Caput mortuum, by its ſweetiſh taſte,

taſte, and by the weight (which exceeded, four drachms, that of all the minium;) yet a convenient quantity of this powdered mixture being put into water, wherein the former weather-glaſs had been kept a while, the tincted ſpirit of wine was not manifeſtly either raiſed or depreſſed. And when, in another glaſs, we proſecuted the trial with the ſal armoniac, that had been ſublimed from the minium, it did indeed make the ſpirit of wine deſcend, but ſcarce a quarter ſo much as it had been made to fall by the lately mentioned ſublimate of ſal armoniac and antimony.

EXPERIMENT XIII.

It is known, that many learned men, beſides ſeveral chemical writers, aſcribe the incaleſcences, that are met with in the diſſolution of metals, to a conflict ariſing from a certain antipathy or hoſtility, which they ſuppoſe between the acid ſalt of the one, and alcalizate ſalt, whether fixed or volatile, of the other. But ſince this doctrine ſuppoſes a hatred between inanimate bodies, in which it is hard to conceive, how there can be any true paſſions, and does not intelligibly declare, by what means their ſuppoſed hoſtility produces heat; it is not likely, that, for theſe and ſome other reaſons, inquiſitive naturaliſts will eaſily acquieſce in it. And on the other ſide it may be conſidered, whether it be not more probable, that heats, ſuddenly produced in mixtures, proceed either from a very quick and copious diffuſion of the parts of one body through thoſe of another, whereby both are confuſedly tumbled and put into a calorific motion; or from this, that the parts of the diſſolved body come to be every way, in great numbers, violently ſcattered; or from the fierce and confuſed ſhocks or juſtlings of the corpuſcles of the conflicting bodies, or maſſes, which may be ſuppoſed to have the motions of their parts differingly modified according to their reſpective natures: or from this, that, by the plentiful ingreſs of corpuſcles of the one into the almoſt commenſurate parts of the other, the motion of ſome etherial matter, that was wont before ſwiftly to permeate the diſtinct bodies, comes to be checked and diſturbed, and forced to either brandiſh or whirl about the parts in a confuſed manner, till it have ſettled itſelf a free paſſage through the new mixture, almoſt as the light does through divers troubled liquors and vitrified bodies, which, at length, it makes tranſparent. But, without here engaging in a ſolemn examination of the hypotheſis of alcali and acidum, and without determining whether any one, or more, of the newly mentioned mechanical cauſes, or whether ſome other, that I have not yet named, is to be entitled to the effect; it will not be impertinent to propoſe divers inſtances of the production of heat by the operation of one agent, oil of vitriol, that it may be conſidered whether it be likely, that this ſingle agent ſhould, upon the ſcore of antipathy, or that of its being an acid menſtruum, be able to produce an intenſe heat in many bodies of ſo differing natures as are ſome of thoſe, that we ſhall have occaſion to name. And now I proceed to the experiments themſelves.

Take ſome ounces of ſtrong oil of vitriol, and ſhaking it with three or four times its weight of common water, though both the liquors were cold, when they were put together, yet their mixture will, in a trice, grow intenſely hot, and continue conſiderably ſo for a good while. In this caſe it cannot probably be pretended by the chemiſts, that the heat ariſes from the conflict of the acid and alcalizate ſalts abounding in the two liquors, ſince the common water is ſuppoſed an elementary body devoid of all ſalts; and at leaſt, being an inſipid liquor, it will ſcarce be thought to have alcali enough to produce, by its re-action ſo intenſe a heat. That the heat emergent upon ſuch a mixture may be very great, when the quantities of the mingled liquors are conſiderably ſo, may be eaſily concluded from one of my memorials, wherein I find, that no more than two ounces of oil of vitriol being poured (but not all at once) into four ounces only of diſtilled rain-water, made and kept it manifeſtly warm for a pretty deal above an hour, and during no ſmall part of that time, kept it ſo hot, that it was troubleſome to be handled.

EXPERIMENT XIV.

THE former experiment brings into my mind one, that I mention, without teaching it in the hiſtory of cold, and it appeared very ſurpriſing to thoſe, that knew not the ground of it. For having ſometimes merrily propoſed to heat cold liquors with ice, the undertaking ſeemed extravagant, if not impoſſible, but was eaſily performed by taking out of a baſon of cold water, wherein divers fragments of ice were ſwimming, one or two pieces, that I perceived were well drenched with the liquor, and immerſing them ſuddenly into a wide-mouthed glaſs, wherein ſtrong oil of vitriol had been put; for this menſtruum, preſently mingling with the water, that adhered to the ice, produced in it a briſk heat, and that ſometimes with a manifeſt ſmoke, which nimbly diſſolved the contiguous parts of ice, and thoſe the next, and ſo the whole ice being ſpeedily reduced to water, and the corroſive menſtruum being, by two or three ſhakes, well diſperſed through it, and mingled with it, the whole mixture would grow, in a trice, ſo hot, that ſometimes the vial, that contained it, was not to be endured in one's hand.

EXPERIMENT XV.

NOTWITHSTANDING the vaſt difference betwixt common water and high rectified ſpirit of wine, whereof men generally take the former for the moſt contrary body to fire, and whereof the chemiſts take the latter to be but a kind of liquid ſulphur, ſince it may preſently be all reduced into flame; yet, as I expected, I found, upon trial, that oil of vitriol, being mingled with pure ſpirit of wine, would as well grow hot, as with common water. Nor does this experiment always require great quantities of the liquors. For when I took but one ounce of ſtrong oil of vitriol, though I put to it leſs than half an ounce of choice ſpirit of wine, yet thoſe two being lightly ſhaken together, did, in a trice, conceive ſo briſk a heat, that they almoſt filled the vial with fumes, and made it ſo hot, that I had, unawares, like to have burned my hand with it before I could lay it aſide.

I made the like trial with the ſame corroſive menſtruum, and common aqua vitæ, bought at a ſtrong-water-ſhop, by the mixture of which liquors heat was produced in the vial, that I could not well endure.

THE like ſucceſs I had in an experiment, wherein oil of vitriol was mixed with common brandy; ſave that in this the heat produced ſeemed not ſo intenſe as in the former trial, which itſelf afforded not ſo fierce a heat, as that, which was made with rectified ſpirit of wine.

EXPERIMENT XVI.

THOSE chemiſts, who conceive, that all the incaleſcences of bodies, upon their being mixed, proceed from their antipathy or hoſtility; will not perhaps expect, that the parts of the ſame body, (either numerically, or in ſpecie, as the ſchools phraſe it,) ſhould, and that without manifeſt conflict, grow very hot together. And yet having for trial's ſake put two ounces of colcothar ſo ſtrongly calcined, that it was burnt almoſt to blackneſs, into a retort, we poured upon it two ounces of ſtrong oil of Engliſh vitriol, and found, that after about a minute of an hour they began to grow ſo hot, that I could not endure to hold my hand to the bottom of the veſſel, to which the mixture gave a heat, that continued ſenſible on the outſide for between twenty and thirty minutes.

EXPERIMENT XVII.

THOUGH I have not obferved any liquor to equal oil of vitriol in the number of liquors, with which it will grow hot; yet I have not met with any liquor, wherewith it came to a greater incalefcence, than it frequently enough did with common oil of turpentine. For when we caufed divers ounces of each to be well fhaken together in a ftrong veffel, faftened, to prevent mifchief, to the end of a pole or ftaff; the ebullition was great and fierce enough to be not undefervedly admired by the fpectators. And this brings into my mind a pleafant adventure afforded by thefe liquors, of each of which, having for the production of heat and other purpofes, caufed a good bottle full to be put up with other things into a box, and fent down into the country, with a great charge, that care fhould be had of the glaffes; the waggon, in which the box was carried, happened, by a great jolt, that had almoft overturned it, to be fo rudely fhaken, that thefe glaffes were both broken, and the liquors, mingling in the box, made fuch a noife and ftink, and fent forth fuch quantities of fmoke by the vents, which the fumes had opened to themfelves, that the paffengers with great out-cries and much hafte threw themfelves out of the waggon, for fear of being burnt in it.

THE trials we made with oil of turpentine, when ftrong fpirit of nitre was fubftituted in the ftead of oil of vitriol, belong not to this place.

EXPERIMENT XVIII.

BUT though petroleum, efpecially when rectified, be, as I have elfewhere noted, a moft fubtile liquor, and the lighteft I have yet had occafion to try; yet to fhew you, how much the incalefcence of liquors may depend upon their texture, I fhall add, that having mixed by degrees one ounce of rectified petroleum, with an equal weight of ftrong oil of vitriol, the former liquor feemed to work upon the furface of this laft named, almoft like a menftruum, upon a metal, in numerous and fmall bubbles continually afcending for a while into the oleum petræ, which had its colour manifeftly altered and deepened by the operation of the fpirituous parts. But by all the action and reaction of thefe liquors, there was produced no fuch fmoking and boiling, or intenfe heat, as if oil of turpentine had been employed inftead of oil of vitriol; the change, which was produced, as to qualities, being but a kind of tepidnefs difcoverable by the touch.

ALMOST the like fuccefs we had in the conjunction of petroleum and fpirit of nitre; a more full account whereof may be elfewhere met with.

IN this, and the late trials, I did not care to make ufe of fpirit of falt, becaufe, at leaft, if it be but ordinarily ftrong, I found its operation on the liquors above-mentioned inconfiderable, (and fometimes perhaps fcarce fenfible) in comparifon of thofe of oil of vitriol, and in fome cafes of dephlegmed fpirit of nitre.

EXPERIMENT XIX.

EXPERIENCED chemifts will eafily believe, that it were not difficult to multiply inftances of heat producible by oil of vitriol upon folid bodies, efpecially mineral ones. For it is known, that, in the ufual preparation of vitriolum martis, there is a great effervefcence excited upon the affufion of the oil of vitriol upon filings of fteel, efpecially, if they be well drenched in common water. And it will fcarce be doubted, but that, as oil of vitriol will (at leaft partly) diffolve a great many, both calcined and teftaceous bodies, as I have tried with lime, oyfter-fhells, &c. fo it will, during the diffolution, grow fenfibly, if not intenfely hot with them, as I found it to do, both

with

with thoſe newly named, and others, as chalk, lapis calaminaris, &c. with the laſt of which, if the liquor be ſtrong, it will heat exceedingly.

EXPERIMENT XX.

WHEREFORE I will rather take notice of its operation upon vegetable, as bodies, which corroſive menſtruums have ſcarce been thought fit to diſſolve and grow hot with. To omit then cherries, and divers fruits abounding in watery juices, with which, perhaps on that very account, oil of vitriol will grow hot; I ſhall here take notice, that, for trial ſake, having mixed a convenient quantity of that liquor with raiſins of the ſun beaten in a mortar, the raiſins grew ſo hot, that, if I miſ-remember not, the glaſs that contained it, had almoſt burnt my hand.

THESE kind of heats may be alſo produced by the mixture of oil of vitriol with divers other vegetable ſubſtances; but, as far as I have obſerved, ſcarce ſo eminently with any dry body, as with the crumbs of white bread, (or even of brown,) with a little of which we have ſometimes produced a ſurpriſing degree of heat, with ſtrong or well-dephlegmed oil of vitriol, which is to be ſuppoſed to have been employed in the foregoing experiments, and all others mentioned to be made by the help of that menſtruum in our papers about qualities, unleſs it be in any particular caſe otherwiſe declared.

EXPERIMENT XXI.

IT is as little obſerved, that corroſive menſtruums are able to work, as ſuch, on the ſoft parts of dead animals, as on thoſe of vegetables; and yet I have, more than once, produced a notable heat, by mixing oil of vitriol with minced fleſh, whether roaſted or raw.

EXPERIMENT XXII.

THOUGH common ſea-ſalt does uſually impart ſome degree, though not an intenſe one, of coldneſs unto common water, during the act of diſſolution; yet ſome trials have informed me, that, if it were caſt into a competent quantity of oil of vitriol, there would, for the moſt part, enſue an incaleſcence, which yet did not appear to ſucceed ſo regularly, as in moſt of the foregoing experiments. But, that heat ſhould be produced uſually, though not perhaps conſtantly, by the above-named menſtruum and ſalt, ſeems therefore worthy of our notice, becauſe it is known to chemiſts, that common ſalt is one main ingredient of the few, that make up common factitious ſal armoniac, that is wont to be ſold in the ſhops. And I have been informed, that the excellent academians of *Florence* have obſerved, that oil of vitriol would not grow hot, but cold, by being put upon ſal armoniac: ſomething like which, I took notice of in rectified ſpirit of ſulphur, made *per campanam*, but found the effect much more conſiderable, when, according to the ingenious Florentine experiment, I made the trial with oil of vitriol; which liquor, having already furniſhed us with as many phænomena for our preſent purpoſe, as could be well expected from one agent, I ſhall ſcarce, in this paper about heat, make any farther uſe of it, but proceed to ſome other experiments, wherein it does not intervene.

EXPERIMENT XXIII.

WE took a good lump of common ſulphur, of a convenient ſhape, and, having rubbed or chafed it well, we found, as we expected, that, by this attrition, it grew ſenſibly warm; and, that there was an inteſtine agitation, which you know is local motion, made by this attrition, did appear not only by the newly mentioned heat, whoſe whofe nature confifts in motion, and by the antecedent preffure, which was fit to put the parts into a diforderly vibration, but alfo by the fulphureous fteams, which it was eafy to fmell, by holding the fulphur to one's nofe as foon as it had been rubbed. Which experiment, though it may feem trivial in itfelf, may be worth the confideration of thofe chemifts, who would derive all the fire and heat we meet with in fublunary bodies from fulphur. For, in our cafe, a mafs of fulphur, before its parts were put into a new and brifk motion, was fenfibly cold; and as foon as its parts were put into a greater agitation than thofe of a man's fingers, it grew fenfibly hot; which argues, that it was not by its bare prefence, or any emanative action, (as the fchools fpeak,) that the fulphur communicated any heat to my hand; and alfo, that, when it was brifkly moved, it did imprefs that quality, was no more than another folid body, though incombuftible as common glafs, would have done, if its parts had been likewife put into an agitation furpaffing that of my organs of feeling; fo that, in our experiment, fulphur itfelf was beholden, for its actual heat, to local motion, produced by external agents in its parts.

EXPERIMENT XXIV.

WE thought it not amifs to try, whether, when fal armoniac, that much infrigidates water, and quick-lime, which is known to heat it, were by the fire exquifitely mingled, the mixture would impart to the liquor a moderate or an intenfe degree of either of thofe qualities. In profecution of which enquiry, we took equal parts of fal armoniac and quick-lime, which we fluxed together, and putting an ounce, by guefs, of the powdered mixture into a vial, with a convenient quantity of cold water, we found, that the colliquated mafs did, in about a minute, ftrike fo great a heat through the glafs upon my hand, that I was glad to remove it haftily, for fear of being fcorched.

EXPERIMENT XXV.

WE have given feveral, and might have given many more, inftances of the incalefcence of mixtures, wherein both the ingredients were liquors, or, at leaft, one of them was a fluid body. But fometimes heat may alfo be produced by the mixture of two powders; fince it has been obferved, in the preparation of the butter or oil of antimony, that, if a fufficient quantity of beaten fublimate be very well mingled with powdered antimony, the mixture, after it has, for a competent time (which varies much according to circumftances, as the weather, veffel, place, &c. wherein the experiment is made,) ftood in the air, would fometimes grow manifeftly hot, and now and then fo intenfely fo, as to fend forth copious and fetid fumes, almoft as if it would take fire. There is another experiment, made by the help of antimony, and a pulverized body, wherein the mixture, after it had been for divers hours expofed to the air, vifibly afforded us mineral fumes. And to thefe I could add more confiderable, and perhaps fcarce credible inftances of bodies growing hot without liquors, if philanthropy did not forbid me. But, to return to our butter of antimony, it feems not unfit to be enquired, whether there do not unobfervedly intervene an aqueous moifture, which (capable of relaxing the falts, and fetting them a-work,) I therefore fufpected might be attracted (as men commonly fpeak) from the air, fince the mixture of the antimomy and the fublimate is prefcribed to be placed in cellars; and in fuch we find, that fublimate, or at leaft the faline part of it, is refolved *per deliquium*, (as they call it,) which is nothing but a folution made by the watery fteams wandering in the air.

EXPERIMENT XXVI.

I have formerly delivered ſome inſtances of the incaleſcence produced by water in bodies, that are readily diſſolved in it, as ſalt of tartar and quick-lime. But one would not lightly expect, that mere water ſhould produce an incaleſcence in ſolid bodies, that are generally granted to be inſoluble in it; and are not wont to be, at leaſt without length of time, viſibly wrought on by it; and yet trial has aſſured me, that a notable incaleſcence may be produced by common water in flower or fine powder of ſulphur, and filings of ſteel or iron. For when, in ſummer-time, I cauſed to be mingled a good quantity, (as half a pound, or rather a pound, of each of the ingredients,) and cauſed them to be thoroughly drenched with common water, in a convenient quantity whereof they were very well ſtirred up and down, and carefully mingled, the mixture would, in a ſhort time, perhaps leſs than an hour, grow ſo hot, that the veſſel, that contained it, could not be ſuffered in one's hand; and the heat was manifeſted to other ſenſes than the touch, by the ſtrong ſulphureous ſtink, that invaded the noſe, and the thick ſmoke, that aſcended out of the mixture, eſpecially, when it was ſtirred with a ſtick or ſpattle. Whether the ſucceſs will be the ſame at all times of the year, I do not know, and ſomewhat doubt, ſince I remember not, that I had occaſion to try it in other ſeaſons, than in ſummer, or in autumn.

EXPERIMENT XXVII.

IN the inſtances, that chemiſtry is wont to afford us of the heat produced by the action of menſtruums upon other bodies, there intervenes ſome liquor, properly ſo called, that wets the hands of thoſe, that touch it; and there are divers of the more judicious chemiſts, that join with the generality of the naturaliſts in denying, that quickſilver, which is indeed a fluid body, but not a moiſt and wetting one, in reference to us, will produce heat by its immediate action on any other body, and particularly on Gold. But, though I was long inclinable to their opinion, yet I cannot now be of it, ſeveral trials having aſſured me, that a mercury, whether afforded by metals and minerals, or impregnated by them, may, by its preparation, be enabled to inſinuate itſelf nimbly into the body of gold, whether calcined or crude, and become manifeſtly incaleſcent with it in leſs than two or three minutes of an hour.

EXPERIMENT XXVIII.

SINCE we know, that ſome natural ſalts, and eſpecially ſalt-petre, can produce a coldneſs in the water they are diſſolved in, I thought it might not be impertinent to our enquiry into heat and cold, and might perhaps alſo contribute ſomewhat to the diſcovery of the ſtructure of metals, and the ſalts, that corrode them, if ſolutions were made of ſome ſaliformed bodies, as chemiſts call them, that are made up of metalline and ſaline parts, and do ſo abound with the latter, that the whole concretions are, on their account, diſſoluble in common water.

OTHER experiments of this ſort belonging leſs to this place than to another, I ſhall here only, for example-ſake, take notice of one, that we made upon quickſilver, which is eſteemed the coldeſt of metals. For having, by diſtilling from it four times its weight of oil of vitriol, reduced it to a powder, which, on the account of the adhering ſalts of the menſtruum, that it detained, was white and gliſtering; we put this powder into a wide-mouthed glaſs of water, wherein a ſealed weather-glaſs had been left, before it began manifeſtly to heat the water, as appeared by the quick and conſiderable aſcent of the tincted ſpirit of wine, that continued to riſe, upon putting in more of the magiſtery;

which

which warm event is the more remarkable, becaufe of the obfervation of *Helmont*, that the falt, adhering to the mercury, corroded in good quantity by oil of vitriol, if it be wafhed off, and coagulated, becomes a kind of alom.

THE event of the former trial deferves the more notice, becaufe, having, after the fame manner, and with the fame weather-glafs made an experiment of common water, and the powder of vitriolum martis, made with oil of vitriol and the filings of fteel, the tincted fpirit of wine was not at all impelled up as before, but rather, after a while, began to fubfide, and fell though very flowly, about a quarter of an inch. The like experiment being tried with powdered fublimate in common water, the liquor in the thermofcope was fcarce at all fenfibly either raifed or depreffed, which argued the alteration, as to heat or cold, to have been either none, or very inconfiderable.

HAVING given warning at the beginning of this fection, that in it I aimed rather at offering various, than numerous experiments about the production of heat, I think, what has been already delivered may allow me to take leave of this fubject, without mentioning divers inftances, that I could eafily add, but think it fitter at prefent to omit. For thofe afforded me by trials about antiperiftafis belong to a paper on that fubject. Thofe, that might be offered about potential heat in human bodies, would, perchance, be thought but unneceffary, after what has been faid of potential coldnefs; from which an attentive confiderer may eafily gather what, according to our doctrine, is to be faid of the contrary quality. And divers phænomena, which would have been of the moft confiderable, I could have mentioned of the production of heat, fince in them that quality is the moft exalted, I referve for the title of combuftiblenefs and incombuftibility, having already fuffered this collection (or rather chaos) of particulars about the production of heat, to fwell to too great a bulk.

EXPERIMENTS and OBSERVATIONS

ABOUT THE

MECHANICAL PRODUCTION of TASTES.

TO make out the mechanical origin, or production of fapors, as far as is neceffary for my prefent purpofe, it will be expedient to premife in general, that, according to our notion of taftes, they may depend upon the bignefs, figure, and motion of the faporifick corpufcles, confidered feparately, and as the affections of fingle, and very minute particles of matter; or elfe in a ftate of conjunction, as two or more of thefe affections, and the particles they belong to, may be combined or affociated, either among themfelves, or with other particles, that were not faporous before. And as thefe coalitions, and other affociations come to be diverfified; fo the taftes, refulting from them, will be altered or deftroyed.

BUT, to handle thefe diftinctly, and fully, were a tafk not only too difficult and long, but improper in this place, where I pretend to deliver not fpeculations, but matters of fact: in fetting down whereof, neverthelefs, to avoid too much confufion, I am content, where I can do it readily and conveniently, in fome of my trials, to couch fuch references, as may beft point at thofe heads, whence the mechanical explications may be derived, and confequently our doctrine confirmed.

By taste considered, as belonging to the object, (under which notion I here treat of it,) I mean, that quality, or whatever else it be, which enables a body, by its operation, to produce in us that sensation, which we feel, or perceive, when we say we taste.

That this something, whether you will call it a quality, or whatever else it be, that makes, or denominates an object saporous, or rather (if I may be allowed a barbarous term) saporifick, may so depend upon the shape, size, motion, and other mechanical affections of the small parts of the tasted body, and result from the association of two or more of them, not excluding their congruity, or incongruity to the organs of tasting, may be made probable by the following instances.

EXPERIMENT I.

To divide a body, almost insipid, into two bodies of very strong, and very differing tastes.

It is observed, that salt-petre refined, and by that purification freed from the sea-salt, that is wont to be mingled with it, does rather cool the tongue, than make any great saporifick impressions on it. And though I will not say, that it is, as some have thought, an insipid body; yet the bitterishness, which seems to be its proper taste, is but very faint and languid. And yet this almost insipid body, being distilled by the way of inflammation, (which I elsewhere teach,) or even by the help of an additament of such clay, as is itself a tasteless body, will afford a nitrous spirit, that is extremely sharp or corrosive upon the tongue, and will dissolve several metals themselves, and a fixed salt, that is likewise very strongly tasted, but of a taste altogether different from that of the spirit, that is extremely sharp or corrosive upon the tongue; and accordingly, this salt will dissolve divers compact bodies, that the other will not work on, and will precipitate divers metals, and other concretes, out of those solutions, that have been made of them by the spirit.

EXPERIMENT II.

Of two bodies, the one highly acid and corrosive, and the other alkalizate and fiery, to produce a body almost insipid.

This may be performed by the way I have elsewhere mentioned of composing salt-petre. For, if upon a liquor of fixed nitre, made *per deliquium*, you warily drop good spirit of nitre, till it be just enough to satiate the alkaly, (for if there be too much, or too little, the experiment may miscarry,) we may, by a gentle evaporation, and sometimes without it, and that in a few minutes, obtain crystals, which, being dried after they have been, if it be needful, freed from any adhering particles, (not of their own nature,) will have upon the tongue, neither a sharp nor an alkalizate taste, but that faint, and scarce sensible bitterness, that belongs to salt-petre, if it be pure salt-petre; for the impure may, perhaps, strongly relish of the common salt, that is usually contained in it.

The like production of salt-petre we have sometimes made in far less time, and sometimes indeed in a trice, by substituting, instead of the fixed salt of nitre, the saline parts of good pot-ashes, carefully freed by solution and filtration from the earthy and feculent ones.

I have sometimes considered, whether the phænomena of these two experiments may not be explicated, by supposing them to arise from the new magnitudes and figures of the particles, which the fire, by breaking them, or forcibly rubbing them one against the other, or also against the corpuscles of the additament, may be presumed to give them; as if, for example, since we find the larger and best formed crystals of nitre to be of a prismatical shape with six sides, we should suppose the corpuscles of nitre to be be little prifms, whofe angles and ends are too obtufe or blunt to make vigorous and deep impreffions on the tongue; and yet, if thefe little prifms be by a violent heat fplit, or otherwife broken, or forcibly made, as it were, to grind one another, they may come to have parts fo much fmaller than before, and endowed with fuch fharp fides, and angles, that, being diffolved and agitated by the fpittle, that ufually moiftens the tongue, their fmallnefs may give them great accefs to the pores of that organ, and the fharpnefs of their fides and points may fit them to ftab and cut, and perhaps, fear the nervous and membranous parts of the organ of tafte, and that varioufly, according to the grand diverfities, as to fhape and bulk of the faporifick particles themfelves. And this being granted, it feemed further conceivable, that when the alkalizate and acid particles come to be put together in the fluid mixture, wherein they fwam, many of them might, after a multitude of various juftlings and occurfions, meet with one another fo luckily and opportunely, as to re-compofe little prifms, or convene into other bodies, almoft like thofe, that made up the cryftals of nitre, before it was expofed to the fire. To illuftrate which, we may conceive, that, though a prifm of iron may be fo fhaped, that it will be wholly unfit to pierce the fkin; yet it may be fo cut by tranfverfe planes reaching to the oppofite bafes or ends, as to afford wedges, which, by the fharpnefs of their edges, may be fit both to cleave wood, and cut the fkin; and thefe wedges, being again put together, after a requifite manner, may re-compofe a prifm, whofe extremes fhall be too blunt to be fit for the former ufe. This may be alfo illuftrated by the breaking of a dry ftick, circularly cut off at the ends, which, though it is unapt, whilft entire, and of that bulk, to prick the hand; yet if it be violently broken, the ragged ends of it, and the fplinters, may prove ftiff, flender, and fharp enough to pierce and run into the hand: to which divers other fuch mechanical illuftrations might be added. But, fince I fear you think, as well as I, the main conjecture may not be worthy any farther profecution, I fhall not infift any longer on it. And becaufe the hiftorical part of thefe experiments was for the main delivered by me already in the effay about the analyfis and redintegration of nitre, I fhall now proceed to other trials.

EXPERIMENT III.

Of two bodies, the one extremely bitter, and the other exceeding falt, to make an infipid mixture.

To make this experiment, we muft very warily pour upon cryftals made of filver, diffolved in good aqua fortis or fpirit of nitre, ftrong brine made of common falt and water. For the mixture of thefe two being dried, and afterwards brought to fufion in a crucible, and kept a competent while in that ftate, will afford a tough mafs, the chemifts call luna cornea, which you may lick divers times, and fcarce judge it other than infipid; nor will it eafily be brought to diffolve in much more piercing menftruums than our fpittle, as I have elfewhere fhewn.

EXPERIMENT IV.

Of two bodies, the one extremely fweet, and the other falter than the ftrongeft brine, to make an infipid mixture.

THE doing of this requires fome fkill and much warinefs in the experimenter, who, to perform it well, muft take a ftrong folution of minium, made with an appropriated menftruum, as good fpirit of vinegar, or elfe faccharum faturni itfelf, diffolved in a convenient vehicle; and then muft have great care and caution to put to it, by degrees, a juft proportion of ftrong fpirit of fal armoniac, or the like urinous fpirit, till the whole

whole be precipitated; and if the two former taftes are not fufficiently deftroyed in the mixture, it may be dried and fluxed, as was above directed about luna cornea.

EXPERIMENT V.

Of an infipid body and a four one, to make a fubftance more bitter than gall or aloes.

THIS is eafily performed by diffolving in ftrong fpirit of nitre or good aqua-fortis as much pure filver as the menftruum will take up; for this folution, being filtrated, has been often efteemed more bitter than fo much gall or wormwood, or any other of thofe fimples, that have been famous for that quality: and if the fuperfluous moifture be abftracted, you may by coagulation obtain cryftals of luna, that have been judged more ftrongly bitter than the folution itfelf. And that the corpufcles of thefe cryftals fhould leave a far more lafting tafte of themfelves, than the above-mentioned bitter bodies are wont to do, will not feem fo marvellous, as I remember fome, that tried, have complained; if we take notice, how deep the particles of thefe cryftals may pierce into the fpungy organs of tafte, fince, if one does but touch the pulp or nail of one's finger, (firft a little wetted with fpittle or otherwife,) with the powder of thefe cryftals, they will fo penetrate the fkin or nail, and ftick fo faft there, that you cannot in a reafonable time wafh the ftain off of the fkin, and much lefs off of the nail, but it will continue to appear many hours on the former, and many days on the other.

EXPERIMENT VI.

Of an infipid body and a highly corrofive one, to make a fubftance as fweet as fugar.

THIS is eafily done, by putting upon good minium purified aqua-fortis or fpirit of nitre, and letting them work upon one another in a gentle heat, till the liquor have diffolved its full proportion of the metal. For then, if the ingredients were good, and the operation rightly performed, the menftruum would have a fweetnefs like that of ordinary faccharum faturni. But it was not for nothing, that I intimated, the ingredients fhould be alfo pure and good in their kind; for, if the minium be adulterated, as often it is, or the fpirit of nitre or aqua fortis be mingled, as it is ufual before it be purged, with fpirit of common falt, or other unfit ingredients, the operation may be fuccefslefs, as I have more than once obferved.

EXPERIMENT VII.

Of obtaining, without addition from the fweeteft bodies, liquors corrofive enough to diffolve metals.

IF fugar be put into a fufficiently capacious retort, and warily diftilled, (for otherwife it will be apt to break the veffel) it will afford, among other things, a copious red fpirit, which, being flowly rectified, will lofe its colour, and come over clear. The *caput mortuum* of the fugar, which I have more than once had of an odd contexture, may be found either almoft or altogether infipid. And though the fpirit will be of a very penetrant tafte, yet it will be very far from any kind of fweetnefs; and though that liquor be thought to be homogeneous, and to be one of the principles of the analized fugar, yet (as I have elfewhere fhewn) I found it to be a mixture of two fpirits; with the one of which, befides bodies of a lefs clofe texture, I diffolved (even in the cold) crude copper, as was eafy to be feen by the deep and lovely colour of the folution. And to thefe four fpirits, afforded by fugar itfelf, we have reftored a kind of faccharine fweetnefs, by compounding them with the particles of fo infipid a body as minium; part of which they will in digeftion diffolve. A like fpirit to that diftilled from fugar may be obtained from honey; but in regard of its aptnefs to fwell exceedingly, chemifts are not wont to diftil it without fand, brick, or fome other additament.

EXPERIMENT VIII.

To divide a body, bitter in the highest degree, into two substances, the one extremely sour, and the other perfectly insipid.

THIS is easily done by putting some fine crystals of *Luna* into a good retort, and then distilling them in a sand-furnace, capable of giving them so strong a fire, as to drive away all the spirits from the silver. For, this remaining behind, according to its metalline nature, will be insipid, and the spirits, that are driven away from it, will unite in the receiver into an acid and corrosive menstruum.

EXPERIMENT IX.

To produce variety of tastes in one insipid body, by associating it with divers menstruums.

As this operation may, upon the account I elsewhere mention, be serviceable to investigate the figures c. the particles of dissolved metals and other bodies; so it is very fit to manifest, what we would here have it shew, how much taste may be diversified by, and consequently depend upon texture; since a body, that has no taste, may, in conjunction with sapid bodies, give them strong tastes, all differing from one another, and each of them from that, which the saporous bodies had before. I could propose divers ways of bringing this to trial, there being several insipid bodies, which I have found this way diversifiable. But because I remember not, that I have met with any mineral, that is dissoluble by near so many saline menstruums, as zink, I look on that, as the fertile subject to afford instances to our present purpose. For I have found, that it will be dissolved, not only by aqua-fortis, aqua-regis, oil of vitriol, spirit of nitre, spirit of salt, and other mineral menstruums, but also by vegetable spirits, as distilled vinegar, and by animal ones too, as spirit of sal armoniac; though the one be acid, and the other urinous. And if the several solutions, which may be made of this mineral, by so many differing liquors, be compared, the number of their differing tastes will suffice to make good the title of the experiment.

EXPERIMENT X.

To produce variety of tastes with one menstruum, by associating it with insipid bodies.

THIS proposition a mathematician would go near to call the converse of the foregoing; and as it may serve, as well as that, to discover the structure of the minute parts of divers metalline and mineral bodies; so it may not only as well, but better than that, serve us to illustrate the corpuscularian doctrine of tastes; by shewing us, that a single, and, as far as chemistry teaches us, a simple body, endowed with a peculiar taste, may, by being compounded with others, each of them insipid of itself, produce a considerable number of differing tastes. There may be more instruments than one made use of in this trial; but of those, that are known, and we may easily obtain, the most proper are spirit of nitre, and good aqua-fortis: for that, with refined silver, will make a solution bitter as gall; with lead, it will be of a saccharine sweetness; with that part of tin, which it will keep dissolved, (for the greatest it is wont but to corrode and precipitate) it produces a taste very distant from both the former, but not odious; with copper, it affords an abominable taste; with mercury and iron, it affords other kinds of bad tastes. Nor are metals the only mineral bodies it will work upon; for, it will dissolve tin-glass, antimony, brass; to which I could add emery, zink, and other bodies, whereon I have tried it. All which together will make up no despicable number of differing tastes.

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EXPERIMENT XI.

Of two liquors, the one highly corrosive, and the other very pungent and not pleasant, to compose a body of a pleasant and aromatick taste.

THIS experiment, which I elsewhere mention to other purposes, does in some regards better suit our present design, than most of the foregoing; since here the corrosive menstruum is neither mortified by fixed nor urinous salts, supposed to be of a contrary nature to it; nor yet, as it were, tired out nor disarmed by corroding of metals or other solid bodies. The experiment being somewhat dangerous to make at first in great, it may suffice for our present turn, to make it in the less quantity, as follows.

TAKE one ounce of strong spirit of nitre, or of very good aqua fortis itself, and put to it by little and little, (which caution, if you neglect, you may soon repent it,) another ounce of such rectified spirit of wine, as, being kindled in a spoon, will flame all away: when these two liquors are well mixed, and grown cold again, you may, after some digestion, or, if haste require, without it, distil them totally over together, to unite them exquisitely into one liquor, in which, if the operation have been well performed, the corrosive particles of the salts will not only lose all their cutting acidity, wherewith they wounded the palate; but by their new composition with the vinous spirits, the liquor acquires a vinous taste, that is not only not acid or offensive, but very pleasing, as if it belonged to some new or unknown spice.

EXPERIMENT XII.

To imitate by art, and sometimes even in minerals, the peculiar tastes of natural bodies, and even vegetables.

THIS is not a fit place to declare, in what sense I do or do not admit of souls in vegetables, nor what I allow or deny to the seminal or plastick principle ascribed to plants: but perhaps it will not be erroneous to conceive, that, whatever be the agent in reference to those tastes, that are said to be specifick to this or that plant, that, on whose immediate account it is, or becomes of this or that nature, is a complication of mechanical affections, as shape, size, &c. in the particles of that matter, which is said to be endowed with such a specifick taste.

To illustrate this, I thought it expedient, to endeavour to imitate the taste of some natural bodies by artificial compositions or preparations, but found it not easy, beforehand, to be assured of the success of such trials: and therefore I shall content myself here to mention three or four instances, that, except the first, are rather observations than such experiments as we are speaking of.

I remember then, that, making some trials to alter the sensible qualities of smell, taste, &c. of oil of vitriol, and spirit of wine, I obtained from them, among other things, that suited with my design, a certain liquor, which, though at first pleasant, would, at a certain nick of time, make one, that had it in his mouth, think it had been imbued with garlick.

AND this brings into my mind, that a skilful person, famous for making good cyder, coming one day to advise with me, what he should do to heighten the taste of it, and make it keep the longer, complained to me, that having, among other trials, put into a good vessel full of juice of apples a certain proportion of mustard seed, with hopes it would make the cyder more spirituous and piquant, he found, to his wonder and loss, that, when he came to draw it, it stunk of garlick so rank, that every body rejected it.

I remember also, that, by fermenting a certain proportion (for that we found requisite) of *semen dauci* with beer or ale, the liquor had a very pleasant relish of lemon-pills.

I BUT

BUT that seems much more considerable, which I shall now add; that, with an insipid metal and a very corrosive menstruum, one may compound a taste, that I have several times observed to be so like a vegetable, that I presume it may deceive many. This may be done by dissolving gold, without any gross salt, in the mixture of aqua-fortis and the spirit of salt, or even in common aqua-regis, made by dissolving sal armoniac in aqua-fortis. For if the experiment be happily made, one may obtain either a solution or a salt, whose austere taste will very much resemble that of sloes, or of unripe bullace. And this taste, with some little variety, I found in gold dissolved without any distilled liquor at all; and also, if I much forget not, in gold, that by a peculiar menstruum I had volatilized.

THE last instance I shall give of the imitation of tastes, I found to have been, for the main, known to some ingenious ladies. But to make the experiment succeed very well, a due proportion is the principal circumstance, which is wont to be neglected. I cannot readily call to mind that, which I found to succeed best; but the trial may be indifferently well made after such a manner as this:

TAKE a pint or a pound of *Malaga* or *Canary* sack, (for though French, and the like wines, may serve the turn, yet they are not so proper;) and put into it a drachm or two of good odoriferous orrice roots, cut into thin slices, and let them infuse in the liquor a convenient time, till you perceive, that they have given it a desired taste and smell; then keep the thus perfumed wine exactly stopped in a cool place: according to which way, I remember, that (when I hit on the right proportion of ingredients, and kept them a due time in infusion) I had many years ago a wine, which, being coloured with cochineal, or some such tinging ingredient, was taken for good rasberry-wine, not only by ordinary persons, but, among others, by a couple of eminent physicians, one of whom pretended to an extraordinary criticalness of palate on such occasions; both of them wondering, how at such an unlikely time of the year, as I chose to present them that liquor among others, I could have such excellent rasberry-wine: some of which (to add that by the by) I found to preseve the specifick taste two or three years after it was made.

A short EXCURSION about some CHANGES made of TASTES by MATURATION.

IT will not perhaps be thought impertinent but rather necessary, to add a word or two on this occasion for their sakes, that think the maturation of fruits, and the changes of tastes, by which it is usually known, must needs be the effect of the vegetable soul of the plant. For, after the fruit is gathered, and so, by being no longer a part of the tree, does, according to the most common opinion, cease to be a part of the living plant, as a hand or a foot cut off is no more reckoned among the limbs of the man it belonged to; yet it is very possible, that some fruits may receive maturation, after they have been severed from the plants, that bore them. For, not to mention, that apples, gathered somewhat before the time, by lying in heaps, do usually obtain a mellowness, which seems to be a kind or degree of maturation; or that medlars, gathered whilst they are hard and harsh, do become afterwards in process of time soft and better tasted; in which state, though some say they are rotten, yet others think that supposed rottenness is the proper maturity of that kind of fruit: not to mention these, I say, or the like instances, it is a famous assertion of several writers of the Indian affairs, that the fruit they call bananas: is usually gathered green, and hung up in bunches or clusters in the house, whereby they ripen by degrees, and have an advantageous change made both of their colour and of their taste. And this an ancient acquaintance of mine, a literate and obſerving perſon, of whom I enquired about it, aſſured me, he had himſelf lately tried and found to be true in *America*. And indeed I ſee not, why a convenient degree of warmth, whether external from the ſun and fire, or internal from ſome degree of fermentation or analogous inteſtine commotion, may not (whether the fruit be united to the plant or no) put the ſaporifick corpuſcles into motion, and make them, by various and inſenſible tranſcurſions, rub againſt each other, and thereby make the little bodies more ſlender or thin, and leſs rigid, or cutting and harſh, than they were before, and by various motions bring the fruit they compoſe to a ſtate, wherein it is more ſoft in point of conſiſtence, and abound in corpuſcles leſs harſh and more pliable, than they were before, and more congruous to the pores of the organ of taſte; and, in a word, make ſuch a change in the conſtitution of the fruit, as men are wont to expreſs by the name of Maturity. And that ſuch mechanical changes of texture may much alter the qualities, and among them the taſte of a fruit, is obvious in bruiſed cherries and apples, which in the bruiſed parts ſoon come to look and taſte otherwiſe than they did before. The poſſibility of this is alſo obvious by wardens, when ſlowly roaſted in embers, with ſo gentle a fire, as not to burn off the paper they are wont to be wrapped in, to be kept clean from the aſhes. And I have ſeen in the bordering country between *France* and *Savoy*, a ſort of pears, (whoſe name I now remember not,) which being kept for ſome hours in a moderate heat, in a veſſel exactly cloſed, with embers and aſhes above and beneath them, will be reduced to a juicy ſubſtance of a lovely red colour, and very ſweet and luſcious to the taſte. Many other ſorts of fruit in other countries, if they were handled after the ſame way, or otherwiſe ſkilfully wrought on by a moderate heat, would admit as great alterations in point of taſte. Neither is that ſort of pear to be here omitted, which by mere compreſſion, duly ordered, without external heat, will in a few minutes be brought to exchange it's former hardneſs and harſhneſs for ſo yielding a contexture and pleaſant a taſte, as I could not but think very remarkable. And that even more ſolid and ſtubborn ſalts, than thoſe of vegetables, may have the ſharpneſs and piercingneſs of their taſtes very much taken off by the bare internal action of one part upon the other, without the addition of any ſweetening body, I have been induced to think by having found, upon trial, that, by the help of inſipid water, we may, without any violence of fire, reduce ſea-ſalt into a brine of ſo mild and peculiar (I had almoſt ſaid) pleaſant a taſte, that one would ſcarce ſuſpect what it had been, or believe, that ſo great a change of a mineral body could be effected by ſo ſlight an inteſtine commotion, as indeed produced it; eſpecially, ſince the alteration of taſtes was not the moſt conſiderable, that was produced by this operation.

As to liquors, that come from vegetables, the emerging of new ſapors upon the inteſtine commotion of the ſaporifick parts, as conſequences of ſuch commotions, is more obvious than is commonly conſidered in the juice of grapes, which, from a ſweet and ſpiritleſs liquor, do by that internal motion, we call fermentation, acquire that pleaſing pungency and briſkneſs of taſte, that belongs to wine, and afterwards degenerates into that acid and cutting taſte, that is proper to vinegar; and all this, by a change of conſtitution made by the action of the parts themſelves on one another, without the help of any external additament.

EXPERIMENTS

EXPERIMENTS and OBSERVATIONS

ABOUT THE

MECHANICAL PRODUCTION of ODOURS.

SINCE taftes and odours (perhaps by reafon of the nearnefs of the organs they affect) are wont, by phyfical writers, to be treated of next to one another, I alfo fhall imitate them in handling thofe two qualities, not only for the intimated reafon, but becaufe, what I have premifed in general, and fome other things, that I have faid already under the title of taftes, being applicable to odours alfo, it will not be neceffary, and therefore it would be tedious, to repeat them here.

EXPERIMENT I.

With two bodies, neither of them odorous, to produce immediately a ftrong urinous fmell.

TAKE good quick-lime and fal armoniac, and rub or grind them well together, and holding your nofe to the mixture, you will be faluted with an urinous fmell produced by the particles of the volatile falt, untied by this operation, which will alfo invade your eyes, and make them to water.

EXPERIMENT II.

By the bare addition of common water, to produce immediately a very ftrong fmell in a body, that had no fuch fmell before.

THIS is one of the phænomena of an experiment made with camphire and oil of vitriol, which I have elfewhere mentioned to another purpofe. For, if in that corrofive menftruum you diffolve a good proportion, but not too much, of the ftrongly fcented gum, the odour of the camphire will be quite concealed in the mixture; but if you pour this mixture into a good quantity of fair water, the diffolved gum will immediately recover out of the menftruum, and fmell as ftrong as before, if not (by reafon of the warmth produced in the operation) more ftrongly.

EXPERIMENT III.

Of producing fome odours, each of them quite differing from that of any of the ingredients.

HAVING taken two ounces, or parts of clear oil of turpentine, and mixed it with one ounce, or part, of oil of vitriol, (which muft be done by degrees, for otherwife the veffel will be endangered,) the clear liquor, that came over, upon the diftillation of the mixture in a fand-furnace, inftead of the odour of turpentine, (for the oil of vitriol alone is wont to be inodorous,) fmelt very ftrong of fulphur; infomuch, that once, when I fhewed this experiment, approaching my nofe very boldly and haftily to the receiver newly fevered from the retort, the fulphureous ftink proved fo ftrong, that it had almoft (to fpeak with the vulgar) taken away my breath. And to illuftrate yet farther the poffible emergency of fuch odours upon the mixture of ingredients, as neither of them was apart endowed with, we caufed the fubftance, that remained behind in the retort (in the form of a thin extract) after one of the newly mentioned diftillations to be farther preffed by a ftronger fire, which forced moft of it over, partly in the form of a thick oil, and partly in that of butter; both which we keep together in the fame vial, becaufe their odour is neither that of oil of turpentine, nor that of brimftone, but they fmell exceedingly like the diftilled oil of bees-wax.

EXPERIMENT IV.

About the production of some odours by local motion.

I shall not now examine, whether the local motion of an external agent may not, without materially concurring to the operation, produce, by agitating and shuffling the parts, odorous corpuscles: but that the celerity and other modifications of the local motion of the effluvia of bodies may not only serve to diversify their odours, but so far produce them, as to make them perceptible by the sense, which otherwise would not be so, may be gathered from some observations, which, being obvious, are not so proper for this place. Wherefore I shall rather take notice, that I know several bodies, that are not only inodorous when cold, but when considerably hot, and are fixed in the fire, and yet, by having their parts put into a peculiar kind of agitation, will presently grow plainly odorous. On this occasion I shall add, that, as there are some very hard woods, that acquire a strong smell by the motion they may be exposed to in a turner's lath, (as I have observed by trials particularly made with the hard and ponderous lignum vitæ,) so some afford, whilst the operation lasts, an unexpected odour. And having enquired about this matter of two eminent artists, (whom I often employ) concerning the odour of beechwood, whilst it is turning, they both agreed, that it would emit well-scented effluviums. And one of them affirmed to me farther, that, having bought a great block of that wood, to make divers pieces of workmanship with it, when he came to turn it, there would issue out not only a copious odour, but of such a peculiar fragrancy, that one, that knew not whence it proceeded, would have concluded he was smelling roses.

EXPERIMENT V.

By mixing a good proportion of a very strongly scented body with an almost inodorous one, to deprive it speedily of all its smell.

TAKE salt of tartar, and drop upon it either spirit of nitre, or aqua fortis, not too much dephlegmed, till all the effervescence cease, and the liquor will no longer work upon the alcali. These, by a slow evaporation of the superfluous moisture, may be made to shoot into crystals, like those of nitre, which, after you have (if need be) by rubbing them with a dried cloth, freed them from loose adhering corpuscles, will emulate salt-petre, as in other qualities, so in its not being odorous; though, if you distil them, or burn them on kindled coals, their fumes will quickly make you sensible, that they abounded with the stinking spirits, that make aqua fortis so offensive to the smell.

EXPERIMENT VI.

By putting a very strongly stinking body to another of a not sweet smell, to produce a mixture of a pleasant and strongly aromatic odour.

WHAT is here proposed is performed at the same time, that the eleventh of the foregoing experiments of tastes is made. For the liquor thereby produced, if it be well prepared, has not only a spicy taste, but also a kind of aromatic and pleasant smell; and I have some now by me, that, though kept not over carefully, does, after some years, retain much of its former odour, though not so much as of its taste.

EXPERIMENT VII.

By digesting two bodies, neither of them well scented, to produce bodies of a very subtile and strongly fragrant odour.

WE took a pound (for instance) of Spanish wine, and put to it some ounces of oil of vitriol; then, keeping them for a reasonable time in digestion, we obtained, as we we expected, a mixture odoriferous enough. But this trial you will find improved by that, which ensues.

EXPERIMENT VIII.

By the bare addition of a body almost inodorous, and not well scented, to give a pleasant and aromatic smell to spirit of wine.

THIS we have several times done, by the ways elsewhere related for another scope, the sum of which, as far as it needs be mentioned in this place, is this:

WE took good oil of blue vitriol (that was brought from *Dantzic*) though the very common will serve well, and having put to it, by degrees, an equal weight of spirit of wine totally inflammable, we digested them together, for two, three, or four weeks, (sometimes much longer, and then with better success;) from which, when we came to distil the mixture, we had a very fragrant spirit, which was sometimes so subtile, that, though distilled in a tall glass with a gentle heat, it would (in spite of our care to secure the closeness of the vessels at the junctures) pierce through, and fill the laboratory with a perfume, which, though men could not guess what body afforded it, yet they could not but wonder at it. Whence we may learn, both how much those spirituous and inflammable particles, the chemists call the vegetable sulphur of wine, may work on and enoble a mineral sulphur; (for, that such an one there is in oil of vitriol, I have elsewhere proved by experience;) and how much the new commistions and contextures, made by digestion, may alter the odours of bodies, whether vegetable or mineral. That also another constitution of the same matter, without any manifest addition or recess of particles, may proceed to exhibit a very differing smell, will appear by the following trial.

EXPERIMENT IX.

To make the formentioned fragrant body, without addition or fire, degenerate into the rank smell of garlick.

To make out this, I need only relate, that I have more than once put the above mentioned fragrant liquor in stopped glasses, whereof the one, and not the other, stood in a warm place, till, in process of time, I found that odoriferous liquor so to degenerate in point of scent, that one would have thought it to have been strongly infected with garlick. And the like unpleasant smell I observed in a certain oil made of vegetable and mineral substances distilled together.

AND on this occasion I will add, (though not as an argument,) this observation, which though I shall not undertake it will always succeed, I think may not impertinently be set down in this place, partly because of the likeness of the odour produced, to that, which was the effect of the last named trial; and partly (or rather chiefly) because it may shew us, that a body, which itself is not only inodorous, but very fixed, may yet, in some cases, have a great stroke in the phænomena of odours; whether by being wrought on by, and sometimes mingled with, the parts of the odorous body, and thereby giving it a new modification, I shall not now stay to enquire.

WE took then good salt of tartar, and put to it, several times, its weight of the expressed juice of onions; we kept them in a light digestion for a day or two, and then unstopping the vial, we found the former smell of the onions quite degenerated into a rank smell of garlick, as was judged, even when fresh juice of garlick was procured to compare them. To vary this experiment, we made, with fixed salts, and some other strongly scented juices, trials, whose events it would perhaps be tedious here to relate.

EXPERIMENT X.

With an inodorous body, and another not well ſcented, to produce a muſky ſmell.

THIS we have ſometimes done by caſting into ſpirit (not oil) of vitriol a large proportion of ſmall pearls unbroken. For the action of the acid menſtruum upon theſe being moderated, partly by the weakneſs of the menſtruum, and partly by the entireneſs of the pearls, the diſſolution would ſometimes laſt many hours. Holding, from time to time, my noſe to the open orifice of the glaſs, it was eaſy to perceive a pleaſant muſky ſmell, which alſo others, to whom I mentioned it, took notice of, as well as I. And, if I miſremember not, I took notice of the like ſmell, upon pearls not only diſſolved in ſpirit of vinegar, but in another liquor, that had but a bad ſcent of its own. The foregoing experiment calls to my mind that, which follows.

EXPERIMENT XI.

With fixed metals, and bodies either inodorous or ſtinking, to produce ſtrong and pleaſant ſmells, like thoſe of ſome vegetables and minerals.

THAT gold is too fixed a body to emit any odour, and that aqua regis has an odour, that is very ſtrong and offenſive, I think will be eaſily granted. But yet aurum fulminans being made (as it is known) by precipitating, with the inodorous oil of tartar, the ſolution made of the former in the latter, and this precipitate being to be farther proceeded with in order to another experiment; we fulminated it *per ſe* in a ſilver veſſel like that, but better contrived, that is (if I miſremember not) ſomewhere deſcribed by *Glauberus*. And among other phænomena of this operation, that belong not to this place, we obſerved with pleaſure, that, when the fulmination was recently made, the ſteams, which were afforded by the metal, that had been fired, were endowed with a delightful ſmell, not unlike that of muſk. From which experiment, and the foregoing, we may learn, that art, by lucky contextures, may imitate the odours that are preſumed to be natural and ſpecifick; and that mineral and vegetable ſubſtances may compound a ſmell, that is thought to be peculiar to animals.

AND as art ſometimes imitates nature in the production of odours, as may be confirmed by what is above related concerning counterfeit raſberry wine, wherein thoſe, that drank it, believed they did not only taſte, but ſmell the raſberry; ſo ſometimes nature ſeems to imitate herſelf, in giving like odours to bodies extremely differing. For, not yet to diſmiſs the ſmell of muſk, there is a certain ſeed, which, for the affinity of its odour to that perfume, they call the muſk-ſeed; and, indeed, having ſome of it preſented me by a gentleman, that had newly brought it from the *Weſt-Indies*, I found it, whilſt it was freſh, to have a fragrancy ſuitable to the name, that was given it. There is alſo a ſort of rats in *Muſcovy*, whoſe ſkins, whereof I have ſeen ſeveral, have a ſmell, that has procured them the name of muſk-rats. To which I know not whether we may not add the mention of a certain ſort of ducks, which ſome call muſk-ducks, becauſe at a certain ſeaſon of the year, if they be chafed by violent motion, they will, under the wing, emit a muſky inſtead of a ſweaty ſcent; which, upon trial, I perceived to be true. On the other ſide, I have known a certain wood, growing in the *Indies*, which, eſpecially when the ſcent is excited by rubbing, ſtinks ſo rankly, and ſo like *Paracelſus*'s zibetum occidentale, (ſtercus humanum) that one would ſwear it were held under his noſe. And ſince I have been ſpeaking of good ſcents, produced by unlikely means, I ſhall not pretermit this obſervation, that, though generally the fire impreſſes a ſtrong offenſive ſmell, which chemiſts therefore call empyreumatical, upon the odorous bodies, that it works ſtrongly on; yet the conſtitution of a body may be ſuch, that the new contexture, that is made of its parts, even by the violence of

See in the paper of taſtes, experiment xii.

of the fire, ſhall be fit to afford effluviums, rather agreeable to the organs of ſmelling, than any way offenſive. For I remember, that, having, for a certain purpoſe, diſtilled ſaccharum Saturni in a retort with a ſtrong fire, I then obtained, (for I dare not undertake for the like ſucceſs to every experiment,) beſides a piercing and empyreumatical liquor, that was driven over into the receiver, a good lump of a caput mortuum, of a grayiſh colour, which, notwithſtanding the ſtrong impreſſion it had received from the fire, was ſo far from having any empyreumatical ſcent, that it had a pleaſing one; and when it was broken, ſmelled almoſt like a fine cake new baked, and broken, whilſt yet warm. And as the fire, notwithſtanding the empyreuma it is wont to give to almoſt all the bodies it burns, may be reduced to confer a good ſmell on ſome of them, if they be fitted upon ſuch a contexture of their parts, to emit ſteams of ſuch a nature, (whatever were the efficient cauſe of ſuch a contexture;) ſo we obſerve in the muſk animal, that nature in that cat, or rather deer, (though it properly belong to neither kind,) produces muſk by ſuch a change, as is wont in other animals to produce a putrefactive ſtink. So that, provided a due conſtitution of parts be introduced into a portion of matter, it may, on that acoount, be endowed with noble and deſirable ſcents, or other qualities, though that conſtitution were introduced by ſuch unlikely means, as combuſtion and putrefaction themſelves. In confirmation of which, I ſhall ſubjoin, in the enſuing account, a notable, though caſual phænomenon, that occurred to a couple of virtuoſi of my acquaintance.

An eminent profeſſor of mathematicks affirmed to me, that, chancing one day in the heat of ſummer, with another mathematician, (who, I remember, was preſent, when this was told,) to paſs by a large dunghil, that was then in *Lincoln's-Inn Fields*, when they came to a certain diſtance from it, they were both of them ſurpriſed to meet with a very ſtrong ſmell of muſk, (occaſioned, probably, by a certain degree or a peculiar kind of putrefaction,) which each was for a while ſhy of taking notice of, for fear his companion ſhould have laughed at him for it; but, when they came much nearer the dunghil, that pleaſing ſmell was ſucceeded by a ſtink proper to ſuch a heap of excrements. This puts me in mind of adding, that, though the excrements of animals, and particularly their ſweat, are uſually fœtid; yet, that it is not the nature of an excrement, but the conſtitutions, that uſually belong to them, make them ſo, hath ſeemed probable to me, upon ſome obſervations. For, not to mention what is related of *Alexander* the Great, I knew a gentleman of a very happy temperature of body, whoſe ſweat, upon a critical examination, wherein I made uſe alſo of a ſurprize, I found to be fragrant; which was confirmed alſo by ſome learned men of my acquaintance, and particularly a phyſician, that lay with him.

Though civet uſually paſſes for a perfume, and as ſuch is wont to be bought at a great rate; yet it ſeems to be but a clammy excrement of the animal, that affords it, which is ſecreted into bags provided by nature to receive it. And I the rather mention civet, becauſe it uſually affords a phænomenon, that agrees very well with the mechanical doctrine concerning odours, though it do not demonſtrate it. For, when I have had the curioſity to viſit divers of thoſe civet-cats, (as they call them) though they have heads liker foxes than cats; I obſerved, that a certain degree of laxity (if I may ſo ſtile it) of the odorous atmoſphere was requiſite to make the ſmell fragrant. For, when I was near the cages, where many of them were kept together, or any great veſſel full of civet, the ſmell (probably by the plenty, and perhaps the over-briſk motion of the effluvia,) was rather rank and offenſive, than agreeable; whereas, when I removed into the next room, or to ſome other convenient diſtance, the ſteams (being leſs crowded, and farther from their fountain,) preſented themſelves to my noſtrils, under the notion of a perfume.

And

AND, not to dismiss this our eleventh experiment, without touching once more upon musk, I shall add, that an ingenious lady, to whom I am nearly related, shewed me an odd monkey, that had been presented her as a rarity by the then admiral of *England*, and told me, among other things, that she had observed in it, that, being sick, he would seek for spiders as his proper remedies, for some of which he then seemed to be looking, and thereby gave her occasion to tell me this; which, when he had eaten, the alteration it made in him, would sometimes fill the room with a musky scent: but he had not the good luck to light on any, whilst my visit lasted.

EXPERIMENT XII.

To heighten good smells by composition.

IT is well known to perfumers, and is easy to be observed, that amber-grease alone, though esteemed the best and richest perfume, that is yet known in the world, has but a very faint, and scarce a pleasant scent. And I remember, that I have seen some hundreds of ounces together, newly brought from the *East-Indies*; but if I had not been before acquainted with the smell of amber-grease alone, and had had only the vulgar conceit of it, that it is the best and strongest of perfumes, my nostrils would scarce have made me suspect those lumps to have been any thing a kin to amber-grease. But if a due proportion of musk, or even civet, be dexterously mixed with amber, the latent fragrancy, though it be thereby somewhat compounded, will quickly be called forth, and exceedingly heightened. And indeed it is not, as it is commonly presumed, the plenty of the richest ingredients, as amber-grease and musk, but the just proportion and skilful mixture of them, that makes the noblest and most lasting perfume, of which I have had sufficient experience; so that with a far less quantity of musk and amber, than not only ordinary persons, but perfumers themselves are wont to employ, we have had several perfumes, that, for fragrancy, were much preferred to those, where musk and amber-grease are so plentifully employed. The proportions and ways of mixture we best approved of, would be too long, and are not necessary to be here set down; but you will not much err in making use of such a proportion as this, viz. eight parts of amber-grease, two of musk, and one of civet: which quantities of ingredients, if they be skilfully and exactly mingled, you will not miss of a good composition, with which you may ennoble other materials, as benzoin, storax, sweet flowers, &c. fit to make pastils, ointments for leather, pomander, &c. And we may here add, that, upon the score of the new texture acquired by composition, some things, that are not fragrant themselves, may yet much heighten the fragrancy of odoriferous bodies. And of liquid perfumes, I remember, it was the secret of some court ladies, noted for curiosity about perfumes, to mingle always a due proportion of wine-vinegar with the odoriferous ingredients. And, on this occasion, to shew the power of mixtures in improving odours, I shall add something about a liquor of mine, that has had the good fortune to be very favourably spoken of by persons of quality accustomed to choice perfumes. This liquor, though thought an elaborate preparation, as well for another reason, as to recommend it to some, whose critical palates can taste the very titles of things, I called it essence of musk, is indeed a very plain simple preparation, which I thus make.

I take an arbitrary quantity of choice musk, without finely powdering it, and pour upon it about a finger's breadth of pure spirit of wine; these, in a glass closely stopped, I set in a quiet place to digest, without the help of any furnace; and after some days, or a few weeks, (according as circumstances determined,) the spirit, which is somewhat odd, will, in the cold, have made a solution of the finest parts of the musk, and will be thereby much tinged, but not of a red colour. This liquor, being decanted, I keep

keep by itself as the richest of all; and pour a like quantity of spirit on the remaining musk, which usually will, in the cold, though more slowly, draw a tincture, but fainter than the former; which being poured off, the remaining musk may be employed for inferior uses. Now that, which made me mention this preparation as pertinent to our present subject, is this phænomenon of it; that the first essence, or rather tincture, being smelled to by itself, has but a faint, and not very pleasing odour of musk, so that every body would not discover, that there was musk in it; but if a single drop, or two drops at most, were mixed with a pint, or perhaps a quart of good sack, the whole body of the wine would presently acquire a considerably musky scent, and be so richly perfumed, as to taste and smell, as seemed strange enough to those, that knew the vast disproportion of the ingredients.

Of the IMPERFECTION OF THE CHEMIST's DOCTRINE of QUALITIES.

CHAP. I.

SINCE a great part of those learned men, especially physicians, who have discerned the defects of the vulgar philosophy, but are not yet come to understand and relish the corpuscularian, have slid into the doctrine of the chemists; and since the spagyrists are wont to pretend to make out all the qualities of bodies, from the predominancy of some one of their three hypostatical principles, I suppose it may both keep my opinion from appearing too presumptuous, and (which is far more considerable) may make way for the fairer reception of the mechanical hypothesis about qualities, if I here intimate (though but briefly and in general) some of those defects, that I have observed in chemist's explications of qualities.

AND I might begin with taking notice of the obscurity of those principles, which is no small defect in notions, whose proper office it should be to conduce to the illustration of others. For, how can that facilitate the understanding of an obscure quality, or phænomenon, which is itself scarcely intelligible, or, at least, needs almost as much explanation, as the thing it is designed and pretended to explicate? Now a man need not be very conversant in the writings of chemists to observe, in how lax, indefinite, and almost arbitrary senses they employ the terms of salt, sulphur, and mercury; of which I could never find, that they were agreed upon any certain definitions, or settled notions; not only differing authors, but not unfrequently one and the same, and perhaps in the same book, employing them in very differing senses. But I will not give the chemists any rise to pretend, that the chief fault, that I find with their hypothesis, is but verbal; though that itself may not a little blemish any hypothesis, one of the first of whose requisites ought to be clearness; and therefore I shall now advance, and take notice of defects, that are manifestly of another kind.

AND, first, the doctrine, that all their theory is grounded on, seems to me inevident, and undemonstrated, not to say precarious. It is somewhat strange to me, that neither the spagyrists themselves, nor yet their adversaries, should have taken notice, that chemists

miſts have rather ſuppoſed than evinced, that the analyſis of bodies by fire, or even, that at leaſt ſome analyſis, is the only inſtrument of inveſtigating what ingredients mixed bodies are made up of, ſince, in divers caſes, that may be diſcovered by compoſition, as well as by reſolution; as it may appear, that vitriol conſiſts of metalline parts, (whether martial or venereal, or both,) aſſociated by coagulation with acid ones, one may, I ſay, diſcover this, as well by making true vitriol with ſpirit (improperly called oil) of ſulphur, or that of ſalt, as by diſtilling or reſolving vitriol by the fire.

But I will not here enlarge on this ſubject, nor yet will I trouble you with what I have largely diſcourſed in the Sceptical Chymiſt, to call in queſtion the grounds on which chemiſts aſſert, that all mixed bodies are compounded of ſalt, ſulphur, and mercury. For it may ſuffice me now to tell you, that, whatſoever they may be able to obtain from other bodies, it does not appear by experience, which is the grand, if not the only argument they rely on, that all mixed bodies, that have qualities, conſiſt of their *tria prima*, ſince they have not been able, that we know, truly, and without new compoſitions, to reſolve into thoſe three, either gold, or ſilver, or cryſtal, or Venetian talck, or ſome other bodies, that I elſewhere name; and yet theſe bodies are endowed with divers qualities, as the two former with fuſibleneſs and malleability, and all of them with weight and fixity; ſo that in theſe and the like bodies, whence chemiſts have not made it yet appear, that their ſalt, ſulphur, and mercury, can be truly and adequately ſeparated, it will ſcarce be other than precarious, to derive the malleableneſs, colour, and other qualities of ſuch bodies from thoſe principles.

Under this head I conſider alſo, that a great part of the chemical doctrine of qualities is bottomed on, or ſuppoſes, beſides their newly queſtioned analyſis by fire, ſome other things, which as far as I know, have not yet been well proved, and I queſtion whether they ever will be.

One of their main ſuppoſitions is, that this or that quality muſt have its πρῶτον δεκτικὸν, as *Sennertus*, the learnedeſt champion of this opinion, calls it, or ſome particular material principle, to the participation of which, as of the primary native and genuine ſubject, all other bodies muſt owe it: but upon this point having purpoſely diſcourſed elſewhere, I ſhall now only obſerve, that, not to mention local motion and figure, I think it will be hard to ſhew, what is the πρῶτον δεκτικὸν of gravity, volatility, heat, ſonorouſneſs, tranſparency, and opacity, which are qualities to be indifferently met with in bodies, whether ſimple or mixed.

And whereas the ſpagyriſts are wont to argue, that becauſe this or that quality is not to be derived truly from this or that particular principle, as ſalt, for inſtance, and mercury; therefore it muſt needs be derivable from the third, as ſulphur. This way of arguing involves a further ſuppoſition than that newly examined. For it implies, that every quality in a compounded body muſt ariſe from ſome one of the *tria prima*, whereas experience aſſures us, that bodies may, by compoſition, obtain qualities, that were not to be found in any of the ſeparate ingredients. As we ſee in painting, that though blue and yellow be neither of them green, yet their mixture will be ſo. And though no ſingle ſound will make an octave or diapaſon; yet two ſounds, whoſe proportion is double, will have an eighth. And tin and copper melted and mingled together in a due proportion, will make a bell metal far more ſonorous than either of them was before. It is obvious enough for chemiſts themſelves to obſerve, that, though lead be an inſipid body, and ſpirit of vinegar a very ſharp one, yet ſaccharum ſaturni, that is compounded out of theſe two, has a ſweetneſs, that makes it not ill deſerve its name.

But this ill-grounded ſuppoſition of the chemiſts, is extended farther in an uſual topic of theirs, according to which they conclude, that I know not how many qualities, as well manifeſt as occult, muſt be explicated by their *tria prima*, becauſe they are not explicable by the four elements of the Peripateticks. To make which argumentation valid, it must be proved, (which I fear it will never be) that there are no other ways, by which those qualities may be explicated, but by a determinate number of material principles, whether four or three: besides that, till they have shewn that such qualities may be intelligibly explicated by their principles, the objection will lie as strong for the Aristotelians against them, as for them against the Aristotelians.

CHAP. II.

NEXT I consider, that there are divers qualities even in mixed bodies, wherein it does not appear, that the use of the chemical doctrine is necessary. As for instance, when pure gold is by heat only brought to fusion, and consequently to the state of fluidity, and upon the remission of that heat, grows a solid and consistent body again, what addition or expulsion, or change of any of the *tria prima*, does appear to be the cause of this change of consistence? which is easy to be accounted for according to the mechanical way, by the vehement agitation, that the fire makes of the minute parts of the gold to bring it to fusion; and the cohesion of those parts, by virtue of their gravity and fitness to adhere to one another, when that agitation ceases. When Venice glass is merely, by being beaten to powder, deprived of its transparency and turned into a body opacous and white, what need or use of the *tria prima* have we in the explication of this phænomenon? Or of that other, which occurs, when by barely melting down this white and opacous body it is deprived of its opacity and colour, and becomes diaphanous; and of this sort of instances you will meet with divers in the following notes about particular qualities; for which reason I shall forbear the mention of them here.

CHAP. III.

I OBSERVE too, that the spagyrical doctrine of qualities is insufficient, and too narrow to reach to all the phænomena, or even to all the notable ones; that ought to be explicable by them. And this insufficiency I find to be twofold; for, first, there are divers qualities, of which chemists will not so much as attempt to give us explications, and of other particular qualities, the explications, such as they are, that they give us, are often very deficient and unsatisfactory; and do not sometimes so much as take notice of divers considerable phænomena, that belong to the qualities, whereof they pretend to give an account; of which you will meet with divers instances in the ensuing notes. And therefore I shall only (to declare my meaning the better) invite you to observe with me, that though gold be the body they affect to be most conversant with; yet it will be very hard to shew, how the specific weight of gold can be deduced from any, or all, of the three principles; since mercury itself, that is, of bodies known to us, the heaviest next to gold, is so much lighter than gold, that, whereas I have usually found mercury to be to an equal weight of water, somewhat, though little, less than fourteen to one, I find pure gold to be about nineteen times as heavy as so much water. Which will make it very difficult, not to say impossible for them to explain, how gold should barely, by participating of mercury, which is a body much lighter than itself, obtain that great specific gravity we find it to have; for the two other hypostatical principles, we know, are far lighter than mercury. And I think it would much puzzle the chemists, to give us any examples of a compounded body, that is specifically heavier than the heaviest of the ingredients, that it is made up of. And this is the first kind of insufficiency I was taking notice of in the chemical doctrine of qualities.

THE other is, that there are ſeveral bodies, which the moſt learned among themſelves confeſs not to conſiſt of their *tria prima*, and yet are endowed with qualities, which conſequently are not in thoſe ſubjects to be explicated by the *tria-prima*, which are granted not to be found in them. Thus elementary water, though never ſo pure, (as diſtilled rain water,) has fluidity and coldneſs and humidity and tranſparency and volatility, without having any of the *tria prima*. And the pureſt earth, as aſhes, carefully freed from the fixed ſalt, has gravity and conſiſtence and dryneſs and colour and fixity, without owing them either to ſalt, ſulphur, or mercury; not to mention, that there are celeſtial bodies, which do not appear, nor are wont to be pretended to conſiſt of the *tria prima*, that yet are endowed with qualities. As the ſun has light and, as many philoſophers think, heat and colour; and the moon has a determinate conſiſtence and figuration, (as appears by her mountains) and aſtronomers obſerve, that the higher planets, and even the fixed ſtars, appear to be differingly coloured. But I ſhall not multitiply inſtances of this kind, becauſe what I have ſaid may not only ſerve for my preſent purpoſe, but bring a great confirmation to what I lately ſaid, when I noted, that the chemical principles were in many caſes not neceſſary to explicate qualities: for ſince in earth, water, &c. ſuch diffuſed qualities, as gravity, fixedneſs, colour, tranſparency and fluidity, muſt be acknowledged not to be derived from the *tria prima*; it is plain, that portions of matter may be endowed with ſuch qualities by other cauſes and agents than ſalt, ſulphur and mercury. And then why ſhould we deny, that alſo in compounded bodies thoſe qualities may be (ſometimes at leaſt) produced by the ſame or the like cauſes? as we ſee, that the reduction of a diaphanous ſolid to powder, produces whiteneſs, whether the comminution happens to rock-cryſtal or to Venice glaſs, or to ice: the firſt of which is acknowledged to be a natural and perfectly mixed body; the ſecond a factitious, and not only mixed, but decompounded body; and the laſt, for aught appears, an elementary body, or at moſt very ſlightly and imperfectly mixed. And ſo by mingling air in ſmall portions with a diaphanous liquor, as we do, when we beat ſuch a liquor into foam, a whiteneſs is produced, as well in pure water, which is acknowledged to be a ſimple body, as in white wine, which is reckoned among perfectly mixed bodies.

CHAP. IV.

I FURTHER obſerve, that the chemiſt's explications do not reach deep and far enough. For firſt, moſt of them are not ſufficiently diſtinct and full, ſo as to come home to the particular phænomena, not oftentimes ſo much as to all the grand ones, that belong to the hiſtory of the qualities they pretend to explicate. You will readily believe, that a chemiſt will not eaſily make out by his ſalt, ſulphur and mercury, why a loadſtone, capped with ſteel, may be made to take up a great deal more iron, ſometimes more than eight or ten times as much, than if it be immediately applied to the iron: or why, if one end of the magnetic needle is diſpoſed to be attracted by the north pole, for inſtance, of the loadſtone, the other pole of the loadſtone will not attract it, but drive it away: or why a bar or rod of iron, being heated red-hot, and cooled perpendicularly, will, with its lower end, drive away the flower-de-luce, or the north end of a mariner's needle, which the upper end of the ſame bar or rod will not repel, but draw to it. In ſhort, of above threeſcore properties, or notable phænomena of magnetic bodies, that ſome writers have reckoned up, I do not remember, that any three have been by chemiſts ſo much as attempted to be ſolved by their three principles. And even in thoſe qualities, in whoſe explications theſe principles may more probably, than elſewhere, pretend to have a place, the Spagyriſts accounts are wont to fall ſo ſhort of

 being

being distinct and particular enough, that they use to leave divers considerable phænomena untouched, and do but very lamely or slightly explicate the more obvious or familiar. And I have so good an opinion of divers of the embracers of the Spagyrical theory of qualities (among whom I have met with very learned and worthy men) that I think, that if a quality being proposed to them, they were at the same time presented with a good catalogue of the phænomena, that they may take, in the history of it, as it were with one view, they would plainly perceive, that there are more particulars to be accounted for, than at first they were aware of; and divers of them such, as may quite discourage considering men from taking upon them to explain them all by the *tria prima*, and oblige them to have recourse to more catholic and comprehensive principles. I know not, whether I may not add on this occasion, that, methinks, a chemist, who by the help of his *tria prima*, takes upon him to interpret that book of nature, of which the qualities of bodies make a great part, acts at but a little better rate than he, that seeing a great book written in a cypher, whereof he were acquainted but with three letters, should undertake to decypher the whole piece. For though it is like he would, in many words, find one of the letters of his short key, and in divers words two of them, and perhaps in some all three; yet, besides that in most of the words, wherein the known letter or letters may be met with, they may be so blended with other unknown letters, as to keep him from decyphering a good part of those very words, it is more than probable, that a great part of the book would consist of words wherein none of his three letters were to be found.

CHAP. V.

AND this is the first account, on which I observe, that the chemical theory of qualities does not reach far enough: but there is another branch of its deficiency. For even, when the explications seem to come home to the phænomena, they are not primary, and, if I may so speak, fontal enough. To make this appear, I shall at present employ but these two considerations. The first is, that those substances themselves, that chemists call their principles, are each of them endowed with several qualities. Thus salt is a consistent, not a fluid body; it has its weight, it is dissoluble in water, is either diaphanous or opacous, fixed or volatile, sapid or insipid; (I speak thus disjunctively, because chemists are not all agreed about these things; and it concerns not my argument, which of the disputable qualities be resolved upon.) And sulphur, according to them, is a body fusible, inflammable, &c. and, according to experience, is consistent, heavy, &c. So that it is by the help of more primary and general principles, that we must explicate some of those qualities, which being found in bodies, supposed to be perfectly similar or homogeneous, cannot be pretended to be derived in one of them from the other. And to say, that it is the nature of a principle to have this or that quality, as for instance, of sulphur to be fusible, and therefore we are not to exact a reason why it is so; though I could say much by way of answer, I shall now only observe, that this argument is grounded but upon a supposition, and will be of no force, if from the primary affections of bodies one may deduce any good mechanical explication of fusibility in the general, without necessarily supposing such a primogeneal sulphur, as the chemists fancy, or deriving it from thence in other bodies. And indeed, since not only salt petre, sea salt, vitriol and allom, but salt of tartar, and the volatile salt of urine, are all of them fusible; I do not well see, how chemists can derive the fusibleness even of salts obtained by their own analysis (such as salt of tartar and of urine) from the participation of the sulphureous ingredient; especially since, if such an attempt should be made, it would overthrow the hypothesis of three simple bodies,

dies, whereof they will have all mixed ones to be compounded; and ſtill it would remain to be explicated, upon what account the principle, that is ſaid to endow the other with ſuch a quality, comes to be endowed therewith itſelf. For it is plain, that a maſs of ſulphur is not an atomical or adamantine body, but conſiſts of a multitude of corpuſcles of determinate figures, and connected after a determinate manner; ſo that it may be reaſonably demanded, why ſuch a convention of particles, rather than many another, that does not, conſtitutes a fuſible body.

CHAP. VI.

AND this leads me to a further conſideration, which makes me look upon the chemiſt's explications, as not deep and radical enough; and it is this, that, when they tell us, for inſtance, that the fuſibleneſs of bodies proceeds from ſulphur, in caſe they ſay true, they do but tell us what material ingredient it is, that being mingled with, and diſperſed through the other parts of a body, makes it apt to melt: but this does not intelligibly declare, what it is, that makes a proportion of matter fuſible, and how the ſulphureous ingredient introduces that diſpoſition into the reſt of the maſs, wherewith it is commixed or united. And yet it is ſuch explications as theſe, that an inquiſitive naturaliſt chiefly looks after, and which I therefore call philoſophical. And to ſhew, that there may be more fontal explications, I ſhall only obſerve, that, not to wander from our preſent inſtance, ſulphur itſelf is fuſible. And therefore, as I lately intimated, fuſibility, which is not the quality of one atome, or particle, but of an aggregate of particles, ought itſelf to be accounted for in that principle, before the fuſibleneſs of all other bodies be derived from it. And it will in the following notes appear, that in ſulphur itſelf, that quality may be probably deduced from the convention of corpuſcles of determinate ſhapes and ſizes, contexed, or connected, after a convenient manner. And if either nature, or art, or chance, ſhould bring together particles endowed with the like mechanical affections, and aſſociate them after the like manner, the reſulting body would be fuſible, though the component particles had never been parts of the chemiſt's primordial ſulphur: and ſuch particles ſo convening, might, perhaps, have made ſulphur itſelf, though before there had been no ſuch body in the world. And what I ſay to thoſe chemiſts, that make the ſulphureous ingredient the cauſe of fuſibility, may eaſily, *mutatis mutandis*, be applied to their hypotheſis, that rather aſcribe that quality to the mercurial, or the ſaline principle, and conſequently cannot give a rational account of the fuſibility of ſulphur. And therefore, though I readily allow, (as I ſhall have afterwards occaſion to declare,) that ſulphur, or another of the *tria prima*, may be met with, and even abound in ſeveral bodies endowed with the quality, that is attributed to their participation of that principle; yet, that this may be no certain ſign, that the propoſed quality muſt flow from that ingredient, you may perhaps be aſſiſted to diſcern by this illuſtration, that if tin be duly mixed with copper or gold, or, as I have tried, with ſilver or iron, it will make them very brittle; and it is alſo an ingredient of divers other bodies, that are likewiſe brittle, as blue, green, white, and otherwiſe coloured amels, which are uſually made of calcined tin, (which the tradeſmen call putty) colliquated with the ingredients of cryſtal glaſs, and ſome ſmall portion of mineral pigment. But though, in all the above-named brittle bodies, tin be a conſiderable ingredient; yet it were very unadviſed to affirm, that brittleneſs, in general, proceeds from tin. For, provided the ſolid parts of conſiſtent bodies touch one another but according to ſmall portions of their ſurfaces, and be not implicated by their contexture, the metalline, or other compoſition, may be brittle, though there be no tin at all in it. And, in effect, the materials of glaſs, being brought

brought to fuſion, will compoſe a brittle body, as well when there is no putty colliquated with them, as when there is. Calcined lead, by the action of the fire, may be melted into a brittle maſs, and even into tranſparent glaſs, without the help of tin, or any other additament. And I need not add, that there are a multitude of other bodies, that cannot be pretended to owe their brittleneſs to any participation of tin, of which they have no need, if the matter they conſiſt of, wants not the requiſite mechanical diſpoſitions.

AND here I ſhall venture to add, that the way employed by the chemiſts, as well as the Peripateticks, of accounting for things by the ingredients, whether elements, principles, or other bodies, that they ſuppoſe them to conſiſt of, will often fruſtrate the naturaliſts expectation of events, which may frequently prove differing from what he promiſed himſelf, upon the conſideration of the qualities of each ingredient. For the enſuing notes contain divers inſtances, wherein there emerges a new quality differing from, or even contrary to any, that is conſpicuous in the ingredients; as two tranſparent bodies may make an opacous mixture, a yellow body and a blue, one, that is green; two malleable bodies, a brittle one; two actually cold bodies, a hot one; two fluid bodies, a conſiſtent one, &c. And as this way of judging, by material principles, hinders the foreknowledge of events from being certain; ſo it much more hinders the aſſignation of cauſes from being ſatisfactory; ſo that, perhaps, ſome would not think it very raſh to ſay, that thoſe, who judge of all mixed bodies, as apothecaries do of medicines, barely by the qualities and proportions of the ingredients (ſuch as, among the Ariſtotelians, are the four elements, and among the chemiſts the *tria prima*,) do, as if one ſhould pretend to give an account of the phænomena and operations of clocks and watches, and their diverſities by this, that ſome are made of braſs wheels, ſome of iron, ſome have plain ungilt wheels, others of wheels overlaid with gold, ſome furniſhed with gutſtrings, others with little chains, &c. and that therefore the qualities and predominancies of theſe metals, that make parts of the watch, ought to have aſcribed to them, what indeed flows from their co-ordination and contrivance.

CHAP. VII.

THE laſt defect I obſerve in the chemical doctrine of qualities, is, that in many caſes it agrees not well with the phænomena of nature, and that by one or both of theſe ways. Firſt, there are divers changes of qualities, wherein one may well expect, that a chemical principle ſhould have a great ſtroke, and yet it does not at all appear to have ſo. He, that conſiders, what great operations divers of the Hermeticks aſcribe to this or that hypoſtatical principle, and how many qualities, according to them, muſt from it be derived, can ſcarce do other than expect, that a great change, as to thoſe qualities, happening in a mixed body, ſhould, at leaſt, be accompanied with ſome notable action of, or alteration in the principle. And yet I have met with many inſtances, wherein qualities are produced, or aboliſhed, or very much altered, without any manifeſt introduction, expulſion, or conſiderable change of the principle, whereon that quality is ſaid to depend, or perhaps of either of the two others: as when a piece of fine ſilver, that having been nealed in the fire, and ſuffered to cool leiſurely, is very flexible, is made ſtiff, and hard to bend, barely by a few ſtrokes of a hammer. And a ſtring of a lute acquires or loſes a ſympathy, as they call it, with another ſtring of the ſame or another inſtrument, barely by being either ſtretched, ſo as to make an uniſon with it, or ſcrewed up, or let down, beyond or beneath that degree of tenſion.

To multiply inſtances of this kind, would be to anticipate thoſe, you will hereafter meet with in their due places. And therefore I ſhall paſs on, from the firſt ſort of

phænomena,

phænomena, that favour not the chemical hypothesis about qualities, to the other, which consists of those, wherein either, that does not happen, which, according to their hypothesis, ought to happen, or the contrary happens to what, according to their hypothesis, may justly be expected. Of this you will meet with instances hereafter; I shall now trouble you but with one, the better to declare my meaning. It is not unknown to those chemists, that work much in silver and in copper, that the former will endure ignition, and become red-hot in the fire, before it will be brought to fusion; and the latter is yet far more difficult to be melted down than the other: yet if you separately dissolve those two metals in aqua fortis, and by evaporation reduce them to crystals, these will be brought to fusion in a very little time, and with a very moderate heat, without breaking the glasses that contain them. If you ask a vulgar chemist the cause of this facility of fusion, he will, probably, tell you, without scruple, that it is from the saline parts of the aqua fortis, which, being imbodied in the metals, and of a very fusible nature, impart that easiness of fusion to the metals they are mixed with. According to which plausible explication one might well expect, that, if the saline corpuscles were exquisitely mingled with tin, they would make it far more fusible than of itself it is. And yet, as I have elsewhere noted, when I put tin into a convenient quantity of aqua fortis, the metal being corroded, subsided, as is usual, in the form of whites of eggs, which being well dried, the tin was so far from being grown more fusible by the addition of the saline particles of the menstruum, that, whereas it is known, that simple tin will melt long before it come to be red-hot, this prepared tin would endure, for a good while, not only a thorough ignition, but the blast of a double pair of bellows, (which we usually employed to melt silver and copper itself,) without being at all brought to fusion. And as for those Spagyrists, that admit, as most of them are granted to do, that all kinds of metals may be turned into gold, by a very small proportion of what they call the philosophers elixir, one may, I think, shew them, from their own concessions, that divers qualities may be changed, even in such constant bodies as metals, without the addition of any considerable proportion of the simple ingredients, to which they are wont to ascribe those qualities; provided the agent, (as an efficient, rather than as a material cause,) be able to make a great change in the mechanical affections of the parts whereof the metal it acts on is made up. Thus if we suppose a pound of silver, a pound of lead, and a pound of iron to be transmuted into gold, each by a grain of the powder of projection, this tinging powder, as a material cause, is inconsiderable, by reason of the smallness of its bulk, and as an efficient cause, it works differing, and even contrary effects, according to the disposition, wherein it finds the metal to be transmuted, and the changes it produces in the constituent texture of it. Thus it brings quicksilver to be fixed, which it was not before, and deprives it of the fluidity, which it had before; it brings silver to be indissolvable in aqua fortis, which readily dissolved it before, and dissoluble in aqua regis, which before would not touch it; and which is very considerable to our present purpose, whereas it makes iron much more fusible than *Mars*, it makes lead much less fusible than whilst it retained its pristine form, since *Saturn* melts ere it come to ignition, which gold requires to bring it to fusion. But this is proposed only as an argument *ad hominem*, till the truth of the transmutation of metals into gold, by way of projection, be sufficiently proved, and the circumstances, and phænomena of it, particularly declared.

I must not forget to take notice, that some learned modern chemists would be thought to explicate divers of the changes, that happen to bodies in point of odours, colours, &c. by saying, that, in such alterations, the sulphur, or other hypostatical principle, is intraverted or extraverted, or, as others speak, inverted. But I confess, to me these seem to be rather new terms, than real explications. For, to omit divers of the argu-

ments

ments mentioned in this present treatise, that may be applied to this way of solving the phænomena of qualities, one may justly object, that the supposed extraversion or intraversion of sulphur, can by no means reach to give an account of so great a variety of odours, colours, and other qualities, as may be found in the changed portions of matter we are speaking of. And, which is more, what they call by these and the like names, cannot be done without local motion transposing the particles of the matter, and consequently producing in it a change of texture, which is the very thing we would infer, and which being supposed, we may grant sulphur to be oftentimes actually present in the altered bodies, without allowing it to be always necessary to produce the alterations in them, since corpuscles, so conditioned and contexed, would perform such effects, whether sulphur, as such, did, or did not make up the subject matter of the change.

AND now I shall conclude, and partly recapitulate what has been delivered in this and the two foregoing chapters, with this summary consideration; that the chemist's salt, sulphur, and mercury themselves are not the first and most simple principles of bodies, but rather primary concretions of corpuscles, or particles more simple than they, as being endowed only with the first, or most radical, (if I may so speak) and most catholick affections of simple bodies, namely, bulk, shape, and motion, or rest; by the different conventions or coalitions of which minutest portions of matter, are made those differing concretions, that chemists name salt, sulphur, and mercury. And to this doctrine it will be consonant, that several effects of this or that spagyrical principle need not be derived from salt, for instance, or sulphur as such, but may be explained by the help of some of those corpuscles, that I have lately called more simple and radical; and such explications being more simple and mechanical, may be thought, upon that score, more fundamental and satisfactory.

CHAP. VIII.

I KNOW it may be objected, in favour of the chemists, that as their hypostatical principles, salt, sulphur, and mercury, are but three, so the corpuscularian principles are but very few; and the chief of them bulk, size, and motion, are but three neither; so that it appears not, why the chemical principles should be more barren than the mechanical. To which allegation I answer, that, besides that these last named principles are more numerous, as taking in the posture, order, and situation, the rest, and, above all, the almost infinitely diversifiable contextures of the small parts, and the thence resulting structures of particular bodies, and fabrick of the world: besides this, I say, each of the three mechanical principles, specified in the objection, though but one in name, is equivalent to many in effect; as figure, for instance, comprehends not only triangles, squares rhomboides, trapeziums, and a multitude of polygons, whether ordinate or irregular; but, besides cubes, prisms, cones, spheres, cylinders, pyramids, and other solids of known denominations, a scarce numerable multitude of hooked, branched, eel-like, screw-like, and other irregular bodies; whereof, though these, and some others, have distinct appellations, yet the greatest part are nameless; so that it need be no wonder, that I should make the mechanical principles so much more fertile, that is, applicable to the production and explication of a far greater number of phænomena, than the chemical; which, whilst they are considered but as similar bodies, that are ingredients of mixed and compounded ones, are chiefly variable but by the greater or lesser quantity, that is employed by nature or art to make up the mixed body. And painters observe, that black and white, though mixed in differing proportions, will still make but lighter and darker greys. And if it be said, that these ingredients, by the texture resulting from their mixtures, may acquire qualities, that neither of them had before; I ſhall anſwer, that to alledge this, is, in effect, to confeſs, that they muſt take in the mechanical principles (for to them belongs the texture or ſtructure of bodies) to aſſiſt the chemical ones. And, on this occaſion, to borrow an illuſtration from our unpubliſhed dialogue of the requiſites of a good hypotheſis, I ſhall add, that a chemiſt, that ſhould pretend, that becauſe his three principles are as many as thoſe of the Corpuſcularians, they are as ſufficient, as theſe, to give an account of the book of nature; methinks, I ſay, he would do like a man, that ſhould pretend, that, with four and twenty words, he would make up a language, as well as others can with the four and twenty letters of the alphabet, becauſe he had as many words already formed, as they had of bare letters; not conſidering, that, inſtead of the ſmall number of variations, that can be made of his words by prepoſitions and terminations, the letters of the alphabet being variouſly combined, placed, and reiterated, can be eaſily made to compoſe not only his four and twenty words, with their variations, but as many others, as a whole language contains.

CHAP. IX.

NOTWITHSTANDING all, that I have been obliged to ſay to the diſadvantage of the chemical principles, in reference to the explication of qualities, I would not be thought to grant, that the Peripateticks have reaſon to triumph, as if their four elements afforded a better theory of qualities. For, if I had, together with leiſure enough to perform ſuch a taſk, any obligation to undertake it, I preſume, it would not be difficult to ſhew, that the Ariſtotelian doctrine, about particular qualities, is liable to ſome of the ſame objections with the chemical, and to ſome others no leſs conſiderable; and that, to derive all the phænomena their doctrine ought to ſolve, from ſubſtantial forms and real qualities elementary, is to impoſe on us a theory more barren and precarious, than that of the Spagyriſts.

THAT, to derive the particular qualities of bodies from thoſe ſubſtantial forms, whence the ſchools would have them to flow, is but an inſufficient and unfit way of accounting for them, may appear by this, that ſubſtantial forms themſelves are things, whoſe exiſtence many learned philoſophers deny, whoſe theory many of them think incomprehenſible, and the moſt candid and judicious of the Peripateticks themſelves confeſs it to be very abſtruſe; ſo that, from ſuch doubtful and obſcure principles, we can hardly expect clear explications of the nature and phænomena of qualities; not to urge, that the Ariſtotelian definitions, both of qualities in general, and of divers of the more familiar qualities in particular, as heat, cold, moiſture, diaphaneity, &c. are far enough from being clear and well framed, as we elſewhere have occaſion to ſhew.

ANOTHER thing, which makes the ſcholaſtic doctrine of qualities unſatisfactory, is, that it ſeldom ſo much as attempts to teach the manner, how the qualities themſelves, and their effects or operations, are produced. Of this you may elſewhere find an inſtance given in the quality, that is wont to be firſt in the liſt, viz. that of heat; which, though it may intelligibly and probably be explicated by the corpuſcular hypotheſis, yet, in the Peripatetic account, that is given of it, is both too queſtionable and too ſuperficial to give much content to a rational inquirer. And indeed to ſay, that a ſubſtantial form (as that of the fire) acts by a quality, (called heat) whoſe nature it is to produce ſuch an effect, (as to ſoften wax, or harden clay,) ſeems to be no other in ſubſtance, than to ſay, that it produces ſuch an effect by ſome power it has to produce it. But what that power is, and how it operates, is that, which, though we moſt deſire to know, we are left to ſeek. But to proſecute the imperfections of the Peripatetic hypotheſis, were to intrench upon another diſcourſe, where they are more fully laid open.

And therefore I fhall now but lightly glance upon a couple of imperfections, that more particularly relate to the doctrine of qualities.

AND firft, I do not think it a convincing argument, that is wont to be employed by the Ariftotelians for their elements, as well as by the chemifts for their principles, that, becaufe this or that quality, which they afcribe to an element or a principle, is found in this or that body, which they call mixed, therefore it muft owe that quality to the participation of that principle or element. For, the fame texture of parts, or other modification of matter, may produce the like quality in the more fimple and the more compounded body, and they may both feparately derive it from the fame caufe, and not one from the participation of the other. So water, and earth, and metals, and ftones, &c. are heavy, upon the account of the common caufe of gravity, and not becaufe the reft partake of the earth; as may appear in elementary water, which is as fimple a body as it, and yet is heavy: fo water and oil, and exactly dephlegmed fpirit of wine, and mercury, and alfo metals and glafs of antimony, and minium or calcined lead, whilft thefe three are in fufion, are fluid, being made fo by the varioufly determined motions of their minute parts, and other caufes of fluidity, and not by the participation of water, fince the arid calces of lead and antimony are not like to have retained, in the fire, fo volatile a liquor as water, and fince fluidity is a quality, that mercury enjoys in a more durable manner than water itfelf: for that metalline liquor, as alfo fpirit of wine well rectified, will not be brought to freeze with the higheft degree of cold of our fharpeft winters, though a far lefs degree of cold would make water ceafe to be fluid, and turn it into ice.

To this I fhall only add, in the fecond place, that it is not unpleafant to fee, how arbitrarily the Peripateticks derive the qualities of bodies from their four elements, as if, to give an inftance in the lately named quality, liquidity, you fhew them exactly dephlegmed fpirit of wine, and afk them, whence it has its great fluidnefs, they will tell you, from water, which yet is far lefs fluid than it; and this fpirit of wine itfelf is much lefs fo than the flame, into which the fpirit of wine is eafily refoluble. But if you afk, whence it becomes totally inflammable, they muft tell you, from the fire; and yet the whole body, at leaft, as far as fenfe can difcover, is fluid, and the whole body becomes flame, (and then is moft fluid of all;) fo that fire and water, as contrary as they make them, muft both be, by vaft odds, predominant in the fame body. This fpirit of wine alfo, being a liquor, whofe leaft parts, that are fenfible, are actually heavy, and compofe a liquor, which is feven or eight hundred times as heavy as air of the fame bulk, which yet experience fhews not to be devoid of weight, muft be fuppofed to abound with earthy particles; and yet this fpirituous liquor may, in a trice, become flame, which they would have to be the lighteft body in the world.

BUT, to enlarge on this fubject, would be to forget, that the defign of this tract engages me to deal not with the Peripatetic fchool, but the Spagyrical. To which I fhall therefore return, and give you this advertifement about it, that what I have hitherto objected, is meant againft the more common and received doctrine about the material principles of bodies reputed mixed, as it is wont, by vulgar chemifts, to be applied to the rendering an account of the qualities of fubftances corporeal; and therefore I pretend not, that the paft objections fhould conclude againft other chemical theories, than that, which I was concerned to queftion. And if adept philofophers, (fuppofing there be fuch) or any other more than ordinarily intelligent Spagyrifts, fhall propofe any particular hypothefis, differing from thofe, that I have queftioned, as their doctrine and reafons are not yet known to me; fo I pretend not, that the paft arguments fhould conclude againft them, and am willing to think, that perfons advantaged with fuch peculiar opportunities, to dive into the myfteries of nature, will be able to give us, if they shall pleaſe, a far better account of the qualities of bodies, than what is wont to be propoſed by the generality of chemiſts.

Thus, dear *Pyrophilus*, I have laid before you ſome of the chief imperfections I have obſerved, in the vulgar chemiſts doctrine of qualities; and conſequently I have given you ſome of the chief reaſons, that hinder me from acquieſcing in it. And as my objections are not taken from the ſcholaſtical ſubtleties, nor the doubtful ſpeculations of the Peripateticks, or other adverſaries of the Hermetick philoſophy, but from the nature of things, and from chemical experiments themſelves; ſo, I hope, if any of your ſpagyrical friends have a mind to convince me, he will attempt to do it by the moſt proper way, which is, by actually giving us clear and particular explications, at leaſt, of the grand phænomena of qualities; which, if he ſhall do, he will find me very ready to acquieſce in a truth, that comes uſhered in, and endeared by ſo acceptable and uſeful a thing, as a philoſophical theory of qualities.

REFLECTIONS

UPON THE

HYPOTHESIS of ALCALI and ACIDUM.

CHAP. I.

I PRESUME, it will not be difficult to diſcern, that much of what has been ſaid about the imperfection of the vulgar chemical doctrine concerning qualities, may, with eaſy variations, be applied to ſome other hypotheſes, that are of kin to that doctrine, and particularly to their theory, that would derive both the qualities of bodies, and the reſt of the phænomena of nature from what they call acidum and alcali. For though theſe two differences may be met with in a great number and variety of bodies, and conſequently the conſideration of them may frequently enough be of good uſe, (eſpecially to Spagyriſts, and phyſicians, when they are converſant about the ſecondary and, if I may ſo call them, chemical cauſes and operations of divers mixed bodies;) yet I confeſs I cannot acquieſce in this hypotheſis of alcali and acidum, in the latitude, wherein I find it urged and applied by the admirers of it, as if it could be uſefully ſubſtituted in the place of matter and motion.

The hypotheſis being in a ſort ſubordinate to that of the *tria prima*, in aſcribing to two contrary ſaline principles, what vulgar chemiſts do to their ſalt, ſulphur, and mercury; moſt of the objections we have made againſt the vulgar chemical doctrine, may, as I lately intimated, be applied, by a little variation, to this, and therefore I ſhall need but to touch upon the main things, that keep me from acquieſcing in this hypotheſis.

CHAP. II.

AND firſt, it ſeems precarious to affirm, that in all bodies, or even in all the ſenſible parts of mixeds, acid and alcalizate parts are found; there not having been, that I know, any experimental induction made of particulars any thing near numerous enough to make out ſo great an aſſertion, and in divers bodies, wherein experience is vouched

vouched for the inexistence of these principles, that inexistence is indeed proved, not by direct and clear experience, but upon a supposition, that such and such effects flow from the operations of the assumed principles.

SOME Spagyrists, when they see aqua-fortis dissolve filings of copper, conclude from thence, that the acid spirits of the menstruum meet in the metal with an alcali upon which they work; which is but an unsafe way of arguing, since good spirit of urine, which they take to be a volatile alcali, and which will make a great conflict with aqua-fortis, will, as I have elsewhere noted, dissolve filings of copper both readily enough and more genuinely than the acid liquor is wont to do. So, when they see the magistery of pearl or coral made by dropping oil of tartar into the solutions of those bodies made with spirit of vinegar, they ascribe the precipitation to the fixed alcali of the tartar, that mortifies the acidity of the spirit of vinegar; whereas the precipitation would no less ensue, if, instead of alcalizate oil of tartar, we employ that highly acid liquor which they call *oleum sulphuris per campanam.*

I think also it may be doubted, whether those, I reason with, are so certain as they suppose, that at least, when they can manifestly discover an acid, for instance, in a body, the operation of that body upon another, which they judge to abound with an alcali, must be the effect of a conflict between those two jarring principles, or, if I may so call them, duellists. For an acid body may do many things, not simply as an acid, but on the score of a texture or modification, which endows it with other qualities as well as acidity, whose being associated with those other qualities in some cases, may be but accidental to the effect to be produced; since by one or more of these other qualities the body may act in cases, where prejudice may make a chemist consider nothing but acidity. Thus when some chemists see an acid menstruum, as aqua-fortis, spirit of salt, oil of vitriol, &c. dissolve iron, they presently ascribe the effect to an acidity of the liquors, whereas well dephlegmed urinous spirits, which they hold to have a great antipathy to acids, will, as I have tried in some of them, readily enough dissolve crude iron even in the cold. And on the other side, mercury will not work on the filings of iron, though this be so open a metal, that even weak liquors will do it; and yet if one should urge, that quicksilver readily dissolves gold in amalgamation, he may expect to be told, according to their doctrine, that mercury has in it an occult acid, by which it performs the solution; whereas it seems much more probable, that mercury has corpuscles of such a shape and size, as fit them to insinuate themselves into the commensurate pores they meet with in gold, but make them unfit to enter readily the pores of iron, to which nature has not made them congruous; as on the other side the saline corpuscles of aqua-fortis will easily find admission into the pores of iron, but not into those of gold, to which they do not correspond as they do to the others. And when a knife, whose blade is touched with a load-stone, cuts bread and takes up filings of iron, it does neither of them upon the score of alkali and acidum, but the one upon the visible shape, and the stiffness of the blade, and the other upon the latent contrivance or change of texture produced by the operation of the loadstone in the particles, that compose the steel.

THIS may perhaps be farther illustrated by adding, that when blue vitriol, being beaten and finely searced, makes a white powder, that whiteness is a quality, which the powder has not, as being of a vitriolate nature; for rock-crystal or Venice-glass, being finely beaten, will have the same operation on the eye. But it proceeds from the transparency of the body and the minuteness, multitude, and confused situation of the corpuscles, that make up the powder. And therefore, if other bodies be brought by comminution into parts endowed with such mechanical affections, as we have named; these aggregates will act upon the organs of sight, as white bodies.

CHAP. III.

AND this leads me to another exception againſt the hypotheſis of the duelliſts, which is, that the framers of it ſeem arbitrarily to have aſſigned provinces or offices to each of their two principles, as the chemiſts do to each of their *tria prima*, and the Peripateticks to each of their four elements. For it is not enough to ſay, that an acid, for inſtance, as ſuch, performs theſe things, and an alcali ſo many others, that they divide the operations and phænomena of nature, or at leaſt (as ſome, more cautious, are content to ſay) of mixed bodies between them; ſince aſſertions of ſuch great moment ought not to be advanced or received without ſufficient proof. And perhaps the very diſtribution of ſalts into acids and alcalies hath ſomewhat of arbitrary in it, ſince others may, without aſſuming much more, take the freedom to diſtribute them otherwiſe, there being not only ſeveral things, wherein acids and alcalies agree, but alſo ſeveral things, wherein ſalts of the ſame denomination widely differ. As for inſtance, ſome alcalies, according to thoſe I reaſon with, are, like ſalt of tartar, fixed, and will endure the violence of the fire; others, like ſalt of urine or harts-horn, are exceedingly fugitive, and will be driven up with a ſcarce ſenſible degree of heat; ſome, as ſalt of tartar, will precipitate the ſolution of ſublimate into an orange-tawny; others, as ſpirit of blood and harts-horn, precipitate ſuch a ſolution into a milky ſubſtance. Oil of tartar will very ſlowly operate upon filings of copper, which ſpirit of urine and harts-horn will readily diſſolve in the fire.

AND among acids themſelves the difference is no leſs, if not much greater. Some of them will diſſolve bodies, that others will not, as aqua-fortis will diſſolve ſilver and mercury, but leave gold untouched; or as aqua-regis, though made without ſal-armoniac, that diſſolves gold readily, will diſſolve mercury but ſcurvily, and ſilver not at all. And this may happen, when the menſtruum, that will not diſſolve the body, is reputed much ſtronger than that, which does; as dephlegmed ſpirit of vinegar will diſſolve lead, reduced to minute parts in the cold; which is an effect, that chemiſts are not wont to expect from ſpirit of ſalt. Nay, which is more, one acid will precipitate what another has diſſolved, and contrarily; as ſpirit of ſalt will precipitate ſilver out of ſpirit of nitre. And I found oil of vitriol to precipitate bodies of divers kinds, minerals and others, out of ſome acid menſtruums, particularly ſpirit of vinegar.

To this might be added the properties, peculiar to ſome particular acids, as that ſpirit of nitre or aqua-fortis will diſſolve camphire into an oil, and coagulate common oil into a conſiſtent and brittle ſubſtance like tallow; and, though it will both corrode ſilver, copper, lead, and mercury, and keep them diſſolved, it will quickly let fall almoſt the whole body of tin, very ſoon after it has corroded as much as it can of it. By all which, and ſome other like inſtances, I am induced to queſtion whether the acidum and alcali, we are ſpeaking of, have the ſimplicity, that philoſophy requires in principles; and ſhall be kept from wondering, if others ſhall think it as free for them to conſtitute other principles, as it is for the learned men I reaſon with, to pitch upon acidum and alkali.

AND ſome perhaps will be bold to ſay, that, ſince the former of thoſe principles comprehend ſuch a number of bodies, that are, many of them, very differing, and ſome of them directly contrary in their operations, it ſeems a ſlight and not philoſophical account of their nature, to define an acid by its hoſtility to an alcali, which, they will ſay, is almoſt as if one ſhould define a man, by ſaying, that he is an animal, that is at enmity with the ſerpent; or a lion, that he is a fourfooted beaſt, that flies from a crowing cock.

CHAP.

CHAP. IV.

BUT although one of the chiefeſt conditions, that philoſophers may juſtly require in principles, is, that, being to explain other things, they ſhould be very clear themſelves; yet I do not much wonder, that the definitions given us of acidum and alcali ſhould be but unaccurate and ſuperficial, ſince I find not, that they have themſelves any clear and determinate notion or ſure marks, whereby to know them diſtinctly, without which chemiſts will ſcarce be able to form clear and ſettled notions of them. For to infer, as is uſual, that, becauſe a body diſſolves another, which is diſſoluble by this or that known acid, the ſolvent muſt alſo be acid; or to conclude, that, if a body precipitates a diſſolved metal out of a confeſſedly acid menſtruum, the precipitant muſt be an alcali; to argue thus, I ſay, it is unſecure; ſince, not to repeat what I ſaid lately of copper, I found, that filings of ſpelter will be diſſolved as well by ſome alcalies, (as ſpirit of ſal armoniac) as by acids. And bodies may be precipitated out of acid menſtruums, both by other acids, and by liquors, where there appears not the leaſt alcali: as I have found, that a ſolution of tin-glaſs, made in aqua-fortis, would be precipitated both by ſpirit of ſalt, and by common, or rain water. And as for the other grand way, that chemiſts employ, to diſtinguiſh acids and alcalies, namely by the heat, commotion, and bubbles, that are excited upon their being put together, that may be no ſuch certain ſign, as they preſume, they having indeed a dependence upon particular contextures, and other mechanical affections, that chemiſts are not wont to take any notice of. For almoſt any thing, that is fitted variouſly and vehemently to agitate the minute parts of a body, will produce heat in it; and ſo, though water be neither an acid, nor an alcalizate liquor, yet it would quickly grow very hot, not only with the highly acid oil of vitriol, but (as I have more than once purpoſely tried and found) with the fiery alcalizate ſalt of tartar. And it is to be noted, that neither in the one, nor the other of theſe incaleſcent mixtures, there is produced any ſuch viſible or audible conflict, as, according to the doctrine of the chemiſts I reaſon with, one would expect. And as for the production of bubbles, eſpecially if accompanied with a hiſſing noiſe, neither is that ſuch a certain ſign as chemiſts imagine: for the production of bubbles is not a neceſſary effect, or concomitant of heat excited by conflicts, but depends very much upon the peculiar diſpoſition of bodies put together to extricate, produce, or intercept particles of air, (or ſteams, for the time equivalent to them;) and therefore as oil of vitriol, mixed in a due proportion with fair water, may be brought to make the water too hot to be held in one's hand, without exciting bubbles; ſo I have found, by trials purpoſely made, that alcalizate ſpirit of urine drawn from ſome kinds of quick-lime, being mixed with oil of vitriol moderately ſtrong, would produce an intenſe heat, whilſt it produced either no manifeſt bubbles at all, or ſcarce any, though the urinous ſpirit was ſtrong, and in other trials operated like an alcali; and although alſo with ſpirit of urine, made *per ſe* the common way, the oil of vitriol will produce a great hiſſing, and a multitude of conſpicuous bubbles. On the other ſide, I have ſometimes, though not ſo conſtantly, found, that ſome acid ſpirits, eſpecially that of verdigreaſe made *per ſe*, would, when poured upon ſalt of tartar, make a conflict with it, and produce a copious froth, though we obſerved it not to be accompanied with any manifeſt heat. And I elſewhere mention two bodies, upon whoſe putting together, numerous bubbles would, for a long time, and not without noiſe, be generated, and ſucceed one another, though I could perceive no heat at all to accompany this tumult.

As for the taſte, which by many is made a great touchſtone, whereby to know acids and alcalies, I conſider, that there is a multitude of mixed bodies, wherein we can ſo little

little difcern by the tafte, which of the principles is predominant, that this fenfe would not oblige one to fufpect, much lefs to conclude, there were one grain of either of them to be found there; fuch bodies are diamonds and rubies, and moft gems, befides many ignobler ftones, and gold, and filver, and mercury, and I know not how many other bodies. On the other fide, there are bodies, that abound with acid or alcalizate falts, which either have no tafte, or a quite differing one from that of the chemical principle. As though Venice-glafs be in great part compofed of a fixed alcali; yet to the tongue it is infipid, and cryftals of lune, and of lead, made with aqua-fortis, and containing great ftore of the acid particles of the menftruum, have nothing of acidity in the mouth, the latter having a faccharine fweetnefs, and the former an extreme bitternefs. And even in vegetable fubftances, that have a manifeft tafte, it is not fo eafy to know by that, whether it be the acid, or the alcalizate principle, that is predominant in them; as in the effential oils of fpices, and other vegetables. And in the grofs empyreumatical oils of woods, and even in high rectified fpirit of wine, which therefore fome will have to be an alcalizate liquor, and others lift it among acids, though I did not find it neither to be deftroyed, or much altered, by being put upon coral, or falt of tartar, as would happen to an acid menftruum, nor yet by being digefted with, and diftilled from fea falt, as might be probably expected from an alcalizate one: and among thofe very bodies, which their taftes perfuade chemifts to reckon among acids, one may (according to what I formerly noted) obferve fo great a difference and variety of relifhes, that, perhaps, without being too fevere, I may fay, that if I were to allow acids to be one principle, it fhould be only in fome fuch metaphyfical fenfe, as that wherein air is faid to be one body, though it confift of the affociated effluviums of a multitude of corpufcles of very differing natures, that agree in very little, fave in their being minute enough to concur to the compofition of a fluid aggregate, confifting of flying parts. But having dwelt longer than I intended on one objection, it is time, that I proceed to thofe that remain.

CHAP. V.

ANOTHER particular, I am unfatisfied with in the hypothefis of alcali and acidum, is, that it is in divers cafes either needlefs or ufelefs to explain the phænomena of qualities, there being feveral of thefe produced, deftroyed, or altered, where there does not appear any acceffion, recefs, or change of either of thofe two principles; as when fluid water by hard beating is turned into confiftent froth, and when tranfparent red coral is, barely by being beaten and fifted finely, changed into a white and opacous powder; and as when a very flexible piece of fine filver being hammered is brought to have a brifk fpring, and after a while will, inftead of continuing malleable, crack or cleave under the hammer; and as when (to difpatch and omit other inftances) a fufficiently thin leaf of gold, held between the light and the eye, appears green.

ANOTHER thing (of kin to the former,) that I like not in the doctrine of acidum and alcali, is, that though the patrons of it, whilft they would feem to conftitute but two principles, are fain, as I lately intimated, to make I know not how many differing forts of acids, befides fome variety of alcalies; yet their principles are too few and narrow, to afford any fatisfactory explication of the phænomena. For I fear, it will be very difficult for them to give a rational account of gravity, fpringinefs, light, and emphatical colours, founds, and fome other qualities, that are wont to be called manifeft; and much more of feveral, that are confeffed to be occult, as electricity, and magnetifm; in which laft I fee not, how the affirming, that there is in the magnet an acid and an alcali, and that thefe two are of contrary natures, will help to explain,

plain, how a load-ſtone does, as they ſpeak, attract the ſame end of a poiſed needle, with one of its poles, which it will drive away with the other, and determine that needle, when freely placed, to point north and ſouth, and enable it to communicate by its bare touch the ſame properties, and abundance of other ſtrange ones, to another piece of ſteel. But I forbear to alledge particular examples referable to the ſeveral qualities above-mentioned, whether manifeſt or hidden, becauſe that in great part is already done in our notes about particular qualities, in which it will appear, how little able the employing of alcali and acidum will be to afford us an account of many things. And though I enlarge not here on this objection, yet I take it to be of that importance, that, though there were no other, this were enough to ſhew, that the hypotheſis, that is liable to it, is inſufficient for the explication of qualities; and therefore it will not, I preſume, be thought ſtrange, that I add, that as for thoſe, that would extend this narrow chemical doctrine to the whole object of natural philoſophy, they muſt do more, than I expect they will be able before they can make me their proſelyte, there being a multitude of phænomena in nature (divers whereof I elſewhere take notice of in reference to the chemiſt's philoſophy) in which what acidum and alkali have to do, I confeſs, I do not underſtand.

CHAP. VI.

THE laſt thing (which comprizes ſeveral others) that ſeems to me a defect in the doctrine of alcali and acidum, is, that divers, if not moſt of thoſe very things, that are pretended to be explicated by them, are not ſatisfactorily explicated, ſome things being taken into the explications, that are either not fundamental enough, or not clearly intelligible, or are chargeable with both thoſe imperfections.

AND firſt I am diſſatisfied with the very fundamental notion of this doctrine, namely a ſuppoſed hoſtility between the tribe of acids and that of alkalies, accompanied, if you will have it ſo, with a friendſhip or ſympathy with bodies belonging to the ſame tribe or family. For I look upon amity and enmity as affections of intelligent beings; and I have not yet found it explained by any, how thoſe appetites can be placed in bodies inanimate and devoid of knowledge, or of ſo much as ſenſe. And I elſewhere endeavour to ſhew, that what is called ſympathy and antipathy between ſuch bodies does, in great part, depend upon the actings of our own intellect, which ſuppoſing in every body an innate appetite to preſerve itſelf both in a defenſive and an offenſive way, inclines us to conclude, that that body, which, though deſignleſly, deſtroys or impairs the ſtate or texture of another body, has an enmity to it, though perhaps a ſlight mechanical change may make bodies, that ſeem extremely hoſtile, ſeem to agree very well and co-operate to the production of the ſame effects. As if the acid ſpirit of ſalt and the volatile alkali (as they will have it) that is commonly called ſpirit of urine be put together, they will, after a ſhort, though fierce conflict, upon a new contexture unite together into a ſalt, little, if at all, differing from ſal armoniac, in which the two reconciled principles will amicably join in cooling of water, diſſolving ſome metalline bodies, and producing divers other effects. And ſo, if upon a ſtrong ſolution of ſalt of pot-aſhes, or of ſalt of tartar, good ſpirit of nitre be dropped in a due proportion, after the heat and tumult and ebullition are over, the acid and the alkalizate ſalts will convene into ſuch a concretion as ſalt-petre, which is taken to be a natural body, either homogeneous, or at leaſt conſiſting of parts, that agree very friendly together, and conſpired to conſtitute the particular kind of ſalt, that chemiſts call nitre.

BUT the ſympathy and antipathy, that is ſaid to be betwixt inanimate bodies, I elſewhere more particularly conſider; and therefore I ſhall now add in the ſecond place, that

that the explications made of phænomena, according to the doctrine of alkali and acidum, do not, in my apprehension, perform what may be justly expected from philosophical explications. It is said indeed, that the acidum working on the alkali, or this upon that, produces the effect proposed; but that is only to tell us, what is the agent, that operates, and not the manner of the operation, or the means and process, whereby it produces the effect proposed, and it is this modus, that inquisitive naturalists chiefly desire to learn. And if it be said, that it is by the mutual hostility of the principles, that the effect is produced, it may be answered, that besides that that hostility itself is not, as we have just now observed, a thing clear, if so much as intelligible; this is so general and indeterminate a way of explicating things, as can afford little or no satisfaction to a searching and cautious naturalist, that considers how very numerous and very various the phænomena of qualities are.

CHAP VII.

TO clear up and to countenance what I have been now saying, I shall only take notice of some few obvious phænomena of one of the most familiar operations wherein acidum and alkali are supposed to be the grand agents. It is known to the very boys of chemists, that aqua regis will dissolve gold, copper and mercury, and that with these metals, especially with the second, it will produce an intense degree of heat. If now the cause of this heat be demanded, it may be expected, that the patrons of the Duellists will answer, that it is from the action of the acid salts of the menstruum upon the alkali they meet with in the metals. But not to mention how many things are here presumed, not proved; nor that I know some acid menstruums, and some much more evidently alkalizate bodies than these metals are, which yet do not upon their mixtures produce any sensible heat; not, I say, to mention these, it is easy to discern, that this answer names indeed two supposed efficients of heat, but does not explicate or declare how these agents produce that quality, which depends upon a certain vehement and various agitation of the singly insensible parts of bodies, whether the Duellists, or any other, though very differing causes, put them into a motion so modified. And therefore gold and copper, by bare concussion, may be brought to an intense degree of heat, without the accession of any acid parts to work upon them. But then further, when we are told, that aqua regis by its acidity working on the metalline alkali makes a dissolution of the metal; I am told indeed, what they think to be the agent in this change, but not at all satisfied how this agent effects it; for, copper being a very hard metal, and gold generally esteemed by chemists the closest and compactest body in nature, I would gladly know, by what power and way such weak, and probably either brittle or flexible bodies, as acid salts, are enabled with that force to disjoin such solid and closely coherent corpuscles, as make up the visible masses of copper and gold, nay, and scatter them with that violence, as, perhaps to toss up multitudes of them into the air. And since in the dissolution of these metals there is another phænomenon to be accounted for, as well as the forcing of the parts asunder, namely the sustentation of the metal in the menstruum, the chemists would have much informed me, if they had well explained, how their acidum and alkali is able to sustain and give fluidity to the corpuscles of the dissolved metal, which though it be but copper, is nine times as heavy as a bulk of water equal to it, and if it be gold, is nineteen times heavier than the liquor, that must keep it from sinking; and at least divers times heavier in specie than the salts, that are mingled with the aqueous parts, can make the menstruum composed of them both. Whereas trial has assured me, that, if a piece of wax, or any other such matter, be made by less than the hundredth part heavier than an equal bulk of water, it will, when

when thoroughly immerſed, fall to the bottom, and reſt there. I might alſo aſk a further queſtion about theſe diſſolutions, as why, whereas aqua regis diſſolves mercury, without being much changed in colour by it, gold retains its own citrinity or yellowneſs in the ſolvent, and the ſolution of copper is of a colour, which being greeniſh-blew is quite differing from that of the metal, that affords it, as well as from that of the ſolvent? And I might recruit theſe with other queries not impertinent, but that theſe may ſuffice (for a ſample) on this occaſion, and allow me to conclude this chapter, by repreſenting one thing, which I would gladly recommend and inculcate to you, namely, that " Thoſe hypotheſes do not a little hinder the progreſs of human knowledge, that " introduce morals and politicks into the explications of corporeal nature, where all " things are indeed tranſacted according to laws mechanical."

C H A P. VIII.

I MIGHT eaſily have been more copious in the inſtances annexed to the foregoing animadverſions, but that, being deſirous to be ſhort as well as clear, I purpoſely declined to make uſe of divers others, that ſeemed proper to be employed, and indeed might ſafely enough have been ſo, becauſe thoſe I have mentioned and eſpecially thoſe (which make a great part of them) that are mechanical, are not liable to the ſame exceptions, that I foreſaw might be made to elude the force of the examples I paſſed by. And though I think I could very well make thoſe foreſeen objections appear groundleſs or unſatisfactory; yet that could ſcarce be done without engaging in controverſies, that would prove more tedious than I judged them neceſſary.

AND yet, although what I have ſaid in this excurſion be but a part of what I could ſay, I would not be thought to have forgot what I intimated at the beginning of it. For though the reaſons I alledged keep me from acquieſcing in the doctrine of alkali and acidum, as it is propoſed under the notion of a philoſophical hypotheſis, ſuch as the Carteſian or Epicurean, which are each of them alledged by their embracers to be mechanical, and of a very Catholick extent; yet I deny not, that the conſideration of the Duelliſts (or the two jarring principles of alkali and acidum) may be of good uſe to Spagyriſts and phyſicians, as I elſewhere further declare. Nor do I pretend by the paſt diſcourſe, that queſtions one doctrine of the chemiſts, to beget a general contempt of their notions, and much leſs of their experiments. For the operations of chemiſtry may be miſapplied by the erroneous reaſonings of the artiſts, without ceaſing to be themſelves things of great uſe, as being applicable, as well to the diſcovery or confirmation of ſolid theories, as the production of new phænomena, and beneficial effects. And though I think, that many notions of *Paracelſus* and *Helmont*, and ſome other eminent Spagyriſts, are unſolid, and not worthy the veneration, that their admirers cheriſh for them; yet divers of the experiments, which either are alledged to favour theſe notions, or on other accounts are to be met with among the followers of theſe men, deſerve the curioſity, if not the eſteem, of the induſtrious inquirers into nature's myſteries. And looking upon chemiſtry in groſs as a diſcipline ſubordinate to phyſicks, even mechanical philoſophers may juſtly, in my opinion, think favourably of it, ſince, whatever imperfections, or, if they pleaſe, extravagancies there may be in the principles and explications of *Paracelſus* or other leading artiſts, theſe faults of the theorical part may be ſufficiently compenſated by the utilities, that may be derived from the practical part. And this I am the rather induced to ſay, becauſe the experiments, that chemiſtry furniſhes, may much aſſiſt a naturaliſt to rectify the erroneous theories, that oftentimes accompany them, and even thoſe (miſtakes) that are endeavoured to be evinced by them.

AND (to conclude) chemiſtry ſeems to deal with men, in reference to notions, as it does in reference to metals, aſſiſting wary men to detect the errors, unto which it may have miſled the unwary: for the ſame art, that has taught ſome to impoſe on others, (and perhaps themſelves firſt) by blanching copper, imitating gold, &c. does alſo ſupply ſay-maſters and refiners, with the means, by the cupel, cements, aqua fortis, &c. to examine, whether coins be true or falſe, and diſcover adulterate gold and ſilver to be counterfeit.

EXPERIMENTS and NOTES
ABOUT THE
Mechanical Origin and Production of VOLATILITY.

ADVERTISEMENTS about the EXPERIMENTS and NOTES relating to CHEMICAL QUALITIES.

WHEN, after I had gone through the common operations of chemiſtry, I began to make ſome ſerious reflections on them, I thought it was pity, that inſtruments, that might prove ſo ſerviceable to the advancement of natural philoſophy, ſhould not be more ſtudiouſly and ſkilfully made uſe of to ſo good a purpoſe. I ſaw indeed, that divers of the chemiſts had, by a diligent and laudable employment of their pains and induſtry, obtained divers productions, and lighted on ſeveral phænomena, conſiderable in their kind, and indeed more numerous, than, the narrowneſs and ſterility of their principles conſidered, could well be expected. But I obſerved too, that the generality of thoſe, that buſy themſelves about chemical operations; ſome, becauſe they practiſe phyſick, and others, becauſe they either much wanted, or greedily coveted money, aimed, in their trials, but at the preparation of good medicines for the human body, or to diſcover the ways of curing the diſeaſes or imperfections of metals, without referring their trials to the advancement of natural philoſophy in general; of which moſt of the alchymiſts ſeem to have been ſo incurious, that not only they did not inſtitute experiments for that purpoſe, but overlooked and deſpiſed thoſe undeſigned ones, that occurred to them, whilſt they were proſecuting a preparation of a medicine, or a tranſmutation of metals. The ſenſe I had of this too general omiſſion of the chemiſts, tempted me ſometimes to try, whether I could do any thing towards the repairing of it by handling chemiſtry, not as a phyſician, or an alchymiſt, but as a meer naturaliſt, and ſo by applying chemical operations to philoſophical purpoſes. And, in purſuance of theſe thoughts, I remember I drew up a ſcheme of what I ventured to call a *chemia philoſophica*, not out of any affectation of a ſplendid title, but to intimate, that the chemical operations, there treated of, were not directed to the uſual ſcopes of phyſicians, or tranſmuters of metals, but partly to illuſtrate, or confirm ſome philoſophical theories by ſuch operations; and partly to explicate thoſe operations, by the help of ſuch theories.

BUT before I had made any great progreſs in the purſuit of this deſign, the fatal peſtilence, that raged in *London*, and in many other parts of *England*, in the years 1664 and 65, obliging me, among the reſt, to make ſeveral removes, which put me upon

upon taking new meaſures, and engaging me in other employments of my time, made me ſo long neglect the papers I had drawn up, that, at laſt, I knew not where to find them, (though, I hope, they are not yet miſlaid beyond recovery,) which I was the leſs troubled at, becauſe the great difficulties, to be met with in ſuch an undertaking, did not a little diſcourage me, ſuch a taſk requiring, as well as deſerving, a perſon better furniſhed, than I had reaſon to think myſelf, with abilities, leiſure, chemical experiments, and conveniences, to try as many more, as ſhould appear needful. But yet, to break the ice for any, that may hereafter think fit to ſet upon ſuch a work, or, to ſhorten my own labour, if I ſhould ſee cauſe to reſume it myſelf, I was content to throw in, among my notes about other particular qualities, ſome experiments and obſervations about ſome of thoſe, that I have elſewhere called chemical qualities, becauſe it is chiefly by the operations of chemiſts, that men have been induced to take ſpecial notice of them. Of theſe notes I have aſſigned to ſome qualities more, and to ſome fewer, as either the nature or importance of the ſubject ſeemed to require, or my leiſure and other circumſtances would permit. And though I have not here handled the ſubjects they belonged to, as if I intended ſuch a *chemia philoſophica*; as I lately mentioned, becauſe my deſign did not make it neceſſary, but did, perhaps, make it impertinent for me to do ſo; yet, in ſome of the larger notes, about volatility and fixedneſs, and eſpecially about precipitation, I have given ſome little ſpecimens of the theorical part of a philoſophical account of thoſe qualities, or operations, that, I hope, will not be wholly uſeleſs. I know, it may be objected, that I ſhould have employed, for inſtances, ſome more conſiderable experiments, if not arcana; but, though poſſibly I am not altogether unfurniſhed with ſuch, yet, aiming rather to promote philoſophy, than appear a poſſeſſor of elaborate proceſſes, I declined ſeveral experiments, that required either more ſkill, or more time, or more expence, than could be well expected from moſt readers, and choſe rather to employ ſuch experiments, as may be more eaſily or cheaply tried; and, which is mainly to be conſidered, being more ſimple, are more clearly intelligible, and more fit to have notions and theories built upon them; eſpecially conſidering, that the doctrine of qualities being itſelf converſant about ſome of the rudimental parts, if I may ſo call them, of natural philoſophy, it ſeemed unfit to employ intricate experiments, and whoſe cauſes were liable to many diſputes, to ſettle a theory of them. In ſhort, my deſign being to hold a taper, not ſo much to chemiſts, as to the naturaliſts, it was fit I ſhould be leſs ſollicitous to gratify the former, than to inform the latter.

EXPERIMENTS and NOTES, about the MECHANICAL ORIGIN and PRODUCTION of VOLATILITY.

CAHP. I.

AS far as I have yet obſerved, the qualifications or attributes, on whoſe account a portion of matter is found to be volatile, are chiefly four; whereof the three former moſt regard the ſingle corpuſcles, as ſuch; and the laſt, the manner of their union in the aggregate or body they make up.

BUT before I enter upon particulars, give me leave to advertiſe you here, once for all, that, in the following notes about volatility and fixedneſs, when I ſpeak of the corpuſcles, or minute parts of a body, I do not mean ſtrictly either the elementary parts, ſuch as earth and water, or the hypoſtatical principles, ſuch as ſalt, ſulphur, or mercury; for theſe things come not here into conſideration: but only ſuch corpuſcles, whether of a ſimple, compounded, or decompounded nature, as have the particles they conſiſt

consist of so firmly united, that they will not be totally disjoined, or dissipated, by that degree of fire or heat, wherein the matter is said to be volatile, or to be fixed. But these combined particles will, in their aggregate, either ascend, or continue unraised *per modum unius*, (as they speak) or as one entire corpuscle. As in a corpuscle of sal armoniac, whether it be a natural or factitious thing, or whether it be perfectly similar, or compounded of differing parts, I look upon the entire corpuscle, as a volatile portion of matter; and so I do on a corpuscle of sulphur, though experience shews, when it is kindled, that it has great store of acid salt in it, but which is not extricated by bare sublimation: and so colcothar of vitriol falls under our consideration, as a fixed body, without enquiring what cupreous or other mineral, and not totally fixed parts, may be united with the earthly ones; since the fires, we expose it to, do not separate them.

AND this being premised in the general, I now proceed to some particulars. And first, to make a volatile body, the parts should be very small. For, *cæteris paribus*, those, that are so, are more easily put into motion by the action of the fire, and other agents, and consequently more apt to be elevated, when, by the determination of the movent, the situation of the neighbouring bodies, or other mechanical circumstances, the agitated corpuscles can continue their motion with less resistance upwards, than any other way, (as either downwards, or horizontally.) And if, as it is highly probable, that, which in light bodies, or at least, in most of them, is wont to pass for positive levity, be but a less degree of gravity, than that of those contiguous bodies, that raise them; it will happen, that, in very many cases, (for, I say, not in all,) the great proportion of the surface of a corpuscle to its bulk, (which is usually greater in the lesser particles,) by making it more apt to be wrought on, either by the air agitated by the fire, or by the effluvia of kindled fuel, or by the impulse of the shaken corpuscles of the body itself, will much facilitate the elevation of such a minute particle, by exposing a greater portion of it to the action of the agent, as it will oftentimes also facilitate the renewed sustentation of such a small body in the air, which resists more the descent of particles, whose surfaces are large, than of others of the same gravity and bulk: as a leaf of paper displayed will much longer hover in the air, than if it were reduced into a ball or pellet. That this minuteness of particles may dispose them to be carried upwards, by the impulse of other bodies, and that of the agitated air, is very obvious to be observed: as we see, that horses in a highway, though they be not able, with the strokes of their feet, to make stones, or gravel, or clods of earth fly up, yet they will easily raise clouds of dust, oftentimes mingled with the smaller grains of sand. And, where timber is sawing, the same wind, that will not, in the least, move the beams, and scarce at all move the chips, will easily carry up the saw-dust into the air. And we see in our chimneys, that the smoke readily ascends, whilst even small clods of soot, which is but an aggregate of the particles of smoke, fall headlong down.

CHAP. II.

THE next qualification requisite in the corpuscles of volatile bodies is, that they be not too solid or heavy. For if they be so, though their bulk be very small, yet, unless other circumstances do much compensate their weight, it will be very difficult to elevate them, because of the great disproportion of their specific gravity to that of the air, (which contributes to sustain and even raise many sorts of volatile parts) and to the strength of the igneous effluvia or other agents, that would carry them up. Thus we see, that filings of lead or iron, and even minium (which is the calx of lead) though the grains they consist of be very small, will not easily be blown up like common dust, or meal, or other powders made of less ponderous materials.

A third

A third qualification to be desired in the corpuscles, that should make up a volatile body, is, that they be conveniently shaped for motion. For if they be of branched, hooked, or other very irregular or inconvenient figures, they will be apt to be stopped and detained by other bodies, or intangled among themselves, and consequently very difficult to be carried upwards, in regard that, whilst they are thus fastened, either to one another, or to any stable body, each single corpuscle is not only to be considered, as having its own peculiar bulk, since its cohesion with the other corpuscle or body, that detains it, makes them fit to be looked upon *per modum unius*; that degree of heat, they are exposed to, being presumed uncapable of disjoining them. And this may be one reason, why water, though it be specifically heavier than oil, yet is much more easily brought to exhale in the form of vapours than is oil, whose corpuscles by the lasting stains they leave on cloth, wood, wool, &c. (which water will but transiently moisten, not stain) seem to be of very intangling figures.

THE fourth and last qualification requisite in a volatile body is, that the parts do loosely adhere, or at least be united in such a manner, as does not much indispose them to be separated by the fire in the form of fumes or vapours.

FOR he, that considers the matter, will easily grant, that, if the contexture of the corpuscles, whereof a body consists, be intricate, or their cohesion strong, their mutual implication, or their adherence to each other, will make one part hinder another from flying separately away, and their conjunction will make them too heavy or unweildy to be elevated together, as intire, though compounded parts. Thus we see, that in spring, or the beginning of summer, a wind, though not faint, is unable to carry off the lightest leaves of trees, because they stick fast to the bows and twigs on which they grow, but in Autumn, when that adhesion ceases, and the leaves sit but loosely on, a wind no stronger than that they resisted before, will with ease blow them off, and perhaps carry them up a good way into the air. But here note, that it was not without some cause, that I added above, that in a fluid body, the parts should at least be united in such a manner, as does not much indispose them to be separated. For it is not impossible, that the parts of a body may, by the figures and smoothness of the surfaces, be sufficiently apt to be put into motion, and yet be indisposed to admit such a motion as would totally separate them and make them fly up into the air. As, if you take two pieces of very flat and well polished marble or glass, and lay them one upon the other, you easily make them slide along each other surfaces, but not easily pull up one of them, whilst the other continues its station. And when glass is in the state of fusion, the parts of it will easily slide along each other, as is usual in those of other fluids, and consequently change places, and yet the continuity of the whole is not intirely broken, but every corpuscle does somewhere touch some other corpuscle, and thereby maintain the cohesion, that indisposes it for that intire separation accompanied with a motion upwards, that we call avolation. And so, when saltpetre alone is in a crucible exposed to the fire, though a very moderate degree of it will suffice to bring the salt to a state of fusion, and consequently to put the corpuscles, that compose it, into a restless motion; yet a greater degree of heat, than is necessary to melt it, will not extricate so much as the spirits, and make them fly away.

CHAP. III.

THE foregoing doctrine of the volatility of bodies may be as well illustrated as applied, if we proceed to deduce from it the general ways of volatilization of bodies, or of introducing volatility into an assigned portion of matter. For these ways seem not inconveniently reducible to five, which I shall severally mention, though nature and

and art do usually employ two or more of them in conjunction. For which reason I would not, when I speak of one of these ways, be understood, as if, excluding the rest, I meant, that no other concurred with it.

THE first of the five ways or means of volatilizing a body is, to reduce it into minute parts, and, *caeteris paribus*, the more minute they are the better.

THAT the bringing a body into very minute parts may much conduce to the volatilizing of it, may be gathered from the vulgar practice of the chemists, who, when they would sublime or distil antimony, sal armoniac, sea-salt, nitre, &c. are wont to beat them to powders to facilitate their receiving a further comminution by the action of the fire. And here I observe, that in some bodies this comminution ought not to be made only at first, but to be continued afterwards. For chemists find by experience, though perhaps, without considering the reason of it, that sea-salt and nitre will very hardly afford their spirits in distillations, without they be mingled with powdered clay or bole, or some such other additament, which usually twice or thrice exceeds the weight of the salt itself: although these additaments, being themselves fixed, seem unlikely to promote the volatilization of the bodies mixed with them, yet by hindering the small grains of salt to melt together into one lump or mass, and consequently by keeping them in the state of comminution, they much conduce to the driving up of the spirits, or the finer parts of the salts by the operation of the fire.

BUT to prosecute a little what I was saying of the conduciveness of bringing a body into small parts to the volatilization of it, I shall add, that in some cases the comminution may be much promoted by employing physical, after mechanical, ways; and that, when the parts are brought to such a pitch of exiguity, they may be elevated much better than before. Thus, if you take filings of Mars, and mix them with sal armoniac, some few parts may be sublimed; but if, as I have done, you dissolve those filings in good spirit of salt, instead of oil of vitriol, and having coagulated the solution, you calcine the greenish crystals or *vitriolum martis*, that will be afforded, you may with ease, and in no long time, obtain a *crocus martis* of very fine parts; so that I remember, when we exquisitely mingled this very fixed powder with a convenient proportion of sal armoniac, and gradually pressed it with a competent fire, we were able to elevate at the first sublimation a considerable part of it; and adding a like, or some what inferior, proportion of fresh sal armoniac to the *caput mortuum*, we could raise so considerable a part of that also, and in it of the crocus, that we thought, if we had had conveniency to pursue the operation, we should, by not many repeated sublimations, have elevated the whole crocus, which (to hint that upon the by,) afforded a sublimate of so very astringent a taste, as may make the trial of it in stanching of blood, stopping of fluxes, and other cases, where potent astriction is desired, worthy of a physician's curiosity.

CHAP. IV.

THE second means to volatilize bodies is, to rub, grind, or otherwise reduce their corpuscles to be either smooth, or otherwise fitly shaped to clear themselves, or be disintangled from each other.

BY reason of the minuteness of the corpuscles, which keeps them from being separately discernible by the eye, it is not to be expected, that immediate and ocular instances should be given on this occasion; but that such a change is to be admitted in the small parts of many bodies, brought to be volatile, seems highly probable from the account formerly given of the requisites or conditions of volatility, whose introduction into a portion of matter, will scarce be explicated without the intervention of such

ſuch a change. To this ſecond inſtrument of volatilization, in concurrence with the firſt, may probably be referred the following phænomena: in the two firſt of which there is employed no additional volatile ingredient; and in the fourth, a fixed body is diſpoſed to volatility by the operation of a liquor, though this be carefully abſtracted from it.

1. If urine freſhly made be put to diſtil, the phlegm will firſt aſcend, and the volatile ſalt will not riſe till that be almoſt totally driven away, and then requires a not inconſiderable degree of fire to elevate it. But, if you putrify or digeſt urine, though in a well-cloſed glaſs veſſel, for ſeven or eight weeks, that gentle warmth will make the ſmall parts ſo rub againſt, or otherwiſe act upon, one another, that the finer ones of the ſalt, will perhaps, be made more ſlender and light, and however will be made to extricate themſelves ſo far, as to become volatile, and, aſcending in a very gentle heat, leave the greateſt part of the phlegm behind them.

2. So, if muſt, or the ſweet juice of grapes, be diſtilled, before it have been fermented, it is obſerved by chemiſts, and we have tried the like in artificial wine made of raiſins, that the phlegm, but no ardent ſpirit, will aſcend. But when this liquor is reduced to wine by fermentation, which is accompanied with a great and inteſtine commotion of the juſtling parts, hitting and rubbing againſt one another, whereby ſome probably come to be broken, others to be variouſly ground and ſubtilized, the more ſubtile parts of the liquor being extricated, or ſome of the parts being, by theſe operations, brought to be ſubtile, they are qualified to be raiſed by a very gentle heat before the phlegm, and convene into that fugitive liquor, that chemiſts, for its activity, call ſpirit of wine. Nor is it only in the ſlighter inſtances afforded by animals and vegetables, that volatility may be effected by the means lately mentioned; for experience hath aſſured me, that it is poſſible, by an artificial and long digeſtion, wherein the parts have leiſure for frequent juſtlings and attritions, ſo to ſubtilize and diſpoſe the corpuſcles, even of common ſalt, for volatility, that we could make them aſcend in a moderate fire of ſand, without the help of bole, oil of vitriol, or any volatilizing additament; and, which is more conſiderable, the ſpirit would, in riſing, precede the phlegm, and leave the greateſt part thereof behind it.

This inteſtine commotion of parts, capable of producing volatility in the more diſpoſed portions of a body, though it be much more eaſy to be found in liquors, or in moiſt and ſoft bodies, yet I have ſometimes, though rarely, met with it in dry ones. And particularly I remember, that, ſome years ago, having, for trial-ſake, taken muſtard-ſeed, which is a body pregnant with ſubtile parts, and cauſed it to be diſtilled *per ſe* in a retort, I had, as I hoped, (without any more ado,) a great many grains of a clear and figured volatile ſalt at the very firſt diſtillation: which experiment having, for the greater ſecurity, made a ſecond time with the like ſucceſs, I mentioned it to ſome lovers of chemiſtry, as what, I juſtly ſuppoſed, they had not heard of. I leave it to farther enquiry, whether, in a body ſo full of ſpirits, as muſtard-ſeed, the action or re-action of the parts among themſelves, perhaps promoted by juſt degrees of fire, might not ſuffice to make in them a change equivalent in order to volatilization, and the yielding a volatile ſalt, to that, which we have obſerved fermentation and putrefaction to have made in the juice of grapes, urine, and ſome other bodies. How far the like ſucceſs may be expected in other trials, I cannot tell; eſpecially, not having by me any notes of the events of ſome attempts, which that enquiry put me upon: only, I remember in general, that, as ſome trials, I made with other ſeeds, and even with aromatic ones, did not afford me any volatile ſalt; ſo the ſucceſs of other trials made me now and then think, that ſome ſubjects of the vegetable kingdom, whence we are wont to drive over acid ſpirits, but no dry ſalt, may be diſtilled with ſo luckily regulated a heat, as to afford something, though but little, of volatile salt; and, that perhaps more bodies would be found to do so, were they not too hastily or violently pressed by the fire, whereby such saline schematisms of the desired parts of the matter are (by being dissipated or confounded) destroyed or vitiated, as in a slow, dextrous, or fortunate way of management would come forth, not in a liquid, but a saline form. Of which observation, we may elsewhere mention some instances, and shall, before the close of this paper, name one, afforded us by crude tartar.

3. THOUGH silver be one of the fixedest bodies, that we know of, yet, that it is not impossible, but that, chiefly by a change of texture, it may strangely be disposed to volatility, I was induced to think, by what I remember once happened to me. A gentleman of my acquaintance, studious of chemical arcana, having lighted on a strange menstruum, which he affirmed, and I had some cause to believe, not to be corrosive, he abstracted it from several metals, (for the same liquor would serve again and again,) and brought me the remainders, with a desire, that I would endeavour to reduce those of lead and silver into the pristine metals again, which he had, in vain, attempted to do: whereupon, though I found the white calx of lead reducible, yet, when I came to the calx of silver, I was not able to bring it into a body; and having, at length, melted some lead in a gentle fire, to try whether I could make it swallow up the calx, in order to a farther operation, I was not a little surprized to find, that this mild heat made the calx of silver presently fly away, and sublime in the form of a *farina volatilis*, which whitened the neighbouring part of the chimney, as well as the upper part of the crucible.

4. FROM that, which chemists themselves tell us, I think we may draw a good argument *ad hominem*, to prove, that volatility depends much upon the texture, and other mechanical affections of a body. For divers of those Hermetick philosophers (as they are called) that write of the elixir, tell us, that when their philosophick mercury or grand solvent, being sealed up together with a third or fourth part of gold in a glass egg, is kept in convenient degrees of fire, the whole matter, and consequently the gold, will, by the mutual operation of the included substances, be so changed, that not only it will circulate up and down in the glass, but, in case the digestion or decoction should be broken off at a certain inconvenient time, the gold would be quite spoiled, being, by the past and untimely ended operation, made too volatile to be reducible again into gold: whereas, if the decoction be duly continued unto the end, not only the gold, but all the philosophical mercury or menstruum will be turned into a sulphur, or powder of a wonderfully fixed nature. I know, there are several Chrysopæans, that speak much otherwise of this operation, and tell us, that the gold employed about it must be philosophic gold: but I know too, that there are divers others, (and those too none of the least candid or rational,) that speak of it, as I have done; and that is sufficient to ground an argument on towards all those, that embrace their doctrine. And, in this case, it is considerable, that it is not by any superadded additament, that the most fixed body of gold is made volatile, but the same massy matter, consisting of gold and philosophic mercury, is, by the change of texture produced, or occasioned by the various degrees and operations of fire upon it, brought to be first volatile, and then extreamly fixed. And having said this, in reference to one tribe of the modern Spagyrists, to another of them, the *Helmontians*, I think, I can offer a good argument *ad hominem* from the testimony and experiments of the founder of their sect.

5. THE acute *Helmont*, among other prodigious powers, that he ascribes to the alkahest, affirms, that, by abstracting it frequently enough, it would so change all tangible bodies, and consequently stones and metals, that they might be distilled over into liquors

equiponderant

equiponderant to the reſpective bodies, that afforded them, and having all the qualities of rain water; which if they have, I need not tell you, that they muſt be very volatile. And I ſee not how thoſe, that admit the truth of this ſtrange alkaheſtical operation, can well deny, that volatility depends upon the mechanical affections of matter, ſince it appears not, that the alkaheſt does, at leaſt in our caſe, work upon bodies otherwiſe than mechanically. And it muſt be confeſſed, that the ſame material parts of a portion of a corporeal ſubſtance, which, when they were aſſociated and contexed (whether by an archeus, ſeed, form, or what elſe you pleaſe,) after ſuch a determinate manner, conſtituted a ſolid and fixed body, as a flint or a lump of gold; by having their texture diſſolved, and (perhaps after being ſubtilized) by being freed from their former implications, or firm coheſions, may become the parts of a fluid body totally volatile.

CHAP. V.

THE fourth means of making a body volatile is, by aſſociating the particles to be raiſed with ſuch as are more volatile than themſelves, and of a figure fit to be faſtened to them, or are at leaſt apt, by being added to them, to make up, with them, corpuſcles more diſpoſed than they to volatility. This being the grand inſtrument of volatilization, I ſhall ſpend ſomewhat the more time about it. But I ſhall firſt here a little explain the laſt clauſe, (that I may not be obliged to reſume it elſewhere) by intimating, that it is not impoſſible, that the particles of an additament, though not more volatile than thoſe of the body it is mixed with, and perhaps, though not volatile at all, will yet conduce to volatilize the body wherewith it is mingled. For the particles of the additament may be of ſuch figures, and ſo aſſociated with thoſe of the body to be elevated, as in this to enlarge the former pores, or produce new ones, by intercepting little cavities (for they muſt not be great ones) between the particles of a body to be raiſed, and thoſe of the additament. For, by theſe and other ſuch ways of aſſociation, the corpuſcles, reſulting from the combination or coalition of two or more of theſe differing particles, may, without becoming too big and unweildy, become more conveniently ſhaped, or more light in proportion to their bulk, and ſo more eaſily buoyed up and ſuſtained in the air, (as when the lid of a copper box being put on, makes the whole box emerge and ſwim in water, becauſe of the intercepted cavity, though neither of the parts of the box would do ſo,) or otherwiſe more fitted for avolation than the particles themſelves were, before their being joined to thoſe of the additament.

BY two things, chiefly, the corpuſcles of the additament may contribute to the elevation of a body. For, firſt, the parts of the former may be much more diſpoſed for avolation than is neceſſary to their own volatility. As when in the making of ſal armoniac, the ſalline particles of urine and of ſoot, are more fugitive than they need be, to be themſelves ſublimed, and thereby are advantaged to carry up with them the more ſluggiſh corpuſcles, whereof ſea ſalt conſiſts. And next, they may be of figures ſo proper to faſten them well to the body to be elevated, that the more fugitive will not be driven away, or disjoined from the more fixed by ſuch a degree of heat as is ſufficient to raiſe them both together: to which effect the congruity, or figuration, is as well required, as the lightneſs or volatility of the particles of the additament. And therefore ſome of the fugitiveſt bodies, that we know, as ſpirit of wine, camphire, &c. will not volatilize many bodies, which will be elevated by far leſs fugitive additaments; becauſe the corpuſcles of ſpirit of wine ſtick not to thoſe of the body they are mingled with, but, eaſily flying up themſelves, leave thoſe behind them, which they did rather barely touch, than firmly adhere to: whereas far leſs fugacious liquors, if they be endowed with figures, that fit them for a competently firm coheſion with the body they are mingled with, will be able to volatilize it. Of which I ſhall now give you ſome inſtances in bodies, that are very ponderous, or very fixed, or both.

AND I ſhall begin with colcothar, though it being a vitriolate calx, made by a laſting and vehement fire, it is (conſequently) capable of reſiſting ſuch a one. This being exquiſitely ground with an equal weight of ſal armoniac, which is itſelf a ſalt, but moderately volatile, will be in good part ſublimed into thoſe yellow flowers, which we have elſewhere more particularly taught to prepare, under the name of *ens primum veneris*; in which, that many vitriolate corpuſcles of the colcothar are really elevated, you may eaſily find, by putting a grain or two of that reddiſh ſubſtance into a ſtrong infuſion of galls, which will thereby immediately acquire an inky colour.

STEEL alſo, which, to deſerve that name, muſt have endured extraordinary violences of the fire, and greater than is needful to obtain other metals from their mother earth; ſteel itſelf, I ſay, being reduced to filings, and diligently ground with about an equal weight of ſal armoniac, will, if degrees of fire be ſkilfully adminiſtered, (for it is eaſy to err in that point,) without any previous calcination or reduction to a crocus, ſuffer ſo much of the metal to be carried up, as will give the ſal armoniac a notable colour, and an ironiſh taſte.

AND here it will be proper to obſerve, for the ſake of practical chemiſts, that the quantity or proportion of the volatile additament is to be regarded; though not ſo much as its nature, yet more than it is wont to be: and divers bodies, that are thought either altogether unfit for ſublimation, or, at leaſt, uncapable to have any conſiderable portion of them elevated, may be copiouſly enough ſublimed, if a greater proportion of the additament, than we uſually content ourſelves with, be ſkilfully employed. And in the newly-mentioned inſtance of filings of ſteel, if, inſtead of an equal weight of ſal armoniac, the treble weight be taken, and the operation be duly managed, a far greater quantity of the metal may be raiſed, eſpecially if freſh ſal armoniac be carefully ground with the caput mortuum. And ſal armoniac may, perhaps, be compounded with ſuch other bodies, heavier than itſelf, as may qualify it, when it is thus clogged, to elevate ſome congruous bodies better than it would of itſelf alone. And I ſhall venture to add this farther advertiſement, that if, beſides the plenty of the additament, there be a ſufficient fitneſs of its particles to lay hold on thoſe of the body to be wrought on, mineral bodies, and thoſe ponderous enough, may be employed to volatilize other heavy bodies. And I am apt to think, that almoſt, if not more than almoſt, all metals themſelves may by copious additaments and frequent cohobations be brought to paſs through the neck of the retort in diſtillation; and perhaps, if you melt them not with equal parts, but with many parts of regulus of antimony, and then proceed as the hints now given will direct you, you will not find cauſe to deſpiſe what I have been ſaying.

YOU know what endeavours have been, and are ſtill fruitleſsly employed by chemiſts to elevate ſo fixed a body, as ſalt of tartar, by additaments. I ſhall not now ſpeak much of the enterprize in general, deſigning chiefly to tell you on this occaſion, that, whereas frequent experience ſhews, that ſal armoniac being abſtracted from ſalt of tartar, not only the ſalt of tartar is left at the bottom, but a good part of the ſal armoniac is left behind with it: I ſuſpected the cauſe might be, that ſal armoniac, by the operation of the alkali of tartar, is reduced into ſea-ſalt, and urinous or fuliginous ſalt, as it was at firſt compoſed of thoſe differing ingredients; and that by this means the volatile ſalt being looſened or diſintangled from the reſt, and being of a very fugacious nature, flies eaſily away itſelf, without ſtaying long enough to take up any other ſalt with it. And therefore, if this analyſis of the ſal armoniac could be prevented, it ſeemed not impoſſible to me, that ſome part of the ſalt of tartar, as well as of colcothar and ſteel, might be carried up by it: and accordingly having cauſed the ingredients to be exceedingly

ingly well dried, and both nimbly and carefully mixed, and ſpeedily expoſed to the fire, I have ſometimes had a portion of ſalt of tartar carried up with the ſal armoniac: but this happened ſo very rarely, that I ſuſpected ſome peculiar fitneſs for this work in ſome parcels of ſal armoniac, that are ſcarce but by the effect to be diſcerned from others. But however, what has happened to us may argue the poſſibility of the thing, and may ſerve to ſhew the volatilizing efficacy of ſal armoniac; which is a compound, that I elſewhere recommend, and do it now again, as one of the uſefulleſt productions of vulgar chemiſtry.

AND ſince I have mentioned the volatilization of ſalt of tartar, preſuming your curioſity will make you deſire my opinion about the poſſibility of it, I ſhall propoſe to you a diſtinction, that perhaps you do not expect, by ſaying, that I think there is a great deal of difference between the making a volatile ſalt of tartar, and the making ſalt of tartar volatile. For though this ſeem to be but a nicety, yet really it is none; and it is very poſſible, that a man may from tartar obtain a volatile ſalt, and yet be no wiſe able to volatilize that tartareous ſalt, that has been once by the incineration of the tartar brought to fixed alkali. I have in the Sceptical Chemiſt ſummarily delivered a way, by which both I, and ſome Spagyriſts, that learned it of me, obtained from a mixture of antimony, nitre and crude tartar, a volatile ſalt, which in probability comes from the laſt named of thoſe three bodies; but experience carefully made has aſſured me, that without any additament, by a diſtillation warily and very ſlowly made, (inſomuch that I have ſpent near a week in diſtilling one pound of matter) very clean tartar, or at leaſt the cryſtals of tartar, may, in conveniently ſhaped veſſels, be brought to afford a ſubſtance, that in rectification will aſcend to the upper part of the veſſel, in the form of a volatile ſalt, as if it were of urine or of hart's-horn; of which tartareous ſalt, I keep ſome by me: but this operation requires not only a dexterous, but a patient diſtiller.

BUT now as to the making a fixed alkali of tartar become volatile, I take it to be another, and have found it to be a far more difficult work; the common proceſſes of performing it being wont to promiſe much more than they can make good; which I may juſtly ſay of ſome other, that private men have vaunted for great arcana, but upon trial have ſatisfied me ſo little, that I have divers times offered pretenders to make ſalt of tartar volatile, that without at all inquiring into their proceſſes, I would lay good wagers, that they could do what they pretended; not only as divers philoſophical Spagyriſts require, without any viſible additament, but by any additament whatever; provided I were allowed to bring the ſalt of tartar myſelf, and to examine the ſucceſs, not by what may appear in the alembic and receiver, but by the weight of what would remain in the bottom. For I have convinced ſome of the more ingenuous artiſts, that the ſalt, that ſublimed, was not indeed the alkali of tartar, but ſomewhat, that was by the operation produced, or rather extricated out of the additaments. But yet I would not be thought to affirm, that it is not poſſible to elevate the fixed ſalt of tartar. For ſometimes I have been able to do it, even at the firſt diſtillation by an artificial additament perhaps more fixed than itſelf; but, though the operation was very grateful to me, as it ſhewed the poſſibility of the thing, yet the paucity of the ſalt ſublimed and other circumſtances, kept me from much valuing it upon any other account. And there are other ways, whereby experience has aſſured me, that ſalt of tartar may be raiſed. And if one of them were not ſo uncertain, that I can never promiſe before-hand, that it will at all ſucceed, and the other ſo laborious, difficult and coſtly, that few would attempt or be able to practiſe it, I ſhould think them very valuable things; ſince by the former way moſt part of the ſalt of tartar was quickly brought over in the form of a liquor, whoſe piercing ſmell was ſcarce tolerable; and by the latter way ſome ſalt of tartar of my own, being put into a retort, and urged but with ſuch a fire as could be given in a portable ſand-furnace, there remained not at the bottom near one half of the firſt weight, the additament having carried up the reſt, partly in the form of a liquor, but chiefly in that of a white ſublimate, which was neither ill-ſcented, nor in taſte corroſive, or alkalizate, but very mild, and ſomewhat ſweetiſh. And I do not much doubt, but that by other ways the fixed alkali of tartar may be elevated, eſpecially if, before it be expoſed to the laſt operation of the fire, it be dexterouſly freed from the moſt of thoſe earthy and viſcous parts, that I think may be juſtly ſuſpected to clog and bind the truly ſaline ones.

BUT I have too long digreſſed, and therefore ſhall intimate only upon the by, that even the ſpurious ſal tartari volatilized, that is made with ſpirit of vinegar, may, if it be well prepared, make amends for its empyreumatical ſmell and taſte, and may, notwithſtanding them, in divers caſes be of no deſpicable uſe, both as a medicine, and a menſtruum.

CHAP. VI.

BEFORE I draw towards a concluſion of theſe notes about volatility, perhaps it will not be amiſs, to take notice of a phænomenon, which may much ſurprize, and ſometimes diſappoint thoſe, that deal in ſublimations, unleſs they be forewarned of it. For though it be taken for granted, and for the moſt part may juſtly be ſo, that by carefully mingling what is ſublimed with what remains, and reſubliming the mixture, a greater quantity of the body to be ſublimed may be elevated the ſecond time than was the firſt, and the third time than the ſecond, and ſo onwards; yet I have not found this rule always to hold, but in ſome bodies, as particularly in ſome kinds of dulcified colcothar, the ſal armoniac, would at the firſt ſublimation carry up more of the fixed powder, than at the ſecond or third. So that I was by ſeveral trials perſuaded, when I found a very well and highly coloured powder elevated, to lay it by for uſe, and thereby ſave myſelf the labour of a proſecution, that would not only have proved uſeleſs, but prejudicial. And if I miſremember not, by often repeated cohobations, if I may ſo call them, of ſal armoniac upon crude or mineral antimony, though the ſublimate, that was obtained by the firſt operation, was much of it variouſly, and in ſome places richly coloured; yet afterwards, the ſalt aſcended from time to time paler and paler, leaving the antimony behind it. Which way of making ſome minerals more fixed and fuſible I conceive may be of great uſe in ſome medicinal preparations, though I think it not fit to particularize them in this place: where my chief intent was, to mention the phænomenon itſelf, and invite you to conſider, whether it may be aſcribed to this, that by the reiterated action of the fire, and grinding together of the body to be raiſed, either the corpuſcles of the ſal armoniac, or thoſe of the other body, may have thoſe little hooked or equivalent particles, whereby they take hold of one another, broken or worn off; and whether the indiſpoſedneſs of the colcotharine or antimonial parts to aſcend, may not in ſome caſes be promoted by their having, by frequent attritions, ſo ſmoothed their ſurfaces, that divers of them may cloſely adhere, like pieces of poliſhed glaſs, and ſo make up cluſters too unweildy to be ſo raiſed, as the ſingle corpuſcles they conſiſt of, were. Which change may diſpoſe them to be at once leſs volatile and more fuſible. Which conjectures I mention to excite you to frame better, or at leaſt to make amends for my omiſſion of examining theſe, by trying whether the ſal armoniac, grown white again, will be as fit as it was at firſt to carry up freſh bodies; and alſo by obſerving the weight of the unelevated part, and employing thoſe other ways of of examen, which I ſhould have done, if I had not then made ſublimations for another end, than to clear up the doctrine of volatility.

AND here it may be profitable to ſome chemiſts, though not neceſſary to my ſubject, to intimate, that ſublimations may be uſeful to make very fine comminutions of divers bodies. That thoſe, that are elevated are reduced to a great fineneſs of parts, is obvious to be obſerved in many examples, whence it has been anciently, not abſurdly, ſaid, that ſublimations are the chemiſts peſtles, ſince (as in flowers of ſulphur and antimony) they do really reſolve the elevated bodies into exceeding fine flower, and much finer than peſtles and mortars are wont to bring them to. But that, which I intend in this paragraph, is not a thing ſo obvious, ſince it is to obſerve, that ſometimes even bodies ſo fixed, as not at all to aſcend in ſublimation, may yet be reduced by that operation into powders extremely fine. For exemplifying of which, I ſhall put you in mind, that though Spagyriſts complain much of the difficulty of making a good calx of gold, and of the imperfection of the few ordinary proceſſes preſcribed to make it, (which would be more complained of, but that chemical phyſicians ſeldom attempt to prepare it,) yet we are informed by trial, that by exactly grinding a thick amalgam of gold and mercury with a competent weight, (at leaſt equal to its own) of finely powdered ſulphur, we may, by putting the mixture to ſublime in a conveniently ſhaped glaſs, by degrees of fire obtain a cinaber, that will leave behind it a finer calx of gold, than will be had by ſome far more difficult proceſſes.

BUT it is now time to draw towards a concluſion of our notes about volatility; which quality depends ſo much upon the contexture of the corpuſcles, that are to be raiſed together, that even very ponderous bodies may ſerve for volatilizing additaments, if they be diſpoſed to faſten themſelves ſufficiently to the bodies they are to carry up along with them. For, though lead be, ſave one, the heavieſt ſolid we know of, and though quickſilver be the heavieſt body in the world, except gold; yet trials have aſſured us, that quickſilver itſelf being united by amalgamation with a ſmall proportion of lead, will, by a fire, that is none of the violenteſt, and in cloſe veſſels, be made to carry over with it ſome of the lead. As we clearly found by the increaſed weight of the quickſilver, that paſſed into the receiver; which, by the way, may make us cautious, how we conclude quickſilver to be pure, merely from its having been diſtilled over.

THERE remains but one body more heavy than thoſe I come from naming, and that is gold; which, being alſo of a fixity ſo great, that it is indeed admirable, I do not wonder, that, not only the more wary naturaliſts, but the more ſevere among the chemiſts themſelves, ſhould think it incapable of being volatilized. But yet, if we conſider, how very minute parts gold may be rationally ſuppoſed to conſiſt of, and to be diviſible into, methinks it ſhould not ſeem impoſſible, that, if men could light on volatile ſalts endowed with figures fit to ſtick faſt to the corpuſcles of the gold, they would carry up with them bodies, whoſe ſolidity can ſcarce be more extraordinary, than their minuteneſs is: and in effect, we have made more than one menſtruum, with which ſome particles of gold may be carried up. But when I employed that, which I recommended to you formerly, under the name of *menſtruum peracutum*, (which conſiſts mainly, and ſometimes only of ſpirit of nitre, ſeveral times drawn from butter of antimony,) I was able, without a very violent fire, in a few hours, to elevate ſo much crude gold, as, in the neck of the retort, afforded me a conſiderable quantity of ſublimate, which I have had red as blood, and whoſe conſiſting partly of gold, manifeſtly appeared by this, that I was able, with eaſe, to reduce that metal out of it.

IN reckoning up the inſtruments of volatilization, we muſt not quite leave out the mention of the air, which I have often obſerved to facilitate the elevation of ſome bodies,

dies, even in close vessels; wherein, though to fill them too full be judged, by many, a compendious practice, because the steams have a less way to ascend, yet experience has several times informed me, that, at least in some cases, they take wrong measures, and that (to pass by another cause of their disappointment) a large proportion of air, purposely left in the vessels, may more than compensate the greater space, that is to be ascended by the vapours or exhalations of the matter, that is to be distilled or sublimed. And if, in close vessels, the presence of the air may promote the ascension of bodies, it may well be expected, that the elevation of divers of them may be furthered, by being attempted in open vessels, to which the air has free access. And if we may give any credit to the probable relations of some chemists, the air does much contribute to the volatilization of some bodies, that are barely, though indeed for no short time, exposed to it. But the account, on which the air, by its bare presence, or peculiar operations, conduces to the volatilization of some bodies, is a thing very difficult to be determined, without having recourse to some notions about gravity and levity, and of the constitution of the corpuscles, that compose the air; which I take to be both very numerous, and no less various. And therefore I must not, in these occasional notes, launch out into such a subject, though, for fear I should be blamed for too much slighting my old acquaintance the air, I durst not quite omit the power it has to dispose some bodies to volatility.

A moderate attention may suffice, to make it be discerned, that, in what hath been hitherto delivered, I have, for the most part, considered the small portions of mattter, to be elevated in volatilization, as entire corpuscles: and therefore it may be now pertinent, to intimate in a line or two, that there may be also cases, wherein a kind of volatilization, improperly so called, may be affected, by making use of such additaments, as break off, or otherwise divide the particles of the corpuscles to be elevated, and by adhering to, and so clogging one of the particles, to which it proves more congruous, enable the other, which is now brought to be more light, or disengaged, to ascend. This may be illustrated by what happens, when sal armoniac is well ground with lapis calaminaris, or with some fixed alkali, and then committed to distillation: for the sea-salt, that enters the composition of the sal armoniac, being detained by the stone or the alkali, there is a divorce made between the common salt and the urinous and fuliginous salts, that were incorporated with it, which being now disengaged from it, are easily elevated. I elsewhere mention, that I have observed, in man's urine, a kind of native sal armoniac, much less volatile than the fugitive, that is sublimed from man's blood, hartshorn, &c. and therefore supposing, that a separation of parts may be made by an alkali, as well in this salt, as in the common factitious sal armoniac, I put to fresh urine a convenient proportion (which was a plentiful one) of salt of pot-ashes, (that being then at hand,) and distilling the liquor, it yielded, according to expectation, a spirit more volatile than the phlegm, and of a very piercing taste; which way of obtaining a spirit without any violence of fire, and without either previously abstracting the phlegm, (as we are fain to do in fresh urine) or tediously waiting for the fermentation of stale urine, I taught some chemists, because of the usefulness of spirit of urine; which being obtained this innocent way, would probably be employed with much less suspicion of corrosiveness, than if in the operation I had made use of quicklime. Another illustration of what I was not long since saying, may be fetched from the experiment of making spirit of nitre, by mixing salt-petre with oil of vitriol, and distilling them together: for the oil does so divide or break the corpuscles of the nitre, that the now disposed particles of that salt, which amount to a great portion of the whole, will be made easily enough to ascend, even with a moderate fire of sand, and some-

times

times without any fire at all, in the form of ſpirits, exceeding unquiet, ſubtle, and apt to ſmoke away.

To which inſtances of this imperfect kind of volatilization more might be added, but that you may well think, I have detained you but too long already with indigeſted notes about one quality.

CHAP. VII.

THE laſt means of volatilizing bodies is, the operation of the fire or ſome other actual heat: but of this, which is obvious, it would be ſuperfluous to diſcourſe. Only this I ſhall intimate, that there may be bodies, which, in ſuch degrees of fire, as are wont to be given in the vulgar operations of chemiſts, will not be elevated, which yet may be forced up by ſuch violent and laſting fires, as are employed by the melters of ores, and founders of guns, and ſometimes by glaſs-makers. And, on this conſideration, I ſhall here obſerve to you, ſince I did not do it at my entrance on theſe notes, that chemiſts are wont to ſpeak, and I have accordingly been led to treat of volatility and fixity, in a popular ſenſe of thoſe terms. For, if we would conſider the matter more ſtrictly, I preſume we ſhould find, that volatility and fixity are but relative qualities, which are to be eſtimated, eſpecially the former of them, by the degree of fire, to which the body, whereto we aſcribe one or other of thoſe qualities, is expoſed; and therefore it is much more difficult, than men are aware of, to determine accurately, when a body ought to be accounted volatile, and when not; ſince there is no determinate degree of heat agreed on, nor indeed eaſy to be deviſed, that may be as a ſtandard, whereby to meaſure volatility and fixedneſs: and it is obvious, that a body, that remains fixed in one degree of fire, may be forced up by another. To which may be added, agreeably to what I lately began to obſerve, that a body may paſs for abſolutely fixed among the generality of chemiſts, and yet be unable to perſevere in the fires of founders and glaſs-makers: which brings into my mind, that not having obſerved, that chemiſts have examined the fixity of other bodies, than metalline ones, by the cupel, I had the curioſity to put dry ſalt of tartar upon it, and found, as I expected, that, in no long time, it manifeſtly waſted in ſo vehement a heat, wherein alſo the air came freely at it, (though quick-lime, handled after the ſame way, loſt not of its weight; and having well mixed one ounce of good ſalt of tartar with treble its weight of tobacco-pipe clay, we kept them but for two, or, at moſt, three hours, in a ſtrong fire; yet, the crucible being purpoſely left uncovered, we found the ſalt of tartar ſo waſted, that the remaining mixture (which was not fluxed) afforded us not near a quarter of an ounce of ſalt. And indeed I ſcarce doubt, but that in ſtrictneſs divers of thoſe bodies, that paſs for abſolutely fixed, are but ſemi-fixed, or, at leaſt, but comparatively and relatively fixed, that is, in reference to ſuch degrees of fire, as they are wont to be expoſed to in diſtillations, ſublimations, &c. of chemiſts; not ſuch as are given in the raging fires of founders and glaſs-makers. And perhaps, even the fires of glaſs-makers, and ſay-maſters themſelves, are not the moſt intenſe, that may poſſibly be made in a ſhort time, provided there be but ſmall portions of matter to be wrought on by them. And, in effect, I know very few bodies, beſides gold, that will perſevere totally fixed in the vehementeſt degrees of fire, that trials have made me acquainted with. And I elſewhere tell you, that, though tin, in our chemical reverberatories themſelves, is wont to be reduced but into a calx, that is reputed very fixed; yet in thoſe intenſe fires, that a virtuoſo of my acquaintance uſes in his tin-mines, there is not ſeldom found quantities of tin carried up to a notable height in the form of a whitiſh powder, which, being in good maſſes, forced off from the places to which it had faſtened itſelf, does, by a ſkilful reduction, yield many a pound weight of good malleable metal, which ſeemed to me to be rather more, than leſs, fine than ordinary tin.

POSTSCRIPT.

Relating to page 296, and here annexed for their ſakes, who have a mind to repeat the experiment there delivered, that ſo they may know the quantities employed in it.

WITH two parts of this crocus, we ground very well three parts of ſal armoniac; and having ſublimed them in a ſtrong fire, we took off the high coloured ſublimate, and put in either an equal weight, or a weight exceeding it by half, to the caput mortuum, we found, after the ſecond ſublimation, which was alſo high coloured, that, of an ounce of crocus, we had raiſed ſix drams, that is, three quarters of the whole weight.

EXPERIMENTAL NOTES

OF THE

Mechanical Origin or Production of FIXEDNESS.

CHAP. I.

FIXITY being the oppoſite quality to volatility, what we have diſcourſed about the latter, will make the nature of the former more eaſily underſtood, and upon that account allow me to make ſomewhat the quicker diſpatch of what I have to ſay of it.

THE qualifications, that conduce moſt to the fixity of a portion of matter, ſeem to be theſe.

FIRST, the groſſneſs, or the bulk of the corpuſcles it conſiſts of. For, if theſe be too big, they will be too unweildy, and unapt to be carried up into the air by the action of ſuch minute particles as thoſe of the fire, and will alſo be unfit to be buoyed up by the weight of the air; as we ſee, that vapours, whilſt they are ſuch, are ſmall enough to ſwim in the air, but can no longer be ſuſtained by it, when they convene into drops of rain, or flakes of ſnow. But here it is to be obſerved, that, when I ſpeak of the corpuſcles, that a fixed body conſiſts of, I mean not either its elementary or its hypoſtatical principles, as ſuch, but only thoſe very little maſſes or cluſters of particles, of what kind ſoever they be, that ſtick ſo firmly to one another, as not to be diviſible and diſſipable by that degree of fire, in which the body is ſaid to be fixed; ſo that each of thoſe little concretions, though it may itſelf be made up of two, three, or more particles of a ſimpler nature, is conſidered here *per modum unius*, or as one entire corpuſcle. And this is one qualification conducive to the fixedneſs of a body.

THE next is the ponderouſneſs, or ſolidity of the corpuſcles it is made up of. For if theſe be very ſolid, and (which ſolid and compact bodies uſually are) of a conſiderable ſpecifick gravity, they will be too heavy to be carried up by the effluvia or the action of the

the fire, and their ponderousness will make them as unweildy, and indisposed to be elevated by such agents, as the grossness of their bulk would make bigger corpuscles, but of a proportionably inferior specific weight. On which account, the calces of some metals and minerals, as gold, silver, &c. though, by the operation of solvents, or of the fire, or of both, reduced to powders exceedingly subtile, will resist such vehement fires, as will easily drive up bigger, but less heavy and compact corpuscles, than those calces consist of.

THE third qualification, that conduces to the fixity of a body, belongs to its integral parts, not barely as they are several parts of it, but as they are aggregated or contexed into one body. For, the qualification, I mean, is the ineptitude of the component corpuscles for avolation, by reason of their branchedness, irregular figures, crookedness, or other inconvenient shape, which entangles the particles among one another, and makes them difficult to be extracted; by which means, if one of them do ascend, others, wherewith it is complicated, must ascend with it; and, whatever be the account, on which divers particles stick firmly together, the aggregate will be too heavy or unweildy to be raised. Which I therefore take notice of, because that, though usually it is on the roughness and irregularity of corpuscles, that their cohesion depends; yet it sometimes happens, that the smoothness and flatness of their surfaces makes them so stick together, as to resist a total divulsion; as may be illustrated by what I have said of the cohesion of polished marbles, and the plates of glass, and by the fixity of glass itself in the fire.

FROM this account of the causes or requisites of fixity, may be deduced the following means of giving or adding fixation to a body, that was before either volatile, or less fixed. These means may be reduced to two general heads; first, the action of the fire, as the parts of the body, exposed to it are thereby made to operate variously on one another. And next, the association of the particles of a volatile body with those of some proper additament: which term [of proper] I rather employ than that, one would expect [of fixed;] because it will ere long appear, that, in certain cases, some volatile bodies may more conduce to the fixation of other volatile bodies, than some fixed ones do. But these two instruments of fixation being but general, I shall propose four or five more particular ones.

C H A P. II.

AND first, in some cases it may conduce to fixation, that, either by an additament, or by the operation of the fire, the parts of a body be brought to touch each other in large portions of their surfaces. For, that from such a contract, there will follow such a mutual cohesion, as will, at least, indispose the touching corpuscles to suffer a total divulsion, may appear probable from what we lately noted of the cohesion of pieces of marble and glass, and from some other phænomena belonging to the history of firmness, from which we may properly enough borrow some instances, at least, for illustration, in the doctrine of fixedness, in regard, that usually, though not always, the same things, that make a body firm, give it some degree of fixity, by keeping it from being dissipated by the wonted degrees of heat, and agitation it meets with in the air. But, to return to the contact we were speaking of, I think it not impossible, (though you may perhaps think it strange) that the bare operation of the fire may, in some cases, procure a cohesion among the particles, (and consequently make them more fixed) as well as in others disjoin them, and thereby make them more volatile. For, as in some bodies, the figures and sizes of the corpuscles may be such, that the action of the fire may rub or tear off the little beards or hooks, or other particles, that entangle them, and by that means make it more eafy for the corpufcles to be difengaged and fly upwards; fo, in other bodies, the fize and fhape of the corpufcles may be fuch, that the agitation, caufed by the fire, may rub them one againft the other, fo as by mutual attrition to grind, as it were, their furfaces, and make them fo broad and fmooth, if not alfo fo flat, as that the contact of the corpufcles fhall come to be made according to a large portion of their fuperficies, from whence will naturally follow a firm cohefion. Which I fhall illuftrate by what we may obferve among thofe, that grind glaffes for telefcopes and mifcrofcopes. For, thefe artificers, by long rubbing a piece of glafs againft a metalline difh, or concave veffel, do, by this attrition, at length bring the two bodies to touch one another in fo many parts of their congruous furfaces, that they will ftick firmly to one another, fo as fometimes to oblige the workman to ufe violence to disjoin them. And this inftance (which is not the fole I could alledge) may fuffice to fhew, how a cohefion of corpufcles may be produced by the mutual adaptation of their congruous furfaces. And if two groffer corpufcles, or a greater number of fmaller, be thus brought to ftick together, you will eafily believe, their aggregate will prove too heavy or unwieldy for avolation. And to fhew, that the fire may effect a lævigation in the furfaces of fome corpufcles, I have fometimes caufed minium, and fome other calces, that I judged convenient, to be melted for a competent time, in a vehement fire conveniently adminiftred; whereby, according to expectation, that, which was before a dull and incoherent powder, was reduced into much groffer corpufcles, multitudes of whofe grains appeared fmooth, glittering, and almoft fpecular, like thofe of fine litharge of gold; and the maffes, that thefe grains compofed, were ufually folid enough, and of difficult fufion. And when we make glafs of lead *per fe*, (which I elfewhere teach you how to do) it is plain, that the particles of the lead are reduced to a great fmoothnefs; fince, wherefoever you break the glafs, the furfaces, produced at the crack, will not be jagged, but fmooth, and confiderably fpecular. Nor do I think it impoffible, that even, when the fire does not make any great attrition of the corpufcles of the body to be fixed, it may yet occafion their fticking together, becaufe by long tumbling them up and down in various manners, it may at length, after multitudes of revolutions, and differing occurfions, bring thofe of their furfaces together, which, by reafon of their breadth, fmoothnefs, or congruity of figure, are fit for mutual cohefion; and when once they come to ftick, there is no neceffity, that the fame caufes, that were able to make them pafs by one another, when their contact was but according to an inconfiderable part of their furfaces, fhould have the fame effect now, when their contact is full; though perhaps, if the degree of fire were much encreafed, a more vehement agitation would furmount this cohefion, and diffipate again thefe clufters of coalefcent corpufcles.

THESE conjectures will, perhaps, appear lefs extravagant, if you confider what happens in the preparation of quickfilver precipitated *per fe*. For there running mercury, being put into a conveniently fhaped glafs, is expofed to a moderate fire for a confiderable time: (for I have fometimes found fix or feven weeks to be too fhort a one.) In this degree of fire the parts are varioufly tumbled, and made many of them to afcend, till convening into drops on the fides of the glafs, their weight carries them down again; but, at length, after many mutual occurfions, if not alfo attritions, fome of the parts begin to ftick together in the form of a red powder, and then more and more mercurial particles are faftened to it, till at length all, or by much the greater part of the mercury, is reduced into the like præcipitate, which, by this cohefion of the parts, being grown more fixed, will not, with the fame degree of heat, be made to rife and circulate, as the mercury would before; and yet, as I elfewhere note, I have found, by trial, that, with a greater and competent degree of heat, this præcipitate

per

per se, would, without the help of any volatilizing additament, be easily reduced into running mercury again. Chemists and physicians, who agree in supposing this præcipitate to be made without any additament, will, perchance, sca ce be able to give a more likely account of the consistency and degree of fixity, that is obtained in the mercury; in which, since no body is added to it, there appears not to be wrought any but a mechanical change. And though, I confess, I have not been without suspicions, that in philosophical strictness this præcipitate may not be made *per se*, but that some penetrating igneous particles, especially saline, may have associated themselves with the mercurial corpuscles; yet even upon this supposition it may be said, that these particles contribute to the effect, that is produced, but by facilitating or procuring, by their opportune interposition, the mutual cohesion of corpuscles, that would not otherwise stick to one another.

PERHAPS it will not be altogether impertinent to add on this occasion, that, as for the generality of chemists, as well others, as Helmontians, that assert the transmutation of all metals into gold by the philosopher's stone, methinks, they may grant it to be probable, that a new and fit contexture of the parts of a volatile body may, especially by procuring a full contact among them, very much contribute to make it highly fixed. For to omit what is related by less credible authors, it is averred, upon his own trial, by *Helmont*, who pretended not to the elixir, that a grain of the powder, that was given him, transmuted a pound (if I mis-remember not) of running mercury; where the proportion of the elixir to the mercury was so inconsiderable, that it cannot reasonably be supposed, that every corpuscle of the quicksilver, that before was volatile, was made extremely fixed, merely by its coalition with a particle of the powder, since, to make one grain suffice for this coalition, the parts it must be divided into must be scarce conceiveably minute, and therefore each single part not likely to be fixed itself, or at least more likely to be carried up by the vehemently agitated mercury, than to restrain that from avolation; whereas, if we suppose the elixir to have made such a commotion among the corpuscles of the mercury, as (having made them perhaps somewhat change their figure, and expelled some inconvenient particles,) to bring them to stick to one another, according to very great portions of their surfaces, and entangle one another, it will not be disagreeable to the mechanical doctrine of fixity, that the mercury should endure the fire as well as gold, on the score of its new texture, which, supposing the story true, appears to have been introduced, by the new colour, specifick gravity, indissolubleness in aqua-fortis, and other qualities, wherein gold differs from mercury, especially malleableness, which, according to our notes about that quality usually requires, that the parts, from whose union it results, be either hooked, branched, or otherwise adapted and fitted to make them take fast hold of one another, or stick close to one another. And since, in the whole mass of the factitious gold, all, save one grain, must be materially the same body, which, before the projection was made, was quicksilver, we may see, how great a proportion of volatile matter may, by an inconsiderable quantity of fixing additament, acquire such a new disposition of its parts, as to become most fixed. And however, this instance will agree much better with the mechanical doctrine about fixity, than with that vulgar opinion of the chemists, (wherewith it will not at all comply,) that if, in a mixture, the volatile part do much exceed the fixed, it will carry up that, or at least, a good portion thereof, with it; and on the contrary. But though this rule holds in many cases, where there is no peculiar indisposition to the effect, that is aimed at; yet if the mechanical affections of the bodies be ill suited to such a purpose, our philosophical experiment manifestly proves, that the rule will not hold, since so great a multitude of grains of mercury, instead of carrying

carrying up with them one grain of the elixir, are detained by it in the ſtrongeſt fire! And thus much for the firſt way of fixing volatile bodies.

CHAP. III.

THE ſecond way of producing fixity is by expelling, breaking, or otherwiſe diſabling thoſe volatile corpuſcles, that are too indiſpoſed to be fixed themſelves, or are fitted to carry up with them ſuch particles, as would not, without their help, aſcend. That the expulſion of ſuch parts is a proper means to make the aggregate of thoſe, that remain more fixed, I preſume you will not put me ſolicitouſly to prove; and we have a manifeſt inſtance of it in ſoot, where, though many active parts were by the violence of the fire and current of the air carried up together by the more volatile parts; yet, when ſoot is well diſtilled in a retort, a competent time being given for the extricating and avolation of the other parts, there will at the bottom remain a ſubſtance, that will not now fly away, as it formerly did. And here let me obſerve, that the receſs of the fugitive corpuſcles may contribute to the fixation of a body, not barely becauſe the remaining matter is freed from ſo many unfixed, if not alſo volatilizing parts; but, as it may often happen, that upon their receſs the pores or intervals, they left behind them, are filled up with more ſolid or heavy matter, and the body becomes, as more homogeneous, ſo more cloſe and compact. And whereas I intimated, that, beſides the expulſion of unfit corpucles, they may be otherwiſe diſabled from hindering the fixation of the maſs they belong to, I did it, becauſe it ſeems very poſſible, that in ſome caſes they may, by the action of the fire, be ſo broken, as with their fragments to fill up the pores or intervals of the body they appertained to; or may make ſuch coalitions with the particles of a convenient additament, as to be no impediment to fixity of the whole maſs, though they remain in it. Which poſſibly you will think may well happen, when you ſhall have peruſed the inſtances annexed to the fourth way of fixing bodies.

THE third means of fixing, or leſſening the volatility of bodies, is by preſerving that reſt among the parts, whoſe contrary is neceſſary to their volatilization. And this may be done by preventing or checking that heat, or other motion, which external agents ſtrive to introduce into the parts of the propoſed body. But this means tending rather to hinder the actual avolation of a portion of matter, or, at moſt, procure a temporary abatement of its volatility, than to give it a ſtable fixity, I ſhall not any longer inſiſt on it.

THE fourth way of producing fixity in a body, is by putting to it ſuch an appropriated additament, whether fixed or volatile, that the corpuſcles of the body may be put among themſelves, or with thoſe of the additament, into a complicated ſtate, or entangled contexture. This being the uſual and principal way of producing fixity, we ſhall dwell ſomewhat the longer upon it, and give inſtances of ſeveral degrees of fixation. For, though they do not produce that quality in the ſtricteſt acceptation of the word, fixity; yet it is uſeful in our preſent enquiry, to take notice, by what means that volatility comes to be gradually abated, ſince that may facilitate our underſtanding, how the volatility of a body comes to be totally abated, and conſequently the body to be fixed.

CHAP. IV.

AND firſt, we find, that a fixed additament, if its parts be conveniently ſhaped, may eaſily give a degree of fixity to a very volatile body. Thus ſpirit of nitre, that will of itſelf eaſily enough fly away in the air, having its ſaline particles aſſociated with thoſe of fixed nitre, or ſalt of tartar, will with the alcali compoſe a ſalt of a nitrous nature, which will endure to be melted in a crucible, without being deprived even of its ſpirits. And I have found, that the ſpirits of nitre, that abound in aqua-fortis, being concoagulated, with the ſilver they corrode, though one would not expect, that ſuch ſubtile corpuſcles ſhould ſtick faſt to ſo compact and ſolid a body as ſilver; yet cryſtals produced by their coalition, being put into a retort, may be kept a pretty while in fuſion, before the metal will let go the nitrous ſpirits. When we poured oil of vitriol upon the calx of vitriol, though many phlegmatick and other ſulphureous particles were driven away by the excited heat; yet the ſaline parts, that combined with the fixed ones of the colcothar, ſtuck faſt enough to them, not to be eaſily driven away. And if oil of vitriol be in a due proportion dropped upon ſalt of tartar, there reſults a *tartarum vitriolatum,* wherein the acid and alkalizate parts cohere ſo ſtrongly, that it is not an ordinary degree of fire will be able to disjoin them. Inſomuch, that divers chemiſts have (though very erroneouſly) thought this compounded ſalt to be indeſtructible. But a leſs heavy liquor than the ponderous oil of vitriol may, by an alkali, be more ſtrongly detained, than that oil itſelf; experience having aſſured me, that ſpirit of ſalt being dropped to ſatiety upon a fixed alkali, (I uſed either that of nitre or of tartar,) there would be made ſo ſtrict an union, that, having, without additaments, diſtilled the reſulting ſalt with a ſtrong and laſting fire, it appeared not at all conſiderably to be wrought upon, and was not ſo much as melted.

BUT it is not the bare mixture or commiſtion of volatile particles with fixed ones, (yea though the former be predominant in quantity,) that will ſuffice to elevate the latter. For, unleſs the figures of the latter be congruous and fitted to faſten to the other, the volatile parts will fly away in the heat, and leave the reſt as fixed as before: as when ſand or aſhes are wetted or drenched with water, they quickly part with that water, without parting with any degree of their fixity. But on the other ſide, it is not always neceſſary, that the body, which is fitted to deſtroy, or much abate the volatility of another ſubſtance, ſhould be itſelf fixed. For, if there be a ſkilful or lucky coaptation of the figures of the particles of both the bodies, theſe particles may take ſuch hold of one another, as to compoſe corpuſcles, that will neither by reaſon of their ſtrict union be divided by heat; nor by reaſon of their reſulting groſſneſs be elevated even by a ſtrong fire, or at leaſt by ſuch a degree of heat, as would have ſufficed to raiſe more indiſpoſed bodies than either of the ſeparate ingredients of the mixture. This obſervation, if duly made out, does ſo much favour our doctrine about the mechanical origin of fixation, and may be of ſuch uſe, not only to chemiſts, in ſome of their operations, but to philoſophers, in aſſigning the cauſes of divers phænomena of nature, that it may be worth while to exemplify it by ſome inſtances.

THE firſt whereof I ſhall take from an uſual practice of the chemiſts themſelves: which I the rather do, to let you ſee, that ſuch known experiments are too often overlooked by them, that make them, but yet may hint or confirm theories to thoſe, that reflect on them. The inſtance, I here ſpeak of, is that, which is afforded by the vulgar preparation of bezoardicum minerale. For, though the rectified butter, or oil of antimony, and the ſpirit of nitre, that are put together to make this white præcipitate, are both of them diſtilled liquors; yet the copious powder, that reſults from their union, is, by that union of volatile parts, ſo far fixed, that, after they have edulcorated it with water, they preſcribe the calcining of it in a crucible for five or ſix hours: which operation it could not bear, unleſs it had attained to a conſiderable fixation. This diſcourſe ſuppoſes with the generality of chemiſts, that the addition of a due quantity of ſpirit of nitre is neceſſary to be employed in making the bezoardicum

cum

cum minerale. But if it be a true obfervation, which is attributed to the learned *Guntherus Billichius*, (but which I had no furnace at hand to examine, when I heard of it,) if, I fay, it be true, that a bezoardicum minerale may be obtained, without fpirit of nitre, barely by a flow evaporation, made in a glafs difh, of the more fugitive parts of the oil of antimony; this inftance will not indeed be proper in this place, but yet will belong to the fecond of the foregoing ways of introducing fixity. I proceed now to alledge other particulars, in favour of the above-mentioned obfervation.

If you take ftrong fpirit of falt, that, when the glafs is unftopped, will fmoke of itfelf in the cold air, and fatiate it with the volatile fpirit of urine, the fuperfluous moifture being abftracted, you will obtain by this preparation (which, you may remember, I long fince communicated to you, and divers other virtuofi,) a compounded falt, fcarce, if at all, diftinguifhable from fal armoniac, and which will not, as the falts it confifts of will do, before their coalition, eafily fly up of itfelf into the air, but will require a not defpicable degree of fire to fublime it.

Of thefe femivolatile compofitions of falt I have made, and elfewhere mentioned, others, which I fhall not here repeat, but pafs on to other inftances, pertinent to our prefent defign.

I lately mentioned, that the volatility of the fpirits of nitre may be very much abated, by bringing them to coagulate into cryftals, with particles of corroded filver; but I fhall now add, that I gueffed, and by trial found, that thefe nitrous fpirits may be made much more fixed by the addition of the fpirit of falt, which, if it be good, will of itfelf fmoke in the air. For, having diffolved a convenient quantity of cryftals of filver in diftilled water, and precipitated them, not with a folution of falt, but the fpirit of falt; the phlegm being abftracted, and fome few of the loofer faline particles; though the remaining mafs were preffed with a violent fire, that kept the retort red hot for a good while; yet the nitrous and faline fpirits would by no means be driven away from the filver, but continued in fufion with it; and when the mafs was taken out, thefe fpirits did fo abound in it, that it had no appearance of a metal, but looked rather like a thick piece of horn.

The next inftance I fhall name is afforded us by that kind of turbith, which may be made by oil of vitriol, inftead of the aqua-fortis employed in the common turpethum minerale. For, though oil of vitriol be a diftilled liquor, and mercury a body volatile enough; yet, when we abftracted four or five parts of oil of vitriol from one of quickfilver, (efpecially if the operation were repeated,) and then wafhed off as much as we could of the faline particles of the oil of vitriol; yet thofe, that, remained adhering to the mercury made it far more fixed, than either of the liquors had been before, and enabled it even in a crucible, to endure fuch a degree of fire, before it could be driven away, as, I confefs, I fomewhat wondered at. The like turbith may be made with oil of fulphur *per campanam*. But this is nothing to what *Helmont* tells us of the operation of his alkaheft, where he affirms, that that menftruum, which is volatile enough, being abftracted from running mercury, not only coagulates it, but leaves it fixed, fo that it will endure the brunt of fires actuated by bellows, *(omnem follium ignem.)* If this be certain, it will not be a flender proof, that fixity may be mechanically produced; and however, the argument will be good in reference to the Helmontian Spagyrifts. For if, as one would expect, there do remain fome particles of the menftruum with thofe of the metal, it will not be denied, that two volatile fubftances may perfectly fix one another. And if, as *Helmont* feems to think, the menftruum be totally abftracted, this fuppofition will the more favour our doctrine about fixity; fince, if there be no material additament left with the quickfilver, the fixation cannot fo reafonably

ably be afcribed to any thing, as to fome new mechanical modification, and particularly to fome change of texture introduced into the mercury itfelf.

And that you may think this the lefs improbable, I will now proceed to fome inftances, whereof the firft fhall be this; that, having put a mixture made of a certain proportion of two dry, as well as volatile bodies, (viz. fal armoniac, and flower or very fine powder of fulphur,) to half its weight of common running mercury, and elevated this mixture three or four times from it, (in a conveniently fhaped, and not over-wide glafs) the mercury, that lay in the bottom, in the form of a ponderous and fomewhat purplifh powder, was, by this operation, fo fixed, that it long endured a ftrong fire, which at length was made fo ftrong, that it melted the glafs, and kept it melted, without being ftrong enough to force up the mercury: which, by fome trials, not fo proper to be here mentioned, feemed to have its falivating and emetick powers extraordinarily infringed, and fometimes quite fuppreffed. But this only upon the bye. In all the other inftances, (wherewith I fhall conclude thefe notes,) I fhall employ one menftruum, oil of vitriol, and fhew you the efficacy of it in fixing fome parts of volatile bodies with fome parts of itfelf; by which examples it may appear, that a volatile body may not only leffen the volatility of another body, as in the lately mentioned cafe of our fpirituous fal armoniac; but that two fubftances, that apart were volatile, may compofe a third, that will not only be lefs volatile, but confiderably, if not altogether, fixed.

We mixed then, by degrees, about equal parts of oil of vitriol and oil of turpentine: and though each of them fingle, efpecially the latter, will afcend with a moderate fire in a fand-furnace; yet, after the diftillation was ended, we had a confiderable quantity, fometimes, if I mif-remember not, a fifth or fixth part, of a caput mortuum, black as a coal, and whereof a great part was of a fcarce to be expected fixednefs in the fire.

To give a higher proof of the difpofition, that oil of vitriol has to let fome of its parts grow fixed by combination with thofe of an exceeding volatile additament, I mixed this liquor with an equal or double weight of highly rectified fpirit of wine, and not only after, but fometimes without, previous digeftion, I found, that the fluid parts of the mixture being totally abftracted, there would remain a pretty quantity of a black fubftance fo fixed, as to afford juft caufe of wonder.

And becaufe camphire is efteemed the moft fugitive of confiftent bodies, in regard that, being but laid in the free air, without any help of the fire, it will fly all away; I tried, what oil of vitriol, abftracted from camphire, would do; and found at the bottom of the retort a greater quantity, than one would expect, of a fubftance as black as pitch, and almoft as far from the volatility, as from the colour of camphire, though it appeared not, that any of the gum had fublimed into the neck of the retort.

From all which inftances it feems manifeftly enough to follow, that in many cafes there needs nothing to make affociated particles, whether volatile or not, become fixed, but either to implicate or entangle them among themfelves, or bring them to touch one another; according to large portions of their furfaces, or by both thefe ways conjointly, or by fome others, to procure the firm cohæfion of fo many particles, that the refulting corpufcles be too big or heavy to be, by the degree of fire, wherein they are faid to be fixed, driven up into the air.

EXPERIMENTS and NOTES ABOUT THE MECHANICAL ORIGIN OR PRODUCTION OF CORROSIVENESS and CORROSIBILITY.

SECTION I.

About the MECHANICAL ORIGIN *of* CORROSIVENESS.

I DO not, in the following notes, treat of corrosiveneſs in their ſtrict ſenſe of the word, who aſcribe this quality only to liquors, that are notably acid or ſour, ſuch as aqua-fortis, ſpirit of ſalt, vinegar, juice of lemons, &c. but, that I may not be obliged to overlook urinous, oleous, and divers other ſolvents, or to coin new names for their differing ſolutive powers, I preſume to employ corroſiveneſs in a greater latitude, ſo as to make it almoſt equivalent to the ſolutive power of liquors, referring other menſtruums to thoſe, that are corroſive or fretting, (though not always as to the moſt proper, yet) as to the principal and beſt known ſpecies; which I the leſs ſcruple here to do, becauſe I have * elſewhere more diſtinctly enumerated and ſorted the ſolvents of bodies.

THE attributes, that ſeem the moſt proper to qualify a liquor to be corroſive, are all of them mechanical, being ſuch as are theſe, that follow:

FIRST, that the menſtruum conſiſt of, or abound with corpuſcles not too big to get in at the pores or commiſſures of the body to be diſſolved; nor yet be ſo very minute, as to paſs through them, as the beams of light do through glaſs; or to be unable, by reaſon of their great ſlenderneſs and flexibility, to disjoin the parts they invade.

SECONDLY, that theſe corpuſcles be of a ſhape fitting them to inſinuate themſelves, more or leſs, into the pores or commiſſures above-mentioned, in order to the diſſociating of the ſolid parts.

THIRDLY, that they have a competent degree of ſolidity to disjoin the particles of the body to be diſſolved; which ſolidity of ſolvent corpuſcles is ſomewhat diſtinct from their bulk, mentioned in the firſt qualification; as may appear, by comparing a ſtalk of wheat and a metalline wire of the ſame diameter, or a flexible wand of oſier, of the bigneſs of one's little finger, with a rigid rod of iron of the ſame length and thickneſs.

FOURTHLY, that the corpuſcles of the menſtruum be agile and advantaged for motion, (ſuch as is fit to disjoin the parts of the invaded body) either by their ſhape, or their minuteneſs, or their fitneſs to have their action befriended by adjuvant cauſes; ſuch as may be (firſt) the preſſure of the atmoſphere, which may impel them into the pores of bodies not filled with a ſubſtance ſo reſiſting as common air: as we ſee, that water will, by the prevalent preſſure of the ambient, whether air or water, be raiſed to the height of ſome inches in capillary glaſſes, and in the pores of ſpunges, whoſe conſiſtent parts, being of eaſier ceſſion than the ſides of glaſs-pipes, thoſe pores will be enlarged, and conſequently thoſe ſides disjoined, as appears by the dilatation and ſwelling

* This refers to an eſſay of the author's about the uſefulneſs of chemiſtry to, &c.

ſwelling of the ſpunge: and (ſecondly) the agitation, that the intruding corpuſcles may be fitted to receive in thoſe pores or commiſſures, by the tranſcurſion of ſome ſubtile aetherial matter; or by the numerous knocks and other pulſes of the ſwimming or tumbling corpuſcles of the menſtruum itſelf, (which, being a fluid body, muſt have its ſmall parts perpetually and variouſly moved) whereby the engaged corpuſcles, like ſo many little wedges and leavers, may be enabled to wrench open, or force aſunder the little parts between which they have inſinuated themſelves. But I ſhall not here proſecute this theory, (which, to be handled fully, would require a diſcourſe apart) ſince theſe conjectures are propoſed but to make it probable in the general, that the corroſiveneſs of bodies may be deduced from mechanical principles: but whether beſt from the newly propoſed ones, or any other, need not be anxiouſly conſidered in theſe notes, where the things mainly intended and relied on are the experiments and phænomena themſelves.

EXPERIMENT I.

It is obvious, that, though the recently expreſſed juice of grapes be ſweet, whilſt it retains the texture, that belongs to it, as it is new, (eſpecially, if it be made of ſome ſorts of grapes, that grow in hot regions;) yet, after fermentation, it will, in tract of time, as it were ſpontaneouſly, degenerate into vinegar. In which liquor, to a multitude of the more ſolid corpuſcles of the muſt, their frequent and mutual attritions may be ſuppoſed to have given edges like thoſe of the blades of ſwords or knives; and in which, perhaps, the confuſed agitation, that preceded, extricated, or, as it were, unſheathed ſome acid particles, that (derived from the ſap of the vine, or, perchance, more originally from the juice of the earth,) were at firſt in the muſt, but lay concealed, and, as it were, ſheathed among the other particles, wherewith they were aſſociated, when they were preſſed out of the grapes. Now this liquor, that by the fore-mentioned, or other like mechanical changes, is become vinegar, does ſo abound with corpuſcles, which, on the account of their edges, or their otherwiſe ſharp and penetrative ſhape, are acid and corroſive, that the better ſort of it will, without any preparation, diſſolve coral, crab's-eyes, and even ſome ſtones, lapis ſtellaris in particular, as alſo minium, or the calx of lead, and even crude copper, as we have often tried. And not only the diſtilled ſpirit of it will do thoſe things more powerfully, and perform ſome other things, that mere vinegar cannot; but the ſaline particles, wont to remain after diſtillation, may, by being diſtilled and cohobated *per ſe*, or by being ſkilfully united with the foregoing ſpirit, be brought to a menſtruum of no ſmall efficacy in the diſſolution, and other preparations of metalline bodies, too compact for the mere ſpirit itſelf to work upon.

From divers other ſweet things alſo may vinegar be made; and even of honey, ſkilfully fermented with a ſmall proportion of common water, may be made a vinegar ſtronger than many of the common wine-vinegars; as has been affirmed to me by a very candid phyſician, who had occaſion to deal much in liquors.

EXPERIMENT II.

Not only ſeveral dry woods, and other bodies, that moſt of them paſs for inſipid, but honey and ſugar themſelves afford by diſtillation acid ſpirits, that will diſſolve coral, pearls, &c. and will alſo corrode ſome metals and metalline bodies themſelves; as I have often found by trial. So that the violent operation of the fire, that deſtroys what they call the form of the diſtilled body, and works, as a mechanical agent, by agitating, breaking, diſſipating, and under a new conſtitution re-aſſembling the parts, procures for the diſtiller an acid corroſive menſtruum; which, whether it be brought to paſs by making the corpuſcles rub one another into the figure of little ſharp blades, or by ſplitting ſome ſolid parts into ſharp or cutting corpuſcles, or by unſheathing, as it were, ſome parts, that, during the former texture of the body, did not appear to be acid; or whether it be rather effected by ſome other mechanical way, may in due time be further conſidered.

EXPERIMENT III.

It is obſerved by refiners, goldſmiths and chemiſts, that aqua-fortis and aqua-regia, which are corroſive menſtruums, diſſolve metals, the former of them ſilver, and the latter gold, much more ſpeedily and copiouſly, when an external heat gives their inteſtine motions a new degree of vehemency or velocity, which is but a mechanical thing; and yet this ſuperadded meaſure of agitation is not only in the above-mentioned inſtances a powerfully aſſiſtant cauſe in the ſolutions made by the lately mentioned corroſive liquors, but is that, without which ſome menſtruums are not wont ſenſibly to corrode ſome bodies at all, as we have tried in keeping quickſilver in three or four times its weight of oil of vitriol; ſince in this menſtruum I found not the mercury to be diſſolved, or corroded, though I kept it a long time in the cold: whereas, when the oil of vitriol was excited by a convenient heat, (which was not faint) it corroded the mercury into a fine white calx or powder, which by the affuſion of fair water, would be preſently turned into a yellowiſh calx of the colour and nature of a turbith. I remember alſo, that having, for trial's ſake, diſſolved in a weak ſpirit of ſalt a fourth part of weight of fine cryſtals of nitre, we found, that it would not in the cold (at leaſt during a good while, that we waited for its operation) diſſolve leaf-gold; but when the menſtruum was a little heated at the fire, the ſolution proceeded readily enough. And in ſome caſes, though the external heat be but ſmall, yet there may intervene a briſk heat, and much co-operate in the diſſolution of a body; as for inſtance, of quickſilver in aqua-fortis. For it is no prodigy to find, that when a full proportion of that fluid metal has been taken, the ſolution, though at firſt altogether liquid, and as to ſenſe uniform, comes to have, after a while, a good quantity of coagulated or cryſtallized matter at the bottom, of which the cauſe may be, that in the very act of corroſion there is excited an intenſe degree of heat, which conferring a new degree of agitation to the menſtruum, makes it diſſolve a good deal more, than afterwards, when the conflict is over, it is able to keep up.

EXPERIMENT IV.

We have obſerved alſo, that agitation does in ſome caſes ſo much promote the diſſolutive power of ſaline bodies, that though they be not reduced to that ſubtilty of parts, to which a ſtrong diſtillation brings them; yet they may in their groſſer and cruder form have the power to work on metals; as I elſewhere ſhew, that by barely boiling ſome ſolutions of ſalts of a convenient ſtructure, as nitre, ſal armoniac, &c. with foliated gold, ſilver, &c. we have corroded theſe metals, and can diſſolve ſome others. And by boiling crude copper (in filings) with ſublimate and common water, we were able, in no long time, to make a ſolution of the metal.

EXPERIMENT V.

Sometimes alſo, ſo languid an agitation, as that, which ſeems but ſufficient to keep a liquor in the ſtate of fluidity, may ſuffice to give ſome dry bodies a corroding power, which they could not otherwiſe exerciſe; as in the way of writing one's name (or a motto) upon the blade of a knife with common ſublimate: for, if having very thinly overlaid which ſide you pleaſe with bees-wax, you write with a bodkin, or ſome pointed

pointed thing upon it; the wax being thereby removed from the ſtrokes made by the ſharp body, it is eaſy to etch with ſublimate; ſince you need but ſtrew the powder of it upon the place bared of the wax, and wet it well with mere common water; for ſtrong vinegar is not neceſſary. For, after a while, all the parts of the blade, that ſhould not be fretted, being protected by the caſe or film of wax, the ſublimate will corrode only where way has been made for it by the bodkin, and the letters will be more or leſs deeply engraven (or rather etched) according to the time the ſublimate is ſuffered to lie on. And if you aim only at a legible impreſſion, a few minutes of an hour (as four or five) may ſerve the turn.

EXPERIMENT VI.

THIS brings into my mind an obſervation I have ſometimes had occaſion to make, that I found more uſeful than common; and it is, that divers bodies, whether diſtilled or not diſtilled, that are not thought capable of diſſolving other bodies, becauſe in moderate degrees of heat they will not work on them, may yet, by intenſe degrees of heat, be brought to be fit ſolvents for them. To which purpoſe I remember, that having a diſtilled liquor, which was rather ſweet to the taſte, than either acid, lixiviate, or urinous, though for that reaſon it ſeemed unfit to work on pearls, and accordingly did not diſſolve them in a conſiderable time, wherein they were kept with it in a more than ordinarily warm digeſtion; yet the glaſs being for many hours (amounting perhaps to ſome days) kept in ſuch an heat of ſand as made the liquor boil, we had a diſſolution of pearls, that uniting with the menſtruum, made it a very valuable liquor. And though the ſolvents of crude gold, wont to be employed by chemiſts, are generally diſtilled liquors, that are acid, and in the lately mentioned ſolvent, made of crude ſalts and common water, acidity ſeemed to be the predominant quality (which makes the uſe of ſolutions made in aqua-regia, &c. ſuſpected by many phyſicians and chemiſts;) yet fitly choſen alcalizate bodies themſelves, as repugnant as they uſe to be to acids, without the help of any liquor, will be enabled, by a melting fire, in no long time, to penetrate and tear aſunder the parts even of crude gold; ſo that it may afterwards be eaſily taken up in liquors, that are not acid, or even by water itſelf.

EXPERIMENT VII.

THE tract about ſalt-petre, that gave occaſion to theſe annotations, may furniſh us with an eminent inſtance of the production of ſolvents. For, though pure ſalt-petre itſelf, when diſſolved in water, is not obſerved to be a menſtruum for the ſolution of the metals hereafter to be named, or ſo much as of coral itſelf; yet when, by a convenient diſtillation, its parts are ſplit, if I may ſo ſpeak, and by attrition, or other mechanical ways of working on them, reduced to the ſhapes of acid and alcalizate ſalts, it then affords two ſorts of menſtruums, of very differing natures, which, betwixt them, diſſolve or corrode a great number and variety of bodies; as the ſpirit of nitre, without addition, is a ſolvent for moſt metals, as ſilver, mercury, copper, lead, &c. and alſo divers mineral bodies, as tin-glaſs, ſpelter, lapis calaminaris, &c. and the fixed ſalt of nitre operates upon ſulphureous minerals, as common ſulphur, antimony, and divers other bodies, of which I elſewhere make mention.

EXPERIMENT VIII.

BY the former trials it has appeared, that the encreaſe of motion, in the more penetrating corpuſcles of a liquor, contributes much to its ſolutive power; and I ſhall now add, that the ſhape and ſize, which are mechanical affections, and ſometimes alſo the ſolidity of the ſame corpuſcles does eminently concur to qualify a liquor to diſſolve this

or

or that particular body. Of this, even ſome of the more familiar practices of chemiſts may ſupply us with inſtances. For there is no account ſo probable as may be given upon this ſuppoſition, why aqua-fortis, which will diſſolve ſilver, without meddling with gold, ſhould, by the addition of a fourth part of its weight of ſal armoniac, be turned into aqua-regia, which, without meddling with ſilver, will diſſolve gold. But there is no neceſſity of having recourſe to ſo groſs and compounded a body as ſal-armoniac, to enable aqua-fortis to diſſolve gold: for, the ſpirit of common ſalt alone, being mingled in a due proportion, will ſuffice for that purpoſe. Which (by the way) ſhews, that the volatile ſalt of urine and ſoot, that concur to the making up of ſal armoniac, are not neceſſary to the diſſolution of gold, for which a ſolvent may be made with aqua-fortis and crude ſea-ſalt. I might add, that the mechanical affections of a menſtruum, may have ſuch an intereſt in its diſſolutive power, that even mineral or metalline corpuſcles may become uſeful ingredients of it, though, perhaps, it be a diſtilled liquor; as might be illuſtrated by the operations of ſome compounded ſolvents, ſuch as is the oil of antimony made by repeated rectifications of what chemiſts call its butter, which, whatever ſome ſay to the contrary, does much abound in antimonial ſubſtance.

EXPERIMENT IX.

BUT I ſhall return to our aqua-regia, becauſe the mention I had occaſion to make of that ſolvent, brought into my mind what I deviſed, to make it probable, that a ſmaller change, than one would lightly imagine, of the bulk, ſhape, or ſolidity of the corpuſcles of a menſtruum, may make it fit to diſſolve a body it would not work on before. And this I the rather attempted, becauſe the warier ſort of chemiſts themſelves are very ſhy of the inward uſe or preparations made of gold by the help of aqua-fortis, becauſe of the odious ſtink they find, and the venenoſity they ſuſpect in that corroſive menſtruum: whereas ſpirit of ſalt we look upon as a much more innocent liquor, whereof, if it be but diluted with fair water, or any ordinary drink, a good doſe may be ſafely given inwardly, though it have not wrought upon gold, or any other body, to take off its acrimony. But, whether or no this prove of any great uſe in phyſick, wherein, perhaps, if any quantity of gold be to be diſſolved, a greater proportion of ſpirit of nitre would be needed; the ſucceſs will not be unfit to be mentioned, in reference to what we were ſaying of ſolvents. For, whereas we find not, that our ſpirit of ſalt here, in *England*, will at all diſſolve crude gold, we found, that by putting ſome leaf-gold into a convenient quantity of good ſpirit of ſalt, when we had dropped in ſpirit of nitre, (ſhaking the glaſs at each drop) till we perceived, that the mixture was juſt able, in a moderate heat, to diſſolve the gold, we found, that we had been obliged to employ but after the rate of twelve drops of the latter liquor to an ounce of the former; ſo that, ſuppoſing each of theſe drops to weigh a grain, the fortieth part of ſpirit of nitre being added, ſerved to turn the ſpirit of ſalt into a kind of aqua-regia. But to know the proportion otherwiſe than by gueſs, we weighed ſix other drops of the ſame ſpirit of ſalt, and found them to amount not fully to three grains and an half: whence it appeared, that we added but about a ſeventieth part of the nitrous ſpirit to that of ſalt.

THE experiments, that have been hitherto recited, relate chiefly to the production of corroſive menſtruums; and therefore I ſhall now add an account of a couple of trials, that I made manifeſtly to leſſen, or quite to deſtroy corroſiveneſs in liquors very conſpicuous for that quality.

EXPERI-

EXPERIMENT X.

WHEREAS one of the moft corrofive menftruums, that is yet known, is oil of vitriol, which will fret in pieces both divers metals and minerals, and a great number and variety of animal and vegetable bodies; yet if you digeft with it, for a while only, an equal weight of highly-rectified fpirit of wine, and afterwards diftil the mixture very warily, (for elfe the experiment may very eafily mifcarry,) you may obtain a pretty deal of liquor not corrofive at all, and the remaining fubftance will be reduced partly into a liquor, which, though acid, is not more fo than one part of good oil of vitriol will make ten times as much common water, by being well mingled with it; and partly into a dry fubftance, that has fcarce any tafte at all, much lefs a corrofive one.

EXPERIMENT XI.

AND though good aqua-fortis be the moft generally employed of corrofive menftruums, as being capable of diffolving or corroding, not only many minerals, as tinglafs, antimony, zink, &c. but all metals, except gold, (for, though it make not a permanent folution of crude tin, it quickly frets the parts afunder, and reduces it to an immalleable fubftance;) yet, to fhew how much the power of corroding may be taken away, by changing the mechanical texture of a menftruum, even without feeming to deftroy the fretting falts, I practifed (and communicated to divers virtuofi) the following experiment, elfewhere mentioned to other purpofes.

WE took equal parts of good aqua-fortis, and highly dephlegmed fpirit of wine, and having mingled them warily, and by degrees, (without which caution the operation may prove dangerous) we united them by two or three diftillations of the whole mixture; which afterwards we found not to have the leaft fretting tafte, and to be fo deprived of its corrofive nature, that it would not work upon filver, though by precipitation, or otherwife, reduced to very fmall parts; nay, it would fcarce fenfibly work, in a good while, on filings of copper, or upon other bodies, which mere vinegar, or, perhaps, rhenifh wine, will corrode. Nay, I remember, that, with another fpirit, (that was not urinous) and afterwards, with alkohol of wine, we fhewed a more furprifing fpecimen of the power of either deftroying, or debilitating, the corrofivenefs of a menftruum, and checking its operation. For, having caufed a piece of copper plate to be put into one ounce of aqua-fortis, when this liquor was eagerly working upon the metal, I caufed an ounce of the alkohol of wine, or the other fpirit to be poured, (which it fhould warily be) upon the agitated mixture; whofe effervefcence, at the firft inftant, feemed to be much encreafed, but prefently after was checked, and the corrofivenefs of the menftruum being fpeedily difabled or corrected, the remaining copper was left undiffolved at the bottom.

NOR are thefe the only acid menftruums, that I have, many years fince, been able to correct by fuch a way: for I applied it to others, as fpirit of nitre, and even aquaregis itfelf; but it has not an equal operation upon all, and leaft of all (as far as I can remember) upon fpirit of falt; as, on the other fide, ftrong fpirit of nitre was the menftruum, upon which its effects were the moft fatisfactory.

MOST of the chemifts pretend, that the folutions of bodies are performed by a certain cognation and fympathy between the menftruum and the body it is to work upon. And it is not to be denied, that, in divers inftances, there is, as it were, a confanguinity between the menftruum and the body to be diffolved; as when fulphur is diffolved by oils, whether expreffed or diftilled: but yet, as the opinion is generally propofed, I cannot acquiefce in it, partly, becaufe there are divers folutions and other phænomena, where it will not take place; and partly, becauſe, even in thoſe inſtances, wherein it is thought moſt applicable, the effect ſeems to depend upon mechanical principles.

EXPERIMENT XII.

AND firſt, it will be difficult to ſhew, what conſanguinity there is between ſal gem, and antimony, and iron, and zink, and bread, and camphire, and lapis calaminaris, and fleſh of divers kinds, and oiſter-ſhells, and hartſhorn, and chalk, and quick-lime; ſome of which belong to the vegetable, ſome to the mineral, and ſome to the animal kingdom; and yet all of them, and divers others, as I have tried, may, even without the aſſiſtance of external heat, be diſſolved or corroded by one ſingle mineral menſtruum, oil of vitriol. And, which is not to be neglected on this occaſion, ſome of them may be bodies, ſuppoſed, by chemiſts, to have an antipathy to each other, in point of corroſion, or diſſolution.

EXPERIMENT XIII.

I obſerve alſo, that a diſſolution may be made of the ſame body by menſtruums, to which the chemiſts attribute (as I juſt now obſerved they did to ſome bodies) a mutual antipathy, and which therefore are not like to have a ſympathy with the ſame third body; as I found by trial, that both aqua-fortis, and ſpirit of urine, upon whoſe mixture there enſues a conflict, with a great effervеſcence, will, each of them apart, readily diſſolve crude zink, and ſo each of them will the filings of copper. Not to mention, that pure ſpirit of wine, and oil of vitriol, as great a difference as there is between them, in I know not how many reſpects, and as notable a heat as will enſue upon their commixture, will each of them diſſolve camphire; to which may be added, other inſtances of the like nature. As for what is commonly ſaid, that oils diſſolve ſulphur, and ſaline menſtruums metals, becauſe (as they ſpeak) *ſimile ſimili gaudet*; I anſwer, that, where there is any ſuch ſimilitude, it may be very probably aſcribed, not ſo much with the chemiſts, that favour *Ariſtotle*, to the eſſential forms of the bodies, that are to work on each other; nor, with the mere chemiſts, to their ſalt, or ſulphur, or mercury, as ſuch; but to the congruity between the pores and figures of the menſtruum, and the body diſſolved by it, and to ſome other mechanical affections of them.

EXPERIMENT XIV.

FOR ſilver, for example, not only will be diſſolved by nitre, which they reckon a ſalt, but be amalgamed with, and conſequently diſſolved by quickſilver, and alſo by the operation of brimſtone, be eaſily incorporated with that mineral, which chemiſts are wont to account of ſo oleaginous a nature, and inſoluble in aqua-fortis.

EXPERIMENT XV.

AND as for thoſe diſſolutions, that are made with oily and inflammable menſtruums, of common ſulphur and other inflammable bodies, the diſſolution does not make for them ſo clearly as they imagine. For, if ſuch menſtruums operate, as is alledged, upon the account of their being, as well as the bodies they work upon, of a ſulphureous nature, whence is it, that highly rectified ſpirit of wine, which, according to them, muſt be of a moſt ſulphureous nature, ſince, being ſet on fire, it will flame all away, without leaving one drop behind it, will not (unleſs, perhaps, after a tedious while) diſſolve even flowers of brimſtone, which eſſential, as well as expreſſed oils, will eaſily take up; as ſpirit of wine itſelf alſo will do almoſt in a trice, if, (as we ſhall ſee anon) by the help of an alkali, the texture of the brimſtone be altered, though the only thing, that is added to the ſulphur, being an incombuſtible ſubſtance, is nothing near of ſo ſulphureous a nature, as the flowers, and need have no conſanguinity upon the ſcore of its origin with ſpirit of wine, as it is alledged, that ſalt of tartar has; ſince I have tried, that fixed nitre, employed inſtead of it, will do the ſame.

EXPERIMENT XVI.

THE mention of nitre brings into my mind, that the ſalt-petre, being wont to be looked upon, by chemiſts, as a very inflammable body, ought, according to them, to be of a very ſulphureous nature; yet we find not, that it is in chemical oils, but in water, readily diſſolved. And whereas chemiſts tells us, that the ſolutions of alkalies, ſuch as ſalt of tartar, or of pot-aſhes in common oils, proceed from the great cognition between them, I demand, whence it happens, that ſalt of tartar will, by boiling, be diſſolved in the expreſſed oil of almonds, or of olives, and be reduced with it to a ſoapy body, and that yet, with the eſſential oil of juniper or anniſeeds, &c. where what they call the ſulphur is made pure and penetrant, being freed from the earthy, aqueous and feculent parts, which diſtillation diſcovers to be in the expreſſed oils, you may boil ſalt of tartar twenty times as long, without making any ſoap of them, or perhaps any ſenſible ſolution of the alkali. And chemiſts know how difficult it is, and how unſucceſsfully it is wont to be attempted, to diſſolve pure ſalt of tartar in pure ſpirit of wine, by digeſting the not peculiarly prepared ſalt in the cognate menſtruum. I will not urge, that, though the moſt conſpicuous mark of ſulphur be inflammability, and is in an eminent degree to be found in oil, as well as ſulphur; yet an alkali and water, which are neither ſingly, nor united inflammable, will diſſolve common ſulphur.

EXPERIMENT XVII.

BUT, to make it probable againſt the chemiſts, (for I purpoſe it but as an argument *ad hominem*) that the ſolution of ſulphur in expreſſed oils depends upon ſomewhat elſe beſides the abundance of the ſecond principle, in both the bodies; I will add to what I ſaid before, an affirmation of divers chemical writers themſelves, who reckon aqua-regis, which is plainly a ſaline menſtruum, and diſſolves copper, iron, coral, &c. like acid liquors, among the ſolvents of ſulphur, and by that power, among other things, diſtinguiſh it from aqua-fortis. And, on the other ſide, if there be a congruity betwixt an expreſſed oil and another body, though it be ſuch as, by its eaſy diſſolubleneſs in acid ſalts, chemiſts ſhould pronounce to be of a ſaline nature, an expreſſed oil will readily enough work upon it; as I have tried, by digeſting even crude copper, in filings, with oil of ſweet almonds, which took up ſo much of the metal, as to be deeply coloured thereby, as if it had been a corroſive liquor: nay, I ſhall add, that, even with milk, as mild a liquor as it is, I have found, by trial, that, without the help of fire, a kind of diſſolution may, though not in few hours, be made of crude copper, as appeared by the greeniſh blue colour the filings acquired, when they had been well drenched in the liquor, and left for a certain time in the veſſel, where the air had very free acceſs to them.

EXPERIMENT XVIII.

BESIDES the argument *ad hominem*, newly drawn from aqua-regia, it may be proper enough to urge another of the ſame kind, upon the generality of the Helmontians and Paracelſians, who admit what the heads of their ſects delivered concerning the operations of the alkaheſt. For whereas it is affirmed, that this irreſiſtible menſtruum will diſſolve all tangible bodies here below, ſo as they may be reduced into inſipid water; as, on the one ſide, it will be very hard to conceive, how a ſpecificated menſtruum, that is determined to be either acid, or lixivate, or urinous, &c. ſhould be able to diſſolve ſo great a variety of bodies of differing, and perhaps contrary natures, in ſome whereof acids, in others, lixiviate ſalts, and in others, urinous are predominant; ſo, on the other ſide, if the alkaheſt be not a ſpecificated menſtruum, it will very much diſfavour the opinion of the chemiſts, that will have ſome bodies diſſoluble only by acids as ſuch, others by fixed alkalies, and others again by volatile ſalts; ſince a menſtruum, that is neither acid, lixiviate, nor urinous, is able to diſſolve bodies, in ſome of which one, and in others, another of thoſe principles is predominant: ſo that, if a liquor be conveniently qualified, it is not neceſſary, that it ſhould be either acid to diſſolve pearl or coral, or alkalizate to diſſolve ſulphur. But upon what mechanical account an analyzing menſtruum may operate, is not neceſſary to be here determined. And I elſewhere offer ſome thoughts of mine about it.

EXPERIMENT XIX.

IF we duly reflect upon the known proceſs, that chemiſts are wont to employ in making mercurius dulcis, we ſhall find it very favourable to our hypotheſis. For though we have already ſhewn in the fifth experiment, and it is generally confeſſed, that common ſublimate made of mercury is a highly corroſive body; yet, if it be well ground with near an equal weight of quickſilver, and be a few times ſublimed, (to mix them the more exactly) it will become ſo mild, that it will not ſo much as taſte ſharp upon the tongue; ſo that chemiſts are wont to call it mercurius dulcis: and yet this dulcification ſeems to be performed in a mechanical way. For moſt part of the ſalts, that made the ſublimate ſo corroſive, abide in the mercurius dulcis; but by being compounded with more quickſilver, they are diluted by it, and (which is more conſiderable) acquire a new texture, which renders them unfit to operate, as they did before, when the fretting ſalts were not joined with a ſufficient quantity of the mercury to inhibit their corroſive activity. It may perhaps ſomewhat help us to conceive, how this change may be made, if we imagine, that a company of mere knife-blades be firſt fitted with hafts, which will in ſome regard leſſen their wounding power by covering or caſing them at that end, which is deſigned for the handle; (though their inſertion into thoſe hafts, turning them into knives, makes them otherwiſe the fitter to cut and pierce) and, that each of them be afterwards ſheathed, (which is, as it were, a hafting of the blades too;) for then they become unfit to cut or ſtab, as before, though the blades be not deſtroyed: or elſe we may conceive theſe blades without hafts or ſheaths to be tied up in bundles, or as it were in little faggots with pieces of wood, ſomewhat longer than themſelves, opportunely placed between them. For neither in this new conſtitution would they be fit to cut and ſtab as before. And by conceiving the edge of more or fewer of the blades to be turned inwards, and thoſe, that are not, to have more or leſs of their points and edges to be ſheathed, or otherwiſe covered by interpoſed bodies, one may be helped to imagine, how the genuine effects of the blades may be variouſly leſſened or diverſified. But, whether theſe, or any other like changes of diſpoſition be fancied, it may by mechanical illuſtrations become intelligible, how the corroſive ſalts of common ſublimate may loſe their efficacy when they are united with a ſufficient quantity of quickſilver in mercurius dulcis: in which new ſtate, the ſalts may indeed, in a chemical phraſe, be ſaid to be ſatiated; but this chemical phraſe does not explicate, how this ſaturation takes away the corroſiveneſs from ſalts, that are ſtill actually preſent in the ſweet mercury. And by analogy to ſome ſuch explication, as the above propoſed, a poſſible account may be rendered, why fretting ſalts do either quite loſe their ſharpneſs, as alkalies, whilſt they are imbodied with ſand in common glaſs; or loſe much of their corroſive acidity, as oil of vitriol does, when with ſteel it compoſes vitriolum martis;

martis; or elſe are tranſmuted or diſguiſed by conjunction with ſome corroded bodies of a peculiar texture, as when aqua-fortis does with ſilver make an extremely bitter ſalt or vitriol, and with lead one, that is poſitively ſweet, almoſt like common ſaccharum ſaturni.

EXPERIMENT XX.

To ſhew, how much the efficacy of a menſtruum may depend even upon ſuch ſeemingly ſlight mechanical circumſtances, as one would not eaſily ſuſpect any neceſſity of, I ſhall employ an experiment, which, though the unpractiſed may eaſily fail of making well, yet, when I tried it after the beſt manner, I did it with good ſucceſs. I put then upon lead a good quantity of well rectified aqua-fortis, in which the metal, as I expected, continued undiſſolved; though, if the chemiſts ſay truly, that the diſſolving power of the menſtruum conſiſts, only in the acid ſalts, that it abounds with, it ſeems naturally to follow, that the more abundance of them there is in a determinate quantity of the liquor, it ſhould be the more powerfully able to diſſolve metalline and mineral bodies. And in effect, we ſee, that, if corroſive menſtruums be not ſufficiently dephlegmed, they will not work on divers of them. But, notwithſtanding this plauſible doctrine of the chemiſts, conjecturing, that the ſaline particles, that ſwam in our aqua-fortis, might be more thronged together, than was convenient for a body of ſuch a texture of ſaline parts, and ſuch intervals between them, I diluted the menſtruum, by adding to it what I thought fit of fair water, and then found, that the deſired congruity betwixt the agent and the patient emerged, and the liquor quickly began to fall upon the metal, and diſſolve it. And if you would try an experiment to the ſame purpoſe, that needs much leſs circumſpection to make it ſucceed, you may, inſtead of employing lead, reiterate what I elſewhere mention myſelf to have tried with ſilver, which would not diſſolve in too ſtrong aqua-fortis, but would be readily fallen upon by that liquor, when I had weakened it with common water.

AND this it may ſuffice to have ſaid at preſent of the power or faculty, that is found in ſome bodies of corroding or diſſolving others. Whereof I have not found among the Ariſtotelians, I have met with, ſo much as an offer at an intelligible account. And I the leſs expect the vulgar chemiſts will from their hypoſtatical principles afford us a ſatisfactory one, when, beſides the particulars, that from the nature of the things, and *Helmont*'s writings, have been lately alledged againſt their hypotheſis, I conſider, how ſlight accounts they are wont to give us even of the familiar phænomena of corroſive liquors. For if, for example, you aſk a vulgar chemiſt, why aqua-fortis diſſolves ſilver and copper, it is great odds but he will tell you, it is becauſe of the abundance of fretting ſalt, that is in it, and has a cognation with the ſalts of the metal. And if you aſk him, why ſpirit of ſalt diſſolves copper, he will tell you it is for the ſame reaſon; and yet, if you put ſpirit of ſalt, though very ſtrong, to aqua-fortis, this liquor will not diſſolve ſilver, becauſe upon the mixture the liquors acquire a new conſtitution as to the ſaline particles, by virtue of which the mixture will diſſolve, inſtead of ſilver, gold. Whence we may argue againſt the chemiſts, that the inability of this compounded liquor to work on ſilver does not proceed from its being weakened by the ſpirit of ſalt; as well becauſe, according to them, gold is far the more compact metal of the two, and requires a more potent menſtruum to work upon it, as becauſe this ſame compounded liquor will readily diſſolve copper. And to the ſame purpoſe, with this experiment I ſhould alledge divers others, if I thought this the fitteſt place, wherein I could propoſe them.

SECTION II.

About the MECHANICAL ORIGIN *of* CORROSIBILITY.

CORROSIBILITY being the quality, that anſwers corroſiveneſs. he, that has taken notice of the advertiſement I formerly gave ab.ut my uſe of the term * corroſiveneſs, in theſe notes, may eaſily judge, in what ſenſe I employ the name of the other quality; which, whether you will ſtile it oppoſite or conjugate, for want of a better word, I call corroſibility.

THIS corroſibility of bodies is, as well as their corroſiveneſs, a relative thing; as we ſee, that gold, for inſtance, will not be diſſolved by aqua-fortis, but will by aqua-regis; whereas ſilver is not ſoluble by the latter of theſe menſtruums, but is by the former. And this relative affection, on whoſe account a body comes to be corrodible by a menſtruum, ſeems to conſiſt chiefly in three things, which all of them depend upon mechanical principles.

OF theſe qualifications, the firſt is, that the body to be corroded be furniſhed with pores of ſuch a bigneſs and figure, that the corpuſcles of the ſolvent may enter them, and yet not be much agitated in them, without giving briſk knocks or ſhakes to the ſolid parts, that make up the walls, if I may ſo call them, of the pores. And it is for want of this condition, that glaſs is penetrated in a multitude of places, but not diſſipated or diſſolved by the incident beams of light, which permeate its pores without any conſiderable reſiſtance; and though the pores and commiſſures of a body were leſs minute, and capable of letting in ſome groſſer corpuſcles, yet if theſe were, for want of ſolidity or rigidneſs, too flexible, or were of a figure incongruous to that of the pores they ſhould enter, the diſſolution would not enſue; as it happens, when pure ſpirit of wine is in the cold put upon ſalt of tartar, or when aqua-fortis is put upon powder of ſulphur.

THE ſecond qualification of a corrodible body is, that its conſiſtent corpuſcles be of ſuch a bulk and ſolidity, as does not render them uncapable of being disjoined by the action of the inſinuating corpuſcles of the menſtruum. Agreeable to this, and the former obſervation, is the practice of chemiſts, who oftentimes, when they would have a body to be wrought on by a menſtruum, otherwiſe too weak for it in its crude eſtate, diſpoſe it to receive the action of the menſtruum by previouſly opening it, (as they ſpeak) that is, by enlarging the pores, making a comminution of the corpuſcles, or weakening their coheſion. And we ſee, that divers bodies are brought by fit preparations to be reſoluble in liquors, that would not work on them before. Thus, as was lately noted, lime ſtone by calcination becomes, in part, diſſoluble in water; and ſome metalline calces will be ſo wrought on by ſolvents, as they would not be by the ſame agents, if the preparation of the metalline or other body had not given them a new diſpoſition. Thus, though crude tartar, eſpecially in lumps, is very ſlowly and difficultly diſſoluble in cold water, yet when it is burnt, it may be preſently diſſolved in that liquor; and thus, though the filings and the calx of ſilver will not be at all diſſolved by common water, or ſpirit of wine; yet if by the interpoſition of the ſaline particles of aqua-fortis, the lunar corpuſcles be ſo disjoined, and ſuffer ſuch a comminution as they do in cryſtals of lune, the metal thus prepared and brought with its ſaline additament into a new texture, will eaſily enough diſſolve, not only in water, but, as I have tried, in well rectified ſpirit of wine. And the like ſolubility I have found in the cryſtals of lead, made with ſpirit of verdigreaſe, or good diſtilled vinegar, and in thoſe of copper made with aqua-fortis.

THE

* See the beginning of the firſt Section.

THE laſt diſpoſition to corroſibility conſiſts in ſuch a coheſion of the parts, whereof a body is made up, as is not too ſtrict to be ſuperable by the action of the menſtruum. This condition, though of kin to the former, is yet ſomewhat differing from it, ſince a body may conſiſt of parts, either bulky or ſolid, which yet may touch one another in ſuch ſmall portions of their ſurfaces, as to be much more eaſily diſſociable than the minute or leſs ſolid parts of another body, whoſe contact is more full and cloſe, and ſo their coheſion more ſtrict.

BY what has been ſaid, it may ſeem probable, that, as I formerly intimated, the corroſibility of bodies is but a mechanical relation, reſulting from the mechanical affections and contexture of its parts, as they intercept pores of ſuch ſizes and figures, as make them congruous to thoſe of the corpuſcles of the menſtruum, that are to pierce between them, and disjoin them.

THAT the quality, that diſpoſes the body it affects to be diſſolved by corroſive and other menſtruums, does, as hath been declared, in many caſes depend upon the mechanical texture and affections of the body in reference to the menſtruum, that is to work upon it, may be made very probable, by what we are in due place to deliver concerning the pores of bodies and figures of corpuſcles. But yet in compliance with the deſign of theſe notes, and agreeably to my cuſtom on other ſubjects, I ſhall ſubjoin a few experiments on this occaſion alſo.

EXPERIMENT I.

IF we put highly rectified ſpirit of wine upon crude ſulphur, or even flowers of ſulphur, the liquor will lie quietly thereon, eſpecially in the cold, for many hours and days, without making any viſible ſolution of it; and if ſuch exactly dephlegmed ſpirit were put on very dry ſalt of tartar, the ſalt would lie in an undiſſolved powder at the bottom: and yet, if before any liquor be employed, the ſulphur be gently melted, and then the alkali of tartar be by degrees put to it, and incorporated with it; as there will reſult a new texture diſcoverable to the eye by the new colour of the compoſition, ſo there will emerge a diſpoſition that was not before in either of the ingredients, to be diſſolved by ſpirit of wine; inſomuch, that though the mixture be kept till it be quite cold, or long after too, provided it be carefully ſecured from the acceſs of the air, the ſpirit of wine being put to it, and ſhaken with it, will, if you have gone to work aright, acquire a yellow tincture in a minute of an hour; and perhaps in leſs than half a quarter of an hour a red one, being richly impregnated with ſulphureous particles diſcoverable by the ſmell, taſte, and divers operations.

EXPERIMENT II.

[IT is known to ſeveral chemiſts, that ſpirit of ſalt does not diſſolve crude mercury in the cold; and I remember, I kept them for a conſiderable time in no contemptible heat without finding any ſolution following. But I ſuppoſe, many of them will be gratified by an experiment once mentioned to me by an ingenious German gentleman, namely, that if mercury be precipitated *per ſe*, that is, reduced to a red powder without additament, by the mere operation of the fire, the texture will be ſo changed, that the abovementioned ſpirit will readily diſſolve it; for I found it upon trial to do ſo; nay, ſometimes ſo readily, that I ſcarce remember, that I ever ſaw any menſtruum ſo nimbly diſſolve any metalline body whatſoever.

EXPERIMENT III.

THE former experiment is the more remarkable, becauſe, that though oil of vitriol will in a good heat corrode quickſilver, (as we have already related in the firſt ſection,) yet I remember I kept a precipitate *per ſe* for divers hours in a conſiderable degree of heat, without finding it to be diffolved or corroded by the menftruum. And yet having, for trial's fake, put another parcel of the fame mercurial powder into fome aquafortis, or fpirit of nitre, there enfued a fpeedy diffolution even in the cold.

AND that this difpofition to be diffolved by fpirit of falt, that mercury acquires by being turned into precipitate *per fe*, that is, by being calcined, is not merely the effect of the operation of the fire upon it, but of fome change of texture produced by that operation; may be probably argued from hence, that, whereas fpirit of falt is a very proper menftruum, as I have often tried, for the diffolving of iron or fteel; yet, when that metal is reduced by the action of the fire (efpecially if a kind of vitrification, and an irroration with diftilled vinegar have preceded) to crocus martis, though it be thereby brought to a very fine powder, yet I found not, that, as fpirit of falt will readily, and with heat and noife, diffolve filings of mars, fo it would have the fame or any thing near fuch an operation upon the crocus; but rather, after a good while, it would leave in the bottom of the glafs a confiderable, if not the greateft part of it fcarce, if at all, fenfibly altered. And the menftruum feemed rather to have extracted a tincture; than made an ordinary folution; fince the colour of it was a high yellow or reddifh, whereas mars, diffolved in fpirit of falt, affords a green folution. Whether by repeated operations with frefh menftruum further diffolutions might in time be made, I had not occafion to try, and it may fuffice for our prefent purpofe, that mars, by the operation of the fire, did evidently acquire, not, as mercury had done, a manifeft facility, but on the contrary, a great indifpofition to be diffolved by fpirit of falt.

To fecond this experiment, we varied it, by employing, inftead of fpirit of falt, ftrong oil of vitriol, which being poured on a little crocus martis made *per fe*, did not, as that menftruum is wont to do upon filings of crude mars, readily and manifeftly fall upon the powder with froth and noife, but, on the contrary, refted for divers hours calmly upon it, without fo much as producing with it any fenfible warmth.

EXPERIMENT IV.

IT agrees very well with our doctrine about the dependance of the corrofibility of bodies upon their texture, that from divers bodies, whilft they are in conjunction with others, there refult maffes, and thofe homogeneous as to fenfe, that are eafily diffoluble in liquors, in which a great part of the matter, if it were feparated from the reft, would not be at all diffolved. Thus we fee, that common vitriol is eafily diffolved in mere water; whereas if it be fkilfully calcined, it will yield fometimes near half its firft weight of infipid colcothar, which not only is not foluble in water, but which neither aqua-fortis nor aqua-regis, though fometimes they will colour themfelves upon it, are able, as far as I have tried, to make folutions of. We fee likewife, that fimple water will, being boiled for a competent time with hart's-horn, diffolve it and make a jelly of it: and yet, when we have taken hart's-horn throughly calcined to whitenefs, not only we found, that common water was no longer a fit folvent for it, but we obferved, that when we put oil of vitriol itfelf upon it, a good part of the white powder was even by that corrofive menftruum left undiffolved.

EXPERIMENT V.

IN the fifteenth of the foregoing experiments I refer to a way of making the flower or powder of common fulphur become eafily diffoluble, which otherwife it is far from being, in highly rectified fpirit of wine. Wherefore I fhall now add, that it is quickly performed by gently melting the fulphur, and incorporating with it by degrees an equal or a greater weight of finely powdered falt of tartar, or of fixed nitre. For if the mixture be put warm into a mortar, that is fo too; and as foon as it reduced to powder,

der, but put into a glaſs, and well ſhaken with pure ſpirit of wine, it will, (as perhaps I may have elſewhere obſerved) in a few minutes acquire a yellow colour, which afterwards will grow deeper, and manifeſt itſelf by the ſmell and effects to be a real ſolution of ſulphur; and yet this ſolubleneſs in ſpirit of wine ſeems procured by the change of texture, reſulting from the commixtion of mere ſalt of tartar, which chemiſts know, to their trouble, to be itſelf a body almoſt as difficult as ſulphur to be diſſolved in phlegmleſs ſpirit of wine, unleſs the conſtitution of it be firſt altered by ſome convenient additament. Which laſt words I add, becauſe, though ſpirit of verdigreaſe be a menſtruum, that uſes to come off in diſtillation much more intirely than other acid menſtruums from the bodies it has diſſolved; yet it will ſerve well for an additament to open (as the chemiſts ſpeak) the body of the ſalt of tartar. For this purpoſe I employ ſpirit of verdigreaſe, not made firſt with ſpirit of vinegar, and then of wine, after the long and laborious way preſcribed by *Baſilius* and *Zwelfer*, but eaſily and expeditiouſly by a ſimple diſtillation of crude verdigreaſe of the better ſort. For when you have with this liquor (being, if there be need, once rectified) diſſolved as much good ſalt of tartar, as it will take up in the cold, if you draw off the menſtruum *ad ſiccitatem*, the remaining dry ſalt will be manifeſtly altered in texture even to the eye, and will readily enough, in high rectified ſpirit of wine, afford a ſolution, which I have found conſiderable in order to divers uſes, that concern not our preſent diſcourſe.

EXPERIMENT VI.

To the conſideration of the followers of *Helmont* I ſhall recommend an experiment of that famous chemiſt's which ſeems to ſuit exceeding well with the doctrine propoſed in this ſection. For he tells us, that, if by a ſubtle menſtruum, to which he aſcribes that power, quickſilver be diveſted (or deprived) of its external ſulphur, as he terms it, all the reſt of the fluid metal, which he wittily enough ſtiles the kernel of mercury, will be no longer corroſible by it. So that upon this ſuppoſition, though common quickſilver be obſerved to be ſo obnoxious to aqua-fortis, that the ſame quantity of that liquor will diſſolve more of it, than of any other metal; yet, if by the deprivation of ſome portion of it the latent texture of the metal be altered, though not (that I remember) the viſible appearance of it; the body, that was before ſo eaſily diſſolved by aqua-fortis, ceaſes to be at all diſſoluble by it.

EXPERIMENT VII.

As for thoſe chemiſts of differing ſects, that agree in giving credit to the ſtrange things, that are affirmed of the operations of the alkaheſt, we may in favour of our doctrine urge them with what is delivered by *Helmont*, where he aſſerts, that all ſolid bodies, as ſtones, minerals, and metals themſelves, by having this liquor duly abſtracted or diſtilled off from them, may be changed into ſalt, equiponderant to the reſpective bodies whereon the menſtruum was put. So, that ſuppoſing the alkaheſt to be totally abſtracted, (as it ſeems very probable to be, ſince the weight of the body, whence it was drawn off, is not altered;) what other change, than of texture, can be reaſonably imagined to have been made in the tranſmuted bodies? and yet divers of them, as flints, rubies, ſapphires, gold, ſilver, &c. that were inſoluble before, ſome of them in any known menſtruums, and others in any but corroſive liquors, come to be capable of being diſſolved in common water.

EXPERIMENT VIII.

It is a remarkable phænomenon, that ſuits very well with our opinion about the intereſt of mechanical principles in the corroſive power of menſtruums, and the corroſibility

ſibility of bodies, that we produced by the following experiment: this we purpoſely made to ſhew, after how differing manners the ſame body may be diſſolved by two menſtruums, whoſe minute parts are very differingly conſtituted and agitated. For whereas it is known, that if we put large grains of ſea-ſalt into common water, they will be diſſolved therein, calmly and ſilently, without any appearance of conflict; if we put ſuch grains of ſalt into good oil of vitriol, that liquor will fall furiouſly upon them, and produce for a good while a hiſſing noiſe with fumes, and a great ſtore of bubbles, as if a potent menſtruum were corroding ſome ſtubborn metal or mineral. And this experiment I the rather mention, becauſe it may be of uſe to us on divers other occaſions. For elſe it is not the only, though it be the remarkableſt, that I made to the ſame purpoſe.

EXPERIMENT IX.

FOR, whereas aqua-fortis, or aqua-regis, being poured upon filings of copper, will work upon them with much noiſe and ebullition, I have tried, that ſpirit of ſal armoniac or urine, being put upon the like filings, and left there without ſtopping the glaſs, will quickly begin to work on them, and quietly diſſolve them almoſt as water diſſolves ſugar. To which may be added, that even with oil of turpentine I have, though but ſlowly, diſſolved crude copper; and the experiment ſeemed to favour our conjecture the more, becauſe having tried it ſeveral times, it appeared, that common unrectified oil would perform the ſolution much quicker than that, which was purified and ſubtilized by rectification; which though more ſubtle and penetrant, yet was, it ſeems, on that account leſs fit to diſſolve the metal, than the groſſer oil, whoſe particles might be more ſolid, or more advantageouſly ſhaped, or on ſome other mechanical account better qualified for the purpoſe.

EXPERIMENT X.

TAKE good ſilver, and, having diſſolved it in aqua-fortis, precipitate it with a ſufficient quantity of good ſpirit of ſalt; then having waſhed the calx, which will be very white, with common water, and dried it well, melt it with a moderate fire into a fuſible maſs, which will be very much of the nature of what chemiſts call cornu lunæ, and which they make by precipitating diſſolved ſilver with a bare ſolution of common ſalt made in common water. And whereas both ſpirit of ſalt and ſilver, diſſolved in aqua-fortis, will each of them apart readily diſſolve in ſimple water, our luna cornea not only will not do ſo, but is ſo indiſpoſed to diſſolution, that I remember I have kept it in digeſtion, ſome in aqua-fortis, and ſome in aqua-regia, and that for a good while, and in no very faint degree of heat, without being able to diſſolve it like a metal, the menſtruums having indeed tinged themſelves upon it, but left the compoſition undiſſolved at the bottom.

WITH this inſtance (of which ſort more might be afforded by chemical precipitations) I ſhall conclude what I deſigned to offer at preſent about the corroſibility of bodies, as it may be conſidered in a more general way. For as to the diſpoſition, that particular bodies have of being diſſolved in, or of reſiſting determinate liquors, it were much eaſier for me to enlarge upon that ſubject, than it was to provide the inſtances above recited. And theſe are not ſo few, but that it is hoped they may ſuffice to make it probable, that in the relation betwixt a ſolvent and a body it is to work upon, that, which depends upon the mechanical affections of one or both, is much to be conſidered, and has a great intereſt in the operations of one of the bodies upon the other.

OF

Of the MECHANICAL CAUSES OF CHEMICAL PRECIPITATION.

ADVERTISEMENT.

THOUGH I ſhall not deny, that, in grammatical ſtrictneſs, precipitation ſhould be reckoned among chemical operations, not qualities; yet I did not much ſcruple to inſert the following diſcourſe among the notes about particular qualities, becauſe many, if not moſt of the phænomena, mentioned in the enſuing eſſay, may be conſidered as depending, ſome of them, upon a power, that certain bodies have to cauſe precipitation, and ſome upon ſuch a diſpoſition, to be ſtruck down by others, as may, if men pleaſe, be called precipitability. And ſo theſe differing affections may, with (at leaſt) tolerable congruity, be referred to thoſe, that we have elſewhere ſtiled chemical qualities.

BUT though, I hope, I may, in theſe few lines, have ſaid enough concerning the name given to theſe attributes, yet, perhaps, it will be found in time, that the things themſelves may deſerve a larger diſcourſe, than my little leiſure would allow them. For, that is not a cauſeleſs intimation of the importance of the ſubject, wherewith I conclude the following tract; ſince, beſides that, many more inſtances might have been particularly referred to the heads treated of in the enſuing eſſay, there are improper kinds of precipitation, (beſides thoſe mentioned in the former part of the diſcourſe) to which one may not incongruouſly refer divers of the phænomena of nature, as well in the greater as in the leſſer world, whereof either no cauſes at all, or but improper ones, are wont to be given. And, beſides the ſimple ſpirits and ſalts uſually employed by chemiſts, there are many compounded and decompounded bodies, not only factitious, but natural, (and ſome ſuch, as one would ſcarce ſuſpect) that may, in congruous ſubjects, produce ſuch precipitations, as I ſpeak of. And the phænomena and conſequents of ſuch operations may, in divers caſes, prove conducive both to the diſcovery of phyſical cauſes, and the production of uſeful effects; though the particularizing of ſuch phænomena do rather belong to a hiſtory of precipitations, than to ſuch a diſcourſe as that, which follows, wherein I propoſed not ſo much to deliver the latent myſteries, as to inveſtigate the mechanical cauſes of precipitation.

Of the MECHANICAL CAUSES of CHEMICAL PRECIPITATION.

CAHP. I.

BY precipitation is here meant, ſuch an agitation or motion of a heterogeneous liquor, as in no long time makes the parts of it ſubſide, and that uſually in the form of a powder, or other conſiſtent body.

AS, on many occaſions, chemiſts call the ſubſtance, that is made to fall to the bottom of the liquor, the precipitate; ſo, for brevity-ſake, we ſhall call the body, that is put into the liquor to procure that ſubſiding, the precipitant; as alſo that, which is to be ſtruck down, the precipitable ſubſtance or matter, and the liquor wherein it ſwims before the ſeparation, the menſtruum or ſolvent.

WHEN a hafty fall of a heterogeneous body is procured by a precipitant, the operation is called precipitation, in the proper or ftrict fenfe: but when the feparation is made without any fuch addition, or the fubftance, feparated from the fluid part of the liquor, inftead of fubfiding emerges, then the word is ufed in a more comprehenfive, but lefs proper acceptation.

As for the caufes of precipitation, the very name itfelf, in its chemical fenfe, having been fcarce heard of it in the peripatetick fchools, it is not to be expected, that they fhould have given us an account of the reafons of the thing. And it is like, that thofe few Ariftotelians, that have, by their converfe with the laboratories or writings of chemifts, taken notice of this operation, would, according to their cuftom on fuch occafions, have recourfe for the explication of it to fome fecret fympathy or antipathy between the bodies, whofe action and re-action intervenes in this operation.

BUT if this be the way propofed of accounting for it, I fhall quickly have occafion to fay fomewhat to it, in confidering the ways propofed by the chemifts, who were wont to refer precipitation, either, as is moft ufual, to a fympathy betwixt the precipitating body and the menftruum, which makes the folvent run to the embraces of the precipitant, and fo let fall the particles of the body fuftained before; or (with others) to a great antipathy, or contrariety between the acid falt of the menftruum and the fixed falt of the oil, or folution of calcined tartar, which is the moft general and ufual precipitant they employ.

BUT I fee not, how either of thefe caufes will either reach to all the phænomena, that have been exhibited, or give a true account even of fome of thofe, to which it feems applicable. For firft, in precipitations, wherein what they call a fympathy between the liquors, is fuppofed to produce the effect, this admired fympathy does not (in my apprehenfion) evince fuch a myfterious occult quality, as is prefumed, but rather confifts in a greater congruity, as to bignefs, fhape, motion, and pores of the minute parts between the menftruum and the precipitant, than between the fame folvent and the body it kept before diffolved. And though this fympathy, rightly explained, may be allowed to have an intereft in fome fuch precipitations, as let fall the diffolved body in its priftine nature and form, and only reduced into minute powder; yet I find not, that, in the generality of precipitations, this doctrine will hold; for in fome, that we have made of gold and filver in proper menftruums, after the fubfiding matter had been well wafhed and dried, feveral precipitates of gold made, fome with oil of tartar, which abounds with a fixed falt, and is the ufual precipitant, and fome with an urinous fpirit, which works by virtue of a falt highly fugitive, or volatile, I found the powder to exceed the weight of the gold and filver I had put to diffolve; and the eye itfelf fufficiently difcovers fuch precipitates not to be mere metalline powders, but compofitions, whofe confifting, not (as hath been by fomebody fufpected) of the combined falts alone, but of the metalline parts alfo, may be ftrongly concluded, not only from the ponderoufnefs of divers of them, in reference to their bulk, but alfo manifeftly from the reduction of true malleable metals from feveral of them.

CHAP. II.

THE other chemical way of explicating precipitations may, in a right fenfe, be made ufe of by a naturalift on fome particular occafions. But I think it much too narrow and defective, as it is in a general way propofed, to be fit to be acquiefced in. For firft it is plain, that it is not only falt of tartar, and other fixed alkalies, that precipitate moft bodies, that are diffolved in acid menftruums; as in making of aurum fulminans, oil of tartar precipitates the gold out of aqua-regis: but acid liquors themfelves ſelves do, on many occaſions, no leſs powerfully precipitate metals and other bodies out of one another. Thus, ſpirit of ſalt (as I have often tried) precipitates ſilver out of aqua-fortis: the corroſive ſpirit of nitre copiouſly precipitates that white powder, whereof they make bezoardicum minerale: ſpirit or oil of ſulphur, made by a glaſs bell, precipitates corals, pearls, &c. diſſolved in ſpirit of vinegar, as is known to many chemiſts, who now uſe this oleum ſulphuris per campanam, to make the magiſtery of pearls, &c. for which vulgar chemiſts employ oleum tartari per deliquium.

I have ſometimes made a menſtruum, wherein, though there were both acid and alkalizate ſalts, yet I did not find, that either acid ſpirits, or oil of tartar, or even ſpirit of urine, would precipitate the diſſolved ſubſtances.

AND I have obſerved, both that ſalts of a contrary nature will precipitate bodies out of the ſame menſtruum, as not only ſalt of tartar, but ſea-ſalt, being diſſolved, will precipitate each other, and, each of them apart, will precipitate ſilver out of aqua-fortis; and that even, where there is a confeſſed contrariety betwixt two liquors, it may be ſo ordered, that neither of them ſhall precipitate what is diſſolved by the other; of which I ſhall have occaſion to give, ere long, a remarkable inſtance.

BUT it will beſt appear, that the above-mentioned theories of the Peripateticks and chemiſts are at leaſt inſufficient to ſolve the phænomena (many of which were probably not known to moſt of them, and perhaps not weighed by any,) if we proceed to obſerve the mechanical ways, by which precipitations may be accounted for; whereof I ſhall, at preſent, propoſe ſome number, and ſay ſomewhat of each of them apart; not that I think all of them to be equally important and comprehenſive, or that I abſolutely deny, that any one of them may be reduced to ſome of the other; but that I think, it may better elucidate the ſubject, to treat of them ſeverally, when I ſhall have premiſed, that I would not thence infer, that though, for the moſt part, nature does principally effect precipitations by one or other of theſe ways, yet, in divers caſes, ſhe may not employ two, or more of them, about performing the operation.

To precipitate the corpuſcles of a metal out of a menſtruum, wherein, being once thoroughly diſſolved, it would, of itſelf, continue in that ſtate, the two general ways, that the nature of the thing ſeems to ſuggeſt to him, that conſiders it, are, either to add to the weight or bulk of the diſſolved corpuſcles, and thereby render them unfit to accompany the particles of the menſtruum in their motions; or to weaken the ſuſtaining power of the menſtruum, and thereby diſable it to keep the metalline particles ſwimming any longer: which falling of the deſerted parts of the metal, or other body, does oftentimes the more eaſily enſue, becauſe in many caſes, when the ſuſtaining particles of the menſtruum come to be too much weakened, that proves an occaſion to the metalline corpuſcles, diſturbed in the former motion, that kept them ſeparate, to make occurſions and coalitions among themſelves, and their fall becomes the effect, though not equally ſo, of both ways of precipitation; as on the other ſide, there are ſeveral occaſions on which the ſame precipitant, that brings the ſwimming particles of the metal to ſtick to one another, does likewiſe, by mortifying or diſabling the ſaline ſpirits, or other parts of the ſolvent, weaken the ſuſtaining power of that liquor.

CHAP. III.

TO deſcend now to the diſtinct conſiderations about theſe two ways: the firſt of the moſt general cauſes of precipitation is ſuch a coheſion procured by the precipitant in the ſolution, as makes the compounded corpuſcles, or at leaſt the aſſociated particles of the diſſolved body, too heavy to be ſuſtained, or too bulky to be kept in a ſtate of fluidity by the liquor.

THAT in many precipitations there is made a coalition betwixt the small parts of the precipitant, and those of the dissolved metal, or other body and frequently also, with the saline spirits of the menstruum, may be easily shewn, by the weight of the precipitate, which, though carefully washed and dried, often surpasses, and sometimes very considerably, that of your crude metal, that was dissolved; of which we lately gave an instance in aurum fulminans, and precipitated silver; and we may yet give a more conspicuous one, in that, which chemists call luna cornea: for, if having dissolved silver in good aqua-fortis, you precipitate it with the solution of sea-salt in fair water, and from the very white precipitate wash the loose adhering salts, the remaining powder, being dried and slowly melted, will look much less like a metalline body, than like a piece of horn, whence also it takes its name; so considerable is the additament of the saline to the metalline particles.

AND that part of such additaments is retained, may not only be found by weighing, but, in divers cases, may be argued from what is obvious to the eye: as if you dissolve mercury in aqua fortis, and into the philtrated solution drop spirit of salt, or salt water, or an urinous spirit, as of sal armoniac, you will have a very white precipitate; but if, instead of any of these, you drop in deliquated salt of tartar, your precipitate will be of a brick, or orange colour. From which experiment, and some others, I would gladly take a rise to persuade chemists and physicians, that it is not so indifferent, as those seem to think, who look on precipitation but as a kind of comminution, by what means the precipitation is performed. For, by reason of the strict adhesion of divers saline particles of the precipitant and the solvent, the precipitated body, notwithstanding all the wonted ablutions, may have its qualities much diversified by those of the particles of the liquors, when these are fitted to stick very fast to it. Which last words I add, because, though that sometimes happens, yet it does not always, there being a greater difference, than every body takes notice of, between precipitations; as you will be induced to think, if you precipitate the solution of silver with copper, with spirit of sal armoniac, with salt water, with oil of tartar, with quicksilver, with crude tartar, and with zink. And in the lately proposed example, you will think it probable, that it is not all one, whether to dissolved mercury or silver, you employ the subtile distilled spirits of salt, or the gross body, whether in a dry form, or barely dissolved in common water. And thus much of the conduciveness of weight to the striking down the corpuscles of a dissolved body.

THAT also the bulk of a body may very much contribute to make it sink or swim in a liquor, appears by obvious instances. Thus salt or sugar, being put into water, either in lumps or even in powder, that is but gross, falls at first to the bottom, and lies there, notwithstanding the air, that may be intercepted between its parts, or externally adhere to it. But when, by the insinuating action of the water, it is dissolved into minute particles, these are carried up and down with those of the liquor, and subside not. The like happens, when a piece of silver is cast into aqua-fortis, and in many other cases.

ON the other side I have several times observed, that some bodies, that had long swam in a menstruum, whilst their minute parts were kept from convening in it, did afterwards, by the coalition of many of those particles into bodies of a visible bulk, coagulate and subside, (though sometimes, to hinder the evaporation of the menstruum, the vessels were kept stopped.) Of this I elsewhere mention divers examples; and particularly in urinous and animal spirits, well dephlegmed, I have found, that after all had, for a considerable time, continued in the form of a perfect liquor, and as to sense homogeneous, store of solid corpuscles, convening together, settled at the bottom of the glasses in the form of saline crystals. Having also long kept a very red solution of

sulphur

sulphur first unlocked, (as they speak) made with highly rectified spirit of urine, I observed, that at length the sulphureous particles, making little concretions between themselves, totally subsided, and left the liquor almost devoid of tincture. By which you may see, that it was not impertinent to mention (as I lately did) among the subordinate causes of precipitation, the associating of the particles of a dissolved body with one another. Of which I elsewhere give a notable example in the shining powder, that I obtained from gold dissolved in a peculiar menstruum, without any precipitant, by the coalition of the metalline particles, to which a tract of time gave opportunity to meet and adhere in a convenient manner.

If in what the chemists call *præcipitate per se*, the mercury be indeed brought to lose its fluidity, and become a powder without being compounded with any additional body, (which doubt I elsewhere state and discourse of,) it will afford us a notable instance to prove, that the coalitions of particles into clusters of the self-same matter will render them unfit for the motion requisite to fluidity. For, in this odd precipitation by fire, wherein the same menstruum is both the liquor and the precipitate, being not all made at once, the corpuscles, that first disclose themselves by their redness, are rejected by those of the mercury, that yet remains fluid, as unable to accompany them in the motions, that belong to mercury as such.

CHAP. IV.

BEFORE I dismiss that way of precipitating, that depends upon the unwieldiness, which the precipitant gives to the body it is to strike down, it may not be impertinent, especially in reference to the foregoing part of this paper, to consider, that perhaps, in divers cases, the corpuscles of a dissolved body may be made unfit to be any longer sustained in the menstruum, though the precipitant adds very little to their bulk, or at least, much more to their specific weight than to it. For I have elsewhere shewn, that in divers solutions made of bodies by acid menstruums, there are either generated or extricated many small aerial particles; and it will be easily granted, that these may be small enough to be detained in the pores of the liquor, and be invisible there, if we consider, what a multitude of aerial and formerly imperceptible bubbles is afforded by common water in our pneumatical receivers, when the incumbent air, that before pressed the liquor, is pumped out. And if the corpuscles of the dissolved body have any little cavities or pores fit to lodge aerial particles, or have asperous surfaces, between whose prominent parts the generated air may conveniently lie; in such cases, I say, these invisible bubbles may be looked upon, as making with the solid corpuscles they adhered to, little aggregates much lighter in specie than the corpuscles themselves would be; and consequently if the precipitant consist of particles of such a size and shape as are fit to expel these little bubbles, and lodge themselves in the cavities possessed by them before, there will be produced new aggregates composed of the corpuscles of the dissolved body and the particles of the precipitant; which aggregates, though they do take up very little, or perhaps not at all more room (taking that word in a popular sense) than those, whereof the aerial bubbles made a part, will yet be specifically heavier than the former aggregates were, and may thereby overcome the sustaining power of the menstruum.

One thing more may be fit to be taken notice of before we pass on further, namely, that it is upon the score of the specifick gravity of a body, and not barely upon the action of the precipitant, that an aggregate, or a convention, of particles, does rather fall to the bottom than rise to the top. For, though the agents, that procured the coalition, make the cluster of particles become of a bulk too unweildy to continue in the liquor

as

as parts of it; yet if each of them be lighter in ſpecie than an equal bulk of the menſtruum, or if they ſo convene, as to intercept a ſufficient number of little bubbles or aerial corpuſcles between them, and ſo become lighter than as much of the menſtruum, as they take up the room of, they will not be precipitated but emerge; as may be ſeen in the preparation of thoſe magiſteries of vegetables, I elſewhere mention; where ſome deeply coloured plants being made to tinge plentifully the lixivium they are boiled in, are afterwards by the addition of alum made to curdle, as it were, into coloured concretions, which being (totally or in part) too big to ſwim, as they did before they convened, and too light in compariſon of the menſtruum to ſubſide, emerge to the top, and float there. An eaſier and neater example to the ſame purpoſe, I remember I ſhewed by diſſolving camphire in highly rectified ſpirit of wine, till the ſolution was very ſtrong. For though the camphire, when put in lumps into the ſpirit, ſunk to the bottom of it; yet, when good ſtore of water, (a liquor ſomewhat heavier in ſpecie than camphire,) was poured upon the ſolution, the camphire quickly concreted and returned to its own nature, and within a while emerged to the top of the mingled liquors and floated there. Theſe particulars I was willing to mention here, that I might give an inſtance or two of thoſe precipitations, that I formerly ſpake of as improperly ſo called. And here I muſt not decline taking notice of a phænomenon, that ſometimes occurs in precipitations, and at firſt ſight may ſeem contrary to our doctrine about them. For now and then it happens, that after ſome drops of the precipitant have begun a precipitation at the top or bottom of the ſolvent, one ſhakes the veſſel, that the precipitant may be the ſooner diffuſed through the other liquor, but then they are quickly ſurprized to find, that inſtead of haſtening the compleat precipitation, the matter already precipitated diſappears, and the ſolvent returns to be clear, or, as to ſenſe, as uniform, as it was before the precipitant was put into it. But this phænomenon does not at all croſs our theory. For, when this happens, though that part of the ſolvent, to which the precipitant reaches, is diſabled for reaſons mentioned in this diſcourſe to ſupport the diſſolved body, yet this quantity of the precipitant is but ſmall in proportion to the whole bulk of the ſolvent. And therefore, when the agitation of the veſſel diſperſes the cluſters of looſly concreted particles through the whole liquor, (which is ſeldom ſo exactly proportioned to the body it was to work on, as to be but juſt ſtrong enough to diſſolve it) that greater part of the liquor, to which before the ſhaking of the veſſel the precipitant did not reach, may well be looked upon as a freſh menſtruum, which is able to mortify or overpower the ſmall quantity of the precipitant, that is mingled with it, and ſo to deſtroy its late operation on the body diſſolved, by which means the ſolution returns, as to ſenſe, to its former ſtate. Which may be illuſtrated by a not unpleaſant experiment, I remember, I have long ſince made by precipitating a brick-coloured powder out of a ſtrong ſolution of ſublimate made in fair water. For this ſubſiding matter, being laid to dry in the philter, by which it was ſeparated from the water, would retain a deep, but ſomewhat dirty colour; and if then, putting it into the bottom of a wine glaſs, I poured upon it, either clear oil of vitriol, or ſome other ſtrong acid menſtruum, the alcalizate particles being diſabled and ſwallowed up by ſome of the acid ones of the menſtruum, the other acid ones would ſo readily diſſolve the reſidue of the powder, that in a trice the colour of it would diſappear, and the whole mixture be reduced into a clear liquor, without any ſediment at the bottom.

Thus much may ſuffice at preſent about the firſt general way of precipitating bodies out of the liquors they ſwam in.

CHAP.

CHAP. V.

THE other of the two principal ways, by which precipitations may be effected, is the difabling the folvent to fuftain the diffolved body.

THERE may be many inftances, wherein this fecond way of effecting precipitations may be affociated by nature with the firft way formerly propofed; but notwithftanding the cafes, wherein nature may (as I formerly noted) employ both the ways therein, yet in moft cafes they fufficiently differ, in regard that in the former way the fubfiding of the diffolved body is chiefly, if not only, caufed by the additional weight, as well as action of the external precipitant; whereas, in moft of the inftances of the later way, the effect is produced either without falt of tartar, or any fuch precipitant, or by fome other quality of the precipitant more than by its weight, or at leaft befides the weight it adds: though I forget not, that I lately gave an example of a fhining powder of gold, that fell to the bottom of a menftruum without the help of an external precipitant: but that was done fo flowly, that it may be difputed, whether it were a true precipitation; and I alledged it not as fuch, but to fhew, that the increafed bulk of particles may make them unfit to fwim in menftruums, wherein they fwam, whilft they were more minute. And the like anfwer may be accommodated to the precipitate *per fe* newly mentioned.

THIS premifed, I proceed now to obferve, that the general way, I laft propofed, contains in it feveral fubordinate ways, that are more particular; of which I fhall now mention the chief, that occur to me, and, though but briefly, illuftrate each of them by examples. And firft a precipitation may be made, if the faline or other diffolving particles of the menftruum are mortified or rendered unfit for their former function, by particles of a precipitant, that are of a contrary nature.

THUS gold and fome other minerals, being diffolved in aqua-regis, will be precipitated with fpirit of urine and other fuch liquors abounding with volatile and falino-fulphureous corpufcles, upon whofe account it is, that they act; whence thefe falts themfelves, though caft into a menftruum in a dry form, will ferve to make the like precipitations. And I the rather on this occafion mention urinous fpirits than falt of tartar, becaufe thofe volatile particles add much lefs of weight to the little concretions, which compofe the precipitated powder.

UPON inftances of this kind, many of the modern chemifts have built that antipathy betwixt the falts of the folvent and thofe of the menftruum, to which they afcribe almoft all precipitations. But againft this I have reprefented fomething already, and fhall partly now, and partly in the fequel of this difcourfe, add fome farther reafons of my not being fatisfied with this doctrine. For befides, that it is infufficient to reach many of the phænomena of precipitation (as will ere long be fhown) and befides that it is not eafy to make out, that there is any real antipathy betwixt inanimate bodies; I confider, firft, that fome of thofe menftruums, to which this antipathy is attributed, do, after a fhort commotion (whereby they are difpofed to make convenient occurfions and coalitions) amicably unite into concretions participating of both the ingredients; as I have fomewhere fhewn by an example purpofely devifed to make this out; to do which I dropped a clear folution of fixed nitre, inftead of the ufual one of common falt, upon a folution of filver, in aqua-fortis: for the faline particles of the folvent and thofe of the precipitant, will, as I have elfewhere recited, for the moft part friendly unite into fuch cryftals of nitre for the main, as they were obtained from: and though this notion of the chemifts, if well explained, be applicable to far more inftances than the propofers of it feemed to have thought on, and may be made good ufe of in practice; yet I take it to be ſuch as is not true univerſally, and, where it is true, ought to be explicated according to mechanical principles. For if the particles of the menſtruum and thoſe of the precipitant be ſo framed, that upon the action of the one upon the other, there will be produced corpuſcles too big and unwieldy to continue in the ſtate of fluidity, there will enſue a precipitation: but if the conſtitution of the corpuſcles of the precipitating and of the diſſolved body be ſuch, that the precipitant alſo itſelf is fit to be a menſtruum to diſſolve that body in; then, though there be an union of the ſalt of the precipitant and the metal (or other ſolutum) and perhaps of the ſolvent too, yet a precipitation will not neceſſarily follow, though the ſaline particles of the two liquors ſeemed, by the heat and ebullition excited between them upon their meeting, to exerciſe a great and mutual antipathy. To ſatisfy ſome ingenious men about this particular, I diſſolved zink or ſpeltar in a certain urinous ſpirit; (for there are more than one, that may ſerve the turn;) and then put to it a convenient quantity of a proper acid ſpirit; but though there would be a manifeſt conflict thereby occaſioned betwixt the two liquors, yet the ſpeltar remained diſſolved in the mixture. And I remember, that for the ſame purpoſe I deviſed another experiment, which is ſomewhat more eaſy and more clear. I diſſolved copper calcined *per ſe*, or even crude, in ſtrong ſpirit of ſalt; (for unleſs it be ſuch, it will not be ſo proper,) and having put to it by degrees a good quantity of ſpirit of ſal armoniac, or fermented urine, though there would be a great commotion with hiſſing and bubbles produced, the copper would not be precipitated, becauſe this urinous ſpirit will, as well as the ſalt, (and much more readily) diſſolve the ſame metal, and it would be kept diſſolved notwithſtanding their operation on one another; the intervening of which, and their action upon the metalline corpuſcles, may be gathered from hence, that the green ſolution, made with ſpirit of ſalt alone, will, by the ſupervening urinous ſpirits, be changed either into a blueiſh green, or, if the proportion of this ſpirit be very great, into a rich blue almoſt like ultramarine. And from theſe two experiments we may probably argue, that when the precipitation of a metal, &c. enſues, it is not barely on the account of the ſuppoſed antipathy betwixt the ſalts, but becauſe the cauſes of that ſeeming antipathy do likewiſe, upon a mechanical account, diſpoſe the corpuſcles of the confounded liquors ſo to cohere, as to be too unweildy for the fluid part.

CHAP. VI.

ANOTHER way, whereby the diſſolving particles of a menſtruum may be rendered unfit to ſuſtain the diſſolved body, is to preſent them another, that they can more eaſily work on.

A notable experiment of this you have in the common practice of refiners, who, to recover the ſilver out of lace, and other ſuch mixtures wherein it abounds, uſe to diſſolve it in aqua fortis, and then in the ſolution leave copper plates for a whole night (or many hours.) But if you have a mind to ſee the experiment, without waiting ſo long, you may employ the way, whereby I have often quickly diſpatched it. As ſoon then as I have diſſolved a convenient quantity, which needs not be a great one, of ſilver in cleanſed aqua-fortis, I add twenty or twenty-five times as much of either diſtilled water, or rain water; (for though common water will ſometimes do well, yet it ſeldom does ſo well;) and then into the clear ſolution, I hang by a ſtring a clean piece of copper, which will be preſently covered with little ſhining plates almoſt like ſcales of fiſh, which one may eaſily ſhake off, and make room for more. And this may illuſtrate what we formerly mentioned about the ſubſiding of metalline corpuſcles, when they convene in liquors, wherein, whilſt they were diſperſed in very minute parts, they

ſwam

ſwam freely. For, in this operation, the little ſcales of ſilver ſeemed to be purely metalline, and there is no ſaline precipitant, as ſalt of tartar or of urine, employed to make them ſubſide. Upon the ſame ground, gold and ſilver, diſſolved in their proper menſtruums, may be precipitated with running mercury; and if a ſolution of blue vitriol (ſuch as the Roman, Eaſt-Indian, or other of the like colours) be made in water, a clean plate of ſteel or iron being immerſed in it, will preſently be over-laid with a very thin caſe of copper, which, after a while, will grow thicker; but does not adhere to the iron ſo looſely as to be ſhaken off, as the precipitated ſilver newly mentioned may be from the copper-plates whereto it adheres. And that, in theſe operations, the ſaline particles may really quit the diſſolved body, and work upon the precipitant, may appear by the lately mentioned practice of refiners, where the aqua-fortis, that forſakes the particles of the ſilver, falls a working upon the copper plates employed about the precipitation, and diſſolves ſo much of them, as to acquire the greeniſh blue colour of a good ſolution of that metal. And the copper we can eaſily again, without ſalts, obtain by precipitation out of that liquor with iron, and that too, remaining diſſolved in its place, we can precipitate with the taſteleſs powder of another mineral.

BESIDES theſe two ways of weakening the menſtruum, namely, by mortifying its ſaline particles, or ſeducing them to work on other bodies, and to forſake thoſe they firſt diſſolved, there are ſome other ways of weakening the menſtruum.

A third way of effecting this, is by leſſening or diſturbing the agitation of the ſolvent. And indeed, ſince we find by experience, that ſome liquors, when they are heated, will either diſſolve ſome bodies they would not diſſolve at all when they were cold, or diſſolve them more powerfully or copiouſly when hot than cold; it is not unreaſonable to ſuppoſe, that what conſiderably leſſens that agitation of the parts of the menſtruum, that is neceſſary to the keeping the diſſolved body in the ſtate of fluidity, ſhould occaſion the falling of it again to the bottom. In ſlow operations, I could give divers examples of the precipitating power of cold; there being divers ſolutions, and particularly that of ambergreaſe, that I had kept fluid all the ſummer, which in the winter would ſubſide. And the like may be ſometimes obſerved in far leſs time in the ſolutions of brimſtone, made in certain oleaginous menſtruums; and I have now and then had ſome ſolutions, and particularly one of benzoin, made in ſpirit of wine, that would ſurprize me with the turbidneſs (which begins the ſtate of precipitation) it would acquire upon a ſudden change of the weather towards cold, though it were not in the winter ſeaſon.

ANOTHER way of weakening the menſtruum, and ſo cauſing the precipitation of a body diſſolved in it, is the diluting or leſſening the tenacity of it, whether that tenacity proceed from viſcoſity, or the competent number and conſtipation of the parts.

OF this we have an inſtance in the magiſteries (as many chemiſts are pleaſed to call them) of jalap, benzoin, and of divers others reſinous and gummous bodies diſſolved in ſpirit of wine. For, by the affuſion of common water, the menſtruum, being too much diluted, is not able to keep thoſe particles in the ſtate of fluidity, but muſt ſuffer them to ſubſide, (as they uſually do, in the form of white powder) or, (as it may happen ſometimes,) make ſome parts emerge. Examples alſo of this kind are afforded us by the common preparations of mercurius vitæ. For, though in oil of antimony, made by the rectification of the butter, the ſaline particles are ſo numerous, and keep ſo cloſe to one another, that they are able to ſuſtain the antimonial corpuſcles they carried over with them in diſtillation, and keep them together with themſelves in the form of a liquor; yet, when by the copious affuſion of the water, thoſe ſuſtaining particles are ſeparated, and removed to a diſtance from each other, the antimonial corpuſcles, and the mercurial, (if any ſuch there were) being of a ponderous nature, will eaſily ſubſide

into

into that emetic powder, which (when well washed) the chemists, flatteringly enough, call mercurius vitæ.

BUT here I must interpose an advertisement, which will help to shew us, how much precipitations depend upon the mechanical contextures of bodies. For, though not only in the newly recited examples, but in divers others, the affusion of water, by diluting the salts, and weakening the menstruum, makes the metal, or other dissolved body, fall precipitately to the bottom; yet if the saline particles of the solvent, and those of the body, be fitted for so strict an union, that the corpuscles resulting from their coalitions, will not so easily be separated by the particles of water, as suffer themselves to be carried up and down with them, whether because of the minuteness of these compounded corpuscles, or because of some congruity betwixt them and those of the water; they will not be precipitated out of the weakened solution, but still continue a part of it; as I have tried partly with some solution of silver and gold, made in acid menstruums, but much more satisfactorily in solutions of copper, made in the urinous spirit of sal armoniac. For, though that blue solution were diluted with many thousand times as much distilled water, as the dissolved metal weighed, yet its swimming corpuscles did, by their colour, manifestly appear to be dispersed through the whole liquor.

CHAP VII.

BUT, to prosecute our former discourse, which we broke off after the mention of mercurius vitæ, it will now be seasonable to add, that we have made divers other precipitations, by the bare affusion of water, out of solutions, and sometimes out of distilled liquors; which, for brevity-sake, I here omit, that I may hasten to the last way I shall now stay to mention.

ANOTHER way then, whereby precipitations of bodies may be produced by debilitating the menstruum they swim in, is by lessening the proportion of the solvent to the solutum, without any evaporation of the liquor. These last words I add, because that, when there is an obstruction, or any other expulsion of the menstruum by heat, if it be total, it is called exsiccation, as when dry salt of tartar is obtained from the filtrated lixivium of the calcined tartar; and though the evaporation be not total, yet the effects of it are not wont to be reckoned amongst precipitations. And although the way I am about to propose, if it be attentively considered, has much affinity with the foregoing, and the phænomena may, perhaps, in some sort, be reduced to them; yet the instances, that I shall name, having not, that I know, been thought of by others, and being such as every one would not deduce from what I have been mentioning, I shall add a word of the inducements I had to make the trials, as well as of the success of them.

CONSIDERING then, that water will not dissolve salts indefinitely, but when it has received its due proportion, it will then dissolve no more; but, if they be put into it, let them fall to the ground, and continue undissolved; and that if, when water is satiated, any of the liquor be evaporated, or otherwise wasted, it will, in proportion, let fall the salt it had already taken up; I conclude, that, if I could mingle with water any liquor, with which its particles would more readily associate than with those of salt, the depriving the solution of so many of its aqueous particles, would be equivalent to the evaporation of as much water, or thereabouts, as they, by being united, could compose. Wherefore, making a lixivium of distilled water, or clean rain water, and of salt of tartar, so strong, that if a grain more were cast in it, it would lie undissolved at the bottom; I put a quantity of this fiery lixivium into a slender cylindrical vessel, till it had therein reached such a height, as I thought fit; then taking as much as I thought sufficient of strong spirit of wine, that would burn every drop away, that so it it might have no phlegm nor water of its own, I poured this upon the faline folution; and fhaking the liquors pretty well together, to bring them to mix as well as I could, I laid the tube in a quiet place, and afterwards found, as I expected, that there was a pretty quantity of white falt of tartar fallen to the bottom of the veffel, which falt had been merely forfaken by the aqueous particles, that fuftained it before, but forfook it to pafs into the fpirit of wine, wherewith they were more difpofed to affociate themfelves; which I concluded, becaufe having, before I poured on this laft named liquor, made a mark on the glafs, to fhew how far the lixivium reached, I found, (what I looked for) that, after the precipitation, the lixivium, that remained yet ftrong enough to continue unmixed with the incumbent fpirit, had its furface, not where the mark fhewed it had been before, but a confiderable diftance beneath it, the fpirit of wine having gained in extent what it loft in ftrength, by receiving fo many aqueous particles into it. I chofe to make this trial, rather with a lixivium of falt of tartar, than with oil of tartar per deliquium, becaufe, in this laft named liquor, the aqueous and faline particles are more clofely combined, and therefore more difficult to be feparated, than I thought they would be in a lixivium haftily made, though very ftrong. And though, by much agitation, I have fometimes obtained fome falt of tartar from the above-mentioned oil; yet the experiment fucceeded nothing near fo well with that liquor, as with a lixivium.

I made alfo the like trial with exceedingly dephlegmed fpirit of wine, and as ftrong a brine as I could make of common falt diffolved, without heat, in common water; and I thereby obtained no defpicable proportion of finely figured falt, that was let fall to the bottom. But this experiment, to be fuccefsful, requires greater care in him, that makes it, than the former needs.

To confirm, and fomewhat to vary this way of precipitation, I fhall add, that having made a clear folution of choice gum Arabic in common water, and poured upon it a little high rectified fpirit of wine; on this occafion there was alfo made, and that in a trice, a copious precipitation of a light and purely white fubftance, not unpleafant to behold. And, for further confirmation, I diffolved a full proportion of myrrh in fair water, and into the filtrated folution, which was tranfparent, but of a high brown colour, I dropped a large proportion (which circumftance is not to be omitted) of carefully dephlegmed fpirit of wine, which, according to expectation, made a copious precipitate of the gum. And thefe inftances I the rather fet down in this place, becaufe they feem to fhew, that fimple water is a real menftruum, which may have its diffolving and fuftaining virtue weakened by the acceffion of liquors, that are not doubted to be much ftronger than it.

By fpecifying the hitherto mentioned ways, whereby precipitations may be mechanically performed and accounted for, I would by no means be thought to deny, that there may be fome omitted here, which either others, that fhall confider the matter with more attention, or I myfelf, if I fhall have leifure to do it, may think on. For I propofe thefe but as the chief, that occur to my prefent thoughts; and I forbear to add more inftances to exemplify them, becaufe I would not injure fome of my other papers, that have a greater right to thofe inftances. Only this I fhall note in general, that the doctrine and hiftory of precipitations, if well delivered, will be a thing of more extent and moment, than feems hitherto to have been imagined; fince not only feveral of the changes in the blood, and other liquors and juices of the human body, may thereby be the better underftood; and they prevented, or their ill confequences remedied; but in the practical part of mineralogy, divers ufeful things may probably be performed by the affiftance of fuch a doctrine and hiftory. To keep which conjecture from feeming extravagant, I fhall only here intimate, that it is not alone in bodies, that are naturally or permanently liquid, but in thoſe ſolid and ponderous bodies, that are for a ſhort time made ſo by the violence of the fire, that many of the things ſuggeſted by this doctrine may have place. For whilſt divers of thoſe bodies are in fuſion, they may be treated as liquors; and metals, and perhaps other heterogeneous bodies, may be obtained from them by fit, though dry precipitants, as in ſome other writings I partly did, and may elſewhere yet further declare.

EXPERIMENTS and NOTES

ABOUT THE

MECHANICAL PRODUCTION of MAGNETISM.

ADVERTISEMENT concerning the following NOTES about OCCULT QUALITIES.

THE following paper (about magnetiſm and electricity) would appear with leſs diſadvantage, if the author's willingneſs and promiſe, that this tome ſhould be furniſhed with notes about ſome occult qualities, as well as about divers ſorts of thoſe, that are preſumed to be manifeſt, did not prevail with him to let the enſuing notes appear, without thoſe about the pores of bodies and figures of corpuſcles, that ſhould have preceded them; and ſome others, that ſhonld have accompanied them. But the author choſe rather to venture theſe papers abroad in the condition, ſuch as it is, they now appear in, than make thoſe already printed about manifeſt quantities ſtay longer for acceſſions, which ſome troubleſome accidents will not ſuffer him to haſten to the preſs; and without which, he now fears this tome may ſwell to a more than competent bulk.

EXPERIMENTS and NOTES about the Mechanical Production of MAGNETICAL QUALITIES.

THOUGH the virtues of the load-ſtone be none of the leaſt famous of occult qualities, and are perhaps the moſt juſtly admired; yet I ſhall venture to offer ſomething to make it probable, that ſome, even of theſe, may be introduced into bodies by the production of mechanical changes in them.

To make way for what I am to deliver to this purpoſe, it will be expedient to remove that general and ſettled prejudice, that has kept men from ſo much as thinking of any mechanical account of magnetiſms, which is a belief, that theſe qualities do immediately flow from the ſubſtantial form of the loadſtone, whoſe abſtruſe nature is diſproportionate to our underſtandings.

EXPERIMENT I.

BUT for my part, I confeſs, I ſee no neceſſity of admitting this ſuppoſition; for I ſee, that a piece of ſteel fitly ſhaped and well excited will, like a loadſtone, have its determinate poles, and with them point at the north and ſouth; it will draw other pieces of iron and ſteel to it, and, which is more, communicate to them the ſame kind, though not degree, of attractive and directive virtue it had itſelf, and will poſſeſs theſe faculties, not as light and tranfient impreffions, but as fuch fettled and durable powers, that it may retain them for many years, if the loadftone, to which it has been duly applied, were vigorous enough; of which fort I remember I have feen one (and made fome trials with it) that yielded an income to the owner, who received money from navigators and others, for fuffering them to touch their needles, fwords, knives, &c. at his excellent magnet. Now, in a piece of fteel or iron thus excited, it is plain, that the magnetic operations may be regularly performed for whole years by a body, to which the form of a loadftone does not belong, fince, as it had its own form before, fo it retains the fame ftill, continuing as malleable, fufible, &c. as an ordinary piece of the fame metal unexcited: fo that, if there be introduced a fit difpofition into the internal parts of the metal by the action of the loadftone, the metal, continuing of the fame fpecies it was before, will need nothing, fave the continuance of that acquired difpofition to be capable of performing magnetical operations; and if this difpofition or internal conftitution of the excited iron be deftroyed, though the form of the metal be not at all injured, yet the former power of attraction fhall be abolifhed, as appears,

EXPERIMENT II.

WHEN an excited iron is made red hot in the fire, and fuffered to cool again.

AND here give me leave to take notice of what I have elfewhere related to another purpofe, namely,

EXPERIMENT III.

THAT a loadftone may, as I have more than once tried, be eafily deprived by ignition of its power of fenfibly attracting martial bodies, and yet be fcarce, if at all, vifibly changed, but continue a true loadftone in other capacities, which, according to the vulgar philofophy ought to depend upon its fubftantial form, and the loadftone thus fpoiled may, notwithftanding this form, have its poles altered at pleafure like a piece of iron; as I have elfewhere particularly declared.

AND I will confirm what I have been faying with an experiment, that you do not perhaps expect; namely, that though it be generally taken for granted (without being contradicted, that I know of, by any man) that, in a found loadftone, that has never been injured by the fire, not only the attractive power, but the particular virtue, that it has to point conftantly, when left to itfelf, with one of its determinate extremes to one determinate pole, flows immediately from the fubftantial, or at leaft effential form; yet this form remaining undeftroyed by fire, the poles may be changed, and that with eafe and fpeed. For among my notes about magnetical experiments, whence I borrow fome paffages of this paper, I find the following account.

EXPERIMENT IV.

To fhew, that the virtue, that a loadftone hath by this determinate pole or extreme to attract, for example, the fouth-end of a poifed needle, and with the oppofite extreme or pole the north-end of the fame needle, I made, among other trials, the following experiment.

TAKING a very fmall fragment of a loadftone, I found, agreeably to my conjecture, that by applying fometimes one pole, fometimes the other, to that pole of (a fmall, but) a very vigorous loadftone, that was fit for my purpofe, I could at pleafure, in a few minutes, change the poles of the little fragment, as I tried by its operations upon a needle freely poifed; though by applying a fragment a pretty deal bigger, (for in itfelf it appeared very fmall,) I was not able, in far more hours than I employed minutes before, to make any fenfible change of the poles.

THIS

THIS short memorial being added to the preceding part of this discourse, will, I hope, satisfy you, that how unanimously so ever men have deduced all magnetick operations from the form of the loadstone, yet some internal change of pores, or some other mechanical alterations, or inward disposition, either of the excited iron, or of the loadstone itself, may suffice to make a body capable or uncapable of exercising some determinate magnetical operations; which may invite you to cast a more unprejudiced eye upon those few particulars, I shall now subjoin, to make it probable, that even magnetical qualities may be mechanically produced or altered.

EXPERIMENT V.

I have often observed in the shops of artificers, as smiths, turners of metals, *&c.* that, when hardened and well tempered tools are well heated by attrition, if, whilst they are thus warned, you apply them to filings or chips, as they call them, or thin fragments of steel or iron, they will take them up, as if the instruments were touched with a loadstone: but as they will not do so, unless they be thus excited by rubbing till they be warmed, by which means a greater commotion is made in the inner parts of the steel, so neither would they retain so vigorous a magnetism, as to support the little fragments of steel, that stuck to them after they were grown cold again. Which may be confirmed by what, if I much mis-remember not, I shewed some acquaintances of yours:

EXPERIMENT VI.

WHICH was, that, by barely rubbing a conveniently shaped piece of steel against the floor, till it had gained a sufficient heat, it would, whilst it continued so, discover a manifest, though but faint attractive power, which vanished together with the adventitious heat.

EXPERIMENT VII.

WE elsewhere observe, which perhaps you also may have done, that the iron bars of windows, by having stood very long in an erected posture, may, at length, grow magnetical, so that, if you apply the north point of a poised and excited needle to the bottom of the bar, it will drive it away, and attract the southern; and if you raise the magnetic needle to the upper part of the bar, and apply it as before, this will draw the northen extreme, which the other end of the bar expelled; probably because, as it is elsewhere declared, the bar is in tract of time, by the continual action of the magnetical effluvia of the terraqueous globe, turned into a kind of magnet, whose lower end becomes the north pole of it, and the other the southern. Therefore, according to the magnetical laws, the former must expel the northern extreme of the needle, and the latter draw it.

EXPERIMENT VIII.

I HAVE found indeed, and I question not but other observers may have done so too, that if a bar of iron, that has not stood long in an erected posture, be but held perpendicular, the forementioned experiment will succeed (probably upon such an account as that I lately intimated:) but then this virtue, displayed by the extremes of the bar of iron, will not be at all permanent, but so transient, that if the bar be but inverted and held again upright, that end, which just before was the uppermost, and drew the north end of the needle, will now, being lowermost, drive it away, which, as was lately observed, will not happen to a bar, which has been some years, or other competent time, kept in the same position. So that, since length of time is requisite to make the verticity of a bar of iron so durable and constant, that the same extreme will have the same virtues in reference to the magnetical needle, whether you make it the

the upper end or the lower end of the bar, it ſeems not improbable to me, that by length of time the whole magnetick virtue of this iron may be encreaſed, and conſequently ſome degree of attraction acquired.

AND by this conſideration I ſhall endeavour to explicate that ſtrange thing, that is reported by ſome moderns to have happened in *Italy*, where a bar of iron is affirmed to have been converted into a loadſtone, whereof a piece was kept, among other rarities, in the curious *Aldrovandus*'s *Muſæum Metallicum*. For conſidering the greatneſs of its ſpecific gravity, the malleableneſs and other properties, wherein iron differs from loadſtone, I cannot eaſily believe, that by ſuch a way, as is mentioned, a metal ſhould be turned into a ſtone. And therefore, having conſulted the book itſelf, whence this relation was borrowed, I found the ſtory imperfectly enough delivered; the chiefeſt and cleareſt thing in it being, that at the top of the church of *Arimini* a great iron bar, that was placed there to ſupport a croſs of an hundred pound weight, was at length turned into a loadſtone. But whether the reality of this tranſmutation was examined, and how it appeared, that the fragment of the loadſtone preſented to *Aldrovandus* was taken from that bar of iron, I am not fully ſatisfied by that narrative. Therefore, when I remember the great reſemblance I have ſometimes ſeen in colour, beſides other manifeſt qualities, betwixt ſome loadſtones and ſome coarſe or almoſt ruſty iron, I am tempted to conjecture, that thoſe that obſerved this iron bar, when broken, to have acquired a ſtrong magnetical virtue, which they dreamed not, that tract of time might communicate to it, might eaſily be perſuaded, by this virtue and the reſemblance of colour, that the iron was turned into loadſtone: eſpecially they being prepoſſeſſed with that Ariſtotelian maxim, whence our author would explain this ſtrange phænomenon, that *inter ſymbolum habentia facilis eſt tranſmutatio*.

BUT leaving this as a bare conjecture, we may take notice, that what virtue an oblong piece of iron may need a long tract of time to acquire, by the help only of its poſition, may be imparted to it in a very ſhort time, by the intervention of ſuch a nimble agent as the fire.

EXPERIMENT IX.

As may be often, though not always, obſerved in tongs, and ſuch like iron utenſils, that, having been ignited, have been ſet to cool, leaning againſt ſome wall or other prop, that kept them in an erected poſture, which makes it probable, that the great commotion of the parts, made by the vehement heat of the fire, diſpoſed the iron, whilſt it was yet ſoft, and had its pores more lax, and parts more pliable, diſpoſed it, I ſay, to receive much quicker impreſſions from the magnetical effluvia of the earth, than it would have done, if it had ſtill been cold.

EXPERIMENT X.

AND it is very obſervable to our preſent purpoſe, what differing effects are produced by the operation of the fire, upon two magnetic bodies, according to their reſpective conſtitutions. For, by keeping a loadſtone red-hot, though you cool it afterwards in a perpendicular poſture, you may deprive it of its former power of manifeſtly attracting: but a bar of iron being ignited, and ſet to cool perpendicularly, does thereby acquire a manifeſt verticity. Of which differing events I muſt not now ſtay to enquire, whether or no the true reaſon be, that the peculiar texture, or internal conſtitution, that makes a loadſtone ſomewhat more than an ordinary ore of iron, (which metal, as far as I have tried, is the uſual ingredient of loadſtones) being ſpoiled by the violence of the fire, this rude agent leaves it in the condition of common iron, or, perhaps, of ignited iron-ore: whereas the fire does ſoften the iron itſelf, (which is a metal, not an ore) agitating its parts, and making them the more flexible, and by relaxing its pores, diſpoſes it to be eaſily and plentifully pervaded by the magnetical ſteams of the earth, from which it may not improbably be thought to receive the verticity it acquires; and this the rather, becauſe, as I have often tried, and elſewhere mentioned,

EXPERIMENT XI.

If an oblong loadſtone, once ſpoiled by the fire, be thoroughly ignited and cooled, either perpendicularly, or lying horizontally north and ſouth, it will, as well as a piece of iron handled after the ſame manner, be made to acquire new poles, or change the old ones, as the ſkilful experimenter pleaſes. But whatever be the true cauſe of the diſparity of the fire's operation upon a ſound loadſtone and a bar of iron, the effect ſeems to ſtrengthen our conjecture, that magnetical operations may much depend upon mechanical principles. And I hope you will find further probability added to it, by ſome phænomena recited in another paper, to which I once committed ſome promiſcuous experiments and obſervations magnetical.

EXPERIMENT XII.

If I may be allowed to borrow an experiment from a little tract *, that yet lies by me, and has been ſeen but by two or three friends, it may be added to the inſtances already given about the production of magnetiſm. For in that experiment I have ſhewn, how having brought a good piece of a certain kind of Engliſh oker, which yet, perhaps, was no fitter than other, to a convenient ſhape, though, till it was altered by the fire, it diſcovered no magnetical quality; yet, after it had been kept red-hot in the fire, and was ſuffered to cool in a convenient poſture, it was enabled to exerciſe magnetical operations upon a poiſed needle.

EXPERIMENT XIII.

As for the abolition of the magnetical virtue in a body endowed with it, it may be made without deſtroying the ſubſtantial, or the eſſential form of the body, and without ſenſibly adding, diminiſhing, or altering any thing, in reference to the ſalt, ſulphur, and mercury, which chemiſts preſume iron and ſteel, as well as other mixed bodies, to be compoſed of. For it has been ſometimes obſerved, that the bare continuance of a loadſtone itſelf, in a contrary poſition to that, which, when freely placed, it ſeems to effect, has either corrupted, or ſenſibly leſſened the virtue of it. What I formerly obſerved to this purpoſe, I elſewhere relate, and ſince that, having a loadſtone, whoſe vigour was looked upon, by ſkilful perſons, as very extraordinary, and which, whilſt it was in an artificer's hand, was therefore held at a high rate, I was careful, being by ſome occaſions called out of *London*, to lock it up, with ſome other rarities, in a cabinet, whereof I took the key along with me, and ſtill kept it in my own pocket. But my ſtay abroad proving much longer than I expected, when, being returned to *London*, I had occaſion to make uſe of this loadſtone for an experiment, I found it indeed where I left it, but ſo exceedingly decayed, as to its attractive power, which I had formerly examined by weight, by having lain almoſt a year in an inconvenient poſture, that if it had not been for the circumſtances newly related, I ſhould have concluded, that ſome body had purpoſely got it out in my abſence, and ſpoiled it by help of the fire, the virtue being ſo much impaired, that I cared little to employ it any more about conſiderable experiments.

EXPE-

* Relating to the magnetiſm of the earth.

EXPERIMENT XIV.

AND this corruption of the magnetical virtue, which may, in tract of time, be made in a loadſtone itſelf, may, in a trice, be made by the help of that ſtone in an excited needle. For, it is obſerved by magnetical writers, and my own trials, purpoſely made, have aſſured me of it, that a well-poiſed needle, being, by the touch of a good loadſtone, excited and brought to turn one of its ends to the north, and the other to the ſouth, it may, by a contrary touch of the ſame loadſtone, be deprived of the faculty it had of directing its determinate extremes to determinate poles. Nay, by another touch (or the ſame, and even without immediate contact, if the magnet be vigorous enough) the needle may preſently have its direction ſo changed, that the end, which formerly pointed to the north pole, ſhall now regard the ſouth, and the other end ſhall, inſtead of the ſouthern, reſpect the northern pole.

EXPERIMENT XV.

AND to make it the more probable, that the change of the magnetiſm, communicated to iron, may be produced, at leaſt, in good part, by mechanical operations, procuring ſome change of texture in the iron; I ſhall ſubjoin a notable experiment of the ingenious Dr. *Power*, which, when I heard of, I tried as well as I could; and though, perhaps for want of conveniency, I could not make it fully anſwer what it promiſed, yet the ſucceſs of the trial was conſiderable enough to make it pertinent in this place, and to induce me to think, it might yet better ſucceed with him, whoſe experiment, as far as it concerns my preſent purpoſe, imports, that if a puncheon, as ſmiths call it, or a rod of iron, be, by being ignited and ſuffered to cool north and ſouth, and hammered at the ends, very manifeſty endowed with magnetical virtue, this virtue will, in a trice be deſtroyed, by two or three ſmart blows of a ſtrong hammer, upon the middle of the oblong piece of iron.

BUT magnetiſm is ſo fertile a ſubject, that, if I had now the leiſure and conveniency to range among magnetical writers, I ſhould ſcarce doubt of finding, among their many experiments and obſervations, divers, that might be added to thoſe above delivered, as being eaſily applicable to my preſent argument. And I hope you will find farther probability added to what has been ſaid, to ſhew, "that magnetical operations "may much depend upon mechanical principles," by ſome phænomena recited in another paper, to which I once committed ſome promiſcuous experiments and obſervations magnetical.

EXPERIMENTS and NOTES

ABOUT THE

Mechanical Origin or Production of ELECTRICITY.

THAT it is not neceſſary to believe electrical attraction (which, you know, is generally liſted among occult qualities) to be the effect of a naked and ſolitary quality, flowing immediately from a ſubſtantial form; but that it may rather be the effect of a material effluvium, iſſuing from, and returning to the electrical body (and perhaps in ſome caſes aſſiſted in its operation by the external air) ſeems agreeable to divers things, that may be obſerved in ſuch bodies and their manner of acting.

There are differing hypotheſes (and all of them mechanical, propoſed by the moderns) to ſolve the phænomena of electrical attraction. Of theſe opinions the firſt is, that of the learned Jeſuit *Cabæus*, who, though a Peripatetick and commentator on *Ariſtotle*, thinks the drawing of light bodies by jet, amber, &c. may be accounted for, by ſuppoſing, that the ſteams, that iſſue, or, if I may ſo ſpeak, ſally out of amber, when heated by rubbing, diſcuſs and expel the neighbouring air; which after it has been driven off a little way, makes as it were a ſmall whirlwind, becauſe of the reſiſtance it finds from the remoter air, which has not been wrought on by the electrical ſteams; and that theſe, ſhrinking back ſwiftly enough to the amber, do in their returns bring along with them ſuch light bodies, as they meet with in their way. On occaſion of which hypotheſis I ſhall offer it to be conſidered, whether by the gravity of the atmoſpherical air, ſurmounting the ſpecifick gravity of the little and rarified atmoſphere, made about the amber by its emiſſions, and compriſing the light body faſtened on by them, the attraction may not in divers caſes be either cauſed or promoted.

Another hypotheſis is that propoſed by that ingenious gentleman, Sir *Kenelm Digby*, and embraced by the very learned Dr. *Browne*, (who ſeems to make our *Gilbert* himſelf to have been of it) and divers other ſagacious men. And, according to this hypotheſis, the amber, or other electrick, being chafed or heated, is made to emit certain rays or files of unctuous ſteams, which, when they come to be a little cooled by the external air, are ſomewhat condenſed, and having loſt of their former agitation, ſhrink back to the body, whence they ſallied out, and carry with them thoſe light bodies, that their further ends happen to adhere to, at the time of their retraction: as when a drop of oil or ſyrup hangs from the end of a ſmall ſtick, if that be dextrouſly and cautiouſly ſtruck, the viſcous ſubſtance will, by that impulſe, be ſtretched out, and preſently retreating, will bring along with it the duſt or other light bodies, that chanced to ſtick to the remoter parts of it.

And this way of explaining electrical attractions is employed alſo by the learned *Gaſſendus*, who adds to it, that theſe electrical rays, if they may be ſo called, being emitted ſeveral ways, and conſequently croſſing one another, get into the pores of the ſtraw, or other light body to be attracted, and by means of their decuſſation, take the faſter hold of it, and have the greater force to carry it along with them, when they ſhrink back to the amber, whence they were emitted.

A third hypotheſis there is, which was deviſed by the acute *Carteſius*, who diſlikes the explications of others, chiefly becauſe he thinks them not applicable to glaſs, which he ſuppoſes unfit to ſend forth effluvia, and which is yet an electrical body; and therefore attempts to account for electrical attractions by the intervention of certain particles, ſhaped almoſt like ſmall pieces of ribband, which he ſuppoſes to be formed of this ſubtile matter harboured in the pores or creviſes of glaſs. But this hypotheſis, though ingenious in itſelf, yet depending upon the knowledge of divers of his peculiar principles, I cannot intelligibly propoſe it in few words, and therefore ſhall refer you to himſelf for an account of it: which I the leſs ſcruple to do, becauſe though it be not unworthy of the wonted acuteneſs of the author, yet he ſeems himſelf to doubt, whether it will reach all electrical bodies; and it ſeems to me, that the reaſon, why he rejects the way of explicating attraction by the emiſſion of the finer parts of the attrahent (to which hypotheſis, if it be rightly propoſed, I confeſs myſelf very inclinable) is grounded upon a miſtake, which, though a philoſopher may, for want of experience in that particular, without diſparagement fall into, is nevertheleſs a miſtake. For whereas our excellent author ſays, that electrical effluvia, ſuch as are ſuppoſed to be emitted

Princip. part IV. *art.* 184.

emitted by amber, wax, &c. cannot be imagined to proceed from glaſs, I grant the ſuppoſition to be plauſible, but cannot allow it to be true. For as ſolid a body as glaſs is, yet if you but dexterouſly rub for two or three minutes a couple of pieces of glaſs againſt one another, you will find, that glaſs is not only capable of emitting effluvia, but ſuch ones, as to be odorous, and ſometimes to be rankly ſtinking.

But it is not neceſſary, that in this paper, where I pretend not to write diſcourſes but notes, I ſhould conſider all, that has been, or I think may be, ſaid for and againſt each of the above-mentioned hypotheſes; ſince they all agree in what is ſufficient for my preſent purpoſe, namely, that electrical attractions are not the effects of a mere quality, but of a ſubſtantial emanation from the attracting body; and it is plain, that they all endeavour to ſolve the phænomena in a mechanical way, without recurring to ſubſtantial forms, and inexplicable qualities, or ſo much as taking notice of the hypoſtatical principles of the chemiſts. Wherefore it may ſuffice in this place, that I mention ſome phænomena, that in general make it probable, that amber, &c. draws ſuch light bodies, as pieces of ſtraw, hair, and the like, by virtue of ſome mechanical affections either of the attracting, or of the attracted bodies, or of both the one and the other.

1. The firſt and moſt general obſervation is, that electrical bodies draw not, unleſs they be warmed; which rule, though I have now and then found to admit of an exception, (whereof I elſewhere offer an account,) yet as to the generality of common electricks, it holds well enough to give much countenance to our doctrine, which teaches the effects of electrical bodies to be performed by corporeal emanations. For it is known, that heat, by agitating the parts of a fit body, ſolicits it as it were to ſend forth its effluvia, as is obvious in odoriferous gums and perfumes, which, being heated, ſend forth their fragrant ſteams, both further and more copiouſly, than otherwiſe they would.

2. Next, it has been obſerved, that amber, &c. warmed by the fire, does not attract ſo vigorouſly, as if it acquire an equal degree of heat by being chafed or rubbed: ſo that the modification of motion in the internal parts, and in the emanations of the amber, may, as well as the degree of it, contribute to the attraction. And my particular obſervations incline me to add, that the effect may oftentimes be much promoted, by employing both theſe ways ſucceſſively; as I thought I manifeſtly found, when I firſt warmed the amber at the fire, and preſently after chafed it a little upon a piece of cloth. For then a very few rubbings ſeemed to excite it more than many more would otherwiſe have done: as if the heat of the fire had put the parts into a general, but confuſed agitation; to which it was eaſy for the ſubſequent attrition (or reciprocation of preſſure) to give a convenient modification in a body, whoſe texture diſpoſed it to become vigorouſly electrical.

3. Another obſervation, that is made about theſe bodies, is, that they require terſion as well as attrition; and though I doubt whether the rule be infallible, yet I deny not, but that weaker electricks require to be as well wiped as chafed; and even good ones will have their operation promoted by the ſame means. And this is very agreeable to our doctrine, ſince terſion, beſides that it is, as I have ſometimes manifeſtly known it, a kind or degree of attrition, frees the ſurface from thoſe adherences, that might choak the pores of the amber, or at leaſt hinder the emanation of the ſteams to be ſo free and copious, as otherwiſe they would be.

4. It is likewiſe obſerved, that whereas the magnetical ſteams are ſo ſubtle, that they penetrate and perform their operation through all kind of mediums hitherto known to us; electrical ſteams are like thoſe of ſome odoriferous bodies, eaſily checked in their progreſs, ſince it is affirmed by learned writers, who ſay they ſpeak

upon particular trial, that the interposition of the finest linnen, or sarsnet, is sufficient to hinder all the operation of excited amber upon a straw or feather placed never so little beyond it.

5. It has been also observed, that the effects of electrical attraction are weakened, if the air be thick and cloudy; and especially if the south wind blows; and that electricks display their virtue more faintly by night than by day, and more vigorously in clear weather, and when the winds are northerly. All which the learned *Kircherus* asserts himself to have found true by experience; insomuch, that those bodies, that are but faintly drawn, when the weather is clear, will not, when it is thick and cloudy, be at all moved.

6. We have also observed, that divers concretes, that are notably electrical, do abound in an effluviable matter, (if I may so call it) which is capable of being manifestly evaporated by heat and rubbing. Thus we see, that most resinous gums, that draw light bodies, do also, being moderately sollicited by heat, (whether this be excited by the fire, or by attrition or contusion) emit steams. And in pieces of sulphur conveniently shaped, I found, upon due attrition, a sulphureous stink. And that piece of amber, which I most employ, being somewhat large, and very well polished, will, being rubbed upon a piece of woollen cloth, emit steams, which the nostrils themselves may perceive; and they sometimes seem to me not unlike those, that I took notice of, when I kept in my mouth a drop or two of the diluted tincture (or solution of the finer parts) of amber made with spirit of wine, or of sal armoniac.

7. It agrees very well with what has been said of the corporeal emanations of amber, that its attractive power will continue some time after it has been once excited. For the attrition having caused an intestine commotion in the parts of the concrete, the heat or warmth, that is thereby excited, ought not to cease, as soon as ever the rubbing is over, but to continue capable of emitting effluvia for some time afterwards, which will be longer or shorter, according to the goodness of the electric, and the degree of the antecedent commotion: which, joined together, may sometimes make the effect considerable, insomuch that in a warm day, about noon, I did, with a certain body, not much, if at all, bigger than a pea, but very vigorously attractive, move to and fro a steel needle, freely poised, about three minutes (or the twentieth part of an hour) after I had left off rubbing the attrahent.

8. That it may not seem impossible, that electrical effluvia should be able to insinuate themselves into the pores of many other bodies, I shall add, that I found them subtile enough to attract not only spirit of wine, but that fluid aggregate of corpuscles we call smoke. For having well lighted a wax taper, which I preferred to a common candle, to avoid the stink of the snuff, I blew out the flame; and, when the smoke ascended in a slender stream, held, at a convenient distance from it, an excited piece of amber, or a chafed diamond, which would manifestly make the ascending smoke deviate from its former line, and turn aside, to beat, as it were, against the electric, which, if it were vigorous, would act at a considerable distance, and seemed to smoke for a pretty while together.

9. That it is not in any peculiar sympathy between an electric and a body, whereon it operates, that electrical attraction depends, seems the more probable, because amber, for instance, does not attract only one determinate sort of bodies, as the loadstone does iron, and those bodies, wherein it abounds; but, as far as I have yet tried, it draws indifferently all bodies whatsoever, being placed within a due distance from it, (as my choicest piece of amber draws not only sand and mineral powders, but filings of steel and copper, and beaten gold itself) provided they be minute or light enough, except, perhaps, it be fire: I employ the word perhaps, because I am not yet so clear in this point.

 For

For having applied a ſtrong electric, at a convenient diſtance, to ſmall fragments of ignited matter, they were readily enough attracted, and ſhined, whilſt they were ſticking to the body, that had drawn them. But, when I looked attentively upon them, I found the ſhining ſparks to be, as it were, cloathed with light aſhes, which, in ſpite of my diligence, had been already formed about the attracted corpuſcles, upon the expiring of a good part of the fire; ſo that it remained ſomewhat doubtful to me, whether the ignited corpuſcles, whilſt they were totally ſuch, were attracted; or whether the immediate objects of the attraction were not the new formed aſhes, which carried up with them thoſe yet unextinguiſhed parts of fire, that chanced to be lodged in them. But, as for flame, our countryman *Gilbert* delivers, as his experiment, that an electric, though duly excited and applied, will not move the flame of the ſlendereſt candle; which ſome will think not ſo eaſy to be well tried with common electricks, as amber, hard wax, ſulphur, and the like unctuous concretes, that very eaſily take fire: therefore I choſe to make my trial, with a rough diamond, extraordinarily attractive, which I could, without injuring it, hold, as near as I pleaſed, to the flame of a candle, or taper; and though I was not ſatisfied, that it did either attract the flame, as it viſibly did the ſmoke, or manifeſtly agitate it; yet, granting, that *Gilbert*'s aſſertion will conſtantly hold true, and ſo, that flame is to be excepted from the general rule, yet this exception may well comport with the hypotheſis hitherto countenanced, ſince it may be ſaid, as it is, if I miſtake not, by *Kircherus*, that the heat of the flame diſſipates the effluvia, by whoſe means the attraction ſhould be performed. To which I ſhall add, that poſſibly the celerity of the motion of the flame upwards may render it very difficult, for the electrical emanations to divert the flame from its courſe.

10. We have found by experiment, that a vigorous and well excited piece of amber will draw, not only the powder of amber, but leſs minute fragments of it. And as, in many caſes, one contrary directs to another, ſo this trial ſuggeſted a further, which, in caſe of good ſucceſs, would probably argue, that, in electrical attraction, not only effluvia are emitted by the electrical body, but theſe effluvia faſten upon the body to be drawn, and that in ſuch a way, that the intervening viſcous ſtrings, which may be ſuppoſed to be made up of thoſe cohering effluvia, are, when their agitation ceaſes, contracted or made to ſhrink inwards, towards both ends, almoſt as a highly ſtretched luteſtring does, when it is permitted to retreat into ſhorter Dimenſions. But the conjecture itſelf was much more eaſy to be made, than the experiment requiſite to examine it. For we found it no eaſy matter to ſuſpend an electric, great and vigorous enough, in ſuch a manner, that it might, whilſt ſuſpended, be excited, and be ſo nicely poiſed, that ſo faint a force as that, wherewith it attracts light bodies, ſhould be able to procure a local motion to the whole body itſelf. But after ſome fruitleſs attempts with other electricks, I had recourſe to the very vigorous piece of poliſhed amber, formerly mentioned; and when we had, with the help of a little wax, ſuſpended it by a ſilken thread, we chafed very well one of the blunt edges of it upon a kind of large pincuſhion, covered with a coarſe and black woollen ſtuff, and then brought the electric, as ſoon as we could, to ſettle, notwithſtanding its hanging freely at the bottom of the ſtring. This courſe of rubbing on the edge of the amber we pitched upon for more than one reaſon; for if we had chafed the flat ſide, the amber could not have approached the body it had been rubbed on, without making a change of place in the whole electric; and, which is worſe, without making it move (contrary to the nature of heavy bodies) ſomewhat upwards; whereas the amber having, by reaſon of its ſuſpenſion, its parts counterpoiſed by one another, to make the excited edge approach to another body, that edge needed not all aſcend, but only be moved horizontally, to which way of moving the gravity of the electric (which the ſtring kept from moving downwards)

downwards) could be but little or no hinderance. And, agreeably to this, we found, that if, as ſoon as the ſuſpended and well rubbed electric was brought to ſettle freely, we applied to the chafed edge, but without touching it, the lately mentioned cuſhion, which, by reaſon of its rough ſuperficies and poroſity, was fit for the electrical effluvia to faſten upon, the edge would manifeſtly be drawn aſide by the cuſhion ſteadily held, and if this were ſlowly removed, would follow it a good way; and when this body no longer detained it, would return to the poſture, wherein it had ſettled before. And this power of approaching the cuſhion, by virtue of the operation of its own ſteams, was ſo durable in our vigorous piece of amber, that, by once chafing it, I was able to make it follow the cuſhion no leſs than ten or eleven times. Whether from ſuch experiments one may argue, that it is but, as it were, by accident, that amber attracts another body, and not this the amber; and whether theſe ought to make us queſtion, if electricks may, with ſo much propriety, as has been hitherto generally ſuppoſed, be ſaid to attract, are doubts, that my deſign does not here oblige me to examine.

SOME other phænomena might be added of the ſame tendency with thoſe already mentioned, (as the advantage, that electrical bodies uſually get by having well poliſhed, or, at leaſt, ſmooth ſurfaces;) but the title of this paper promiſing ſome experiments about the production of electricity, I muſt not omit to recite, how I have been ſometimes able to produce or deſtroy this quality in certain bodies, by means of alterations, that appeared not to be other than mechanical.

EXPERIMENT I.

AND firſt, having, with a very mild heat, ſlowly evaporated about a fourth part of good turpentine, I found, that the remaining body would not, when cold, continue a liquor, but hardened into a tranſparent gum almoſt like amber, which, as I looked for, proved electrical.

EXPERIMENT II.

SECONLY, by mixing two ſuch liquid bodies, as petroleum and ſtrong ſpirit of nitre, in a certain proportion, and then diſtilling them till there remained a dry maſs, I obtained a brittle ſubſtance as black as jet; and whoſe ſuperficies (where it was contiguous to the retort) was gloſſy, like that mineral, when poliſhed; and, as I expected, I found it alſo to reſemble jet, in being endowed with an electrical faculty.

EXPERIMENT III.

THIRDLY, having burnt antimony to aſhes, and of thoſe aſhes, without any addition, made a tranſparent glaſs, I found, that, when rubbed, as electrical bodies ought to be to excite them, it anſwered my expectation, by manifeſting a not inconſiderable electricity. And this is the worthier of notice, becauſe, that as a vitrum antimonii, that is ſaid to be purer than ordinary, may be made of regulus of the ſame mineral, in whoſe preparation you know a great part of the antimonial ſulphur is ſeparated, and left among the ſcoriæ; ſo glaſs of antimony, made without additament, may eaſily, as experience has informed us, be in part reduced to a regulus, (a body not reckoned amongſt electrical ones.) And that you may not think, that it is only ſome peculiar and fixed part of the antimony, that is capable of vitrification, let me aſſure you, that, even with the other part, that is wont to fly away, (namely, the flowers) an antimonial glaſs may, without an addition of other ingredients, be made.

EXPERIMENT IV.

FOURTHLY, the mention of a vitrified body brings into my mind, that I more than once made ſome glaſs of lead *per ſe*, (which I found no very eaſy work) that alſo was

not

not wholly destitute of an electrical virtue, though it had but a very languid one. And it is not here to be overlooked, that this glass might easily be brought to afford again malleable lead, which was never reckoned, that I know of, among electrical bodies.

EXPERIMENT V.

FIFTHLY, having taken some amber, and warily distilled it, not with sand or powdered brick, or some such additament, as chemists are wont to use, for fear it should boil over, or break their vessels; but by itself, that I might have an unmixed caput mortuum: having made this distillation, I say, and continued it, till it had afforded a good proportion of phlegm, spirit, volatile salt, and oil, the retort was warily broken, and the remaining matter was taken out in a lump, which, though it had quite lost its colour, being burned quite black, and though it were grown strangely brittle, in comparison of amber, so that they, who believe the virtue of attracting light bodies to flow from the substantial form of amber, would not expect it in a body so changed and deprived of its noblest parts; yet this caput mortuum was so far from having lost its electrical faculty, that it seemed to attract more vigorously than amber itself is wont to do, before it be committed to distillation.

AND from the foregoing instances afforded us by the glass of antimony, we may learn, that when the form of a body seems to be destroyed by a fiery analysis, that dissipates the parts of it, the remaining substance may yet be endowed with electricity, as the *caput mortuum* of amber may acquire it; as in the case of the glass of antimony made of the calx and of the flowers. And from the second example above-mentioned, and from common glass, which is electrical, we may also learn, that bodies, that are neither of them apart observed to be endowed with electricity, may have that virtue result in the compounded substance, that they constitute, though it be but a factitious body.

To the foregoing experiments, whose success is wont to be uniform enough, I shall add the recital of a surprising phænomenon, which, though not constant, may help to make it probable, that electrical attractions need not be supposed still to proceed from the substantial, or even from the essential form of the attrahent, but may be the effects of unheeded, and, as it were, fortuitous causes. And, however, I dare not suppress so strange an observation, and therefore shall relate that, which I had the luck to make of an odd sort of electrical attraction (as it seemed,) not taken notice of (that I know of) by any either naturalist or other writer, and it is this.

EXPERIMENT VI.

THAT false locks, as they call them, of some hair, being by curling or otherwise brought to a certain degree of dryness, or of stiffness, will be attracted by the flesh of some persons, or seem to apply themselves to it, as hair is wont to do to amber or jet excited by rubbing. Of this I had a proof in such locks worn by two very fair ladies, that you know. For at some times I observed, that they could not keep their locks from flying to their cheeks, and (though neither of them made any use, or had any need of painting) from sticking there. When one of these beauties first shewed me this experiment, I turned it into a complemental raillery, as suspecting there might be some trick in it, though I after saw the same thing happen to the others locks too. But as she is no ordinary virtuosa, she very ingeniously removed my suspicions, and, as I requested, gave me leave to satisfy myself further, by desiring her to hold her warm hand at a convenient distance from one of those locks taken off and held in the air. For as soon as she did this, the lower end of the lock, which was free, applied itself presently to her hand: which seemed the more strange, because so great a multi-

 tude

tude of hair would not have been eaſily attracted by an ordinary electrical body, that had not been conſiderably large, or extraordinarily vigorous. This repeated obſervation put me upon enquiring among ſome other young ladies, whether they had obſerved any ſuch like thing; but I found little ſatisfaction to my queſtion, except from one of them eminent for being ingenious, who told me, that ſometimes ſhe had met with theſe troubleſome locks; but that all ſhe could tell me of the circumſtances, which I would have been informed about, was, that they ſeemed to her to fly moſt to her cheeks, when they had been put into a ſomewhat ſtiff curl, and when the weather was froſty *.

You will probably be the leſs diſpoſed to believe, that electrical attractions muſt proceed from the ſubſtantial forms of the attrahents, or from the predominancy of this or that chemical principle in them, if I acquaint you with ſome odd trials, wherein the attraction of light bodies ſeemed to depend upon very ſmall circumſtances. And though forbearing at preſent, to offer you my thoughts about the cauſe of theſe ſurpriſing phænomena, I propoſe it only as a problem to yourſelf, and your curious friends, yet the main circumſtances ſeeming to be of a mechanical nature, the recital of my trials will not be impertinent to the deſign and ſubject of this paper.

EXPERIMENT VII.

I TOOK then a large and vigorous piece of amber, conveniently ſhaped for my purpoſe, and a downy feather, ſuch as grows upon the bodies, not wings or tails of a ſomewhat large chicken: then having moderately excited the electrick, I held the amber ſo near it, that the neighbouring part of the feather was drawn by it, and ſtuck faſt to it; but the remoter parts continued in their former poſture. This done, I applied my fore-finger to theſe erected downy feathers, and immediately, as I expected, they left their preceding poſture, and applied themſelves to it, as if it had been an electrical body. And whether I offered to them my nail, or the pulpy part of my finger, or held my finger towards the right hand or the left, or directly over, theſe downy feathers, that were near the little quill, did nimbly, and, for aught appeared, equally turn themſelves towards it. And to ſhew, that the ſteams, that iſſued out of ſo warm a body as my finger, were not neceſſary to attract, as men ſpeak, the above-mentioned feathers, inſtead of my finger, I applied to them, after the ſame manner, a little cylindrical inſtrument of ſilver, to which they bowed and faſtened themſelves, as they had done to my finger, though the tip of this inſtrument were preſented to them in ſeveral poſtures. The like ſucceſs I had with the end of an iron key, and the like alſo with a cold piece of poliſhed black marble; and ſometimes the feathers did ſo readily and ſtrongly faſten themſelves to theſe extraneous and unexcited bodies, that I have been able, though not eaſily, to make one of them draw the feather from the amber itſelf.

BUT

* Some years after the making the experiments about the production of electricity, having a deſire to try, whether in the attractions made by amber, the motions excited by the air had a conſiderable intereſt, or whether the effect were not due rather to the emiſſion and retraction of effluvia, which being of a viſcous nature may conſiſt of particles either branched or hooked, or otherwiſe fit for ſome kind of coheſion, and capable of being ſtretched, and of ſhrinking again, as leather thongs are: to examine this, I ſay, I thought the fitteſt way, if it were practicable, would be, to try, whether amber would draw a light body in a glaſs whence the air was pumped out. And though the trial of this ſeemed very difficult to make, and we were ſomewhat diſcouraged by our firſt attempt, wherein the weight of the ambient air broke our receiver, which chanced to prove too weak, when the internal air had been with extraordinary diligence pumped out; yet having a vigorous piece of amber, which I had cauſed to be purpoſely turned and poliſhed for electrical experiments, I afterwards repeated the trial, and found, that in warm weather, it would retain a manifeſt power of attracting for ſeveral minutes (for it ſtirred a poiſed needle after above one quarter of an hour) after we had done rubbing it. Upon which encouragement we ſuſpended it, being firſt well chafed, in a glaſs receiver, that was not great, juſt over a light body; and making haſte with our air pump to exhauſt the glaſs, when the air was withdrawn, we did by a contrivance let down the ſuſpended amber, till it came very near the ſtraw or feather, and perceived, as we expected, that in ſome trials, upon the leaſt contact it would lift it up; and in others, (for we repeated the experiment,) the amber would raiſe it without touching it, that is, would attract it.

But it is diligently to be obferved, that this unufual attraction happened only, whilft the electrical operation of the excited amber continued ftrong enough to fuftain the feathers. For afterwards, neither the approach of my finger, nor that of the other bodies, would make the downy feathers change their pofture. Yet, as foon as ever the amber was by light affriction excited again, the feather would be difpofed to apply itfelf again to the abovementioned bodies.

And left there fhould be any peculiarity in that particular feather, I made the trials, with others, (provided they were not long enough to exceed the fphere of activity of the amber) and found the experiment to anfwer my expectation.

I made the experiment alfo at differing times, and with fome months, if not rather years, of interval, but with the like fuccefs.

And left you fhould think thefe phænomena proceed from fome peculiarity in the piece of amber I employed, I fhall add, that I found uniformity enough in the fuccefs, when, in the place of amber, I fubftituted another electrick, and particularly a fmooth mafs of melted brimftone.

These are the phænomena I thought fit to mention, at prefent, of this unufual way of drawing light bodies, and with this experiment I fhould conclude my notes about electricity, but that, I think, it will not be amifs, before I take leave of this fubject, to give this advertifement, that the event of electrical experiments is not always fo certain as that of many others, being fometimes much varied by feemingly flight circumftances, and now and then by fome, that are altogether over-looked. This obfervation may receive credit from fome of the particulars above recited (efpecially concerning the intereft of the weather, &c. in electrical phænomena.) But now I fhall add, that, not only there may happen fome variations in the fuccefs of trials made with electrical bodies, but that it is not fo certain as many think, whether fome particular bodies be or be not electrical. For the inquifitive *Kircherus* reckons cryftal among thofe gems, to whom nature has denied the attractive power we are fpeaking of; and yet I remember not, that among all the trials I have made with natural cryftal, I have found any, that was deftitute of the power he refufes them. Alfo a late moft learned writer reciting the electricks, reckoned up by our induftrious countryman *Gilbert*, and increafing their number by fome obferved by himfelf, (to which I fhall now add, befides white fapphires, and white Englifh amethyfts, the almoft diaphanous fpar of lead ore) denies electricity to a couple of tranfparent gems, the cornelian and the emerald. And I do the lefs wonder he fhould do fo to the former, becaufe I have myfelf, in vain, tried to make any attraction with a piece of cornelian fo large and fair, that it was kept for a rarity; and yet with divers other fine cornelians I have been able to attract fome light bodies very manifeftly, if not brifkly; and I ufually wear a cornelian ring, that is richly enough endowed with electricity. But as for emeralds, as I thought it ftrange, that nature fhould have denied them a quality fhe has granted to fo many other diaphanous gems, and even to cryftal, fo I thought the affertion deferved an examen, upon which I concluded, that, at leaft, it does not univerfally and conftantly hold true. I had, indeed, feen in a ring a ftone of price and great luftre, which, though green, I found to be, (as I gueffed it would prove) vigoroufly enough electrical. But this experiment, though feemingly conclufive, I did not look upon as a fair trial, becaufe the ftone was not a true emerald, but, which is rare, a green fapphire. And I learned by enquiry of the fkilful jeweller, that cut it, that it was fo far from having the foftnefs of an emerald, that he found it harder than blue fapphires themfelves, which yet are gems of great hardnefs, and by fome reputed fecond to none, but diamonds. Without therefore concluding any thing from this experiment, fave that, if the affertion I was to examine were true, the want of an electrical faculty might be thought a concomitant rather of the peculiar texture of the emerald than of its green colour, I proceeded to make trial with three or four emeralds, whofe being true was not doubted, and found them all fomewhat, though not equally, endowed with electricity, which I found to be yet more confiderable in an emerald of my own, whofe colour was fo excellent, that by fkilful perfons it was looked on as a rarity. And though, by this fuccefs of my enquiry, I perceived I could not, as elfe I have done, fhew the curious a new way of judging of true and falfe emeralds, yet the like may be, though not always certain, yet oftentimes of ufe, in the eftimating whether diamonds be true or counterfeit, efpecially if, being fet in rings, the fureft way of trying them cannot conveniently be employed. For whereas glafs, though it have fome electricity, feems, as far as I have obferved, to have but a faint one, there are often found diamonds that have a very vigorous one. And I do not remember I met with any electrick of the fame bulk, that was more vigorous than a rough diamond I have, which is the fame, that I formerly mentioned to have moved a needle above three minutes after I had ceafed to chafe it. And this brings into my mind, that it has been obferved, that diamonds draw better whilft rough, than they do after they are cut and polifhed; which feeming to contradict what has been obferved by others, and by us alfo, that amber, for inftance, attracts more vigoroufly, if the furface be made very fmooth than otherwife, it induces me to conjecture, that, if this obfervation about diamonds be true, as fome of my trials have now and then inclined me to think it, and if it do not in fome cafes confiderably depend upon the lofs of the (electrical) fubftance of the ftone, by its being cut and ground, the reafon may poffibly be, that the great rapidnefs, with which the wheels, that ferve to cut and polifh diamonds muft be moved, does excite a great degree of heat, (which the fenfes may eafily difcover) in the ftone, and by that and the ftrong concuffion it makes of its parts, may force it to fpend its effluviable matter, if I may fo call it, fo plentifully, that the ftone may be impoverifhed, and perhaps alfo, on the account of fome little change in its texture, be rendered lefs difpofed to emit thofe effluvia, that are inftruments of electrical attraction. But as I willingly leave the matter of fact to further trial, fo I do the caufe of it, in cafe it prove true, to further enquiry.

New EXPERIMENTS about the fuperficial Figures of FLUIDS, efpecially of Liquors contiguous to other Liquors.

First printed in the PHILOSOPHICAL TRANSACTIONS, N°. cxxxi. p. 775.

SIR,

IN compliance with your curiofity, I herewith fend you my rude notes about the fuperficial figures of contiguous liquors, which belonging to a paper (concerning the pores and figures of bodies,) whereof they made the laft part, and being themfelves very indigefted, I fhould by no means venture to expofe them even to a lefs critical eye than yours, if I did not hope, that, though a more difcerning reader will fooner difcover their imperfections, yet he may be more inclined than an ordinary one would be to think them not ufelefs trifles; fince, if thefe trials and hints, as mean as they are, be profecuted by naturalifts, that have mathematical heads, perhaps they may conduce more to the phyfical theory of the grand fyftem of the world, than at firft one would fufpect. And that I may leave you and your ingenious friends the greater opportunity

portunity and freedom to exercise their sagacity on these phænomena, I have purposely forborn to engage in speculative discourses upon them, contenting myself to have faithfully recited matter of fact, and thereby to have sprung game for those, that have more leisure and ability to fly at it.

———WHAT has been said about the pores of liquors may be somewhat illustrated or confirmed, if I subjoin to it some of the trials I have made about the surfaces of fluids contiguous to other fluids. For this being, for ought I know, a neglected subject, and the little, that has been taken notice of about it, consisting of a few slight and casual observations, that seem to have been rather presented to us, not to say obtruded upon us, than designedly made by us; I many years ago thought, it might be worth while to spend some hours upon Experiments of this sort: which I was especially induced to do, because I think, one may probably enough suppose, that in the tract of the universe, that is yet known to us, there is not the hundredth, perhaps not the thousandth, part, that is formed into solid bodies, such as the earth, the moon, and the other planets; and consequently all the rest is made up of celestial fluids and the atmospheres of solid globes, which, for ought we know, though not manifestly differing in transparency, may be disterminated by distinct surfaces. So that, to observe and consider the effects of the congruity and incongruity, that liquors, or such fluid bodies, as directly or otherwise fall under sensible observation, have, when they are contiguous to one another, or to the surfaces of solid bodies, may not only improve what is yet known about the ascension of liquors in small pipes, but may perchance serve to illustrate the formation of those great masses of matter, of which the divine architect has framed the mundane globes, and some other considerable parts of the universe; especially if we admit the Cartesian hypothesis, *That the sun, and all the fixt stars are fluid bodies.*

THE cause, why water in narrow pipes ascends above the level of the surrounding water, having been already enquired into by some ingenious men, and particularly by Mr. *Hooke*, I shall not now discourse of that subject, nor so much as mention what I have tried about it; but shall rather take notice, that, because I suspected, that the concave figure, which may be observed in the surface of water included in slender pipes, may, at least in great part, depend upon its relation to the contiguous fluid, which, in ordinary cases, is the air; I thought fit to try, whether this concave figure would not Exp. i. be altered by substituting another liquor in the room of the air: and accordingly having procured a strongly *Alcalizate Menstruum* (I used that made of fixt nitre, dissolved by the moisture of a cellar) into a pipe of glass, sealed at one end, and not full a quarter of an inch in bore; that the cavity, which in a greater breadth would seem less deep, might be the more conspicuous; we gently poured on it some highly dephlegmed spirit of wine, which we knew would not mix with it, but swim above it; and presently, as we had guessed, we found the figure of the surface of the lower liquor changed, and the cavity quite destroyed; the surface that seemed, as it were, common to the two contiguous liquors, appearing flat or horizontal. And such a level superficies we had, by putting those two liquors together in a much wider glass.

WE found also, that by employing oil of turpentine instead of spirit of wine, the Exp. ii. liquor did almost totally lose its cavity.

BUT if, instead of deliquated tartar, we put common water into the pipe, we found Exp. iii. this liquor to retain its concave surface, though we put to it some oil of turpentine and left it to rest upon the water a good while.

IN regard that, when oil and water are put together, the oil, that has been employed to produce the phænomena, wont to be afforded by their contact, has usually been common oil, as that of olives, which is lighter than water; I thought it expedient to try what figures would be afforded by the surface of water and also by that of air, when thoſe fluids ſhould become contiguous to an oil, heavier than water: of which ſort chymiſtry had afforded me more than one or two beſides the eſſential oils of cloves and Exp. iv. cinnamon. Having therefore provided ſome pure oil of the gum of *Guajacum*, and poured a little of it into a ſlender pipe, we found the upper ſuperficies of it to be concave; almoſt, if not altogether, like that, which water would have had in the ſame pipe. But when I put a little water upon this oil, it preſently changed the figure of its ſurface, which became viſibly, though not very much, protuberant or convex.

AND in regard this oil, though heavier than water, is not ſo heavy as deliquated ſalt of Tartar, I thought fit to try, whether the phænomenon would not be differing upon Exp. v. the contact of thoſe two liquors; and accordingly having put ſome oil of Tartar into the ſlender pipe, and put ſome drops of the oil of Guajacum to it, we found, that this liquor did not manifeſtly alter the concave figure of the ſurface of the liquor Alcali, as the oil of turpentine had done: and having, for curioſity ſake, warily poured a little water upon the oil of Guajacum, I found, as I had reaſon to ſuſpect, that the upper ſuperficies of it changed preſently from a concave figure to a convex, ſo that this oil in the midſt of the other two liquors appeared like a little red cylinder, which, inſtead of having circular baſes, was protuberant at both ends, but more at that which touched the oil of Tartar.

Exp. vi. To vary a little the experiment, I put ſome eſſential oil (as chymiſts call it) of cloves into a new ſlender pipe, and having obſerved it to be ſomewhat concave at the top, where it was contiguous to the air, we cauſed a little common water (perhaps a quarter of a ſpoonful or leſs) to be put to it, and found, as we expected, the ſurface of this oil alſo to become tumid. And in regard this liquor, as well as the forementioned oil of Guajacum, though it were ſo heavy as to ſink in water, would not do ſo in deliquated ſalt of Tartar, we did, into another ſlender pipe, put firſt ſome of this laſt named liquor, then ſome of the aromatic oil, and laſtly, a little common water; by which means we found, that the little cylinder of oil did, like that of the oil of Guajacum, appear convex at both ends; but was unlike it in one circumſtance, that the oil of cloves appeared more convex at the upper end, where it was contiguous to the water, than at the lower, that leaned upon the ſurface of the oil of Tartar.

Exp. vii. HAVING made theſe trials, to alter, by another contiguous fluid than the air, the concave ſuperficies of water and ſome aqueous liquors, I proceeded to try, whether a change would not likewiſe be made on the convex figure of the ſurface of quickſilver included in the like ſlender glaſſes; and accordingly, having taken one that was much longer, but of the like bore with the former (for to make the trials more uniform, I had cauſed a long pipe to be by the flame of a lamp unequally divided into ſeveral ſhort ones) we put into it a ſmall quantity of quickſilver; and having taken notice how the upper ſuperficies ſwelled in the middle above the level of the parts, where it touched the glaſs, we poured ſome water upon it, and found a manifeſt and conſiderable depreſſion of the ſurface, though the protuberance were not quite ſuppreſſed.

Exp. viii. THIS phænomenon having been for greater ſecurity ſeveral times repeated, I thought fit to try, what variation would be made, by the greater or leſſer height of the water incumbent on the mercury. And ſometimes it ſeemed, that, when the aqueous cylinder was much longer, the depreſſion of the mercurial ſurface was ſomewhat greater. But this did not ſo conſtantly happen: but we often obſerved, that, though a very little water ſufficed by its contact to make, in the judgment of the eye, a manifeſt abatement of the protuberance of the quickſilver, yet it had not the ſame effect on that ponderous fluid, that it had, when, being increaſed almoſt as high as the length of the pipe would permit, a greater weight of it was incumbent on the mercury. For then I manifeſtly perceived and ſhewed to others, that the ſurface of the quickſilver being depreſſed almoſt

moſt to a level in thoſe parts of it, that were near the inſide of the glaſs, there was about the middle of the ſurface an elevation of mercurial matter, that appeared to be rather more than a half globe, and was to the height of its full ſemidiameter, raiſed above the reſt of the mercurial ſurface; and in that ſtate it continued as long as I thought fit to let it do ſo. And leſt this trial ſhould impoſe upon me, I cauſed it to be more than once repeated; and, the better to confirm it, I afterwards cauſed the incumbent water to be little by little ſuckt up, and found, as I expected, that when the incumbent water began to be too much ſhortened, the little teat or ſegment of ſphere, lately mentioned, began to be ſomewhat flattened, and ſubſided more and more as the water was further taken off.

BECAUSE the common atmoſpherical air we breath is a fluid body abounding with groſſer particles, and is by divers philoſophers probably ſuppoſed to be much more denſe and heavy than the æthereal ſubſtance, that makes the other part of the atmoſphere; I thought fit to try for their ſakes, whether or no the ſuperficial figure of liquors would be altered by having the contiguous air withdrawn from about them, and ſo being left to be touched by the purer æther without it; and accordingly having conveyed into one of our pneumatical receivers a couple of ſuch ſlender pipes as have been already deſcribed, one of them furniſhed with common water, and the other with quickſilver, we cauſed the common air to be diligently pumped out, without obſerving any ſenſible change in the concave figure of the water: but as for the quickſilver, I knew not what to conclude about it. For having repeated the trial twice or thrice, the mercury ſometimes ſeemed manifeſtly to ſwell or be more protuberant upon the exhauſtion of the receiver, than when it was put in, eſpecially when its figure was attentively viewed, and the external air, that was pumpt out but ſlowly, was ſuffered to re-enter with all convenient celerity. But that which yet kept me doubtful was, that I obſerved, that upon the diligent withdrawing of the air's preſſure on the quickſilver, there diſcloſed themſelves in it ſome little bubbles, which I feared we had not been able to free it altogether from, and which might be ſuſpected to have ſome intereſt in the phænomenon; which though I was at that time hindered by ſome occaſions from proſecuting further, yet I think it may be well worth the while, becauſe, if any ſenſible change do certainly appear to be made in the ſuperficial figure of the mercury, it may teach us ſomewhat relating to the conſtitution of the æther, which ſeems to make up far the greater part of the univerſe known to us: and I ſhould not in that caſe think it impoſſible, that by expoſing many and differing liquors to its contact *in vacuo Boyliano* (as it is called) ſome diſcovery may be made of differing ſubſtances, whereof one may ſuſpect the æther itſelf not to be uncapable. But to leave ſuſpicions, that probably will be thought chimerical, I ſhall only add, which I forgot before, that we conveyed into our receiver a clear chymical oil that was heavier than water, and whilſt it was contiguous to it, had not a concave but a convex ſurface; and having placed the pipe furniſhed with both liquors in the pneumatical receiver, we pumpt out the air without finding that the oil ſenſibly altered its protuberant ſurface, as neither did the water loſe the concave figure of its upper ſurface. Exp. ix.

WHEN clouds are condenſed into rain, and lower aggregates of vapors into dew, it is ſuppoſed to be obvious, that the drops of thoſe meteors do, in their paſſage through the air, (which to them is a heterogeneous fluid) acquire a round figure; and when we ſhake oil into water, the portions of the former fluid, during the little time they remain diſtinct (for they quickly reunite into maſſes) are found to be globular. But theſe phænomena are too few and too tranſient to afford any conſiderable obſervation of the figures of fluid bodies, eſpecially if they be quieſcent, and every way encompaſſed by other fluids. Wherefore I thought fit to try what I could do with chymical liquors

unapt

unapt for mingling, to produce phænomena, that may last long enough to allow us to observe them attentively, and in some cases to vary them.

Exp. x. FOR this purpose, I first took fixt niter, (or, which is analogous to it, salt of Tartar) resolved *per deliquium* into a transparent liquor; and having filled a clear vial half full with this, I poured on it a convenient quantity of vinous spirit exactly rectified, that there might be no phlegm to occasion an union between the two liquors, which ought, as ours did, to retain distinct surfaces, and speedily regain them though the glass were well shaken. Then having found by a trial formerly mentioned, that common oil of turpentine, if employed in a competent quantity, will not totally (and much less will readily) dissolve in spirit of wine, and also having observed (what may seem somewhat strange) that if this spirit of wine be exquisitely dephlegmed, the oil, though a chymical one, will not swim on it, but sink in it; I warily let fall some drops of the oil into the spirit, and had the pleasure to see, as I expected, that they fell towards the bottom of the glass, till their descent was stopt by the horizontal (for it was not concave) surface of the Alcalizat liquor of fixt niter. And because my design was chiefly to observe the superficial figure of a fluid encompassed by other fluids without touching any solid body, I shall here take notice of the chief phænomena, that were produced of that kind, without staying to enquire into the causes or the consequences of them.

1. IF the oily drops were but small, they seemed to the eye exactly enough spherical. For the oil differing but very little in specific gravity from the spirit of wine, the drops did but just touch the surface of the subjacent Alcali; and the same drops being but small, their own weight was not great enough visibly to depress them, and hinder that roundness, which the pressure of the ambient spirit, or their own viscosity endeavoured to give them.

2. IF an aggregate of drops were considerably bigger than those newly mentioned, as if it had about a third part of an inch in diameter, it would then manifestly lean upon the Alcalizat liquor as upon a floor, and appear somewhat elliptical, (for some little part of the bottom was a plain;) the weight of the upper parts depressing the drops, and making the horizontal diameter somewhat longer than the transverse.

3. IF a yet greater portion of oil were let fall upon the heavy liquor, it would for a pretty while appear in the form of a somewhat imperfect hemisphere, or some other large section of a sphere, the lower part being cut off, (as if a globe were divided by a plain) by the horizontal surface of the deliquated salt.

4. BUT if the quantity of oil were not too great, it was pretty to observe, that, though at first putting in, it did perhaps spread itself over the subjacent liquor, and lie as it were flat upon it; yet by little and little, (for it was but slowly) it would by the action of the ambient, concurring with its own tenacity, be crowded together into a figure of a lesser surface, and consequently less hindering the motions of the vinous liquor. For by the action of this spirit, the oil would by degrees be raised above the surface of the fluid niter, and be reduced to the figure, either of half a globe, or of a greater segment of a globe, or even of an imperfect ellipsis, according as the bulk or weight of the oil made it more or less apt to resist the action of the ambient spirit, to whose effect, as I lately intimated, the natural viscosity of the oil might (more or less) co-operate. as also might the weight of the spirit of wine, which in great part disabled the endeavour of the oil's gravity to make its figure less convex.

5. THOUGH these globules or portions of oil did oftentimes readily mingle, when they touched one another, yet divers times also we observed, that having warily approached them, we were able (as if some odd subtile matter, that the eye could not discern, interposed, to keep them unconfounded,) to make them touch without mingling;

ling; inſomuch, that we have with pleaſure made them ſo far bear againſt one anothers ſurfaces, as manifeſtly to preſs them inwards, though being parted they would preſently reſume their former figure: which circumſtance ſuggeſted to me ſuſpicions, that I cannot now ſtay to name. But in caſe any of theſe oily portions came by a more preſſing contact to be united, they would then alter the figures they had whilſt ſeparate, and take another, ſuitable to the bulk of the aggregate.

6. WHEN a large portion of oil reſted upon the ſaline liquors, if then the ambient ſpirit were moderately and warily agitated, it was not unpleaſant to obſerve the various figurations, which the convex and protuberant part of the multilated globe would be put into by theſe ſhakes, without any viſible ſolution of continuity, or conſiderable motion of the whole body, which would very quickly recover its former figure. Though, if the agitation were too ſtrong, ſome portions would be quite broken off, and preſently turned into little globes.

I TRIED to produce another phænomenon, that would not have been unpleaſant, Exp. x. by putting together in a ſomewhat large veſſel, with other liquors, two oils, (whereof one, if I miſtake not, was from turpentine,) which firſt, by reaſon of the oleaginous nature, wherein they agreed, might exactly mingle and make a compounded liquor; and then, by reaſon of their being one heavier, and the other lighter *in ſpecie* than water, might by this liquor be again ſeparated, and include betwixt them the liquor, that had divided them. But I found, that the oils being once united would not be eaſily parted, but, according to the prevalency of the lighter or heavier ingredient, in the mixture, the compounded oil, would almoſt totally either emerge to the top of the water, or lie beneath the bottom of it; I ſay, almoſt totally, becauſe ſome parts of the oil, which was not perhaps all uniformly mixt, did not keep in a body with the reſt; but either was ſeparated from the maſs in the form of globules, or elſe, ſticking to the ſide of the glaſs, had the other part of its ſuperficies, which was contiguous to the water, very variouſly figured, according as the bulk and degree of gravity of the adhering oil and other circumſtances happened to determine. And it is chiefly upon the account of this various and odd figuration of our mixture, that I here make mention of this trial; which though it proved not ſucceſsful to me, yet perhaps may ſucceed in the hands of another, that ſhall make it with more leiſure and diligence, than I could afford it.

THESE are ſome of the phænomena I obſerved in oil of turpentine, when it was invironed only with fluids; but, if it were permitted to be contiguous to the inſide of the glaſs, and ſo to faſten part of its ſurface to a ſolid, the greater part of the ſurface, which remained expoſed to one or both of the contiguous liquors, would, partly by their action, and partly by the gravity of the oil itſelf, be put into figures ſo various, and ſometimes ſo extravagant, that it was much more pleaſant to behold them, than it would be eaſy to deſcribe them; which therefore I ſhall not here attempt to do.

WHEREAS I intimated in the preamble to theſe notes, that confining fluids may have Exp. xii. diſtinct ſurfaces, without having, at leaſt in many poſitions, refractions differing enough, or reflections ſtrong enough, to make the plain, that diſterminates them, obvious to the eye; I ſhall here obſerve, that when the oil of Tartar, or nitrous Alcali, that I employed, happened to be very clear and colourleſs, I have more than once made highly rectified ſpirit of wine float upon it ſo, that in moſt poſitions the vial ſeemed to have in it but one uniform liquor; the plain, that divided the two fluids, being unapt to be diſcerned, but in a poſition, wherein the rays of light paſſing thence to the eye fell very obliquely on it; and indeed, when there was no little duſt or other feculency ſwimming upon the ſurface of the oil of Tartar, I had ſometimes much ado to convince ordinary ſpectators, that the vial, in two diſtinct regions of it, contained two unſociable liquors.

Exp. xiii. ON this occasion, I shall add an experiment, which, though it does not so directly belong to our subject, as to make its omission a fault, is not yet perhaps so impertinent as to be unwelcome.

WE took a deliquated Alcali, made of niter and tartar, and deeply tinged with cochineel; and, that the liquors might not only be heterogeneous, but as differing in gravity and density as we could make them, we poured on it a peculiar kind of oil lighter than spirit of wine, and holding the plain where the two liquors were contiguous in a convenient position, in respect of the light and the eye, I observed it to make a strangely vivid reflection of the incident beams of light: so that this physical surface, which was flat, looked almost, for it was not so specular, like that of quicksilver; and when I kept it till night, and considered it by the light of a candle, the bright figure of the flame was strongly reflected almost as from a close specular body; which tempted me to suspect, that there might be something else than the bare smoothness of the surface of the Alcalizat liquor to produce so brisk a reflection; and the rather, because I did not observe, that the remains of the same tinged Alcali, which I kept in another glass, nor a portion of the same oil, which I had also by me in a separate vial did either of them afford so vivid a reflection from its surface; though I did the less wonder at this, because of the great disposition to reflect light, which I had formerly the curiosity to observe in the forementioned oil, when I joined it with other liquors. But, whether this strongly reflecting power, taken notice of in our late recited experiment, proceeded from some mixture, as it were, or confusion of singly unperceived particles in the physical superficies or plain, where the two liquors confine; or, whether some such *materia subtilis*, or æthereal fluid, as *Cartesius* and some of the antients maintained, insinuated itself between our two liquors, and made the disterminating surface more specular; or whether the phænomenon be rather due to some other cause, I shall not now stay to make inquiry. But to help towards it, I shall add on this occasion, that looking on this liquor, as a body, which, though it have all the necessary qualities of an oil, does, in regard of its origin, and some properties I have found in it, differ from common chymical oils; I was invited the more to observe its phænomena in reference to reflection; and I found, among other things, (not pertinent to this place,) *First*, that the confining plain, often mentioned between the tinged Alcali and this liquor, did not appear red itself, nor communicate that colour to the image of the flame of a candle reflected from it. *Secondly*, that when I warily shook the vial, which contained the two liquors, the uppermost would be reduced into a seeming froth, consisting of a great number of imperfectly globular bodies, which after a while would make a kind of a rude physical plain; which, though neither very horizontal nor sensibly smooth, would, at its upper superficies, send back the incident light with more briskness than one would expect. And when the seeming froth consisted of smaller particles, these, when they were of a certain size, and conveniently placed, in reference to the flame of a candle and the eye, would, (as more than one trial informed me,) reflect the incident light so many ways, and so visibly, that they seemed, for multitude and splendor, like little sparkling corpuscles of polished silver; or almost like those glistering ones, that appear, when a clean plate of copper is first immersed into a much allayed solution of good silver, made in *aqua-fortis*.

AND to these phænomena I shall add a third, which is, that, though pure spirit of wine be so thin a liquor, and our oil is nevertheless so light as to swim upon it; yet I found the confining surface very strongly reflexive. But of this liquor more perhaps may be said in another place.

AND it may, in the mean while, not be impertinent here to intimate to you, that I found, that some other essential oils (as chymists call those, that are distilled with water

ter in limbecks) and particularly an unsophisticated oil of limons, did, with our tinged Alcali, afford most of the same phænomena; but not so brisk a reflection: I say, most, chiefly because with spirit of wine these subtile oils, as I formerly noted, will readily be confounded; though our anomalous oil be unsociable with it.

A Continuation of the EXPERIMENTS published in the next foregoing Tract, about FLUIDS, contiguous to other FLUIDS.

First printed in the PHILOSOPHICAL TRANSACTIONS, N°. cxxxii. p. 799.

IN the winter time, and at other times also when the air is cold enough, the figure, acquired by the surface of an oil contiguous to the water on one side, and the air on the other, may be preserved from varying, and so may be at leisure observed by the direction afforded by the following experiment, which I devised for this purpose.

In cold weather we took essential oil of annifeeds, whose property it is to coagulate in such weather; and having in a gentle warmth brought it to be fluid, we poured it into a slender vial more than half filled with common water, that had been also a little warmed, that the oil might not be too hastily reduced to its former state. This oil being lighter than so much water, and being poured on in a convenient quantity, had its upper surface somewhat concave, as that of the water was; but the lower surface, surrounded by the water, was very convex, appearing almost (for it was not perfectly) of the figure of a great portion of a sphere. This being done, the vial was stopt, and suffered to rest for some time in a cold place, by which means the water continuing fluid as before, the oil of annifeeds was, as I expected, found coagulated in a form approaching to that it had whilst in a fluid state; I say, approaching, because it was not easy to discern the exact figure in the vial I was fain to make use of: and I suspected, that the oil grown consistent was become less convex than before; but the two surfaces of it gave it some resemblance in point of shape, but not of transparency, to a concavo convex glass; but yet much thicker in the middle, than is usual in glasses of the like breadth, employed for dioptrical purposes. And indeed (to give here this Advertisement once for all) I would not have you understand in too strict a sense what my intended brevity, and some other motives, make me deliver in naming the figures of fluids. For I confess, that if I were to write for a rigid geometrician, especially if he were nice and critical in the doctrine of conic sections, I should think myself obliged on some occasions to a greater curiosity in naming the figures of fluids, than you will meet with in this paper. But since I write but notes, and design to give you rather experimental hints, than geometrical determinations; I presume, that when you are once cautioned by a plain advertisement, it may suffice for me to refer the fluids, I describe, to such of the more known figures as they seem to be the least remote from, without troubling you or myself with maimed figures, or with spheroids, conoids, paraboloids, and other hard words; which I the rather abstain from, not only because the particulars, wherein my fluids resembled them and differed from them, could not be intelligibly declared without many words; but because I observed the figures themselves of the Exp. xiv.

the fluids to vary, and sometimes considerably too, according to contingent circumstances. And for this reason also I will not persuade you to expect, that the event of every trial, you shall make of these experiments, will be precisely the same with the event of mine. For by reason of those contingent circumstances, I dare only speak historically of these experiments; and, without pretending that they shall always uniformly succeed, content myself to relate *bona fide*, what trials have been made, and what happened to me thereupon, not despairing, that this variation itself of events according to circumstances may be instructive.

BUT to return to our lately mentioned oil of anniseeds, it was worth observing, how great a difference there was between the dull reflection it made when it was coagulated, and the fine reflection it had made whilst it was a liquor. The latter of which reflections brought into my mind, how vivid the reflective power of some fluids is in comparison of that of the generality of solid bodies, of which there is scarce any, if there be any at all, that is observed to have a stronger reflection than clean quicksilver; and yet (to add that upon the by) I have sometimes found, that this itself may be increased by the addition of a liquor. For having observed, as I elsewhere note, that quicksilver, and rectified *Oleum Petræ* are, the former of them the heaviest, and the latter the lightest of all the visible fluids, that are yet known to us, or at least to me; and having also observed the latter of them to be considerably reflective, I had the curiosity to try among other things, that related to them, the following experiment. Some (distilled) quicksilver being put into a small vial, and held in such a posture, that the incident light was strongly remitted to my eye, I slowly put to it some petroleum, that being well rectified was very clear; and observed, that, as this liquor covered the quicksilver, there was at the imaginary plain, where they both confined, a brisker reflection than the quicksilver alone had given before. Whether this increase of reflective power proceeded from any thing produced upon the confines of the two bodies, or from some ethereal fluid that slipped in there, I have above declined, and shall now forbear, to examine: but on this occasion it will not be amiss to take notice, that either the surface of the air itself, as thin and yielding a fluid as it is, or the surface of a solid, contiguous to included air, or some interposed subtile matter, may reflect the incident beams of light more strongly than most men would expect. To this purpose I remember, that a curious person having one day brought me a couple of rarities, which he told me were two pieces of a solid, but transparent, body, that he had casually found, in one of which there was a pearl, large, round, and orient, and in the other a less perfect one; and having desired my opinion, whether they were considerable enough to be presented to the king; I, after I had sufficiently viewed them in differing positions, and especially against the light, asked him, whether he were sure the included bodies were pearls. To which when he answered, that his eyes permitted him not to doubt of it, especially because he knew of no other gems nor stones, that had so strong and fine a reflection; I replied, that I thought they might be only bubbles of air, casually intercepted in the viscous matter of the containing bodies (which I supposed, upon good grounds, to have been once somewhat fluid) before it came to be hard; adding, that his majesty, who was critical in these matters, might probably have the curiosity, I had, to have the worst of them broken, to be satisfied what kind of bodies the included were. Hereupon, to content me, one of them was opened; and that which had appeared a pearl was found to be but a cavity, that contained no grosser substance than air. And I have by me a well shaped piece of glass of a good thickness, with an aereal bubble in the middle, which by some qualities, particularly its pear-like shape and vivid reflection, not ill resembles a fair, though not orient, pearl. But in such like observations, the position

Exp. xv.

pofition of the eye, and that wherein the body receives the beams of light, may be very confiderable. For I have by me a fmall ftone (with which I have puzzled the fkilful jeweller of a great prince to determine what kind of gem it is) that being laid flat upon ones hand, or a piece of paper, and lo ked on directly downwards, looks almoft like a piece of common glafs, and is tranfparent: but if the eye be fo placed, that the incident beams of light, by whofe reflection it is feen, fall with a convenient degree of obliquity upon the ftone, it makes an exceeding pretty fhew, fometimes appearing like a fine opal, and fometimes not very unlike an orient pearl. Exp. xvi.

It may not be altogether impertinent, and at leaft, for the novelty of the way of trial, it will not probably be unpleafing, if I here mention an attempt to try, whether, when the rays of light rebound from bubbles invironed with an uniform folid body (which cafe is fomewhat differing from that of bubbles looked upon in an exhaufted receiver) the reflection be only, or almoft only, from the groffer particles of the air, and not alfo from fome fubtile matter harboured, as well as they, in the fame cavities? But to bring this queftion to trial feemed difficult enough, becaufe it is fo, to include very rarified air in a confiftent body, diaphanous enough to let its reflection be eafily obferved. To compafs this, I thought upon the following expedient. We made, according to the eafy direction * elfewhere given (for other purpofes) a competent quantity of a refinous or gummous fubftance, that looked like high coloured amber, but was eafy to melt. This we put into a deep round glafs with a wide mouth, and held it by the fire-fide in a moderate warmth, till it was brought into a fluid ftate; then we transferred it into one of our pneumatical receivers, where we prefumed, that this temporary liquor would, as well as liquors that are conftantly fuch, difclofe aereal bubbles, when the preffure of the air was withdrawn from it; and accordingly having caufed the air to be pumpt out by degrees, we found, that ftore of bubbles appeared at the top of the liquor, and made there a copious froth, many of them being, by reafon of the vifcofity of the fluid, very large; and divers of them, becaufe of the nature and texture of it and the thinnefs of the films, being adorned with the colours of the rainbow, whofe vividnefs made them pleafant to behold, and fuggefted to us fome optical confiderations. But notwithftanding this froth, I caufed the pumping to be continued, that thofe bubbles, that had moft of common air in them, and which therefore are wont to rife firft, might get to the top, and the fubfequent bubbles might meet with more refiftance from the liquor ftill tending to grow cold, and fo might be the more expanded, and yet kept from emerging by the concretion of the refinous fubftance; and anfwerably to this we found, that, when this fubftance had refumed its confiftent form, there were intercepted, between the upper and the lower furfaces of it, fome bubbles, that were not fmall, which yet had a confiderable reflection, notwithftanding the fmall quantity of the groffer particles of the air, that may be fuppofed to be contained in bubbles fo very much expanded (perhaps fo, as to exceed fome hundreds of times their former dimenfions) I might add, that by letting the outward air into the receiver, the air in divers of the formerly mentioned large bubbles, at the top of the glafs, was too much rarified to keep them from being broken by the preffure of the returning air. But I am fenfible, that, in what I have faid of the reflective power of the air, I have already too far digreffed, and therefore I fhall ftep into the way again, and proceed to other obfervations.

* In the [illegible]

WATER being fo confiderable a body here below, I thought it would be worth while, to endeavour to obferve its furface, when contiguous to other fluids than air, and, if it were poffible, when furrounded by them. For though it is taken for granted, that the falling drops of rain are fpherical, yet their defcent is fo fwift, both by reafon of their gravity in refpect of the air, and the height from whence they fall, that Exp. xvii.

I fear men have rather ſuppoſed than obſerved that their figure is ſpherical; which will be the more queſtionable, if it be true, which is vulgarly thought, that hail is but rain frozen in its paſſage through the air. For it is evident, that the grains of hail are frequently of other figures than truly orbicular. But becauſe there may another poſſible account be given of this irregular figuration of hail, I ſhall not inſiſt on this phænomenon, but proceed to what I tried about the ſurface of water; of which I found it the more difficult to make obſervations, becauſe that liquor will readily mingle both with ſpirit of wine and with oil of tartar, and with other liquors, that are analagous to either of theſe.

Exp. xviii. THE ſurface of water may have differing figures, according as it is totally incompaſſed with heterogeneous fluids, or, as it is only in ſome places contiguous to one or more of them. In the former caſe we found it not ſo eaſy to make an obſervation, both becauſe, that, as I lately noted, we know not of any two liquors (ſetting mercury aſide) that will not mingle either with one another, or with water. And becauſe alſo our oil of guajacum itſelf, though heavier than water, would not be ſerviceable on this occaſion, in regard of its being of ſo deep a red, that the figure of the water incloſed in it could not be diſcerned through it; wherefore I made uſe of chymical oil of cloves, as being ſomewhat, and but a little, heavier in ſpecie than water, ſo that ſome drops or ſmaller portions of this laſt named liquor would be almoſt quite invironed with the other: we cautiouſly therefore conveyed into ſome oil of cloves, whoſe ſurface the veſſel permitted to be large enough, ſome portions of common water of differing bigneſſes, taking care, as far as we could, that they might not touch one another; by which means the oil being tranſparent, and yet ſomewhat coloured, it was eaſy to obſerve, that the ſmaller portions of water were ſo near totally invironed with the oil, that they were reduced into almoſt perfect globes; thoſe portions, that were ſomewhat bigger (as about twice the bigneſs of a pea) would be of a figure ſomewhat approaching to that of an ellipſis (for it was not the ſame) and thoſe portions, that were yet ſomewhat larger, though they ſeemed to be ſunk almoſt totally beneath the oil, yet they held to it by a ſmall portion of themſelves, whoſe ſurface was eaſily enough diſtinguiſhable from that of the oil. Theſe large portions of immerſed water, being almoſt wholly invironed with the other liquor, were by it reduced into a round figure, which was ordinarily ſomewhat elliptical, but more depreſſed in the middle than that figure requires. But all this is to be underſtood of thoſe portions of water, that touched only the oil and the air; for thoſe, that touched one another without mingling, and much more thoſe that adhered more or leſs to the ſides of the glaſs, had their ſurfaces too differingly and irregularly figured to be here attempted to be deſcribed.

As for the ſuperficial figure of water, contiguous, both above and beneath, to other

Exp. xix. fluids, and laterally to ſome ſolid body, it is not ſo eaſy to be ſure, which of the contiguous liquors is of moſt force to determine the figuration of their common ſuperficies or commiſſure. But however I ſhall relate, that, having into a ſlender pipe of that ſort that has been deſcribed before, put a little oil of cloves, and upon this ſome oil of turpentine, that ſo the water might both above and beneath be touched by heterogeneous liquors, I obſerved not the oil of cloves to be very manifeſtly tumid at the top, nor the lower ſurface of the oil of turpentine (for the upper was concave) to be very convex; for ſomewhat convex it was, downwards. And from this it will be eaſy to conclude the figure of the cylindrical portion of water intercepted between theſe two oils.

THAT agent or force, whatever it be, that keeps liquors fluid, does likewiſe, whilſt they are ſo, keep their ſurfaces exceeding ſmooth, when they are contiguous to the air and other fluids. But becauſe I thought it doubtful, whether even thoſe liquors, that are

are (as men usually speak) naturally fluid, I mean, such as are not made so by fusion, produced in them by the action of the fire, would retain smooth surfaces, when they have lost their fluidity, and have their parts no longer inflected and agitated, so as to enable them, by the help of gravity, viscosity, or both, to levigate (if I may so speak) or polish each others surfaces, as it may be guessed in their fluid state they did; I thought it not amiss, in order to the clearing of the doubt, to make some trials with contiguous liquors, whereof one would continue fluid when the other had lost its fluidity.

I TOOK then oil of anniseeds, thawed by a gentle warmth, and common water, and having put them together in a conveniently shaped glass; they were suffered to stand in a cold place till the oil was coagulated; which done, it was parted from the water, and by the roughness of its superficies manifested, as I expected, that, when its parts were no longer agitated and kept easily displaceable by the subtile permeating matter, or whatever other agent or cause it were, to which it owed its fluidity, then the contiguous water grew unable to inflect, or otherwise place them after the manner requisite to constitute a smooth surface. And what happened to that part of the oil's surface, that was touched by the water, happened also to that, which was contiguous to the air; save that the asperity of the last named surface was differing from the other, which, whether it were an accidental to constant phænomenon, further trial must determine. But I have often observed, that the upper surface of oil of anniseeds, when this liquor comes to be coagulated by the cold air, was far enough from being smooth, being variously asperated by many flaky particles, some of which lay with their broad, and others with their edged parts upwards. Exp. xx.

AN inequality and ruggedness of superficies I have also observed in water, when, having covered it with chymical oil of Juniper, and exposed it in very cold weather, though the oil continued fluid, yet the water, being frozen, had no longer a smooth superficies, as whilst in its liquid state it was contiguous to the oil. And the like inequality, or rather a greater, we observed in the surface of water frozen, which had chymical oil of turpentine swimming over it; yet a no less, if not a much greater, roughness may be oftentimes observed in the surfaces of divers liquors that abound with water, when those liquors being frozen, their surfaces have an immediate contact with the air. This I, among others, (elsewhere) observed; and I shall here add, that having purposely caused a strong and blood-red decoction of the soot of wood to be exposed in a large glass in a very cold night, I was more pleased than surprized, to find in the morning a cake of ice, that was curiously figured, being full of large flakes shaped almost like the broad blades of daggers, but neatly fringed at the edges. But that which I chiefly mention these figures for, is, that they seem to be as it were imbost, being both to the eye and the touch raised above the horizontal plain or level of the other ice. Exp. xxi.

AND here I must not omit to take notice, that whereas in the recited experiments the rugged surface was produced at the confines of two heterogeneous and unsociable liquors, I have sometimes observed the like phænomenon in one and the same liquor; and particularly, not long since looking in frosty weather on a vial, where I had long kept oil of vitriol, I perceived, that the cold had reduced far the greatest part of the Menstruum into a consistent mass, whose upper surface was very rugged and oddly figured, though it lay covered all over with a pretty deal of high coloured liquor, that was not frozen or coagulated, nor seemed disposed to be so, at least in that degree of cold. Exp. xxii.

THIS brings into my mind, that not only bodies, which in their natural state (as it is wont to be called) are fluid; but also such, as, by the violence of the fire, are made Exp. xxiii.
to

to flow, may be conformable to some naturally fluid bodies in their superficial figures. This may be observed in the best sort of what the chemists call *Regulus Martis stellatus*, where the figure of a star, or a figure somewhat like that of the decoction of soot lately mentioned, will frequently appear imbost upon the upper superficies of the Regulus; and such a raised figure I think I can yet shew you, on a mass of Regulus made of Antimony without Mars. But if, to those two bodies, copper be also skilfully added, the superficies will be oftentimes adorned with new figures according to circumstances; though the most usual I took notice of was that of a net, that seemed to cover the surface of the compounded Regulus. But this is not so constant, but that I have by me a mass of a conical figure, consisting of two very contiguous, but easily separable, parts, whereof the lowermost, which abounds more in metal, hath its upper surface covered with round protuberances, in shape and bigness not unlike to small pease cut in two; and these are so really imbost and elevated above the rest of the superficies, that the other part of the cone, which is of a more scorious nature, has, in its lower surface, which exactly fits the upper of the Regulus, cavities, for number, shape, and bigness, answering to the protuberances lately mentioned; which argues, that the Regulus cooled first with that inequality of surface we have described, and that the lighter and more recrementitious substance, continuing longer fluid, had thereby opportunity to accommodate itself to the superficial figure of the Regulus, on which it first leaned, and was afterwards coagulated.

A short Memorial of some OBSERVATIONS made upon an ARTIFICIAL SUBSTANCE, that shines without any precedent Illustration.

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ON Saturday the fifteenth of this month I was after supper visited by Mr. *Kraft*, a famous German chymist, who was pleased to come and shew me a strange rarity he hath newly brought into *England*, to the sight whereof he allowed me to invite several members of the Royal Society, he being desirous, because the matter he employs is very costly and of difficult preparation, to be a good husband of it, and by shewing it to several curious persons at once, to exempt himself from the need of shewing it often. The company being met, the artist took out of a pretty large box he had brought with him divers glass vessels, and laid them in order on the table. The largest of them was a sphere of glass, which I guessed to be four or five inches in diameter, being hollow and intire, save that in one place there was a little hole, at that time stopt with sealing wax, whereat to pour in the liquor, which seemed to me to be about two spoonfuls or somewhat more, and to look like muddy water made a little reddish with brick-dust or some other powder of that colour: he also took out of his box three or four little pipes of glass sealed, or otherwise stopt at both ends, being each of them somewhat bigger than a swan's quill, and about five or six inches long, and having at one end a small fragment or two of that matter, that was to shine in the dark.

HE likewise laid upon the table three or four vials of several sizes, but none of them judged capable to hold above very few ounces of water: in each of which vials there was

was ſome liquor or other, that was neither tranſparent nor well coloured, which liquors I confeſs upon his making no particular mention of what they were to do, I was not curious to compare together, either as to quantity or as to colour. Beſides all theſe ſubſtances, which were fluid, he had in a ſmall cryſtalline button bottle a little lump of matter, of which he ſeemed to make much more account than of all the liquors, and which he took out for a few moments to let us look upon it; whereby I ſaw, that it was a conſiſtent body, that appeared of a whitiſh colour, and ſeemed not to exceed a couple of ordinary peaſe, or the kernel of a hazel nut in bigneſs. Some other things it is poſſible Mr. *Kraft* took out of his box, but neither I or (for ought I know) others of the company took notice of them, partly becauſe of his haſte, and partly becauſe the confuſed curioſity of many ſpectators in a narrow compaſs kept me from being able to obſerve things as particularly and deliberately as I would gladly have done, and as the occaſion deſerved. Which advertiſement may, I fear, be but too applicable to a great part of the following narrative.

THE fore-mentioned glaſſes being laid in order upon the table, the windows were cloſed with wooden-ſhuts, and the candles were removed into another room by that we were in: being left in the dark we were entertained with the enſuing phænomena.

I. THOUGH I noted above, that the hollow ſphere of glaſs had in it but about two ſpoonfuls (or three at moſt) of matter, yet the whole ſphere was illuminated by it, ſo that it ſeemed to be not unlike a cannon bullet taken red hot out of the fire, except that the light of our ſphere looked ſomewhat more pale and faint. But when I took the liberty to hold this glaſs in my hand, and ſhake it a little, the contained liquor appeared to ſhine more vividly, and ſometimes as it were to flaſh.

II. I TOOK one of the little pipes of glaſs formerly mentioned into my hand, and obſerved, that though the ſhining matter had been lodged but at one end, yet the whole glaſs was enlightened, ſo that it appeared a luminous cylinder, whoſe light yet I did not judge to be always uniform, nor did it laſt like that, which was included in the vials.

III. IN the largeſt of the vials next the ſpherical already mentioned, the liquor that lay in the bottom being ſhaken, I obſerved a kind of ſmoke to aſcend, and almoſt to fill the cavity of the vial; and near the ſame time there manifeſtly appeared as it were a flaſh of lightning, that was conſiderably diffuſed, and pleaſingly ſurprized me.

IV. AFTER this I took up that ſmall cryſtaline vial, that I lately called (by a name familiar in our glaſs-ſhops) a button bottle, wherein was contained the dry ſubſtance, which the artiſt chiefly valued, as that, which had continued luminous about theſe two years; and having held that vial long in my hand, in the ſame poſition in reference to my eye, and looked attentively at it, I had the opportunity to obſerve (what I think none of the company did) that not only this ſtuff did, in proportion to its bulk, ſhine more vividly than the fluid ſubſtances, but that (which was the phænomenon I chiefly attended) though I could perceive no ſmoke or fumes aſcend from the luminous matter, yet I could plainly perceive by a new and briſker light, that appeared from time to time in a certain place near the top of the glaſs, that there muſt be ſome kind of flaſhy motion in the matter that lay at the bottom, which was the cauſe of theſe little coruſcations, if I may ſo call them.

V. THE artiſt having taken a very little of his conſiſtent matter, and broken it into parts ſo minute, that I judged the fragments to be between twenty and thirty, he ſcattered them without any order about the carpet, where it was very delightful to ſee how vividly they ſhined; and that which made the ſpectacle more taking, eſpecially to me, was this, that not only in the darkneſs, that invironed them, they ſeemed like fixt ſtars of the ſixth or leaſt magnitude, but twinkled alſo like them, diſcovering ſuch a

ſcintillation

scintillation as that whereby we distinguish the fixt stars from most of the planets. And these twinkling sparks without doing any harm (that we took notice of) to the Turky carpet they lay on, continued to shine for a good while, some of them remaining yet vivid enough, till the candles being brought in again made them disappear.

VI. Mr. *Kraft* also calling for a sheet of paper, and taking some of his stuff upon the tip of his finger, writ in large characters two or three words, whereof one being *DOMINI*, was made up of capital letters, which being large enough to reach from one side of the page to the other, and being (at least as I guessed) invigorated by the free contact of the external air, shone so briskly, and looked so oddly, that the sight was extreamly pleasing, having in it a mixture of strangeness, beauty, and frightfulness, wherein yet the last of those qualities was far from being predominant. And this phænomenon did in more senses than one afford us the most of light, since not only the characters shone very vividly upon the white paper, but approaching it to my eyes and nostrils, I could discern, that there ascended from them a fume, and could smell that fume to be strong enough, and (as it seemed to me) to participate of the odour of sulphur and of that of onions. And before I past from the mention of these resplendent characters, I must not forget, that either by their light, or that of the globe, or both by the one and the other a man might discern those of his fingers, that were nearest the shining stuff, and that this being held to the face though without touching it, some of the conspicuousest parts, especially the nose, were discoverable.

VII. AFTER we had seen with pleasure, and not without some wonder, the foregoing particulars, the artist desired me to give him my hand, which when I had done, he rubbed partly upon the back of it, and partly on my cuff, some of his luminous matter, which as if it had been assisted by the warmth of my hand shone very vividly, and though I took not notice of any thing upon my skin, that was either unctuous or rough, yet I oftentimes tried in vain by rubbing it with my other hand to take it off, or manifestly diminish its splendor, and when I divers times blowed upon some of the smaller parts of it, though they seemed at the instant that my breath beat upon it, to be blown out, yet the tenacious parts were not really extinguished, but presently after recovered their former splendor. And all this while this light, that was so permanent, was yet so mild and innocent, that in that part of my hand, where it was largely enough spread, I felt no sensible heat produced by it.

BY that time these things were done it was grown late, which made Mr. *Kraft*, who had a great way to go home, take leave of the company, after he had received our deserved thanks for the new and instructive phænomena, wherewith he had so delightfully entertained us.

BECAUSE Mr. *Kraft* had twice attempted to fire heated gun-powder with his phosphorus, but without success; probably because the powder was not very good (as by some circumstances I conjectured) and because it was not sufficiently heated, before the matter that should set it on fire was put upon it he promised me he would come another time to repair that unsuccessfulness: and accordingly, on the two and twentieth of September in the afternoon I received a visit from Mr. *Kraft*, who told me, he came to make good his promise of letting me see, that his shining matter was able to kindle heated gunpowder; and because no strangers were present, I had the fairer opportunity to view it, which I was able to do better by day-light, than I had done by its own light; for when he had taken it with a new pen out of the liquor, with which he kept it covered to preserve it, I perceived it to be somewhat less than the nail of one of my fingers, and not much thicker than a shilling; and I observed, that when it had lain a little while upon a piece of clean paper, and discharged itself from its superfluous moisture, it began to emit whitish fumes, which seemed to be very ponderous, since for the moſt part they did not aſcend but ſurrounding the matter, whence they iſſued, by their ſtagnation made as it were a little pond or ſmall atmoſphere about it; ſo that leſt it ſhould waſte too faſt, he was obliged, as ſoon as he had cut off a little corner leſs than half a pin's head, to put the ſtuff nimbly back into the vial, out of which he had taken it; where I obſerved it for a very ſhort time to ſend up exhalations into the liquor that covered it, and quickly after, as it were, quenched it. This done the artiſt divided the little corner he had cut off into two parts, one of which he ſpread as far as it would reach upon a piece of white paper, which he preſently after held at a diſtance over a chafing-diſh of burning coals, by whoſe heat being excited, it preſently flaſhed and burnt away; and I having perceived, that there was another part of the paper, which though not heeded by him, had been lightly beſmeared by the ſame matter, I held it over the coals, but at a conſiderable diſtance from them, and yet this little matter nimbly took fire and burnt a hole in the paper. And to ſatisfy myſelf, that the heat did but excite the luminous matter, and that it was this itſelf that lighted the paper, I held the reſt of the ſame piece of paper far nearer the fire, and kept it there a pretty while without finding it at all ſcorched or diſcoloured. Laſtly, the other part of the divided fragment of the hitherto mentioned matter, Mr. *Kraft* put upon the tip of a quill, and having at a diſtance from the fire, very well dried and warmed ſome gunpowder upon another piece of paper, he laid that paper upon the ground, and then holding his quill upon it, as if it had been a match, within half a minute (by my gueſs) that powder took fire and blew up.

IT will not perhaps be impertinent to add, that on occaſion of the operation I obſerved the air to have on the ſhining ſubſtance when freely expoſed to it, I took a riſe to tell Mr. *Kraft*, that I preſumed it might be worth while to try, whether his phoſphorus did ſhine by virtue of a kind of real or (if I may ſo call it) living flame, which like almoſt all other flames required the preſence and concourſe of the air to maintain it; or whether it were of ſuch a kind of nature, as the phoſphorus of the learned *Baldwinus*, which I ſuſpected to ſhine not like a flame or a truly kindled ſubſtance; but like a red hot iron, or an ignited piece of glaſs, wherein the ſhining parts are not repaired by fuel, as in other burning bodies, but are put by the action of the fire into ſo vehement an agitation, as whilſt it laſts ſuffices to make the body appear luminous. This conjecture Mr. *Kraft* ſeemed much to approve of, when I told him, that the way I propoſed to examine his *noctiluca* by, was to put a little of it into our pneumatic engine, and pump out the air, whoſe abſence, if it were of the nature of other flames, would probably extinguiſh, or very much impair its light; but yet ſince he offered not to have the trial made, probably becauſe he had but very little of his ſhining ſubſtance left, I thought it not civil to preſs him. But to countenance what I ſaid of the nature of Baldwinus's Phoſphorus, I ſhall recite an experiment, that I purpoſely made, to examine whether the preſence of the air were neceſſary to the ſhining of this phoſphorus, as I had long ſince found it to that of ſome pieces of ſhining wood.

WE expoſed for a competent time to the beams of a vigorous light a portion of matter of about the breadth of the palm of ones hand, which we had prepared to be made luminous by them. And then cauſing the candles to be removed (for we choſe to make trial by night) we nimbly conveyed the matter into a receiver, that was kept in readineſs for it, preſuming (as the event ſhewed we might) that by uſing diligence the light would laſt as long as the experiment would need to do: making haſte therefore to pump out the air, we heedfully watched, whether the withdrawing of it would, contrary to my conjecture, notably diminiſh the light of the ſhining matter. And after we had thus withdrawn the air gradually, we tried, whether by letting it return haſtily it would produce a more ſenſible change in the matter (which had been purpoſely put in without any thing to cover it, that it might be the more expofed to the air's action.) But neither upon the gradual recefs of the air, nor yet upon its rufhing in, when it was permitted to return, could we certainly obferve any manifeft alteration in the luminoufnefs of the phofphorus, other than that flow decrement, that might well be imputed to the time, during which the experiment was making. It being well known, that this luminous fubftance requires no long time to make it decay, and by degrees to lofe all its light; fo that though once there feemed to one or two of the byftanders, upon the return of the air, to be fome recovery of part of the loft fplendor, yet after repeated experiments it was concluded, that the prefence of the air was not at all neceffary to the fhining of our matter; and it was judged moft probable, that the abfence or prefence of the air, had no manifeft operation on it. I might add to this, that perhaps the prefence of the air is rather hurtful than advantageous to this fort of lights, for fince having had a large phofphorus, that was much efteemed, and, whilft I kept it, exactly protected from the air did very well; a part of the glafs that covered it, having by mifchance been fomewhat cracked, though none of the fplinters appeared difplaced, yet it feems fome of the corpufcles of the air made a fhift to infinuate themfelves at thefe chinks (as narrow as they were) and in not many days made the matter ceafe to be capable of being made luminous as before. I cannot ftay to enquire, whether this unfitnefs or indifpofition may be imputed to the bare moifture of the air, or to fome other fubftance or quality, that alone, or in conjunction with the moifture, may fpoil that peculiar texture, or conftitution, that fits the matter of the phofphorus, affifted by the impreffions of external light to become luminous. This, I fay, I cannot ftay to examine, though, that this phofphorus is of a nice and tender conftitution, and eafily alterable, I was induced to think, by finding, that the want of circumftances, feemingly flight enough, would keep it from being made; and I guefs, that a convention of circumftances did more contribute to the production, than any peculiar and incommunicable nature of the matter: becaufe having had the curiofity to make fome trial upon fo obvious a material as quicklime, though the fuccefs did not anfwer my defigns, yet, neither was it fo bad, but that fome luminous quality was produced in the lime by the action of the fire, and a faline liquor; and I fcarce queftion, but other materials will be found capable of being made luminous by the fame or the like operation, that is employed by Baldwinus, when that learned man fhall think fit to communicate his way to the public. But to return to what I was faying, that the contact of the air might be rather hurtful than advantageous to the phofphorus, I fhall only add here, as matter of fact (for my conjectures about light belong to my yet unpolifhed notes, of the origin of qualities) that whereas the contact of the air, though it were not free, did in a few days deftroy the luminoufnefs of a good phofphorus, yet having included another in a receiver, whence we afterwards pumpt out the air, this matter, though inferior to the other in vividnefs, was fo little fpoiled by lying open in our vacuum, that at the end of not only fome weeks, but fome months, I found, that the beams of a candle paffing to it through the receiver, would notwithftanding the vacuum it yet continues in, fuffice to re-excite in it a manifeft light.

An Hiſtorical Account of a DEGRADATION of GOLD, made by an ANTI-ELIXIR: A ſtrange Chemical Narrative.

The PUBLISHER to the READER.

HAVING been allowed the liberty of peruſing the following paper at my own lodging, I found myſelf ſtrongly tempted, by the ſtrangeneſs of the things mentioned in it, to venture to releaſe it: the knowledge I had of the author's inclination to gratify the virtuoſi, forbidding me to deſpair of his pardon, if the ſame diſpoſition prevailed with me, to make the curious partakers with me of ſo ſurpriſing a piece of philoſophical news. And, though it ſufficiently appeared, that the inſuing conference was but a continuation of a larger diſcourſe; yet conſidering, that this part conſiſts chiefly, not to ſay only, of a narrative, which (if I may ſo ſpeak) ſtands upon its own legs, without any need of depending upon any thing that was delivered before, I thought it was no great venture, nor incongruity, to let it come abroad by itſelf. And, I the leſs ſcrupled to make this publication, becauſe I found, that the honourable Mr. *Boyle* confeſſes himſelf to be fully ſatisfied of the truth of ſo much of the matter of fact, as delivers the phænomena of the trial: the truth whereof was further confirmed to me, by the teſtimony, and particular account, which that moſt learned and experienced phyſician, who was aſſiſtant to *Pyrophilus* in making the experiment, and with whom I have the honour to be acquainted (being now in *London*) gave me with his own mouth, of all the circumſtances of the trial. And, where the truth of this ſhall be once granted, there is little cauſe to doubt, that the novelty of the thing will ſufficiently indear the relation; eſpecially to thoſe, who are ſtudious of the higher arcana in the Hermetic philoſophy. For moſt of the phænomena here mentioned will probably ſeem wholly new, not only to vulgar chemiſts, but alſo to the greater part of the more knowing ſpagyriſts, and natural philoſophers themſelves; none of the orthodox authors, as far as I can remember, having taken notice of ſuch an anti-elixir. And, though *Pyrophilus*'s ſcrupuloſity (which makes him very unwilling to ſpeak the utmoſt of a thing) allows it to be a deterioration into an imperfect metal only; yet, to tell the truth, I think it was more imbaſed than ſo; for the part left of it, (and kept for ſome farther diſcoveries) which I once got a ſight of, looked more like a mineral or marchaſite, than like any imperfect metal: and therefore this degradation is not the ſame, but much greater than that, which *Lullius* doth intimate in ſome places. Theſe conſiderations make me preſume it will eaſily be granted, that the effects of his anti-philoſopher's ſtone, as I think it may not unfitly be called, will not only ſeem very ſtrange to Hermetic, as well as other philoſophers, but may prove very inſtructive to ſpeculative wits; eſpecially if *Pyrophilus* ſhall pleaſe to acquaint them with that more odd phænomenon, which he mentions darkly in the cloſe of his diſcourſe.

An Hiſtorical Account of the DEGRADATION of GOLD.

AFTER the whole company had, as it were by common conſent, continued ſilent for ſome time, which others ſpent in reflections upon the preceding conference, and *Pyrophilus* in conſideration of what he was about to deliver; this virtuoſo at length ſtood up, and addreſſing himſelf to the reſt: "I hope, gentlemen, ſays he, that what "has been already diſcourſed, has inclined, if not perſuaded you to think, that the exaltation, or change of other metals into gold, is not a thing abſolutely impoſſible;

" and, though I confeſs, I cannot remove all your doubts and objections, or my own, " by being able to affirm to you, that I have with my own hands made a projection, " (as chymiſts are wont to call the ſudden tranſmutation made by a ſmall quantity of " their admirable elixir) yet I can confirm much of what hath been argued for the poſſi- " bility of ſuch a ſudden change of a metalline body, by a way, which, I preſume, " will ſurprize you: for, to make it more credible, that other metals are capable of " being graduated, or exalted into gold, by way of projection, I will relate to you, " that by the like way gold has been degraded, or imbaſed."

THE novelty of this preamble having much ſurpriſed the auditory, at length *Simplicius*, with a diſdainful ſmile, told *Pyrophilus:* " That the company would have much " thanked him, if he could have aſſured them, that he had ſeen another metal exalted " into gold; but that to find a way of ſpoiling gold, was not only an uſeleſs diſcovery, " but a prejudicial practice."

Pyrophilus was going to make ſome return to this animadverſion, when he was prevented by *Ariſtander*; who, turning himſelf to *Simplicius*, told him, with a countenance and tone, that argued ſome diſpleaſure: " If *Pyrophilus* had been diſcourſing to a com- " pany of goldſmiths, or merchants, your ſevere reflection, upon what he ſaid, would " have been proper: but, you might well have forborn it, if you had conſidered, as I " ſuppoſe he did, that he was ſpeaking to an aſſembly of philoſophers and virtuoſi, who " are wont to eſtimate experiments, not as they enrich mens purſes, but their brains; " and think knowledge, eſpecially of uncommon things, very deſirable, even when it " is not accompanied with any other thing than the light that ſtill attends it, and indears " it. It hath been thought an uſeful ſecret, by a kind of retrogradation to turn tin " and lead into brittle bodies, like the ores of thoſe metals. And if I thought it pro- " per, I could ſhew, that ſuch a change might be of uſe in the inveſtigation of the " nature of thoſe metals, beſides the practical uſe, that I know may be made of it. " To find the nature of wine, we are aſſiſted, not only by the methods of obtaining " from it a ſpirit, but by the ways of readily turning it into vinegar; the knowledge " of which ways hath not been deſpiſed by chemiſts or phyſicians, and hath at *Paris*, " and divers other places, ſet up a profitable trade. It is well known, that divers " eminent ſpagyriſts have reckoned, amongſt their higheſt arcana, the ways, by which " they pretended, (and I fear did but pretend) to extract the mercury of gold, and " conſequently deſtroy that metal; and it were not hard to ſhew, by particular in- " ſtances, that all the experiments, wherein bodies are in ſome reſpects deteriorated, " are not without diſtinction to be rejected or deſpiſed; ſince in ſome of them, the light " they may afford may more than countervail the degradation of a ſmall quantity of " matter, though it be gold itſelf. And indeed, (continues he) if we will conſider " things as philoſophers, and look upon them as nature hath made them, not as opinion " hath diſguiſed them; the prerogatives and uſefulneſs of gold, in compariſon of other " metals, is nothing near ſo great as alchymiſts and uſurers imagine. For, as it is " true, that gold is more ponderous, and more fixed, and perhaps more difficult to be " ſpoiled, than iron; yet the qualities (whereof the firſt makes it burthenſome, and " the two others ſerve chiefly but to diſtinguiſh the true from counterfeit) are ſo balanced " by the hardneſs, ſtiffneſs, ſpringineſs, and other uſeful qualities of iron; that if thoſe " two metals I ſpeak of (gold and iron) were equally plentiful in the world, it is ſcarce " to be doubted, but that men would prefer the more uſeful before the more ſplendid, " conſidering how much worſe it were for mankind to want hatchets, and knives and " ſwords, than coin and plate. Wherefore, (concludes he) I think *Pyrophilus* ought " to be both deſired and incouraged to go on with his intended diſcourſe, ſince whether " gold be or be not the beſt of metals; an aſſurance, that it may be degraded, may

" prove a novelty very instructive, and perhaps more so than the transmutation of a " baser metal into a nobler. For I remember it hath long passed for a maxim among " chemical philosophers, that *facilius est aurum construere quam destruere:* And whatever " becomes of that, it is certain, that gold being the closest, the constantest, and the " least destructible of metals, to be able to work a notable and almost essential change " in such a body, (though, by deteriorating it) is more than to work a like change, " (though in popular estimation for the better) in any metal less indisposed to admit " alterations, especially in such an one as *Pyrophilus* intimates, by telling us, that it " was made by way of projection, and consequently by a very small proportion of " active matter; whereas the destructions, that vulgar chemists pretend to make of " gold, are wont to be attempted to be made by considerable proportions of corrosive " menstruums, or other fretting bodies; and even these experience shews to be usually " too weak to ruin, though sometimes they may much disguise the most stable texture " of gold. *Cuncta adeo miris illic complexibus hærent.*"

Pyrophilus perceiving by several signs, that he needed not add any thing of apologetical to what *Aristander* had already said for him, resumed his discourse, by saying, " I was going, gentlemen, when *Simplicius* diverted me, to tell you, that looking up" on the vulgar objections, that have been wont to be framed against the possibility of " metalline transmutations, from the authority and prejudices of *Aristotle*, and the " school-philosophers, as arguments, that in such an assembly as this need not now be " solemnly discussed; I consider, that the difficulties, that really deserve to be called " so, and are of weight even with mechanical philosophers, and judicious naturalists, " are principally these. First, That the great change, that must be wrought by " the elixir, (if there be such an agent) is effected upon bodies of so stable and al" most immutable a nature as metals. Next, That this great change is said to be " brought to pass in a very short time. And thirdly, (which is yet more strange) " that this great and sudden alteration is said to be effected by a very small, and per" haps inconsiderable, proportion of the transmuting powder. To which three grand " difficulties, I shall add another, that to me appears, and perhaps will seem to di" vers of the new philosophers, worthy to be looked upon as a fourth; namely, the " notable change, that must by a real transmutation be made in the specifick gravity " of the matter wrought upon: which difficulty I therefore think not unworthy to " be added to the rest, because upon several trials of my own and other men, I have " found no known quality of gold, (as its colour, malleableness, fixity, or the like) " so difficult, if not so impossible, to be introduced into any other metalline matter, " as the great specifick gravity, that is peculiar to gold. So that, gentlemen, (con" cludes *Pyrophilus*, if it can be made appear, that art has produced an anti-elixir, " (if I may so call it) or agent, that is able in a very short time, to work a very not" able, though deteriorating, change upon a metal; in proportion to which its quan" tity is very inconsiderable; I see not why it should be thought impossible, that art " may also make a true elixir, or powder capable of speedily transmuting a great pro" portion of a baser metal into silver or gold: especially if it be considered, that those, " that treat of these arcana, confess, that it is not every matter, which may be justly " called the philosopher's stone, that is able to transmute other metals in vast quanti" ties; since several of these writers, (and even *Lully* himself) make differing orders " or degrees of the elixir, and acknowledge, that a medicine or tincture of the first or " lowest order will not transmute about ten times its weight of an inferior metal.

Pyrophilus having at this part of his discourse made a short pause to take breath, *Cratippus* took occasion from his silence to say to him, " I presume, *Pyrophilus*, I " shall be disavowed by very few of these gentlemen, if I tell you, that the company

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" is impatient to hear the narrative of your experiment; and that if it do so much " as probably make out the particulars you have been mentioning, you will in likelihood persuade most of them, and will certainly oblige them all. I shall therefore on their behalf, as well as my own, solicite you to hasten to the historical part " of a discourse, that is so like to gratify our curiosity "

THE company having by their unanimous silence, testified their approbation of what *Cratippus* had said; and appearing more than ordinary attentive;

As I was one day abroad, saith *Pyrophilus*, to return visits to my friends, I was by a happy providence (for it was beside my first intention) directed to make one to an ingenious foreigner, with whom a few, that I had received from him, had given me some little acquaintance.

WHILST this gentleman and I were discoursing together of several matters, there came into visit him a stranger, whom I had but once seen before; and though that were in a promiscuous company yet he addressed himself to me in a way, that quickly satisfied me of the greatness of his civility; which he soon after also did of that of his curiosity. For the virtuoso, in whose lodgings we met, having (to gratify me) put him upon the discourse of his voyages; the curious stranger entertained us an hour or two with pertinent and judicious answers to the questions I asked him about places so remote, or so much within land, that I had not met with any of our English navigators or travellers, that had penetrated so far as to visit them. And because I found by his discourse, that I was like to enjoy such good company but a very little while, (since he told me, that he came the other day into *England* but to dispatch a business, which he had already done as far as he could do it, after which he was with speed to return, as (to my trouble) he did to his patron that sent him I made the more haste to propose such questions to him, as I most desired to be satisfied about; and among other things, enquiring whether in the eastern parts he had traversed, he had met with any chemists; he answered me, that he had; and that though they were fewer, and more reserved than ours, yet he did not find them at all less skilful. And on this occasion, before he left the town to go aboard the ship he was to overtake, he in a very obliging way put into my hands, at parting, a little piece of paper, folded up; which he said contained all that he had left of a rarity he had received from an eastern virtuoso, and which he intimated would give me occasion both to remember him, and to exercise my thoughts in uncommon speculations.

THE great delight I took in conversing with a person, that had travelled so far, and could give me so good an account of what he had seen, made me so much resent the being so soon deprived of it, that though I judged such a virtuoso would not, as a great token of kindness, have presented me a trifle, yet the present did but very imperfectly console me for the loss of so pleasing and instructive a conversation.

NEVERTHELESS, that I might comply with the curiosity he himself had excited in me, and know how much I was his debtor, I resolved to see what it was he had given me, and try whether I could make it do what I thought he intimated, by the help of those few hints rather than directions how to use it, which the parting haste he was in (or perhaps some other reason best known to himself) confined him to give me. But in regard that I could not but think the experiment would one way or other prove extraordinary, I thought fit to take a witness or two and an assistant in the trying of it; and for that purpose made choice of an experienced doctor of physick, very well versed in the separating and copelling of metals.

THOUGH the company, says *Heliodorus*, be so confident of your sincerity and wariness, that they would give credit even to unlikely experiments, upon your single testimony; yet we cannot but approve your discretion in taking an assistant and a witness, because becaufe in nice and uncommon experiments we can fcarce ufe too much circumfpection, efpecially when we have not the means of reiterating the trial: for in fuch new, as well as difficult cafes, it is eafy, even for a clear-fighted experimenter to over-look fome important circumftance, that a far lefs fkilful by-ftander may take notice of.

As I have ever judged, faith *Pyrophilus*, that cautioufnefs is a very requifite qualification for him, that would fatisfactorily make curious experiments; fo I thought fit to imploy a more than ordinary meafure of it, in making a trial, whofe event I imagined might prove odd enough. And therefore having feveral times obferved, that fome men are prepoffeffed, by having a particular expectation raifed in them, and are inclined to think, that they do fee that happen, which they think they fhould fee happen; I refolved to obviate this prejudication as much as innocently I could, and (without telling him any thing but the truth, to which philofophy, as well as religion, obliges us to be ftrictly loyal) I told him but thus much of the truth, that I expected, that a fmall proportion of a powder prefented me by a foreign virtuofo would give a brittlenefs to the moft flexible and malleable of metals, gold itfelf. Which chance I perceived he judged fo confiderable and unlikely to be affected, that he was greedy of feeing it feverely tried,

Having thus prepared him not to look for all that I myfelf expected, I cautioufly opened the paper I lately mentioned, but was both furprized and troubled (as he alfo was) to find in it fo very little powder, that inftead of two differing trials, that I defigned to make with it, there feemed very fmall hope left, that it would ferve for one (and that but an imperfect one neither). For there was fo very little powder, that we could fcarce fee the colour of it (fave that, as far as I could judge, it was of a darkifh red) and we thought it not only dangerous, but ufelefs to attempt to weigh it, in regard we might eafily lofe it by putting it into, and out of the balance; and the weights we had were not fmall enough for fo defpicable a quantity of matter; which in words I eftimated at an eighth part of a grain; but my affiftant (whofe conjecture I confefs my thoughts inclined to prefer) would allow it to be at the moft but a tenth part of a grain. Wherefore feeing the utmoft we could reafonably hope to do with fo very little powder, was to make one trial with it, we weighed out in differing balances two drams of gold, that had been formerly Englifh coin, and that I caufed by one, that I ufually imploy, to be cupelled with a fufficient quantity of lead, and quarted, as they fpeak, with refined filver, and purged aqua-fortis, to be fure of the goodnefs of the gold. Thefe two drams I put into a new crucible, firft carefully nealed, and having brought them to fufion by the mere action of the fire, without the help of borax, or any other additament (which courfe, though fomewhat more laborious than the moft ufual, we took to obviate fcruples) I put into the well-melted metal with my own hand the little parcel of powder lately mentioned, and continuing the veffel in the fire for about a quarter of an hour, that the powder might have time to difufe itfelf every way into the metal, we poured out the well-melted gold into another crucible that I had brought with me, and that had been gradually heated before to prevent cracking. But though from the firft fufion of the metal to the pouring out, it had turned in the crucible like ordinary gold, fave that once my affiftant told me, he faw that for two or three moments it looked almoft like an opale; yet I was fomewhat furprized to find, when the matter was grown cold, that though it appeared upon the balance, that we had not loft any thing of the weight we put in, yet, inftead of fine gold, we had a lump of metal of a dirty colour, as it were overcaft with a thin coat, almoft like half vitrified litharge; and fomewhat to increafe the wonder, we perceived, that there ftuck to one fide of the crucible a little globule of metal, that looked not at all yellowifh, but like coarfe filver, and the bottom of the crucible was overlaid with a vitrified fubftance, whereof one part was of a tranſparent yellow, and the other of a deep brown, inclining to red; and in this vitrified ſubſtance I could plainly perceive ſticking at leaſt five or ſix little globules, that looked more like impure ſilver than pure gold. In ſhort, this ſtuff looked ſo little like refined, or ſo much as ordinary, gold, that though my friend did much more than I marvel at this change, yet I confeſs I was ſurprized at it myſelf. For though in ſome particulars it anſwered what I looked for, yet in others, it was very differing from that, which the donor of the powder had, as I thought, given me ground to expect. Whether the cauſe of my diſappointment were, that (as I formerly intimated) this virtuoſo's haſte or deſign made him leave me in the dark; or whether it were, that finding myſelf in want of ſufficient directions, I happily pitched upon ſuch a proportion of materials and way of operating, as were proper to make a new diſcovery, which the excellent giver of the powder had not deſigned, or perhaps thought of.

I SHALL not at all wonder, ſaith *Cratippus*, either at your friend's amazement, or at your ſurprize, if your further trials did in any meaſure confirm what the ſuperficial change, that appeared in your metal, could not but incline you to conjecture.

You will beſt judge of that (replies *Pyrophilus*) by the account I was going to give you of what we did with our odd metal. And firſt, having rubbed it upon a good touchſtone, whereon we had likewiſe rubbed a piece of coined gold, we manifeſtly found, that the mark left upon the ſtone by our maſs, between the marks of the two other metals, was notoriouſly more like the touch of the ſilver than that of the gold. Next, having knocked our little lump with a hammer, it was (according to my prediction) found brittle, and flew into ſeveral pieces. Thirdly (which is more) even the inſides of thoſe pieces looked of a baſe dirty colour, like that of braſs or worſe, for the fragments had a far greater reſemblance to bell-metal, than either to gold or to ſilver. To which we added this fourth, and more conſiderable examen; that having carefully weighed out one dram of our ſtuff (reſerving the reſt for trials to be ſuggeſted by ſecond thought) and put it upon an excellent new and well-nealed coppel, with about half a dozen times its weight of lead, we found, ſomewhat to our wonder, that though it turned very well like good gold, yet it continued in the fire above an hour and an half (which was twice as long as we expected) and yet almoſt to the very laſt the fumes copiouſly aſcended, which ſufficiently argued the operation to have been well carried on; and when at laſt it was quite ended, we found the coppel very ſmooth and intire, but tinged with a fine purpliſh red (which did ſomewhat ſurprize us;) and beſides, the refined gold, there lay upon the cavity of the coppel ſome dark-coloured recrements, which we concluded to have proceeded from the deteriorated metal, not from the lead. But when we came to put our gold again into the balance, we found it to weigh only about fifty three grains, and conſequently to have loſt ſeven; which yet we found to be fully made up by that little quantity of recrements, that I have lately mentioned, whoſe weight and fixity, compared with their unpromiſing colour, did not a little puzzle us, eſpecially becauſe we had not enough either of them, or of leiſure, to examine their nature. To all which circumſtances I ſhall ſubjoin this, that to prevent any ſcruples, that might ariſe touching the gold we employed, I cauſed a dram and a half, that had been purpoſely reſerved out of the ſame portion with that, which had been debaſed; I cauſed this (I ſay) to be in my aſſiſtant's preſence melted by itſelf, and found it (as I doubted not but I ſhould do) fine and well coloured gold.

I hope you will pardon my curioſity, ſaith *Ariſtander* to the gentleman that ſpoke laſt, if I aſk, why you take no notice of the effect of aqua-fortis upon your imbaſed metal? Your queſtion, replies *Pyrophilus*, I confeſs to be very reaſonable, and I am ſomewhat troubled, that I cannot anſwer it but by telling you, that we had not at hand any aqua-fortis we durſt rely on; which yet I was the leſs troubled at, becauſe heretofore ſome trials purpoſely made had informed me, that in ſome metalline mixtures the gold, if it were much predominant in quantity, may protect another metal, (for inſtance ſilver,) from being diſſolved by that menſtruum, though not from being at all invaded by it.

THERE yet remained, ſaith *Heliodorus*, one examen more of your odd metal, which would have ſatisfied me, at leaſt as much as any of the reſt, of its having been notably imbaſed: for if it were altered in its ſpecific gravity, that quality I have always obſerved (as I lately perceived you alſo have done) to ſtick ſo cloſe to gold, that it could not by an additament ſo inconſiderable in point of bulk, be conſiderably altered without a notable and almoſt eſſential change in the texture of the metal.

To this pertinent diſcourſe, *Pyrophilus*, with the reſpect due to a perſon, that ſo worthily ſuſtained the dignity he had of preſiding in that choice company, made this return: I owe you, ſir, my humble thanks for calling upon me to give you an account I might have forgotten, and which is yet of ſo important a thing, that none of the other phænomena of our experiment ſeemed to me to deſerve ſo much notice. Wherefore I ſhall now inform you, that having provided myſelf of all the requiſites to make hydroſtatical trials (to which perhaps I am not altogether a ſtranger) I carefully weighed in water the ill-looked maſs (before it was divided for the coppelling of the above-mentioned dram) and found, to the great confirmation of my former wonder and conjectures, that inſtead of weighing about nineteen times as much as a bulk of water equal to it, its proportion to that liquor was but that of fifteen and about two thirds to one: ſo that its ſpecific gravity was leſs by about one-third than it would if it had been pure gold.

AT the recital of this notable circumſtance, ſuperadded to the reſt, the generality of the company, and the preſident too, by looking and ſmiling upon one another, expreſſed themſelves to be as well delighted as ſurprized; and after the murmuring, occaſioned by the various whiſpers that paſſed amongſt them, was a little over, *Heliodorus* addreſſed himſelf to *Pyrophilus*, and told him; I need not, and therefore ſhall not, ſtay for an expreſs order from the company to give you their hearty thanks: for as the obliging ſtranger did very much gratify you by the preſent of his wonderful powder, ſo you have not a little gratified us by ſo candid and particular a narrative of the effects of it; and I hope (continues he) that if you have not yet otherwiſe diſpoſed of that part of your deteriorated gold that you did not coppel, you will ſometime or other favour us with a ſight of it.

I join in this requeſt, ſaid *Cratippus*, as ſoon as he perceived the preſident had done ſpeaking; and to facilitate the grant of it, I ſhall not ſcruple to tell *Pyrophilus*, he may be confident, that the degradation of his gold will not depreciate it amongſt us: ſince if it be allowable for opinion to ſtamp ſuch a value upon old coins and medals, that in the judgment of good antiquaries, a ruſty piece of braſs or copper, with a half defaced image or inſcription on it, is to be highlier valued than as big a piece of well-ſtamped gold; I ſee not, why it ſhould not be lawful for philoſophers to prize ſuch a lump of depraved gold as yours before the fineſt gold the chymiſts or mint-maſters are wont to afford us. And though I freely grant, that ſome old copper medals are of good uſe in hiſtory, to keep alive by their inſcriptions the memory of the taking of a town, or the winning of a battle; though theſe be but things, that almoſt every day are ſome where or other done, yet I think *Pyrophilus*'s imbaſed metal is much to be preferred, as not only preſerving the memory, but being an effect of ſuch a victory of art over nature, and the conquering of ſuch generally believed inſuperable difficulties, as no ſtory that I know of gives us an example of.

As ſoon as ever *Cratippus* had made a pauſe, *Pyrophilus*, to prevent complimental diſcourſe, did in a few words tell the preſident, that his part had been but that of a relator of matter of fact; and that therefore he could deſerve but little thanks and no praiſe at all; though a good meaſure of both of them were due to the obliging virtuoſo, that had given him the powder; and in that, the opportunity of complying with his duty, and his inclination, to ſerve that learned company.

THESE gentlemen, ſaith *Ariſtander*, are not perſons, among whom modeſty is either reſtrained from expreſſing itſelf, or conſtrued according to the letter; and therefore whatever you have been pleaſed to ſay, the company cannot but think itſelf much obliged to you; and I know the obligation would be much increaſed, if you would favour us with your reflections upon the extraordinary experiment you have been pleaſed to relate to us.

IF, replies *Pyrophilus*, I had had wherewithal to repeat the experiment, and vary it according to the hints afforded me by the firſt trial, I ſhould be leſs unfit to comply with *Ariſtander*'s motion: but the phænomena are too new and too difficult for me to attempt to unriddle them by the help of ſo ſlender an information as a perſon ſo little ſagacious as I could get by a ſingle trial; and though I will not deny, that I have had ſome roving thoughts about this puzzling ſubject, yet I hope I ſhall be eaſily pardoned, if I decline to preſent crude and immature thoughts to a company, that ſo well deſerve the moſt ripe ones, and can ſo ſkilfully diſcover thoſe that are not ſo.

I confeſs, ſaith *Heliodorus*, that I think *Pyrophilus*'s warineſs deſerves, not only to be allowed, but imitated; and therefore by my conſent the further diſcourſe of ſo abſtruſe a ſubject ſhall be deferred, till we ſhall have had time to conſider ſeriouſly of phænomena, that will be ſure to employ our moſt ſpeculative thoughts, and I fear to poſe them too: only we muſt not forget, that *Pyrophilus* himſelf ought to be not barely allowed, but invited to draw, before we riſe, what corollalaries he thinks fit to propoſe from what he hath already delivered.

THE inference, ſaid *Pyrophilus*, I meant to make, will not detain you long; having for the main been already intimated in what you may remember I told you I deſigned in the mention I was about to make of the now recited experiment. For without launching into difficult ſpeculations, or making uſe of diſputable hypotheſes, it ſeems evident enough from the matter of fact faithfully laid before you, that an operation very near, if not altogether as ſtrange as that, which is called projection, and in the difficulteſt points much of the ſame nature with it, may ſafely be admitted, For our experiment plainly ſhews, that gold, though confeſſedly the moſt homogeneous, and the leaſt mutable of metals, may be in a very ſhort time (perhaps not amounting to many minutes) exceedingly changed, both as to malleableneſs, colour, homogeneity, and (which is more) ſpecific gravity; and all this by ſo very inconſiderable a portion of injected powder, that ſince the gold that was wrought on weighed two of our Engliſh drams, and conſequently an hundred and twenty grains, an eaſy computation will aſſure us, that the medicine did thus powerfully act, according to my eſtimate (which was the modeſteſt) upon near a thouſand times (for it was above nine hundred and fifty) its weight of gold, and according to my aſſiſtant's eſtimate did (as they ſpeak) go near upon twelve hundred; ſo that if it were fit to apply to this anti-elixir (as I formerly ventured to call it) what is ſaid of the true elixir by divers of the chymical philoſophers, who will have their virtue of their ſtone increaſed in ſuch a proportion, as that at firſt it will tranſmute but ten times its weight, after the next rotation an hundred times, and after the next to that a thouſand; our powder may in their language be ſtiled a medicine of the third order.

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THE computation, saith *Aristander*, is very obvious, but the change of so great a portion of metal is so wonderful and unexampled that I hope we shall among other things learn from it this lesson, that we ought not to be so forward, as many men otherwise of great parts are wont to be, in prescribing narrow limits to the power of nature and art, and in condemning and deriding all those that pretend to, or believe, uncommon things in chymistry, as either cheats or credulous. And therefore I hope, that though (at least in my opinion) it be very allowable to call fables, fables, and to detect and expose the impostures or deceits of ignorant or vain-glorious pretenders to chymical mysteries; yet we shall not, by too hasty and general censures of the sober and diligent indigators of the arcana of chymistry, blemish (as much as in us lies) that excellent art itself, and thereby disoblige the genuine sons of it, and divert those, that are indeed possessors of noble secrets, from vouchsafing to gratify our curiosity, as we see that one of them did *Pyrophilus*'s, with the sight at least of some of their highly instructive rarities.

I WHOLLY approve, saith *Heliodorus* rising from his seat, the discreet and seasonable motion made by *Aristander*.

AND I presume, subjoins *Pyrophilus*, that it will not be the less liked, if I add, that I will allow the company to believe, that as extraordinary, as I perceive most of you think the phænomena of the lately recited experiment; yet I have not (because I must not do it) as yet acquainted you with the strangest effect of our admirable powder.

The AERIAL NOCTILUCA: Or, some NEW PHÆNOMENA, and a Process of a factitious self-shining Substance.

An ADVERTISEMENT of the PUBLISHER to the READER.

THE honourable author of the following papers, thinking it probable, that the processes delivered in them, having hitherto been published by no man, will, as well for that reason, as for the nobleness of the subject, prove not unwelcome to the curious, in divers countries, where English is not understood: he was very willing, for their sakes, that this tract should be turned into Latin. And now, to prevent the needless pains of any, that may have a mind to make such a version, without having the opportunity to consult the author, upon any doubt of his meaning, I think fit to give notice, that the translation is, by the author's consent, made already, and, God permitting, will quickly appear in public. Perhaps it will not be improper to add, that the reason, why the following English tract is printed in octavo (as they speak) is, that it may be conveniently bound up, either with the notes, already published in the same form about divers particular qualities, or with those other notes, that yet remain to be published about other qualities; to whose number light and inflammability may be referred. The ensuing discourse having been written to a virtuoso living in the country, who has been for many years absent from London, it was thought fit in the begining of these papers to give him some informations about phosphorus's, and their several kinds in general; but it was not thought fit to publish at the beginning of the letter any thing of compliment; since in that, neither the main subject, nor the reader, was concerned.

To my very learned FRIEND Dr. J. B.

S I R,

TO gratify your curiosity about phosphoruses, as much as I can without indiscretion at present do, I must, in the first place, take notice to you, that though phosphoruses may well be distinguished into two sorts; those that may be stiled natural, as glow-worms, some sorts of rotten wood and fishes, and a few others; and those that are properly artificial: yet waving, at present, further mention of the former sort of bodies, that without manifest heat shine in the dark (which absence of sensible heat distinguishes phosphoruses from common fire and flame;) I shall now confine my discourse to the latter sort, and tell you, that as far as I have hitherto observed, those factitious shining bodies, that do or may pass under the name of phosphoruses, may be reduced to two principal kinds, one of which may be subdivided into two or three, so that in all they will amount to three or four.

The first of these consists of such bodies, as shine only by the help of external illustration, or (if you please) such bodies, as being exposed to the beams of the sun, or those of a vigorous flame, will retain a lucidness, and continue to shine some time in the dark. Of this kind is the Bolonian stone, skilfully prepared; and of this sort also is the Phosphorus Hermeticus of Balduinus, of whose phænomena, but not the way of making it, the author has given the learned world an account. This phosphorus was therefore very welcome to divers of the curious, because the Bolonian stone was for some years before grown very rare, even in its own country, *Italy*, which scarceness, an ingenious traveller, than lately come out of those parts, told me he imputed to the death of the person, that used to prepare the stone at *Bologna*, without having left a sufficient account of his way of making it lucid. And the phosphorus of Balduinus, which, or the like, may be made (as I have tried) both of chalk, and another substance, seemed to me, when the preparation succeeded best, to catch the external light (if I may so speak) far more readily than the Bolonian stone: for I remember I have had one, that being freshly made, would within about half a minute of an hour be manifestly excited, and as it were kindled; so that being presently removed into a dark place, it would retain a very sensible light, for so many times as long as it had been exposed to the beams of the external light; and this (if I much mis-remember not) was even when that external light was but the flame of a candle.

But, on the other side, whereas I have more than once or twice observed, with trouble, that these phosphoruses could very hardly be preserved for any long time (which I was apt to impute to the action of insinuating air) so that some of them in not many months, and others even in a few weeks (or perhaps days) would appear cracked, and lose their virtue of being excited by the beams of light; the Bolonian stone, skilfully prepared, would retain its virtue for a much longer time: for I remember (whatever learned men have delivered to the contrary) I had a small piece of it, which, though I kept it negligently enough in an ordinary little wooden box, retained its virtue for several years after I had it, which was not till a great while after it was first prepared. What I have further observed concerning the Phosphorus Hermeticus, I have not now the leisure to acquaint you with.

But besides this first kind of phosphoruses that to be able to shine, must have their faculty excited by the beams of the sun, or those of some other actually shining body: there is another sort, which needs not be previously illustrated by any external lucid, and yet continues to shine far longer than the Bolonian stone, or the phosphorus of Balduinus. This by some learned men has been called, to discriminate it from the former, a noctiluca; which, though in strictness I cannot think it as proper a name as could be be wiſhed, ſince the other phoſphorus will ſhine in the night as well as the day, if it be excited with the flame of a culinary fire, or of a large candle; yet ſince the name has been received by ſeveral, and ſince it is not eaſy in our language to expreſs the thing clearly in one word, I ſhall (though for brevity, as much as diſtinction-ſake) admit the uſe of this name, yet without forbearing ſometimes to ſubſtitute for it that of a ſelf-ſhining ſubſtance, which is more expreſſive of its nature. Of this ſubſtance Mr. *Daniel Kraft*, a German Chymiſt, ſhewed his majeſty two ſorts or degrees. To the firſt of which I took the liberty to give the name of conſiſtent (or gummous) noctiluca, not in that ſenſe, wherein the word is oppoſed to ſoft, for this ſubſtance was at leaſt as yielding as bees-wax in ſummer; but as the conſiſtent is employed as equivalent to firm, and oppoſed to liquid and fluid. By reaſon alſo of its ſomewhat viſcous texture, not very unlike that of gum of cherries, and ſome others newly taken from the tree, it may be called the gummous noctiluca: and, I am informed, on the ſcore of its uninterrupted action, it is called by ſome in *Germany*, the conſtant noctiluca; which title it does not ill deſerve, ſince this phoſphorus is much the nobleſt we have yet ſeen. For though there were not much of it, and though it were kept by itſelf in a little vial, well ſtopped, it would, without being externally excited, inceſſantly ſhine, as he affirmed, both day and night. Yet the light it afforded ſeemed but little, if at all, more vivid, than I have ſometimes obſerved in the liquor of glow-worms, and ſome other phoſphoruſes of nature's producing: nor had the poſſeſſor enough of this ſubſtance to invite his conſent to any trial to improve it, the quantity he had at *London* ſcarce exceeding in bulk the kernel of an almond.

Besides this gummous noctiluca, Mr. *Kraft* had a liquid one, that, perhaps, was made only by diſſolution of the former in water, or ſome convenient liquor; but the lucidneſs of this was not permanent like that of the other, as I have noted in another paper; but within no very long time, eſpecially when it was divided into ſmaller portions, and left expoſed to the air, would expire or vaniſh.

But beſides the gummous and the liquid noctiluca hitherto mentioned, I know not whether we may not add a third kind, that we ourſelves lately prepared, which ſeems to be of a ſomewhat differing nature, both from the conſiſtent, and the liquid noctiluca newly deſcribed, at leaſt as far as I obſerved their phænomena. For this of ours would not ſhine of itſelf, like the conſtant noctiluca, nor yet in that manner, that the liquid noctiluca did; but the bare contact of the air, without any external illuſtration or heat, would immediately produce a light (which might eaſily be made to laſt a good while in a well ſtopped veſſel;) and, which is conſiderable, the ſubſtance, that ſhined, was not the body of the liquor included in the vial, but an exhalation or effluvium mingled with the admitted air: for both which reaſons, I gave in the name Aerial Noctiluca.

These are the ſeveral phoſphoruſes, that I have yet had opportunity to ſee; but, for ought I know, their variety may extend ſomewhat further, becauſe I have heard of a paper printed in *Germany* by an ingenious man, whoſe name (if I miſtake not) is *El-ſholtz*, wherein particular mention is made, in an hiſtorical way, of the German Noctiluca: but this paper I cannot yet procure, and therefore you would do well to conſult it, if you can get it; and I am not averſe from thinking, that future induſtry may diſcover ſome new kinds or variations of ſelf-ſhining ſubſtances, that will deſerve new names, and among them, perhaps, that of ſolid Noctilucas.

Having ſaid thus much of the ſeveral ſorts of artificial phoſphoruſes, I ſhall be very brief in ſpeaking of their inventors, whereof I have but an imperfect information. For though I find it generally agreed, that the Phoſphorus Hermeticus was firſt found and publiſhed to the world by the learned and ingenious *Balduinus*, a German lawyer;

yet as to the gummous and liquid noctilucas, I find the firſt invention is by ſome aſcribed to the above-mentioned Mr. *Kraft*, (though I remember not, that when he was here, he plainly aſſerted it to himſelf;) by others, atttributed to an antient chymiſt, dwelling at *Hamburgh*, whoſe name (if I miſtake not) is Mr. *Branc*, and by others again, with great confidence, aſſerted to a famous German chymiſt in the court of *Saxony*, called *Kunckelius*. But to which of theſe ſo noble an invention, as that of the two German noctilucas, is juſtly due, I neither am qualified nor deſirous to judge; and therefore, without prejudicing any man's right, I will proceed to that, which, I preſume, is the chief thing you would know of me, namely, *An account of the occaſion and ſteps of my own attempt to make a Noctiluca.* Concerning this I ſhall give you the following narrative, wherein, though my urgent avocations will not (I fear) permit me to be other than immethodical, yet I ſhall not decline to mention ſome circumſtances, that I know may be omitted, becauſe they will not, perhaps, be found ſo barely hiſtorical, but that they may prove of ſome uſe to a leſs ſagacity than yours, in an enquiry into a ſubject, wherein I cannot yet plainly tell you all you could wiſh to know, and which is both new and abſtruſe, as well as noble.

AFTER the experienced chymiſt Mr. *Daniel Kraft* had, in a viſit that he purpoſely made me, ſhewn me and ſome of my friends, both his liquid and conſiſtent phoſphorus, being by the phænomena I then obſerved, (and whereof the curious have ſince had publick notice*,) made certain, that there is really ſuch a factitious body to be made, as would ſhine in the dark, without having been before illuſtrated by any lucid ſubſtance, and without being hot as to ſenſe: after this, I ſay, I took into conſideration, by what ways it might be moſt probable, to produce, by art, ſuch a ſhining ſubſtance. To ſeek for which I was both inclined, and hopeful to be ſomewhat aſſiſted, becauſe I had lying by me, among my yet unpubliſhed notes of the mechanical origin of divers qualities, a collection of ſome obſervations and thoughts concerning light. And I was alſo the more encouraged to attempt ſomewhat this way, becauſe having, at Mr. *Kraft*'s deſire, imparted to him ſomewhat, that I diſcovered about uncommon mercuries, (which I had then communicated but to one perſon in the world) he, in requital, confeſt to me at parting, that at leaſt the principal matter of his phoſphoruſes was ſomewhat that belonged to the body of man. This intimation, though but very general, was therefore very welcome to me, becauſe, though I have often thought it probable, that a ſhining ſubſtance may, by ſpagyrical art, be obtained from more kinds of bodies than one: yet deſigning in the firſt place, to try, if I could hit upon ſuch a phoſphorus as I ſaw was preparable, the advertiſement ſaved me (for ſome time) the labour of ranging among various bodies, and directed me to exerciſe my induſtry in a narrower compaſs. But there being divers parts of the human body, that have been taken to taſk by chymiſts, and, perhaps, by me as carefully, as by ſome others, my choice might have been diſtracted between the blood, the ſolid excrements, the bones, the urine, and the hair, of the human body; if various former trials and ſpeculations upon more than one of thoſe ſubjects had not directed me to pitch upon that, which was fitteſt to be choſen, and of which, as I had formerly ſet down divers experiments and obſervations, ſo I had made proviſion of a quantity of it, and ſo far prepared it, that it wanted but little of being fit for my preſent purpoſe. But before I had made any great progreſs in my deſign, I was by divers removes, indiſpoſitions of body, law-ſuits, and other avocations, ſo diſtracted, or at leaſt diverted, that I laid aſide the proſecution of the phoſphorus for a long time. And when afterwards I reſumed it, though I wrought upon the right matter, yet I was diverted from the right way,

* This clauſe refers to one of the *Philoſophical Collections* publiſhed by the ingenious Mr. *Hook*, who hath therein inſerted *verbatim* the paper he received from Mr. *Boyle*.

way, by a procefs that I received from beyond fea, as a great arcanum, that would certainly produce the noctiluca afpired to; for partly upon this account, but more, becaufe I faw that the chief ingredient in this procefs was that, which I, with reafon, took to be the beft matter, I was induced to purfue the prefcribed method for fome months, but without fuccefs; the true matter being, as I concluded, too much either altered or clogged by the additional ingredients, that were defigned to improve it; befides, that the degree of fire, though a circumftance of the greateft moment, was overlooked, or not rightly prefcribed. However, adhering to the firft choice I had made of a fit matter, I did not defift to work upon it by the ways I judged the moft hopeful, when a learned and ingenious ftranger, (*A. G.* M. D. countryman, if I miftake not, to Mr. *Kraft*) who had newly made an excurfion into *England*, to fee the country, having, in a vifit he was pleafed to make me, occafionally difcourfed, among other things, about the German noctiluca, whereof he foon perceived I knew the true matter, and had wrought much upon it; he faid fomething about the degree of fire, that made me afterwards think, when I reflected on it, that that was the only thing I wanted to fucceed in my endeavours. And there was the more reafon to think fo, becaufe for want of a due management of the fire, we had divers times failed of making the phofphorus of *Balduinus*, not only after we had more than once wrought upon the right matter, but after we had actually made the phofphorus. Wherefore when he left *London*, having yet fome quantity of the matter in fuch readinefs, that it needed but the fire to let me fee, what I ought to think of the hint the ingenious traveller had given me, I caufed the trial to be renewed, which, proving unfuccefsful, diminifhed much of my ftock of prepared matter, but it did not fo difcourage me, as to hinder me from reiterating the attempt (without much varying it) with a good part of what remained. And though at this time alfo all the care and diligence, that could be employed, did not hinder an unlucky mifcarriage, that kept the trial from being fully fatisfactory; yet being confident upon the nature of the thing, I would not believe the fkilful laborant, when he told me with trouble, that what I expected, was not at all produced: but going myfelf to the laboratory, I quickly found, that by the help of the air, or fome agitation of what had paffed into the receiver, I could, in a dark place (though it was then day) perceive fome glimmerings of light, which, you will eafily believe, I was not ill pleafed to fee. And now you have the hiftory of my purfuit of the liquid phofphorus, that has made fome noife among the curious: but I freely confefs, that the fuccefs, though welcome, was not fo full as I aimed at, for I obtained no fuch confiftent phofphorus as that, whereof Mr. *Kraft* fhewed me, as I formerly told you, a fmall parcel. But as I was willing to think, that this defect may be imputed to the cracking of the retort, before the operation was quite finifhed, fo I hope another diftillation in a more luckily chofen veffel may make me amends for the newly mentioned mifcarriage, and thereby enable me to difcover other, and perhaps nobler phænomena of our fhining fubftance, than hitherto I have been able to obferve. Efpecially confidering, that the fame misfortune, that I hope was the principal caufe of my miffing the nobleft thing I aimed at, the conftant noctiluca, left me fo little even of liquid matter, fit for my purpofe, that I have not dared, for fear of wafting it, to try feveral things with it, that I prefume may be of good ufe in an enquiry into the nature of this light, and perhaps alfo of light in general. And becaufe I fear by what I have obferved, that, though the veffel had not cracked, yet the matter diftilled would have afforded but a fmall proportion of lucid fubftance, I am the more unwilling to fall upon this troublefome work again, till, befides other requifites, I be provided of a competent quantity of a matter, which I fear contains but very little of the defired fubftance. However, I have endeavoured to make that ufe of our experiment, fuch as it was, that though the noctiluca it produced, be not perhaps ſo lucid as that of Mr. *Kraft's* yet it may prove as luciferous as his hath hitherto been, ſince (as you will ſee hereafter) I have found a ſubſtance, that needs the air, and nothing but the air to kindle it, and that in a moment.

IN this narrative I have been the more particular, that it may ſhew you, (what I hope may make you amends for the length of it) that an inquiſitive man ſhould not always be deterred by the difficulties, or even diſappointments he may meet with, in proſecuting a noble experiment, as long as he judges himſelf to proceed upon good and rational grounds.

THE uſes, that may be made of noctilucas, eſpecially of the conſiſtent, are not, in probability, all of them to be eaſily foreſeen and declared; eſpecially by me, who have not yet had time and ability to make thoſe improvements of ſelf-ſhining ſubſtances, that, by the aſſiſtance of the father of lights, I hope will, in proceſs of time, be attained. If the lucid virtue of the conſtant noctiluca could be (as I ſee not, why it may not be) conſiderably invigorated, it may prevent a great deal of danger, to which men of war, and other ſhips are expoſed, by the neceſſity men often have to come into the gun-room with common flames or fire, to take out powder, which has occaſioned the blowing up of many a brave ſhip. Our light may, perhaps, be of uſe to thoſe, that dive in deep waters; and alſo may very ſafely and conveniently be let down into the ſea, to what depth one pleaſes, and kept there a long time, to draw together the fiſhes, that are wont to reſort to the light of a fire or candle; as in divers parts of *Scotland* and *Ireland* is well known to the fiſhermen, who get much profit by this reſort. The ſame ſelf-ſhining ſubſtance, which in our aërial noctiluca affords a light, that, as faint as it yet is, was able, when I waked in the night, to ſhew me diſtinctly enough the bigneſs and ſhape of ſome joints of my fingers, and to diſcover itſelf in the ſhape of a capital letter of the alphabet, that was cut out of a piece of blacked paper paſted upon the vial; this light, I ſay, may probably, (at leaſt when ſomewhat invigorated) ſuffice to ſhew the hour of the night when one wakes, (with eyes unaccuſtomed to light) if it be placed, inſtead of a lamp or candle, behind an index, where the figures employed to mark the hours are cut out. It may alſo ſerve to make a guide knowable at a good diſtance off, in ſpite of tempeſtuous winds and great ſhowers, and this in the darkeſt night. Divers ludicrous experiments, very pleaſant and ſurprizing, may be made with the noctiluca, by him that has enough of it. But theſe trifles, though very pretty in their kind, I purpoſely paſs over: as alſo an uſe, that may be of great, but I fear of miſchievous, conſequences; reſerving what I have further to ſay of the uſefulneſs of theſe ſelf-ſhining ſubſtances, till time ſhall give me more information and leiſure. In the mean while I ſhall only intimate, that probably the utilities, that ſo ſubtle and noble a ſubſtance may be brought to afford in medicine, may be more conſiderable than any of its other particular uſes; and that though our noctiluca had none of theſe, yet it may be highly valuable, if it ſhall (as in all likelihood it will) be found conducive to diſcover the nature of ſo noble a ſubject, as light, whoſe encomiums would require more time than I can allow this writing. And perhaps they will ſeem needleſs, when I ſhall have obſerved, that light was the firſt corporeal thing the great creator of the univerſe was pleaſed to make; and that (as our excellent *Bacon* has well noted, to another purpoſe) he was pleaſed to allot the whole firſt day to the creation of light alone, without aſſociating with it in that honour any other corporeal thing.

THESE things being premiſed, I ſhall proceed to what I chiefly intended in this paper, *viz.* the mention of the obſervations themſelves; as ſoon as, to facilitate the underſtanding of them, I ſhall have advertiſed you, that though I fear it will always be difficult to get out without loſs the ſelf-ſhining ſubſtance raiſed by diſtillation, yet in our experiment, becauſe the veſſels would not hold out intire to the laſt, we had more difficulty,

culty, than even we expected, to get out the luciferous matter, and were fain to save, as much as we could of it, by small parcels, in distinct vials. Whereof that, which was first employed, though it was judged to have received the vigorousest portion of the shining liquor; yet for a reason I elsewhere intimated, (and because it was not at hand, when I had first the opportunity to use it) I thought fit to make my trials with the noctiluca, I had saved in the second vial; setting aside some more faint and aqueous liquor, that was afterwards saved in a third vial; and a thicker stuff, that remained upon the paper, when some of the liquor had been put into it to be filtrated. Which paper was kept in a fourth glass, which, though (that it might admit the paper and adhering luciferous stuff) it was wide-mouthed, yet was it kept carefully stopt. Of the phænomena I observed in the second of these four glasses, I shall, God permitting, at this time, give you a short account; designing, if my haste will give me leave, to add some particulars, that I may afterwards observe in those portions of our noctiluca, that were received in the three other glasses.

OBSERVATIONS made by Mr. *Boyle* about the AERIAL NOCTILUCA contained in his Second Vial.

Note, *That this vial was capable of holding, by our guess, about two ounces of water, but there was not in it above one small spoonful of our liquor.*

OBSERVATION I.

THE liquor, that afforded the aerial noctiluca, (for which reason, and for brevity, I often call it the shining liquor) by day-light was not near diaphanous, and appeared muddy, and of a greyish colour, somewhat like common water, rendered opacous, by having a quantity of wood-ashes mingled with it.

OBSERVATION II.

WHEN no light appeared in the glass, we observed all the cavity of the vial, that reached from the liquor to the neck, to be transparent, as if there were nothing in the glass, save a spoonful of dirty water at the bottom.

OBSERVATION III.

BUT when the liquor was made to shine vividly, then all the cavity of the glass, untaken up by the liquor, appeared in an external light to be full of fumes. And this seeming smoke, being, in the vial that contained it, removed into a dark place, appeared lucid, and sometimes looked like a flame, that seemed to be reverberated, and to be made, as it were, to circulate by the close stopped neck and the sides of the vial. And the appearance of whitish fumes, when the glass was looked upon in an external light, was so usual a concomitant of its fitness to shine in the dark, that by looking upon the vial by day-light, I could readily tell, by the presence or absence of the whitish mist above-mentioned, whether the matter would, in a dark place, appear luminous or not.

OBSERVATION IV.

WHEN this liquor had been kept for a competent time (as an hour or two, and sometimes much less) in some dark and quiet place, or even in my pocket; if in a darkened room my eyes were caſt toward the place, where the vial was held, I could not perceive it to afford any light at all. And, though I ſhaked the liquor ſtrongly enough, to give it at leaſt a moderate agitation, yet I could not diſcern, that this motion alone was able to bring the included liquor, or the vapours it may be ſuppoſed to have ſent up, to be manifeſtly lucid.

OBSERVATION V.

BUT as ſoon as I unſtopt the vial in the dark, there began to appear, as I expected, a light or flame in the cavity of it. I call it light or flame, becauſe I dare not yet ſpeak dogmatically of it; though it agrees with flame in divers particulars, and though alſo I am not ſure, that all flames muſt agree in all points with common flames, experience having taught me the contrary; and particularly, that ſome flames will burn, and be propagated in cloſe-ſtopt veſſels. I ſhall therefore on this account, and for brevity's ſake, allow the aggregate of our ſhining fumes the name of flame, (which *Ariſtotle* himſelf ſomewhere ſtiles *fumus accenſus)* but without poſitively aſſerting, that it deſerves it, unleſs further phænomena ſhall be found to intitle it thereunto. But whatever be the nature and ſubject of this light, the light itſelf appeared to have, in great part, a dependance on the freſh air, as I judged probable by the following phænomena.

OBSERVATION VI.

Firſt, I never obſerved the light to diſcloſe itſelf firſt, either in the liquor, or upon the ſurface of it; but ſtill the ſhining began at the upper part, which was firſt touched by the outward air, and made a progreſs, quick indeed, but not ſo inſtantaneous, as that the eye could not follow it, from the top to the bottom of the vial.

OBSERVATION VII.

Secondly, the contact of the air ſeemed neceſſary to the propagation as well as production of this flame or light: for if, having ſhaken the vial, that the liquor might either wet the ſtopple, or communicate ſomething to it, I warily bended the cork this way and that way, ſo that only a few particles of the outward air could inſinuate themſelves between the ſtopple and the neck of the glaſs; there would appear on the ſides, and perhaps beneath the cork, little flames, as it were; which yet, though very vivid, were not able to propagate themſelves downwards: whereas, when the cork was quite removed, and acceſs was thereby allowed to a greater quantity of air, the flame or light (as was lately noted) preſently diffuſed itſelf through the whole cavity of the vial, and reached as low as the ſurface of the liquor.

OBSERVATION VIII.

Thirdly, Though oftentimes the light ſeemed more vivid near the ſurface of the liquor, than elſewhere; (whether becauſe the lucid matter was there more denſe, I now examine not) yet when by ſtopping the vial again, preſently after I had opened it, I endeavoured to deſtroy the flame or light; I generally obſerved, that when it was ready to vaniſh, (which in that caſe it uſually did in no long time) it began to diſappear firſt in the bottom of the vial, and ſeemed to ſhrink, as it were, more and more upwards, till it expired at the neck of the vial, (where it was neareſt to the air.)

OBSERVATION IX.

Fourthly, But on the other ſide, when I kept it unſtopt for ſome time, as for two or three minutes of an hour, though I afterwards ſtopt the vial very cloſe, the air, that had more leiſure than ordinary to inſinuate itſelf, would ſo cheriſh the flame, that the light

light would continue ſometimes an hour or two, and laſted once or twice no leſs than three hours.

OBSERVATION X.

Fifthly and *laſtly*, It ſeemed, that ſome elaſtical particles of the included air, or ſome ſubſtance, that concurred to the maintenance of the flame, was waſted, or depraved and weakned, by being pent up in the vial with the emanations of the liquor; ſince, when the vial had been kept ſtopt a competent time, and its cavity appeared tranſparent in the outward light, if I cautiouſly took out the ſtopple, the external air ſeemed manifeſtly to ruſh in, as if the ſpringyneſs of the internal had been notably debilitated by the operation of the flame upon the matter, with which it was kept impriſoned.

SOME of theſe phænomena eaſily brought into my mind ſome of thoſe of an odd experiment, that I formerly imparted to the curious. In which experiment I obſerved (among other things) that the ſpirit of urine, impregnated with copper, after the manner there preſcribed, would continue limpid and colourleſs, as long as the vial, that contained it, was kept cloſe ſtopt. But when once the air came to touch the ſurface of it, it would (ſometimes in leſs than a minute of an hour) be ſo affected thereby, that in a very ſhort time (for it was often within ſome minutes) the liquor would become of a tranſparent ſky-colour; and afterwards, the vial being well ſtopt, and kept in a quiet place, would by degrees grow diaphanous, and the air included with it was wont to have its ſpring weakned. And as the change of colour was firſt produced at the ſurface, where the liquor and air touched one another, and was afterwards thence propagated downwards; ſo when this cœruleous colour began to diſappear, the liquor manifeſtly became limped firſt at and near the bottom, that is, the part, which is remoteſt from the ſuperior air.

BUT to return to our noctiluca, the five phænomena laſt recited, and ſome others, ſeem to favour the conjecture or ſuſpicion I lately propoſed, *about the intereſt of the air in our unburning flame*. And to examine that ſuſpicion, I thought it leſs proper to make the foregoing trials with a more vigorous noctiluca, than in a ſubſtance, wherein, as in that we have hitherto employed, the diſpoſition to be kindled, or excited to ſhine, was but faint; ſo that being, as long as it remained, unexcited, opacous and dark, the abſolute, or almoſt abſolute, neceſſity of the concurrence of air to the actual ſhining (that conſtantly enſued upon its contact) of the diſpoſed matter, ſeemed manifeſt enough.

AN OCCASIONAL DIGRESSION.

BUT to what this concurrence or efficacy of the air ought to be aſcribed, is a problem, that ſeemed to me ſo difficult, that my thoughts were put upon ſeveral conjectures for ſo much as a tolerable ſolution of it; for a taſte of which, I ſhall venture to offer to you one or two of thoſe, that leaſt diſpleaſe me.

I thought it not improbable, that the admitted air, either by ſome ſubtil ſalt that it contained, or upon ſome ſuch account, excited in the fumes, it mingled with, a kind of fermentation, or (if you pleaſe) a commotion, by which means the matter acquired ſo briſk an agitation, as to propagate the motion to the eye, and there make an impreſſion, the ſenſe whereof we call light: though it ſeemed alſo not unlikely, that ſome of the particles of the ſupervening air may ſo aſſociate themſelves with thoſe congruous ones, they met with in the cavity of the vial, that, by that coalition, corpuſcles were produced, fitted to be, by the ſubtil æthereal matter, that abounds in the pores of the air, ſo pervaded and briſkly agitated, as to produce light. And it was

 not

not new to me, that the air should associate itself with invisible exhalations, and concur with them to make new concretions; since I have several times prepared a volatile sulphureous liquor, red as a ruby, which, when the vial has been kept close for some time, suffers the empty cavity of the vessel to be transparent; but upon the unstopping it, and giving access to the outward air, it appears presently full of white fumes, more opacous than a mist. And something like this, though in an inferior degree, may be observed, when we unstop glasses, that are but partly full of spirit of salt, or *aqua fortis*, provided those liquors be rectified as much, and no more, than is fit. For the contact of the air will presently make the former manifestly afford white fumes, and the latter sometimes red ones, and sometimes otherwise coloured. But if I durst mention, what my love to mankind has obliged me to conceal, even from my nearest friends, I could give an instance of a strange power of the air to excite a vehement motion in fitly disposed matter, though it be of a consistence far more unlikely to be thus agitated, than the fluid substances of our phosphorus: since I experimentally know a body, dry, and solid enough to be pulverable, that barely by the contact of the common air, will, even when it is actually cold, in very few minutes have its parts brought to such a degree of agitation, that its heat is little less intense than that of some actually ignited bodies, and may, if I please, by the further action of the air, be brought to afford some light also.

BUT against this conjecture *about the cause of the air's concurrence to the shining of our noctiluca*, there came into my mind, among other things, a strong objection, that may be drawn from the constant noctiluca formerly mentioned to have been shewn by Mr. *Kraft*, in which the lucidness was constant, though the vial, that contained it, was kept stopt. In answer to this, I thought it might be said, that the particles of the lucid substance, being in great numbers crowded together into a little room, these concentrated particles may be supposed to have been brought to such a state, that they needed not the renewed assistance of the outward air, to continue shining; either because their intestine motions were brisk enough to discuss the minute parts of the matter, wherewith they were associated, and so from time to time to generate, or extricate, and supply themselves with as many small aerial particles, as were necessary to keep the mass they belonged to, luminous. Which conjecture may be illustrated by observing, that though our common culinary flames are presently extinguished, unless they be cherished with fresh air, yet I elsewhere recite an experiment of a composition, which is so fitted to generate as much air as it needs, that I have several times found, that it may be kindled, and made to flame away, even *in vacuo Boyleano*, (as they call that made by our air-pump.)

OTHER things may be alledged both for and against the proposed conjecture, about the account, on which the air concurs to the light of our liquid noctiluca; but, I hope, it will not be impertinent to add, that perhaps the concurrence of the air may be considerable to both the phosphoruses, the fluid and the consistent, but the external air be necessary only to the former: because in the latter, the luciferous particles may have acquired such a texture, as that of rotten wood, or rather of whitings, or the liquor of glow worms, taken out after they are dead. For in that state (whatever others have written) I have kept that juice luminous for very many hours, (not to say some days;) and it is conceivable enough, that in the consistent noctiluca, by reason of the great numerousness and extreme minuteness of the parts, and the unctuousness or viscosity, or, in a word, tenacity of them, the mass they make up is much less dissipable than that, wherein the shining virtue of rotten wood, or the juice of dead glow-worms resides. This conjecture may be confirmed, by observing, as a thing very analogous to our phænomena, that I have found some lights in putrid bodies to be so faint, that they

they would, like that of our fluid noctiluca, (but far more quickly) disappear, when they were totally deprived of air, as I several times found in parcels of rotten wood. And on the contrary, others had so vigorous or tenacious a light or flame, that, like the splendor of the constant noctiluca, it would continue (though perhaps not in its full lustre) when the outward air was, in our pneumatick engine, diligently drawn off from it. And on this occasion I call to mind another experiment, which seems yet more analogous, than any hitherto alledged, to our present production of flame or light. For having purposely kept certain fish in a glass, freed from air, till I concluded it had lain longer than was necessary to bring it to that degree of putrefaction, which was wont to make such fish, at that time of the year, to shine, I could not perceive in the cavity of the glass the least glimpse of light: and presently after I had let in the outward air, it did (according to my expectation) as it were, kindle a flame, in the proximately disposed matter, or at least produce in it a manifest light. And it may much conduce to shew, that the lately mentioned difference of shining bodies may be but gradual, if I here observe, that I found by trial, that in bodies of the self same kind, as for instance glow-worms, or the same species of rotten fishes; if the light were but faint, the withdrawing of the air would, after a while, make it quite disappear; and the re-admission of the air would presently make it re-appear, as it happens in our aerial noctiluca. But in those individuals, wherein the luciferous matter was more copious and vigorous, and probably more tenacious, the absence of the external air did somewhat lessen or impair, but not quite destroy the light; and so possibly it might happen in Mr. *Kraft*'s consistent noctiluca: For though it shone without the renewed accession of external air, yet, that it would have been more brisk and active, if it had been assisted by such air, I was induced to think, because (if I much misremember not) when once, to gratify my curiosity, he took it out of the vial he usually kept it in, it did manifestly smoke and waste by the action of the air, and produced considerable effects of actual heat; for this being done in the day-time, in a room we could not darken, it could not indeed be expected, that we should discern any augmentation of light; but yet that there was one, may probably be argued from the newly mentioned things, that used to be its concomitants.

SUCH observations and reflections incline me to think, that, to speak in a general way, the light of our noctilucas depends upon a peculiar and very brisk agitation of some minute particles of the shining matter, in point of bulk, shape, and contexture, peculiarly fitted to impel the contiguous æther to the bottom of our eyes; and made me think it not improbable, that the contact of fresh external air might contribute to this peculiar kind of agitation in the gummous noctiluca, as an helpful thing, and in the aerial noctiluca, as an almost necessary concurrent But whether the air concurs to this effect, as it does itself excite a brisk commotion in the fumid matter it invades, or whether the air, or some fine substance contained in it, operates on this occasion as a kind of vital spirit, such as is found necessary, not only to common flame, but to that which is supposed to keep animals alive; or whether the corpuscles of the admitted air so combine with those, that exhale from the grosser liquor, as to become fit to be vehemently agitated by some æthereal pervading substance; whether or no, I say, the agency of the air in our phænomena be to be referred to one or more of the newly mentioned things, or to some other cause of a peculiar and very brisk agitation, which, to speak in general, seems to have the main stroke in the production of light, is left to further inquiry.

BUT I forget, that my intention was to set down observations, not hypotheses. And indeed the historical part of what I had to say of phosphoruses, is far more useful and certain, than the conjectures I can yet make upon it. Because, though I am content to let them paſs, in regard they may afford you ſome hints of further ſpeculations; yet the true ſolution of the problem, that has occaſioned this excurſion, may depend ſo much upon further experiments and obſervations, that though it is not impoſſible, that future phænomena may favour the propoſed conjectures, yet, it is not very unlikely, that I ſhall hereafter ſee cauſe to change them for ſome hypotheſes, exceedingly different from them. To return therefore now to our hiſtorical obſervations.

OBSERVATION XI.

ALTHOUGH, in the moderately ſhaken vial, when the light was quite vaniſhed, I could not make the liquor begin to ſhine; yet when by unſtopping it a little, the flame was kindled in the cavity of the glaſs, then, by ſhaking it again, though it were done more faintly than before, the light ſeemed to be manifeſtly increaſed by this agitation.

OBSERVATION XII.

IF I took a little of our liquor, when it was in its dark ſtate, and laid it upon my hand, or on the ſtopple of the vial, it would oftentimes lie there without diſcloſing any glimpſe of light; but if I rubbed it with my finger, or ſome other fit body, it would then not only ſhine, but ſhine more vividly, than at beſt it uſed to do in the vial, when the neck of it was ſtopt; and this vivid light, whilſt I continued to rub the matter it reſided in, ſeemed from time to time to flame and flaſh, and did not only invade the noſtrils with a ſtrong and offenſive ſmell, but viſibly ſent up ſtore of ſmoke, as if it had been ſome common culinary flame; and when, upon my ceaſing to rub the extravaſated liquor, it ceaſed to ſhine for a pretty while, yet when I returned to rub it again, it would again appear luminous: but by little and little the lucid virtue decayed, till it was to no purpoſe to rub any more.

OBSERVATION XIII.

THE light of our liquor, when excited ſeemed for degree much like that, that I obſerved in ſome ſpecies of rotten wood, that were not of the moſt vivid ſort; and when ſurrounded with bodies of black colour, the reflection of its light from them was little or none. But very white bodies, that were held contiguous to it, were manifeſtly illuſtrated by it, eſpecially, if the eye, having been long kept in the dark (whereby the pupil uſes to be much opened, and conſequently capable of admitting more numerous beams) was made more ſuſceptible of the fainter impreſſions of light. Inſomuch, that, when having placed the vial by me when I went to bed, and was awake ſome time before break of day, I encloſed both the glaſs and my head between the ſheets, the light ſeemed to me to be very conſiderable, and to enlighten the compaſs of a foot or more in diameter, and probably would have diffuſed itſelf further, if it had not been bounded by the ſheets, whoſe whiteneſs made the reflection of the light from them appear very prettily. And by the help of this light, I could eaſily perceive my fingers, and a ring I wore upon one of them, though I could not diſtinguiſh the colours of a reddiſh diamond, and a couple of emeralds, that were ſet in it.

OBSERVATION XIV.

IN reference to the light within, the included flame in our vial was opacous; for both at ſome other times, and even when I made the laſt recited obſervation, I could not at all perceive my finger, when the ſhining ſubſtance was interpoſed betwixt it and my eye. But in reference to external light, the flame or ſhining matter was diaphanous; for even in a very faint light, by which, I think, I could ſcarce have read an ordinary print, if I held our luminous vial between the window and my eye, I could very plainly ſee fee my finger on the further fide of the glafs, though, if my eye were placed between that and the light, the tranfparency would appear fomewhat leffened, becaufe the cavity feemed, as was formerly noted, filled with a kind of whitifh mift. And the like tranfparency and whitifh fumes, obfervable in the fame luminous fteams or flame, when the vial was looked, on, againft, and from, the light, I found, if, inftead of the day-light, I employed the light of the candle.

OBSERVATION XV.

Having the opportunity of a convenient place, and a fair day, I fet the vial about noon in a window, opened towards the fouth, and left it there expofed to the fun-beams for a confiderable time, to try, whether they would, upon the account of their agitation, or fome imaginable affinity of nature, kindle or excite the luciferous liquor, or its effluvia. But I could not perceive, that the fun beams had fuch an operation, which I chiefly concluded from my not being able to perceive any whitifh or miftlike fumes in the cavity of the glafs; for I durft not rely upon my not perceiving any light in the darkeft corner of the room, becaufe I fufpected, that might proceed from my eyes having been accuftomed to the great light of the then fair day, which made it lefs fufceptible of impreffions from a faint light.

OBSERVATION XVI.

Acid and alcalifate fpirits being reckoned by chymifts amongft the moft fubtle and operative fubftances, obtainable from mixt bodies by diftillation, I thought it very well worth while to try by tafte, whether our fhining liquor did notably abound with particles of either of thofe kinds. I did not find, that the liquor I put upon my tongue was in the leaft acid; nor that it was fenfibly alcalifate, as divers modern chymifts call fuch volatile falts and fpirits, as are afforded by harts-horn, blood, and fuch like fubjects of the animal kingdom: but it feemed to me to have an odd empyreumatical tafte, almoft like that of the fpirit of crude tartar; its fmell being alfo like that of fome empyreumatical oil, compounded with a ftink, fomewhat like that of ftale urine. I likewife, for further trial, let fall upon a piece of white paper fome drops of blue fyrup of violets, to which I put a little of our liquor, ftirring them together with the tip of my finger; but the mixture was not thereby turned green, which it would have been by a quarter fo much of fpirits of harts-horn, of blood, or of fome other fpirit, abounding with falt of an urinous nature, or (as fome love to fpeak) with a volatile alcali. Some other trials I made, though but with very fmall quantities of our liquor, (becaufe I had but very little of it to fpare;) and thefe trials did, no more than the former, evince the liquor to belong manifeftly to the tribe of acids, or that of alcalies; though perhaps this may not be the cafe of all the the portions of liquor, whether more denfe, or more aqueous and dilute, that may be obtained by feveral degrees of fire, and fome other varying circumftances, from the matter, that affords noctilucas.

OBSERVATION XVII.

Sometimes, when for curiofity's fake I fhook the vial, fo that the whole body, even to the bottom, of the liquor, was fpread all over the infide of the glafs, I could obferve with pleafure, that in many places divers little grains or corpufcles, belonging to the opacous matter, that concurred to compofe the liquor, ftuck here and there to the infide of the vial; and that thefe, being of a confiftent, nor fluid nature, and therefore probably more denfe than the thinner parts of the phofphorus, did fhine very prettily and diftinctly, and looked almoft like extreamly little ftars, or rather radiant fparks of fire, whofe light was brifk enough to be diftinctly notable, notwithftanding that

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of the flame, that was contiguous to them, and filled the cavity of the vial. And theſe ſhining corpuſcles uſually continued their peculiar vividneſs, as long as I thought fit to look on them. Which great vigour of theirs, together with their duration, gave me hopes, that the further proſecution of what had been brought thus far may afford us ſome, not altogether deſpicable, quantity of the conſiſtent noctiluca, which, by reaſon of its denſity, tenacity, or other peculiar diſpoſition of parts, may ſhine like the conſtant noctiluca of Mr. *Kraft* formerly mentioned.

OBSERVATION XVIII.

Being deſirous to try, not ſo much what the air and agitation would do, towards the kindling or exciting (not the impriſoned exhalation, but) the liquor itſelf of our noctiluca, (that having been partly done already *) as what water would do to quench it; I thought fit to make the experiment, when time and many trials had much impaired its vigour. And accordingly having, in a dark place, unſtopt the vial, and wetted the tip of my finger with the included liquor, I could not perceive, that then (as when it was freſhly made) it gave any ſenſible light. Wherefore, having rubbed the moiſtened finger againſt my other hand ſomewhat briſkly, for a few moments, both the rubbed part of my hand and my finger appeared adorned, each of them, with a flame; and though upon my dipping my finger in water (that ſtood by ready for the purpoſe) the flame was, as it were, extinguiſhed, ſince the light preſently vaniſhed; yet, having taken out my wet finger again, and rubbed, without having previouſly dried it upon the other hand, as I had done before, the light, as I expected it would, did quickly re-appear.

* See above, Obſerv. XII.

Besides the foregoing phænomena of our luciferous matter, that occurred more regularly, there was one, that happened unexpected, and may perchance, (for till I have further obſerved, I dare not ſpeak it confidently) prove referable to the paper elſewhere publiſhed, *about ſome latent qualities of the air.*

OBSERVATION XIX.

The phænomenon was this: having one night opened the vial ſo often mentioned, to ſhew the production of light to a virtuoſo, I quickly ſtopt it again, and put it in my pocket, till I went to ſleep; and then laying it by me in the bed (as I often did) when the candles were carried out of the room, I perceived the light, whoſe laſting, I did not expect, ſhould exceed one hour, to continue ſtill vivid enough; and then ſhaking it a little, before I compoſed myſelf to ſleep, I laid it by, till I waked in the morning, and then looking upon it again, it appeared to my eyes (that then for ſeveral hours had been unaccuſtomed to the light) to ſhine more vigorouſly, than it had done at firſt. And from the time I opened it over night, till the laſt time I had occaſion to look upon it the next morning, it had continued ſhining for twelve hours; to which, whether the extraordinary warmth, that was obſerved that particular night had contributed any thing, I dare not determine, but ſhall rather add, that though this phænomenon happened very rarely, yet this was not the only time that I obſerved it: for once more it occurred to me, and that time the light continued about fifteen hours, that I took notice of, and how much longer it might have laſted, I was hindered from obſerving. But this circumſtance ſeemed conſiderable, that the long duration of our unburning flame happened after the reſt of the trials and obſervations had been made; when by them the vigour of the luciferous matter might reaſonably be expected to have been very much impaired.

OBSER-

OBSERVATION XX.

WHEN I had ſet down the laſt mentioned phænomenon, I thought I had concluded the obſervations, peculiarly belonging to the aerial noctiluca, contained in our ſecond vial, and hitherto treated of. But now I find myſelf, by philoſophical ſincerity, obliged to add another phænomenon, which did ſomewhat trouble, as well as ſurprize me, and it was this. After the foregoing obſervations had been made with our ſecond vial, one night, that I came to open it, to ſhew one of my beſt friends the production of light, I found (little to my content) that none at all appeared, though I ſhook the contained liquor, and kept the vial a pretty while unſtopt; ſo that, if he had not known me well, he might have entertained ſiniſter thoughts of me, till having taken out ſome drops of the liquor, and rubbed it upon my hand, it afforded ſo vivid a light or flame, as ſatisfied him of the poſſibility of a true noctiluca. And ſince that time, I have not found the vial to afford any light, barely upon its being unſtopt; ſo that either (in ſpite of my care) ſomebody's unſkilful curioſity has, unknown to me, ſpoiled the liquor; or, (which is more likely) ſo little a quantity, as I had at firſt, by the many and various trials I made with it, is diſpirited and become, as it were, effœte; which it was lucky it did not do, till the forecited obſervations had been made with it. But, as in one of thoſe, it had been conjectured, that one of the chief accounts, on which the air itſelf may concur to the ſhining of our noctiluca, is, as it excited a certain kind of briſk motion in the parts of it, I thought fit to try, whether, though I had found the bare ſhaking of the vial to be ineffectual, yet an actual heat, whereby the parts muſt be more vehemently and variouſly agitated, might not enable the air to do, what otherwiſe it could not perform; I therefore held our vial near the fire, till it grew conſiderably warm, and then by ſhaking it a little, and unſtopping it in a dark place, I perceived the exhalations, that poſſeſſed the cavity of the vial, to ſhine, as formerly; but their light was ſo momentary, that it ſcarce ſooner appeared, than vaniſhed; and though afterwards it ſometimes appeared, it was not vivid, nor laſted a minute of an hour, nor perhaps half ſo long; though it ſeemed, that when freſh air was then allowed acceſs to it, its duration was thereby ſomewhat lengthned. But how long our matter will retain a diſpoſition to be excited, even by theſe means, to ſhine, experience alone can determine.

Additional OBSERVATIONS about the AERIAL NOCTILUCA.

YOU may remember, Sir, that, to clear the way to the twenty foregoing obſervations, I formerly told you, that we received the luciferous matter, obtained by our diſtillation, in ſeveral ſmall glaſſes, as we were able to ſave it. The parcel, that was received in the ſecond vial, afforded us the phænomena hitherto recited; and now it will be fit to add to thoſe ſuch as more lately occurred, upon our conſidering the portions of luciferous matter, preſerved in the other glaſſes, and ſome alſo of the like lucid ſubſtance, prepared another way. And though theſe obſervations be not ſo numerous, as the former, and be, a few of them, near of kin to ſome of the others; yet I ſhall not ſcruple here to ſubjoin them, both becauſe moſt of them are new, and thoſe, that are not, will ſerve to confirm and elucidate ſome of the foregoing obſervations. Beſides that, it is not eaſy to know, what phænomena may, and what cannot, be uſeful, to frame or verify an hypothefis of a fubject new and fingular, about which we have not as yet (that I know of) any good hypothefes fettled.

A fmall portion of liquor, (not much exceeding a fpoonful) that was the firft, and was judged the beft, I faved, being put into a long, and fomewhat flender cylindrical vial, made of white or chryftalline glafs, afforded us the enfuing phænomena.

OBSERVATION I.

SOON after the muddy liquor (for fuch it appeared to the eye) was poured into the vial, it was fo vigoroufly luminous, (probably, in great part, from the contact and infinuation of fo much air, as it met with in its transfufion,) that not only it fhone vividly, but continued to fhine ten hours, that I took notice of, before my occafions made me defift from obferving it.

THIS experiment minds me of an objection, which I fhould have propofed and anfwered at the beginning of the foregoing paper, if I had then remembred to do it. For, whereas it may by fome be thought improper for me to call our luciferous matter a felf-fhining fubftance, in regard that it is not lucid, without the concurrence or help of the air: I anfwer, that I do (and juftly may) employ the word felf-fhining, to fignify, that the light our matter affords, is not a light borrowed from any external lucid, as is done by the Bolonian ftone, and the *phofphorus Balduini*, but proceeds, as it were, from an inward principle of light. And men fcruple not, upon fuch an account, to reckon the flame of a candle, and a glowing coal, to be felf-fhining bodies, though neither of thefe will be kindled, or continue to fhine, without the affiftance of renewed air, no not for a few minutes: whereas, the newly recited phænomenon of our noctiluca fhews, that our prepared matter being for a very fhort time, (perhaps but few minutes) impregnated by the air, it will continue to fhine many hours in a well ftopped glafs, that hinders it from being relieved by any fupply of frefh air.

OBSERVATION II.

WHEN I fet down the fifth, and fome other of the foregoing obfervations, I was not at leifure to difcourfe the reafons, that induced me to try for an aerial noctiluca; and now alfo to fave time, I fhall forbear launching into fpeculations upon that fubject, and only tell you hiftorically, that, prefuming the matter, that would fhine in our cylindrical glafs, would not be fo much the liquor itfelf, as an aggregate of fuch effluviums of it, as, affected and excited by the air, would become lucid; I thought fit to take particular notice, how the air would work upon the exhalations of this more vigorous liquor. And accordingly, having heedfully opened the vial, though I very foon after ftopped it again, I obferved a great commotion to be made in the cavity of the glafs, unpoffeffed by the liquor: for the now lucid exhalations feemed to have a nimble and almoft circular motion along the fides of the glafs, and to make, as it were, a little whirlwind, that impetuoufly carried it round; and this renewed rotation was not only manifeft, but lafted much longer than one would have expected: fo great a commotion did the air feem to have produced in the effluviums of the liquor, and perhaps in the neighbouring parts of the liquor itfelf. Upon the ceafing of this unufual motion, the light did not ceafe, but perfevered, though I had not occafion to obferve how long it would have lafted.

OBSERVATION III.

I WILL not determine, whether the vertiginous motion, mentioned in the newly recited obfervation, was in part produced by what happened in the enfuing phænomenon, which was, that having heedfully taken out the ftopple of our vial in a dark place, after

after it had for a long time ceased from ſhining, I obſerved the external air to ruſh into the cavity of the glaſs with noiſe, and ſo ſwiftly, as did, I confeſs, ſurprize me: as if the preceding flame, though not ſenſibly hot, had, after the manner of culinary flames, conſiderably weakned the ſpring of the included air, and ſo diſabled it to reſiſt the whole preſſure of the external air, when, by the removal of the ſtopple, it was expoſed thereunto. But I will not, as I was ſaying, determine, whether the irruption of the air may not have contributed to the circular motion of the lucid ſteams mentioned in the foregoing obſervation; becauſe, though the affirmative ſeems a probable cauſe, yet I was kept from concluding it a neceſſary or only cauſe of the turbinous motion, by my having ſometimes, when no ſuch irruption of the air had in a long time preceded, obſerved rotations of lucid matter in the cavity of the vial: which motion therefore ſeemed to proceed from ſome other cauſe, though (to add that by the by) this cauſe, whatever it was, produced but ſuch a rotation, as was leſs general, leſs nimble, and leſs laſting.

OBSERVATION IV.

I FORGOT to tell you in its due place, (which was before the precedent obſervations) that, whilſt our liquor was yet freſh and vigorous, I dipt my finger in it, and moiſtned with it ſeveral places of my hands, and thoſe of ſome ladies, that were deſirous to be preſent at the ſpectacle. Which done, we obſerved, that the places that were touched, eſpecially if they were a little rubbed, ſhone very vividly, as if actual flames, but not of a blue colour, like that of common ſulphur, or of ſpirit of wine, were burning on them. And theſe flames were not at all uniform in their manner of burning, for they often ſeemed to tremble much, and ſometimes, as it were, to blaze out with ſudden flaſhes, that were not laſting, (which put me in mind of ſome of the *faculæ ſolares.*) And though it might ſeem ſtrange, that ſo ſmall a quantity of matter, that ſtuck to this or that part of the hand, ſhould afford ſo durable a flame; yet if that part itſelf were rubbed againſt the ſame perſon's other hand, or the ſkin or linnen of a by-ſtander, the part new touched would ſhine, as the other continued to do. And though theſe flames were remarkable for their vividneſs, yet they continued for a good while to afford the company a very pleaſing ſpectacle; and (which was remarkable) notwithſtanding the darkneſs of the room, it was manifeſt, that they emitted great ſtore of whitiſh ſmoke, which, or ſome other effluviums from the ſame matter, imbued the neighbouring air with a rank and offenſive ſmell. The colour of theſe ſeeming flames was not like the phoſphorus of Balduinus, when it is very well prepared, and has been expoſed to a vigorous light, red, almoſt like a well kindled charcoal; but yellow, like that of the middle part of the flame of a candle.

AND notwithſtanding the blazes and ſmoke, that accompanied theſe flames, we could not perceive in them any ſenſible heat (that is, any confuſed agitation of parts, exceeding that of the parts of our organs of touch) nor did they at all ſinge the fine linnen of the ladies, whereon ſome of them ſeemed to burn; ſo that if we admit, with many learned moderns, a *flamma vitalis* in the heart, this unburning and innoxious flame may ſupply us with a far better ſpecimen or illuſtration thereof, than the flame of ſpirit of wine, that is ſtill commonly employed, for an example; though I have many years ago endeavoured to rectify the error, by proving experimentally, that the flame of ſpirit of wine is hot and devouring, inſomuch that I have melted glaſs and gold itſelf with it.

OBSERVATION V.

WHEN, with my finger dipt in the forementioned liquor, I drew ſhort lines upon linnen, there was left a ſhining track upon that part, over which my finger had newly paſſed; ſo that it is not to be denied, that one may write lucid characters upon white paper; and yet, when, having found our liquor too thick, or too faintly lucid, to be employed, like ink in an ordinary pen, I thought fit to try whether I could draw lucid letters with a (middle-ſized) pencil, inſtead of a pen, and had, for that purpoſe, dipt it in our liqor; I was ſomewhat ſurprized to find, that the characters I had newly drawn, did not at all ſhine in the dark: but ſuſpecting, that the pencil might have retained, among the hairs it conſiſted of, the more tenacious and vigorous parts of the matter it had imbibed, and had left only the more aqueous and ſtrengthleſs parts upon the paper, I took the pencil in one hand, and with the other compreſſed and wreathed a little bruſhy part of it, to excite the matter, that probably was lodged there. By which means, that part of the pencil was brought to look as if it were all of a light fire, and ſeemed to burn like a ſmall wax taper; but with a more blazing and pleaſant flame, which ſometimes ſhooting downwards, and playing about the hairs, that compoſed that part of the pencil, brought into my mind thoſe verſes of *Virgil*.

> *Ecce levis ſummo de vertice viſus Juli*
> *Fundere lumen apex, tactuque innoxia molli*
> *Lambere flamma comas, &c.* Æneid.

BUT this delightful flame laſted not very long in its firſt vigour, but decayed by degrees, till no more light at all was ſeen; after which, nevertheleſs, the flame would of itſelf break out, as if it came from the internal parts of the pencil, and would ſhine a pretty while, and then ſeem quite to expire; after which, our light would on a ſudden diſcloſe itſelf again, and, when it had continued a while in a tremulous motion, die again in all appearance. And it is to be noted, that though this artificial *Ignis Lambens*, if I may ſo call it, did not, that I perceived, burn, or ſinge the ſlender hairs, among which it ſeemed to flame, yet, as often as it appeared, it did manifeſtly emit, perhaps as much, if not more ſmoke, than another burning taper of that bigneſs would have done. And this viciſſitude of extinction and re-appearance of light, laſted, till I was weary of obſerving it; and then, having again with my fingers compreſſed, and ſomewhat ſtrongly twiſted the hairs of the pencil, I made them, as formerly, afford a conſiderable light, which I thought was, whilſt I was in the very act of wreathing the hairs, accompanied with a very ſenſible, but momentary heat.

OBSERVATION VI.

BUT notwithſtanding the newly recited heat, it was in vain that I tried, by compreſſing the pencil firſt, and then rubbing it upon gunpowder, well dried, and ſomewhat heated, to fire the powder. This I failed to do likewiſe, when I made the trial with circumſtances ſomewhat more likely to make it ſucceed. Which I the leſs wondered at, becauſe I remember Mr. *Kraft*, when he kindled gunpowder in my lodging, was fain to make uſe of his conſiſtent and conſtant noctiluca; and beſides, to have the gunpowder prepared, by being made ſo hot, that it was almoſt ready to take fire of itſelf. Which circumſtance, I confeſs, I was glad of, as I alſo was of my own diſappointments, and ſome alſo of his, becauſe it gave me occaſion to think, that this, otherwiſe innocent, fire would not eaſily be perverted to the prejudice of mankind, which, I have ſuppreſſed more dangerous inventions than this, to avoid contributing to.

BUT upon this occaſion I muſt not pretermit what happened to my laborant, when the diſtillation of our luciferous matter had been freſhly made; namely, that, having taken up ſome of the thicker ſubſtance with a knife to put it into a vial, and having found,

found, that some of it afterwards stuck to the blade, he, being in some haste to wipe off the adhering matter, did with his apron take strong hold of the blade on both sides, and then with his right hand drawing out the blade nimbly, so that it was strongly compressed in its passage between the thumb and fingers of his left hand, he was much surprized to feel a smart heat; and presently looking upon that part of the apron, where it had been produced, perceived, that it had in it two holes of some bigness, which he concluded must have been produced there by burning, both because of the intense heat he had felt before, and because it was a new apron; which when I had called for, and heedfully inspected, I did, with him, impute those holes to the action of the fire. Whence I judged it very probable, that the thicker and almost unguentous part (if so I may call it) of our luciferous matter had a great disposition or propensity to admit a very brisk agitation, since by an almost momentary, and not very vehement motion, it was put into an agitation, that made it capable of burning new callicoe (for of that the apron was made.)

OBSERVATION VII.

SINCE I usually set down the nocturnal observations about our noctiluca from time to time, as I make them, whilst they are fresh in my memory; and also have sent away to a friend many of the precedent, before I wrote (or made) the subsequent; you will not, I hope, think it strange, either, that, not having most of my materials at once together before me, I have not methodized them; or, that having been able to make but gradual discoveries of the subject I inquire into, the things, I write of it, should now and then chance to be coincident, and my expressions about it should sometimes not be altogether uniform, but the latter parts should agree more or less with the former, as new or varying phænomena happened to require.

UPON this account, I shall not scruple to subjoin, what has since occurred to me about the phænomenon, formerly mentioned in the sixteenth observation; where I told you, that I could not then clearly find, either an acid or an alcalisate salt to be predominant in the luciferous matter, I then made use of. But, having since employed some of the water, that was taken out of a receiver, after it had there been somewhat impregnated with that matter, I thought fit to try, whether this water, wherein probably the saline particles of our subject might be more copiously dissolved, or more active, would not discover itself to contain somewhat of volatile alcali. And to satisfy myself of this, I dropt a little of the liquor upon some syrup of violets, that I had put upon a piece of clean paper, and found I was not mistaken, in thinking it would change the colour of the syrup from blue to green; which yet it did more faintly, than the volatile alcalies, (as they call them) even when they are phlegmatic, are wont to do. This liquor likewise, as I remember, made some conflict with spirit of salt, when I first put them together, as I inferred from the commotion of the mixture, and the bubbles thereby produced. Nor were these the only ways, by which I was induced to think, that a volatile alcali, not an acid salt or spirit, was the predominant, if not the only salt, contained in the faintly impregnated liquor.

OBSERVATION VIII.

BEFORE I had set down many of the observations contained in the first paper, I was desirous to try, what would happen to our luciferous matter in such a vacuum, or, if you please, in such highly rarified air, as is wont to be produced by our air-pump. But, in regard a glass was to be opened in the exhausted receiver, which is difficult work to do, I was fain, for want of conveniences, to desist from my endeavours, and prosecute some other experiments, (most of them already recited) till at length being furnished, though

though not with accurate, yet with tolerable means of making a trial, and thinking an imperfect one, better than none at all, I took a vial, that had some luciferous matter in it, though but such, as was not apt to shine long at a time; and this vial being well stopt, I kept till the flame or light within it expired; then, having placed the vial in a receiver on our pneumatic engine, we pumpt out the air, and then (not without some difficulty) pulled out the cork in a dark place, whereupon there presently appeared some light in the cavity of the vial, which I the less wondered at, because we found by certain signs, that by reason of some disadvantageous circumstances, we could not so well pump out the air, and hinder the ingress of new, as not to leave, (though but very little, yet) enough to excite a flame, that by former experience we found to need but an inconsiderable quantity of fresh air: but we observed, that by the commotion of the air, occasioned by the pumping, the flame would be, as it were, ventilated, and blown up, or made to shine more vividly.

OBSERVATION IX.

BUT, not being satisfied by the foregoing experiment, I thought fit to vary it, after the following manner. There was taken a pretty large piece of paper, which, being well moistned, and partly besmeared with our luciferous matter, was thrust into a somewhat wide-mouthed glass, which, being put unstopt into a receiver fastened to our pneumatic pump, and with it kept in a dark place, did there shine, as I expected it would, by reason of the contact of the air yet contained in the receiver. Presently after this, the pump was set at work, and we observed, as formerly, that the commotion made of the air about the vial did manifestly enough increase the light for a while; and that the light seemed to be lessened, during the pauses intercepted between these commotions, both by reason of the rest, as of the absence of the air. And I likewise took notice, that the flame, that seemed to pass from one part of the wrinkled paper to the other, did sometimes appear to have, as it were, a palpitation, and to afford a very unequal light; and though, when the external air was let in through the pump into the exhausted receiver, the flame seemed to be quenched, yet I judge that to be only a temporary effect of the waterish vapours, that the air had taken along with it in its way through the pump; and therefore I caused the receiver to be taken off the engine, and then, the spectators were quickly of my opinion, observing, that upon the free contact of the fresh outward air, which was not like that last mentioned, depraved by moist vapours, the matter adhering to the paper was quickly seen to shine again, and that more vividly, than it had done in the receiver. But because I suspected, that this vessel could not at that time, for want of some conveniences, be so well exhausted, as on other occasions it has often been, though, by the phænomena hitherto recited, it seemed to the spectators, that the flame was manifestly befriended, and the light increased by the air, yet, I think, the experiment deserves to be repeated, when I shall be able to do it with more exactness.

OBSERVATION X.

BESIDES the liquors, that afforded us the foregoing experiments, we saved a little, (though but very little) of a substance, that was not liquid, but yet almost as soft as mud. This we obtained, by pouring some of our liquor, taken out of the vessels, when the distillation was ended, into a glass funnel, lined with cap paper, to try whether it would filter. But finding, that that, which passed thorough, was too thin and aqueous, the filter was hastily, and (for that reason) not very orderly wrapt up, and put into a glass, not capacious, but yet of a moderate wideness at the mouth; that, both

both the filter might be eafily thruft in, and the glafs might be exactly enough ftopt with a ftrong cork.

After other experiments (formerly recited) had been made, I took this glafs, and carried it into a dark place; and though I could not perceive the leaft glimpfe of light, yet prefuming, that it contained fome of the true matter of the aerial phofporus, or noctiluca, and confequently exhalations, that, having been hindered by the ftopple to flie away, might be kindled or excited by the appulfe of the air; I opened the glafs, and faw (as I expected) an immediate apparition of light. Which light did difclofe itfelf, fometimes upon a leffer, and fometimes upon a much greater part of the very uneven furface of the included paper, and feemed to pafs for a great while (as long as I thought fit to ftay to obferve it) from one part of the filter, and one fide of the glafs, to another: I fay, feemed, becaufe perhaps the phænomenon was produced by a train of eruptions of flames newly excited in feveral places, rather than a bare propagation of the fame. But whatever it was, the motion (which was pleafant enough to behold) was fo odd and irregular, that it did not ill refemble the motion of fire kindled by fparks, ftruck into a good quantity of tinder. And this virtue of fhining, upon the ingrefs of the air, lafted many days in the above-mentioned paper.

OBSERVATION XI.

But there was another filter, that afforded us a pleafing variation of this phænomenon; the matter wrapt up in the infide of this paper, being fomewhat more copious, or better conditioned, than that, which adhered to the other lately fpoken of. We took then this paper, and having unfolded it, and kept it difplayed in a dark place, we had the pleafure to fee a confiderable number of flames, of differing fizes and figures, difclofe themfelves at the fame time; and though moft of them were vivid, yet few of them continued long in the felf fame place, but they feemed frequently to change their fituations among themfelves, as well as their figures, and extent; or elfe new flames did inceffantly break forth in new places, according as the exhalations, that did copioufly and irregularly mingle with the contiguous air, did in feveral places happen to be in part, as it were, kindled by it; I fay, in part, becaufe, from the flames themfelves, as well as the unfhining parts of the filter, there did manifeftly afcend good ftore of fmoke, vifibly by the light afforded by the fhining matter: and thefe flames did not keep a conftant tenour in their way of blazing, but had their tremblings and emications, and thefe being ufually accompanied with changes of figure, and eruptions of light in feveral places at the fame time, it was a very pleafant fight to fee the whole area or furface of the difplayed filter look as the fky fometimes does, efpecially in hot countries, when the eye may perceive flafhes of lightning break forth in feveral places at once: but our corufcations, being as well more numerous, as innocent, made the filter appear almoft as variegated as marble paper; but with this advantage, that, befides that the appearance was almoft perpetually changing, the yellow parts were not only coloured, but lucid, and afforded thofe, that looked on them with me, a delightful fpectacle, that lafted as long as we thought fit to gaze at it.

OBSERVATION XII.

Having ftrongly fufpected, that the agitation, duly modified, of a difpofed matter, was at leaft one of the chief agents in the production of light; I was not difcouraged, by finding that fhaking of the vial, or making the contained liquor more than lukewarm, would not produce any apparition of light: I was not, I fay, thereby difcouraged from trying, whether a more intenfe heat, which would communicate a brifk

and

and various motion to a multitude of the corpuſcles of the luciferous matter, diſperſed through the liquor, would not do, what a fainter agitation was not able to perform.

I THOUGHT alſo, it deſerved to be tried, whether a conſiderable variation of phænomena would not be conſequent to our changing the figure and capacity of the glaſs; eſpecially, if all immediate commerce between the cavity of the veſſel, and the outward air, were carefully prevented.

IN order to both theſe trials, I took ſome ſpoonfuls of aqueous liquor, impregnated with ſome of the more ſoluble portion of the luciferous matter; which liquor, when it was ſettled, was tranſparent, as having but an inconſiderable quantity (which could not eaſily be ſeparated from it) of that muddy ſubſtance, formerly more than once mentioned. And this clear liquor, which, (perhaps becauſe of the abſence of that thicker ſubſtance) was, as it ought to be for my purpoſe, ſo faintly impregnated, that it would not, with ſhaking, or a mild heat, afford any light, was put into a round bolt glaſs, whoſe globous part was capable of holding three or four times as much, and whoſe ſtem (or pipe) was proportionable in wideneſs to it, and above a foot in length. Having carefully ſtopt this veſſel with a cork and ſealing-wax, it was in the night-time ſet in ſuch a poſture, that, by the intervention of ſand, it might be heated without breaking, (as otherwiſe it would have been in danger of doing,) and when the ball was made ſo hot, that I could not well endure it in my naked hand, I ſpeedily removed the veſſel into a dark place, and having ſhaken the liquor, I perceived a light to break forth in the ball, which preſently diffuſed itſelf thorough the whole cavity of it, but as quickly diſappeared. And ſome time after, eſpecially upon ſhaking the glaſs, the light would break forth again, and ſoon after vaniſh; and theſe fulgurations or flaſhings of light continued for a while to appear now and then; but were unequal, both as to their extent, vividneſs, and duration; and when the liquor grew cold, they ceaſed quite.

OBSERVATION XIII.

BUT whilſt it was yet conſiderably hot, I thought fit to try, whether by breaking the liquor by a ſtrong concuſſion, ſome lucid ſubſtance would not be made to paſs out of the globous into the cylindrical part, and ſo vary the phænomena. And to this purpoſe, having violently ſhaken the liquor at ſeveral times, with pauſes interpoſed, I perceived ſome conſiderable portions of the lucid matter to aſcend into the pipe; and particularly once I had the pleaſure to ſee a portion of ſhining ſubſtance, about the bigneſs of a filbert, or ſmall almond, mount directly upwards like a flame, but not very ſwiftly, from the globous part of the glaſs, all along the pipe, till it reached the upper part of it. And at other times, ſuch flames aſcended into the pipe, but not ſo high; whence many would have confidently inferred a poſitive levity in flame; which yet I forbear to conclude, becauſe I once (at leaſt) obſerved, one of theſe portions of ſhining matter to deſcend from the higher to the lower part of the ſtem, ſtill retaining its lucidneſs all the way.

I CANNOT now ſtay to debate, whether the phænomena, appearing in this glaſs, may illuſtrate, or facilitate the explication of what happens in the production and motions of ſome of thoſe meteors, that are called fiery; ſuch as the *Ignis Lambens*, falling ſtars, frequent lightnings without thunder, in hot ſummer-nights, and that wandering flame, called *Ignis Fatuus:* And whether or no, it may be ſaid, that when ſuch bodies are generated, there happens to be a convention of particles ſo aſſociated, that they mutually agitate each other, or are fitted to be agitated by a pervading æthereal ſubſtance, and put into a motion, like that, which in the lately mentioned portions of our ſhining matter, was able to produce light?

OBSER-

OBSERVATION XIV.

BUT, inſtead of purſuing this enquiry, I ſhall relate to you a phænomenon, that to me, as well as thoſe I ſhewed it to, was not a little delightful. For having, by a concuſſion, fit for that purpoſe, as it were ſpread the liquor at once all over the inſide of the globe, and of part of the ſtem, it was pleaſant to behold, how the luciferous matter, dividing itſelf variouſly in its paſſage downwards, adorned the whole cavity of the glaſs with a company of ſmall lucid bodies, that both ſhined and twinkled, like ſo many little ſtars, adorning the celeſtial globe; and the pleaſantneſs of the ſpectacle was increaſed, by their having manifeſt motions, as well as true light. The ſlowneſs of their deſcent, in lines, many of them very oblique, made this pleaſant ſight laſt the longer; and having more than once reiterated the experiment, (though not ſtill with equal ſucceſs,) it afforded me ſome varied phænomena; which I ſhall now forbear to mention, both becauſe I want time to write, and am weary of writing, as I fear you may be of reading. And therefore I ſhall here conclude your trouble and my own, as ſoon as I ſhall have added the two following particulars.

OBSERVATION XV.

THE firſt whereof is this, that having in ſuch a bolt-glaſs, as has been lately deſcribed, given purpoſely and heedfully a certain kind of ſtrong ſhake to the included liquor, when it was at a due degree of heat, (which was not intenſe) I obſerved, that on one ſide of the globous part of the glaſs, and above the body of the liquor, there was generated, as it were, a great ſpark of lucid matter, about the bigneſs of a pin's head; and yet hence, (as I expected) there quickly was a flame or light diffuſed through the capacity of the globe, where it ſoon after vaniſhed. From which phænomena, and ſome others of affinity to it, whether it may be argued, that this was a true flame, which from a very ſmall beginning, was increaſed by propagation, and kindled the diſpoſed exhalations, that it found diſperſed throughout the cavity of the glaſs; or, that the motion of all light is not neceſſarily inſtantaneous, ſince the progreſs of it, even in ſo ſmall a ſpace as our glaſs comprized, was diſcernable, I have not now the leiſure to debate, but muſt haſten to the laſt of the two promiſed particulars, which is,

OBSERVATION XVI.

THAT, (not here to mention, how I have preſerved a diſtilled luciferous matter, both with and without additaments in a conſiſtent form) to try how long I could preſerve our liquor, in a capacity to exhibit ſuch pleaſing phænomena, without giving it new air from time to time, but only by keeping in the ſpirituous parts; I cauſed the ſtem to be hermetically ſealed; preſuming, that, notwithſtanding this, I could, by a certain cautious way of holding the veſſel, ſafely bring the included liquor to an heat, ſufficiently intenſe, to afford us the phænomena of light. In which ſuppoſition I was not miſtaken, ſince the laſt recited phænomenon, beſides ſome others, was made in this hermetically ſealed veſſel, in which the contained liquor does, as I this night tried, continue fit for that purpoſe.

Of the Way of preparing the AERIAL NOCTILUCA.

THE ſeveral phænomena of our aerial phoſphorus or noctiluca, wherewith you have hitherto been entertained, have, I doubt not, raiſed in you a preſſing curioſity to know, of what matter this ſelf-ſhining ſubſtance was made, and how that matter was prepared, to be capable of affording it.

THOUGH two or three years are now paſt, ſince I cauſed to be made, more than once, in my furnaces, a phoſphorus, not unlike that of the learned *Balduinus*, (I ſpeak thus cautiouſly, becauſe I am not ſure, what particular matter he employs, and I have brought more than one ſort of mineral bodies, to ſhine;) yet I forbore to divulge what I knew, becauſe (as I declared to ſome curious men, that preſſed me to do it,) I was willing to leave him the liberty of publiſhing his invention. But finding he has not yet thought fit to impart it to the world, there appeared the leſs cauſe to expect, that the ſecret of the noctiluca, which is a much more valuable thing, would be ſuddenly made public: and therefore, without long waiting any man's leiſure, I reſolved to impart to the curious, (and particularly, ſir, to yourſelf,) the knowledge of the matter I wrought upon, and ſome directions how to manage it. And in purſuit of that reſolution, I am willing to gratify the virtuoſi with that very proceſs (for ſubſtance) which I ſet down for my own remembrance, after I had the firſt time actually made the aerial noctiluca; and which I afterwards depoſited, ſealed up, in the hands of the very ingenious ſecretary of the Royal Society, in the preſence of divers members of that illuſtrious company. And though, ſince that time, ſome other trials have enabled me to obſerve ſome circumſtances, pertinent to that purpoſe; yet I thought fit to leave it as it was, that others finding themſelves, in ſome ſort, obliged to employ their own induſtry, their trials may, as mine have done, produce an inſtructive diverſification of effects, in an attempt, where experience invites me to think, that various degrees of fire and other circumſtances, (and perhaps caſualties too) may diverſify the phænomena, and thereby both inrich the yet wanted and deſigned hiſtory of light, and aſſiſt the ſpeculative, to accommodate a good hypotheſis to them. Reſerving then for another time my latter remarks upon the obſervations and proceſs, delivered in this paper, I ſhall now only give you a few ſhort advertiſements about it.

FIRST, I will not poſitively affirm, that the matter, I employed, is the very ſame, that was made uſe of, by the ingenious German chymiſts in their noctiluca; for ſome inquiſitive men have very lately told me, that the Germans mingle two or more diſtillable materials; whereas I employed but one matter, capable of diſtillation.

SECONDLY, though all the twenty foregoing obſervations, and moſt of the ten additional ones annexed to them, were made with that ſubſtance, which I gueſs to be at leaſt the chief, that is employed by the Germans, (which was done for a particular reaſon, not needful to be here expreſſed,) yet I firſt thought, and upon my very firſt trial, found, that it is poſſible to make a noctiluca of a dry and pulverable ſubſtance, that, for ought I can gueſs, was never employed by Mr. *Kraft*, or thoſe he had his ſecret from. And beſides this ſecond ſort of phoſphoruſes, we made a third, that was obtained from a body, that never had been either a part, or an excrement, of a human body, nor was mingled with any thing, that had been ſo. But though I found theſe ſelf-ſhining ſubſtances ſomewhat differing from thoſe made of the liquor, hereafter to be named; yet, I cannot ſtay at preſent to ſay any thing more of them, being content to have intimated, that ſelf-ſhining phoſphoruſes have been actually obtained from more ſingle ſubjects, than one.

THIRDLY, to name the matter, though never ſo explicitely, would not, in my opinion, have ſufficed to inform thoſe, that would work upon it. For chymiſts themſelves would, in all probability, work, (as hitherto, on other occaſions, they have wrought) upon the volatile and ſaline, which they preſume to be the only ſpirituous and noble parts of the concrete, throwing away the reſt as uſeleſs and abominable. And on this occaſion, let me add, that I was the rather induced to ſet down this proceſs, that we may both obſerve and thankfully acknowledge the wiſdom and bounty of the great author of nature, who, for our encouragement to ſtudy even his meaneſt works, has been pleaſed, in a body, that is commonly thought one of the deſpicableſt of the univerſe, to lodge ſo glorious and excellent a thing, as a ſelf-ſhining ſubſtance.

FOURTHLY, and I ſcarce doubt, but this, though it will be admired now, will be much more prized hereafter, when it ſhall be brought to greater perfection; and when men ſhall have diſcovered more of its uſes, which probably will be great in phyſic, and, perhaps I might add, to ſome purpoſes, that few chymiſts themſelves do yet dream of.

FIFTHLY, one thing remains, that, to ſave ingenious men ſome labour and charge, I think fit to give early notice of; namely, that having, for trial ſake, employed the liquor, hereafter to be named, without previous fermentation or putrefaction; though it was proceeded with after the ſame manner with that, whereby we obtained our noctiluca; and though it afforded a ſubſtance, for colour and conſiſtence, not unlike our luciferous matter; yet I could not find, that that ſubſtance would at all ſhine. And indeed, there are ſo many circumſtances, whoſe miſtake may make the experiment miſcarry, (as I have found to my trouble, even ſince the phoſphorus, whoſe phænomena are firſt ſet down, was made,) that, though I were not now in haſte, I ſhould be content to take time to learn better from experience, how to inſtruct others, before I venture to do it circumſtantially; and he, that ſhall, at the firſt attempt, ſucceed in preparing this liquor, ſhall be thought by me, either a very ſkilful, or a lucky operator.

SIXTHLY and laſtly, that it may appear, as well by the very different preparations, as by the differing phænomena of the phoſphorus hermeticus, and of the aerial noctiluca, that there is a great diſparity between thoſe lucid bodies, I ſhall here briefly add the way we employed to make either the phoſphorus Balduini, or ſome other like it, (for I am not certain, what is the very way of that learned man) as it was practiſed in my furnaces; which, in ſhort, is this.

A diſſolution being made of fine white chalk in good ſpirit of nitre, or clean aqua fortis, it is to be filtrated thorough cap-paper, and the clear ſolution is to be evaporated, till there remain a dry ſubſtance: with this white calx, you are to overlay the inſide of ſome veſſel, made of good earth, that will endure the fire, and that of a round figure, which is more convenient, than that of ordinary crucibles; and to the matter contained in this veſſel, you are to give for about half an hour or an hour, (according to the largeneſs of it, and other circumſtances) a due degree of fire, which it is not eaſy to hit, and which ordinarily requires a conveniently ſhaped veſſel, whereby the flame or heat may be reverberated, till you perceive the matter to have acquired a diſpoſition, to retain the light; and then the earthen veſſel, which uſually ought to be ſomewhat ſhallow, and not to exceed many inches in diameter, is to have a cover of fine glaſs or cryſtal carefully cemented on to it, to preſerve it from its great enemy, the air.

WHAT we have obſerved, in proſecuting this preparation, is not ſo proper to be delivered at this time, when my haſte, as well as ſome other things, make it more fit, that we ſhould forthwith return to our aerial noctiluca, of which, after the foregoing things have been premiſed, it is time, that now there ſhould follow the proceſs.

The PROCESS.

THERE was taken a confiderable quantity of human urine, (becaufe the liquor yields but a fmall proportion of luciferous matter,) that had been, (a good part of it at leaft) for a competent while, digefted or putrified, before it was ufed. This liquor was diftilled with a moderate heat, till the fpirituous parts were drawn off; after which, the fuperfluous moifture alfo was abftracted, (or evaporated away) till the remaining fubftance was brought to the confiftence of a fomewhat thick fyrup, or a thin extract. This was well incorporated with about thrice its weight of fine white fand, and the mixture was put into a ftrong retort; to which was joined a large receiver, in good part filled with water. Then, the two veffels being carefully luted together, a naked fire was gradually adminiftered for five or fix hours, that all, that was either phlegmatic, or otherwife volatile, might come over firft. When this was done, the fire was increafed, and at length, for five or fix hours made, (N. B. which it fhould be in this operation) as ftrong and intenfe, as the furnace (which was not bad) was capable of giving. By this means, there came over good ftore of white fumes, almoft like thofe, that appear in the diftillation of oil of vitriol; and when thofe fumes were paft, and the receiver grew clear, they were after a while fucceeded by another fort, that feemed in the receiver to give a faint bluifh light, almoft like that of little burning matches dipt in fulphur. And laft of all, the fire being very vehement, there paffed over another fubftance, that was judged more ponderous than the former, becaufe (N. B.) much of it fell through the water to the bottom of the receiver: whence being taken out, (and partly even whilft it ftaid there) it appeared by feveral effects, and other phænomena, to be (as we expected) of a luciferous nature.

THE ways I employed to make a felf fhining fubftance, out of other matters than that expreffed in this procefs, I muft, for certain reafons, forbear to acquaint you with, at this time.

I might, from the foregoing procefs, take occafion to enquire, whether the matter, wherein the fhining faculty chiefly refides, do not confift, not (as one would expect) of the volatile and fpirituous parts of our animal liquor, but of its (not abfolutely, but) more fixt falt, and ponderous fœtid oil, affociated in a peculiar manner and proportion. And from thence I might take a rife, to propofe my conjectures of the caufe of the lucidnefs of our luciferous matter; and alfo, both to add fomewhat to what, (two or three years ago) I wrote about the defpifed fapa of urine, in reference to fome uncommon menftruums; and to make enquiry into other things relating to the nature of light and flame, efpecially as found in our noctiluca: thefe things, I fay, I might hence take occafion to propofe my thoughts of; but want of time, together with hopes of further difcoveries, make me willing to defer the doing it, till I fhall have more leifure to frame conjectures, and perhaps more phænomena to ground them upon. In the mean while, that, I may no further lengthen a letter too prolix already, by apologies for myfelf, or compliments to you; I fhall at prefent only beg the favour of your candid animadverfions upon what I have written, and of thofe fingular obfervations I hear you have made, about the light of ftinking fifhes; both which, you need not doubt, will be as welcome, as, I doubt not, they will prove inftructive to,

SIR,

Your moft affectionate,

and moft humble fervant,

R. B.

A NEW

A NEW LAMP, contrived by the Honourable *Robert Boyle*, Esq;

First printed in *Hooke*'s *Philoſ. Collect.* N°. ii. p. 33. *An.* 1680.

A B C D is a veſſel of latton well ſodered every where. B C, E F, are two bottoms ſodered to that veſſel. F G is a pipe ſodered to the bottoms aforeſaid, and whoſe aperture is in the great cavity F A. H is a hole in the pipe F G, opening between the two bottoms B C, E F. I is another hole, to which is ſodered a pipe I G bended upwards at G. P P is a little veſſel fit to receive the wick of the lamp. L M is a ſlender pipe open at both ends, and ſodered to the cover A D in L, and to the bottom E F in M; ſo that by that pipe the external air may communicate between the two bottoms, without penetrating into the cavity A F. N is a ſhort pipe ſodered to a hole in the cover A D, ſo that thereby one may pour oil into the cavity A F; and ſtop it afterwards very cloſe with cork.

FOR the filling up this engine, you muſt ſtop the aperture G of the pipe I G with a long pin fitted for that purpoſe, and the upper end of the pipe L M muſt be ſtopped too: then pour in your oil by the aperture N; which done, this ſame aperture N is to be ſhut up exactly, and both the others to be opened, *viz.* G and L. Then it will come to paſs, that the oil through the pipe I G will run and fill the veſſel P, until its ſuperficies be in the ſame level with the hole H and no more, as might be eaſily demonſtrated.

Now it is eaſy to ſee, that this lamp is free from all the inconveniences the lamp of *Cardan* is ſubject to; for,

1. THE air doth not get into it by ſtarts, or gluts, as it doth in *Cardan*'s lamp; but when the oil in P P being waſted by the flame, comes to have its ſuperficies lower than the hole H; the oil from the cavity A F runs into P P gently, becauſe its place, left in the cavity A F, is eaſily ſupplied by the external air, which thorough the pipe L M and the hole H gets up into the ſaid cavity A F.

2. WHEN the air contained in the cavity A F comes to be rarified by ſome heat, it drives out much oil, and ſo is able to choak *Cardan*'s lamp; but in this, the oil being ſo driven out, gets into the ſpace between the two bottoms, as well as into the veſſel P P: now the ſaid ſpace between the two bottoms, by reaſon of its largeneſs, receiving twenty or thirty times more oil than the veſſel P P, it follows, that the ſuperficies of the oil therein riſeth twenty or thirty times leſs, than if all the oil had been driven into the ſaid veſſel. Therefore, when we fill the lamp, we muſt take care, that the pipe L may be well ſhut, ſo that the air between the two bottoms, finding no iſſue, may keep the oil from filling that ſpace, which by that means, when the hole L is open, will be fit to receive the oil driven by the rarefaction of the air in the cavity A F.

3. THE oil being always kept almoſt at the ſame diſtance from the flame, the wick will not be quickly conſumed.

4. YOU have the conveniency to put new oil into the lamp, without moving or extinguiſhing the ſame; for you need but ſhut up G and L, and pour the oil through N, as hath been ſaid in the beginning.

ALTHOUGH this lamp cannot ſerve for all the uſes, that may be expected from the many contrivances publiſhed in the year 1677, by Mr. *Hooke*, yet it may be looked upon as uſeful too, becauſe it is eaſy to be made good and cheap without incumbrance, and ſo may do very well for ordinary uſe.

A DISCOURSE of THINGS above REASON. Inquiring whether a PHILOSOPHER ſhould admit there are any ſuch. To which are annexed by the Publiſher (for Affinity of the Subjects) ſome Advices about judging of THINGS, ſaid to tranſcend REASON.

An ADVERTISEMENT.

THE latter of the two following dialogues is but a part of a diſcourſe, conſiſting of ſome conferences, whereof that was neither the firſt nor the laſt. This it was thought fit the reader ſhould have notice of, that he may the more eaſily gueſs upon what account it is, that ſome clauſes in the firſt page, (and perhaps a few other paſſages elſewhere,) contain ſomewhat, that appears not altogether the ſame it would have done, if there had been no need to make any alteration at all in that page. But becauſe, though there was a connection between that dialogue and the reſt of the papers, from which it is diſmembered, yet its dependency upon the others is not ſo very great, but that the publiſher thought the divulging of it might be uſeful and ſeaſonable; and therefore finding, that want of leiſure, and much diffidence, made the author unwilling to reviſe, and part with the other papers, that accompanied this, which now comes forth; he prevailed with him to ſuffer that dialogue to take its fortune, which the publiſher hopes may be ſuch, as may encourage the author to communicate what he has further meditated upon ſuch ſubjects.

A DISCOURSE of THINGS above REASON.

The ſpeakers are, *Sophronius*, *Eugenius*, *Pyrocles*, and *Timotheus*.

Eugen. THE ſeriouſneſs you yet retain in your looks, and the poſture we found you in at our entrance, makes me fear theſe two gentlemen and I are unſeaſonable intruders, that are ſo unhappy as to diſturb your meditations.

Sophron. INSTEAD of doing that, you will much promote them, if you pleaſe to accompany me in them: for the ſubject, that buſied my thoughts, is both ſo abſtruſe and ſo important, that it needs more than one to conſider it, and deſerves, that he ſhould be a far better conſiderer than I, who therefore muſt think myſelf far leſs fit for that taſk than you.

Eugen. I will puniſh the flattery of theſe laſt words, by declining to make any return to it.

Pyroc. AND I, gentlemen, to prevent the loſs of time and words between you, ſhall, without farther ceremony, aſk *Sophronius*, what his thoughts were employed about when we came in.

Sophron. I was then muſing upon a ſubject, that was newly propoſed to me by our common friend *Arnobius*, who would needs have my opinion, whether, and if at all, how far, we may employ our reaſonings about things that are above our reaſon, as Chriſtians grant ſome myſteries of their religion to be.

Eugen. IF, by things above reaſon, be meant only thoſe, that are undiſcoverable by reaſon without revelation; I ſhould not heſitate to ſay, that there may be divers things of that kind: for the free decrees of God, and his determinations concerning the government of the world, and the future ſtate of mankind (to name now no others) are things,

things, which no human reaſon can pry into, but muſt owe the fundamental diſcovery it makes of them to the revelation of him, whoſe purpoſes they are.

BUT if, by things above reaſon, be meant ſuch, as though delivered in words free from ſuch darkneſs and ambiguity, are not to be conceived, and comprehended by our rational faculty, I ſhall freely confeſs, that I ſcarce know what to ſay upon ſo unuſual and ſublime a ſubject.

Pyroc. FOR my part, gentlemen, I think it were very requiſite to be ſure in the firſt place, that the ſubject of our diſcourſes is not chimerical, but that we can really know, that there are things we cannot comprehend, though they be propoſed to us in expreſſions no leſs clear than ſuch, as would ſuffice to make other things intelligible to us.

Sophron. YOUR cautiouſneſs, *Pyrocles*, muſt not be rejected by me, who when, before you came in, I was putting my thoughts into ſome order, judged it unfit to conſider, either how one might know what things were to be looked on as above reaſon; how far we may diſcourſe on them; or whether or no any ſupernaturally revealed propoſitions, ſuch as divines call articles of faith, ought to be reckoned among them, till I ſhould have firſt ſeriouſly enquired, whether in general we ought to admit any ſuch objects of our contemplation, as theſe, and the like queſtions ſuppoſe.

Eugen. I hope then, that this being the firſt you propoſed to enquire into, we may, without too much boldneſs, deſire to know what came into your mind about it.

Sophron. IF I had brought my conſiderations to an iſſue upon that ſubject, I ſhould, with leſs reluctancy, acquaint you with them; but ſince I have yet made but an imperfect progreſs in my enquiry, inſtead of delivering any poſitive opinion upon ſo abſtruſe a ſubject, I ſhall only tell you, that, as far as I could yet diſcern, it ſeemed to me, that among the objects our reaſon may contemplate, there are ſome, whoſe nature we cannot comprehend; others, whoſe attributes or actions are ſuch, as that we cannot underſtand how they ſhould belong to the ſubject, or elſe that we cannot conceive how they ſhould conſiſt with ſome acknowledged truth.

Eugen. So that, if I apprehend you right, you do not only admit ſome things to be above reaſon, but make no leſs than three ſorts of them.

Sophron. IF you will needs have two of them to be coincident, I ſhall not much contend; but, I think, the number you have named may, without any great inconvenience, be admitted: for by things above reaſon, I here underſtand (not falſe or abſurd ones, but) ſuch, as though the intellect ſees ſufficient cauſe (whether on the ſcore of experience, authentic teſtimony, or mathematical demonſtration) to aſſent to; yet it finds itſelf reduced, when it is converſant about them, to be ſo with a notable and peculiar diſadvantage: and this diſadvantage does uſually proceed, either from the nature of the thing propoſed, which is ſuch, that we cannot ſufficiently comprehend it; or from our not being able to conceive the manner of its exiſting and operating; or from this, that it involves ſome notion or propoſition, that we ſee not how to reconcile with ſome other thing, that we are perſwaded to be a truth. The firſt of theſe three ſorts of things may, for brevity and diſtinction ſake, be called incomprehenſible, the ſecond inexplicable, and the third unſociable. But for fear leſt the ſhortneſs I have uſed in my expreſſions may have kept them from being ſo clear, I ſhall ſomewhat more explicitely reckon up the three ſorts of things, that ſeem to me above reaſon.

THE firſt conſiſts of thoſe, whoſe nature is not diſtinctly and adequately comprehenſible by us: to which ſort perhaps we may refer all thoſe intellectual beings (if it be granted that there are ſuch) as are by nature of a higher order than human ſouls. To which ſort ſome of the angels (at leaſt of the good ones) may probably belong; but more than probably we may refer to this head the divine author of nature and of our ſouls, Almighty God, whoſe perfections are ſo boundleſs, and his nature ſo very ſingu-

lar,

lar, that it is no lefs weaknefs than prefumption to imagine, that fuch finite beings as our fouls can frame full and adequate ideas of them. We may indeed know by the confideration of his works, and particularly thofe parts of them that we ourfelves are, both that he is, and in a great meafure what he is not; but to underftand thoroughly what he is, is a tafk too great for any but his own infinite intellect: and therefore, I think, we may truly call this immenfe object, in the newly declared fenfe, *fupra-intellectual.*

Eugen. I fuppofe I may now afk, what is the fecond fort of things above reafon?

Sophron. IT confifts of fuch, as though we cannot deny that they are, yet we cannot clearly and fatisfactorily conceive, how they can be fuch as we acknowledge they are. As how matter can be infinitely, (or which is all one, in our prefent difcourfe, indefinitely) divifible; and how there fhould be fuch an incommenfurablenefs betwixt the fide and diagonal of a fquare, that no meafure, how fmall foever, can adequately meafure both the one and the other.

THAT matter is endlefly divifible, is not only the affertion of *Ariftotle* and the fchools, but generally embraced by thofe rigid reafoners, geometricians themfelves; and may be farther confirmed by the other inftance of the fide and diagonal of a fquare, whofe incommenfurablenefs is believed upon no lefs firm a proof, than a demonftration of *Euclid*, and was fo known a truth among the antients, that *Plato* is faid to have pronounced him rather a beaft than a man, that was a ftranger to it. And yet if continued quantity be not divifible without ftop, how can we conceive but that there may be found fome determinate part of the fide of a fquare, which being often enough repeated, would exactly meafure the diagonal too. But though mathematical demonftrations affure us, that thefe things are fo, yet thofe, that have ftrained their brains, have not been able clearly to conceive how it fhould be poffible, that a line (for inftance) of not a quarter of an inch long fhould be ftill divifible into leffer and leffer portions, without ever coming to an end of thofe fub-divifions; or how among the innumerable differing partitions into aliquot parts, that may be made of the fide of a fquare, not one of thofe parts can be found exactly to meafure fo fhort a line as the diagonal may be.

Eugen. THERE is yet behind, *Sophronius*, the third fort of thofe things, which, according to you, furpafs our reafon.

Sophr. I SHALL name that too, *Eugenius*, as foon as I have premifed, that fome of the reafons, that moved me to refer fome inftances to this head, do not fo peculiarly belong to thofe inftances, but that they may be applicable to others, which it was thought convenient to refer to the fecond or firft of the foregoing heads: and this being once intimated, I fhall proceed to tell you, that the third fort of things, that feem to furpafs our reafon, confifts of thofe, to which the rules and axioms and notions, whereby we judge of the truth and falfhood of ordinary, or other things, feem not to agree.

THIS third fort being fuch as are incumbred with difficulties or objections, that cannot directly and fatisfactorily be removed by them, that acquiefce in the received rules of fubordinate fciences, and do reafon but at the common rate, fuch objects of contemplation as this third fort confifts of, having fomething belonging to them, that feems not reconcilable with fome very manifeft, or at leaft acknowledged truths.

THIS it may here fuffice to make out by a couple of inftances, the one of a moral, the other of a mathematical nature. And firft, that man has a free will, in reference at leaft to civil matters, is the general confeffion of mankind: all the laws, that forbid and punifh murder, adultery, theft, and other crimes, being founded on a fuppofition, that men have a power to forbear committing them; and the fenfe men have of their being poffeft of this power over their own actions is great enough to make malefactors acknowledge

acknowledge their puniſhments to be juſt, being no leſs condemned by their own conſciences, than by their judges.

AND yet (ſome Socinians, and ſome few others excepted) the generality of mankind, whether Chriſtians, Jews, Mahometans, or Heathens, aſcribe to God an infallible preſcience of human actions, which is ſuppoſed by the belief of prophecies, and the recourſe to oracles, by one or other of which two ways the embracers of the ſeveral religions newly mentioned have endeavoured and expected to receive the informations of future things, and ſuch as depend upon the actings of men. But how a certain foreknowledge can be had of contingent things, and ſuch as depend upon the free will of man, is that, which many great wits, that have ſolicitouſly tried, have found themſelves unable clearly to comprehend; nor is it much to be admired, that they ſhould be puzzled to conceive, how an infinitely perfect being ſhould want preſcience, or that their will ſhould want that liberty, whereof they feel in themſelves the almoſt perpetual exerciſe.

THE other inſtance I promiſed you, *Eugenius*, is afforded me by geometricians: for theſe (you know) teach the diviſibility of quantity *in infinitum*, or without ſtop, to be mathematically demonſtrable. Give me leave then to propoſe to you a ſtrait line of three foot long divided into two parts, the one double to the other. I ſuppoſe then, that according to their doctrine a line of two foot is diviſible into infinite parts, or it is not: if you ſay it is not, you contradict the demonſtrations of the geometricians; if you ſay that it is, then you muſt confeſs, either that the line of one foot is diviſible into as many parts as the line of two foot, though the one be but half the other, or elſe that the infinite parts, into which the line of one foot is granted to be diviſible, is exceeded in number by the parts, into which the line of two foot is diviſible, and conſequently that the line of two foot has a multitude of parts greater than infinite. Which reaſonings may let us ſee, that we may be reduced either to reject inferences legitimately drawn from manifeſt or granted truths, or to admit concluſions, that appear abſurd; if we will have all the common rules, whereby we judge of other things, to be applicable to infinites.

AND now, gentlemen, having acquainted you with what ſorts of things ſeem to be above reaſon, I muſt, to prevent miſtakes, deſire you to take along with you this advertiſement; that though the nobleneſs and difficulty of ſo uncultivated a ſubject inclined me to offer ſomething towards the elucidating of it, by ſorting thoſe things into three kinds; yet I ſhall not, and need not in this conference, inſiſt on them ſeverally, or lay any ſtreſs on this partition. For though I have above intimated, that a propoſition may ſpeak of ſomewhat that is ſupra-intellectual; or elſe contain ſomewhat, which we cannot conceive how it may be true; or laſtly teach us ſomewhat for a truth, that we cannot reconcile with ſome other thing, that we are convinced is true; yet if but any one of theſe have true inſtances belonging to it, that may ſuffice for my main purpoſe in this place, where I need only ſhew in general, that there may be things, that ſurpaſs our reaſon, at leaſt ſo far, that they are not to be judged of by the ſame meaſures and rules, by which men are wont to judge of ordinary things; for which reaſon I ſhall often give them one common name, calling them priviledged things.

Eugen. METHINKS that to manifeſt the imperfections of our reaſon, in reference to what you call priviledged things, you need not have recourſe to the unfathomable abyſſes of the divine nature, ſince for aught I know *Pyrocles*, as well as I, may be nonplus'd by an inſtance, that came into my mind *de compoſitione continui*.

Timoth. SINCE *Sophronius* has not thought fit to give us any of the arguments of the contending parties, I ſhall be glad to know what difficulty occurred to you.

Eugen. SUPPOSE a great circle divided into its three hundred and sixty degrees; and suppose, that as great a number as you please, or can conceive, of strait lines, be drawn from the several designable parts of some one of these degrees to the centre; it is manifest, that the degrees being equal, as many lines may be drawn from any, and so from every one of the others, as from that degree, which was pitched upon.

THEN suppose a circular arch, equal to the assumed degree, to be further bent into the circumference of a little circle, having the same centre with a great one, it follows from the nature of a circle, and has been geometrically demonstrated, that the semi-diameters of a circle, how many soever they be, can no where touch one another but in the centre. Whence it is evident, that all the lines, that are drawn from the circumference to the centre of the greater circle, must pass by differing points of the circumference of the smaller, (for else they would touch one another before they arrive at the centre) and consequently, that as many lines soever, as can even mentally be drawn from the several points of the circumference of the great circle to the common centre of both circles, must all pass through different points of the little circle, and thereby divide it into as many parts (proportionably smaller) as the greater circle is divided into: so that here the circumference of the lesser circle presents us with a curve line, which was not possibly divisible into more parts than an arch of one degree, or the three hundred and sixtieth part of the circumference of the greater circle, and yet, without being lengthned, becomes divisible into as many parts as the whole circumference of the same greater circle. And though we should suppose the circumference of the internal circles not to exceed one inch, and that of the exterior circle to exceed the circumference of the terrestrial globe, or even of the firmament itself, yet still the demonstration would hold, and all the lines drawn from this vast circle would find distinct points in the lesser, to pass through to their common centre.

Timoth. THOUGH I will not pretend to confirm what *Sophronius* has been proving, by adding arguments *a priori*; yet I shall venture to say, that I think it very agreeable both to the nature of God and to that of man, that what he has endeavoured to prove true should be so; for we men mistake and flatter human nature too much, when we think our faculties of understanding so unlimited, both in point of capacity and of extent, and so free and unprepossest, as many philosophers seem to suppose. For, whatever our self-love may incline us to imagine, we are really but created and finite beings (and that probably of none of the highest orders of intellectual creatures;) and we come into the world but such, as it pleased the almighty and most free author of our nature to make us. And from this dependency and limitedness of our natures, it follows not only, that we may be (for I now dispute not whether we are) born with certain congenit notions and impressions and appetites or tendencies of mind; but also that the means or measures, which are furnished us to employ in the searching or judging of truth, are but such as are proportionable to God's designs in creating us, and therefore may probably be supposed not to be capable of reaching to all kinds, or if you please of truths, many of which may be unnecessary for us to know here, and some may be reserved, partly to make us sensible of the imperfections of our natures, and partly to make us aspire to that condition, wherein our faculties shall be much enlarged and heightned. It seems not therefore unreasonable to think, both that God has made our faculties so limited, that in our present mortal condition there should be some objects beyond the comprehension of our intellects; that is, that some of his creatures should not be able perfectly to understand some others, and yet that he has given us light enough to perceive, that we cannot attain to a clear and full knowledge of them.

Pyrot.

Pyroc. I THINK, *Sophronius*, that I now underftand what you mean by things above reafon, or as you (not unfitly) ftiled them, priviledged things: but I prefume you need not be told, that to explain the fenfe of a propofition, and to make out the truth of it, (unlefs in common notions, or things evident by their own light) are always two things, and oftentimes two very diftant ones.

Sophr. I NEED not fcruple, *Pyrocles*, to grant the truth of what you fay, but I muft not fo eafily admit your application of it; for among the examples I have been propofing, there are fome at leaft, that do not only declare what I mean by things above reafon, but are inftances, and confequently may be proofs, that fuch things there are. And to thofe I could have added others, if I had thought it unlikely, that in the progrefs of our conference, there may be occafions offered of mentioning them more opportunely.

Pyroc. I HAVE long thought, that the wit of man was able to lay a fine varnifh upon any thing, that it would recommend; but I have not till now found reafon fet awork to degrade itfelf, as if it were a noble exercife of its power to eftablifh its own impotency. And indeed it is ftrange to me, how you would have our reafon comprehend and reach things, that you yourfelf confefs to be above reafon, which is, methinks, as if we were told, that we may fee things with our eyes, that are invifible.

Sophr. I DO not think that it is to degrade the underftanding, to refufe to idolize it; and it is not an injury to reafon, to think it a limited faculty, but an injury to the author of it, to think man's underftanding infinite like his. And if what I propofed be well grounded, I affign reafon its moft noble and genuine exercife, which is to clofe with difcovered truths, in whofe embraces the perfection of the intellect too much confifts, to fuffer that perfective action to be juftly difparaging to it: and a fincere underftanding is to give, or refufe its affent to propofitions, according as they are or are not true, not according as we could or could not wifh they were fo; and methinks it were fomewhat ftrange, that impartiality fhould be made a difparagement in a judge. But, *Pyrocles*, leaving the reflection, with which you ufhered in your objection, I fhall now confider the argument itfelf, which being the weightieft, that can be framed againft the opinion you oppofe, I fhall beg leave to offer fome confiderations, wherein I fhall endeavour to anfwer it, both by proving my opinion by experience, and by fhewing that experience not to be difagreeable to reafon.

Pyroc. I SHALL very willingly liften to what you have to fay on fuch a fubject.

Sophr. I SHALL then in the firft place alledge the experience of many perfons, and divers of them great wits, who have perplexed themfelves to reconcile, I fay, not the grace of God, but even his prefcience to the liberty of man's will, even in bare moral actions: and I have found partly by their writings, and by difcourfe with fome of them, that the moft towering and fubtle fort of fpeculators, metaphyficians, and mathematicians, perchance after much racking of their brains, confefs themfelves quite baffled by the unconquerable difficulties they met with, not only in fuch abftrufe fubjects as the nature of God, or of the human foul, but in the nature of what belongs in common to the moft obvious bodies in the world, and even to the laft portions of them. You will eafily guefs, that I have my eye on that famous controverfy, whether or no a continued quantity (which every body, as having length, breadth, depth, muft be allowed to have) be made up of indivifibles. Of the perplexing difficulties of this controverfy I might give you divers confeffions, or complaints made by a fort of men too much accuftomed to bold affertions and fubtle arguments, to be much difpofed to make acknowledgments of that kind: but I fhall content myfelf with the teftimony, which one of the more famous modern fchoolmen gives both of himfelf and other learned men, and which, if I well remember, he thus expreffes. *Aggredimur cer-*

Ovied. contr. 17. Phyſ.

tinui compoſitionem, cujus hucuſque non ſuperata difficultas omnium doctorum male ingenia vexavit, neque ullus fuit, qui illam non pene inſuperabilem agnoſcat. Hanc plerique terminorum obſcuritate, illorumque replicatis & implicatis diſtinctionibus, & ſubdiſtinctionibus obtenebrant, ne aperté capiantur deſperantes rem poſſe alio modo tractari, neque rationis lucem ſuſtinere, ſed neceſſario confuſionis tenebris obtegendam, ne argumentorum evidentiâ detegatur.

AND though he had not been thus candid in his confeſſion, yet what he ſays might be eaſily concluded by him, that ſhall duly weigh, with how great, though not eqnal force of arguments, each of the contending parties imputes to the opinion it oppoſes great and intolerable abſurdities, as contained in it, or legitimately deducible from it.

Eugen. I HAVE not the vanity to think, that the weakneſs of my reaſon ought to make another diffident of the ſtrength of his: but as to myſelf, what *Sophronius* has been ſaying cannot but be confirmed by ſeveral trials, wherein having exerted the ſmall abilities I had, to clear up to myſelf ſome of the difficulties about infinites, I perceived, to my trouble, that my ſpeculations ſatiſfied me of nothing ſo much, as the diſproportionateneſs of thoſe abſtruſe ſubjects to my reaſon. But *Sophronius*, may it not be well objected, that though the inſtances you have given have not been hitherto cleared by the light of reaſon; yet it is probable they may be ſo hereafter, conſidering how great progreſs is, from time to time, made in the diſcoveries of nature, in this learned age of ours?

Sophr. IN anſwer to this queſtion, *Eugenius*, give me leave to tell you firſt, that you allow my paſt diſcourſe to hold good, for aught yet appears to the contrary: whence it will follow, that your objection is grounded upon a hope, or at moſt a conjecture, about which I need not therefore trouble myſelf, till ſome new diſcoveries about the things in queſtion engage me to a new conſideration of them. But in the mean while, give me leave to repreſent to you in the ſecond place, that though I am very willing to believe, as well as I both deſire and hope it, that this inquiſitive age we live in will produce diſcoveries, that will explicate divers of the more hidden myſteries of nature, yet I expect, that theſe diſcoveries will chiefly concern thoſe things, which either we are ignorant of for want of a competent hiſtory of nature, or we miſtake by reaſon of erroneous prepoſſeſſions, or for want of freedom and attention in our ſpeculations. But I have not the like expectations as to all metaphyſical difficulties, (if I may ſo call them,) wherein neither matters of fact, nor the hypotheſes of ſubordinate parts of learning, are wont much to avail. But however it be as to other abſtruſe objects, I am very apt to think, that there are ſome things relating to that infinite and moſt monadical being (if I may ſo ſpeak) that we call God, which will ſtill remain incomprehenſible even to philoſophical underſtandings. And I can ſcarce allow myſelf to hope to ſee thoſe obſtacles ſurmounted, that proceed not from any perſonal infirmity, or evitable faults, but from the limited nature of the intellect. And to theſe two conſiderations, *Eugenius*, I ſhall, in anſwer to your queſtion, add this alſo; that as mens inquiſitiveneſs may hereafter extricate ſome of thoſe grand difficulties, that have hitherto perplexed philoſophers, ſo it may poſſibly lead them to diſcover new difficulties, more capable than the firſt of baffling human underſtandings. For even among the things, wherewith we are already converſant, there are divers, which we think we know, only becauſe we never, with due attention, tried, whether we can frame ſuch ideas of them, as are clear and worthy for a rational ſeeker and lover of truth to acquieſce in. This the great intricacy, that conſidering men find, in the notions commonly received of ſpace, time, motion, &c. and the difficulties of framing perſpicuous and ſatisfactory apprehenſions even of ſuch obvious things, may render highly probable. We ſee alſo, that

that the angle of contact, the doctrine of *asymptotes*, and that of surd numbers and incommensurable lines, all which trouble not common accomptants and surveyors, (who, though they deal so much in numbers and lines, seldom take notice of any of them perplex the greatest mathematicians, and some of them so much, that they can rather demonstrate, that such affections belong to them, than they can conceive how they can do so. All which may render it probable, that mens growing curiosity is not more likely to find the solutions of some difficulties, than to take notice of other things, that may prove more insuperable than they.

Tim. THIS conjecture of yours, *Sophronius*, is not a little favoured by the *rota Aristotelica*; for though the motion of a cart-wheel is so obvious, and seems so plain a thing, that the carman himself never looks upon it with wonder; yet after *Aristotle* had taken notice of the difficulty, that occurred about it, this trivial phænomenon has perplexed divers great wits, not only schoolmen, but mathematicians, and continues yet to do so; there being some circumstances in the progressive motion and rotation of the circumference of a wheel, and its nave, or of two points assigned, the one in the former, and the other in the latter, that have appeared too subtle, and even to modern writers, so hard to be conceived and reconciled to some plain and granted truths, that some of them have given over the solution of the attending difficulties as desperate; which perchance, *Pyrocles*, would not think strange, if I had time to insist on the intricacies, that are to be met with in a speculation, that seems so easy as to be despicable.

Sophr. YOUR instance, *Timotheus*, must be acknowledged a very pregnant one, if you are certain, that a better account cannot be given of the *rota Aristotelica*, than is wont to be in the schools, by those peripateticks, that either frankly confess the difficulties to be insoluble, or less ingenuously pretend to give solutions of them, that suppose things not to be proved, or perhaps so much as understood (as rarefaction and condensation strictly so called,) or lose the question and perhaps themselves, by running up the dispute into that most obscure and perplexing controversy *de compositio e continui*.

Eugen. I AM content to forbear pressing any further at present an objection, much of whose force depends on future contingents; and I shall the rather dismiss the proof drawn from experience, that I may the sooner put you in mind of your having promised us another argument to the same purpose, by manifesting the opinion to be agreeable to reason.

Sophr. I UNDERSTAND your pleasure, *Eugenius*, and shall endeavour to comply with it; but the difficulty and intricateness of the subject of our discourse obliges me to do it by steps; and for fear we should want time for more necessary things, I will not now stay to examine, whether all the things, that hitherto have appeared above reason, be impenetrable to us, because of an essential disability of our own understandings, proceeding from the imperfection and limitedness of their nature; or only because of some other impediment, such as may be especially the condition of the soul in this life, or the infirmities resulting from its state of union with a gross and mortal body.

FORBEARING then to discourse how this came into my mind, and what thoughts I had upon it, I shall proceed in my considerations; and to clear the way for those that are to follow, I shall in the first place observe to you, that whatever be thought of the faculty *in abstracto*, yet reason operates according to certain notions or ideas, and certain axioms and propositions, by which, as by prototypes or models, and rules and measures, it conceives things, and makes estimates and judgments of them. And indeed when we say, that such a thing is consonant to reason, or repugnant to it, we use to mean, that it is either immediately or mediately deducible from, or at least consistent with, or contradictory to one or other of those standard notions or rules.

AND

AND this being premised, I consider in the next place, that if these rules and notions be such, as are abstracted only from finite things, or are congruous but to them; they may prove useless or deceitful to us, when we go about to stretch them beyond their measure, and apply them to the infinite God, or to things, that involve an infiniteness either in multitude, magnitude, or littleness.

To illustrate and confirm this notion, give me leave to represent in the third place, that, in my opinion, all the things, that we naturally do know or can know, may be divided into these two sorts; the one such as we may know without a medium; and the other such as we cannot attain to, but by intervention of a medium, or by a discursive act. To the first belong such notions as are supposed to be connate, or, if you please, innate, such as that two contradictories cannot be both together true; the whole is greater than any part of it; every (entire) number is either even or odd, *&c.* and also those other truths, that are assented to upon their own account without needing any medium to prove them; because that as soon as, by perspicuous terms, or fit examples, they are clearly proposed to the understanding, they discover themselves to be true so manifestly by their own light, that they need not be assisted by any intervening proposition, to make the intellect acquiesce in them; of which kind are some of *Euclid*'s axioms, as that, if to equal things equal things be added, the totals will be equal; and that two right lines cannot include a space. To the second sort of things knowable by us belong all, that we acquire the knowledge of by ratiocinations, wherein by the help of intervening propositions or mediums we deduce one thing from another, or conclude affirmatively or negatively one thing of another. This being supposed, and we being conscious to ourselves, if it were but upon the score of our own infirmities and imperfections, that we are not authors of our own nature; for aught we know, it may be true, and all the experience we have hitherto had leads us to think it is true, that the measures suggested to us either by sensations, the results of sensible observation, or the other instruments of knowledge, are such as fully reach but to finite things or beings, and therefore are not safely applicable to others. And divers of those very principles, that we think very general, may be (if I may so speak) but gradual notions of truth, and but limited and respective, not absolute and universal.

AND here give me leave, as a farther consideration, to take notice to you, that though perfect syllogisms be counted the best and most regular forms, that our ratiocinations can assume; yet even the laws of these are grounded on the doctrine of proportions: for even between things equal there may be a proportion, (namely that of equality;) upon which ground I suppose it is, that mathematical demonstrations have been publickly proposed of the grand syllogistical rules. And in consequence of this, I shall add, that geometricians will tell you, that there is no proportion betwixt a finite line and an infinite, because the former can never be so often taken, as to exceed the latter, which, according to *Euclid*'s definition of proportion, it should be capable to do. Of which premises the use I would make is to persuade you, that since the understanding operates but by the notions and truths it is furnished with, and these are its instruments, by proportion to which it takes measures, and makes judgment of other things; these instruments may be too disproportionate to some objects to be securely employed to determine divers particulars about them: so the eye being an instrument, which the understanding employs to estimate distances, we cannot by that safely take the breadth of the ocean, because our sight cannot reach far enough to discover, how far so vast an object extends itself. And not only the common instruments of surveyors, that would serve to measure the height of an house or a steeple, or even a mountain, cannot enable them to take the distance of the moon; but, when astronomers do, by supposition, take a chain, that reaches to the centre of the earth, (and therefore is by the moderns judged to be above four thousand miles long;) even then I say, though by help of

Rationem habere inter se quantitates dicuntur quæ possunt multiplicatæ sese mutuo superare.
Definit. v. Elem. V. Euclidis.

of this and the parallaxes, they may tolerably well meaſure the diſtance of ſome of the nearer planets, eſpecially the moon; yet with all their great induſtry, they cannot by the ſame way (or perhaps any other yet known) with any thing tolerable accurateneſs, meaſure the diſtance of the fixed ſtars; the ſemi-diameter of the earth bearing no ſenſible proportion to that of ſo vaſt a ſphere as the firmament, whoſe diſtance makes the parallaxes vaniſh, it being as to ſenſe all one, whether, at ſo great a remove, a ſtar be obſerved from the centre, or from the ſurface of the earth.

Eugen. In a matter ſo abſtruſe, a little illuſtration by examples may be very proper and welcome.

Sophron. It is ſcarce poſſible to find very appoſite examples, to illuſtrate things of a kind ſo abſtruſe and heteroclite, as thoſe may well be ſuppoſed, that do ſurpaſs our reaſon.

But yet ſome aſſiſtance may be borrowed from what we may obſerve in that other faculty of the mind, which is moſt of kin to the intellect, I mean the imagination; for when, for inſtance, I think of a triangle or a ſquare, I find in my fancy an intruitive idea (if I may ſo call it) of thoſe figures; that is a picture clear and diſtinct, as if a figure of three ſides, or four equal ſides and angles, were placed before my eyes.

But if I would fancy a myriagon, or a figure conſiſting of ten thouſand equal ſides, my imagination is overpowered with ſo great a multitude of them, and frames but a confuſed idea of a polygon with a very great many ſides: for if (to ſpeak ſuitably to what the excellent *Des Cartes* has well obſerved in the like caſe) a man ſhould endeavour to frame ideas of a myriagon or a chiliagon, they would be both ſo confuſed, that his imagination would not be able clearly to diſcriminate them, though the one has ten times as many ſides as the other. So if you would imagine an atom, of which perhaps ten thouſand would ſcarce make up the bulk of one of the light particles of duſt, that ſeem to play in the ſun-beams, when they are ſhot into a darkened place; ſo extraordinary a littleneſs, not having fallen under any of our ſenſes, cannot truly be repreſented in our imagination. So when we ſpeak of God's primity (if I may ſo call it,) omnipotence, and ſome other of his infinite attributes and perfections, we have ſome conceptions of the things we ſpeak of, but may very well diſcern them to be but inadequate ones: and though divers propoſitions relating to things above reaſon ſeem clear enough to ordinary wits; yet he, that ſhall with a competent meaſure of attention, curioſity, and ſkill, conſider and examine them, ſhall find, that either their parts are inconſiſtent with one another, or they involve contradictions to ſome acknowledged or manifeſt truths, or they are veiled over with darkneſs, and incumbered with difficulties, from whence we are not able to reſcue them. Thus when the ſide and diagonal of a ſquare are propoſed, we have clear and diſtinct ideas of each of them apart; and when they are compared, we may have a conception of their incommenſurableneſs. But yet this negative notion, if it be thoroughly conſidered, and far enough purſued, clearly contains that of a ſtrait line's being diviſible *in infinitum*; and that diviſibility is incumbered with ſo many difficulties, and is ſo hard to be reconciled to ſome confeſſed dictates of reaſon, that (as we have ſeen already) philoſophers and geometricians, that are convinced of the truth, are to this day labouring to extricate themſelves out of thoſe perplexing intricacies.

I will not trouble you with the puzzling, if not inſuperable difficulties, that incumber the doctrine of eternity, as it is wont to be propoſed in the ſchools of divines and philoſophers, leſt you ſhould alledge, that theſe difficulties ſpring rather from the bold aſſumptions and groundleſs ſubtleties of the ſchoolmen, than from the nature of the thing itſelf: but I will propoſe ſomewhat, that cannot be denied, which is, that ſome ſubſtance or other, whether, as I believe, God, or as the Peripateticks ſay, the world, or as the Epicureans contend, matter, never had a beginning, that is, has been for ever. But when we ſpeak of an eternity *à parte ante* (as they call it,) we do not ſpeak of a thing, whereof we have no conception at all, as will appear to a conſidering perſon; and yet this general notion we have is ſuch, that when we come attentively to examine it by the ſame ways, by which we judge of almoſt all other things, the intellect is non-plus'd: for we muſt conceive, that the time effluxed ſince *Adam* (or any other man as remote from us as he is ſaid to have been) began to live, bears no more proportion to the duration of God, or of matter, than to thoſe few minutes I have employed about mentioning this inſtance. Nay, if we would be Ariſtotelians, the ſame thing may be ſaid as to thoſe men, that lived many thouſand millions of years before the time we reckon that *Adam* began to live in: for each of theſe times being finite, and meaſurable by a determinate number of years, bear no proportion to that infinite number of years (or ſomewhat that is equivalent) which muſt be allowed to a duration, that never had a beginning. And as there are ſome things, whoſe nature and conſequences poſe our faculties, ſo there are others, whereof though we have a notion, yet the *modus operandi* is beyond our comprehenſion; I do not mean only the true and certain *modus operandi*, but even an intelligible one. As, though divers learned men, eſpecially Carteſians, and that upon a philoſophical account, aſſert, that God created the world; yet how a ſubſtance could be made out of nothing (as they, and the generality of Chriſtians confeſſedly hold) I fear we cannot conceive. And though all philoſophers, very few excepted, believe God to be the maker of the world (out of preexiſtent matter;) yet how he could make it but by locally moving the parts of the matter it was to conſiſt of, and how an incorporeal ſubſtance can move a body, which it may paſs through without reſiſtance, is that, which I fear will be found hardly explicable: for if it be ſaid, that the ſoul, being an immaterial ſubſtance, can nevertheleſs move the limbs of the human body rightly diſpoſed, I ſhall anſwer, that it does not appear, that the rational ſoul doth give any motion to the parts of the body, but only guide or regulate that, which ſhe finds in them already.

Timoth. MAY it not then be rationally ſaid, that by making obſervations of ſuch things, that are the proper objects of our faculties, and by making legitimate deductions from ſuch obſervations, and from our other knowledges whether innate or acquired, we may come to be certain, that ſome things are, and ſo have general and dark ideas of them, when at the ſame time we are at a loſs to conceive how they can be ſuch, or how they can operate and perform what they do, ſuppoſing the truth and ſufficiency of ſome other things we are convinced of? To be ſhort, negative apprehenſions we may have of ſome priviledged things; and poſitive, but indiſtinct, apprehenſions we may have of others; and that is enough to make us in ſome ſort underſtand ourſelves, and one another, when we ſpeak of them; though yet when we ſufficiently conſider what we ſay, we may find, that our words are not accompanied with clear, diſtinct, and ſymmetrical conceptions of thoſe abſtruſe and perplexing things we ſpeak of. And ſince, as hath been already ſhewn, we find by experience, that we are unable ſufficiently to comprehend things, that by clear and legitimate conſequences may be evinced to be, why ſhould not this cogently argue, that ſome of our conceptions may be of things, to which ſomewhat belongs, that tranſcends our reaſon, and ſurpaſſes our comprehenſion? And if I would play the logician with *Pyrocles*, I would tell him, that his objection deſtroys his opinion: for ſince he talks to us of what is incomprehenſible, that term muſt or muſt not be attended with ſome ſuitable idea: if it be not, let him conſider, whether, in his own phraſe, he ſpeaks ſenſe and not like a parrot; but if it be, let him then confeſs, that one may have ſome kind of idea of a thing incomprehenſible. But, *Pyrocles*, whether or no you think I prevaricate in this, you will not, I hope, ſuſpect me of doing it, in adding, that when natural theology had taught men, (as well

well philosophers as others,) to believe God to be an infinitely perfect being, we ought not to say, that they had no idea of such a being, because they had not a clear and adequate one. And since *Aristotle* discourses *ex professo* and prolixly enough *de infinito*, and cites the antienter philosophers for having done so before him; and since (besides his commentators and followers) *Democritus*, *Epicurus*, followed by *Gassendus* and other late philosophers, maintain, either that the world is boundless, or that space (real or imaginary) is not finite in extent, or that the world consists of atoms infinite in number; I hope you will not put such an affront upon all these great persons, as to think they said they knew not what, when they discoursed *de infinito*, as they must have done, if they spake without ideas of the things they spake of, though it may be justly supposed, that the subject being infinite, the ideas they framed of it could not be comprehensive and accurate.

Eugen. So that according to you, *Sophronius*, it may be said, that by reason we do not properly perceive things above reason, but only perceive, that they are above reason, there being a dark and peculiar kind of impression made upon the understanding, while it sets itself to contemplate such confounding objects; by which peculiarity of impression, as by a distinct and unwonted kind of internal sensation, the understanding is brought to distinguish this sort of things, namely, transcendent or priviledged ones, from others, and discern them to be disproportionate to the powers, with which it uses throughly to penetrate subjects, that are not impervious to it. As when the eye looks into a deep sea, though it may pierce a little way into it, yet when it would look deeper, it discovers nothing but somewhat which is dark and indistinct, which affects the sensory so differingly from what other more genuine objects are wont to do, that by it we easily discern, that our sight fails us in the way before it arrives at the bottom, and consequently that there may be many things concealed there, that our sight is unable to reach.

Timoth. I GUESS, gentlemen, by the silence you seem to conspire in, after so long a debate, that you have now said as much as at present you think fit to say for and against this proposition, that there are things above our reason.

Sophr. I SHALL not, for my part, cross your observations, *Timotheus*, but instead of adding any new proofs, shall only desire you to look back upon those I have presented you already, and to let me remind you, that of the two arguments, by which I attempted to shew, that there are some things above reason, the first and chiefest was suggested by experience, and the other, which was drawn from the nature of things and of man, was brought, as it were, *ex abundanti*, to illustrate and confirm the former, and give occasion to some hints about priviledged subjects. And therefore though I hope what has been discoursed by these gentlemen and me, may be able to persuade *Pyrocles*, that the acknowledgment, that some things are above reason, may fairly comply with the dictates of it; yet whatever he thinks of the cogency of our discourse, the truth of the main conclusion may be sufficiently evinced by our first argument drawn from experience: for if we really find, that there are things, which our reason cannot comprehend, then whether the account these gentlemen and I have given, why our faculties are insufficient for these things, be good or not; yet still some true account or other there must be of that insufficiency. And as we should very thankfully receive from *Pyrocles* any better account than what we have propounded, so if he cannot assign any other, I hope he will join with us in looking upon this as very agreeable to our hypothesis; since hereby some things must appear to us so sublime and abstruse, that not only we find we are not able to comprehend them, but that we are unable to discern so much as upon what account it is, that they cannot be comprehended by us.

Eugen. I AM not averse, *Sophronius,* from your paradox about gradual notions; and I am the more inclined to think, that some of the axioms and rules, that are reputed to be very general, are not to be indifferently extended to all subjects and cases whatsoever; when I consider the differing apprehensions, that the mind may frame of the same object, as well according to the vigour, or (if I may so call it) rank of the understanding, as according to the differing information it is furnished with. For if one should propose to a child, for instance, of four or five years old the demonstration of the one hundred and seventeenth proposition of *Euclid*'s tenth book, wherein he proves the side and diagonal of a square to be incommensurable; though possibly he may be able to read the words, that express the theorem, and though he have eyes to see the scheme imployed for the demonstration, yet if you should spend a whole year about it, you would never be able to make him understand it, because it is quite above the reach of a child's capacity. And if one should stay till he be grown a man, yet supposing him to have never learned geometry, though he may easily know what you mean by two incommensurable lines, yet all the reason he has attained to in his virile age would but indispose him to attain to that demonstration; for all the experience he may have had of lines will but have suggested to him as a manifest and general truth, that of any two strait lines we may, by measuring, find how many feet, inches, or other determinate measure, the one exceeds the other. And though one, that has been orderly instructed in all that long train of propositions, that in *Euclid*'s elements precede the one hundred and seventeenth of the tenth book, will be also able to arrive at an evidence of this truth, that those two lines are incommensurable; yet (as *Sophronius* formerly noted) how it should be possible, that two short lines being proposed, whereof each by itself is easily measurable among those innumerable multitudes of parts, into which each of them may be mentally divided, there should not be any one capable of exactly measuring both, is that, which even a geometrician, that knows it is true, is not well able to conceive. But, gentlemen, that you may not accuse my digression, I shall urge these comparisons no further, my scope in mentioning them being to observe to you, that for aught we know to the contrary, such a difference of intellectual abilities, as is but gradual in children and men, may be essential in differing ranks of intellectual beings. And so it may be, that some of those axioms, that we think general, may, when we apply them to things, whereof they are not the true and proper measures, lead us into error, though perhaps intellects of an higher order may unriddle those difficulties, that confound us men; which conjecture I should confirm by some things, that would be readily granted me by Christians, if I thought it proper to play the divine in a discourse purely philosophical.

Pyrocl. You, gentlemen, have taken the liberty to make long discourses; and I shall not much blame you for it, because it is a thing as more easily, so more speedily done, to propose difficulties than to solve them: yet methinks amongst you all, you have left one part of my objection unanswered, not to say untouched.

Sophr. I SUPPOSE, *Pyrocles,* you mean what you said about discerning invisible things with the eye: but I purposely forbore to take notice of that, because I foresaw it might be more seasonably done, after some other points had been cleared. Wherefore give me leave now to represent to you, as a corollary from the foregoing discourses, that nothing hinders, but that we may reasonably suppose, that the great and free author of human nature, God, so framed the nature of man, as to have furnished his intellective faculty with a light, whereby it cannot only make estimates of the power of a multitude of other things, but also judge of its own nature and power, and discern some at least of the limits, beyond which it cannot safely exercise its act of particularly and peremptorily judging and defining. And now that God, who (as I said) is a most free

free agent, may have given the mind of man ſuch a limited nature, accompanied with ſuch a meaſure of light, you will not, I preſume, deny: but the queſtion is, you will tell me, whether he hath done ſo? But I hope what has been formerly diſcourſed by theſe gentlemen and me has put that almoſt quite out of queſtion. However, I ſhall now invite you to obſerve with me, that the rational ſoul does not only paſs judgments about things without her, but about herſelf, and what paſſes within her: ſhe ſearches out and contemplates her own ſpirituality and union with the body. The intellect judges, wherein its own nature conſiſts, and whether or no itſelf be a diſtinct faculty from the will. And to come yet cloſer to the point, be pleaſed to conſider, that logic and metaphyſics are the works of the human intellect, which, by framing thoſe diſciplines, manifeſts, that it does not only judge of ratiocinations, but of the very principles and laws of reaſoning, and teaches what things are neceſſary to the obtaining of an evidence and certainty, and what kind of mediums they are, from whence you muſt not expect any demonſtrative arguments concerning ſuch or ſuch a ſubject. To theſe things it is agreeable, that if we will compare the bodily eye with the underſtanding, which is the eye of the mind, we muſt allow this difference, that the intellect is as well a looking-glaſs as a ſenſory, ſince it does not only ſee other things, but itſelf too, and can diſcern its own blemiſhes or bad conformation, or whatever other infirmities it labours under. Upon which conſideration, we may juſtify the boldneſs of our excellent *Verulam*, who, when he ſets forth the four ſorts of idols (as he calls them,) that miſlead the ſtudiers of philoſophy, makes one of them to be *Idola Tribus*, by which he means thoſe notions, that, though radicated in the very nature of mankind, are yet apt to miſlead us: which may confirm what I was ſaying before, that the ſoul, when duly excited, is furniſhed with a light, that may enable her to judge even of divers of thoſe original notions, by which ſhe is wont to judge of other things. To be ſhort, the ſoul upon trial may find by an inward ſenſe, that ſome things ſurpaſs her forces, as a blind man, that were ſet to lift up a rock, would quickly find it too unweildy to be managed by him; and the utmoſt exerciſe of his ſtrength would but convince him of the inſufficiency of it to ſurmount ſo great a weight or reſiſtance; ſo that we do not pretend, that the eye of the mind ſhould ſee inviſibles, but only that it ſhall diſcern the limits of that ſphere of activity, within which nature hath bounded it, and conſequently that ſome objects are diſproportionate to it. And I remember, that *Ariſtotle* himſelf ſays, that the eye ſees both light and darkneſs; which expreſſion, though ſomewhat odd, may be defended by ſaying, that though ſince darkneſs is a privation, not a being, it cannot properly be the object of ſight, yet it may be perceived by means of the eye, by the very differing affection, which that organ reſents, when it is impreſſed on by luminous or enlightned objects, and when it is made uſeleſs to us by darkneſs.

Timoth. WHAT you have ſaid, *Sophronius*, has in great part prevented one thing, that might be ſaid to ſtrengthen *Pyrocles*'s objection; namely, that whereas when we ſee with our bodily eyes, there is beſides the outward organ an internal and rational faculty, that perceives by the help of the eye that, which is not directly the object of ſight; in the eye of the mind, the intellect, there is but one faculty to perceive and judge: for, according to your notion, it may be well anſwered, that the intellect being capable by its proper light to judge of itſelf and its own acts as well as of other things, there is no need of two principles, the one to perceive and the other to judge, ſince one is ſufficient for both thoſe purpoſes.

Pyrocl. WHEN I have time to reflect on all that I have heard alledged amongſt you, gentlemen, I ſhall conſider how far your arguments ought to obtain my aſſent: but in the mean while I muſt tell you, that they will ſcarce have all the ſucceſs I preſume you desire, unless you add somewhat to free me from what yet sticks with me of a scruple; that is much of the nature of that, which I formerly proposed, being this; how we can justify our presuming to discourse at all of things transcending reason? For I cannot understand, how a man, that admits your opinions, can intelligibly speak (and to speak otherwise misbecomes a rational creature) of what is infinite, or any thing that surpasses our reason; since, when we discourse of such things, either our words are, or are not accompanied with clear and distinct ideas or conceptions of the things we speak of: if they be not, what do we other than speak nonsense, or (as hath been already said) like parrots entertain our hearers with words, that we ourselves do not understand? and if they be, then we do in effect comprehend those things, which yet you would have me think to be, on some account or other, incomprehensible.

Sophr. I acknowledge this difficulty, *Pyrocles*, to be a great one; but yet I think it not so great, as that it ought to interdict us all discoursing of things above reason: and this would perhaps appear probable enough, if, as your objection borrows much of what you have formerly alledged, so I may be allowed, as well to repeat some things as propose others, in making answer to it.

Timoth. I for my part shall not only give you my consent to do so, but make it my request, that you would do it; for when I look back upon our conference, methinks I plainly perceive, that partly the objections of *Pyrocles*, and partly some (I fear impertinent) interpositions of mine, have kept your discourse from being so methodical as otherwise you would have made it; and therefore to be reminded of some of the chief points of your doctrine, as well as to connect them with those you shall judge fit to strengthen or illustrate them, may much conduce to make us both understand it more clearly, and remember it better.

Eugen. I am much of your mind, *Timotheus*; but though my interpositions have been far more frequent and much less pertinent than yours, yet I am not troubled, that the method of our conference has been so much disturbed; because I think such a free way of discoursing, wherein emergent thoughts, if they be considerable, are permitted to appear as they arise in the mind, is more useful than a nice method in a debate about an uncultivated and highly important subject, in which I think we should aim at first rather to inquire than to resolve, and to procure as many hints and considerations as we can, in order to our fuller information against our next meeting, without suppressing any that is true or useful, only because it agrees not so well with a regular method, as it does with the design of our conference.

Sophron. Without reflecting upon either of those gentlemen, that have been pleased to accuse themselves, I shall readily comply with the motion made by *Timotheus*, and after having proposed some distinctions, make application of them.

And the better to clear this matter in reference to *Pyrocles*'s objection, I shall first take the liberty to make some distinctions of the notions or conceptions of the mind, and for bevity sake give names to those I have now occasion to employ. I consider then, that whether the conceptions or ideas we have of things be simple or compounded, they may be distinguished into such as are particular or distinct, and such as are only general, dark, and confused, or indistinct. So when a navigator to unknown countries first gets a sight of land, though he may be satisfied that it is land, yet he has but a very dark and confused picture of it made in his eye, and cannot descry, whether or no the shore be rocky, or what creeks or harbours (if any) it have in it; much less whether the coast be well inhabited, and if it be, what kind of buildings it has; all which he may plainly and distinctly see upon his going ashore. And this mention of the sea puts me in mind to point at another distinction, which is, that of some things we have an adequate, of others but an inadequate conception; as if we suppose the navigator, I was speaking

ſpeaking of, ſhould look towards the main ſea, though he might ſee a good way diſtinctly, yet at length it would appear ſo darkly and confuſedly to him, that at the verge of the ſenſible horizon, his ſight would make him judge, that the ſea and ſky come together; and yet he would conclude, that the utmoſt part of the ſea he could deſcry, was but a part of the ocean, which may, for aught he knows, reach to a vaſt extent beyond the viſible horizon.

To our confuſed, and often alſo to our inadequate conceptions, belong many of thoſe, that may be called negative, which we are wont to employ, when we ſpeak of privations or negations, as blindneſs, ignorance, death, *&c.* We have a poſitive idea of things, that are ſquare and round, and black and white, and in ſhort of other things, whoſe ſhapes and colours make them the objects of our ſight: but when we ſay, for inſtance, that a ſpirit or an atom is inviſible, thoſe words are attended with a negative conception, which is commonly but dark and confuſed, becauſe it is indefinite, and removes or lays aſide thoſe marks, by which we are wont clearly to perceive and diſtinguiſh viſible ſubſtances. And when we ſay, that ſuch a thing is impoſſible, we have ſome kind of conception of what we ſpeak of, but it is a very obſcure and indiſtinct one at beſt, exhibiting only a general and very confuſed repreſentation of ſome ways, whereby one might think the thing likely to be effected, if it were at all performable, accompanied with a perception of the inſufficiency of thoſe ways. There is yet another difference in the notions we have of things, which though not wont to be obſerved, is too important to be here pretermitted, and it is this; that of ſome things we have a knowledge, that, for want of a fitter term, may be called primary or direct; and of ſome other things the knowledge we have is acquired but by inferring it from ſome more known or clearer truth, and ſo may be called inferred or illative knowledge. As when a geometrician defines to me an hyperbola, I quickly gain a clear and diſtinct idea of it; but when he proves to me, that this hyperbola may have ſuch a relation to a ſtrait line, which he calls aſymptote; that this line being continued ſtill comes nearer or nearer to the prolonged ſide of the hyperbole, and yet how far ſoever both be drawn, it will never come to touch it; his ſubtle demonſtrations preſent me with an inferred and illative truth, at which we arrived not but by the help of a train of ratiocinations, and on which if we exerciſe our imagination, we ſhall find this factitious truth, if we may ſo call it, accompanied but with a very dim and confuſed idea. To the foregoing diſtinctions give me leave to add but this one more, which belongs chiefly to the notions we have of true or falſe propoſitions; namely, that of our conceptions of things, ſome are ſymmetrical (if I may ſo call them) or every way conſiſtent, by which I mean thoſe that have theſe two qualifications, the one, that all the parts are conſiſtent among themſelves, and the other, that the entire idea is conſiſtent with all other truths; and ſome are chimerical or aſymmetrical, by which I underſtand thoſe, that are either ſelf-deſtroying by the contrariety of the parts themſelves they are made up of, as if one ſhould talk of a triangular ſquare, or a ſun-ſhiny night; or being extravagant, lead to ſome manifeſt abſurdity, that may be legitimately inferred from them, or into inextricable difficulties, or involve a real repugnancy to ſome acknowledged truth, or rule of reaſon.

To what I have hitherto ſaid I muſt add theſe two obſervations: the firſt, that the mind of man is ſo framed, that when ſhe is duly inſtructed and is not wanting to herſelf, ſhe can perceive a want of light in herſelf for ſome purpoſes, or of clearneſs and completeneſs in the beſt ideas ſhe is able to frame of ſome things, and on this account can ſo far take notice of the extent and imperfection of her own faculties, as to diſcern, that ſome objects are diſproportionate to her; as when we attentively conſider the dimenſions of ſpace, or (if the Carteſians judge aright, that body is nothing but extended ſubſtance) thoſe of the univerſe, we may by trial perceive, that we cannot conceive them ſo great, but that they may be yet greater, or, if you pleaſe, may exceed the bounds, how remote ſoever, which our former conception preſumed to aſſign them; which may be illuſtrated by what happens to the eye, when it looks upon the main ſea, ſince we eaſily grow ſenſible, that how far ſoever we can diſcover it, yet our ſight falls far ſhort of the extent of that vaſt object. And it is by the ſenſe, which the mind has of her own limitedneſs and imperfection on certain occaſions, that I think we may eſtimate what things ought not, and what ought to be looked upon as things above reaſon; for by that term, I would not have you think I mean ſuch things, as our rational faculty cannot at all reach to, or has not any kind of perception of, for of ſuch things we cannot in particular either ſpeak or think like men; but my meaning is this, that whereas the rational ſoul is conſcious to her own acts, and feels, that ſhe knows divers ſorts of things truly and clearly, and thereby juſtly concludes them to be within the compaſs of her faculties; when ſhe contemplates ſome few things, that ſeem to be of another order, ſhe is convinced, that however ſhe ſtrain her power, ſhe has no ſuch ideas or perception of them, as ſhe has or may have of thoſe objects, that are not diſproportionate to her faculties: and this is my firſt obſervation.

THE other thing that I was to obſerve about the nature of the mind is, that it is ſo conſtituted, that its faculty of drawing conſequences from known truths is of greater extent than its power of framing clear and diſtinct ideas of things; ſo that by ſubtle or ſucceſſive inferences, it may attain to a clear conviction, that ſome things are, of whoſe nature and properties (or at leaſt of ſome of them) it can frame no clear and ſatisfactory conceptions. And that men ſhould be better able to infer propoſitions about divers things, than to penetrate their nature, needs the leſs be wondered at, both becauſe it is oftentimes ſufficient for our uſes to know that ſuch things are, though that knowledge be not accompanied with a clear and diſtinct idea; and becauſe oftentimes the rules (ſuch as, whatever is produced muſt have a cauſe; and, from truth nothing rightly follows but truth) are clear and eaſy, that enable the mind to infer concluſions about things, whoſe nature is very dark and abſtruſe.

Eugen. I know, *Sophronius*, that you have not laid down theſe preliminary diſtinctions and remarks, without deſigning to make uſe of them, which the little time, that now remains to manage our conference in, calls upon to proceed to do.

Sophron. I was juſt going to ſay, *Eugenius*, that, after what I have premiſed, I hope it may be ſeaſonable to apply the newly delivered notions to the three ſorts of things, that I formerly repreſented as being in ſome ſenſe above reaſon. For I conſider, that there are ſome objects of ſo immenſe and peculiar a nature, that (if I may ſo ſpeak) by an eaſy view of the mind, that is without any ſubtle and laborious diſquiſition, the ſoul diſcerns, and, as it were, feels the object to be diſproportionate to her powers: and accordingly if ſhe thinks fit to try, ſhe quickly finds herſelf unable to frame conceptions of them fit to be acquieſced in; and this ſort of objects I do upon that account call inconceivable, or (on ſome occaſions) ſupra-intellectual.

BUT when by attentively conſidering the attributes and operations of things, we ſometimes find, that a thing hath ſome property belonging to it, or doth perform ſomewhat, which by reflecting on the beings and ways of working that we know already, we cannot diſcern to be reducible to them or derivable from them, we then conclude this property or this operation to be inexplicable; that is, ſuch, as that it cannot ſo much as in a general way be intelligibly accounted for; and this makes the ſecond ſort of our things above reaſon. But this is not all, for the rational ſoul, that is already furniſhed with innate, or at leaſt primitive ideas and rules of true and falſe, when ſhe comes to examine certain things, and make ſucceſſive inferences about them, ſhe finds (ſometimes to her wonder

wonder as well as trouble) that ſhe cannot avoid admitting ſome conſequences as true and good, which ſhe is not able to reconcile to ſome other manifeſt truth or acknowledged propoſition. And whereas other truths are ſo harmonious, that there is no diſagreement between any two of them, the heteroclite truths I ſpeak of appear not ſymmetrical with the reſt of the body of truths, and we ſee not how we can at once embrace theſe and the reſt, without admitting that grand abſurdity, which ſubverts the very foundation of our reaſonings, that contradictories may both be true. As in the controverſy about the endleſs diviſibility of a ſtrait line, ſince it is manifeſt, that a line of three foot, for inſtance, is thrice as long as a line of one foot, ſo that the ſhorter line is but the third part of the longer, it would follow, that a part of a line may contain as many parts as a whole, ſince each of them is diviſible into infinite parts; which ſeems repugnant to common ſenſe, and to contradict one of thoſe common notions in *Euclid*, whereupon geometry itſelf is built. Upon which account I have ventured to call this third ſort of things above reaſon aſymmetrical or unſociable, of which eminent inſtances are afforded us by thoſe controverſies (ſuch as that of the *compoſitio continui)* wherein which ſide ſoever of the queſtion you take, you will be unable directly and truly to anſwer the objections, that may be urged to ſhew, that you contradict ſome primitive or ſome other acknowledged truth.

THESE, *Eugenius*, are ſome of the conſiderations, by which I have been induced to diſtinguiſh the things, that to me ſeem to overmatch our reaſon, into three kinds. For of thoſe things I have ſtiled unconceivable, our ideas are but ſuch, as a moderate attention ſuffices to make the mind ſenſible, that ſhe wants either light or extent enough to have a clear and full comprehenſion of them: and thoſe things, that I have called inexplicable, are thoſe, which we cannot perceive to depend upon the ideas we are furniſhed with, and to reſemble in their manner of working any of the agents, whoſe nature we are acquainted with: and laſtly, thoſe things, which I have named unſociable, are ſuch as have notions belonging to them, or have concluſions deducible from them, that are (for aught we can diſcern) either incongruous to our primitive ideas, or, when they are driven home, inconſiſtent with the manifeſt rules we are furniſhed with, to judge of true and falſe.

Eugen I preſume, *Sophronius*, that by ſorting things above reaſon into three kinds, you do not intend to deny, but that it is poſſible one object may in differing regards be referred to more than one of theſe ſorts.

Sophron. You apprehend me very right, *Eugenius*, and the truth of what you ſay may ſufficiently appear in that nobleſt of objects, God.

Timoth. WE owe ſo much to God, the moſt perfect of beings, not only for other bleſſings, but for thoſe very intellects, that enable us to contemplate him, that I ſhall be very glad to learn any thing, that may increaſe my wonder and veneration for an object, to whom I can never pay enough of either.

Sophron. You ſpeak like yourſelf, *Timotheus*, and I wiſh I were as able, as I ought to be willing, to ſatisfy your deſire: but ſince we are now diſcourſing like philoſophers, not divines, I ſhall proceed to ſpeak of that glorioueſt of objects, but as his nature or ſome of his attributes afford me inſtances to the purpoſe, for which I preſumed to mention him. When God therefore made the world out of nothing, or (if *Pyrocles* will not admit the creation) when he diſcerns the ſecreteſt thoughts and intentions of the mind, when he unites an immaterial ſpirit to a human body, and maintains, perhaps for very many years, that unparalleled union with all the wonderful conditions he has annexed to it; when, I ſay, he doth theſe and many other things, that I muſt not now ſtay to mention, he ſupplies us with inſtances of things that are inexplicable: for ſuch operations are not reducible to any of the ways of working known to us; ſince our own minds can but modify themfelves by divers manners of thinking; and as for things without us, all that one body can do to another by acting on it, is to communicate local motion to it, and thereby produce in it the natural confequences of fuch motion; in all which there is no action like any of thofe I juft now afcribed to God. And if we confider, that the prefcience of thofe future events, that we call contingent, being a perfection, is not to be denied to God, who is by all acknowledged the perfecteft of beings; and that yet the greateft wits, that have laboured to reconcile this infallible precognition with the liberty of man's will, have been reduced to maintain fomething or other, that thwarts fome acknowledged truth or dictate of reafon: if we duly confider this, I fay, it will afford us an inftance of truths, whofe confiftency and whofe fymmetry with the body of other truths our reafon cannot difcern, and which therefore ought to be referred to that fort of things above reafon, that I call unfociable. And now I come to the third fort of thefe things, which is that I formerly mentioned, firft under the name of incomprehenfible or fupra-intellectual; which title, whether or no it belongs to any other object, (which I will not now enquire) doth certainly belong to God, whofe nature, comprehending all perfections in their utmoft poffible degrees, is not like to be comprehenfible by our minds, who altogether want divers of thofe perfections, and have but moderate meafures, (not to call them fhadows) of the reft. We are indeed born with, or at leaft have a power and divers occafions to frame an idea of a being infinitely perfect, and by this idea we may fufficiently difcriminate the original of it, God, from all other objects whatfoever. But then, when we come to confider attentively and minutely, what is contained in the notion of omnipotence, omnifcience, eternity, and thofe other divine attributes, that are all united in that great confluence and abyfs of perfections, God; we may be fure to find, that our faculties are exceedingly furmounted by the vaftnefs and gloriousnefs of that unlimited and unparallel'd object; about which, as we can difcover that it exifts, and that it poffeffes all the perfection we can conceive, fo we may at the fame time difcern, that it muft have degrees of perfection, which, becaufe of the inferiority of our nature, we are not able to conceive.

AND yet this difcovery of God's incomprehenfiblenefs may be made without fubtle difquifitions, and without trains, of confequences, though not without due attention, by a direct view of the mind (if I may fo term it,) who finds herfelf upon trial as unable fully to meafure the divine perfections as the dimenfions of fpace, which we can conceive to be greater and greater, without ever being able to determine any extent, beyond whofe limits they cannot reach.

Pyroc. I SUSPECTED, *Sophronius*, by the tenour of your difcourfe, that the laft queftions thefe gentlemen afked you diverted you from faying fomewhat more than you did by way of application of your preceding difcourfe.

Sophr. I WAS then indeed about to make, as I now fhall, this ufe of what I had been faying; that I readily acknowledge, that it is an arrogance to talk of infinite or of priviledged things with the fame confidence, or to pretend to do it with the fame clearnefs, wherewith knowing men may fpeak of things unqueftionably within the compafs of our intellect; but that this need not hinder us from fpeaking rationally of priviledged things themfelves. For all the notions, that are allowable, are not of the fame fort or order; and if none were to be admitted, but thofe that enable us to comprehend the object, that is, which give us a clear and diftinct knowledge of all that it contains or that belongs to it, I muft confefs, that we have no good notion of priviledged things in particular: but then I muft add, that I fear we have few or none even of many things, that we think ourfelves very knowing in. And when we fpeak of things as being above reafon, though we have no clear, diftinct, and adequate notion of them, yet we may have

have a general confufed and inadequate notion of them, which may fuffice to make us difcriminate their refpective objects from all elfe, and from one another; as may be obferved in feveral ideas, that are negatively framed, fuch as thofe we have of invifible, incomprehenfible, and in others, which I formerly called inferred; becaufe they accompany the remote inferences, whereby one truth is concluded from another: as when geometricians infer from fome propofitions in *Euclid*, that any ftrait line may be divided farther and farther without ftop. For of this, and fome other propofitions about priviledged things, we are not quite deftitute of allowable notions; as may appear by fome of the admirably ingenious fpeculations of mathematicians about the affections of furd numbers, and about incommenfurable magnitudes; about fome of which we have no fuch clear and fymmetrical conceptions, as we have of many other things, that are of a nearer and more intelligible order. And on this occafion I fhall not fcruple to acknowledge, that partly by my own experience, and partly by the confeffions of others, and by their unfuccefsful attempts, I am induced to think, that God, who is a moft free agent, having been pleafed to make intelligent beings, may perhaps have made them of differing ranks or orders, whereof men may not be of the principal; and that whether there be fuch orders or no, he hath at leaft made us men of a limited nature (in general) and of a bounded capacity. Congruoufly to this I think alfo, that he hath furnifhed man either with certain innate ideas or models and principles, or with a faculty or power and difpofition eafily to frame them, as it meets with occafions (which readily occur) to excite them: but becaufe that (as I lately noted) God intended the mind of man but of a limited capacity, his underftanding is fo conftituted, that the inbred or eafily acquired ideas and primitive axioms, wherewith it is furnifhed, and by relation or analogy whereunto it judges of all other notions and propofitions, do not extend to all knowable objects whatfoever, but reach only to fuch, as have a fufficient affinity, or bear fome proportion to thofe primary ideas and rules of truth, which are fufficient, if duly improved, to help us to the attainment, though not of the perfect knowledge of truths of the higheft orders, yet to the competent knowledge of as much truth, as God thought fit to allow our minds in their prefent (and perchance lapfed) condition, or ftate of union with their mortal bodies.

Eugen. YOUR opinion, *Sophronius*, if I apprehend it aright, contains two very differing affertions; one, that it is allowable to contemplate, and even to difcourfe of things above reafon, fince we may have fome conceptions of them, though they be but very dim and imperfect: and the other, that we ought not to look upon, or fpeak of fuch objects, as things that we comprehend, or have even fuch a meafure of knowledge of, as we have of things that are not priviledged. For of thefe we are not to fpeak but with a peculiar warinefs, and modeft diffidence.

Sophr. You have expreffed my thoughts, *Eugenius*, fince I intend not to injoin filence, or diffuade curiofity, but yet forbid prefumption, in reference to priviledged things.

Timoth. AND truly, *Sophronius*, I fee no reafon to repine at the limits, which your late difcourfe hath, in imitation of the author of nature himfelf, affigned to human knowledge. For the number of priviledged things is altogether inconfiderable, in comparifon of the multitude of other things, to which our knowledge may be improved to reach, and which it far more concerns us to know well, than it doth to refolve puzzling queftions about things incomprehenfible; there being within the compafs of thofe truths enough to employ and reward our curiofity, without ftraining and tiring our reafon about objects that tranfcend it. And yet even about thefe fome difquifitions may be allowed us; for an object, that on the account of fome of its properties may be a priviledged one, may have divers other things belonging to it, that do not ſurpaſs our reaſon, and whoſe knowledge may therefore be attained, by the due employment of it.

Thus we uſefully ſtudy the nature of bodies, which make up the object of the excellent ſcience of natural philoſophy; though the true notion of body in general be a thing ſo difficult to frame, that the beſt of our modern philoſophers can by no means agree about it. Which I do not wonder at; becauſe if we purſue the notion of a body to the uttermoſt, it will lead us to the perplexing controverſy *de compoſitione continui*, and there you will not deny, but that the underſtanding will be left in the dark. Thus ſurveyors, carpenters, architects, and many others, know divers affections of the ſquare figure, that are of great uſe to them in their reſpective employments, though that property of the ſquare, that its ſide and diagonal are incommenſurable, be unknown to moſt of them; and if they were told of it, and would proſecute the ſpeculation, would involve them in exceeding great, and probably inſuperable, difficulties.

Sophr. To confirm what you have been telling us, *Timotheus*, I ſhall venture to add, that even about priviledged things, our inquiries, if modeſtly and diſcreetly managed, may not only be allowable, but ſometimes profitable. For even of ſuch ſubjects a ſtudious ſearch may bring us to know more than we did, though not ſo much as we would, nor enough to be acquieſced in. So that ſuch enquiries may probably teach us to know the objects better, and ourſelves better too; by giving us ſuch a ſenſible diſcovery of the inſufficiency of our underſtandings to comprehend all ſorts of things, as may be very uſeful, though not pleaſing, and may richly recompence us for the pains, that ended in ſo inſtructive a diſappointment. And let me add to the pertinent inſtances, that have been mentioned, the nobleſt that can be given; I mean the contemplation of God himſelf. For he hath ſo ordered all things, that it is ſcarce poſſible for us to be deſtitute of an idea of him, which will at leaſt repreſent him as an exiſtent being, and more perfect than any other being; and yet when we come with ſufficient application of mind to pry into the wonderful attributes of this moſt ſingular and adorable being, we are, as was lately obſerved, ſure to find ourſelves unable to comprehend ſo unbounded an object. Which yet ought not to diſcourage us from ſo noble a ſtudy, ſince we are allowed the great contentment and honour to make further and further diſcoveries of the excellenteſt of objects, by that very immenſity of his perfections, that makes it impoſſible for us to reach to the bounds of his excellency, or rather to diſcover, that it has any bounds at all.

But, gentlemen, I perceive I have been ſo tranſported by the mention of this vaſt and divine ſubject, in whoſe contemplation it is ſo eaſy and ſo pleaſant to loſe ones ſelf, that I have forgot the notice *Eugenius* gave me a pretty while ſince, that the time allotted for our preſent conference was then near expiring. And therefore I ſhall leave you to pick out of the excurſions, to which your interpoſitions tempted (not to ſay obliged) me, the applications, that I intended to make more methodically, of the diſtinctions I laid down. And I am the leſs troubled to be hindred from propoſing to you my thoughts about the way of diſtinguiſhing priviledged things from others, becauſe we have a domeſtick monitor, or a kind of an internal criterium, always at hand to help us. For I think it may well be ſaid, that the wiſe author of nature has endued the underſtanding with ſuch a quick, though internal, ſenſation, (if I may ſo call it,) that when due attention is not wanting, it can feelingly diſcern between other objects, and thoſe that are diſproportionate to its ability. As even in beaſts, the eye is ſo framed (according to the inſtitution of nature,) that if it be obverted to the bright noon-day-ſun, there needs no monitor, but the operation of the ſame ſun, to make it wink, (and perhaps water,) and thereby diſcover itſelf to be dazled and overpowered by the diſproportionate object.

Pyroc.

Pyroc. I CONFESS your diſcourſes, gentlemen, have made an unexpected impreſſion upon me; but whether that will amount to a conviction, will ſcarce appear till our next conference. Only thus much I ſhall tell you now, that it would much facilitate our agreement in opinion, if you did not contend for altogether ſo much; but would be pleaſed to leave it undetermined, whether man's intellectual faculty itſelf is uncapable, by the help of any degree of light, to diſcover and know thoſe things, which you call above reaſon: and would content yourſelves to ſay, that there are ſome things belonging to theſe ſubjects, which we muſt confeſs we have leſs clear and diſtinct notions of, than we have even of the difficulteſt of thoſe things, that are acknowledged not to ſurpaſs our reaſon: and that if we will take upon us, to determine poſitively and particularly about theſe tranſcendent things, we muſt employ ways of reaſoning congruous to their peculiar natures.

Sophr. I SHALL readily conſent not to expect your final reſolution, before our next meeting, having no cauſe to fear, that time will be unfriendly to her daughter truth.

Timoth. AND in the mean while *Pyrocles*, I am glad to find, by the laſt part of what you juſt now ſaid, that you ſeem to be no longer indiſpoſed to admit ſome things, that (at leaſt in our preſent ſtate) do ſome way or other ſurpaſs our reaſon. For I think, that inſtead of exalting that faculty, we injure and defraud it, if we do not freely allow it as much enjoyment of truth as we are able to procure it: and conſequently if geometry, or revelation, or experience, aſſure us of divers things, of which we can know but that they are, and what they do, not, what they are, and how they act, we muſt neither refuſe, nor neglect the ſtudy of ſuch truths, any more than we would refuſe to look into any other objects than thoſe that we can look through; and therefore to enrich the intellect as much as we are able, we muſt entertain, not only thoſe truths, that we can comprehend, but thoſe alſo, how ſublime ſoever, that we can have any certain, though but a very imperfect knowledge of, eſpecially ſince thoſe remote and abſtruſe ſubjects may be as much more noble as more dark than others, and thereby render an imperfect diſcovery of them more deſirable, than a far clearer one of inferior things.

ADVICES in judging of THINGS ſaid to tranſcend REASON.

The ſpeakers *Arnobius*, *Eugenius*, *Pyrocles*, and *Timotheus.*

Arnob. I WAS very glad, gentlemen, to learn this morning of *Sophronius* ſome things, whence it was eaſy to conclude, that by the diſcourſe you had with him laſt night, he has made it allowable for me to demand, and rational for you to grant, nay to proffer me a diſpenſation of the taſk you impoſed on me at our laſt meeting. For though he ſpoke with the modeſty that became him of your conference, and gave me but a haſty and imperfect account of what paſſed between you; yet I think I may preſume, that by his diſcourſe, *Pyrocles* himſelf was at leaſt inclined, and you two, gentlemen, fully perſwaded to admit, that there are things above reaſon; which was the main point, about which you expected, at our laſt congreſs, that I ſhould entertain you, at our then next, or now preſent meeting.

Eugen. I deny not, that *Sophronius*'s considerations were prevalent on *Timotheus* and me, and have, I hope, made a good impression on *Pyrocles* himself; but that ought not to hinder us from coming, as we now do, to claim your promise of entertaining us about things above reason. And if you will needs be dispensed with from repeating those considerations, that *Sophronius* has employed already, (though I doubt not, but by repeating them, you would both strengthen and advance them;) we will not be rigid exactors of our right: but yet we must not remit your task, though we are content to change it. For I question not but these gentlemen will consent with me, to discharge you of your promise of discoursing of the arguments, that may infer some things to be above reason, if you will please to afford us your thoughts, about the ways of avoiding to be imposed on by ourselves or others, when such sublime subjects are treated or discoursed of.

Arnob. THOUGH in the recital of your conference, *Sophronius* did but touch on several subjects, whereon it would be proper for me to insist, in the discourse you seem to expect from me; yet I am apt to fear, that he has so prevented me in what I should say, that he has left little or nothing for me to do, but to make repetitions of what you have heard already much better expressed: which will be an employment far enough from being grateful, either to you or me.

Eugen. YOUR modesty, sir, is not like to defeat our curiosity; and that you may not think yourself hardly used, or condemned to bear repetition, be pleased to take notice, both that what we desire as a favour, we might claim as a compensation, and that the things we expect from you now, are not arguments to make out, that there are things above reason, but that you would afford us some "rules and directions, "how to regulate the ratiocinations we make, and estimate those we meet with, about "such transcendent subjects."

Arnob. I hope, *Eugenius*, you do not in earnest think me so vain as to pretend to frame a logic about things above logic; or magisterially to deliver rules about things, that are as anomalous, as they are either remote or abstruse. Besides that all you have said does not exempt me from a fear, that by reason of *Sophronius*'s omitting divers points of his discourse, and my imperfect remembrance of those he transiently and summarily mentioned, he has anticipated much of what were otherwise proper for me to say. But yet because it is possible, that his thoughts and mine may have led us to have made some reflections, that are not at all the same; and that even when others happen to be coincident, it may be not altogether useless, that I should endeavour to enlarge some things, that he has but hinted, and illustrate or vindicate some others, that will not be prejudiced by being cleared, or confirmed; and above all this, because I would shew you, that I am willing to comply with you somewhat to the hazard of my discretion, I shall not refuse to offer you some, not rules, but advices; provided you freely interrupt me, when I begin to trouble you with the repetition of any thing, that you have, though I have not, heard before; and provided too, that you look not on these advices so much as directions to find the truth, in such abstruse matters, as cautions that may chance to assist you to avoid some errors and mistakes.

Eugen. WE are not so scrupulous, but that we shall upon your own terms gladly receive your thoughts, whatever names you please to give them.

Arnob. I shall then, without further preamble, comply with your commands, and propose as my

First ADVICE.

That about privileged subjects themselves, we do not admit any (affirmative) assertion, without such proofs, to evince it, as are sufficient in their kind.

I hope gentlemen, that *Sophronius* has so far declared to you, what is to be meant by priviledged things, that though it be a new term, yet I need not solicitously explain it; and may think it sufficient to intimate in few words, that they are things of a very heteroclite and abstruse nature, and have belonging to them such peculiar affections and attributes, as require, that in judging and reasoning of them, we should employ notions and rules congruous to their particular condition; some of them superadded to, and others perhaps differing from those, that men generally and safely enough make use of about common and familiar things, that are of a nature less impervious to our understandings. And if the shortness of this summary description should leave it less clear than I hope you find it, I foresee there will divers occasions of illustrating it, by instances and other ways, occur in the sequel of our discourse: in order to which I shall, after this short and necessary digression, return to the lately given first advice; and tell you, that it is grounded upon this consideration, that it is not reasonable to give assent to any thing as a truth, but upon a sufficient reason of that assent. And though we may well grant in the general, that a thing, which surpasses our reason, may have belonging to it some affection, that is also above reason; yet we are not in particular to believe, that this or that affection doth belong to it, without particular and competent proof. For since about a priviledged thing, as well as about any other, propositions may be framed, and often are so, that are contrary to one another; to assent to both were to be sure to believe one falsity, if not two. And if we will assent but to one, we must either judge at adventures, or allow ourselves to examine the mediums of probation, employed on both sides, and thereupon judge, why one of the propositions is to be assented to, and the other rejected.

Pyrocl. I AM glad, *Arnobius*, that you allow yourself and us this manly freedom, without which our understandings were liable to be imposed on in matters of the highest concernment: for there scarce ever did, or I fear ever will, want some men, who either out of ignorance and passive delusion, or out of self-confidence, or out of design, take upon them, with great boldness, to affirm what they please about priviledged subjects; and when they are opposed in their extravagancies by ratiocinations they cannot answer, they urge, that these things being above reason, are not to be judged of by it. But of such men as these I usually demand, whether their own assent to the things they would have us believe, be grounded upon some rational argument, or not? If they say, it is not, they are fools to believe it themselves; and I should add to the number of fools, if after this acknowledgment I should believe them: but if they say they do, I desire them to produce their argument; for since it is framed by a human understanding, the force of it may be also comprehended and judged of by a human understanding: and it is to no purpose to say, that the subject surpasses human reason; for if it do so indeed, it will surpass theirs as well as mine, and so leave us upon even terms. And let the thing assented to be what it will, the assent itself ought to be founded upon a sufficient reason, and consequently upon one, that is intelligible to the human intellect, that is wrought on by it.

Eugen. I WILLINGLY allow, that there is a great difference between the being able and obliged to know the nature or cause of a thing, and the being able to give an intelligible account of the motives, that induce our assent to it; and without such motives, the assent may by chance be given to what is a truth, but that will not hinder it from being an irrational assent.

Timoth. I WAS not ill pleaſed, *Arnobius*, with the caution you employed in the cloſe of your advice, where, by ſaying, that the poſitive proofs you require to evince an aſſertion about a priviledged thing, muſt be ſufficient in their kind, you plainly intimate, that you do not exact rigid demonſtrations of ſuch aſſertions. And indeed it were not reaſonable you ſhould; for ſince it is manifeſt, that there are many truths, ſuch as hiſtorical and political ones, that, by the nature of the things, are not capable of mathematical or metaphyſical demonſtrations, and yet, being really truths, have a juſt title to our aſſent; it muſt be acknowledged, that a rational aſſent may be founded upon proofs, that reach not to rigid demonſtrations, it being ſufficient, that they are ſtrong enough to deſerve a wiſe man's acquieſcence in them. And therefore if any things can be made out to be revealed by God, concerning his own nature, or actions, or decrees, we ought firmly to believe them, becauſe that, of ſome of thoſe things, as his preſcience, mercy, *&c.* we can have no better proofs; and of others, as what he did before our world was made, and what he will do with us after we are dead, we can have no other conſiderable proofs at all. And the objection made by *Pyrocles* againſt the aſſenting to audacious propoſitions framed by impoſing men, will not reach our caſe: for there is no reaſon to think, that becauſe an object ſurpaſſes an human underſtanding, it muſt therefore ſurpaſs the divine intellect itſelf. And even in things, that are tranſacted in the mind of man himſelf, I may learn from another, that is not my ſuperior, what I can by no means attain to know, unleſs he be pleaſed to diſcover it to me; as that he was at ſuch a time thinking of the creation of the world, or reſolving how to diſpoſe of his ſon, and what recompence he deſigns to give a ſervant, that he has not yet entertained.

Pyrocl. ABOUT things of ſuch a kind as you now mention, *Timotheus*, I ſhall not diſſent from you; becauſe theſe are things, that though not diſcoverable by our reaſon till we be informed of them, are yet clearly knowable by our reaſon, when we are informed of them. But that there ſhould be things, which though perſpicuouſly propoſed, ſhould not be comprehenſible by our underſtanding, is ſuch an affront to that noble faculty, that I confeſs it has much indiſpoſed me to grant (what I am yet unwilling peremptorily to deny,) that there are, as *Sophronius* would have us think, not only ſome priviledged things, but more than one kind of them; which if we do admit, it will place ſuch narrow limits to our underſtandings, that we muſt deſpair of the deſireableſt knowledge of all, namely that, which is converſant about the nobleſt and ſublimeſt objects.

Eugen. LEAVING to *Sophronius* the management of a point he has ſtudied, and which I have not now time ſolemnly to argue; I ſhall only tell you in general, that I ſee no neceſſity, that intelligibility to a human underſtanding ſhould be neceſſary to the truth or exiſtence of a thing, any more than that viſibility to a human eye ſhould be neceſſary to the exiſtence of an atom, or of a corpuſcle of air, or of effluviums of a loadſtone, or the fragment exhalations of ambergreece, and muſk from a perfumed glove. I might here obſerve, that even by the ſame ſenſe, ſome creatures may diſcern things, that may not be perceptible to others: as no attention or application of the organ (or the noſe) will enable a man to perceive the effluvia expiring from the ſtale footſteps of a hunted and unſeen hare or deer, though hounds, and eſpecially blood-hounds, will have a vivid perception of ſuch odours, and by their help trace and purſue the flying and unſeen beaſt. This, I ſay, may be obſerved in favour of my preſent argument; but it will perhaps be a more proper illuſtration to repreſent, that the natural incapacity of a child's intellect, to underſtand the abſtruſe affections of parabolas, hyperbolas, and the incommenſurable lines of a ſquare, hinders not thoſe figures from being contained in *rerum naturâ*, or their affections from being true and demonſtrable. And

though

though we do admit ſome priviledged things in the ſenſe above declared, yet, (to ſay ſomewhat to obviate *Pyrocles*'s fear) there is no neceſſity, that we ſhould be interdicted all knowledge of thoſe ſublime objects, in which there are many things, whereof, or of their conſequences, we muſt confeſs ourſelves ignorant. Thus elder geometricians knew very well what a rectangular triangle was, when they conceived it to be a figure conſiſting of three ſtrait lines, two of which comprize a right angle; though probably, for a great while, they did not know ſo much as all its chief properties or affections: ſince, for aught appears, before *Pythagoras*, (who offered a heccatomb to the muſes in gratitude for their diſcovery) it was not known, that the ſquare of the hypothenuſe is equal to the ſquares of both the other ſides; and much more likely it is, that they were not able to ſolve thoſe difficulties (that continue to perplex even our age) which attend that endleſs diviſibility of lines, that is inferrible from that equality of the two ſquares to the ſingle ſquare.

AND beſidss the inſcrutable perfections of God, ſome of his works are ſuch, that, notwithſtanding the compleat knowledge of them ſurpaſſes our forces, yet there remain ſo many things, as well worthy to be known, as poſſible to be attained by us, that they will allow exerciſe enough to the wits of all the philoſophers in the world. And beſides that, as I have been ſaying, even about theſe priviledged ſubjects themſelves, divers conſiderable things may be diſcovered, if they were altogether impenetrable by our underſtandings, yet their number is ſo ſmall, that they would leave a large ſcope for human knowledge to diffuſe and improve itſelf. For it is not every thing, that is hard to be underſtood, or contrary to the common rules of probability, that has a right to paſs for a priviledged thing; for ſo the paradoxes about ſurd quantities, of iſoperimetal figures, duplicate and triplicate proportion, and divers, other ſurpriſing doctrines, that are capable of mathematical demonſtrations, would be priviledged things. Nor are all thoſe worthy of this title, that are by many propoſed and embraced as philoſophical myſteries; for, ſuch are the Peripatetics ſubſtantial forms, which really are not priviledged things, but ſcholaſtic chimeras. But though I ſhall not preſume poſitively to ſet down the diſcriminating bounds and ſigns of priviledged things, yet moſt, if not all, of them being ſuch, as are either primary in their kind, as God himſelf, and the things, whoſe nature flows immediately from him; or elſe things, that if thoroughly inſpected, do neceſſarily involve the conſideration of ſome kind of *infinitum*; or elſe are ſuch, that, though in ſome main queſtions about them one ſide muſt be taken, both ſides are encumbred with abſurdities: thoſe, I ſay, being all (or ſome of them) the uſual marks, that belong to priviledged things, you will eaſily grant, that their number is not near ſo great as their abſtruſeneſs; and that therefore *Pyrocles* and his philoſophical friends need not fear to want employment for their curioſity. And for farther anſwer to his objection, I ſhall add, that we muſt regulate our belief by our perceptions, not our wiſhes; and muſt not conclude, that becauſe it were deſirable for us, that all things were penetrable to our human underſtandings, there is really nothing that is not ſo: and we can no more conclude, that we are as knowing as angels, becauſe we wiſh we were ſo, than that we are as immortal as they, becauſe we would never die. But as for thoſe few things, that have belonging to them properties ſo extraordinary, as to make it probable, even at the firſt ſight, that their nature muſt be very abſtruſe, and difficult to be fully diſcovered by us; I hope *Pyrocles* will allow, that things of ſo heteroclite a nature may challenge an exemption from ſome of the rules employed about common things; and that really ſuch rules as I mean, and ſome alſo of the vulgar notions, cannot always be ſafely extended to ſuch ſubjects, I forbear to ſhew in this place; only becauſe I would not too long at once interrupt *Arnobius*, and I expect to have a good opportunity to ſpeak again of this ſubject, before our conference be ended.

Timoth. You may then, I presume, *Arnobius*, as soon as you please, favour us with your second advice.

Arnob. I shall readily obey you, *Timotheus*, by proposing it thus:

The second ADVICE, or RULE.

That we be not hasty to frame negatives about priviledged things, or to reject propositions or explications concerning them; at least, as if they were absurd or impossible.

IT is easy to observe in the speculation of natural things themselves, how unsafe it is not only to affirm, but in divers cases also reject opinions, before men have any thing near a competent historical information of what belongs to the subject they take upon them peremptorily to judge of. And therefore it must in reason be thought much more unwary, to be forward to resolve upon negative propositions, about things, which we ourselves acknowledge to be above the reach of human reason, which since they are, it will become us at least to forbear a rude insulting way of rejecting the opinion of learned men, that dissent from us about such things; since the sublimity of the subject should make mistakes about them the more easy to be pardoned, because they are difficult to be avoided; and our own sharing in the disability of penetrating such abstruse things should keep us from being over-confident, that we also may not be mistaken, and incline us to tolerate other mens opinions about matters, wherein we ourselves have but opinion, not science.

Pyrocl. BUT have not you formerly advised us not to suffer ourselves to be imposed upon by proofless assertions, even about priviledged things?

Arnob. I did so, and do so still: but there is a great deal of difference between believing a proofless affirmation about things, which the affirmer does not know to be true, and framing negative conclusions against opinions, which, for aught we yet clearly know, may be true; and therefore my present advice is very consistent with my former: for here I counsel only, either a suspension of judgment when there appears no proof on either side sufficient to sway the intellect; or such a wary and unprejudiced assent to opinions, that are but faintly probable, that the mind may be ready to receive, without either obstinacy or surprize, any better argument, that shall conclude the contrary of the opinion we favoured before.

Eugen. BUT methinks it is hard to avoid the framing of conjectures, even about those sublime subjects, concerning which we can frame but conjectures, and those often very slight ones.

Arnob I CONFESS an absolute suspension of judgment is a very uneasy thing, nor do I strictly require you should entertain no conjectures; but only that we should consider, that we may be easily mistaken in them, and by further information see cause to lay them down, and perhaps exchange them for contrary ones. My thoughts of this matter may be perchance somewhat illustrated, by supposing, that we four were walking in a highway, and discovered, as far off as our eyes could reach, some erected and moving body of human stature; though we should, by its shape and walking, safely enough conclude, that it were no other animal than a man, yet what manner of man he were, as old, or young, handsome, or ugly, we should not be able to discern, and consequently, could have no sufficient ground to determine. And as if I should affirm him to be a young man or handsome, you may justly censure me of rashness; so if, because I cannot prove my conjecture, you should resolutely deny, that he is a young man or handsome, I should think you guilty, though not of an equal, yet of a censurable unwariness, because, for aught you know to the contrary, he may be what I guessed him to be. And though we are naturally so uneasy under fluctuation of mind, that

that for my part I confess (and it may be you may be subject to the same infirmity) I should scarce forbear resembling in my thoughts the man we speak of to some body or other that I knew; yet I should justly think that conjecture to be very fallible, and both expect, that when I should come to have a nearer and clearer view of him, I might see cause to dismiss my first idea for that, which this new and better prospect would afford me, though it were quite differing from that I had formerly entertained, and should represent him, that my forward thought perhaps resembles to a young man of my acquaintance with black curled hair, and a ruddy complexion, to be pale and wrinkled, with grey hair curled like a pound of candles. The application, I suppose, I may spare.

But gentlemen, I would not be understood in the preceding discourse, as if I were against all framing of negative propositions about priviledged things; my design being but to dissuade from hasty ones: for sometimes it is much more easy and safe to deny things, than to affirm them to belong to a subject that surpasses our reason. And the observation may be of use, especially in two cases; one, when the negative we assert is grounded not upon axioms taken from the usual course of nature, or upon propositions dubious, or remote from the first principles of knowledge, but upon either catholic and metaphysical axioms, or else upon truths manifestly flowing from some clear, though inadequate, notion we have of the nature of the things we treat of. The other case is, when we have a clear and sufficient proof by revelation, or otherwise, of the positive attributes of the things we contemplate; for then we may safely deny of that subject any other thing, that is really inconsistent with that positive attribute. Upon which account it is, that though we do not fully comprehend what God is, yet knowing by the clear light of nature (and if we be Christians) believing it upon the account of revelation, that he is a being intelligent and infinitely perfect, we may safely deny against *Epicurus*, *Vorstius*, and Mr. *Hobbes*, that he is a corporeal substance, as also that he is mortal, or corruptible.

Pyrocl. I shall not trouble you, *Arnobius*, to enlarge upon your last advice, but willingly receive the favour of your next.

Arnob. Which shall be this:

The third ADVICE, or RULE.

That a matter of fact, or other truth, about priviledged things, being proved by arguments competent in their kind, we ought not to deny it meerly because we cannot explain, or perhaps so much as conceive, the modus of it.

It is no very difficult task to justify this advice; but I may do it the better, if you give me leave to frame and premise a distinction, for want of which I have observed a want of clearness in several discourses, where the term modus has been employed: for sometimes we would deny so much as a possibility, that one thing can belong to, or be truly said of another; as when we say, we understand not how one creature can create another; or how there can be a line, that is neither straight nor crooked; or a finite (whole) number, that is neither even nor odd. But most commonly we mean, by our not understanding the modus of a thing, that we do not clearly and distinctly conceive, after what manner the property or other attribute of a subject belongs to it, or performs its operations. The first kind of modus may, for distinction sake, be called a possible modus; and the other, an actual modus. Now in both the foregoing acceptions of the term modus, we may find instances fit for our present purpose. For we cannot imagine, how a short line, or other finite quantity can be endlesly divisible, or (on the contrary) how infinite parts should make but a finite total: and yet geometry constrains us to admit, that it is so. But though there be but few instances of this kind, yet of the other sort of our nescience of the modus of things, there may be found more instances than we could wish there were; for even in natural and corporeal things, the eager disputes of the acutest philosophers, and the ingenious confessions of the most judicious and moderate, sufficiently manifest, that as yet we know not the manner of operating, whereby several bodies perform what we well know they bring to pass. And not to enter into those nice and tedious disputes of the cause of the cohesion of the parts of matter in the smallest, most principal, and most primary bodies, perhaps without going out of ourselves, the way, whereby the rational soul can exercise any power over the human body, and the way, whereby the understanding and the will act upon one another, have not yet been intelligibly explained by any. And the like I may say of the phænomena of the memory, especially in those, in whom that faculty is eminent. For it is a thing much more fit to be admired, than easy to be conceived, how in so narrow a compass, as part of a human brain, there should be so many thousand distinct cells, or impressions, as are requisite to harbour the characters or signatures of many languages, each of them consisting of many thousand differing words, besides the images or models of so many thousand faces, schemes, buildings, and other sensible objects, and the ideas of so many thousand notions and thoughts, and the distinct footsteps of almost innumerable multitudes of other things: and how all these shall, in so narrow a compass, have such deep and lasting impressions made for them, and be oftentimes lodged so exactly in the order, wherein they were at first committed to the memory, (and that perhaps many years before) that upon a sudden command of the will, or a slight casual hint, a whole set of words, things, and circumstances, will in a trice, as it were, start up and present themselves even in the very series, order, and manner, that so long before belonged to them. And I doubt not, but that besides those abstruse things about the modus, of which the more candid philosophers have confessed their ignorance, that there would many others have been taken notice of, if we did but as seriously and impartially enquire into the nature of all the things we are pleased to think we know. And when I reflect on the yet depending disputes, between philosophers and mathematicians, about the nature of place and local motion, which are things so obvious and familiar to us, I should, though I had no other inducements, be inclined to think, that we should find difficulties enough in many other subjects, wherein we do not now take notice of any, if we particularly studied their nature; and that our acquiescence in what we have learned about many things proceeds not from our knowledge of their nature, but from our having exercised less curiosity and attention in considering it.

AND if in things corporeal, that are the familiar objects of our senses, we are often reduced to confess our ignorance of the modes of their existing or operating, I hope it will not be denied, that to a being wholly unapproachable by our senses, natural theology may be allowed to ascribe some things, whose modus is not attainable by our understanding: as the divine prescience of future contingents, which as it were impious to deny, as to the truth of the thing, so I fear it is impossible to explicate, as to the modus of it.

Eugen. IF it were at this time proper for me to meddle with things of that kind, I should not much scruple to say in favour of the Christian religion, that divers tenets, granted both by Christians, Jews, and Heathens, as parts of natural theology, to me seem as difficult to be conceived, as divers of those mysteries, that for their unintelligibleness are fiercely opposed in revealed theology. I will not take upon me to judge of others; but, for my part, I confess, I do not much better understand, how an intellect, and a will, and affections, are distinctly existent in God, in such sort as they are

are wont to be attributed to him, than how in him there can be a Trinity, stated, not as some schoolmen explicate, or rather darken it, but as the gospel delivers it. I can as little explain by any thing in nature, how God, who is an immaterial substance, can move matter, as how he can create it: nor would it at all satisfy me to tell me, that a rational soul moves a human body; for I do not allow, that it gives any motion to the body, but only guides that, which other agents have put the parts of it into. And though it did produce motion in the body, my scruple would yet remain; for the Cartesians themselves confess, that the power the soul has of so much as determining the motion of the body belongs to it, not upon any physical account, but by the particular appointment and immediate power of God, who would have that power one of the conditions or properties of the union of the soul and body. So that to me, who desire to have it explained, how an immaterial substance can move matter, and consequently, how God can do it, it will be no satisfaction to say, that the rational soul can move the body it is joined to, since that power is referred merely to God's appointment: and the question is, how God himself can be conceived to move matter.

Arnob. I know not, whether upon the same grounds, which I do not disallow, I may not add, that whereas by many it is looked upon as an inconceivable thing, that God should see mens thoughts, to me it appears as little intelligible, how he can know their outward actions: for since we have no way of discerning the particular motions of mens bodies, but by some of our senses, especially our sight; and since those sensations themselves necessarily require organs duly constituted, that is, made up of divers parts, framed and joined after such a determinate manner, I see not how we can explain the perception of visible objects without an eye, or so much as any corporeal organ, or substance; especially since it is, and that very justly, asserted, that the deity is not united to any portion of matter, as the human soul. And to these instances others to the same purpose might be added, but that I think it fitter to mind you, that of those already mentioned amongst us there are some, that I presume you will judge referable to that, which I lately called a possible modus; since, it seems, *toto genere*, as they speak, inexplicable, how the attribute exists in the subject, and after what manner the cause can produce the effect ascribed to it.

Timoth. I know you too well, gentlemen, to suspect, you mean by this, to deny to God either the power of moving matter, or that of perceiving all its motions.

Arnob. You may well take that for granted; and you may remember, that to prevent mistakes, I was careful, in proposing my advice, to except those things, for which there is some positive proof competent in its kind.

Pyrocl. One may then, without surprising you, ask what kind of proofs those may be?

Arnob. A full answer to that question would take up too much of that little time, that is allowed us, before it grow dark, to go thorough the advices, that yet remain unspoken of. But yet to comply with you, as far as my haste will permit, I shall name two or three kinds of positive proofs, that may be employed on such occasions as we speak of. And first, if there be an effect, that we discern must proceed from such a cause, or agent, we may conclude, that such a cause there is, though we do not particularly conceive, how, or by what operation it is able to produce the acknowledged effect. Thus, though a man, otherwise of a good judgment, being wholly a stranger to the mathematics, cannot conceive how a skilful astronomer can many years beforehand foretell eclipses to a day and hour, and perhaps to a few minutes; yet when the success does, as it often happens, verify such predictions, he will be satisfied, that the maker of them had the skill to foreknow the things foretold in them. And so the generality of learned men among us, who are not so much acquainted with that part of navigation, which some moderns have by a Greek name called *Limen-Euretica*, or the art of steering to harbours, cannot well conceive, how a ship, that is, for instance, in the vast Atlantick ocean above a thousand miles from any shore, should be so directed, as to arrive just at a little harbour not cannon shot over, which perhaps neither the pilot, nor any other in the ship ever saw. And yet as little as we can distinctly conceive, how such an art of finding ports can be framed, we scruple not to allow there is such an one, because navigators to the *East* and *West-Indies* could not without the guidance of such an art find the remotest ports they are bound for.

A SECOND sort there is of positive proofs, consisting of those consequences, that are clearly and legitimately inferred from any manifest acknowledged, or already demonstrated truth. To this sort belong divers mathematical propositions and corollaries, which though being nakedly proposed they seem incredible to the generality of learned men, and sometimes to mathematicians themselves, are yet fully assented to, because they clearly follow from either manifested or demonstrated truths. Thus many cannot conceive, how it is possible there may be a million, for instance, of circles, (or as many more as you please) whose circumferences shall each of them come nearer and nearer to one another, and to a strait line assigned, and yet none of them either touch, much less cut, either any other circle, or that line, but in one and the same point. And yet this is one of the odd propositions, that geometers have rightly deduced as corollaries from the sixteenth of *Euclid*'s third element. And though we cannot clearly conceive, how two lines, that at their remotest ends are but little distant from each other, should perpetually incline towards each other without ever concurring; yet geometricians, that is, the rigidest reasoners that we know of, have been compelled to admit this in the *Linea Conchoides* of *Nicomedes*, to name no more. But though, (not to touch the same strings too often) I thought fit to mention these instances, yet whether you judge them sufficient or no, you will allow that, which may be taken from the endless divisibility of a line: for though, if I mis-remember not, *Sophronius* told me, he took notice to you, how unable we are to have satisfactory apprehension, how a short line as well as a long can be divided into more and more parts, without any stop; yet geometricians generally admit this, because it may be clearly deduced from some geometrical truths, and particularly from the incommensurableness of the side and diagonal of a square. And if you will allow me to have once more recourse to divine prescience, I may add another acknowledged instance, by representing, that philosophers have admitted that, because they judged it clearly to follow from the infinite perfections of God; though, how he can foresee contingency, the most judicious and modest of them did not pretend their reason was able to conceive.

Timoth. To these two kinds of positive proofs mentioned by *Arnobius*, I doubt not but he will give me leave to add divine revelations, if competently attested ones can be produced; and therefore I will not, by going about to evince this, spend any of the time he reserves for the remaining rules, to which he may, for me, advance as soon as he thinks fit.

Arnob. I ACCEPT the liberty you offer me, *Timotheus*, to proceed to my next advice; which is this.

The fourth ADVICE, or RULE.

That when we treat of priviledged subjects, we are not bound always to think every thing false, that seems to thwart some received dictate of reason.

As great a paradox as this may at first blush, appear, yet it will need little more to make it out, than the application of some things, already delivered on occasion of the two foregoing advices, of which this is indeed little more than a corollary. For it being evident,

evident, that as a great part of the dictates of reaſon are negative, ſo negative propoſitions do uſually ſpring from the repugnancy we judge that ſome things have to ſome poſitive dictate of reaſon; if thoſe poſitive dictates contain but gradual and limited truths, (to borrow *Sophronius*'s terms) and come to be unduly extended to priviledged ſubjects, it may very poſſibly happen, that a thing may be really true, that yet muſt appear falſe, if it be judged of by its congruity to one of thoſe limited and but reſpective dictates of reaſon. It is alſo clear, that not only in philoſophy, but natural (as well as revealed) theology, the uſual ground, on which we reject many things, is, that we judge them unintelligible. And I cenſure not the practice in general, but I think it may eaſily miſlead us, when it is extended to things, that we may diſcern to tranſcend our reaſon, as, for aught yet appears, ſome of the moduſes, even of things corporeal, are found to do. And we think we have made complete enumerations of the ſeveral ways of inexiſtence of an attribute in a ſubject, or of the operation of one thing upon another, when indeed we have overlooked one or other, and perhaps that, which we have thus pretermitted, may be the true one; though it may be alſo, that no attention and diligence of ours could, in ſome caſes, have ſerved our turn, the modus inquired after being not conceivable to us, though it may be to a higher than a human intellect.

Pyroc. THE ſchool-philoſophers for many ages, in the catalogues they made of the ways of a body's working upon another at a diſtance, did not think of the true ways, by which odours and ſounds are communicated to us; and therefore had recourſe to certain unintelligible things, which they were pleaſed to call *ſpecies intentionales.* Whereas thoſe modern naturaliſts, that philoſophize freely, acknowledge, that odours are communicated by effluviums, exhaling from the odorous body, and fitted to affect our noſtrils; and ſounds are tranſmitted to the ear by the undulating motion, which the air is put into by the impulſe of the vibrating, or otherwiſe agitated parts of the ſonorous body.

Timoth. METHINKS we need not go out of ourſelves to find inſtances of both the parts of what *Arnobius* was laſt ſaying, if we admit, as I queſtion not but we rationally may, this tenet of the generality of philoſophers, both ancient and modern, that the reaſonable ſoul is an immaterial ſubſtance: for then, whereas men think they have ſufficiently enumerated the ways of determining the motion of a body, by ſaying, that the determination muſt be made, either in the line, wherein the impellent, that put it into motion, made it move, or in the line, wherein it was determined to move by the ſituation of the reſiſting body, that it met with in its way; the motions of the animal ſpirits, if not alſo ſome other internal parts of the body, may, the body being duly diſpoſed, be determined by the human will; which is a way quite differing from the other. And how this attribute, I mean the power of determining the motion of a body, without any power to impart motion to that body, ſhould belong to an immaterial creature, which has no corporeal parts to reſiſt the free paſſage of a body, and thereby change the line of its motion, is not yet, nor perhaps ever will be in this life, clearly conceived by us men; though there is no doubt, but that he, who endowed the ſoul with this attribute or power, perfectly underſtands, both how it exiſts in the ſoul, and how the ſoul, by exerting it, operates on the body.

Pyroc. BUT can any thing ſeem more unreaſonable, than to embrace opinions, that contradict the rules of reaſon? which practice, if it be once allowed, why ſhould we trouble ourſelves to inveſtigate what is congruous or incongruous to reaſon, ſince the making a diſcovery, that an opinion is repugnant to it, will not aſſure us of that opinion's being falſe?

Arnob. A PERSON leſs knowing and equitable than *Pyrocles* would have ſpared this double objection, if he had remembered what hath been formerly ſaid, applicable to our

our present purpose, and what kind of things there are, that we are discoursing of. But to remind him a little of them, I shall desire him to consider with me, that I no way disallow the rejecting of opinions, that are found contrary to those rules of reason, at the framing of which the things opined about were duly taken into consideration: but in cases not thought on when such rules were devised, we are not always bound to submit to be judged by them; and to maintain an opinion unconformable to such a rule, may be not to oppose a genuine and absolute dictate of reason, but to rectify one, that is erroneously thought so, by shewing, that the rule is expressed in more catholick and indefinite terms than it ought to have been. And of two opinions, you will not deny, that that is the most rational, that is most agreeable to those rules of reason, that are framed upon the fullest information.

Eugen. It is not difficult to gather from what you have said, *Arnobius*, that in the rule you proposed to us, very few of the cases, that occur in ordinary discourse, or even in that of philosophers, will be at all concerned. And in these few cases wherein you intend the rule should take place, you are careful to obviate inconveniences by a double caution. The first, that you suppose, that the opinion, that claims an exemption from the common rules, is not an arbitrary or precarious tenet, but sufficiently made out by proper arguments. And the second, by declaring, that it is not to contradict right reason, but bad reasoners, to give limitation to rules, that have been too hastily framed and conceived in too general terms, by men, who either were not competently informed of the variety of particulars, when they took upon them to make analyses and enumerations; or else presumed to infer, that a thing was not, because they did not understand the modus of its existence or operation.

Arnob. You take my sense right, *Eugenius*, and I have often thought, that the causes of the great clamor, that is made against some men, for not obsequiously submitting to what some others call the rules of reason, are, that men do not sufficiently understand the nature of things, and themselves, but entertain too narrow conceptions of the former, and too high an opinion of the latter.

Pyroc. THE dictates of reason being the surest, if not the only safe rules, that nature have given us to frame our discourses and ratiocinations by, I confess I am, though not fully resolved, yet very unwilling to allow any conclusion, that is not conformable to them; or to admit, that any thing should be so highly priviledged as to be exempted from the jurisdiction of reason, whose genuine declarations they are.

Eugen. THIS objection, *Pyrocles*, seems to me to be grounded rather upon an ambiguity of terms, than the true nature of things. For reason is oftentimes taken for a set of notions and propositions, employed and acquiesced in by this, or that, sort of reasoners, that are wont to have names given them, from this or that particular discipline, as astronomy, chymistry, opticks, &c. of whose received doctrines they are supposed to be entirely maintainers. But it is also with, at least, as much propriety used to signify the rational faculty itself, furnished with the light, that accompanies it, when it is rightly disposed and informed. In the first of these two senses it seems but reasonable to allow, that some things ought to have the privilege to be exempted from being judged by some of the same rules, that are employed to judge of other things by; for some of these rules were framed upon a slight consideration of common and familiar things, either by the vulgar, or by men, that for want of skill, or application of mind, did not critically consider the distinct natures of things, and yet presumed to settle rules, that other mens inadvertence, or laziness, has made them receive for certain dictates of reason: whereas other natures should have been then considered, as well as those; and by reason of their not having been so, the rules, I speak of, are not always proper and safe when they are applied to these overlooked natures. Thus ſucceſſive beings, as time and local motion, do, in ſome caſes, require to be eſtimated by other meaſures than ſubſtances, whether material, or incorporeal. And ſo alſo the more nice metaphyſicians, eſpecially among the moderns, have thought themſelves obliged to diſcourſe of moduſes, relations, privations, extrinſecal denominations, *&c.* in a very differing way from that which belongs to bodies and ſpirits; though the unſkilful (even among otherwiſe learned men) have been wont, and ſtill are apt, to confound all theſe ſubjects, by applying to them indiſcriminately the ſame rules, or, as they think them, dictates of reaſon.

BUT, beſides what may be ſaid of theſe long unregarded, or undiſtinguiſhed natures, there are other entities, that are more generally and familiarly taken notice of, wherein I may think one may find inſtances more applicable to my preſent purpoſe. For I obſerve, that though all other actual beings are compounded (to ſpeak in the language of the ſchools) of eſſence and exiſtence; yet, according to the notion of metaphyſicians, as well as divines, it muſt be acknowledged, that the ſimplicity of the Divine Nature is ſuch, as to exclude from God even this kind of compoſition. And indeed the notion we have of a being infinitely perfect, imports, that, though in no other being, yet in this thoſe two are inſeparable; for actual exiſtence, being a perfection, muſt needs belong to the nature of a being infinitely perfect. The generality of philoſophers, after *Ariſtotle*, conceive place to be the immoveable and immediately contiguous concave ſurface of the ambient body, ſo that it is a kind of veſſel, that every way contains the body lodged in it; but with this difference, that a veſſel is a kind of moveable place, as when a bottle of wine is carried from the cellar to the table; but place is an immoveable veſſel, or a veſſel conſidered as immoveable. Now, ſuppoſing with *Ariſtotle*, and the generality of philoſophers, the plenitude of the world, it may be truly ſaid, that all plants, animals, minerals, ſtars, and other bodies, are each of them in ſuch an Ariſtotelian place as has been deſcribed; whence it has been uſually ſaid by philoſophers, that what is in no place (I hope they meant it only of bodies) is not at all: yet it appears not, how the outermoſt heaven, whether that be the firmament, or no, I need not here inquire, can be properly ſaid to be in a place, ſince theſe philoſophers aſſerting the world to be finite, muſt grant there is no ambient body without it to contain it. And I ſhall add, on this occaſion, that if the outermoſt heaven ſhould be impelled, by the irreſiſtible power of God, in a ſtraight line, this way, or that way, there ſhould enſue a motion without change of place, for the outermoſt heaven was in none before, and does not, by its progreſſion, come to be contained by a new ambient body. And, in this caſe, even according to thoſe modern favourers of *Ariſtotle*, that approve *Des Cartes*'s definition of local motion, (which indeed is far more intelligible than *Ariſtotle*'s) the world may be ſaid to move without changing of place; for it does not paſs from the neighbourhood of ſome bodies to that of others, ſince compriſing all bodies, and yet being bounded, there is no body for it to leave behind, nor any beyond it for it to approach to. And though the Carteſians in their hypotheſis of the indefinitneſs of the world, do partly avoid the force of what I have been ſaying; yet, beſides what may be rationally urged to ſhew, that if the world be not more than indefinite, it muſt be really finite, I conſider, that the Carteſians, though upon grounds of their own, muſt allow what I was obſerving; namely, that though every particular body in the univerſe is naturally capable of local motion, yet the univerſe itſelf is not; and though every particular body in the world have ſome determinate figure, yet the world itſelf, being indefinite, has not ſo.

WHEREAS *Ariſtotle*, and the philoſophers, that have lived ſince his time, have generally admitted the diviſion eſtabliſhed by him of all entities, into ſubſtance, and accident, and accommodated their rules to one of them, or both; the learned *Gaſſendus*,

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and his followers, have introduced a third sort of things, as not being either substances, or accidents; and these, if you will admit, you will, I presume, admit too, that they may be privileged from their rules calculated for other natures. Of this kind of things the Gassendists make place or space to be; for they will not allow it to be a substance, because it is neither body, nor spirit, but only somewhat, that has a capacity to receive or contain bodies, and would subsist, though God should annihilate all the substances he has created. And for the same reason it is not to be called an accident, since that necessarily requires a substance to reside in (according to that received axiom) *Accidentis esse, est inesse*; whereas in case of the annihilation of the world itself, and consequently all substances that compose it, their place or space would still remain, and be capable of admitting a new world of the same extent, if God should be pleased to create it: whence *Gassendus* wittily infers, that bodies are rather accidental in respect of place, than space in respect of bodies. But without staying to examine this paradox, I shall venture to say in general, that he who shall, with an heedful and unprejudiced eye, survey the several hypotheses, or systems, maintained by the different sects of philosophers, may find, that though the instances will not be all of them the same, there is none of these systems, in which one may not observe something or other, to which every one of the rules, that reach to the other subjects treated of in that philosophy, cannot safely be applied. And indeed the mind of man being naturally far more desirous to know much, than to take the pains requisite to examine, whether he does so or not, is very prone to think, that any small number of things, that it has not distinctly considered, must be of the same nature and condition with the rest, that he judges to be of the same kind. For by thus attaining to the knowledge of things, by way of inference, the mind gratifies, at once, both its vanity, and its laziness; looking upon these conclusions, as marks of the excellency of its rational faculty, whilst they rather proceed from a want of the due exercise of it.

Pyroc. BUT if the received dictates of reason be not always safe grounds to proceed upon in our discourse, I would gladly know, by what rules we shall judge of those rules, and discover them to be erroneous in case they be so; and by what measures we shall estimate truth and falshood, in those things, wherein the use of those rules must be laid aside.

Arnob. YOUR double objection, *Pyrocles*, I confess to be weighty enough to deserve a considerate answer; and to give you the sum of mine, in few words, I shall tell you, that, in my opinion, since there is no progress *in infinitum* in the criteria of truth, and that our faculties are the best instruments, that God has given us to discover and to examine it by; I think a clear light, or evidence of perception, shining in the understanding, affords us the greatest assurance we can have (I mean in a natural way) of the truth of the judgments we pass upon things, whether they be other things, or the vulgar rules of reasoning, or subjects, that claim a privilege from those rules.

AND here give me leave to consider, that it is not by induction, but by evidence, that we know, that *ex vero nil nisi verum sequitur*. By which it appears, that the innate light of the rational faculty is more primary, than the very rules of reasoning, since, by that light, we judge even of the lately mentioned axiom, which is itself the grand principle of ratiocinations made by inference.

Eugen. THIS matter may be, perchance, somewhat illustrated, by observing, that as the understanding is wont to be looked upon as the eye of the mind, so there is this analogy between them, that there are some things, that the eye may discern (and does judge of) organically, if I may so speak, that is, by the help of instruments: as when it judges of a line to be straight, by the application of a ruler to it, or to be perpendicular, by the help of a plumb-line, or a circle to be perfect, by the help of a pair

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of compaſſes. But there are other things, which the eye does perceive (and judge of) immediately and by intuition, and without the help of organs or inſtruments; as when, by the bare evidence of the perception, it knows, that this colour is red, and that other blue, and that ſnow is white, not black, and a charcoal black, not white; and ſuch a picture is very like, or another unlike to the face it was drawn to repreſent. For thus there are ſome things, that the intellect uſually judges of in a kind of organical way, that is, by the help of certain rules, or hypotheſes, ſuch as are a great part of the theorems and concluſions in philoſophy and divinity. But there are others, which it knows, without the help of theſe rules, more immediately, and, as it were, intuitively, by evidence of perception; by which way we know many prime notions and effata, or axioms metaphyſical, &c. as, that contradictory propoſitions cannot both be true; that from truth nothing but truth can legitimately be deduced; that two things, that are each of them equal to a third thing, are equal to one another; that a whole number is either even or odd. And it is alſo upon this evidence of perception, that we receive with an undoubted aſſent many primitive ideas and notions, ſuch as thoſe of extended ſubſtance or body, diviſibility, or local motion, a ſtraight line, a circle, a right angle, and many other things, that it would be here ſuperfluous to mention.

Arnob. I think the internal light, that the Author of Nature has ſet up in man's intellect, qualifies him, if he makes a right uſe of it, not only to apply the inſtruments of knowledge, but alſo to frame and to examine them. For, by the help of this light, the underſtanding is enabled to look about, and both to conſider apart, and compare together, the natures of all kinds of things, without being neceſſitated to employ in its ſpeculations the rules or dictates of any particular ſcience or diſcipline, being ſufficiently aſſiſted by its own light, and thoſe general axioms and notions, that are of a catholic nature, and perpetual truth, and ſo of a higher order, than the dictates or rules of any particular or ſubordinate ſcience or art. And by theſe means the underſtanding may perceive the imperfection and falſity of ſuch rules or theorems, as thoſe men, that look no higher, nor no further than their own particular ſcience or art, embrace for certain and unqueſtionable. Thus when philoſophers obſerved, that they could frame a clear notion of a thing, without conſidering, whether it were actually in being or not; or, even when they ſuppoſe, that it is not actually in being, as we can frame a clear conception of a roſe in winter, when there are none to be found growing, and have a clear notion of a myriagon, though it is very like there is no ſuch figure really exiſtent in the world: they have generally concluded, that the eſſence of things is differing and ſeparable from their exiſtence. And yet, when we conſider that God is a being infinitely perfect, and that actual exiſtence being a perfection, muſt belong to him, we may, by the ſame light of reaſon, that dictated eſſence and exiſtence to be two ſeparable things in all other beings, diſcern, that they muſt be inſeparable in God; and conſequently that the fore-mentioned rule, though more general than almoſt any other, is not abſolutely univerſal, but muſt be limited by the light of reaſon. And thus alſo philoſophers conſidering, that not only all ſorts of bodies, but the immaterial ſouls of men, (and angels themſelves, ſuppoſing ſuch beings) are all endowed with qualities, which are accidents, have included it in the very notion of a ſubſtance, to be the ſubject of accidents, or, as the ſchoolmen ſpeak, *ſubſtare accidentibus*; and accordingly *ſubſtantia* is wont to be derived *à ſubſtando*: but the infranchiſed intellect finding in itſelf a notion of an abſolutely perfect, and therefore exiſtent being; and conſidering, that to be the ſubject of accidents is not a thing agreeable to the higheſt perfection poſſible, it concludes, that in God there are no accidents. And this concluſion has been embraced as a part, not only of Chriſtian, but of natural theology, and maintained by divers philoſophers themſelves, upon metaphyſical and other meerly rational grounds. In ſhort, the native light of the mind may enable a man, that will make a free and induſtrious uſe of it, both to pass a right judgment of the extent of those very dictates, that are commonly taken for rules of reason, and to frame others on purpose for privileged things, so far forth as they are so. But I fear, gentlemen, the fourth advice I have ventured to offer you, has, by its tediousness, made you justly impatient of being detained by it so long; and therefore I shall advance to the fifth, which imports,

The fifth ADVICE, or RULE.

That where priviledged things are concerned, we are not always bound to reject every thing, as false, that we know not how to reconcile with some thing, that is true.

Pyrocl. You may call this an advice, but I doubt others will stile it a paradox, and possibly think it one of the greatest, that ever was broached.

Arnob. YET perhaps you will find by and by, that it may be in great part made good by what has been already discoursed, and by you admitted. I think it will not be doubted, but that there are, or may be conceived straight lines, whereof one is a hundred or a thousand times longer than another. It is also generally granted, that a longer line consists of, or may afford, more parts than a shorter; for a line equal to the shorter being taken out of the longer, and consequently just as divisible as it is, there will remain of the longer line another line, perhaps many times exceeding the shorter line. And lastly, it is generally acknowledged, that no number can be greater than infinite; since if the lesser number were capable of accession (as it must be, if it fall short of another number) it would need that accession (or a greater) to make it infinite, which yet it is supposed to be already.

Pyrocl. I see not yet, to what all this may tend.

Arnob. YOU will quickly perceive it, when I shall have desired you to reconcile these propositions with the demonstrations of geometers of the endless divisibility of all straight lines; whence they deduce, that though they be very unequal among themselves, yet the shortest of them contains, or may afford, infinite parts.

Pyrocl. BUT is there any thing more clear to human understanding, or more supposed in almost all our ratiocinations, than that two truths cannot be contradictory to each other?

Arnob. THOUGH I am far from affirming, that one truth can really contradict another truth; yet I think that, which is but a gradual or limited truth, may in some few cases not be reconcileable by us to an absolute and universal truth. For, I think we may (with *Sophronius)* distinguish those propositions we call true, into anxioms metaphysical, or universal, that hold in all cases without reservation; and axioms collected or emergent; by which I mean such as result from comparing together many particulars, that agree in something, that is common to them all. And some of these, though they be so general, that in the usual subjects of our ratiocinations they admit of no exceptions, yet may not be absolutely and unlimitedly true; of which I know not whether I formerly gave you an instance, even in that anxiom, which (almost) all meerly natural philosophers have supposed and built on, that *ex nihilo nihil fit*, which, though at least one of the highest of gradual or collected truths, may yet be not universally true, since, for aught we know, God, that is acknowledged to be a being, that is infinitely perfect, may have, and may have exercised, the power of creating. And in such case as this, not to be able to reconcile a truth, concerning a priviledged thing, with a proposition that generally passes for true, (and in other cases is so indeed) will not presently oblige us to reject either proposition as false, but sometimes, without destroying either, only to give one of them a due limitation, and restrain it to those sorts of things, on which it was at first grounded, and to which it was, because of man's ignorance, or inconsiderateness, that it was not at first confined. And if the miracles vouchsafed either for the Christian, or for any other religion, be any of them granted to to be true; (as almost all mankind agrees in believing in general, that there have been true miracles;) it cannot well be denied, but that physical propositions are but limited, and such as I called collected truths, being gathered from the settled phænomena of nature, and are liable to this limitation or exception, that they are true, where the irresistible power of God, or some other supernatural agent, is not interposed to alter the course of nature.

Pyrocl. But do you think there are no inconsistent propositions, that you would call truths, wherein you cannot shew, that one of them is but a gradual or emergent truth?

Arnob. It is one thing to enquire, whether men have yet discerned, or I am able to make out, that one of the propositions you speak of is but a limited truth; and another, to enquire, whether, speaking absolutely and universally, it may to any intellect appear to be no more than such. For first I consider, that the reason, why we judge things to be repugnant, being, that the notions or ideas we have of them seem to us inconsistent; if either of these notions be wrong framed, or be judged of by an unfit rule, we may think those propositions to be contradictory that really are not so; as, if you heedfully mark it, you shall find, that those, that are wont to employ their imaginations about things, that are the proper objects of the intellect, are apt to pronounce things to be unconceivable, only because they find them unimaginable; as if the fancy and the intellect were faculties of the same extent: upon which account some have so grosly erred, as to deny all immaterial substances, and chose rather so far to degrade the deity itself, as to impute to it a corporeal nature, than to allow any thing to have a being, that is not comprehensible by their imagination, which themselves acknowledge to be but a corporeal faculty. But besides this mistake of things repugnant, which arises from the mis-application or mis-management of our discerning faculties; I consider in the next place, that there may be another, that proceeds from the imperfection and limitedness of our understanding, which being unable to judge of priviledged things at the same rate that it does of other objects, may sometimes be unable to discover that reconcileableness, that a more illuminated and penetrating faculty may discern. This may be illustrated by what usually happens at sea, (for there mens prospect is the most free) when looking towards the main, the sky and the waters seem to meet at the edge of the (sensible) horizon, though indeed they are as far distant as heaven is from earth; and on the other side, if you skilfully mix together the dry and fine powder of orpiment and that of indico, you will produce a green colour, as is known to painters, and the eye takes notice but of an uniform mixture, in which it sees neither blue nor yellow: but if (as experience shews) you look on this mixture with a very good microscope, the emergent colour will disappear, and you will plainly see, instead of it, blue and yellow grains of the powders distinct from one another. Which instances may serve to shew the imbecility of our visive faculty; and the latter of them may teach us, that a thing may appear one and differing, as it is looked upon by a more or less discerning sight. But an instance more home to our present purpose may be afforded by yellow diamonds, which, because of their colour, not only other men, but the generality of goldsmiths (in whose error I have sometimes shared) take to be counterfeit gems, or at best but right topazes; whereas very skilful lapidaries will by sure signs discover and acknowledge them to be true diamonds, notwithstanding their seeming difference from unquestioned ones, and account them to be of the same nature with that noblest kind of jewels. Whence we may learn, that a more skilful judge may discern an agreement in things, that almost all other men think they see manifestly to be of distant natures.

Eugen. Give me leave, gentlemen, to say on this occasion, that I have several times observed, that men judge some things to be irreconcileable, not only when they are both of them represented to the understanding in the form of propositions; but when one of them is but a notion, or a current definition. For divers of these notions do contain in them a propofition, or are equivalent to it; as when a circle is defined to be a figure contained in a line, all whofe parts are equally diftant from the middlemoft point or center, this definition contains an affirmation of the effential property of a circle, and by the generality of geometricians is therefore difcriminated from that conic fection, which they call an ellipfis, though that be alfo a figure terminated by one curve line.

AND becaufe you are verfed in mathematicks, I fhall on this occafion fhew you by a geometrical inftance, that if a man have not genuine and adequate notions of the things he judges of, he may confidently, and even upon very probable grounds, judge things to be inconfiftent, that in reality are not fo. For if an ordinary cultivator of mathematical difciplines fhould hear one man fay, that fuch a figure is an ellipfis, and another affirm it to be a circle, he would think their affertions to be inconfiftent, having his mind prepoffeffed with an ellipfis being a conical fection, whofe properties muft therefore (he fuppofes) be very differing from thofe of a circle; whereas fuch wary geometricians as the learned Dr. *Wallis* * will tell him, that the vulgar notions of conic fections are not adequate to the figures producible by them: for when a right cone is cut quite through by an inclining plane, the figure produced by the fection agrees well with the received notion of an ellipfis, in which the diameters are of unequal length; yet if the plane cut the cone parallel to the bafis, that conic fection will be a true circle, having all its diameters equal.

* See his treatife *de Sectionibus Conicis.*

IT is indeed an uncommon and unheeded account, but fuch an one, upon which I have obferved not only logicians, but philofophers themfelves, to err about judging things reconcileable or inconfiftent; that if a man be not fufficiently acquainted with the nature of any of the two things under confideration, (and much more if he be ignorant of, or miftaken about both) he may think there is a contradiction between things, wherein a fuperior, or more piercing intellect, may difcern a confiftency: for taking it for granted, that he knows one thing to be a truth, if fome other thing be affirmed to be fo, which he has not underftanding or fkill enough to fee how to reconcile to it; it is no wonder, that how well foever this may be evinced, he fhould as little know how to admit, as how to reject it. This may be partly illuftrated, and partly proved by inftances, drawn from the mathematicks themfelves: for a novice in arithmetic, for example, finding that, according to his rules, there is not one mean proportional number between 4 and 32, will fcarce be able to reconcile that propofition to this other, that there are two mean proportionals between the mentioned numbers; for he may with great appearance of reafon afk, how, if there be not fo much as one mean proportional, there can be two? Whereas thofe, that are acquainted with the nature of ranks or feries of numbers proceeding in geometrical proportion, will eafily difcern, that between thofe two recited, both the number 8, and the number 16, are mean proportionals.

Timoth. THOUGH I difallow not your inftance, *Eugenius*, yet I fhall be willing to hear one or two others of a lefs abftracted nature.

Eugen. To obey you, *Timotheus*, I fhall add, that if an old fchool philofopher, or a mathematician, not acquainted with the latter difcoveries made by telefcopes, fhould hear one man fay, that the moon is the moft enlightned, when fhe appears full to us; and another affirm, that fhe is more enlightned at the new moon than at the full, he would readily conclude, upon the fuppofition (which he makes no doubt of) that the moon receives all her light immediately from the fun, that the affirmation of the latter (aftronomer) cannot be true; which yet he would not conclude, if he knew (what is difcovered by telefcopes) that the moon is as well enlightned by the earth, as the earth by the moon; whereas at the full fhe receives but thofe beams, that come to her directly from the fun, at the change fhe receives both them in that part of her body that is obverted to him, and thoſe other beams of his, that are reflected from the terreſtrial globe to that part of the moon that is neareſt to us.

AND to the foregoing inſtance I ſhall add one more, that ſeems appoſite enough to *Arnobius*'s purpoſe, and it is, that before *Pythagoras*, not only the vulgar of the Greeks, but their philoſophers and mathematicians too, obſerving oftentimes, that a bright ſtar preceded the riſing ſun, and that frequently alſo (on other days) after ſun-ſet, another ſtar appeared, that was none of the fixed ones; they confidently concluded from the ſo diſtant times of apparition, that the ſun was attended by two differing ſtars, to which accordingly they gave two differing names: but *Pythagoras*, who was a far better aſtronomer (as may be gueſſed, among other things, by his maintaining in thoſe early times the motion of the earth about the ſun) undertook to diſabuſe them, and effected it. Now if one, that had obſerved *Venus* only in the mornings, ſhould have affirmed, that beſides the ſix known planets, there was but a ſeventh (namely the phoſphorus) which preceded the riſing ſun; and another, (that had taken notice of her only in the evenings) ſhould aſſert, that beſides the ſame ſix known ones, the only ſeventh was that called *Heſperus*, which ſometimes appeared after his ſetting; a by-ſtander would preſently have concluded, that their aſſertions were not reconcileable, either to one another, or to the truth, which (in his judgment) was, that there muſt be no leſs than eight viſible planets: and yet *Pythagoras*, who had more ſkill, and more piercing wit, did, (as was lately noted) diſcern and teach, that theſe two phænomena were produced by one and the ſame planet *Venus*, determined by its peculiar motion (about the ſun) to ſhew itſelf near our horizon, ſometimes before he aſcends it, and ſometimes after he had left it. Such inſtances as theſe, though offered but as illuſtrations, may perſwade us from being too forward to reject every propoſition, that we ſee not how to reconcile to what we take for a truth; provided the diſtruſted propoſition be ſuch, as we would acquieſce in, if we could reconcile it to that ſuppoſed truth.

Timoth. FROM this diſcourſe, *Eugenius*, and that of *Arnobius*, which preceded it, I think one may gather, that, according to you two, when two propoſitions are laid down, whereof one is made evident to us by experience, or by reaſon, acting within its own juriſdiction or compaſs; and the other is ſufficiently proved by being mathematically demonſtrated, or duly atteſted by divine revelation, we ought not to reject either of theſe propoſitions, as no truth, meerly becauſe we do not yet know how to reconcile them: but we ſhould rather think, that the collected propoſition is but a gradual, or limited truth; or elſe we ſhould conſider, that we knowing but ſo imperfectly as we do the particular natures of priviledged ſubjects, for aught we know, a ſuperior intellect may be able to diſcern a friendly agreement between what is delivered about that ſubject, and the affirmation that ſeems repugnant to it, though we are not quick ſighted enough to perceive this agreement. And this, how ſtrange ſoever you may think it, *Pyrocles*, may not only be countenanced by ſuch things as *Eugenius* lately had, but both you yourſelf, and almoſt all mankind do *de facto* ſeem to practiſe it, in the caſe of the divine preſcience of man's free actions.

Eugen. WHAT you contend for, gentlemen, may perhaps be thought the more receivable, if one ſhould argue: Firſt, either the propoſitions ſaid to be repugnant are both really true, or they are not; if it be anſwered, that they are not, the difficulty is at an end: for there is none at all to conceive a true propoſition ſhould contradict a falſe one. But, ſecondly, if both the propoſitions be ſuppoſed to be true, it muſt be affirmed, either that they are reconcileable, or that they are not; if it be ſaid they are not, then *Pyrocles*'s objection is out of doors; for it cannot then be reaſonable to ſay, that the two propoſitions, though inconſiſtent with one another, muſt neceſſarily be one

or

or other of them inconsistent with the truth. But this I presume he will by no means assert, and consequently must say, that the propositions are reconcileable. Upon which answer I shall demand, how that can be, unless a superior intellect, such as unquestionably the divine is, can discover an agreement between propositions, wherein we cannot discern it? For our not being able to discern it is, you know, professedly supposed in the case we discourse of.

Pyrocl. BUT, *Arnobius*, will not this doctrine make us very liable to have falsities imposed on us, at the pleasure of bold and dictating men?

Arnob. NOT, if it be limited to the subjects, wherein alone I would have it admitted: for if neither of the things treated of be a priviledged one, but both in the jurisdiction of ordinary reason, I do not only consent, but (in my first advice) require, that the propositions framed about them be estimated according to the common dictates of reason. And even in cases, where one of the propositions is about a priviledged thing, I do not at all think fit, that it should be received in spite of its being repugnant to the gradual truth delivered in the other, unless it can by some other argument, sufficient in its kind, be proved to be true; and in that case, that what I plead for ought to be admitted, is employed by the suffrage of almost all mankind in that case, which was just now pertinently mentioned by *Timotheus*: for though men know not how to reconcile the liberty of man's will with the infallible knowledge, that God has of those actions that flow from it, yet they have unanimously judged it reasonable to believe both free-will and prescience; the former, because they felt it in themselves; and the latter, partly because the fore knowledge of things being manifestly a perfection, ought not to be denied to God, whom they looked upon as a being supremely perfect; and partly because some actions and events, that they all judged to flow from mens free will, were, as the generality of men believed, foretold by prophetic oracles. But except in such cases as I have been naming, I am altogether of *Pyrocles*'s mind, that since we have scarce any way of discovering a falsity, but by its being repugnant to somewhat that is true; to deny, that in cases within the jurisdiction of ordinary reason, the repugnancy of a proposition to any manifest truth ought to sway our judgments, were to deprive us of the usefullest *criterion* to discriminate between falshood and truth.

Timoth. FOR my part, who believe with many philosophers, as well Heathen as Christian, that human souls owe their origin to God, and with almost all philosophers, (for I know what the *Stoicks* held) that as he is the supreme being, so he is a most free agent, I see not why, as he has given to corporeal beings divers qualities, very differing in their degrees of nobleness; so he might not give to the intelligent productions of his power and will various degrees of intellectual capacities, as well as a limitedness of nature. And as it will not follow, that because we can see with our eyes very small objects, and imagine such as are yet much smaller, either the eye or the imagination can ever reach to so small an object as an atom; so it will not follow, that because we are able to frame conceptions of immaterial beings, we must therefore be able to understand the nature of God, and the harmony of all his monadical attributes. A little boy may have a clear notion of three, four, five, or other smaller numbers, and yet may be unable to frame good conceptions of triangular and other polygon numbers, (as some call them,) and much more of the abstruse affections of surd numbers, and the roots of the higher algebraical powers. To discern particular truths is one thing, and to be able to discover the intercourse and harmony between all truths, is another thing, and a far more difficult one; as a traveller may upon the English shore know, that he sees the ocean, and upon the coast of *Africk* be made to do the like, and at the *East-Indies* also he may know, that he sees the ocean; and yet not know how those so distant seas

ſeas communicate with each other, though that may be manifeſt enough to a coſmographer.

Arnob. WHAT you ſay brings into my mind, that I have ſometimes thought God and men enjoy truth as differingly as they do time. For we men, as we enjoy time but by parcels, and always leave far the greateſt part of it unreached to by us; ſo we know but ſome particular truths, and are always ignorant of far more than we attain to. Whereas God, as his eternity reaches to all the portions of time (or meaſured durations) ſo his omniſcience gives him at one view a proſpect of the whole extent of truth: (as if a man could ſee the whole river of *Nilus* with its turnings and windings, from its hidden ſprings to its entrance into the ſea.) Upon which account he ſees all particular truths, not only diſtinct, but in their ſyſtem, and ſo ſees a connexion between thoſe, that to us ſeemed the moſt diſtant ones.

Arnob. THERE remains now, gentlemen, but one part more of your penance to be undergone; for it is high time I ſhould haſten to the relief of a patience I have ſo long diſtreſſed; and therefore I ſhall give it but one exerciſe more, and conclude your trouble with ſome reflections on this laſt advice.

The ſixth ADVICE, or RULE.

That in priviledged things we ought not always to condemn that opinion, which is liable to ill conſequences, and incumbred with great inconveniencies, provided the poſitive proofs of it be ſufficient in their kind.

THAT this advice may be the more eaſily admitted, I ſhall ſeparately ſuggeſt three things, which I deſire may be afterwards conſidered all together.

FIRST, that clear poſitive proofs, proportionate to the nature of things, are genuine and proper motives to induce the underſtanding to aſſent to a propoſition as true; ſo that it is not always neceſſary to the evidence and firmneſs of an aſſent, that the intellect takes notice of the conſequences, that may be drawn from it, or the difficulties, wherewith it may be incumbred. This is plain in thoſe aſſents, which of all others, at leaſt that are meerly natural, are by knowing men thought to be the moſt undoubted and the beſt grounded; I mean the aſſents, that are given to the truth of geometrical demonſtrations: and yet, *Euclid*, for inſtance, in all his elements of geometry, in ſome of which ſurpriſing paradoxes are delivered, (as in the ſixteenth propoſition of the third book, and the 117th of the tenth book, to name no more) contents himſelf to demonſtrate his aſſertions in a mathematical way, and does not, that I remember, anſwer or take notice of any one objection: and the geometricians of our days think they may ſafely receive his propoſitions upon the demonſtrations annexed to them, without knowing or troubling themſelves about the ſubtleties employed by the ſceptic *Sextus Empiricus*, or others of that ſect, in their writings againſt the mathematicians, and all aſſertors of aſſured knowledge.

THE ſecond thing I would offer to your conſideration, is, that the former part of our diſcourſe has manifeſted, that there are ſome things, which our human and imperfect underſtandings either cannot, or at leaſt do not, perfectly comprehend; and that, nevertheleſs, men have not refrained from preſuming to dogmatize, and frame notions and rules about ſuch things, as if they underſtood them very well. Whence it muſt needs come to paſs, that if they were miſtaken (as in things ſo abſtruſe it is very like they often were) thoſe, that judge by the rules they laid down, muſt conceive the propoſitions oppoſite to their miſtakes to be liable to very great, if not inſuperable difficulties and objections.

AND

AND this second consideration, in conjunction with the first, will make way for the third, as a natural production of them; which is, that, as we need not wonder, that priviledged things, which are wont to be so sublime as to have been out of the view of those that framed the rules, whereby we judge of other things, should be thought liable to great objections by them, who judge of all things only by those rules; so we should not require or expect more evidence of a truth relating to such things, than that there are for it such sufficient positive reasons, as, notwithstanding objections and inconveniencies, make it, upon the whole matter, worthy to be embraced.

Pyrocl. BUT can that be worthy to be assented to, which is liable to objections and inconveniencies, which the maintainers confess they know not how to avoid? Does not, your *Euclid* himself, in some of his demonstrations, employ that way of reasoning, which some of his Latin Interpreters call *deductio ad absurdum?*

Arnob. EUCLID indeed, (as well as other mathematicians) besides that more satisfactory way of direct probation, which perhaps he might have oftner employed than he did, has sometimes, where he thought it needful, made use of the Επαγωγὴ you speak of. But in these cases he never goes out of the discipline he treats of, and confining himself to arguments drawn from quantity, he urges nothing as absurd, but what is undeniably repugnant to some truth he had already demonstrated, or to those clear and undisputed definitions, axioms, or postulata, which he supposes to have been already granted by those he would convince. But though he thus argues, to prove, that his readers cannot contradict him without contradicting themselves; yet we find not, that he was at all sollicitous to clear those difficulties, that so quick-sighted a man could not but know some of his theorems to be attended with: but contents himself to demonstrate the incommensurableness of the side and diagonal of a square, without troubling himself to take notice of the difficulties, that attend the endless divisibility of a line, which would follow from what he demonstrated. But, *Pyrocles*, to look back to the first part of your objection, though what you say will hold in ordinary cases; yet such peculiar ones, as we are speaking of, deserve a particular consideration. About some priviledged things there are, and about some others there may be contradictory opinions (taking that term in a strict sense) maintained. Now as both of these cannot be true, so one of them must be so: as, though it be hotly disputed, whether quantity be endlesly divisible, yet certainly either it must, or must not be divisible without end: and, as was formerly observed, which side soever you take, the inconveniencies will be exceeding great, and perhaps there will lie objections scarce to be directly answered. And since one of the two opposite opinions must be true, it will not always be necessary, that an opinion must be false, which is incumbred with great difficulties, or liable to puzzling objections. And therefore if the positive proofs on one side be clear and cogent, though there be perplexing difficulties objected by the other, the truth ought not for their sake to be rejected; because such difficulties proceeding usually either from notions, that men presume to frame about things above their reaches, or from rules, that were not made for such points as are in dispute, the objections are not to be judged so well founded, as is that acknowledged principle in reasoning, that from truth nothing but truth can be legitimately inferred.

Eugen. I confess I have always thought it reasonable in such cases, to compare, as well the positive proofs of one opinion with those of the other, as those objections that are urged on either side; and there make my estimate upon the whole matter, though with a peculiar regard to that opinion, that has a great advantage in point of positive arguments; because, as *Arnobius* observed, those are the proper inducements to the assent of the intellect: and then the objections may well enough be suspected to proceed from the abstruse nature of priviledged things, and the over-great narrowness of the

the rules, that men are wont to judge of all things by: for we may have a sufficiently clear proof, that a thing is, whilst we have no satisfactory conception of its manner of existing or operating; our illative knowledge, if you will allow me so to speak, being clearer, and extending further, than our intuitive or apprehensive knowledge.

Arnob. But even about things, that we cannot sufficiently understand, we may in some cases exercise our reason in answering objections, that are thought not to be at all answerable, because they are not directly so. For we may sometimes shew, by framing in another case a like argument, which the adversary must confess does not conclude well, that neither does the argument, that contains his objection, conclude aright.

This I could exemplify (though that may seem no easy task) but that I fear I should want time to propose examples, whose being very paradoxical would make them need much proof: which you who, I fear, are quite tired already, would want patience to hear. Wherefore I shall rather recommend to you one observation, which I take to be of no small moment and use, when we contemplate things of the nature of those we have been discoursing of; and it is this, that we must not expect to be able, as to priviledged things, and the propositions that may be framed about them, to resolve all difficulties, and answer all objections; since we can never directly answer those, which require for their solution a perfect comprehension of what is infinite: as a man cannot well answer the objections, that may be made against the *Antipodes*, the doctrine of eclipses, that of the different phases of the moon, and of the long days and nights of some months apiece near the poles, (not now to name that more abstruse part of astronomy, the theory of the planets) unless he understand the nature of the sphere, and some other principles of cosmography. So that where priviledged things are concerned, clear and positive arguments ought to be of great weight in favour of the opinion they conclude for; even when on the contrary side, we may discourse ourselves into such difficulties, as may perplex, and perchance pose our limited understandings.

Eugen. This is a point of such abstruse speculation, that if when we remember ourselves to be but men, it shall be thought fit to adventure upon launching out into it, it will be necessary to adjourn the prosecution, till we shall have more time before us than we are masters of now, that the hour is so late, that it is high time we should, after our deserved thanks to him, release *Arnobius*, by wishing him a good night.

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THOSE, who have published some former tracts of this honourable author's, have made their complaints, that his writings have not met with the same candor and ingenuity from all writers: for though some have very civilly mentioned his name in those experiments, observations, and phænomena, which they have borrowed from his writings; yet others have vendicated that to themſelves, which was none of their own; for having lighted their candle at his torch, and raiſed ſome ſuperſtructures upon thoſe foundations he had laid, they have made no mention of him at all; in which matter the ſincerity of philoſophers is not a little required.

IT is known, that Mr. *Boyle* was the firſt inventor of that pneumatic engine, or air-pump, called from him, *Machina Boyliana*; the figure and many of the uſes whereof he hath deſcribed in his tract, entituled, *New phyſico-mechanical experiments concerning the ſpring and weight of the air*, (and in the continuation of it;) which book, being tranſlated into *Latin*, and diſperſed into foreign parts, hath given occaſion to tranſmarine virtuoſi to make eſſays (not altogether unſucceſsful) concerning the fabricating of the like engines; though none of their tracts upon that ſubject have equalled the original. In like manner, it is highly probable, that his diſſertations concerning phoſphoruſes, and his free diſtribution of ſeveral parcels of that luminous ſubſtance, have excited others to deſcant upon the ſame ſubject.

BUT that it may juſtly appear, that the honourable author ſhines only by his own light, and hath not any thing mutuatitious in the following treatiſe, the reader is to be advertiſed, that this tract had much earlier come abroad, if, after I had received the whole, even to the laſt ſheet, in 1681, that ſo the Engliſh edition, and my Latin verſion thereof (wherewith the noble author was willing to gratify the curious in foreign parts) might be carried on together, I had not been by long ſickneſs, and divers unfavourable circumſtances, obliged to retain it ſome months in my hands.

WHICH I am the more troubled at, becauſe I find, that my neceſſitated delay hath given opportunity to the publication of ſome experiments about noctilucas. But the virtuoſi, who have the honour to be acquainted with the noble author of this book, and have ſeen the laſt ſummer and autumn divers of the chief experiments made, which are mentioned therein, will eaſily believe, that the author needed not to borrow any thing from thoſe ſpecimens, which have been lately publiſhed by an ingenious man about noctilucas; which, beſide that they contain but few phænomena, or trials coincident with his, were ſo far from affording him any information, that (to be ſure to be no borrower) he never would to this day read any one of them, or hear them read. And it appears, by the cloſe of the aerial noctiluca, preſented in print to the Royal Society, towards the end of the year 1680, that he then knew, and had practiſed, ſeveral other ways of making noctilucas, than that, which he then, and before any other man, imparted to the world.

THE uſefulneſs of the enſuing diſcoveries and reflexions will be beſt judged of by diſcerning readers, who therefore need not to be previouſly informed thereof by me.

The PREFACE.

SIR,

THE very kind reception you were pleaſed to give thoſe papers about the aerial noctiluca, that I addreſſed to you about the beginning of the laſt winter, does not only invite me, but in a manner oblige me to impart to you ſome trials, that I made, after I had ſent you the others, about the ſame kind of phoſphorus; to which you may now be pleaſed to annex them, by way of continuation or appendix. But that being true, which is noted by *Lucretius*, where he prettily ſays, that——*res accendunt lumina rebus*, you will not think it ſtrange, that if this obſervation holds in other things, it ſhould particularly take place in luminous things themſelves. And therefore, without any further preamble, I ſhall tell you, that, whilſt my occaſions permitted me to ſtay in *London*, I was willing to try, whether I could carry on ſomewhat further that, which had already been not unsuccessfully begun. And accordingly I caused to be prepared and distilled four or five several materials, which I thought the curious, especially those addicted to chymistry, would wish to have had trials made upon, and would perhaps blame me, if I had omitted to employ. But those materials not answering what was desired, we made choice of another or two, which I forbear to name, for certain reasons, and particularly for this, because they are to be found but in very few places, and perhaps would not have served my turn, if I had not luckily procured them in a season, whose dryness continued almost to a wonder.

From these materials sometimes, and sometimes also from the dried residence of urine, we obtained now and then some of the aerial noctiluca, such as that you have hitherto received accounts of; but more frequently a nobler sort of phosphorus, about which I now proceed to impart to you some observations, as I did, without any curious method or ornament, set them down from time to time, as I happened to make or remember them. And, to facilitate the pardon I beg of you, for having omitted some experiments, that perhaps you will wish I had made, I must acquaint you, that being by some necessary occasions enforced to leave *London* for a much longer time than I have done for several years last past, I have been obliged to make most of the following trials in a small village, where I yet am, and where, being unaccommodated with furnaces, instruments, and other chymical conveniences, for making and varying experiments, you receive an account, not of all the trials I would have made, (some of the chief of my catalogue remaining still unattempted) but of all those I could make in my present circumstances. However, such as they are, I think not fit to delay any longer the acquainting you with them; and therefore have ordered them to be gathered into one small collection of memoirs, to which paper I have given the title of *the glacial or icy noctiluca*, not only to distinguish it from the aerial one, whereof you have already had an account; but for another reason, that you will quickly meet with in the little tract itself, from whose perusal I must not now any longer detain you.

An APPENDIX to the AERIAL NOCTILUCA.

SECTION. I.

THE shining matter, contained in our best phials, being wasted in experiments, and partly given away, I thought fit to try, whether by the help of heat and other motion, our want might not be somewhat supplied, till more noctilucous matter could be prepared.

EXPERIMENT I.

In pursuit of this design, I took an old phial, that had been long laid aside as useless, because the matter had been poured out of it into a clearer and smaller glass; and having held that side of this phial, to which I perceived some feculent matter stuck, near the fire, till it had conceived a considerable degree of warmth, I removed it into a dark place, and as I expected, found it to shine, and that vividly enough, whilst it retained a competent degree of heat; and when it cooled too much, the light ceased with the agitation, that as a cause or a condition accompanied it. But if afterwards the phial were again held to the fire as before, the shining power would be excited, and the splendor would continue a pretty while. But after some days or weeks (for I remember not which,) this disposition to be made luminous by external heat was utterly lost.

EXPERIMENT II.

HAVING alſo taken notice of a little feculent ſtuff, that ſtuck to the inſide of the glaſs, that had contained ſome of our noctilucal matter, I imagined, that though it would not ſhine in the dark by the contact of the air, to which it had been too long accuſtomed; yet when once that diſpirited or diſanimated ſuperficies, (if I may ſo call it,) that had lain expoſed to the air, was removed, the more internal part of the matter might not be deſtitute of a ſhining power. I carefully ſcraped off the outermoſt ſurface, and rubbing a little of the reſt with my finger upon my hand, I found it to ſhine well enough. And though the matter, being once more left expoſed to the air, did loſe in its ſuperficial parts the virtue of ſhining in the dark; yet thoſe parts being taken off, the remaining matter (being rubbed) did not appear deſtitute of a luminous quality; ſo that it ſeemed, that though the air did, after a while, mortify, as it were, and ſpoil the ſuperficial parts, that were expoſed to its immediate contact, yet this vitiated ſurface ſerved for a kind of cover or fence to the matter, that lay beneath it, and kept it from evaporating or ſpending thoſe ſpirituous or ſubtile parts, on whoſe account it was capable of becoming luminous.

EXPERIMENT III.

AND as I had obſerved on other occaſions, that liquors abounding in tenacious parts, though the liquors did not appear opacous or feculent, would leave ſticking to the inſides of the glaſſes, that contained them, ſomewhat, that though generally not perceived, becauſe not very manifeſt, was by ſome other ways, that I had tried, diſcoverable: having, as I was ſaying, obſerved this, in ſome other caſes, I ſuſpected, that even in a phial, that had formerly contained ſome of our ſhining ſubſtance, though it ſeemed to have been well emptied, and to have no groſs feculency adhering to it, there might ſtick ſomewhat, which, though unobſerved by the naked eye, might be made viſible to the naked eye by heat or motion. In purſuance of this conjecture, I took this glaſs; and having crackt it into fragments, that it might be put into the neck of a phial of a convenient ſhape and ſize; and having well ſtopt the veſſel, and removed it into a dark place, we there ſhook it, and had the pleaſure to ſee, not only that light was readily produced by the motion excited in the juſtling of the parts one againſt another, but that, by reaſon of the various poſition of the fragments of glaſs, ſome looking upwards, ſome downwards, ſome to the right hand, and ſome to the left, the light ſeemed to be vibrated every way with a very delightful vividneſs. This production of a kind of blazing light was often repeated with theſe broken pieces of glaſs, and if the phial were heated, the effect ſeemed more quick and conſiderable; and (if I miſ-remember not,) by only warming the phial, without ſhaking it, a light would be produced.

EXPERIMENT IV.

AFTERWARDS having beaten ſome of the glaſs into ſuch ſmall pieces as were capable of paſſing through the neck of a glaſs-egg with a flat bottom, that it might ſtand upright of itſelf, we hermetically ſealed up the neck, to try whether, by this hindring the included matter from exhaling or tranſpiring, we could keep the beaten glaſs always fit to exhibit the phænomenon: but we found not the event anſwer our deſires; for after no long time, we could no more produce any light in our ſealed veſſel, though an unlucky accident happening in one of our laſt trials, keeps me from being fully ſatisfied of the unpracticableneſs of the thing deſigned.

EXPE-

EXPERIMENT V.

In the printed tract of the aerial noctiluca, there is mention made of ſome liquor, that was hermetically ſealed up in a bolt-glaſs, that was not great, to try, whether by this way we could for any long time preſerve the ſhining faculty of that liquor, wherein it was already exceeding faint, and not to be excited but by a conſiderable degree of heat, and a vehement agitation of the veſſel itſelf. This ſealed glaſs having been left in the corner of a window, for what was judged a competent time, we yeſternight approached the veſſel by degrees to the fire, and ſhaking it from time to time, till the included liquor, had acquired a conſiderable degree of heat; then removing it to a dark place, and ſhaking the veſſel ſomewhat ſtrongly, we perceived, that the diſpoſition the liquor had to ſhine was very much impaired, but not quite aboliſhed, For there would from time to time, upon the rude agitation it was put into, appear little portions of matter, that looked prettily, and ſhone very vividly, like ſparks of fire; and ſome of theſe appeared in the ſpherical part of the glaſs, and ſome in the neck; ſome of them ſeemed as it were fixed to their firſt ſtation, and others moved upwards and downwards, and moſt of them continued ſome time to ſhine a pretty while before they diſappeared; and when they vaniſhed, few of them did ſo by degrees, but each luminous ſpeck, when it had laſted out its time, loſt its whole light at once.

SECTION II.

THE new liquid phoſphorus, I lately mentioned to have been made ſince the publiſhing the aerial noctiluca, was poured into a large phial, that might certain (by our gueſs) ten or twelve times as much as was put into it; ſo that the ſhining matter, having by ſo much air included with it, might thereby be aſſiſted to diverſify at leaſt ſome of the phænomena afforded by former noctilucas.

OBSERVATION I.

And accordingly I obſerved in the firſt place, that though the ſhining ſteams filled the whole cavity of the large glaſs, that was untaken up by the liquor and the reſidence, and this lighter flame continued much longer at once, than any we have hitherto mentioned, for it continued vivid ſeveral days and nights together, without ever unſtopping the phial to give it freſh air. And, if I miſ-remember not, I obſerved it to do ſo for about a week, before my occaſions hindered me from obſerving it any longer.

OBSERVATION II.

I ſometimes took notice with pleaſure, that ſome exhalations or vapours, that appeared conſiderably luminous, ſeemed to roll to and fro, like little clouds or aggregates of ſmoak, in the cavity of the veſſel, though it ſeemed difficult to determine, what ſhould give them, and maintain in them, ſuch a motion.

OBSERVATION III.

The bigneſs of the glaſs being conſiderable, it happened, that ſometimes when I went into the darkned place where I kept it, ſo much luminous matter would make a ſurpriſing ſhow; but though its extent were far greater, yet its intenſity did not much exceed that of the light afforded by the noctilucas of the firſt ſort, as, for diſtinction's ſake, thoſe may be called, that are mentioned in the printed account. Only this I often took notice of, that, in caſe I ſhook the matter gently, the light would appear much more vivid, and, as it were, would flaſh in and about the ſurface of the liquor where it was contiguous to the air, than it did elsewhere. And this splendor was such, that if it had been lasting, I thought it would have made our phosphorus useful for considerable purposes.

OBSERVATION IV.

When after having so many days kept this glass stopt, at last it would not longer shine of itself, we supposed it to be reduced to the condition of a phosphorus of the first sort; and accordingly found, that upon the removal of the stoppel, and ingress of fresh air, the cavity would in a moment be filled with fumes, that looked white in an enlightned place, but luminous in a dark one, and (probably by reason of the quantity of the air contained in so capacious a glass) the light usually continued much longer than it was wont to do in noctilucas of the first sort.

OBSERVATION V.

Being desirous to try, whether this more vigorous matter, if it were kept so exactly stopped, that none of the luminous vapours could exhale, would not last very long, I put near two spoonfuls of the liquor, with some of the sediment, into a bolt glass (with a flat bottom, that it might stand without leaning) capable of containing in all near twice as much: this glass being hermetically sealed, the included liquor continued to shine, without any external help either of air or heat, for about six days and nights, but then it gave over shining, nor would be made luminous again by moderate shaking.

OBSERVATION VI.

After having poured out some more of the liquor and sediment, that had been kept in the great phial, formerly mentioned, into a smaller phial, to make a present of it to a virtuoso, the remaining matter, having now a greater proportion of air included with it, was very apt to be put into a luminous agitation, if I may so call it, and would emit exhalations, that would not only fill the cavity of the glass, but manifestly move to and fro in it after a somewhat odd manner. And being one night willing to give a lady, and some other company, the divertisement of a new phænomenon, after having opened the phial, and then having stopt it again, I shook it, and turned it in such a way, that much the greatest part of liquor having been before poured out, the residence was, as it were, spread over the inside of the glass, to which its particles stuck, because there wanted liquor enough to wash them down: by which means, those little portions of the sediment being not covered, as they were wont to be with water, but exposed to the immediate contact of the air, shone much more vividly than the luminous exhalations were wont to do; and the light being tremulous and twinkling, as well as brisk, they seemed to emulate so many little stars in a cloudless but dark night, and continued this scintillation longer than one would have expected, to the no small delight of the beholders, for whose sake the experiment we several times repeated with success.

New PHÆNOMENA exhibited by an Icy Noctiluca; or, Solid Self-shining Substance.

SECTION III.

In the address of the foregoing appendix to the aerial noctiluca, I intimated the reason, why I did not think fit to give you a more particular account of the materials I employed in prosecuting my design, of making better qualified noctilucas. And therefore I ſhall not for the preſent trouble you with the mention of proceedings, that for want of ſome things, ſeldom procurable, you would not, where you live, be able to imitate; but ſhall ſave you and myſelf the trouble of a further preamble.

HAVING then by proceſſes, not unlike that I annexed to the cloſe of the aerial noctiluca, obtained a ſelf-ſhining ſubſtance of a conſiſtent form; I proceed to give you ſome account of what I have obſerved about it, and tried with it, which will take up the leſs time to do, becauſe many things belonging to it in common with the ſhining liquor, with which I have already entertained you, thoſe will be the fewer, that belong peculiarly to the ſelf-ſhining matter, endowed with a conſiſtent form.

ABOUT which it may be proper to take notice of ſome affections, that ſeem more immediately to belong to the ſubſtance itſelf, than moſt of the things do, that are to follow.

Some qualities of the noctiluca itſelf.

1. And firſt, though it uſually came over in diſtillation in the form of divers little grains or fragments, differing for the moſt part from one another, both in bigneſs (ſome being of the ſize of grains of corn, and others of peaſe, or large cherry-ſtones) and alſo as to their ſhapes, which moſt commonly were irregular, as concretions are wont to be, that are caſually produced; yet when the diſtillation was carried on proſperouſly, we obtained the deſired matter in greater lumps, ſometimes as large as ſmall beans, and ſometimes at leaſt three or four times as large, but not proportionably thick.

2. THESE lumps, whether ſmall or great, were colourleſs, and uſually, when they were held againſt the light, tranſparent; ſo that divers bodies, placed beyond them at a convenient diſtance, might be plainly ſeen through them. And ſome of the bigger appeared ſo like ſuch fragments of ice, as being thin. are oftentimes very clear, and almoſt quite deſtitute of manifeſt bubbles, that, becauſe of this great reſemblance, and for diſtinction ſake, I thought it not amiſs to call our conſiſtent ſelf-ſhining ſubſtance, the icy or glacial noctiluca (and for variety phoſphorus;) which name I choſe to give it, rather than that of chryſtalline, becauſe this epithet is not unfrequently given to every diaphanous liquor, as well as to tranſparent ſolids. But when I ſaid, that our noctiluca was tranſparent and colourleſs, I meant it only in reference to what uſually appeared. For whether it were any real difference in the texture or conſtitution of the body itſelf, or the effect of ſome caſual junctures of circumſtances, I am not ſure; but this occurred to us, that ſometimes, eſpecially by candle-light, ſome leſſer fragments appeared not diaphanous, nor always either colourleſs, nor of the ſame colour. For ſometimes the matter looked reddiſh, ſometimes of a faint, but pleaſing blue, and ſometimes too of a colour to which I cannot eaſily aſſign a known name.

3. OUR icy noctiluca or phoſphorus, is manifeſtly heavier in ſpecie than common water, in which being put, it readily ſinks to the bottom, and quietly lies there.

4. THE ice-like body, though conſiſtent, is not hard, being far leſs ſo, than common ice; but yet it is not ſo ſoft, but that it is brittle, and will more eaſily be broken in pieces by the preſſure of ones fingers, than receive ſhapes from them; and yet by him, that goes ſomewhat warily to work, it may be ſpread upon a ſolid body, almoſt like the unmelted tallow of a candle.

5. THE conſiſtent phoſphorus is fuſible enough. For though in the air it will not be brought to melt, without ſome difficulty and waſte, yet by the help of hot liquors, and even of water itſelf, it may with a little care and dexterity be brought to melt, which is an obſervation of good uſe; becauſe by means of fuſion, ſeveral fragments (if the matter be pure enough) may be brought to run into one lump, and in that condition

may

may both be the better preserved, and become fit to be applied to some considerable uses, which cannot so well, if at all, be made of lesser, though numerous fragments.

6. THIS glacial noctiluca is, as to sense, cold, but of a texture, that disposes it to be easily agitated, and by agitation become incalescent, as will appear hereafter. When this solid noctiluca is held in the free air, though perhaps its superficies be wet, it affords a very vivid light, usually surpassing that of the aerial noctiluca; and this light seems to proceed from, if not also to reside in, the body itself.

7. WHEN our icy phosphorus is taken out of its receptacle, and exposed to the immediate contact of the free air, it usually emits a wonderful deal of smoak, discernable by the light of the body it ascends from; and this plentiful emission of effluviums usually lasts as long as the phosphorus is kept in the air.

8. BUT it is pleasant to observe, and deserves to be considered, that as soon as it is plunged in water, so as to be quite covered with that liquor, it ceases not only to smoak as before, but to shine, as if a thoroughly kindled coal were suddenly quenched in water. And if it were not for this, our noctiluca would effluviate so fast, that it would be quickly wasted; whereas the water, fencing it from the contact of the air, keeps it from spending itself as formerly, and yet does really make but a seeming and temporary extinction of this anomalous fire. For as soon as it is again taken out of the water (though it have lain there perhaps a great while) it falls to shine again, even whilst it is yet dropping wet.

9. AND I have sometimes had the pleasure to observe, that when I had so large a piece of noctiluca, that I could conveniently hold one half of it under the surface of the water, and the other half above it, whilst the immersed part afforded no light, the extant part shone vividly.

HAVING thus mentioned most of the qualities, that belong to the noctiluca itself, I shall now proceed to the phænomena my trials on it, or with it, afforded me, without confining myself to any sollicitous order, since my circumstances permitted me not to keep one in making those trials. But before I descend to other particulars, it will not, I think, be amiss to take notice of a few, that, having more affinity than others with the last mentioned quality of our phosphorus, seem proper to be annexed to what has been delivered of it.

OBSERVATIONS about the WATER wherein the NOCTILUCA was kept.

SECTION IV.

BECAUSE I guessed, that the water, whereing the noctiluca had been long kept covered, to fence it from the air, though it did not manifestly dissolve the mass, yet might be impregnated at least with the more saline, and, on that account, resoluble parts of it, I thought fit to make a few trials upon this liquor.

EXPERIMENT I.

AND first, I found, that it had a strong and penetrant taste, that seemed near of kin to that of sea-salt, but was more piercing, as if brine were mingled with spirit of salt, and it relished also somewhat of vitriol.

EXPERI-

EXPERIMENT II.

Being put into a ſmall concave veſſel of refined ſilver upon lighted coals and aſhes, it evaporated but very ſlowly, and would not be brought to ſhoot into cryſtals, nor yet to afford a dry ſalt, but coagulated into a ſubſtance, ſometimes like a jelly, and ſometimes, as to conſiſtence, like whites of eggs; which ſubſtance was eaſily melted by heat.

EXPERIMENT III.

When this ſubſtance was kept a while on a hotter fire, it only boiled at firſt, but ſoon after began, as I gueſſed it would, to make a crackling noiſe; wherein this was remarkable and pretty, that the exploſions were accompanied with flaſhes of fire and light, which, if they were ſmall, were generally very blue, like the flames of ſulphur, but more vivid, and ſometimes alſo more blue; but the greater cracks, whoſe noiſe was conſiderable, were wont to appear of a yellow colour and very luminous. And theſe phænomena did not only appear, whilſt the matter was boiling over the fire, but a pretty while after the veſſel was taken off and held in the air.

EXPERIMENT IV.

If before the coagulated matter were too far waſted by the heat, it were ſuffered to cool a little, it appeared to have acquired a conſiſtence like melted roſin, or rather ſtiff bird-lime, for it would draw out into threads of, perhaps, a foot or more in length; and having held one of theſe threads to the flame of a candle, it did not take fire, but melted into little globules, as capillary threads of glaſs are in like circumſtances wont to do. And having made ſome of them ſtick to the wick of a candle towards the bottom of the flame, they coloured the lower part of the flame quite round with a very fine blue, which laſted much longer than one would have expected.

EXPERIMENT V.

This glutinous ſubſtance had, by the action of the fire, acquired an odd kind of ſtrong ſmell, almoſt like that of garlick, and being left all night in the air, attracted to it (to uſe the vulgar phraſe) the moiſture of it exceeding faſt, being diſſolved in a good part into a liquor almoſt as ſtrong as ſpirit of ſalt.

EXPERIMENT VI.

Putting this ſubſtance again over the fire, as before, it appeared to be more fixt than one would have looked for; for though there were not ſo much as a ſpoonful of it, yet it continued boiling for a great while, and afforded a multitude of ſhining exploſions, whereof ſome made a conſiderable noiſe, and gave notable flaſhes of light, which ſeemed to be made by condenſed and agitated fumes, ſuppreſſed by the ſomewhat hardned ſurface of the matter, and kindled in their eruption into the air, into which ſome parts of theſe fumes, that were not kindled, eſcaped in the form of a ſmoke, whoſe ſmell was very ſtrong and rank, but of a peculiar kind. To which I ſhall add, what ſeemed ſtrange, that though oftentimes two, and ſometimes more flaſhes appeared at once, yet ſo ſmall a quantity of matter continued to afford them for almoſt an hour together, and probably would have done it longer, but that the late time of the night obliged me to go to bed, before the experiment was finiſhed.

What Liquors would, or would not, diſſolve the Icy Noctiluca.

Section V.

Among other ways of inveſtigating the nature of our icy phoſphorus, I thought fit to try, whether or no it would be diſſolved in ſome liquors of differing kinds, hoping, that if it would be ſo, in any of them, it might ſomewhat aſſiſt us to gueſs at its texture.

Experiment I.

We found then by trial, that common water would not, in the cold, diſſolve it, though the liquor was thereby impregnated; as when *crocus metellorum*, or glaſs of antimony, being infuſed in wine or water, the menſtruum will be impregnated by its emetic particles, and yet the bulk, ſhape and colour of the crocus, or the glaſs, will not thereby be viſibly diminiſhed or altered.

Experiment II.

Afterwards we put a grain, or two, of our lucid matter into a little urinous ſpirit of ſal armoniac, but it ſeemed not to make any conflict with it, nor manifeſtly to work upon it, though, to give the liquor time to make a ſolution, we left them together for ſeveral days. But, as ſoon as we had poured aſide the ſpirit, it appeared, that it had not, by any contrariety, deſtroyed the power of the noctiluca, which began readily to ſhine as formerly, and yet might be immediately ſuppreſſed again, by ſuffering the liquor to cover it as before; but when we had, by keeping the phial, for ſome time, in a moderate heat, impregnated the liquor with it, this liquor, being then dropt into water, had a like effect with that mentioned in the experiment of impregnated ſpirit of wine.

Experiment III.

Seeing a volatile and urinous ſalt would not work ſenſibly upon our phoſphorus, we thought fit to try, what corroſive liquors would do; and accordingly, we put a grain or two of our ſplendent matter into a very ſmall phial, wherein was a little oil of vitriol, that menſtruum appearing, in many caſes, more corroſive than other vulgar acids: but neither did this menſtruum diſſolve our icy noctiluca in the cold, and therefore putting it in ſome heat, we found, that though it did not manifeſtly diſſolve the ſhining matter, yet the warmed oil made it melt, and appear, at leaſt for the time, a fluid body; in which this ſeemed to be remarkable, that this ſo fugitive a ſubſtance, ſhould be ponderous enough to lye at the bottom of oil of vitriol, which is one of the heavieſt fluids we yet know, except quickſilver, which many will not allow to be a liquor. What we did with this melted noctiluca, was not unpleaſant to ſee, and will, God permitting, be hereafter mentioned.

Experiment IV.

Afterwards we put a ſmall fragment of our icy phoſphorus into aqua fortis; and though we kept it in that menſtruum two or three days, and ſet the phial, that contained them, for many hours, in a warm place, (the chimney corner) yet we found the matter ſo little altered, as to its viſible appearance, that we doubted, whether the liquor had diſſolved any ſenſible quantity of it.

Having tried ſaline menſtruums upon our icy phoſphorus, I thought fit to try oils, and alſo ſpirit of wine, that is reckoned by chymiſts to be of great affinity with them.

EXPERIMENT V.

WHEREUPON I put a little of our noctiluca into some oil of turpentine, which not dissolving it in the cold, the small phial, that contained it, was left all night in the chimney, upon warm ashes. But though, the next day, none of the phosphorus appeared any longer in the glass, yet we could not perceive, by two or three differing trials, that the oil was much altered by it, and particularly I observed, that though the glass were unstopt, and kept so for a while, yet the ingress of the air did not produce any sensible light, nor did we perceive the upper part of the glass to be full of white fumes, as is usual in divers other liquors impregnated with our noctiluca, when they are unstopt.

SECTION VI.

IT has rendered the experiments, made with the aerial noctiluca, much less acceptable, than otherwise they would have been, to the delicate sort of spectators, especially to ladies, that the light they produced was accompanied with a very unpleasant smell, that issued out of the phial, whenever it was unstopped to let in the air. But, by the help of our icy noctiluca, I found a way to prevent this ungrateful concomitant of our artificial light. But, not being discouraged by the bad success of the forementioned experiment, I hoped an aromatical oil might do, what subtil oils had not done.

EXPERIMENT I.

AND therefore having, in a very small phial, put about a grain of noctilucal matter, and covered it with as much pure essential oil of cinnamon, as would swim less than a finger's breadth above it, we carefully stopt our little phial, and having warily held the bottom of it against a fire, till the phosphorus began to melt, I suffered it to cool; and then unstopping it in a dark place, had the pleasure to see produced a vanishing indeed, but a vivid light. So that by this means, I could afterwards shew the production of light to the nearest persons of quality, not only without offending their noses, whilst their eyes were gratified, but with adding to the pleasure of a delightful apparition that of a fragrant smell. But because oil of cloves is more easy to be had good than the oil of cinnamon, and is also much cheaper, I tried the experiment more fully with that, and therefore shall proceed to give you (for an example of aromatic oils) the phænomena of it.

EXPERIMENT II.

WE put some of our luminous ice into a little pure oil of cloves, such as the chymists call essential, but found, after a considerable time, (no less than some days) that at least a good part of the matter was undissolved, but yet the liquor was richly impregnated by it, as we found by a pretty phænomenon that it afforded us. For the little phial it was kept in, being opened in a dark place, there immediately ensued a kind of flash of light, far more vivid, its small bulk considered, than any liquor had afforded us before. But the brightness of this apparition was, it seems, too great to be lasting; for this flame like substance, usually expired in less than a minute of an hour, sometimes perhaps in half that time. And there were two other circumstances particular enough in this phænomenon; one, that sometimes, especially if a candle were in the room, the shining fluid would appear of a pleasant, and somewhat surprizing blueish colour; and the other, that the light would cease, whilst yet there remained in the upper part of the glass, pretty store of whitish fumes, such as we have formerly often observed in the aerial noctiluca, to be the usual causes, or concomitant of light, as if, in our present case, the ſhining ſubſtance prey'd on, or reſided in, only the finer and more delicate particles of the whitiſh exhalations.

EXPERIMENT III.

INSTEAD of the oil of cloves, we ſubſtituted ſome chymical oil of mace, into which we put a competent quantity of the glacial phoſphorus: but though we warmed the bottom of the phial, at leaſt as much as we judged neceſſary, yet, upon the unſtopping of it, there appeared no ſign of light, though the trial was made much more than once or twice, and ſometimes with favourable circumſtances: which event was the leſs expected, becauſe the oil, made uſe of, was preſented me as very pure, by the ſame traveller, who gave me that of cloves newly mentioned; and becauſe alſo the warmed phoſphorus was ſo well conditioned, that as ſoon as ever the oil was removed, it ſhone with a ſomewhat extraordinary vividneſs.

EXPERIMENT IV.

WE made alſo a trial or two with diſtilled and fragrant oil of aniſeeds, to ſee, if that being an eſſential oil, as chymiſts ſpeak, and being looked upon, by many, as a kind of aromatic oil, it would better diſſolve the noctiluca, or be impregnated by its luciferous parts; but we found, that it neither diſſolved the matter, nor, upon the unſtopping of the phial, that contained it, did it afford any light, or ſo much as whitiſh fumes, which ſeemed ſomewhat ſtrange, becauſe the oil was very ſubtile, and, by its aptneſs to coagulate of itſelf, ſhewed, that it was genuine, and not as chymical oils, that are venal, too frequently (if not moſt commonly) are, ſophiſticated.

IF theſe two laſt recited experiments prove conſtant, they will argue, that not every fragrant, no, nor every aromatical oil, properly ſo called, has the like operation upon our noctilucal matter, as the oil of cloves and cinnamon have.

IF I had had, or could have procured, other eſſential oils, on whoſe genuineneſs I could have depended, I had tried their effects upon our phoſphorus.

EXPERIMENT V.

BUT, having no more oils fit for my turn, I next tried, whether I could diſſolve our ſhining matter in ardent ſpirits, which are thought, by chemiſts, to be of near conſanguinity with diſtilled oils; (not now to enquire, whether they do not conſiſt of the finer parts of the highly rarified oil of bodies, united with a great proportion of their phlegm) and, having accordingly, put ſome of our icy phoſphorus into the ſpirit of wine, though the menſtruum did, ſome hundreds of times, exceed the body it was to work upon, yet after divers days, wherein it ſtood in a window, expoſed to the ſun beams, in the hotteſt part of the ſummer, it appeared undiſſolved, at the bottom of the liquor, and ſcarce ſenſibly diminiſhed. But of the operation of ſpirit of wine upon the noctilucal matter, further trials will require, that more be ſaid hereafter.

Of a Way of ſuddenly producing LIGHT in common Water, by the Help of another, not luminous Liquor.

SECTION VII.

I COME now to recite to you a phænomenon, which, I preſume, may not diſpleaſe you. I had a hint of it from a caſual obſervation made by my induſtrious laborant. For having, to incourage him, allowed him, for his own uſe, ſome fragments of our icy

icy noctiluca, he mingled a portion of this ſhining ſubſtance with a ſpirituous medicinal liquor, that he had prepared, by extracting ſeveral drugs with it; and having afterwards, upon ſome occaſion or other, diluted it with water, it afforded him a phænomenon, at which being ſurprized, he came to acquaint me with it, bringing me withal ſome of the liquor. But I thinking, that the phænomenon did not depend upon the peculiar nature of the liquor, whoſe being very compounded and high coloured, made me judge it not fit for luciferous experiments, but proceeded from the vinous ſpirits, wherein that liquor abounded, I thought fit to make the experiment with a liquor, as colourleſs and ſimple as I could; the effects of ſuch liquors being more eaſy to be diſcerned, and judged of, and reaſoned upon. And accordingly we weighed out, in a tender balance, one grain of our glacial phoſphorus, wiped dry, and broken in four or five pieces, for the eaſier diſſolution. And to theſe, in a cryſtalline phial, we put a convenient quantity of highly rectified vinous ſpirit, and ſtopping the phial cloſe, we ſuffered the things contained in it to remain for many hours, ſometimes (and indeed for the moſt part) in the cold, and ſometimes in the warm ſun, but perceived not, that near a total diſſolution was made of the noctilucal matter by the liquor, in which it lay, even one of the leſſer fragments appearing, as well as the others, undiſſolved in the bottom of it. However, ſince a body, conſiſting of ſuch ſubtle parts, may communicate many of them to a contiguous liquor, without any diminution of its bulk, obſervable by the eye, I thought fit to try what effects this body had upon the vinous ſpirit.

OBSERVATION I.

AND firſt, I obſerved, that it did not manifeſtly diſcolour the liquor, but left it tranſparent and limpid, as before; ſave that there appeared ſome very ſmall earthy corpuſcles, like duſt, at the bottom of the liquor, when being a little ſhaken, (to raiſe them) it was attentively viewed.

OBSERVATION II.

WE did not obſerve, that, upon the unſtopping of the phial, and the reſtored commerce between the inward and outward air, there appeared any flame or luminous exhalations, as is uſual upon opening phials, that contain the liquid aerial noctiluca.

OBSERVATION III.

BUT the phænomenon I chiefly intended to relate was this, that, having in a dark night, dropt a little of this impregnated ſpirit into a ſmall china cup, with common water in it, though the ſpirit, neither in the phial, nor in its paſſage through the air, diſcloſed any degree of luminouſneſs, yet, as ſoon as ever the drops came to touch the liquor, they would be, as it were, kindled by the cold water, and afford little flaſhes of light, which was more vivid than the noctiluca itſelf, affording a ſplendor, that made not only the brims of the cup, but divers of the neighbouring objects manifeſtly viſible, not to ſay conſpicuous. But theſe coruſcations had the property of other lightning, to vaniſh almoſt as ſoon as they appeared, nor would the water that produced them, by being agitated, ſhine; but others might immediately be produced, by letting freſh drops fall into the ſame water, upon whoſe ſurface they ſeemed to diffuſe themſelves, and would ſometimes leave, for a little while, a faintly luminous, as it were, film or membrane.

OBSERVATION IV.

AND that it might not be thought, that this accenſion (if I may ſo call it) was produced, or occaſioned, by any antiperiſtaſis, which the ſchool-men, and the generality, even of philosophers, are pleased to fancy (whose opinion I have, in a particular discourse, examined) I thought fit to try, whether our phænomenon would not be produced with hot water, as well as with cold; and accordingly I found, that the impregnated spirit of wine produced rather a greater than a lesser light, in hot water, than it had done in cold.

ONE of my designs, I had in making this experiment, being to examine a conjecture I had made about the great diffusedness of the noctilucal matter, the subtilty of whose particles made me think they were not to be judged absent, where-ever they were not numerous, or agitated enough, to be of themselves visible: this, I say, being in my thoughts, I judged it not fit to put our splendent icy matter into the spirit of wine, at adventures. Wherefore, having in a very good balance weighed out one grain of our noctiluca, (first wiped dry) we put to it, at several times, that it might the better dissolve, about two thousand grains of spirit of wine, that would burn all away; and yet, which may seem strange, this small quantity of noctilucal matter did so impregnate all the liquor put upon it, that though nothing of luminous did appear in the menstruum, nor in any exhalations rising from it, though the phial were unstopped, or the liquor poured out of it into the air; yet, as soon as ever it was dropt into common water, there would be produced a vivid apparition of light, such as has been a little above described.

IT seemed not very improbable, that these sudden and vanishing flashes might, at least in great part, proceed from the quick disengagement and extrusion of the noctilucal particles, made by the water, which, diluting the vinous spirit, disabled them from retaining with them the luciferous corpuscles. As if into one ounce of high rectified spirit of wine, you put half a dram or a dram of camphire, the liquor will dissolve it, without being thereby manifestly altered, as to colour or transparency; but if you drop this solution into common water, the vinous spirits will immediately diffuse themselves into the liquor, and let go the corpuscles of the camphire, which will float like a white powder upon the su face of the water. To this conjecture is agreeable, what, upon trial, we observed with our impregnated spirit of wine, namely, that being dropt into other, well deflegmed spirit of wine, we saw no light produced; but when it was dropt into an urinous spirit of sal armoniac, which seems to consist of the volatile salt dissolved in the phlegm or aqueous liquor, the noctilucal corpuscles by this waterish part were freed from the vinous spirits, almost as much as they would have been by common water, and did accordingly shine with much briskness.

EXPERIMENTS discovering a strange Subtilty of Parts in the GLACIAL NOCTILUCA.

SECTION VIII.

BUT what has been above recited, is not all that I thought fit to try with the shining matter, that I told you we dissolved in spirit of wine; for after having, as I lately recited, brought one grain to impregnate between four and five ounces of alcohol, as the chymists call the high rectified spirit of wine, which did at least two thousand times exceed the weight of the noctilucal matter, I presumed, that this very parcel of spirit of wine, wherein the shining matter was already diffused and scattered into so many thousand corpuscles, as sufficed to impregnate all the liquor, would yet communicate

communicate to a good quantity of water particles enough to make it ſhine, when agitated: wherefore when we had weighed out in a very truſty balance one dram of our impregnated ſpirit of wine, we mixed it with, and ſhook it in as much fair water as we thought fit, (but not all at once) that is, till we had to our dram of ſpirit of wine put above fifty times its weight of water; and that alcohol itſelf weighing at leaſt two thouſand times as much as the noctilucal matter, that impregnated it, it follows, (though it may ſeem ſtrange it ſhould be true,) that the ſingle grain of icy noctiluca was able to diffuſe itſelf through, and impregnate full a hundred thouſand grains of liquor, ſo as (when duly ordered) to make it luminous. For having preſently after the laſt water was put into the glaſs, ſtopt the veſſel cloſe with a good cork, and ſhaken it a little in a dark place, the whole phial appeared to be full of light, which though it were not more than ordinary intenſe, yet, by reaſon of the bulk of the liquor, made a glorious ſhew, and diſcovered divers of the neighbouring objects. And after we had done ſhaking that phial, not only the upper part, which was filled with exhalations and vapours, ſhined like thoſe other liquid phoſphoruſes formerly mentioned, but what was not obſerved in them, the water itſelf had a luminouſneſs, though of an inferior degree, of its whole maſs; which yet will not keep me from thinking of ſome expedient, that may ſatiſfy thoſe, who may ſuſpect, as I did, that ſome of this light proceeded from the exhalations, that ſhined through that diaphanous water, though this did not ſeem the only, nor perhaps the chief cauſe of its appearing luminous, ſince when the glaſs was ſhaken, the whole maſs of the liquor appeared to ſhine, ſo that we could plainly ſee through the ſides of the veſſel the conical figure of its bottom.

AFTER this, I proſecuted the experiment a good way further, increaſing the proportion of the water to freſh impregnated ſpirit; and I found (what perhaps you will think ſtrange) that one part of the noctiluca, being firſt diſſolved in alcohol of wine, and afterwards briſkly ſhaken into a convenient quantity of water, rendered luminous as much liquor as upon calculation amounted to four hundred thouſand times its weight. And this did not ſeem to proceed from the irradiation of the luminous corpuſcles or exhalations, ſhining in the empty ſpace at the top of the glaſs; becauſe the phial was ſo near filled with liquor, that there was but little room left for vapours; and becauſe alſo the vapours, that did play in that ſpace, ſhined but very faintly, and when the glaſs was at reſt much leſs than a minute of an hour, the light would reach but a little way downwards in the water, and yet was there ſo dim, as to be ſcarce diſcernable. Whereas in our experiment, not only the agitated liquor appeared luminous throughout, but the light was briſk enough, inſomuch that the conical figure of the bottom of the glaſs was clearly viſible by help of it.

BUT leſt ſome ſhould think, that if this experiment had been further and further proſecuted, the luminouſneſs would have ſtill extended to greater and greater quantities of water, I ſhall add, that when I encreaſed the proportion of this liquor to the noctilucal matter, to be diſperſed through it, by putting in near three or four ounces of water more than I gueſſed would be convenient, the luminous matter ſeemed to be, as it were drowned or loſt in ſo much liquor; for though we gave it much more agitation, than had in the former experiments been needful to produce light, yet no luminouſneſs at all appeared in the mixture. Wherefore, taking ſome freſh ſpirit, and ſhaking it into ſuch a quantity of water as I thought it might ſerve to impregnate, I found by ſupputation, that the luminous maſs of liquor, thereby produced, amounted to no leſs, but a pretty deal more, than five hundred thouſand times the weight of the noctilucal matter diſperſed through it, which is a viſible expanſion, very much greater, than, I think, has been hitherto obſerved in any corporeal ſubſtance diſſolved in a viſible liquor, ſince it it four times exceeds that expanſion of cochineal, which I many years ſince imparted to the ingenious, and which ſeveral of them have in their writings, been pleaſed to take notice of, as a prodigious thing; one part of the cochineal, ordered as I there mention, having in that experiment produced a diſcernable colour in an hundred twenty-five thouſand parts of water. To what has been ſaid I ſhall add theſe three circumſtances, which may increaſe the ſtrangeneſs of the experiment.

1. THE firſt is, that I here only delivered the manifeſt impregnation of the water itſelf, which is a groſs and tangible liquor, by the diſperſed particles of the noctilucal matter, but have made no eſtimate of the incomparably greater expanſion of the light, that, from the matter included in the phial, illuminated the ambient air, to a conſiderable diſtance from it, though, by reaſon the darkened room was not great, I was diſabled to make an eſtimate, how far the enlightened ſphere of air might have extended.

2. THE ſecond is, that this experiment was not favourably made, but rather invidiouſly, ſince we purpoſely weighed out ſomewhat leſs of the lucid matter, and now and then more of the water, than the preciſe quantities that calculation ſuppoſes, that I might be ſure the experiment was made ſeverely enough.

3. AND laſtly, upon ſearch I found, that the grain of icy phoſphorus, that was firſt put into the ſpirit of wine, we made uſe of all this while, was not, though after ſo long a time, totally diſſolved; a ſmall fragment, amounting to about an eighth part, if not more, remaining at the bottom of the phial; upon which having poured ſome freſh alcohol of wine, and kept it a while in a little heat, to further the diſſolution, I found that liquor did, as I thought it would, grow very luminous, when dropt into common water; ſo that it ſeemed probable, that if the whole grain of icy phoſphorus had been diſſolved at firſt in the ſpirit of wine, it would have impregnated above ſix hundred thouſand times its weight of water, ſufficient to make it ſhine.

SECTION. IX.

I COME now to another way, by which I thought the great ſubtilty of parts in our noctilucal matter might appear with good advantage, and poſſibly you will think by the ſucceſs, that I miſſed not of what I expected from the intended trial.

EXPERIMENT I.

WE carefully weighed out a ſmall lump of our ſhining matter, amounting to three grains, and having purpoſely broken it into divers leſſer fragments, perhaps ſix or ſeven at leaſt, we laid them upon a flat bottomed glaſs, that was broader at the top than the bottom, and ſhallow too, (not being near an inch deep) that the matter might be more fully expoſed to the free air. This glaſs we placed in a ſouth window, laying it very ſhelving, that the liquor to be produced by its reſolution in the moiſt air might preſently run down, and not hinder the free evaporation of the remaining matter. In which poſture of the glaſs, we had alſo another aim, to be hereafter mentioned. The veſſel being thus placed, about ten of the clock at night, all the fragments of the noctiluca ſhined briſkly, and ſo continued to do, till moſt of them were reſolved into other ſubſtances, and the biggeſt of them continued to ſhine, till they were reduced to ſuch a ſmallneſs, that they would ſcarce have been ſeen, had not their own light made them viſible. BUT the main thing, that I am to take notice of in this experiment, (and which perhaps will ſomewhat ſurprize you,) is, that ſo little quantity of noctilucal matter continued to emit viſible fumes for a good many more than an hundred and fifty hours, and this with circumſtances, that made the thing more ſtrange.

1. As

1. As firſt, that this ſmoke was not only viſible but manifeſt, and that as well in the night (as I often obſerved from time to time) as in the day.

2. SECONDLY, that the ſeveral parcels of matter did each emit theſe fumes all at once, as if it were from ſo many little chimneys.

3. THIRDLY, that this ſmoke was ſo copious, and withal ſo tenacious, that it would eaſily retain the form of ſmoke at a conſiderable diſtance from the bodies that emitted it; ſo that, as I walked to and fro in the room, a careleſs look towards the glaſs would often diſcover it to me, and ſometimes it would manifeſtly appear at a diſtance, that I eſtimated to be near a foot from the matter that afforded it.

I ſhall now add, to another purpoſe, the following circumſtances; namely,

1. FIRST, that the motion of the ſmoke was ſwift enough, conſidering that it had no channel or chimney to aſſiſt it. It was not always, nor for the moſt part, directly upwards, but ſometimes horizontal, ſometimes downwards, ſometimes towards the right hand, and ſometimes towards the left, as if the motion of the fumes had been determined by the ſituation of thoſe parts of the noctilucal fragment, by which they were emitted, and as it were diſcharged. I uſe this laſt expreſſion, becauſe taking pleaſure to watch attentively the circumſtances of our delightful experiment, I thought, I many times obſerved a kind of palpitation or æſtuation in the little ſhining fragments, which I gathered from the apparent great inequality I perceived in the plenty of the ſmoke, that was emitted at ſeveral times, all of them perhaps within the compaſs of a minute or two. But on this I forbear to diſcourſe, till I ſhall have made further obſervations; and therefore I ſhall proceed to take notice of one circumſtance more in our experiment; which is,

2. SECONDLY, that even in the latter part of it, after the ſhining matter had been ſo long expoſed to the air, it emitted a ſmell ſtrong enough; which ſeemed to be cauſed by odorous exhalations, diſtinct from the viſible fumes.

SECTION. X.

AND now it is time, that I give you ſome account of the reaſon, (that I but pointed at before,) why I choſe ſuch a glaſs, and kept it in a ſhelving poſture: this I did, that I might not loſe, but preſerve the liquor, which I knew would fall from the ſaline part of the ſhining matter, which liquor I thought fit to examine, in order to diſcover certain things; particularly, whether the limpid water, as it appeared to be, that was, as it were, the cadaver, or, to employ chymical terms, the deliquated *Caput Mortuum* of the ſhining ſubſtance did not yet contain ſomething, as well luminous as ſaline.

EXPERIMENT I.

To ſatisfy myſelf about this, I cauſed this liquor to be put into a ſmall concave veſſel of carefully refined ſilver, (that other ſalts than nitre and allum might not corrode it) which I had purpoſely provided for the quick evaporation and cryſtallization of ſmaller quantities of matter. Our liquor, being in this veſſel put upon ſome ſmall coals, and aſhes, did not evaporate near ſo eaſily, as one would have thought, but turned into an unctuous ſubſtance of a dark reddiſh colour: wherefore we placed the veſſel upon quick coals, that by their briſk heat they might make the liquor boil, and free it from ſuperfluous moiſture. By this means, after a while, it reduced to a ſubſtance, that afforded us a pretty phænomena, not unlike to that elſewhere mentioned, where we ſpake of the infuſion or ſolution of the ſolid phoſphorus in common water. For the boiling liquor crackled like a handful of bay-ſalt caſt into the fire; and whilſt theſe cracklings continued, (which they did much longer than one would have expected) they imitated little volleys of ſhot, not only in the great number of the noiſes they made, but in the little flaſhes that accompanied them; which flaſhes, when the fire was ſomewhat increaſed, were ſo many, and followed one another ſo faſt, that they appeared to make up a continued flame, not unpleaſant to behold.

EXPERIMENT II.

AFTER the foregoing experiment, I had a mind to be able to make ſome eſtimate, how far the breaking of the ſhining matter into fragments, and the conditions of the veſſel, contributed to the quick conſumption of it. To this purpoſe, we took a lump of three grains, carefully weighed out, and put it into a ſmall glaſs funnel, whoſe upper end was wide and capacious enough, in reference to the lower part, which was exceeding ſlender, that the noctiluca might have air both above and (oftentimes) below, and yet the matter might not ſlide down, till it were ſo waſted as to be leſs than a ſmall pin's head. A veſſel of this ſhape I choſe to make uſe of, that I might catch the liquor, that would be afforded by the deliquation of our icy phoſphorus; for which purpoſe, the ſlender pipe of this funnel was put into the orifice of a ſmall cylindrical phial, and there kept in a quiet place, which was a ſouth window, from whence every night, after I was in bed, I cauſed it to be brought into my chamber, to ſee if it continued to ſhine. By which trials I found, that it remained luminous, and was not yet ſo waſted, to fall quite through the funnel in the phial, at ſomewhat beyond the end of the fifteenth day; ſo that it continued to ſhine three hundred and ſixty hours.

THE very limpid liquor, that was brought into the phial by this experiment, was unhappily loſt before I could make any trials with it, but not before I had done the chief thing I aimed at, in ſaving of it, which was to know its weight to be by and by mentioned.

WHAT has been hitherto related may juſtly enough make a man reflect, with ſome wonder, upon the ſtrange minuteneſs and multitude of parts, that are crouded together in our noctilucal matter; if we conſider what a multitude of luminous beams of viſible ſmoke, of odorable, though unſeen, effluviums ſo ſmall a quantity of it, as three grains, which are but the twentieth part of a drachm, could inceſſantly afford for two or three hundred hours; leaving, after all this, behind it above three times its weight, (for ſo we found it to be) of a liquor, which itſelf was not a cadaverous one, or, what it looked like, common water, but (as may be argued from what was lately recited of the ſame kind of liquor) might have been impregnated with very many ſaline parts, and not a few capable of ſhining briſkly.

OBSERVATIONS about the INFLAMMABILITY of the NOCTILUCA itſelf.

SECTION XI.

OBSERVATION I.

I TOOK a little of the conſiſtent noctiluca, and having broken it, and, as its britteneſs would permit, ſpread it here and there, upon a piece of folded paper, I lighted that paper at the flame of a candle, and obſerved, that when the flame reached this or that little fragment of the ſhining matter, it would take fire, and burn away in

3 a flaſh-

a flashing and very sputtering manner, accompanied with noise, almost as grains of salt-petre are wont to do, when they are put upon a live coal.

OBSERVATION II.

I observed also, that if I put pieces of paper, on which I had placed some of these grains of noctilucal matter, upon some embers covered with ashes, before the paper itself took fire, the shining matter would communicate its flame to the contiguous paper.

EXPERIMENT I.

WE took a fragment of our shining matter, not amounting to a grain: this we put into half a spoonful or less of high rectified spirit of wine, and kindling that liquor with the flame of a candle, the spirit burned away, as is usual, in a flame partly yellow, (and especially at the outside) but chiefly blue. But though the heat of the silver spoon, wherein the trial was made, did quickly (as might well be expected) melt the noctilucal matter, and gave it a globular form, yet it continued at the bottom without manifestly mixing with the vinous spirits, or considerably altering the colour of their flame: but when the spirit of wine was all consumed, without leaving any jot of phlegm behind it, the last drops coming, when they were actually kindled, to touch the shining matter, presently set it a fire, but its flame was very differing from that of the vinous spirits. For besides somewhat, that was odd in its figure, its colour was not at all blue, or bluish, but of an intense yellow, and burned so fiercely, and with so vivid a light, as was somewhat surprising to behold, and continued to burn a pretty while, considering the paucity of the combustible matter. And whilst it burned it emitted good store of smoke, that seemed to be darted up to a considerable height. The matter did not burn all away at first, but left a kind of *Caput Mortuum*, which lay in the form of a little cake, partly of a deep yellow, and partly of a fine red. This matter being more bulky in proportion to that, that was consumed, than I thought it likely that so little of the phosphorus should contain of incombustible matter; I proceeded to burn it, as elsewhere will be related after another manner, till there remained but some very few light feces, that seemed to be of the same nature with those, that are to be mentioned in the next experiment.

EXPERIMENT II.

WE took a small fragment, not amounting to a grain of the noctilucal matter, and putting it into a silver spoon, we cast upon it the sun beams concentrated by our smallest (dioptrical) burning glass: by these it was presently set on fire, and afforded, together with a great deal of smoke, a flame exceeding yellow, and so very fierce and bright, that it was conspicuous, though the window being purposely set open, the beams of the sun, then in the meridian, were suffered to beat full upon it, and a brisk wind did also blow upon it without extinguishing it. At the bottom of the spoon, the expiring flame left a round and broad *Caput Mortuum*, consisting of divers circles, like those of a sardonix, whereof the largest was white, another yellow, and the third red, all the three colours being pleasant and vivid enough. Some part of this *Caput Mortuum*, being again brought to be freely touched by the air, appeared combustible, and the rest being left, in the spoon, that the air might work upon it, did for the most part soon resolve itself *per deliquium* into a liquor, almost as sharp as spirit of salt, the rest being a light black feculency, of which, because there was so very little of it, we could make no examen.

EXPERIMENT III.

BEING defirous to try, whether the noctilucal matter would, by bare preffure or motion, be brought to burn, I thought not fit to depend upon fuch other experiments, as are here related, wherein it appears able to fet fire on divers bodies, belonging to the vegetable kingdom, or are otherwife eafily inflamable: wherefore we put two grains of our dried noctilucal matter into a glafs mortar (furnifhed with a glafs peftle) whofe coldnefs and thicknefs were able to keep it from being put into any fenfible heat by the operation, that was to be performed in it, and confequently from communicating any heat of its own to the noctilucal matter. This was pretty brifkly rubbed in the mortar, with the glafs peftle; but though it was thereby brought to fhine much more vividly than before, yet it did not take fire; which I was apt to impute to the great coldnefs of the glafs, which much oppofed the incalefcence of the phofphorus; but after a while longer it took fire, and began to burn away in an actual flame, with much fmoke: but this did not laft fo long, as might have been expected, which fhort duration might proceed from the veffel, that continued fenfibly cold, and perhaps alfo from the narrownefs and depth of it, which fomewhat hindred the free accefs of the air; for fome matter, that was taken out on the peftle, feemed to burn better, than that which remained in the mortar, which being extinguifhed, was once more kindled by trituration, but foon expiring again, could not by the fame means be rekindled, but only was brought to fhine brifkly.

EXPERIMENTS about burning other BODIES with the NOCTILUCA.

SECTION. XII.

I Formerly intimated, that our confiftent noctiluca, notwithftanding the appearannce it had of ice, and its actual coldnefs to the touch, was much difpofed to have its parts eafily put into motion, and might by that means be brought to be fenfibly hot. And I think it time now to proceed to make this good by particular inftances.

1. AND firft, if our phofphorus be for any time preffed hard between ones fingers, or againft a board, or fome fuch hard and not very cold body, it will oftentimes be felt actually and very fenfibly hot, and fometimes the degree of heat will be fo vehement as to fcorch the fkin, as my venturous laborant found feveral times, to his no fmall pain, his fingers being almoft covered with blifters raifed on them, by handling our fhining matter with too bold a curiofity: and he complained to me, that, though according to the ufual fate of chymifts, he had been often burned on other occafions, yet he found blifters, excited by the phofphorus, more painful than others; and he is not the only perfon, that has complained to me of their finding the burning made with this matter to be more tedious and difficult to be cured, than ordinary ones. But, as our noctiluca was not always made of the fame matter, nor with care equally fuccefsful, fo I obferved its pronenefs to incalefcence, and the degrees of heat, to which it would be brought by motion, to be differing enough; upon which account, I did not find, that fome portions of it would produce thofe higher effects of heat, that fome others did, befides that thefe higher effects did gradually differ among themfelves.

2. AGREEABLY to this, after having in vain tried to fire paper by preffing and rubbing fome of our phofphorus upon it with the blade of a knife, I took a piece of fine paper,

paper, and having dried and warmed it at the fire, I put a little of our noctiluca in a fold of it, and rubbing the paper between my hands, though by that attrition there were produced a ſenſible, and even conſiderable heat, yet it did not reach to what I deſired; but continuing a little while to rub the paper to and fro, it did on a ſudden take fire, and blazed out, ſo that it would have burned my hand, if this had not been kept from receiving much harm (for all it did not eſcape) by a thin glove, that was thereby ſcorched, and in part ſhrivelled up. After the ſame manner, to make the experiment the more certain, I fired another piece of paper, but then deſiſted, that I might not unneceſſarily waſte a ſubſtance, wherewith I was but meanly ſtored.

3. IF the firing of gun powder could be performed with our phoſphorus, without the aſſiſtance of circumſtances, whoſe difficult concourſe will keep it from being more than an inſtructive curioſity; the fear, that it might be applied to uſes miſchievous to men, would keep me ſilent of the power our phoſphorus may be brought to have of kindling gun-powder, when it is befriended with favourable circumſtances. To try therefore, whether our phoſphorus, which appeared not inferior to that of Mr. *Craft*'s would (as his did ſometimes, though not eaſily) fire gun-powder, we took a little of our ſhining matter, and having a little wiped it, to dry it, we put it upon ſome dry gun-powder, and with a knife preſſed it, and in ſome ſort rubbed it upon the black grains, but found, that though a heat were produced, and ſometimes ſuch as would make ſome of the corns of powder have a bluiſh flame, yet the mixture would not go off: ſo that the laborant, to whom I left the care of reiterating the experiment in my preſence, preſuming it would not ſucceed, ſcrupled not to hold his head over it, that he might the better ſee what change was made in the mixture; but then upon a ſudden the powder took fire, and the flame ſhooting up, caught hold of his hair, which made a blaze, that proving innocent enough, became more diverting than the ſmell of the ſmoke, that ſucceeded it, was delightful.

4. BUT the ſame laborant, who was very helpful to me in varying the preparation of the phoſphorus, had a worſe miſ-adventure not long after; for bringing me ſome newly diſtilled grains of our noctiluca, covered with ſome of the ſhining water, that came over with it, he unluckily broke the glaſs in his pocket; whereupon the heat of his body, increaſed by the motion his long walk had put it into, did ſo excite the matter, that was fallen out of the broken phial, that it burned two or three great holes in his breeches, before he could come to me to relate his misfortune, the recent effects of which I could not look upon without ſome wonder as well as ſmiles.

5. HAVING already told you the effect of our noctiluca upon gun-powder; I thought fit to try, whether it would not kindle a body, that is thought ſomewhat leſs prone to take fire. And accordingly having put together about half a grain of our dry noctilucal matter, and ſix times its weight of common flowers of ſulphur, they were lodged in the fold of a piece of white paper, which was laid upon a board, and when I had a little bruiſed and rubbed this with the haft of a knife, it ſhone through the intercepted paper very vividly, but did no more: wherefore ſuſpecting, that the want of air was the cauſe why it did but ſhine, not burn too, I opened the paper, and found, that as ſoon as the air had acceſs, it took fire and furiouſly burned the paper, and, if I had not been wary, had burned me too. Another time, the ſame experiment being tried, afforded this notable phænomenon, that the ingredients, being well rubbed together in folded paper, though before the paper was diſplaid and expoſed to the air, they did not kindle, yet upon the contact of this the mixture took fire, and did not burn away with a ſlow flame, as brimſtone is wont to do, but flaſhed away at once with a great blaze, like fired gun-powder, ſave that the flame appeared more luminous.

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6. THE higheſt effect of the heat of our icy noctiluca was caſually produced by the laborant, who being deſirous to try, whether ſome that was newly prepared was good and fit to be brought to me, began to write letters with it upon a piece of plank, that had been long uſed in the laboratory as part of a ſtove; and he, chancing to preſs the recent matter hard upon this board, that the conſtant heat of the place had brought to an unuſual degree of dryneſs, found to his ſurprize, that he had not only ſhining but burning letters; the lucid matter having actually ſet on fire thoſe parts of the wood, againſt which he had ſtrongly preſſed it.

SECTION. XIII.

EXPERIMENT I.

TO examine ſomewhat particularly, to what family, or ſort of ſalts, the ſaline part of our noctilucal matter either does belong, or has moſt cognation with (for I thought it poſſible it might not fully agree with any known ſpecies of ſalts, but have ſomewhat peculiar to itſelf) I ſuffered a little of the ſmall ſtock, I then had, to reſolve itſelf *per deliquium* into a clear liquor, and then made with it ſome of the trials elſewhere delivered, by which I am wont to examine what ſpecies a ſalt belongs to, gueſſing this liquor by the taſte, and the manner how it was made to be ſomewhat, though not altogether, of the nature of ſpirit of ſea-ſalt: I dropt a little of it upon a convenient proportion of ſyrup of violets, and found, that it turned it not green, as a volatile urinous ſalt would have done, but of a fine carnation colour, ſuch as that ſyrup is wont to acquire, upon the mixture of an acid ſpirit with it. I found alſo, that a very little of our anomalous liquor preſently deſtroyed the blue colour, and not the other of a tincture of *Lignum Nephriticum.*

EXPERIMENT II.

I alſo put ſome of this liquor, that came by deliquium from the noctiluca, upon ſome filings of copper, which being thoroughly wetted, and ſome of them covered with it, I expoſed in a hollow glaſs for two or three days to the air; and by this means had, as I expected, without the help of heat, a ſolution of ſome of the filings of copper, the colour of which was not a deep azure, as if it had been made with a volatile urinous ſalt; but ſeemed to partake of green and blue, and to be an intermediate or compounded colour.

EXPERIMENT III.

To make the ſaline nature of this liquor the more manifeſt, I put ſome of it upon powder of red coral, which it preſently fell upon, and corroded with noiſe and froth; and putting another parcel of the ſame liquor upon ſome dry ſalt of tartar, there preſently enſued a fierce conflict between them, whereby ſome noiſe and much froth was produced; ſo that I thought it needleſs to waſte any more of the noctilucal matter, (wherewith I was but ſlenderly ſtored) to make it more apparent, that our liquor was not, as moſt chymiſts would have expected, of an urinous nature, but belonged to the family of acid ſalts, and ſeemed to be near of kin to that branch of them, to which the ſpirituous part of common or ſea-ſalt belongs.

EXPERIMENT IV.

SOME virtuoſi may be apt to think, that ſince our icy noctiluca is of a more ſolid ſubſtance than the aerial, and uſes to continue to ſhine much longer; ſince, I ſay, this is ſo, if the conſiſtent phoſphorus were included in a glaſs, whence its expirations could have

have no vent, the matter being kept from wafting, the luminoufnefs may alfo be kept from ceafing. This conjecture being plaufible, though the notion I have of the nature of our noctiluca could not promife me a confirmation of the conjecture, yet, to prevent the being blamed for an eafily evitable omiffion, I put fome of our dry phofphorus into a clear phial, capable, as was gueffed, to hold about an ounce of water, and having very carefully clofed this glafs, I laid it afide, obferved it to continue to fhine for fome few days; after which the light manifeftly decayed and foon after quite difappeared, though I thought it poffible, that it did not altogether fo foon expire, as it ceafed to be vifible to me; becaufe the whitifh fumes, emitted by the matter whilft it continued to fhine, had covered the infide of the glafs with a kind of whitifh foot, that, at length opacating it, might well hinder a faint light from pervading the veffel, and reaching our eyes. But it feems, that the air included with the phofphorus, either had fome vital fubftance (if I may fo call it) preyed upon thereby, or elfe was by the fumes of the phofphorus, to name no other poffible ways, tamed and rendered at length unfit to continue the flame [*fui generis*] of our noctiluca.

EXPERIMENT V.

YET to purfue the defign of making a light more lafting than ordinary, by keeping the matter from commerce with the external air, I took fome of our noctilucal matter, that came over with the aqueous, from which it was not fo eafily feparable, but that I thought it beft to leave them together, in regard that it fhone fo well, that it might pafs for an excellent portion of the aerial noctiluca: this we fealed up in a glafs egg (whofe bottom had been made flat on purpofe, that it might ftand without leaning) and fetting it in a place, where it would be frequently in my eyes I obferved it from time to time, efpecially at night, and found it continue to fhine (if I much mif-remember not) a week or longer, and that with fo little decay of light, that I was furprized, when, coming in the night time to look upon it, I found it to fhine no more at all, efpecially fince I could not reftore any manifeft light to it, either by agitation, or by moderately warming the fealed glafs, that contained it.

EXPERIMENT VI.

AFTER many obfervations made of the degrees of light, that our icy noctiluca afforded, as it were of its own accord, without external heat, I thought fit to try, whether by the application of a moderate heat of the fire, the light might not be much invigorated, and perhaps the phofphorus itfelf be brought actually to kindle, even in a clofe veffel. This defign I was the rather induced to profecute, becaufe I had fome hopes, that by this way of increafing the light of our phofphorus, though it fhould not long retain its acquired degree of luminoufnefs, yet this increafe might continue long enough for fome not inconfiderable ufes. And efpecially (in cafe much noctilucal matter were heated at once) to give light enough for taking of gun-powder out of the gun-room of a fhip, or out of a magazine, without danger of firing the powder, which would be a means to prevent thofe fad accidents, that have but too frequently happened to fhips, efpecially of war, of which we had very lately a notable inftance in the river of *Thames*. In profecution of this defign, we took fome grains of our confiftent phofphorus, and having put them into a round glafs egg, fomewhat larger than an ordinary hen egg, fitted with a ftem of a proportionable bignefs, and about two thirds of a foot long. This being hermetically fealed up, (at leaft as far as we difcerned) the globulous part of it was warily and by degrees warmed at the fire, and then we inftantly removed it into a dark place, where the included matter not only fhone by great odds more vividly than before it was heated, but fome portions of it were brought to an actual flame, as appeared both by the radiant ſplendor of the burning matter, and by the condition of the ſmoke it emitted; and yet more manifeſtly, by the intenſe heat, which the flaming part of the matter (and not the other parts) communicated to that part of the glaſs, which it adhered to; for there the veſſel was not to be ſo much as touched without inconvenience: and when this flame expired, which it did after no long time, the portion of the lately kindled matter did no more ſhine or burn as before, but was reduced to the condition of the reſt of the noctilucal matter, together with which it did for a good while retain a conſiderable degree of light, upon the account of the heat it had been expoſed to, over and above that luminouſneſs, that ordinarily belonged to it.

THIS experiment appeared ſo ſtrange, and was ſo delightful to thoſe, that had never ſeen it, that partly to gratify the curious, and partly to purſue my own deſign, it was reiterated, within the compaſs of a month or two, between (if I miſtake not) twenty and thirty times, the ſame matter being ſtill kept in the ſame veſſel, though by being melted, and in great part ſublimed by its frequent approaches to the fire, it was divided into ſeveral parcels. But this made the experiment ſo much the more pleaſant, in regard, that ſometimes (for it was not always) more than one or perhaps than two portions of the matter would ſeem to burn at once. This was looked upon as a very new and ſcarce credible thing, that one ſhould be able to bring a body to burn with an actual flame, and for no inconſiderable time, in a glaſs hermetically ſealed, and not large neither. But to deal with philoſophical ſincerity, I muſt not conceal from you, that after we had made many trials in the abovementioned glaſs, there happened a phænomenon, which gave me ſome ſuſpicion, that at that time it was not actually ſealed: but it did not appear, but that it had been very well ſcaled at firſt, and might continue ſo during ſeveral trials; for after this ſuſpicion we uſed this glaſs ten or twelve times, or perhaps oftner, to make the before recited experiment, and after all thoſe we could perceive no crack or flaw at all in the ball or ſtem of the glaſs, and found it difficult to get in the point of a ſmall pin into a little hole, which we either found, or, by endeavouring to find one, made at the apex. However, by the things formerly related, it appeared, that our noctilucal matter would burn with leſs vent by great odds, than other fewels known to us; and that a ſmall quantity may be made to burn and ſhine longer, than one would expect. And we were encouraged by what we ſaw, to hope, that if a more conſiderable quantity of matter were put into a convenient ſhaped glaſs, and aſſiſted with other friendly circumſtances, eſpecially if the luminouſneſs could be a little heightened, it may be rendered fit to be of ſome uſe in ſhips and magazines of powder.

IF I had been furniſhed with accommodations, when I firſt made the foregoing experiment, I would have purſued the trial ſomewhat further, by making a pretty quantity of our noctilucal matter burn ſeveral times, in a thin glaſs veſſel exquiſitely cloſed with *Hermes*'s ſeal, that, by weighing the veſſel in exact ſcales, both before and after the accenſion of the included phoſphorus, I might find, whether any ponderable parts were ſubtile enough to pervade the pores of the glaſs; and in caſe they were not, I then hoped to diſcover, what change of texture might be made in the matter of a body, reduced to an actual flame, in a veſſel, wherein it could not receive the free air, nor emit any fumes or exhalations, which would have been to me a very acceptable experiment; and perhaps would have proved a very inſtructive one too. Since, as I have in another place complained, in the analyſes hitherto made by chymiſts, either the body expoſed to the fire has not been actually inflamed, which is the caſe of thoſe diſtilled in exactly cloſed veſſels, or elſe there has been ſome commerce betwixt them and the external air, which may juſtly render

ender it doubtful, whether the bodies produced by this analyſis were the ſame both for number, nature, &c. that would have been produced in veſſels, exquiſitely cloſed; ſince we ſee, that wood, for inſtance, burned in a chimney, affords ſtore of ſoot and aſhes, which are very differing bodies from thoſe, that chymiſts obtain from the ſame wood diſtilled in cloſe veſſels.

BUT to trouble you no further with what I would have done, I ſhall add one circumſtance I obſerved in what was done. Namely, that ſometimes there appeared a little liquor in the glaſs (whether it conſiſted of ſome aqueous particles, that may be ſuſpected to have lain hid in the noctilucal matter, or were produced by the actual deflagration of a part of the matter) and the reſt of the matter, by the reiterated operations of the fire, was turned to a red colour, which it yet retains.

SECTION XIV.

EXPERIMENT I.

I HAVE formerly related, that, upon the immerſion of our phoſphorus into water, it would immediately ceaſe ſhining, and continue without light as long as it was kept under that liquor. This gave me ground to ſuppoſe, that, by the interpoſition of water between the noctilucal matter and the air, the phoſphorus may be kept unactive, till it be fitted, by an extraordinary agitation of its parts, to act with an unwonted vigour, when the air ſhall come to touch it ſuddenly: this ſuppoſition, I ſay, induced me to put two grains of our icy noctiluca into a ſmall glaſs egg, and pour a pretty quantity of water on it. In order to the following experiment, we heated the liquor well, yet without making it at all boil, and thereby melted the little fragments of ſolid matter, and made them flow into one liquid maſs, that kept itſelf at the bottom, diſtinct from the water: this done, we preſently removed the glaſs into a dark place, and pouring out the water, we obſerved, that, as ſoon as the air came to touch the noctilucal matter, it ſeemed to be kindled into an actual flame, that afforded a very vivid light; which ſucceſs pleaſed me the better, becauſe it ſhewed, that a kind of fire may be kept under water, as long as one pleaſes, without ſenſibly burning, and yet, in a moment, upon the bare removal of the water, ſhew itſelf in the form of actual fire. That our ſhining ſubſtance was of this nature, appeared manifeſtly by this, that the water, being poured out ſomewhat too haſtily, carried along with it, which I did not intend it ſhould do, the whole maſs of the noctilucal matter; and this by its fall into the ſilver cup, that was employed to receive the liquor, was divided into two or three parts, which coming to a more free, or full contact of the air, blazed out much more than when they were in the glaſs, and afforded us a delightful ſpectacle, ſince the flame burned upon the water, with much light and fierceneſs, and a ſtrange deal of ſmoke, and it did, ever and anon, ſputter with noiſe, like ſalt-petre made to burn upon a live coal. Theſe flames continued the pleaſure we had to ſee them burn upon the water a pretty while; and after their extinction, looking into the ſilver cup, we found divers flakes of a reddiſh matter, (which the chymiſts would call a *caput mortuum*) that lay at the bottom of the water; and the ſides of the ſilver cup, that were next to that liquor, looked almoſt as if fine brick-duſt had been ſtrewed upon them.

EXPERIMENT II.

BEING deſirous to ſee, whether our noctilucal matter ſhining through a coloured glaſs, the beams of light would be tinged in their paſſage, we took two or three grains of our matter, and put it into a phial, of an almoſt ſpherical figure, capable of holding, by our eſtimate, about twelve ounces of water; which phial was made of fine glaſs, of a very pleaſant colour, participating of thoſe, that are called orange and aurora: but the lucid matter being ſhut up in this phial, and carried into a dark room, did not appear through the glaſs to be conſiderably altered in colour; which becauſe I imputed partly to the ſmallneſs of the fragment of the phoſphorus, in reference to the capacity of the veſſel, through which it would give no more than a faint light, I cauſed the glaſs to be conſiderably heated, and then brought it into the dark room I ſtayed for it in: there, as ſoon as it was come, the included matter ſeemed to be actually flaming, and the trajected beams of light appeared of an unuſual and glorious colour, the light being ſo conſiderable, that it made divers bodies diſtinctly viſible, at a pretty diſtance from the glaſs; and we judged that, by the help of it, a book of a good print might have been eaſily read: but this light, which was the greateſt we had till then, produced with our phoſphorus, did not laſt long in its vigour, but, in a ſhort time, gradually decayed, till it came to little more than the uſual ſplendor of the noctilucal matter.

EXPERIMENT III.

I formerly related, that I could not make ſuch an experiment, as I ſucceſsfully tryed with the oil of cinnamon and the oil of cloves, to ſucceed with the oil of mace. But now I muſt add, that the little phial, wherein the noctilucal matter, and that oil were included, having been ſet aſide as uſeleſs, I afterwards chanced to caſt my eyes on it, and to have the curioſity to try, whether or no the unſucceſsful experiment, I had made before, were not one of that kind, which, in another paper, I have diſcourſed of, under the name of contingent ones; and accordingly, there being a ſomewhat dark corner in the room, I carried the phial thither, and although it were yet broad day, I unſtopt it there, and was ſomewhat ſurpriſed to find the included matter to afford immediately a vigorous light, which put me afterwards upon repeating the experiment, at different times, which I did with the like ſucceſs, without being able to determine the cauſe of this odd phænomenon.

EXPERIMENT IV.

ONE experiment I ſhall now relate, which though (becauſe it ſeems, as well as the laſt recited, a contingent one,) I forbore to ſet down with the reſt, will perhaps be thought more ſingular than any of them.

WE had, in one of our receivers, that was but ſmall, (ſince it was not judged capable of containing a gallon of water) a parcel of our conſiſtent noctiluca, in which my laborant told me, that he had met with a phænomenon, that to him, who knew nothing of what is related *ſect.* XIV. *exper.* I. was very ſuprizing, and ſeemed to appear by chance, ſince he often tried, in vain, to produce it when he pleaſed. This receiver I took into my cuſtody, and pouring out the common water, with which the ſplendid matter was kept covered to hinder it from ſteaming away, we obſerved no other change, than that, upon the removal of the water, and the contact of the air, the noctiluca would immediately ſhine, and continue to do ſo, till we thought fit to extinguiſh it *pro tempore*, by pouring water on it again.

THIS being done in the morning, I conſidered the following night, that this receiver having been kept in the laboratory, which conſtant, and ſometimes vehement fires, made a very warm place, it was but fit, in order to make the trial a fair one, to bring the ſhining matter to as great a warmth as it had in the laboratory, where it exhibited the phænomenon I was deſirous to ſee. Having then cauſed the receiver, with the water in it, to be held in a hot place, till the liquor had attained, by our gueſs, a fit degree of tepidity, we poured out the water, and, within a minute or two after, by our eſtimate, we had the pleaſure to ſee, that the conſiſtent matter, notwithſtanding the wetnefs, that in probability the water had left on it; we obferved, I fay, that this wet matter, upon the contact of the air, took fire of itfelf, not without noife, and burnt with a manifeft and actual flame. But our pleafure was fomewhat moderated, though the experiment was the more afcertained, by this accident; that, before we could pour in water to quench the fire, the violence of the flame had broken the receiver, which was thick enough, and thrown off a piece above half as broad as the palm of one's hand; by which unlucky chance we were hindred from endeavouring to find, as we intended to do, whether we could, by repeated trials, difcover the caufe of the appearing contingency of this odd phænomenon, which had, far oftner in vain than fuccefsfully, been endeavoured to be produced.

THIS experiment recalls into my memory a notable phænomenon, belonging to that formerly recited (*fect.* XI. *exper.* 2.) about the kindling of our phofphorus with the fun-beams, united by a burning-glafs: for whereas I there mention, that the noctilucal matter did not burn all away at firft, but left a kind of *caput mortuum*, which lay in the form of a cake varioufly coloured, I fhall now add, that fo much matter could not be left unfired, unlefs fomething hindered its accenfion; we warily turned over the little cake, with the point of a fharp knife, and then the under part, being I prefumed hot, prefently took fire upon the contact of the air, and flamed away, till the matter was almoft totally confumed.

The CONCLUSION.

AND now I have acquainted you with all the chief things, that I have hitherto been able to try or obferve, about our icy noctiluca, or folid phofphorus: and though I have been obliged to deliver them without any exact method, yet perhaps their novelty will ferve to make them acceptable to you. Light is fo noble a thing, that the matter our phofphorus affords it to refide in being endued with fome uncommon qualities, and particularly with a ftrange, and almoft incredible fubtilty of parts, I cannot but hope, that, if improvements, upon fuch a matter, were more induftrioufly attempted, by perfons better qualified for fuch a work, than I (efpecially in my prefent circumftances) pretend to be, fomething would be produced, tending to the difcovery of the nature, not only of light, but divers other bodies, and perhaps alfo, of good ufe to human life. If fome unwelcome circumftances did not, for the prefent, difcourage me, I would contribute my weak endeavours towards fuch a defign. For, fometimes I think a naturalift's pen ought to be like a merchant's fhip, that comes from time to time into port to reft, but not always to ftay there, but to take in new lading, and refit itfelf for a new voyage to the fame, or other parts. In the mean time, I recommend this fubject to yourfelf, and thofe excellent virtuofi, you hold correfpondence with, whofe ingenious attempts to advance true philofophy will have, for their good fuccefs, the hearty wifhes of

Your moft affectionate, and moft humble fervant,

R. B.

A CHYMICAL PARADOX, grounded upon New Experiments, making it probable, that Chymical Principles are transmutable; so that, out of one of them, others may be produced.

An ADVERTISEMENT of the Publisher to the Reader.

THE following paradox, having been written in or before the year 1680, was kept in the author's hands, that it might come out with the Latin version of his treatise, intitled, *The produciblenefs of chymical principles*, which is annexed to the second edition of his *Sceptical Chymist*, printed 1680; but some unlucky accidents having kept that translation from being finished, the author thought fit the ensuing paper should accompany his icy noctiluca, both in English and in Latin: upon this account, he sent me not only the discourse, that now comes forth, but some other papers, containing the minutes taken from time to time by his laborant, of what occured in the long train of distillations, on which the following reflections are grounded. For, the reader, whether foreign or domestic, may here be pleased to be advertised, once for all, that as the author hath been pleased to publish all his works in the English tongue, for reasons best known to himself; so the province of the translating them into Latin, hath been undertaken by others: for, indeed, his assiduity and diligent attendance on his daily and growing experiments will not allow him leisure or opportunity to undertake that work himself; though otherwise, if he had a desire to polish any thing in that tongue, his pen can command a fluent stile. This by the by. But I having returned to the author, both his own papers, and my version of them, in one roll, it unfortunately happened, that before the icy noctiluca was printed off, there broke out, in the night, a great fire, not far from the author's lodging, which was so threatning, even after the blowing up of several houses to stop it, that, as many others were obliged hastily to remove their goods, our author thought fit, by the same way, to endeavour to secure his own manuscripts; but did it not so succefsfully, but that some are yet missing; and, among others, the English and Latin papers lately mentioned. Notwithstanding which, the importance of the subject, and the novelty of the experiments, prevailed with the author, (to prevent the like mischance from happening to what he could retrieve concerning them) to communicate them to the curious, who will, by what I have here related, be enabled to understand what he writes at the beginning of that part of the following paper, which, because it was written after the fire abovementioned, and very long after the rest, he calls his postscript.

A CHYMICAL PARADOX.

I Adventured many years ago, in the *Sceptical Chymist*, and long after in other papers, to lay down some reasons of questioning, whether the fire be the true and proper instrument of analyzing mixed bodies, and do but dissociate their principles or ingredients, without altering them or compounding them anew. But I shall now present you a discovery, that will perhaps make you think the vulgar opinion of chymists to be less fit to be doubted of, than rejected. The occasion of making the following experiments was afforded me by the complaint of an ingenious chymist, (and great distiller) who told me, that endeavouring to purify an essential oil by rectification, he found

found to his diſappointment, that he diſtilled it four or five times ſucceſſively, yet it ſtill left ſome fæces, (but much leſs than at the firſt) though he concluded, that if he ſhould undergo the trouble of diſtilling the liquor a few times more, it would come over perfectly pure, without leaving any fæculency behind it. But it was more congruous to my hypotheſis, to conjecture, that the *caput mortuum* he complained of, was not, (at leaſt after the firſt or ſecond diſtillation,) a more groſs or fæculent part of the oil, ſeparated from the more pure, but a new compound produced, as other concretes alſo might be, by the operation of the fire. This conjecture of mine was favoured by ſome experiments I had made many years before, and imparted to ſome inquiſitive men, whereby two diſtilled liquors were made, barely by their mutual re-actions, to afford great ſtore of an earthly and very fixed ſubſtance. And to the ſame conjecture it was ſuitable, that, by obſtinately reiterating the experiment, the action of the fire upon the parts of the body expoſed to it, and their mutual operations and combinations among themſelves, and, not improbably too, the material concurrence of igneous particles might produce, beſides earthly fæces, other bodies not unlike thoſe, that paſs for the chymiſts principles. How far the event proved agreeable to his hypotheſis, will be the beſt gathered from the phænomena themſelves of the trials, which I ſhall proceed to ſet down a ſuccinct account of, as ſoon as I have premiſed, to make way for it, that by an eſſential oil, chymiſts are wont to mean ſuch a fine oleagenous liquor, as, to prevent empyreume and fæculent parts, has been diſtilled with ſtore of water in a (veſica or) limbeck.

WE took a pound of eſſential oil of anniſeeds, which liquor we therefore made choice of, becauſe it is more eaſy to diſcern it, by its ſelf-coagulating property, not to be ſophiſticated; and having put it into a glaſs retort of a convenient ſize, we cauſed it to be diſtilled (in a ſand-furnace, capable of giving a ſtrong fire) thirty-ſix times, in which train of operations, the enſuing phænomena were both congruous to our hypotheſis, and in themſelves obſervable.

1. As pure as the eſſential, or, as ſome ſpagyriſts ſtile them, the ætherial oil of vegetables are preſumed to be, and as confidently as chymiſts pretend they are, the pure ſulphurs or unctuous principles of the bodies that afford them; yet, not only the firſt diſtillation left a ſubſtance black like pitch, at the bottom of the retort, but at every one of the following diſtillations, ſuch a ſubſtance was either ſeparated or generated.

2. THOUGH after a diſtillation or two it ſeemed likely, that this pitchy ſubſtance would be found every time leſs and leſs, which made the perſon, formerly mentioned, tell me, that he ſuppoſed within ten, or at moſt twelve diſtillations, in all, if one ſhould make ſo many, this, which he looked upon as an earthy recrement, would be quite ſeparated, and leave the oil a moſt pure and homogeneous liquor; yet I, that (as I formerly intimated) looked upon this black ſtuff not as a ſeparated excrement, but generated ſubſtance, cauſed the diſtillations to be repeated, till they had attained thrice that number, and not only found, that at each time ſuch a black ſubſtance was left; but that now and then a ſubſequent diſtillation yielded much more of it than the precedent had done; which change, from leſs to more, and from more to leſs, was not obſerved only once or twice, but ſeveral times. And though this odd pitchy ſubſtance were, towards the latter end, found in leſs quantity than at the beginning, yet the cauſe of that may well be, that the oil to be diſtilled did ſenſibly, from time to time, decreaſe in bulk, partly by reaſon of the receſs of that portion of the oil which could not but be diſſipated and loſt in ſo many cohobations; and partly, and indeed chiefly, by the loſs of ſo much oil, as was tranſmuted into pitch and other ſubſtances.

3. THE oil appeared in diſtillation more fixed or unapt to riſe, than one would have expected from ſo fine and light a liquor; and eſpecially towards the latter end of the

diſtillations, it was often neceſſary to employ a ſcarce credible degree of fire to elevate all, that was not turned into pitch.

4. THE liquor did not diſtill like a pure principle or homogeneous body, as quickſilver is wont to do, but firſt ſome fine and light oil uſually came over, after which followed a leſs volatile oil with another ſubſtance or two, and after that, another aſcended in a diſtinct manner.

5. FOR it is to be noted, that, beſides the forementioned black earth, there were produced, by the operation of the fire, divers other ſubſtances, whereof the firſt was a wateriſh liquor or phlegm, which, after the oil had been expoſed to ſome diſtillations, began to grow very troubleſome. For being rarified by the heat of the fire into large bubbles, the antipathy, or rather incongruity, between them and the oil, occaſioned a kind of conflict, wherein theſe bubbles did often ſuddenly break, (uſually not without much noiſe) and ſometimes with ſuch violence, as to ſhake and endanger the retort; which once, by this conteſt, was actually broken; yet not ſo, but that the liquors, and other products of the fire, were ſaved by the watchful laborant, and ſeaſonably transferred into a new retort.

6. BESIDES this phlegm and the pitch formerly taken notice of, our operation afforded us, from time to time, a pretty quantity of a certain ſubſtance, which, with ſome not unſkilful perſons, paſſed for a volatile ſalt; becauſe it aſcended to the upper part of the veſſel, and appeared in a dry form, almoſt like ſhort needles; and becauſe alſo it ſeemed to the laborant, that, like a ſalt, it was (part of it at leaſt) diſſoluble in the ſpirituous phlegm mentioned in the laſt number. But though at firſt I inclined to this opinion, yet having made ſome few trials to examine the truth of it, I was, and ſtill am, a little doubtful, whether this ſublimed body deſerve the name of a true volatile ſalt, though poſſibly there may be a pretty deal of that contained in it. For I found the lumps of it, notwithſtanding their ſeeming ſpungineſs, to ſink in common water, and continue at the bottom of it, without manifeſtly being diſſolved by that liquor, (as meer volatile ſalts are wont eaſily to be) either in the cold, or by being kept awhile in a moderate heat. I found this ſubſtance fuſible, like bees-wax, at the flame of a ſmall taper; and if a lump of it were kindled thereat, it would burn away, partly with a yellow flame, and partly with a flame more intenſly blue than that of rectified ſpirit of wine; but it appeared apt enough to go out of itſelf.

THESE, and ſome other things inclined me to look upon our anomalous ſublimate, as a ſubſtance *ſui generis*; but yet ſuch a one, as I ſuſpected to be ſomewhat of kin to a *ſal volatile oleoſum*, ſuch as camphire ſeems to be. For our ſublimate riſes without ſtrong fire, and that in a dry form, and is eaſily enough fuſible; all which I have obſerved in camphire, as well as in volatile ſalts; and our ſublimate will, like camphire, diſſolve in a high rectified vinous ſpirit without at all colouring the liquor. And having long ſince found by trial, that camphire will, though ſlowly, diſſolve in good oil of vitriol, and make the menſtruum look of a reddiſh brown; I put ſome of that ſolvent upon our ſublimate, and after having left them ſome hours together, though but in the cold, the liquor ſeemed to have diſſolved part of the dry body, having, by its action upon it, acquired a brown colour ſomewhat inclining to red; and part of this liquor being put into a pretty deal of common water, there ſeemed to emerge by degrees a dry body or flores, which brought into my mind what I have elſewhere obſerved, when I employed the ſame method, to recover camphire out of oil of vitriol. But, as I was lately intimating, though I think it not improbable, that our anomalous ſublimate may be nearer of kin to a *ſal volatile oleoſum*, than to any of the chymical principles; yet I have not hitherto found the reſemblance betwixt it and ſuch a ſalt to be compleat enough, to make me dogmatical in referring it to any chymical product of a known denomination:

denomination: and therefore, till I be further ſatisfied, I ſhall only add, that this volatile ſalt, or oily ſublimate, or whatever name it may deſerve, was very pretty to look upon, being glittering almoſt like ſome fine flowers of *Benzoin*; and of this we had, though it were very light, between two or three drams.

7. BESIDES all theſe differing ſubſtances, our oil of anniſeeds afforded us, from time to time, a little quantity of ſpirit, as we concluded from two or three ſigns: one, that it came not over with the phlegm, and yet would mingle with that liquor, but not with the diſtilled oil: and another, that, as it was more fixed than the oil and phlegm, ſo it roſe later than they; and not only needed a ſtronger degree of fire, but, which is chiefly conſiderable, it was uſually obſerved to come over in white fumes, as many ſpirits, that are ſomewhat fixed, are wont to do; which was the more eaſy to be obſerved, becauſe, that being willing to make uſe of the ſame retort as long as we could, for the greater certainty of the experiment, and the pitch being not to be taken out, whilſt it was any thing ſoft, becauſe of its cloſe ſticking to the glaſs, it was thought, to give at the latter end of the diſtillation ſo ſtrong a fire, as, by making the ſand and retort red hot, (or very little leſs hot) would make the pitch ſo dry and brittle, that it might afterwards be looſened from the ſides of the glaſs, and got out in the form of a dry and brittle, though exceeding black ſubſtance. And to the two foregoing ſigns I ſhall now add a third, that does more clearly evince, that the ſubſtance, we have been ſpeaking of, was a ſpirit: for, though it could not but be very much weakned by being diffuſed through ſo great a quantity of phlegm as came over before it, yet its corpuſcles were ſo many and vigorous, than when I put them upon the powder of crude coral, they preſently began to diſſolve it; and the phlegmatic ſpirit did in a trice make a great ebullition with noiſe and bubbles, whether I poured it on the fixed ſalt of tartar, or the urinous and volatile ſalt of ſal armoniack.

THE ſucceſs of this experiment, being anſwerable enough to what I deſired and expected from it, allowed me to make divers reflections on it, and particularly thoſe that follow.

AND, 1. Our trial argues, that a ſubſtance, that is looked upon by chymiſts as a homogeneous body, and which paſſes for one of their principles, (for that is the caſe of our eſſential oil of anniſeeds) may yet be of ſuch a nature, that, barely by the further action of the fire it may be made to afford a very conſiderable proportion of a ſubſtance, exceedingly different from that which afforded it. For in our experiment, we obtained a *caput mortuum*, whoſe qualities were quite other than thoſe of oil of anniſeeds; ſince it was opacous, black, dry, very difficultly fuſible, and fixt in ſo ſtrong a degree of fire, as made the retort, that contained it, and the ſand about it, red hot. This ſubſtance would lie undiſſolved in a highly rectified vinous ſpirit, into which oil of anniſeeds would readily have diffuſed itſelf (if they had been a little ſhaken together) without the help of heat. And though we made the liquor actually boil a pretty while, yet moſt of the *caput mortuum* continued a black ſubſtance; only it diſcoloured the menſtruum, which looked as if it had rather extracted a tincture, than made a ſolution properly ſo called. And of this black and pitch like *caput mortuum*, we had at the end of our diſtillations, when we weighed the ſeveral parcels altogether, not much leſs (which you may juſtly think ſtrange) than half the weight of the whole oil of anniſeeds; this black ſtuff amounting to a pretty deal more than ſeven ounces. And from hence we may collect, that the analyſis, wont to be acquieſced in by vulgar chymiſts, is as yet but an undetermined thing, ſince they have not declared (nor perhaps thought of any ſuch thing) what number of diſtillations, (whether one or more, or if more than one, how many,) ſhall be made the ſtandard, by which we are to conclude, that a ſubſtance, obtained by diſtillation, is a chymical principle.

2. IT

diſtillations, it was often neceſſary to employ a ſcarce credible degree of fire to elevate all, that was not turned into pitch.

4. THE liquor did not diſtill like a pure principle or homogeneous body, as quickſilver is wont to do, but firſt ſome fine and light oil uſually came over, after which followed a leſs volatile oil with another ſubſtance or two, and after that, another aſcended in a diſtinct manner.

5. FOR it is to be noted, that, beſides the forementioned black earth, there were produced, by the operation of the fire, divers other ſubſtances, whereof the firſt was a wateriſh liquor or phlegm, which, after the oil had been expoſed to ſome diſtillations, began to grow very troubleſome. For being rarified by the heat of the fire into large bubbles, the antipathy, or rather incongruity, between them and the oil, occaſioned a kind of conflict, wherein theſe bubbles did often ſuddenly break, (uſually not without much noiſe) and ſometimes with ſuch violence, as to ſhake and endanger the retort; which once, by this conteſt, was actually broken; yet not ſo, but that the liquors, and other products of the fire, were ſaved by the watchful laborant, and ſeaſonably transferred into a new retort.

6. BESIDES this phlegm and the pitch formerly taken notice of, our operation afforded us, from time to time, a pretty quantity of a certain ſubſtance, which, with ſome not unſkilful perſons, paſſed for a volatile ſalt; becauſe it aſcended to the upper part of the veſſel, and appeared in a dry form, almoſt like ſhort needles; and becauſe alſo it ſeemed to the laborant, that, like a ſalt, it was (part of it at leaſt) diſſoluble in the ſpirituous phlegm mentioned in the laſt number. But though at firſt I inclined to this opinion, yet having made ſome few trials to examine the truth of it, I was, and ſtill am, a little doubtful, whether this ſublimed body deſerve the name of a true volatile ſalt, though poſſibly there may be a pretty deal of that contained in it. For I found the lumps of it, notwithſtanding their ſeeming ſpungineſs, to ſink in common water, and continue at the bottom of it, without manifeſtly being diſſolved by that liquor, (as meer volatile ſalts are wont eaſily to be) either in the cold, or by being kept awhile in a moderate heat. I found this ſubſtance fuſible, like bees-wax, at the flame of a ſmall taper; and if a lump of it were kindled thereat, it would burn away, partly with a yellow flame, and partly with a flame more intenſly blue than that of rectified ſpirit of wine; but it appeared apt enough to go out of itſelf.

THESE, and ſome other things inclined me to look upon our anomalous ſublimate, as a ſubſtance *ſui generis*; but yet ſuch a one, as I ſuſpected to be ſomewhat of kin to a *ſal volatile oleoſum*, ſuch as camphire ſeems to be. For our ſublimate riſes without ſtrong fire, and that in a dry form, and is eaſily enough fuſible; all which I have obſerved in camphire, as well as in volatile ſalts; and our ſublimate will, like camphire, diſſolve in a high rectified vinous ſpirit without at all colouring the liquor. And having long ſince found by trial, that camphire will, though ſlowly, diſſolve in good oil of vitriol, and make the menſtruum look of a reddiſh brown; I put ſome of that ſolvent upon our ſublimate, and after having left them ſome hours together, though but in the cold, the liquor ſeemed to have diſſolved part of the dry body, having, by its action upon it, acquired a brown colour ſomewhat inclining to red; and part of this liquor being put into a pretty deal of common water, there ſeemed to emerge by degrees a dry body or flores, which brought into my mind what I have elſewhere obſerved, when I employed the ſame method, to recover camphire out of oil of vitriol. But, as I was lately intimating, though I think it not improbable, that our anomalous ſublimate may be nearer of kin to a *ſal volatile oleoſum*, than to any of the chymical principles; yet I have not hitherto found the reſemblance betwixt it and ſuch a ſalt to be compleat enough, to make me dogmatical in referring it to any chymical product of a known denomination:

denomination: and therefore, till I be further satisfied, I shall only add, that this volatile salt, or oily sublimate, or whatever name it may deserve, was very pretty to look upon, being glittering almost like some fine flowers of *Benzoin*; and of this we had, though it were very light, between two or three drams.

7. BESIDES all these differing substances, our oil of anniseeds afforded us, from time to time, a little quantity of spirit, as we concluded from two or three signs: one, that it came not over with the phlegm, and yet would mingle with that liquor, but not with the distilled oil: and another, that, as it was more fixed than the oil and phlegm, so it rose later than they; and not only needed a stronger degree of fire, but, which is chiefly considerable, it was usually observed to come over in white fumes, as many spirits, that are somewhat fixed, are wont to do; which was the more easy to be observed, because, that being willing to make use of the same retort as long as we could, for the greater certainty of the experiment, and the pitch being not to be taken out, whilst it was any thing soft, because of its close sticking to the glass, it was thought, to give at the latter end of the distillation so strong a fire, as, by making the sand and retort red hot, (or very little less hot) would make the pitch so dry and brittle, that it might afterwards be loosened from the sides of the glass, and got out in the form of a dry and brittle, though exceeding black substance. And to the two foregoing signs I shall now add a third, that does more clearly evince, that the substance, we have been speaking of, was a spirit: for, though it could not but be very much weakned by being diffused through so great a quantity of phlegm as came over before it, yet its corpuscles were so many and vigorous, than when I put them upon the powder of crude coral, they presently began to dissolve it; and the phlegmatic spirit did in a trice make a great ebullition with noise and bubbles, whether I poured it on the fixed salt of tartar, or the urinous and volatile salt of sal armoniack.

THE success of this experiment, being answerable enough to what I desired and expected from it, allowed me to make divers reflections on it, and particularly those that follow.

AND, 1. Our trial argues, that a substance, that is looked upon by chymists as a homogeneous body, and which passes for one of their principles, (for that is the case of our essential oil of anniseeds) may yet be of such a nature, that, barely by the further action of the fire it may be made to afford a very considerable proportion of a substance, exceedingly different from that which afforded it. For in our experiment, we obtained a *caput mortuum*, whose qualities were quite other than those of oil of anniseeds; since it was opacous, black, dry, very difficultly fusible, and fixt in so strong a degree of fire, as made the retort, that contained it, and the sand about it, red hot. This substance would lie undissolved in a highly rectified vinous spirit, into which oil of anniseeds would readily have diffused itself (if they had been a little shaken together) without the help of heat. And though we made the liquor actually boil a pretty while, yet most of the *caput mortuum* continued a black substance; only it discoloured the menstruum, which looked as if it had rather extracted a tincture, than made a solution properly so called. And of this black and pitch like *caput mortuum*, we had at the end of our distillations, when we weighed the several parcels altogether, not much less (which you may justly think strange) than half the weight of the whole oil of anniseeds; this black stuff amounting to a pretty deal more than seven ounces. And from hence we may collect, that the analysis, wont to be acquiesced in by vulgar chymists, is as yet but an undetermined thing, since they have not declared (nor perhaps thought of any such thing) what number of distillations, (whether one or more, or if more than one, how many,) shall be made the standard, by which we are to conclude, that a substance, obtained by distillation, is a chymical principle.

2. IT

2. IT may be alſo inferred from ſome phænomena of our experiment, that the fire, as it is wont to be employed, is not, as chymiſts pretend it to be, the true and genuine inſtrument of the analyſis of bodies, ſince it does not, as they preſume it does, barely extricate and ſeparate the ſeveral ſimilar ſubſtances, that, though concealed and diſguiſed by commixtures with each other, were pre-exiſtent (in the ſame forms wherein they afterwards appear) in the body expoſed to its operation. For in our caſe, the oil of anniſeeds is, according to the chymiſt's eſtimate, the ſulphureous principle of the concrete; and therefore has not any other principles, eſpecially in any conſiderable quantity, contained in it. And yet, by the bare action of the fire, this oil, which they acknowledge to be a ſimilar body, is brought to afford three or four other ſubſtances of differing natures from one another.

3. FROM the two foregoing obſervations we may likewiſe deduce, that the fire, at leaſt in divers caſes, may not only ſeparate, but variouſly compound and alter, the parts of a body expoſed to its action; and thereby give conſidering men cauſe to ſuſpect, that divers ſubſtances, that are preſumed to be only extricated or extracted by the operation of the fire, may be really produced by it.

4. AGREEABLY to which ſuſpicion, it happens unluckily enough for the chymical hypotheſis; that in our experiment the ſubſtances, that were obtained, were a fixt and earthy powder, a not inconſiderable portion of phlegm, a ſpirituous liquor, and a dry ſublimate, a-kin to a volatile ſalt: and theſe, though they all proceed from a body, that, according to the chymiſts, ſhould not, and indeed, for aught appears, did not actually contain any of them before, did yet ſo reſemble in qualities the earths, phlegm, and ſpirit, that chymiſts obtain by diſtillation from thoſe bodies they look upon as perfectly mixed and compounded; that if they are not the ſame, they have divers qualities ſo like thoſe, that entitle what is afforded by confeſſedly mixed bodies to the name of phlegm or ſalt, or ſome other primordial ingredient, that the earth, for inſtance, or the (acid) ſpirit obtained in our experiment, are far leſs differing from the earth of blood, or the ſpirit of amber, than theſe are from the earth and ſalt of thoroughly calcined hartſhorn.

5. IT will ſcarce be neceſſary to draw ſo obvious a corollary from what has been already delivered, as this; that it is poſſible, that chymical principles (or at leaſt ſuch ſubſtances, as a vulgar chymiſt, not knowing from what body they were obtained, would look upon as ſuch) may be made of another.

BUT though I ſhall not enlarge on this corollary, it will not be amiſs to make one conſiderable obſervation, that belongs to it; namely, that acid and aqueous ſubſtances, how differing ſoever each of them is from a pure oil, may be produced from it by the action of the fire. For I found by trial, that in that liquor, which the laborant took for the phlegm of oil of anniſeeds, there was ſuch ſtore of acid parts, that (as was above recited) they would readily diſſolve powdered coral, though crude, even in the cold; and would make a great conflict with ſalt of tartar, and with that of urine, as ſoon as the liquor was put upon it. But yet the greateſt part of the liquor, by far, ſeemed to be of an aqueous nature, which a chymiſt would call phlegm; which is a body, that will not mingle with oil, and is otherwiſe exceedingly different from oil, eſpecially an eſſential one, (ſuch as that of anniſeeds,) whoſe purity makes it totally inflammable. And the quantity of the whole liquor, conſiſting of acid and phlegmatic parts, was far from being inconſiderable, amounting to about two ounces and three quarters. It may alſo be worth while to obſerve, as another thing belonging to our corollary, that the truth declared in it allows us to queſtion, whether it be neceſſary to ſuppoſe with the chymiſts, that nature has been obliged to make proviſion of great quantities of primordial and ſimple bodies; and that ſhe is ſollicitous to mix them all together,

together, for the composing of a body, capable of affording, by the analysis or separatory operation of the fire, salt, spirit, sulphur, phlegm, and earth. I will not hence universally infer, that there are no such substances, one or more, to be found, in any of those bodies, that are called perfectly mixed, antecedently to their being exposed to the fire. But this, I think, will follow from what has been delivered, that the pre-existence of such substances must be made out by some other way, than the bare operation of the fire; and that the grand chymical supposition will not hold in all bodies, that what similar body soever is obtained by the operation of the fire from a concrete, committed to distillation, was formerly and actually pre-existent in it.

6. AND lastly, our experiment affords us a considerable argument in favour of that part of the corpuscular or mechanical hypothesis, that teaches inanimate bodies to differ from one another, but in the bigness, shape, motion, contexture, and, in a word, the mechanical affections of the minute parts they consist of. For in our experiment, we see, that oil of anniseeds, which is not only an uniform or similar body, as to sense, but is judged so by chymists, upon an analysis of the concrete that afforded it, is, by having its parts variously agitated, shaked and rubbed against one another, and in differing manners broken, associated and ranged, transmuted into four bodies of such differing natures and qualities, as the chymists principles and elements are known to be; and this, without the help of any true seed or plastick principle, by the bare operation of the fire, which *Helmont* calls *the artificial death of things*, and *the destroyer of seminalities*. And this is the more considerable, because, whereas the ancient corpuscularian philosophers assigned three general ways, whereby bodies may be produced; namely, by the addition of new parts, the dividing and sometimes taking away a portion of the former, and the transposition of the constituent parts; in our experiment, the whole work seems to be performed only by what they would have called transposition, and that guided but by so simple, impetuous, and unruly an agent, as the fire. Unless it be said, that divers igneous particles, that penetrated the glass retort, did substantially associate themselves with some parts of the oil of anniseeds, and concur with them to compose the pitch-like *caput mortuum*; which they will perhaps say, that (whatever others may do) I ought to allow as a possible thing, after what I have elsewhere written, purposely to shew, that such a penetration of the pores of glass, by the atoms of fire, is not impossible. But the experiments I have delivered, of the ponderability of flame, prove only, that some of its particles may combine with heavy bodies, and fixed in the degree of fire I exposed them to; my trials having been made upon tin and lead. And it will not perhaps be thought likely, that igneous atoms should, by their combination with the particles of an inflammable oil, produce an aqueous liquor, and that in great quantity; as we lately noted of the acid phlegm of anniseeds, amounting to two ounces and almost six drams.

UPON the whole matter, the phænomena observable in our experiment upon oil of anniseeds seem very congruous to the mechanical hypothesis, and very unfavourable to that of the chymists. For, whether the fire be supposed to have acted meerly as an agent, or efficient cause, or to have also concurred as a material one; it appears, that by changes of texture, without the addition of any visible parts, or of any seed, bodies very differing in colour, consistence, fixity, and divers other qualities, may be produced from a body, not only homogeneous as to sense, but pure enough to pass for a chymical principle. And though the suspicion lately proposed should be allowed to be probable, as to the other products, as well as to the black substance; yet still our process upon the oil of anniseeds would afford considerable corollaries against the chymical doctrine I call in question. For, 1. The objection we are considering will give wary men just ground to suspect, that in ordinary chymical distillations the fire is not always to be looked upon as a meer instrument of analysis; but may in many, if not in most cases, be also a material cause of the supposed principles produced by its means; which is quite contrary to the received doctrine of the vulgar spagyrists, (who are those, I now dispute with.) But, not to insist on this remark, I think our process upon oil of anniseeds will not serve to prove the paradox proposed at the beginning of this paper; since, treating of ingredients chymically obtainable, I may be allowed to speak of them in the usual sense of chymists, who suppose the different substances, obtained from a body committed to distillation, to be the hypostatical principles, and other ingredients of it, without supposing the fire to do any more than extricate or disentangle them from one another. So that, secondly, (to conclude at last this long paragraph) our experiment seems to prove the production of other chymical principles out of one, as validly as their experiments prove, that such principles are obtained from the bodies they call mixed. For, whereas the force of the lately proposed surmise or objection lies in this, that the association of igneous particles hinders the different substances, that our oil of anniseeds afforded us, from being uncompounded bodies, and therefore, from being true principles, they may, for all this, have as good right to the title of principles, as most of these they will have us take for such; since the atoms of the fire may every whit as reasonably be supposed to associate themselves variously with the corpuscles of a mixt body, committed to distillation, as with those of oil of anniseeds, and so their supposed principles will, no more than ours, be free from composition. Nay, the advantage in the comparison lies on our side, since the different substances, that chymists are wont to obtain from a mixt body, may be compounded, not only with igneous particles, but with the differing ingredients of the same body; since this, in their hypothesis, is a mixt body; whereas essential oil of anniseeds is by them granted to be a simple one; and therefore each of its productions is compounded with no extraneous substance, save the corpuscles of the fire.

WHEN I had framed the conjecture, that from a chymical principle several differing bodies might be obtained by the meer operation of the fire; I thought fit to endeavour the confirmation of it, by making experiments upon other distilled oils, (for those made by expression are, by chymists themselves, owned to be compounded bodies,) of natures different, both from the oil of anniseeds, and from one another. And accordingly, whilst our process with this oil, which the chymists call essential, and which was drawn from a vegetable seed, was carrying on, I took care to have distilled in the same furnace oil of turpentine, which, though drawn from a vegetable substance, differs very much from oil of anniseeds; as also oil of amber, which, according to chymists, belongs to the mineral kingdom; and lastly, oil of hartshorn, afforded by a subject, which (to speak in their phrase) belongs to the animal kingdom.

POSTSCRIPT.

THESE oils were committed to the same laborant, that managed the lately mentioned oil of anniseeds; and he accordingly kept a kind of journal of the number of rectifications, the quantities of pitchy matter from time to time afforded by them, and divers other phænomena, or circumstances, that occurred in so tedious a prosecution, as I thought experiments of such moment deserved. But the papers, containing the minutes of these things, having been unhappily lost, with divers others, by occasion of a great fire, that obliged me, after midnight, to make a hasty and very disorderly remove of my writings, I could never since retrieve the particulars. And therefore I must content myself, to set down as much of the trials, (which I hope will comprize the most substantial part of them) as by examining my own memory and the laborant's

and

and by ſome few other helps, I am able to collect; as ſoon as I have given here one advertiſement, becauſe I think it was omitted in the papers, whence the foregoing part of this writing was tranſcribed. The advertiſement is this; that as well in the precedent, as in the ſubſequent part of this diſcourſe, I do not take chymical principles in the ſtricteſt ſenſe of that term, wherein it is confined to ſalt, ſulphur, and mercury; but in the larger acception, wherein the learned Doctor *Willis*, and divers other chymiſts (that are not all his juniors) employ it, when they comprize under it two elementary bodies; as they do, when they conſtitute five principles, (which perhaps might be more clearly called ſimilar ingredients;) in which number they comprize phlegm and earth, which other chymiſts, as well as the Ariſtotelians, would call elements, (of the body that affords them.) Taking then, in his whole writing, the word principles in the larger ſenſe above declared (congruouſly whereunto I uſually call them the chymical principles indefinitely, not the hypoſtatical principles, which are accounted but three;) I proceed to the particular obſervations and reflections I was going to begin.

Firſt, THEN it plainly appears, that two of the three lately mentioned oils, namely, the oil of turpentine, and the oil of amber, and therefore very probably the third alſo, (my deſign being the ſame in all three) were diſtilled at leaſt fifty times; for I intended not to ſtop ſhort of that number. And by an inſcription upon one of the phials, I found, that the oil, which the contained liquor was drawn from, had been diſtilled one and fifty times; which number exceeds, by fifteen, that of the diſtillations formerly mentioned to have been made of the oil of anniſeeds.

2. EACH of the three other oils, (*viz.* that of hartſhorn, that of turpentine, and that of amber,) did leave, from time to time, in the bottom of the retorts, whence they were drawn off, a not inconſiderable quantity of black fæces, much like thoſe, that were left by the oil of anniſeeds, and which I have formerly called a pitchy ſubſtance. I found in a ſuperſcribed paper ſome quantity of this ſtuff left by the oil of turpentine; and, though I am not certain, whether it were all the ſtuff of this kind, that was afforded by the pound of oil we employed, yet it amounted to above two ounces and five drams; and, if I much miſtake not, the other two oils did each of them afford a conſiderable quantity of black terreſtrial matter, though the heedful laborant obſerved, that the oil of hartſhorn did ſooner leave off yielding copious fæces (as I often in this paper call them, to comply with the chymiſts terms, though I aſſent not to their notion) than the oil of turpentine, or that of amber. But if neither of the two afforded any more than did the oil of turpentine, I look upon it as a remarkable thing, that the oil of anniſeeds, which is a fine eſſential oil, diſtilled in a veſica or limbeck, ſhould yield above twice more of earthy matter, than either of the three other oils, that were diſtilled but in retorts.

3. OF the colour of the rectified oil of hartſhorn I can ſay little, having unluckily loſt the liquor itſelf; but the oil of amber, after the one and fiftieth diſtillation, was indeed very clear, but yet of an amber colour, that was far from pale; and the oil of turpentine, that is uſually after one rectification a clear and colourleſs liquor, after fifty diſtillations appeared almoſt red.

4. BUT that is much more conſiderable, which I obſerved in the quantities of the different liquors afforded by the long ſeries of our diſtillations; for at the end of the proceſs, the remaining oil of turpentine, for inſtance, did not appear to my eyes to be conſiderably, if at all, ſuperior in bulk to another liquor, that came over with it in diſtillation, and was not true oil; for it would readily enough mix with water, but keep itſelf in a maſs diſtinct from the oil, and weighed above three ounces and three quarters.

5. This oddly produced liquor I looked upon as compounded of ſpirit and of phlegm: for though the latter did ſo much exceed the other in quantity, that, in an expert chy-

mist's opinion, the whole liquor passed for phlegm; yet, I not only judged some parts of it to be spirits, but found them to be of an acid nature too; since, besides what the taste made me suspect, I found, that the compounded liquor would readily enough begin to corrode beaten coral, even in the cold, and some of it being poured upon good salt of tartar, presently made with it a conflict and ebullition, not without a hissing noise and a multitude of bubbles. So likewise the spirituous phlegm of amber made a conflict with salt of tartar, and dissolved crude coral, as other weak acid spirits are also wont to do. This liquor of amber was not pale, as the phlegmatic spirit of oil of anniseeds was; and that lately mentioned to have been obtained from oil of turpentine was high coloured, being of a brownish red.

6. I forgot to take notice in due place (and therefore do it in this) that, among other trials made to discover, that the spirits afforded by our oils were really acid, we put two ounces of the spirituous phlegm of oil of anniseeds (that being the mildest oil) upon some minium; and, having digested them a while together, found the liquor turned sweet, and fit (as we judged) to make *Saccharum Saturni*: and this liquor (after filtration through cap-paper) being gently abstracted, left in the bottom of the retort a thick honey like substance, from which (the distillation being continued in the same retort, but with a much stronger heat) there came over some liquor, which being in too little quantity to be rectified, we could not free it from its phlegm, and therefore did not find it inflammable; as I guessed the spirituous part would have been after rectification, in regard the liquor was exceeding like the *Spiritus ardens Saturni*, I have elsewhere described, in its peculiar and very penetrating taste and smell.

7. It may not be unfit to be taken notice of in this place, that having lighted on two phials, that I long knew not what was become of, one of which contained some of the oil of turpentine, and the other some of the oil of amber, that I formerly mentioned to have been each of them distilled at least fifty times; I thought fit to try, whether, after these liquors had been laid by about a year and half, or longer, they would, after having had so long a time to defecate themselves in, yield still such a black substance as has been oftentimes mentioned; and having, to satisfy myself, caused each of these liquors to be again distilled in a retort, it left behind it a greater quantity of a black and shining substance, than could well have been expected.

8. It may seem very odd in itself, and may much serve to confirm what I have elsewhere delivered about the mechanical origin of fixity, as also of the qualities opposite to fusiblenefs and to fluidity; that so volatile and thin a liquor, as a chymical rectified oil, should, by the bare operation of the fire, be brought to yield a great quantity of what looks so like terrestrial fæces, as our pitchy *Capita Mortua* did. But this reflection will, I presume, appear the better grounded, if I add the success of one trial (among others) that I made, to examine the terrestrial nature of the black bodies I am speaking of: for, although many bodies, that will not ascend, or be dissipated in close vessels, will easily be driven away in open ones; yet having put one ounce of the *Caput Mortuum* of the oil of anniseeds, and as much of that afforded by oil of turpentine, each of them into a distinct crucible, and kept them three hours and a half, or near four hours, in such a heat as made the vessels all the while red hot; though we kept not the crucibles closely covered, but only loosly to keep out the ashes, yet we found, when the vessels were removed from the fire, that the contained bodies had not at all been brought to fusion, only that from the oil of anniseeds was in part somewhat caked together: and notwithstanding all the heat they had been exposed to, the pitchy substance of the oil of turpentine retained not far from three quarters, of its first weight; and the *Caput mortuum* of oil of anniseeds lost about eighteen grains less than that.

9. AFTER this we alfo endeavoured to difcover, whether our pitchy fubftances would afford any fixt or alcalifate falt, as the *Capita Mortua* of moft bodies belonging to the vegetable kingdom are wont to do. But though we kept an ounce a-piece of the black fubftances, left by the oil of anniieeds and that of turpentine, for nine or ten hours, red hot in the crucibles; yet we found indeed the bodies very much diminifhed in quantity, but they did not appear at all calcined.

10. INTO a couple of ounces of this laft named liquor, we put, by little and little, as much of dry falt of tartar, as it would work upon, as an acid, or (if you pleafe) till there would no longer be any vifible conflict excited by adding more falt. The acid being thus mortified or fatiated, I intended to draw off all the reft of the liquor, and to try with the phlegm drawn off, whether, by cohobating it very flowly in a new glafs head and body, I could not make a farther tranfmutation of the terebinthinate oil, and change, at leaft, part of this aqueous phlegmatic liquor into a whitifh earth. About the poffibility of which tranfmutation, and of fome others alfo, I might here fubjoin an account, if, fince the lately mentioned fire, I could have found a fhort difcourfe I wrote, to propofe and examine this grand phyfico-chymical problem, whether we ought to admit any other elements or hypoftatical principles at all, even fo much as one of the bodies, that are commonly called, mixt?

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Wherein are contained divers EXPERIMENTS made both in compreffed and alfo in factitious AIR, about FIRE, ANIMALS, *&c.* Together with a DESCRIPTION of the ENGINES, wherein they were made.

The PREFACE to the LATIN Edition.

AFTER I had firft publifhed my phyfico-mechanical experiments to the curious world, and, fome years after, the continuation of them, (together with a full defcription of the engines, and leffer veffels, which I ufed in the making of them) I thought it a very venial thing in me, if, fuperfeding any farther labour upon fuch fubjects, I left that argument to be ftudied, and, if they had pleafed, cultivated by others. And therefore I was content to annex only fome experiments, occafionally made, concerning refpiration, concerning the fcarce credible rarefaction of the air, and laftly, concerning the prefervation of fome bodies, whilft they are defended from the contact of the air; in regard thofe tracts were of kin to other arguments, which I had occafion to handle at feveral times. But in feven or eight years fpace, hearing of very few experiments made, either in the engine I ufed, or in any other made after the model thereof, I began to reaffume fome thoughts concerning the farther ufe thereof myfelf; at which time it happened very opportunely, that a certain tract written in *French*, fmall in bulk, but very ingenious, containing fundry experiments concerning the prefervation of fruits, and fome other tracts of a different nature, was brought unto me by monfieur *Papin*, who had joined his pains with the eminent monfieur sieur *Christian Hugenius*, in making the said experiments; and, upon farther discourse with him, finding, that he came out of *France* into *England* but a little before, in hopes to obtain some place here, which might be fit for his genius, and, whilst he was in that expectancy, that he was willing to bestow his pains about experimental philosophy, upon which, I had an inclination, at my cost, to gratify his curiosity, whilst I also indulged my own; and, seeing he had a pneumatic-pump of his own, made by himself, to the use of which he was more accustomed, though it differed from the structure of my pump, I gave him the freedom to use his own, because he best knew how to ply it alone, and (if any disorder should happen, from the luxation of its parts, or any other casualty) how to repair it more easily. Though, in his absence, I chose rather to use my own pump, both because my domestics were better acquainted with it, and also because it was not subject to so many and frequent inconveniencies, by reason of its more solid structure.

BUT, seeing several sorts of experiments, long since made on divers bodies, had left me little to do about the same subjects, there were only two things, which I chiefly designed to prosecute. One of which contained those experiments, which, when I first published my Physico-mechanical Experiments, I had wished in general had been made, not in rarefied or expanded, but in condensed or rather compressed air. The other was to be versant about those trials, which were not to be made, as the former, with natural air, either in its wonted state, or any way rarefied, but factitious air, (that I may so speak,) such as, in my former writings, I had mentioned to be producible by the help of fermentations or corrosions. The divers ways of producing or extricating that factitious air, and the ways of trying it, when it was produced, having been some years ago presented to the Royal Society, I was invited, by that learned assembly, to prosecute farther those disquisitions. Now, although those were the chief kinds of experiments, which I applied my mind unto, yet it will appear by the following sheets, that I did not confine myself to them alone.

BUT, before I could make any considerable progress in this work, it pleased the most just and wise God, the supreme arbiter and ruler of all things, to afflict me with the stone (the pains whereof do as yet now and then trouble me,) so that I was enforced to take another course of proceeding. For, to ease myself, it was judged meet, that monsieur *Papin* should set down in writing all the experiments and the phænomena arising therefrom, as if they had been made and observed by his own skill; and moreover, the calculation of the degrees of the rarefaction and condensation of the air, included in our mercurial gage, was intrusted to his care. But I, myself, was always present at the making of the chief experiments, and also at some of those of an inferior sort, to observe whether all things were done according to my mind. But, as for those experiments, which required a longer time in making, such as those about the conservation of bodies, he did from time to time, with great diligence, acquaint me with those alterations, which happened in them in my absence; and he also brought the glass-instruments to me, and declared to me the effects of the experiments, when they were finished, that so I might take into consideration the changes made in the materials, when taken out of the vessels. Yet, I confess, I was purposely somewhat more incurious and remiss about those experiments, which were made concerning the preservation of fruits, and of flesh in liquors, which was made chiefly by the help of compression; and also about the coction of meat. For, as some of these later experiments were propounded for trial by monsieur *Papin*, for a particular end of his own, somewhat different from my design in the other experiments; so I was very willing, that he should use his own method about them, not doubting but he would use his greatest industry therein, as I found, by the event, that he had done. Yea, I did judge, that I might more safely acquiesce in

his

his relations, concerning the experiments about flesh, about fruits, and about boiling of meat; because, as these were some of the last which we made, so I had cause enough to trust his skill and diligence used about the former experiments; some of which, *viz.* those which are marked with an asterisk, he himself propounded, as if they had been formed in his own brain; as also not a few of the mechanical instruments, (especially the double-pump, and wind-gun) which sometimes were of necessary use to us in our work, are to be referred to his invention, who also made some of them, at least in part, with his own hands.

IN the following tract, the reader will not find the reasons subjoined, which moved me to make these experiments, (which I usually did in my former Physico-mechanical Experiments, and in the Continuation of them) for I had neither leisure, nor a mind free from other businesses, to make such a preface; and I did also hope, that the sagacious reader would find out my sense well enough, though purposely not expressed in plain words, if he did but attentively consider the nature of the things treated of; especially if calling in to his aid those short corollaries, which he will find annexed to the several experiments, whereby he may fish out my aim. Though, to speak the truth, some few of those inferences owe themselves more to my assistant than to me.

I am well assured, that very many of the following experiments will not be thought weighty enough by many readers, as to deserve to be printed; and indeed I myself was so far of their mind, that I had once thoughts of expunging them out of the following collection; but at last I was more easily persuaded to afford them a place amongst the rest, because, however they may be considered apart, yet, in consort with the rest, they may be, at least, of moderate use.

I was not very solicitous about the style, because, being infirm in point of health, and besides, surrounded with many businesses, I was enforced to leave the choice of words to monsieur *Papin*; my chief care being to have the whole work diligently read over to me, that so no mistake might pass by unobserved about the experiments themselves. Besides, seeing the things here treated of are merely physical, and their manner of handling but historical, there is no need of any farther apology to excuse the incomptness of the style: yet this may be alledged in excuse thereof; that the heads of things (or memorials as they are called) being at first set down, for haste, by monsieur *Papin* in his own native tongue, *scil.* the *French*, and afterwards turned into *Latin*, (in which habit they now appear) do labour with that inconvenience, which doth usually attend all translations, especially where the interpreter must have a greater care of the propriety of words, than of the elegancy of them.

MOREOVER, he that shall attentively consider the following experiments, will not wonder, that they are delivered in a less accurate method. For we accounted it sufficient for our purpose, to reduce those experiments, which did differ, and had least affinity amongst themselves, into some certain heads, to which they seemed most commodiously to be referrable: and, besides, considering the nature of the experiments themselves, I hope the reader will easily grant, that at least many of them ought to have been set down in the way of a diary, yet distinguished, and, as it were, intercalated by frequent intervals, because the examination of some of them was protracted for many days, the nature of the experiments themselves, and also the design of the experimentators, requiring such chasms. Add hereto, that I was more willing to set down divers things with their minute circumstances; because I was of opinion, that probably many of these experiments would be never either re-examined by others, or re-iterated by myself. For though they may be easily read, when set down with pen and ink in paper-sheets, yet he, that shall really go about to repeat them, will find it no easy task.

FOR there are so many, and such sundry sorts of instruments, both of a greater and lesser size, which are necessarily required for use herein, some of them to be made on purpose for the present occasion; respect also being had to the time and assiduity, requisite and necessary for making the experiments and observations, in cases, wherein so subtile and elastic a body as the air is, must be violently reduced into a preternatural state, and must be long kept in that disposition; that, as it is a very difficult thing to prevent those inconveniencies, which do attend so unusual experiments, so it is far more difficult to apply remedies to those inconveniencies, after they have once happened. For these, and other reasons, so much time is to be spent, that I am almost ashamed to tell, how much thereof was impended on these trials which are contained in the present book, though but small, to which this poeme is prefixed.

NEVERTHELESS, though all these things are alledged in excuse, yet the deficiency of this collection is so well known to me, (there being little to be found therein which may commend books) that I would invite very few philosophers to the reading of so incult and unpolite a rhapsody, especially from the beginning to the end. For though it may probably happen, that some experiments contained herein may not be disallowed by the curious, yet they may have leave from me, to esteem this whole tract but as a loose heap (or rather chaos) of particulars belonging to the air, especially as constituted in its preternatural state, and to the operations of it upon some bodies, as clothed with such and such circumstances: so that it is free for them to cull out only those experiments, which please their curiosity, or any other of their concerns best, without being obliged to read over the whole book, no more than a lexicon, which we use not to consult, but now and then, for the sake of a word. In short, it is not probable, that a book so impolite, as this is, will be either wholly read over, or can conciliate any favour from the reading, unless with those readers, to whom a book comes sufficiently commended only upon this accompt, that it contains things new and also true. For if those two privileges are enough to obtain favour, then there is no cause, that the following tract should wholly despair of the reader's benevolence, especially since some trials contained therein do treat of the properties and operations of the air; I say, of the air, which, notwithstanding the laudable endeavours of some ingenious modern writers in the explication thereof, yet is a body, which, I fear at present, we have greater use and necessity of than knowledge.

An ADVERTISEMENT of the PUBLISHER to the READER, before the Latin Edition.

SEVERAL tracts, made by our author, printed at *Geneva*, and bound up in one volume, were not long since transported into *England*: in which matter, though the author himself doth not complain (which yet he might lawfully do) of the immoderate liberty of some men, who have presumed, unknown to him, to bind up so many of his writings together, and to publish them; not to mention the print, as being but bad, (or at least not accurate) yet there are two things in that edition, which, in our author's behalf, cannot be concealed without just reprehension, for they may impair his credit much, especially with those, to whom his writings are not otherwise known than by that collection.

For, first, there is no signification made therein, that any of Mr. *Boyle*'s tracts were ever written in any other language than that, wherewith they are there cloathed, *viz.* the *Latin*; whence it may probably come to pass, that all the faults and defects of style, which are wont to blemish translations, especially such as are literally made, may, by readers, who are not otherwise informed, be imputed to the author himself, who,

for

for reasons often rendered by him, was induced to write all his works in the English tongue; the versions of some of them into Latin being not so much as seen by him, till, being come from the press, they were put into his hands.

SECONDLY, the several tracts, making up that collection, are all dated in one and the same year, *viz.* 1677, as if they had been all, both writ, and also published, by our author, at once; whereas indeed some of them were made publick eight or 10 years, some 11 or 12, others 17 or 18 years before ever this collection saw the light. Hence an injury, greater than the former, may be offered to our author; for those readers, to whom neither himself nor his lucubrations are known but from that volume, may be easily persuaded to believe, that those experiments, if perhaps they meet with the same, which are comprehended in these books, and are also found in other mens works printed before 1677, were transferred, by our author, out of their tracts into his own; than which nothing can be imagined or spoken at a greater distance from truth. For, indeed, if, applying myself for three whole years to manage the experiments of so great a person, and thereby having frequent opportunity to converse with him, I sometimes casually light upon something new, yet who sees not, that thanks is to be returned to him alone, who afforded me both the occasion of meditation, and also leisure to operate? Yet such is the humanity of this noble person, that he mentions my name in the preface to this book, as if some things therein were mine. Who then can justly say, that he hath excerped any thing from other authors, who gives his own freely unto others?

[*The rest of this advertisement relates to a catalogue of Mr.* Boyle's *philosophical writings, subjoined to it.*]

The TRANSLATOR to the READER.

THOUGH the first part of the Physico-mechanical Experiments of this honourable author was published by him in the English tongue, as was also, some years after, his first Continuation of the same; yet so welcomely were they entertained by the curious, especially in transmarine parts, that the first part hath been long since published in the Latin tongue; and the first Continuation is also translated in the same tongue, though (for reasons, in part mentioned at the end of the publisher's advertisement to the reader, prefixed before this tract) not yet printed.

THIS second Continuation of the aforesaid experiments speaks Latin at the first hand; but that all those three tracts might be cloathed in one habit, it was the desire of some ingenious persons, that it might also be rendered into English; which province hath been recommended to me by the bookseller.

I may without vanity affirm, that I have an advantage beyond some others, in reference to the versions of any tracts of this noble author, either out of English into Latin, or out of Latin into English; in that, by reason of the vicinity of my habitation, I have conveniency, at fitting seasons, to consult the author himself, about his sense in any place, which may be doubtful to me; which I mention, not only to declare the candour and condescension of so eminent a person, but also to account for any mistakes I may be guilty of, which are therefore rendered the more inexcusable in me, and properly to be laid at my own door.

IT is not to be doubted, but that, if the honourable author of these following experiments had himself at first drest them up in their English habit, they had appeared far more terse and polite, than my inability can trim them up to; yet, besides the necessary inconveniencies, which attend all translations of books, (especially those, which treat of nice and curious subjects) which I alledge for part of my apology, I do farther re-

 lieve

lieve myself with that saying of the orator, *Si quis summa desperet, tamen est pulchrum in secundis tertiisve consistere.* Quintil.

AND in reference to the elegancies of this noble author, in the English as well as other languages (of which he is so great a master) I may farther add, with the same orator, *Ut transeundi spes non sit, magna tamen est dignitas consequendi.* It is sufficient honour for me to write after his copy. In fine, I conclude with the poet;

Veniam pro laude peto, laudatus abunde,
Si fastiditus non tibi, lector, ero.

The SECOND CONTINUATION of

PHYSICO-MECHANICAL EXPERIMENTS.

ICONISME I.

The description of the engine, with a double tube, for the exhausting of the air.

AA ARE two pumps made of brass.

BB are two plugs hollow within, and open below.

CC are two holes in the upper part of the plugs, with valves opening outwardly, that they may afford passage to the air to go out, and hinder it from coming in.

DDDD are iron rods serving to move the plugs, and annexed to them, by means of the gnomons FF.

EE are two flat iron stirrups at the top of the rods DD, on which the operator must stand to set a work the engine.

GGG is a cord joined to the two stirrups, and compassing the pully H.

LL are two valves at the bottom of the pumps, opening inwardly, for the admission of the air out of the tube MM.

MM is a tube reaching from both pumps to the plate OO, by means of the curvature PP QQ; which curvature ought to be of so great length, that the tube PQQ may not hinder the exerciser of the pumps, but that he may conveniently stand on the stirrups EE.

OO is a plate bored in the middle, on which the receivers, to be evacuated, are to be put; as R for example.

BEFORE this engine can be fit for use, it is to be put into a frame of wood to support it, as is shewed in the second scheme, and as much water is to be poured through the hole Q in the plate OO into the pumps, as is sufficient to fill the cavities of the plugs, and a little more; and then somebody must stand on the two iron stirrups EE, and must alternately depress and elevate them. For by this means it will come to pass, that the plugs, following the motion of the stirrups in their ascent, will leave the space in the bottom of the pumps empty, and seeing all other passage is intercluded from the air, that air alone which is contained in the receiver R, is conveyed into the aforesaid pumps by the tube QQ PP M, and opens the valve L, which, being presently shut, hinders the same air from making a regress; wherefore the plug, afterwards descending, compresseth that air, whence of necessity, the valve C must be opened, and all the air must pass out at it, *viz.* because the water, in the bottom of the pumps, doth exactly fill all the spaces, and doth also regurgitate through the valve C.

HERE we may observe, that this double engine is, upon many occasions, to be preferred before a single one, that is moved with the foot) for it doth not only produce a double effect, but performs it also much more easily; for in those engines, which are furnished but with one tube, whilst the plug is drawn up to evacuate the pump, the

L. IV. Pl. 1.

ICONISMUS 1.us

Fig. 1. p. 510.

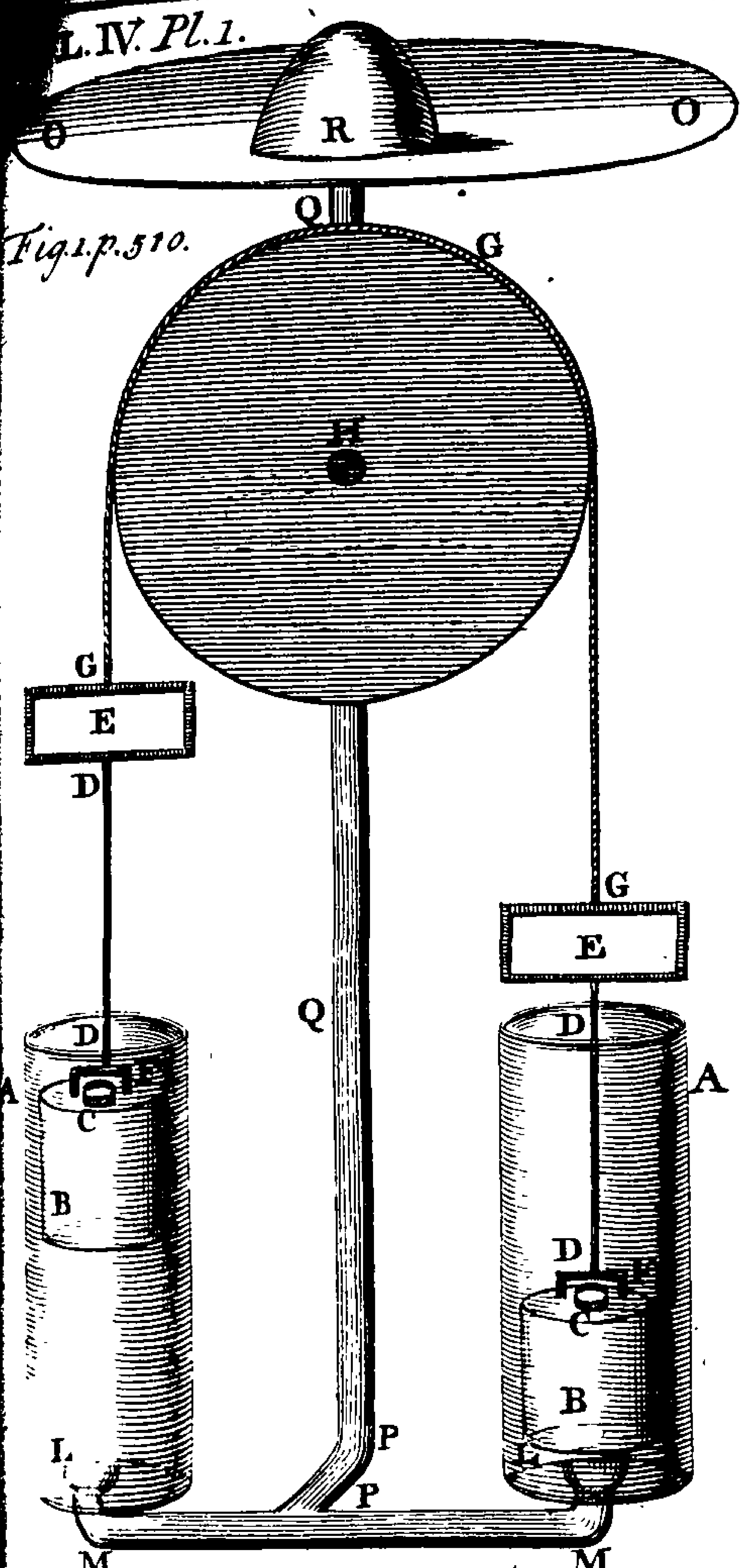
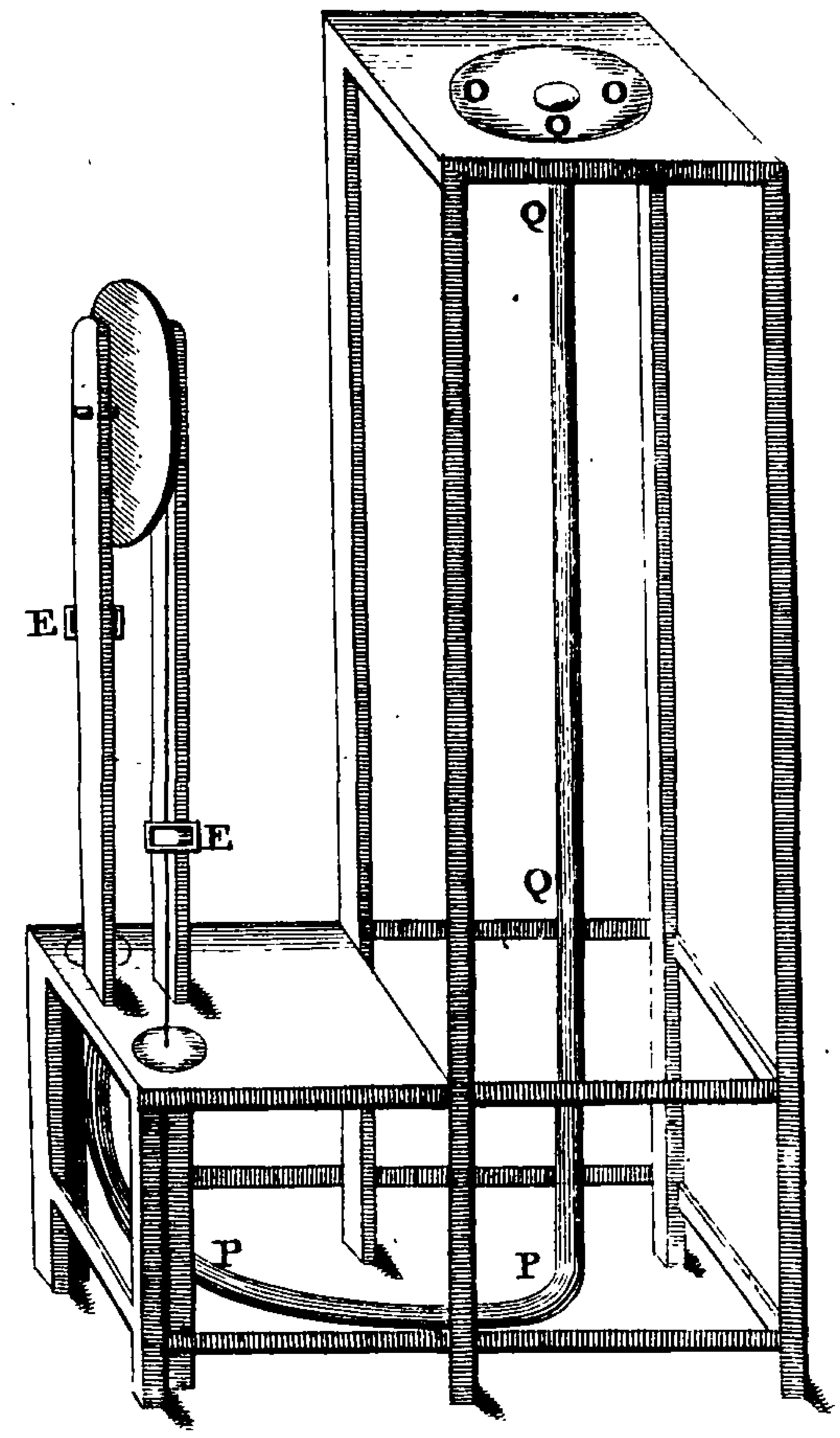


Fig. 2. p. 510.



Vol. IV. Pl. 2.

ICONISMUS 2.us

Fig. 3. p. 513.

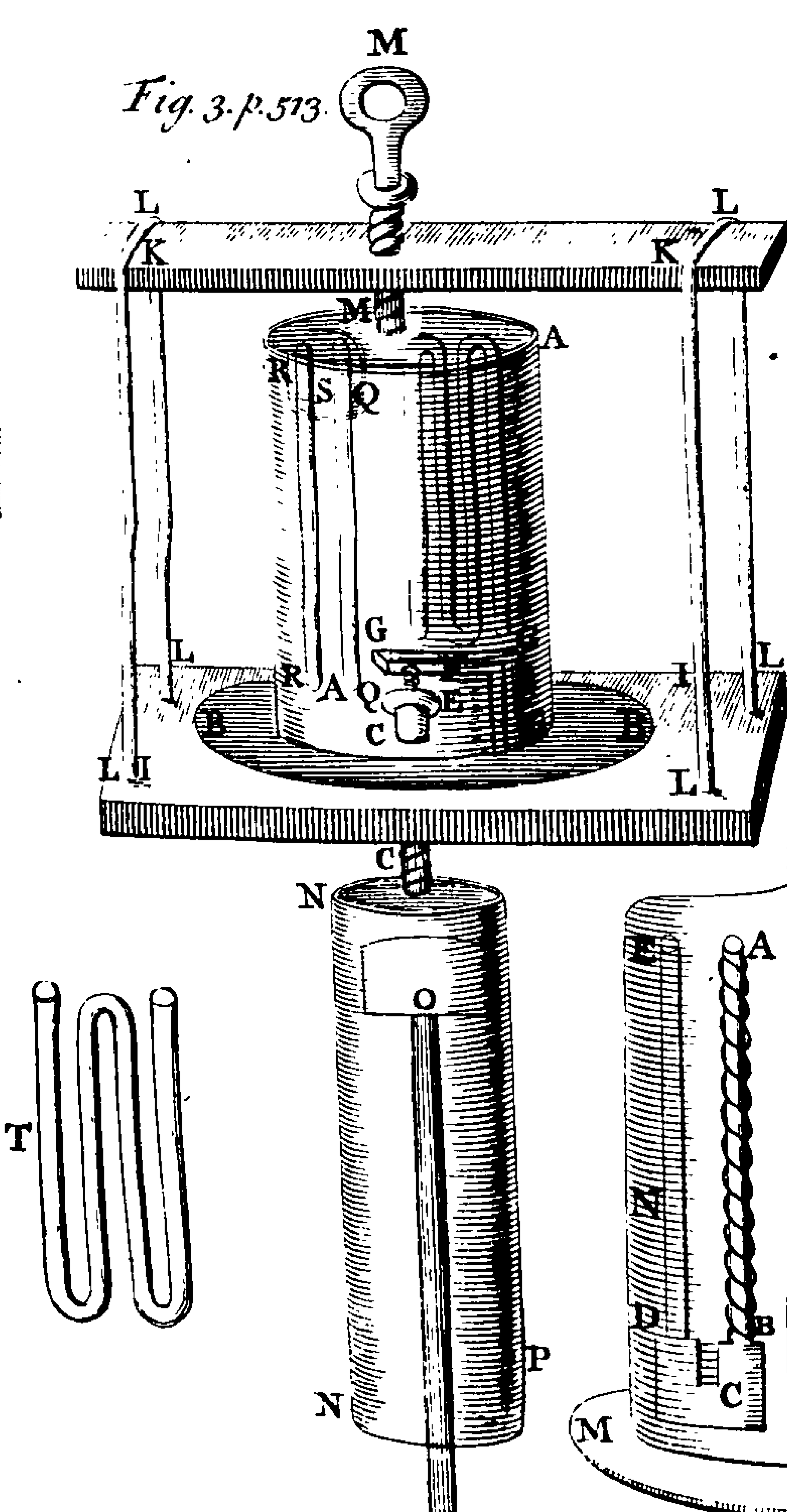


Fig. 1. p. 511.

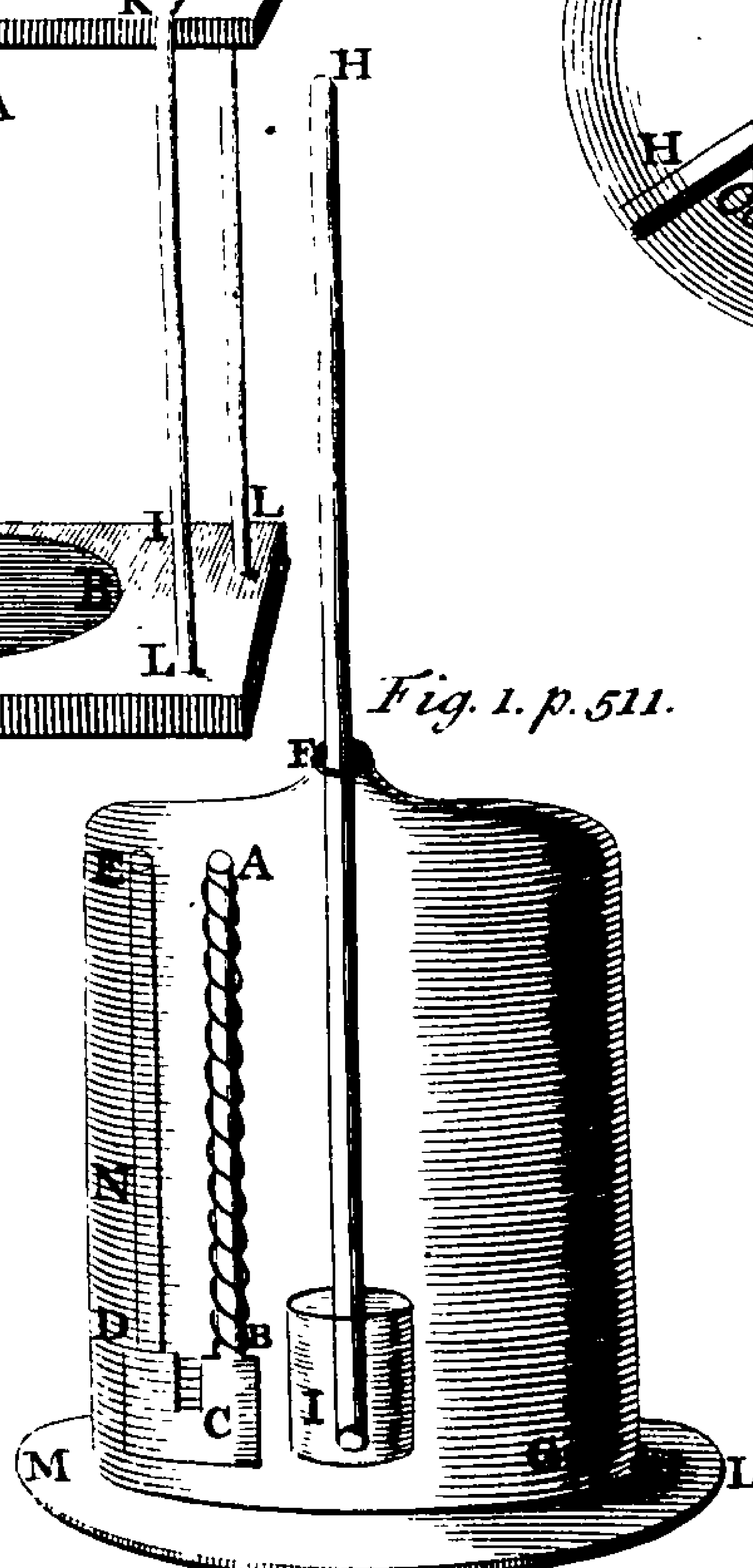


Fig. 4. p. 516.

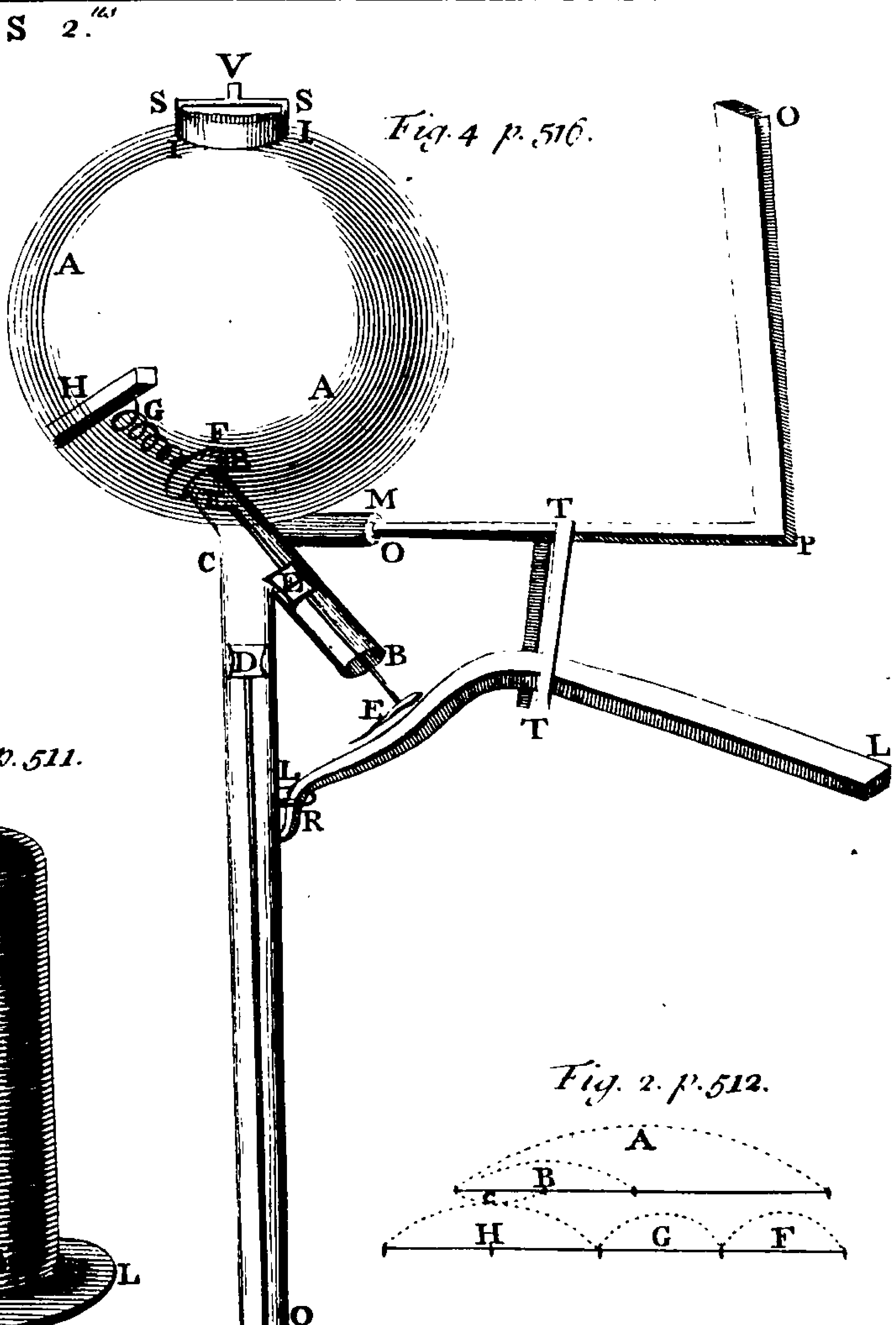


Fig. 2. p. 512.

J. Mynde fc.

the whole pillar of the air, incumbent on the plug, is to be elevated by force; and again, when the plug returns back, it is also by force to be restrained, lest it should be too swiftly impelled by the air, and so break the bottom of the engine; but in these double engines, the plyer of them is in a manner wholly free from that toil. For in the first suctions, the plugs are easily lifted up, because the air, immediately derived from the receiver R into the pumps, presseth the plugs downwards, almost as strongly as the external air incumbent on the opposite part; and when the quantity of the internal air is diminished, it comes to pass, that the plug to be depressed tends downwards with so much the greater force; and so, by means of the cord GGG compassing the pully, draws the other plug upwards, and, at the same time, hinders it from too much velocity of descent. And by this means both plugs, at one and the same time, will be helpful to him, that exerciseth the pumps.

SEEING the plugs make but a very small resistance, a man may easily judge, that the two pumps of this engine may be plyed with greater ease, and also with more speed, than one pump in single engines can; so that this engine is of great use in order to those experiments, which cannot be well made but with velocity and speed.

ICONISME II.

The description of the mercurial gage.

THE first description of a mercurial gage, to discover the degrees both of the rarefied and condensed air, may be seen about the begining of the Continuation of our Physico-mechanical Experiments; but those gages, which I used in the following experiments, are declared in the subsequent scheme.

Fig. 1. THE whole gage ABCDE consists of three glass tubes, all very well fastned and cemented together, yet so, that a passage is open from one to the other; the first of these tubes AB being open at the extreme A, is of less capacity than the tube BCD, but of greater than the tube ED. The tube BCD is crooked in the middle, and the tube ED ought to be hermetically sealed, at the extreme E, but the part BCD must first be filled with mercury.

THIS instrument, thus prepared, if it be put into a receiver, out of which the air is afterwards to be extracted, it will come to pass, that the air remaining in the part ED, will, by its spring, compress the mercury DCB, and force it to ascend into the part BA, and itsself will be dilated in the cavity DC. If then the proportions be duly observed between the bigness and length of the tubes, as shall be declared hereafter, when the air is extracted, the mercury will almost reach to the top A, and the air in the other leg, being so dilated, that in cannot sustain a greater body of mercury, will be kept included in that place.

BUT that this instrument may exactly tell the quantity of the air produced in its receiver, the tubes AB ED are to be distinguished by marks into several parts; and when the Torricellian experiment is tried, above the plain plate LM of the pneumatick engine, as you may see in the figure, a receiver FGE is to be taken, being perforated in the top F, and the tube HI is to be transmitted through the hole, that so the receiver may be applied to the plate; and then the hole F being stopped, and the gage ABCDE being put into the receiver, the air is to be exhausted; the air then being dilated in the receiver, the mercury cannot be sustained so high in the tube HI, but must descend by degrees; and at the same time the air of the tube ED, drives the mercury, by little and little, into the tube AB. When then the mercury in the tube HI descends to the height of 29 digits (I take digits for inches throughout all this tract) and stays at that height, if we mark to what height the mercury hath ascended into the tube AB, we may know, that as often as the mercury in our gage shall rest at that height, the air in the same receiver will be able to ſuſtain only 29 digits of mercury; ſo that the place in the gage, or in the paper ſemblably divided, muſt be marked with the figure 29. And ſo further, every digit of the mercury in the tube HI may be marked in our mercurial gage, and the part AB will be fit to ſhew all the degrees of the rarefied air.

But now if the air be condenſed in the receiver above its wonted preſſure, and all ways of its eſcape be ſtopped, you may immediately know it by the tube ED; for the mercury will be impelled into it by the incumbent air, through the open hole, ſo much the higher, as the compreſſion of the air in the receiver ſhall be the greater; and how great that is, and what an altitude of the mercury it can ſuſtain, may eaſily enough be found out, if the computation be made after the manner following:

It is evident, from the experiments long ſince publiſhed by Mr. *Boyle*, in his anſwer to *Linus*, that the ſpace poſſeſſed by the air, is diminiſhed in the ſame proportion, as the compreſſing force is increaſed, and *vice verſâ*.

Fig. 2. Let then (for example) the ſpace A be poſſeſſed by a certain quantity of air, when (for inſtance) the compreſſing force is F: if now we encreaſe that force by the addition of G, which is equal to it, it will happen, that our ſelf-ſame quantity of air will be reduced to half its ſpace; ſo that B, the remaining ſpace, will be the half of the total ſpace A, even as the former preſſure F is the half of total preſſure F+G. So further, if we encreaſe the preſſure more, by the addition of H, ſo that the firſt preſſure F is only $\frac{1}{4}$ of the total preſſure F+G+H, it will come to paſs, that the air can poſſeſs only the ſpace C, which is $\frac{1}{4}$ of the total ſpace A. And ſo afterwards, the remaining ſpace will be in the ſame proportion to the total ſpace, as the firſt preſſure is to the total preſſure.

The remaining ſpace : the total ſpace :: the firſt preſſure : the total preſſure.

So that three of thoſe terms or quantities being known, it will be eaſy to find out a fourth, by the rule of proportion. For inſtance, in our gage let the tube ED be the total ſpace, in which the air is compreſſed by the wonted preſſure of the air, which in *England* is wont to be equivalent to 30 digits of mercury, or thereabouts; and therefore the firſt preſſure will be 30 digits of mercury. Now, if that preſſure be encreaſed, and the air be reduced into a narrower ſpace, ſuppoſe into the ſpace NE; if I would find out the quantity of this preſſure, I meaſure the remaining ſpace NE exactly, and I conſtitute that, ſuppoſe ſix digits or inches, for the firſt term of proportion; the ſecond term will be the total ſpace DE, ſuppoſe 12 digits; the third term will be the height of 30 digits of the mercury, which was the firſt preſſure; and ſo the fourth term, or total preſſure will be found to be 60 digits of mercury; whence I may conclude, that the preſſure of the air in the receiver can ſuſtain the mercury to the height of 60 digits: and ſo of the reſt.

From the ſame principle before laid down, it will be eaſy to collect, what ought to be the proportion between the largeneſs of the tubes AB and ED. For that depends on the length of the legs, which the higher they are, ſo much the better they can reſtrain and keep in the air, being but a little dilated in the ſealed part. For inſtance, let the length AB be of 10 inches, which height of the mercury is $\frac{1}{3}$ of the accuſtomed preſſure, it will be ſufficient, that the tube HB be twice as big as the tube ED; for after the mercury hath aſcended to the top of the tube AB, the air included in the other leg, expanding itſelf into the ſpace, forſaken by the mercury, will poſſeſs three times more than its former ſpace, and ſo $\frac{1}{3}$ of the firſt preſſure, which is 10 digits, will be ſufficient to curb its ſpring. But if the legs were of leſs length, then the mercury would be expelled by the included air, at leaſt in part. And therefore the bigneſs of the tube AB ought to have a greater proportion to the bigneſs

bigneſs of the tube ED, that the aſcending mercury may afford greater place to the air to be dilated, and ſo, the ſpring of the air being weakened, the weight of the mercury cannot be overcome. And that would happen ſo, if the height of the gage be to the height of 30 digits, in the ſame proportion which the firſt ſpace of the air is in, to the total ſpace, which the air would poſſeſs *in vacuo*, according to the principle before laid down.

It is better, that the height of the tube be longer than ſhorter; becauſe if it be ſhorter, the mercury will be expelled in part, and ſo will not be able to ſhew all the degrees of rarefaction; but if it be longer, this only will happen, that the mercury will not reach to the top, and ſo the gage will nevertheleſs indicate all the variations, though they be leſs ſenſible ones.

But the tube DC ought to contain that quantity of mercury, at the leaſt, which may be ſufficient to fill the tube AB, before any way of eruption be opened for the air included in the tube ED. If the capacity of it be much greater, the matter is not much; nor need we be very ſolicitous concerning the figure of this tube.

ICONISME II.

A deſcription of the engine to compreſs the air.

Fig. 3. AA Is a glaſs veſſel, whoſe orifice is exquiſitely fitted to the plain plate BB.

BB Is a plain plate of braſs, made to cover the veſſel AA exactly.

CC Is a ſmall tube of braſs, paſſing through the middle of the ſaid plate, and faſtened thereunto.

E Is a little valve, opening inwardly, to ſhut the ſmall tube C aforeſaid.

F Is the ſpring depreſſing the valve E.

GGG Is the gnomon faſtened to the plate BB, made for reſtraining the ſpring F.

II Is a ſquare lath, ſuſtaining the plate BB, and bored through in the middle to tranſmit the little tube C.

LLL LLL Are two iron wires, which paſſing through the holes in the lath II, and compaſſing the upper part of the iron plate KK, do hinder the ſaid plate, that it cannot be much moved from the lath.

KK Is an iron plate, with a hole in the middle, formed into a female-ſcrew, to receive the male-ſcrew MM.

MM Is an iron ſcrew, whoſe uſe is, ſtraitly to conjoin the receiver AA with the plate BB. And leſt the braſs veſſel ſhould be broken, it is convenient to put ſome wood with leather between the ſcrew and the upper part of the receiver: alſo leather is to be put upon the plate BB, both to prevent the breaking of the glaſs, and alſo for the more exact ſhutting of the receiver.

NN Is a pump faſtened to the tube C, below the plate BB.

OO Is the ſucker or plug of the pump NN.

P Is a little hole in the lower part of the pump, by which the air enters into it, when the plug is brought to the loweſt part thereof.

Now if we would compreſs the air by the help of this engine, we put the bodies, about which the experiment is to be made, into the receiver AA; and laying it on the plate BB, we firmly bind it thereto by the help of the ſcrew MM. This being done, the ſucker or plug OO is to be drawn, till the external air, by the hole P, can fill all the upper part of the pump: then if the plug be drawn upwards, it will come to paſs, that the air, finding no other way of egreſs, will open the valve E, and enter into the receiver AA, from whence there is no regreſs, becauſe the valve E is preſently depreſſed by the ſpring F, and doth ſhut the hole C. And ſo we may iterate the compreſſion of the air into the veſſel AA, as often as we pleaſe, and the quantity thereof is eaſily known by the mercurial gages.

But I am wont ſo to faſhion the pump, that it may be fitted by a ſcrew to the tube C; for ſo when one receiver is full, we may take away the pump, and uſe it to fill other receivers.

Now,

Now, becaufe in thefe engines mercurial gages are ufed only to fhew the degrees of compreffion, there is no need of ufing the gages here, which are defcribed in the firft figure; for they are made with more difficulty, and befides, they afford but a fmall fpace to note the degrees of compreffion in. And therefore it is better to fold the glafs tube, fealed at one end, in feveral places, as the figure T fhews, that a long tube may be contained in a fhorter receiver; fo that the mercury being put in, through the open end, as much as will fuffice to fill the length of one digit, all the reft of the fpace filled with air, will ferve for the making of the degrees of compreffion, much more fenfibly than can be done in a fhorter tube.

HERE we muft note, that when the mercury tends downwards in fuch an inflected gage, the weight thereof doth help the external preffure; but when it is impelled upwards, the fame weight makes refiftance: this difference muft be heeded, if we have a mind to try very accurate experiments.

ICONISME II.

How mixtures may be made in compreffed air.

Fig. 3. LET the receiver be AA, in which we have a mind to mix either liquors or powders.

LET QQ RR be two tubes, each of them, fealed at one end, and open at the other.

LET RQS be a veffel of brafs, to be laid upon the orifice of the tubes, as is fhewed in the figure.

THE liquors to be mixed muft be poured into the tubes QQ RR, each liquor in his own tube, and let the veffel inverted RQS, be laid on the orifices of the tubes, and in that pofture let all be covered with the receiver AA; let the fcrew be wrung or ftraitened, and the air intruded after the manner defcribed fol. 513. And when you fhall underftand by the gage TT, that the compreffion is arrived at that degree, which you intend, the engine is to be inverted, and fo the liquors will flow down from the tubes into the veffel RQS, and be mixed there. If you defire to mix more liquors or powders, then the number of the tubes is to be encreafed accordingly.

ICONISME III.

How factitious air may be tranfmitted out of one receiver into another.

I tried two ways (principally) to tranfmit air out of one receiver into another; but becaufe the firft of them feemed lefs convenient, I fhall here only defcribe the latter.

AA Is a plain plate made of metal, having an hole in the middle.

BB Is the ftop-cock faftened to the hole in the middle of the plate AA, one of whofe ends is formed into a male-fcrew.

DC Is a copper funnel open below, with a broad orifice (that fo it might be eafily fet upon the pneumatick engine, and there ftand firm) and in the upper part the orifice D is fafhioned into a female-fcrew, to receive the male-fcrew of the ftop-cock BB.

Fig. 2. EE Is a fmall tube, open at both ends, both whofe orifices are excavated in a female-fcrew, to receive the male-fcrew of the ftop-cock BB.

Fig 1. FF Is the receiver laid on the plate AA, and exquifitely fitted thereunto.

Now, if we would make factitious air, we muft put the matter, which is to produce the air, into the receiver FF, and placing the faid receiver on the plate AA, by means of the fcrew, we muft ftrongly faften it thereto, after the fame manner as hath been defcribed in our engine for compreffing the air; and the ftop-cock BB we infert into the female fcrew D; then the orifice C, and with it the receiver, is to be placed upon the pneumatick engine, and the ftop-cock B being opened, the air is to be extracted; when the receiver FF is emptied of air, the ftop-cock B is to be fhut, that fo all paffage of external air into the receiver may be intercluded, and the ftop-

Pl. 3. ICONISMUS 3.us

Fig. 1. p.514

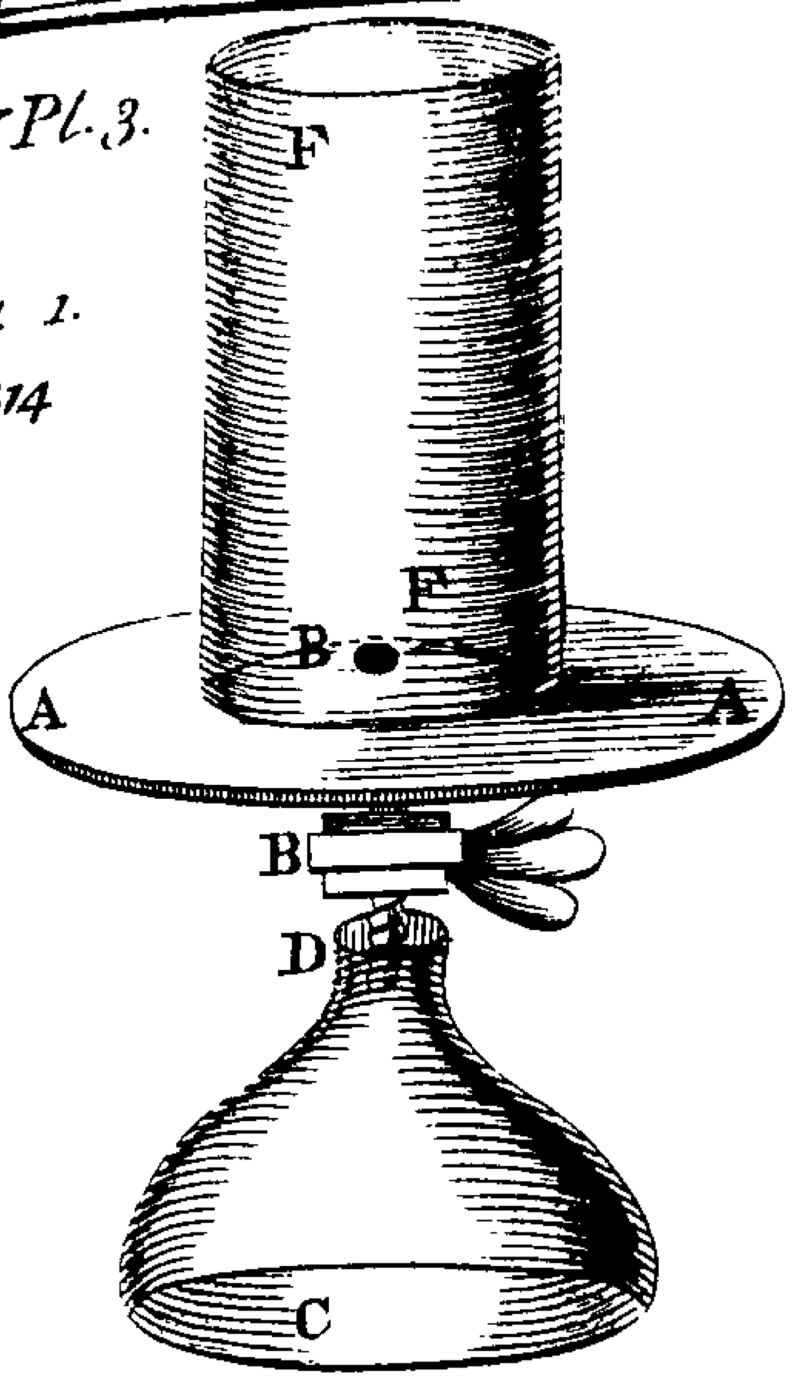


Fig. 2. p. 514

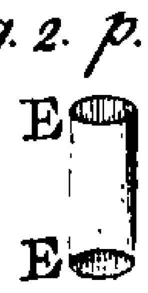


Fig. 4. p. 515

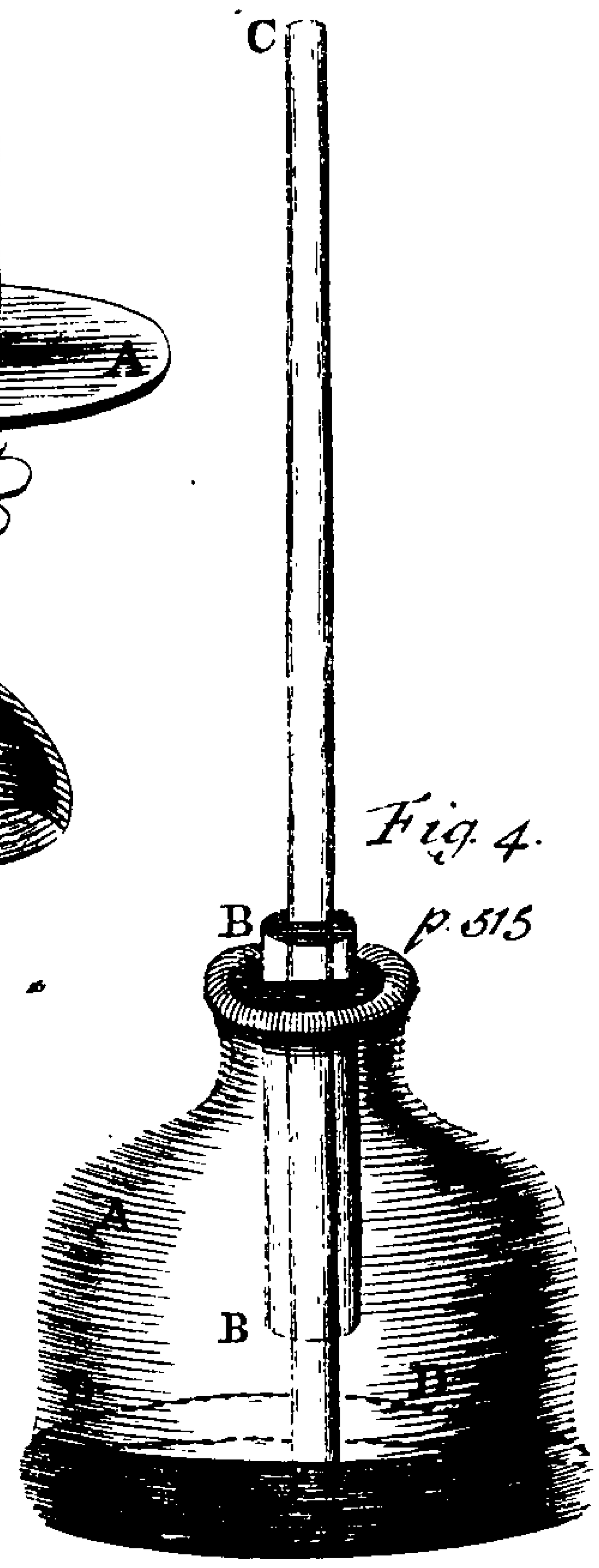
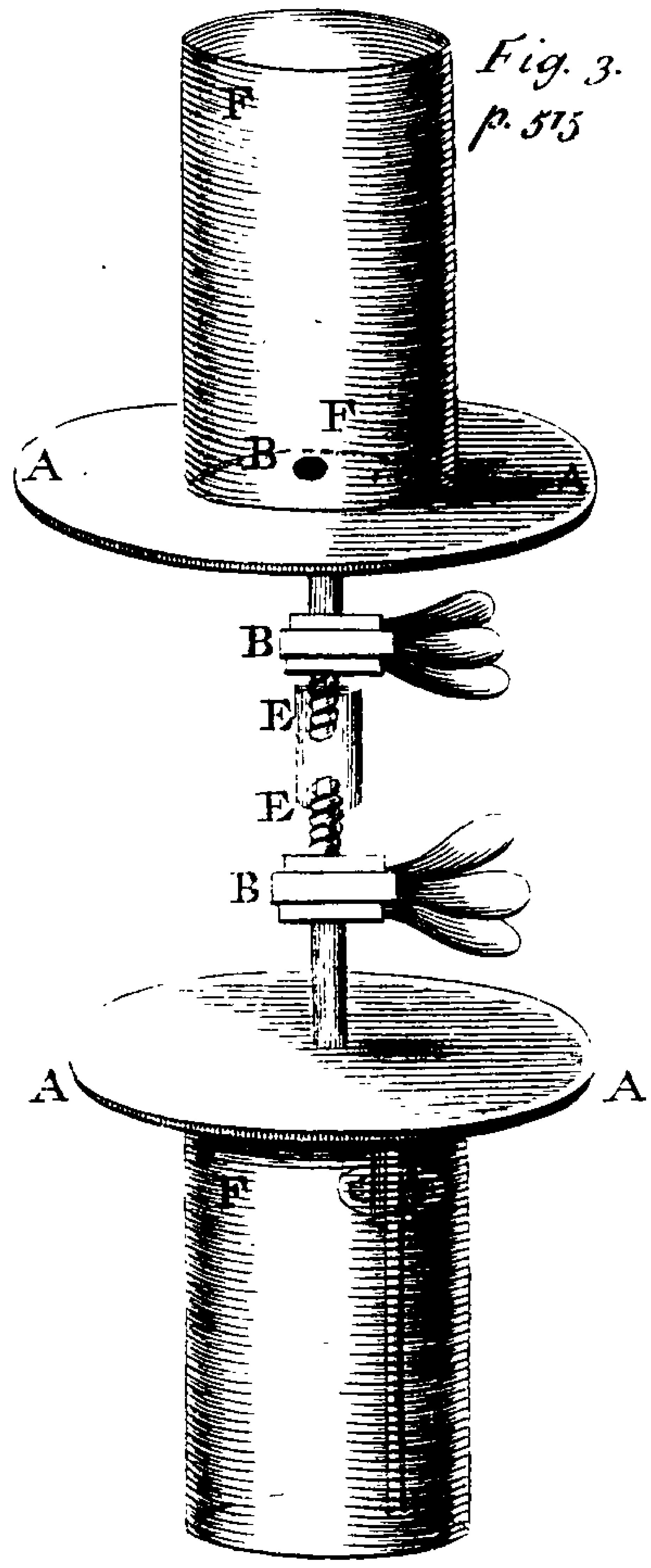


Fig. 3. p. 515



IV. Pl. 4. ICONISMUS 4.us

Fig. 1. p. 515.

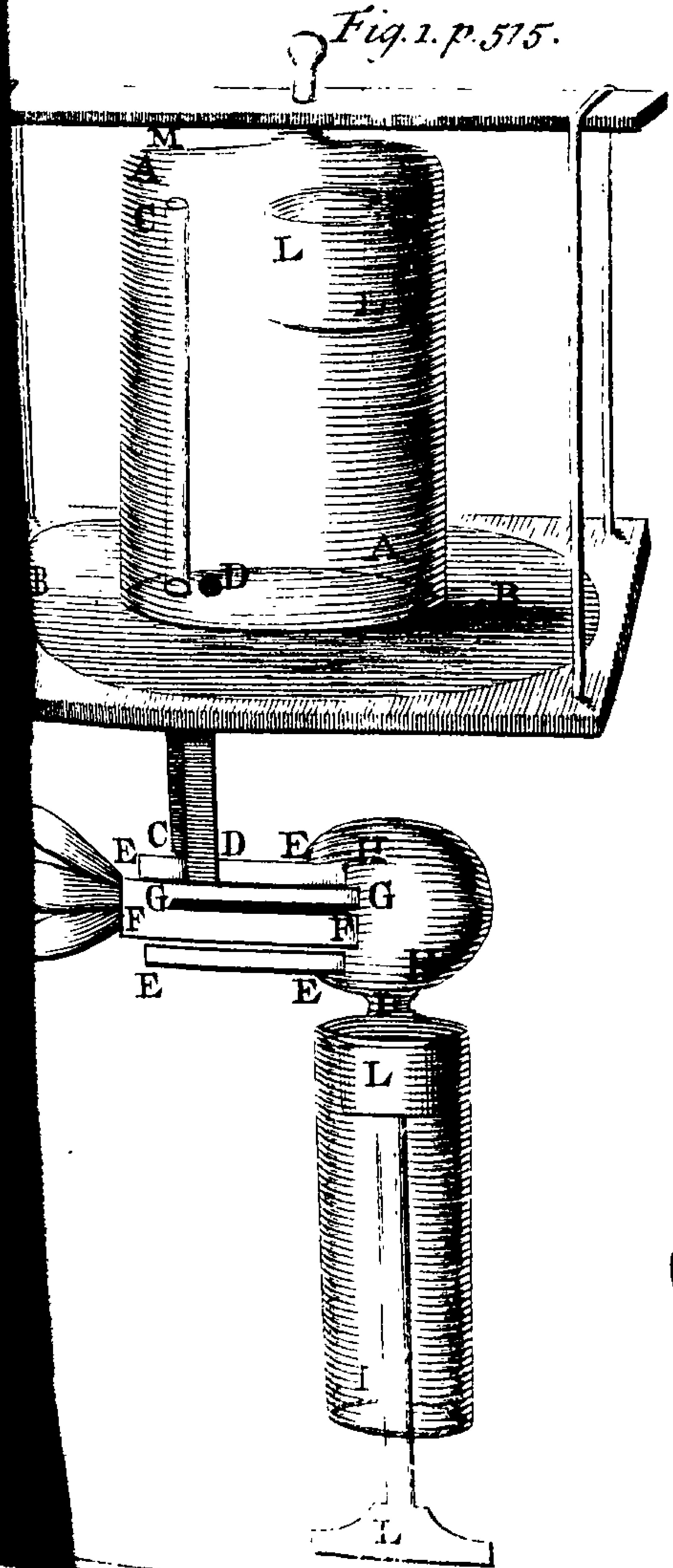


Fig. 2. p. 516.

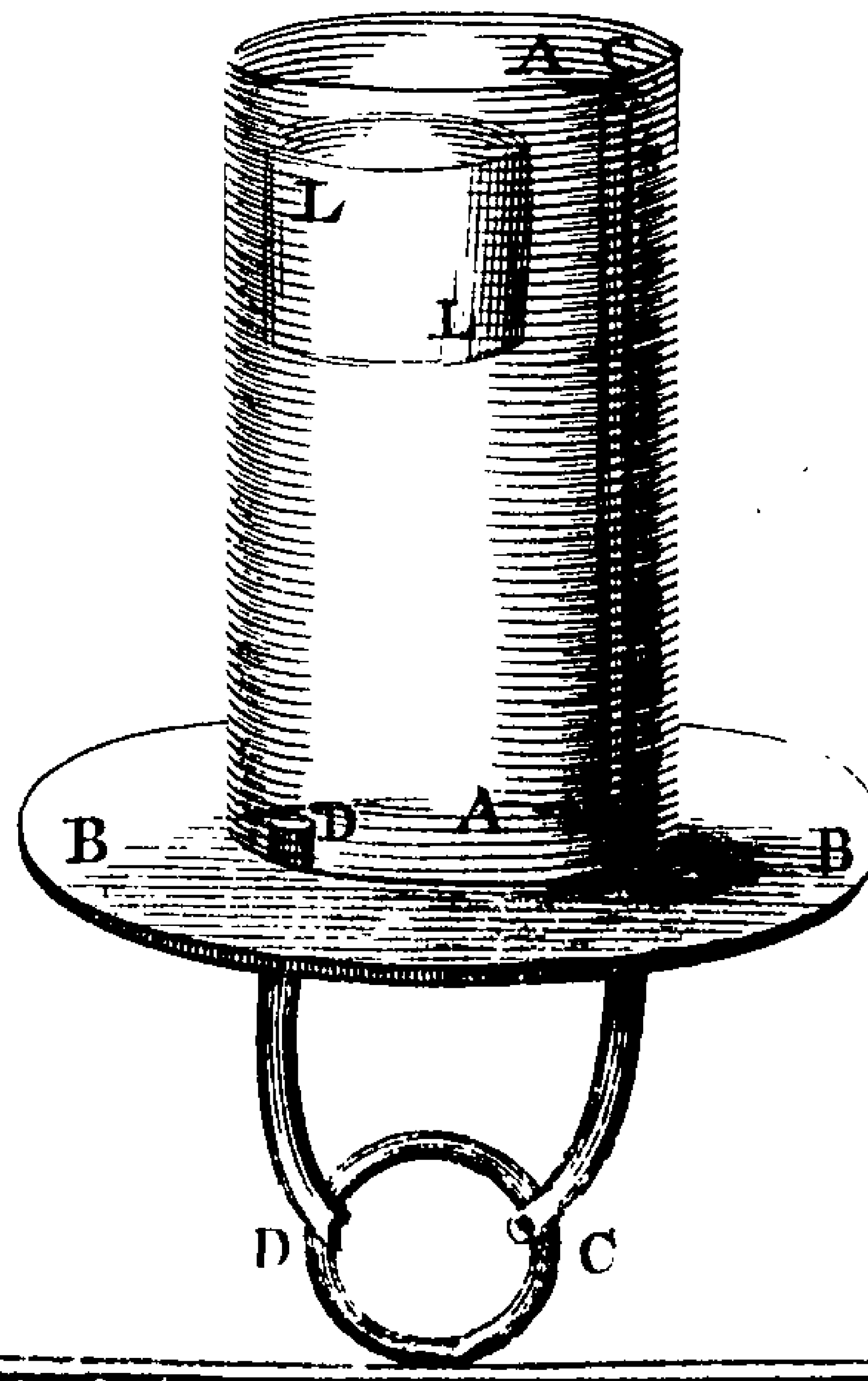
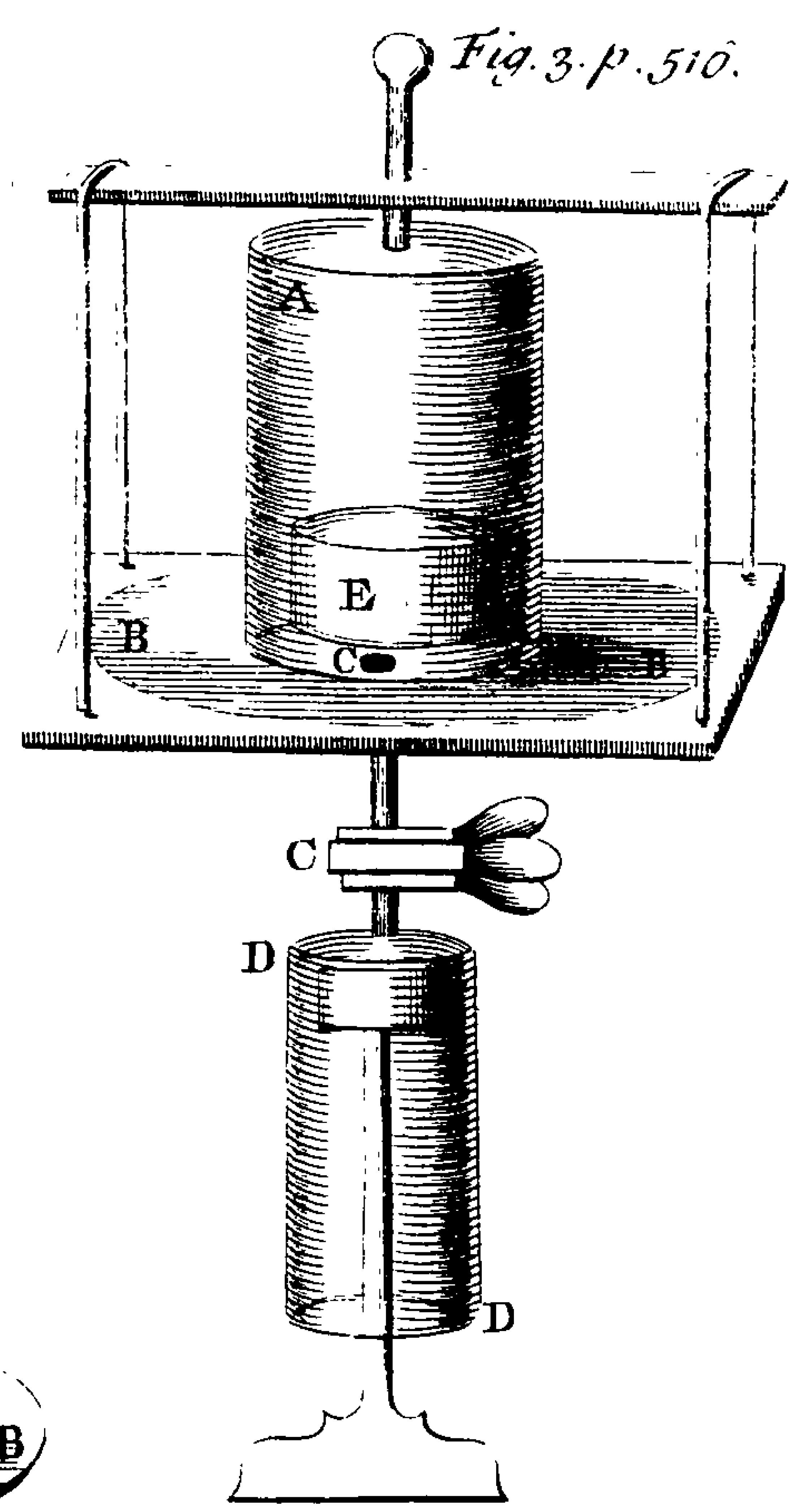


Fig. 3. p. 516.



J. Mynde sc.

ftop-cock being taken out from the female-fcrew D, the receiver is prefently to be immerged in water, fo that at leaft the plate AA, with the ftop-cock, may be covered therewith; for fo it will be clear, that no air from without can find ingrefs, and the air produced out of the matter included in the receiver will be preferved unmixed, and the degrees of its rarefaction or compreffion are known after the fame manner, as hath been defcribed p. 512.

Fig. 3. Now if we would tranfmit that air into another receiver; another receiver FF with another plate AA, and a ftop-cock BB is to be procured and excavated after the fame manner, as was before defcribed; then, by means of the fmall tube EE, we join the ftop-cocks BB of both receivers, as is fhewn in fig. 3, and all fufpected places are to be ftopped with cement or turpentine, that no external air may find admiffion; then, the ftop-cocks being opened, the air produced in the former receiver flows into the latter, and the ftop-cocks being again fhut and plucked out from the tube EE, the receivers may be kept apart; and if there be any matter included in the latter receiver, we may eafily view what influence the factitious air hath upon it.

BUT becaufe the mercurial gages defcribed fol. 511. are fpoiled, if they be inverted, and the gages, mentioned fol. 514, do prefently expel their mercury, if the air be rarefied in their receivers; and feeing the operation, here defcribed, cannot be perfected, but both receivers muft be inverted, and both likewife emptied of air; we muft make gages of another fort, after the manner following. See fig. 4.

AA Is a glafs phial filled with mercury to the fuperficies DD, or thereabout.

BB Is a glafs tube very well cemented, in the orifice of the phial.

CC Is another tube tranfmitted through the tube BB, and reaching to the bottom of the glafs. This tube muft be fealed above and open below; neither muft it fo exactly fill the tube BB, but that paffage may be opened to the external air within the glafs AA.

Now if you put this inftrument into a receiver, from which the air muft be afterwards extracted, it will come to pafs, that both tubes will be exhaufted of air; and when you invert the receiver, to take in new air, as in fig. 3. is declared, the mercury will flow down to the orifices of the phial, and will be there kept below the orifice of the tube BB; and the new air entring, will eafily fill both tubes and phial: then the receiver being erected, the mercury will again be ftagnant in the bottom of the phial, and the orifice of the tube CC will be found demerfed in it. Then if any air be produced out of the bodies included in the fame receiver, it will come to pafs, that the mercury will afcend into the tube CC, and there, reducing the air into a narrower place, will fhew the degrees of compreffion.

Note, that almoft all the kinds of factitious air in the begining are in part deftroyed, that therefore the degrees of compreffion cannot here be fo exactly known, unlefs we know by experiments, what part of the air is wont to be deftroyed.

ICONISME IV.

An inftrument, by which air may be filtrated through water.

Fig. 1. AA Is a glafs receiver, whofe orifice, laid upon the plate BB, agrees exquifitely therewith.

BB Is a plain plate, with an hole in the middle, to tranfmit the tubes CC DD.

CC DD ARE two tubes cemented to the plate BB, one of which is no higher than the plate, but the other reacheth almoft to the top of the receiver.

EEEE Is a ftop-cock, to whofe holes the extremities of the tubes CC DD are faftned.

FF Is the key of the ftop-cock unperforated, wherein only one chinck GG is excavated.

HH Is the receiver, compaffing the end of of the ſtop-cock, and faſtned to it, ſerving againſt the ingreſs of the outer air, and communicating with the pump II.

LL Is a glaſs veſſel.

M Is a hole in the top of the receiver, whoſe ſtopple is faſtned with a ſcrew.

IN the ſecond figure there is exhibited a ſtop-cock, cut tranſverſly, that the two tubes CC DD may be the better diſtinguiſhed, and their inſertion into the ſtop-cock be perceived.

THIS inſtrument is thus to be uſed: we put the thing, about which the experiment is to be made, into the veſſel; and the receiver AA being laid on the plate BB, we pour water into the hole M till the receiver be half full, or thereabouts, and the veſſel LL, with the matter contained therein, do ſwim on the top thereof; then we ſtop the hole exactly, and faſten it with a ſcrew, in the ſame manner as hath been deſcribed in the firſt ſcheme. Theſe things being thus prepared, the key is to be ſet in that poſture that the chink G G may communicate with the tube CC; then the plug being brought to the loweſt part of the pump, the air of the receiver AA, entering through the upper orifice of the tube CC, will flow down through the chink GG into the receiver HH, and into the pump. Then the key being inverted, ſo that the chink GG do anſwer to the inſertion of the tube DD, the plug is to be impelled upward, and then the air will be expelled from thence, and, finding no other paſſage, will be driven through the chink GG into the tube DD; and from thence will emerge to the upper part through the water ſtagnant in the receiver. Iterating this labour, we ſtrain the air through the water, as often as we pleaſe; and by this means, we know, whether it be cloathed with any new qualities, in reſpect of the body included with it.

ICONISME IV.

How the ſame numerical air may be ſometimes condenſed, ſometimes rarefied.

LET the receiver AA be placed upon the plate BB, and ſcrewed in, as is deſcribed, fol. 102.

CC Is the ſtop-cock, faſtned to the hole in the midſt of the plate BB.

DD Is a pump joined to the ſtop-cock C with a ſcrew.

E Is a veſſel of that bigneſs, that it may fluctuate in the receiver AA without danger of inverſion.

LET ſome animal be put into the veſſel E, and let the receiver AA be put upon it and ſcrewed to it, as the ſcheme ſhews. Then let the pump be filled with water, and by a ſcrew fitted to the ſtop-cock; the ſtop-cock being then opened, let the plug P be forced upwards, then the water aſcending through the ſtop-cock, will, in part, fill the receiver AA, and will reduce the air, contained therein, into a narrower ſpace, without any addition of new air; if then you draw the plug downwards, the ſame numerical air will be again rarefied. Thus you may both condenſe and rarefy the ſame air as often as you pleaſe; and by this means you may find out, whether the condenſation of the air do contribute any thing to prolong the life or health of animals, yea or no?

ICONISME II.

The deſcription of a wind-gun.

Fig. 4. AA Is a copper globe, hollow within.

BB Is a tube, faſtned to the globe.

F Is a valve opening inwardly, and ſhutting the globe BB.

G Is the ſpring depreſſing the foreſaid valve.

H Is a gnomon affixed to the globe AA, and making faſt the ſpring G.

CC Is a tube of iron, faſtened to the tube BB and the globe AA.

DD Is a plug exactly fitted to the foreſaid tube.

EEE Is another plug fitted alſo to the tube BB with an iron wire, reaching almoſt to the valve F.

R Is the protuberance of the tube CC, ſomewhat hollowed above to receive the end of the iron LL.

LL Is a crooked iron, moveable about the extremity in R, ſo that it is like a leaver to lift up the plug EEE.

OPO

OPO Is a crooked iron, faſtned in M, that the thumb ſticking in the angle P, the reſt of the fingers may attract the leaver L, and ſo force the plug EEE upwards. But the curvature is made for this uſe, that the one end O might be applied to the ſhoulder, if it be thought fit to aim at any mark.

TT Is a rectangle of iron, compaſſing the leaver LL and the iron OPO, to keep the leaver in that poſture, which the preſent ſcheme holds forth; for otherwiſe the plug EEE, would be thruſt out far away, whilſt we intrude the air into the globe AA.

II Is an elliptic hole, in the upper part of the globe, very well ſhut with a valve, opening inwardly, whoſe uſe is to give liberty of inſpection, and of amending what is amiſs; for the valve may be drawn through the hole by reaſon of its elliptic figure.

SS Is a metalline plate tranſverſly placed above the hole II, and perforated to tranſmit the ſcrew V, by whoſe help the valve ſhutting, the hole II is ſuſtained and is applied cloſely to the hole.

Q Is an hole in the inferior part of the tube CC, by which the air enters into the tube, whilſt the plug D is brought to the loweſt part of the tube.

THE air is thruſt into this engine after this ſort; I tread with my foot upon the crooked end of the plug DD, that it may not be removed from the ground, and I lift the engine upward, till the upper part of the plug be found below the hole Q, and then the air entring through the foreſaid hole, doth wholly fill the tube CC.

THEN I forceably depreſs the engine, and ſo the air, contained in the tube CC, opens the valve F, and is thruſt into the globe AA; whence it cannot return, becauſe the ſaid valves preſently ſtop the paſſage; and thus by iterated turns, we may condenſe the air in the globe, until the force of its ſpring cannot be overcome by our ſtrength.

Now if we would diſcharge the air, ſo condenſed, the plug DD is wholly to be drawn out, and a bullet of lead to be put into the bottom of the tube CC: then by means of the leaver LLL the plug EEE is to be impelled upward, as we ſaid before, and then the extremity of the iron wire opens the valve B, and the air breaking out therefrom, expels the leaden bullet through the tube CC with great violence.

NOTE, that before the plug DD is again put into the tube CC for the compreſſion of the air, about half an ounce of water is to be poured into the ſaid tube. For by this means no air at all can eſcape out by the plug, and moreover, that water exactly filling the upper part of the tube CC, will cauſe, that the whole compreſſed air will be intruded within the cavity AA, and ſo the condenſation will be perfected much ſooner, than if, at every turn, part of the compreſſed air did remain below the valve F.

THIS engine is much better than any wind-guns hitherto mentioned in print.

1. BECAUSE that ſeeing only one valve ſerves, both for the letting in and diſcharging forth of the air, it is leſs ſubject to be ſpoiled or impaired, than if two valves were uſed for that purpoſe.

2. IF any diſorder happen in other guns, the engine remains uſeleſs; but here, by the elliptic hole, a man may take out the ſpring and the valve, and ſo mend whatſoever is amiſs.

3. IN other guns, the valves being covered with leather were put in before the engine was on every ſide ſhut, and therefore ſilver ſoder could not be uſed in cementing the parts, but only lead ſoder, by which the air, being much compreſſed, could by no means be reſtrained; but here all things are well cemented with ſilver ſoder, without danger of burning, in regard the valve covered with leather is put in afterwards through the elliptic hole II.

4. BUT this engine is chiefly to be preferred before others on this account, becauſe we emit ſeveral bodies into the receiver, through the elliptic hole, and ſo make many experiments in highly compreſſed air.

ICONISME V.

An instrument to distill in vacuo.

Fig. 1. AA Is a brass vessel, shut below and open above.

BB Is a diaphragma or midriff of tin, whose edges are so polished on both sides, that they exquisitely do agree and suit with the edges of the vessels AA DD, which are also polished, and so keep the external air from ingress.

CC Is a tube fastened to a hole in the middle of the diaphragma BB.

DD Is a brass vessel, whose aperture is applied to the diaphragma BB.

EE Is a stop-cock fastned to the hole of the diaphragma BB.

FF Is a tube reaching from the stop-cock EE to the hole for suction in the pneumatic engine.

GG Is a metalline vessel shutting in the commissures of the vessels with the diaphragma, and also the stop-cock, that it, being filled with water, may keep all safe from the external air. This vessel is to be sodred to the vessel AA.

We use this engine after the following manner: taking away the diaphragma BB, we put the things to be boiled into the vessel AA, and so set it in a convenient place, that it be not shaken, whilst it is evacuated; then putting on the diaphragma BB and the vessel DD, we put to the pneumatic engine, and making use of the tube FF, the air is pumped out of the vessels, the vessel GG being yet first filled with water. Then the stop-cock is to be shut, and taking away the tube FF, we may place the evacuated engine on the fire, and the vapours ascending through the tube CC, are condensed in the upper vessel, and so we have a liquor distilled *in vacuo*; and the quantity of the generated air, is known by the mercurial gage H, but that must be kept up in the top of the receiver, lest the mercury do exhale, by reason of too much heat.

Note, that round pieces of paper, perforated in the middle, are to be laid over the orifices of the vessels AA DD, to the end they may be better joined with the diaphragma; and the commissures of the tube FF with the stop-cock and pneumatic engine are to be fortified with cement, and the stop-cock EE is so to be disposed with the vessel GG, that part of the key may be prominent without the vessel through the hole, that so it may conveniently be turned, and yet nevertheless, the stop-cock, with the diaphragma, may be taken out of the vessel GG, whilst the vessel AA is to be filled with flesh or any other matter. And that is very easily done in this manner: the key consists of two parts, one of which M is turned in the stop-cock itself, by means of a certain chink, which receives the small protuberance of the other part OO, which other part doth exactly fill the small pipe NN, fastned to the vessel GG, and being prominent outwardly may easily be turned in it, and communicate its motions to the other part M, but it is drawn outward whilst the diaphragma BB is to be taken out of the vessel GG.

Fig. 2. Shews you another instrument, herein differing from the former, that it is almost all of glass, and affords a longer passage for the vapours.

BB Is not a diaphragma, but only a small tube, polished at both ends, that it may exquisitely suit with the orifices of the vessels A and D.

AA DD Are two vessels, whose orifices are applied to the tube BB, and so the vapours are easily transmitted from the one to the other.

EE FF GG I, have the same use as in the former scheme, and the whole instrument is to be evacuated after the same manner, and placed upon the fire; except, that here the vessel AA, as being made of glass, must not be put on an open fire, but *in balneo Mariæ*, or on sand, and the vapours will be condensed in the vessel DD.

ARTI-

L.IV. *Pl. 5.*

ICONISMUS 5^us^

Fig. 1. p. 518.

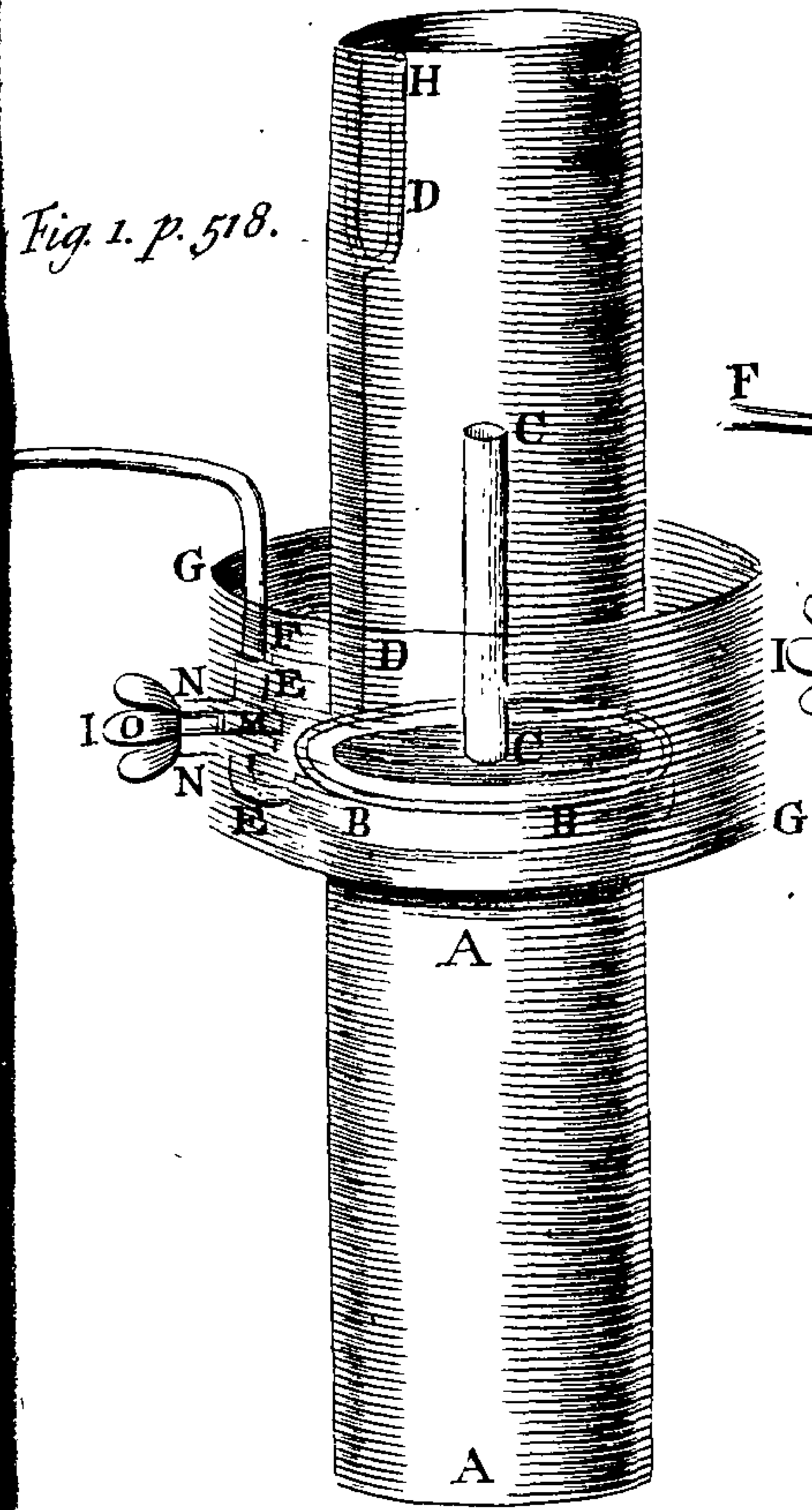
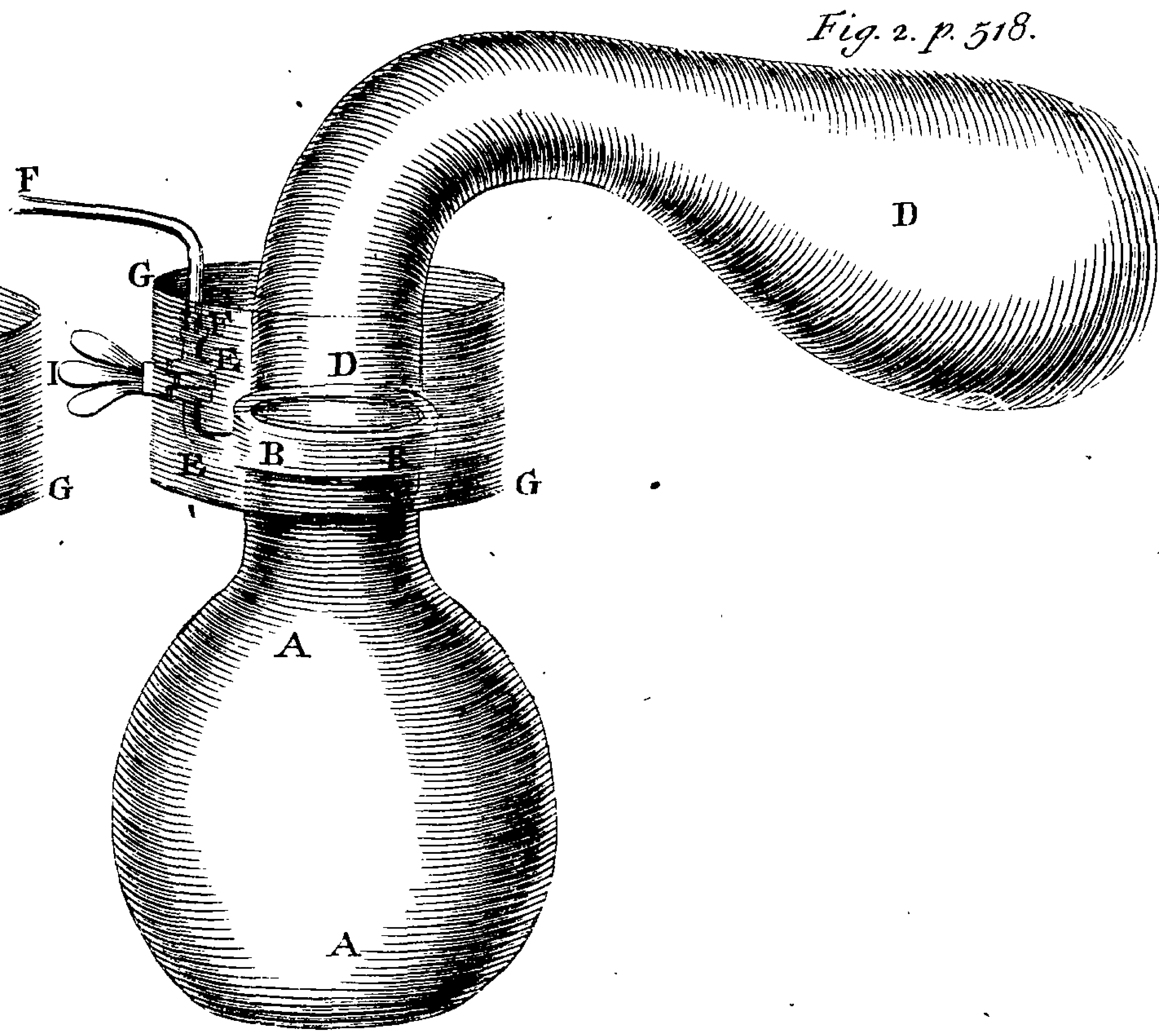
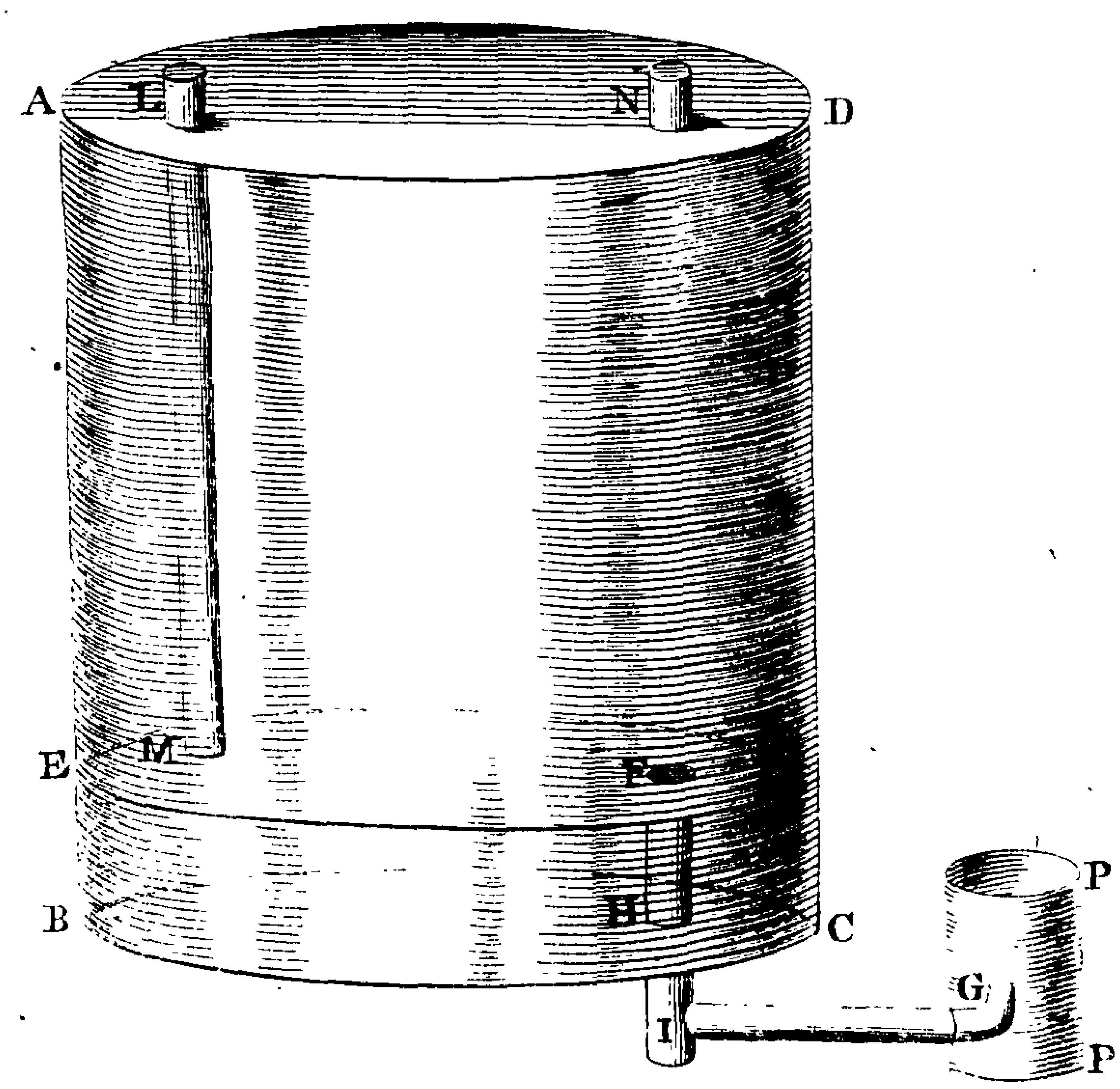


Fig. 2. p. 518.



L.IV. *Pl. 6.* *p. 405.*



J. Mynde f.

ARTICLE I.

Several ways used to help the production of the air.

EXPERIMENT I.

July 11, 1676. BECAUSE it appears by the new experiments published at *Paris*, in the year 1674, and which are to be sold by *John Cusson* in St. *James's-street*, that bread alone can produce no air *in vacuo*, we were willing to try, whether yet it did not contain some air, which might come forth some other way. I therefore included a little piece of bread, very moist, and a little kneaded, *in vacuo* with a mercurial gage.

July 12. IN six hours space no air was produced yesterday, but this night a little brake into the receiver, as much as did suffice to sustain three digits of mercury: the reason was, because I had neglected to fortify the cover with turpentine.

Towards the evening, I found the mercury higher, by one inch, or thereabout, and I am very certain, that nothing had entered from without.

July 13. This night also the mercury ascended higher, but my gage was not of that sort, as exactly to discover many degrees.

July 26. THIS day the piece of bread dis-joined its receiver from the cover, by the force of the produced air, and the smell of it was acid.

HENCE it follows, that water is a fit dissolvent to draw forth air out of bread.

EXPERIMENT II.

July 11. I tried another way to extract air from bread, for by the help of a burning glass I burnt bread *in vacuo*, and so I found, that the bread did generate much air, and that air did ever and anon break out, as by fulmination: whence it seems probable, that air is contained in bread, but it is so closely coarctated therein, that no easy operation can give it a discharge; but if any thing could dissolve and loose that knot, it may then produce great effects.

EXPERIMENT III.

Sept. 22. I took eight ounces of dried grapes, and, with seven ounces of water, included them in a receiver, able to hold twenty-two ounces of water; the grapes were bruised.

Sept. 23. THE receiver was demersed under the water all this night, yet the mercury ascended two whole inches.

Sept. 30. IN seven days space, the mercury came to the height of thirteen inches.

Octob. 5. IN five days space, the mercury ran up twelve inches, and was now twenty-five inches high.

Octob. 18. THE mercury did not proceed to ascend with the same swiftness, and the air began to pass out of the receiver, but not before this day; yet these grapes produced much more air than those, which I had included without water. See *Art.* IX. *Exper.* I.

EXPERIMENT IV.

July 12. I included of raisins of the sun bruised ten ounces *in vacuo*, with a sufficient quantity of water to promote fermentation.

July 14. IN two days space, the raisins had produced ten inches of air.

ABOUT the evening the mercury was about fifteen inches high: the fifteenth day, the mercury had almost reached to its accustomed height.

July 16. THIS day, in the morning, I found the receiver severed from its cover, and the air breaking forth through the water, in which it was demerged: I included the same raisins again *in vacuo*.

July 18. THIS day, in the morning, I found the air again breaking out.

July 19. I shut up the same raisins in the same empty receiver.

July 21. THIS day I found the receiver full, and the air breaking out of it.

I again shut in the same raisins in the same exhausted receiver.

July 23. YESTERDAY, about noon, I found the whole receiver almoſt full of air, and this day in the morning I perceived the air to paſs out very often. From the firſt experiment of art. IX. it appears, that grapes, without water, can generate but little air: ſo that it is manifeſtly hereby, that water is a fit medium to elicit air out of them: it is alſo evident, that the production of air is not begun preſently upon the affuſion of water; but it proceeds on with greater ſwiftneſs, after that the parts of the water in five or ſix days time have more deeply ſunk into, and pervaded the grapes.

EXPERIMENT V.

Aug. 13, 1677. I included pears in two receivers *in vacuo*; and plums in another.

Aug. 16. IN three days ſpace, all my receivers were filled with air, newly generated; yea, one of them, which included the pears, becauſe I had left it expoſed to the rays of the ſun, in the ſpace of twenty-four hours, was ſeparated from its cover; whence we may conjecture, that the production of air is very much promoted by the heat of the ſun.

EXPERIMENT VI.

Octob. 16, 1677. I took two ounces of grapes bruiſed, and ſecured them from the ingreſs of air, in an exhauſted receiver capable of containing twenty ounces of water.

Octob. 17. THE mercury roſe higher about one half inch.

Octob. 18. THESE laſt twenty four hours the mercury ran up about another half inch.

Octob. 20. THE height of the mercury was two inches.

THE 22 it was almoſt 4.

THE 27 it was almoſt 6 inches.

Jan. 2, 1678. THE mercury, as yet, came not to the height of 10 inches.

Octob. 16, 1677, I put three ounces of bruiſed grapes, with half an ounce of ſpirit of wine, into a receiver able to hold 30 ounces of water, and then I exhauſted the air.

Octob. 17. THE mercury aſcended but a very little.

Octob. 18. THE mercury came not up to the height of one quarter of an inch.

Octob. 20. THE mercurial gage was out of order.

Jan. 2. 1678. I this day found my receiver filled with air, and alſo, when ſome of the liquor was poured out, ſome bubbles were formed in the turpentine about the orifice, and were broke outwardly.

FROM this experiment, made in two receivers together, it ſeems to follow, that ſpirit of wine doth much advance the production of air *in vacuo*, though in common air it wholly hinders it. See the IId, VIIIth, and XIVth experiments of the IId article.

EXPERIMENT VII.

July 19, 1678. I put muſt, expreſſed from grapes bruiſed, and kept for 10 months in a veſſel ſtopt with a ſcrew, into the ſame receiver, being alſo ſtopped with a ſcrew.

July 21. THE mercury had not aſcended at all.

23. THE height of it was 3.

24. THE height was 5.

25. IN the morning it was 104.

TOWARDS the evening the height was 137; and the muſt got out.

26. THE muſt was almoſt all out of the receiver; and although the air now did poſſeſs double the ſpace it did yeſterday, yet it kept up the mercury in the ſame height.

27. ABOUT half of the remaining muſt brake forth this night, becauſe I had omitted to ſet the ſcrew, leſt the receiver ſhould have been broken in pieces.

FROM this experiment it follows, that grapes kept ſo long a time do rather acquire than loſe a fermentative virtue.

EXPE-

EXPERIMENT VIII.

Jan. 30. I put two quantities of apples, boiled the day before, into two receivers ftopped with a fcrew; with one of them I mixed one third part of fugar, the other had no fugar at all.

N. ALL thefe receivers were quite full.

Jan. 31. I included raw apples bruifed in three receivers; in one of them I mixed one third part of fugar; the fecond was without fugar, and fo was the third, but it differed herein from the fecond, that it was fix times as big: for by this means, we may know, whether the capacity of the veffel, or the mixing of fugar, or the crudity of the fruit, can promote or retard the production of air.

Feb. 10. IN that receiver only, which contained the raw apples with fugar, fome air was produced.

Feb. 14. THE raw apples with fugar had impelled the mercury up to 10 inches; thofe that were boiled with fugar, to two only; in the other receivers no air was produced.

Feb. 18. IN the receiver containing the raw apples with fugar, the mercury came to the height of 56 inches; in that containing the boiled apples with fugar, the height was three; in the other receivers there was alfo fome air produced, except in that wherein the boiled apples without fugar were put. I opened that receiver in which the apples had produced fo great a quantity of air; yet the apples feemed hardly to be fermented, but were endued with a moft pleafant tafte.

Feb. 21. THE boiled apples without fugar had loft fome of their juice; and, opening the receiver, I found the cover to be broke, and yet the apples were not rotten at all.

March 1. IN the great receiver, containing the raw apples, the mercury was 25 inches high; and in the little one, only 7; but in that where the apples were boiled with fugar, the mercury had afcended to 9 inches.

March 8. IN the great receiver the height of the mercury was 29; in the leffer 22 $\frac{1}{2}$; and where the boiled apples with fugar were, the altitude abode at 9 digits.

March 17. THE juice got out of the great receiver; in the little one the height was 67; where the apples were boiled with fugar, it was 15 digits.

FROM this experiment it feems inferrable, that fugar, the crudity of the fruit, and the largenefs of the receiver, do all contribute to the production of air.

ARTICLE II.

Several ways to hinder the production of air.

EXPERIMENT I.

Dec. 21, 1678. I made pafte of bread-corn-meal, without leaven, and put it into an empty receiver; and then I put the receiver in a certain apartment, with fire, which there kept a greater heat than is wont to be in the middle of fummer; yet the dough or pafte produced no air in 10 hours fpace: whence it feems to follow, that if dough hath once fuffered too much cold, it can fcare recover its faculty of fermenting; for, fome years ago, when I made dough without leaven in the fummer time, it produced very much air *in vacuo* in a fhort time.

EXPERIMENT II.

May 23. I included three ounces of dough, kneaded with leaven, in a receiver capable of holding 50 ounces of water; I alfo poured upon it fome quantity of fpirit of wine, to try whether fermentation would be hindred by that means.

May 24. The mercury was there inches high.
26. Little change.
27. No change.
29. No change.
June 2. It feemed to have afcended a little higher.
14. No change.

Dec. 14. No more air being produced from the dough, I took it out from the receiver, and found the fmell of it not grateful,

ful, but fubacid: I put it into an empty receiver, and there it rofe or fwelled to double its accuftomed fpace, and made a little ebullition.

May 23. I included three ounces of dough kneaded with leaven in a receiver able to hold 50 ounces of water, but here I mixed no fpirit of wine.

May 24. The mercury was 19 $\frac{1}{2}$ inches high.

May 26. IT was 38 inches high.

27. THERE was no change.

Decem. 14. THE mercury perfifted in the fame height; and this day, opening the receiver, I found the dough of a moft acid fmell.

FROM which experiment it feems to follow, that fpirit of wine, even in dough kneaded with leaven, doth hinder the production of air.

EXPERIMENT III.

Aug. 29. I included pears, with a mercurial gage, in a receiver full of water, and then I intruded air into it, till the mercury ftaid at 26 inches higher than it was wont; within a quarter of an hour one of the pears was broken, and afterwards almoft all of it was reduced to the confiftence of a pultis.

Aug. 30. IN 24 hours fpace, the pears feemed to have afforded no air; but on the contrary, the mercury in the gage was depreffed an inch and half.

Aug. 31. I this day found no change in the height of the mercury.

Sept. 1. Now the pears began to produce air, and the mercury was almoft 27 digits high.

Sept. 2. IN 24 hours the mercury afcended more than eight digits, and now it was 35 digits high.

Sept. 3. THE height of the mercury was increafed 17 digits high or thereabout.

Sept. 4. WITHIN thefe 24 hours the mercury rofe feven digits higher, and refted then in 59.

Sept. 5. IT was 64 digits high; a pear being broken, was become black.

Sept. 6. THREE digits and more being added to the height of the mercury, it came now to the 67 digits and $\frac{1}{4}$ beyond what it was accuftomed.

Sept. 7. IT defcended three digits, and refted again in 64.

Sept. 8. THIS day the mercury was depreffed to the 58th digit, and fome of the water had broke out; and therefore I ftraitned or fet the receiver with a fcrew.

Sept. 9. THE mercury afcended full three digits, and now ftuck fufpended above 67.

Sept. 10. IN 24 hours it mounted 1 $\frac{1}{2}$, and ftopped almoft in 69,

Sept. 11. Now it began to defcend again, and was no higher than 67 digits; yet I am certain, nothing had efcaped out of the receiver, but it was a fharp cold night.

Sept. 12. No change did evene.

Sept. 13. THE height of the mercury did again decreafe; it was not above 64 digits: the cold increafed.

Sept. 14. IN 24 hours it became higher by fix digits, reaching to 70.

Sept. 16. IT was 69 digits high, or thereabouts.

19. IT remained in the fame place.

20. IT again reached to 71.

Sept. 23. The mercury was again depreffed to 69.

Octob. 1. It came now to the height of 75 digits.

Octob. 3. YESTERDAY I found no change at all in the mercury; but this day it ftuck in 70; and the cold was very bitter.

Octob. 5. YESTERDAY the mercury did abide in the fame place; but this day it reached to 75: it was a rainy day.

Octob. 7. IT continued rainy; and the mercury continued in the fame place.

Octob. 10. HITHERTO the mercury was not changed; but this day I found it had defcended to 69 digits; though the rain ceafed not.

Octob. 12. YESTERDAY the mercury ftood ftill; but this day it was depreffed to 65 digits: and the cold weather returned.

Octob.

Octob. 13. The height of the mercury was 64.

14. } The height was { 69.
15. } { 74.

24. The height was 68. It was a cold ſeaſon.

Nov. 2. The height was 64. The cold encreaſed.

5. The height was $80\frac{1}{2}$. The cold abated.

2. The height was 65. It was a hard froſt.

27. The height was 68. It was a thaw.

Dec. 6. The height was 61. It was a very bitter froſt.

FROM the former experiment we may learn, that fruits in a great compreſſion of the air, cannot produce ſo great a quantity of air; for when I made an eſtimate of the quantity of the fruits, and of the ſmall ſpace which is to be filled with air; I found, that that quantity of air was not $\frac{1}{8}$ part of that, which had been produced in an empty and a large receiver; yet the cold of the water might alſo give ſome impediment to the generation thereof, as the following experiment will confirm.

IT is alſo farther manifeſt, that the air is produced by iterated turns, and, as it were by reciprocations, even as all bodies in motion, by the force of their gravity or of their ſpring, are carried beyond their point of reſt, and ſo ſuffer many vibrations, or goings and returnings. Now although cold and heat are not the ſole cauſes of ſuch reciprocations, yet they ſeem to contribute much thereunto.

EXPERIMENT IV.

Feb. 22, 1677. I INCLUDED 10 ounces of paſte in a receiver capable of holding 22 ounces of water, and afterward I thruſt as much air into it as was ſufficient to ſuſtain 73 digits of mercury, beſides the wonted preſſure, in two hours ſpace I perceived no ſenſible change.

Feb. 23. IN 18 whole hours the mercury ran up ſeven digits only, its height being 80.

IN ſix hours ſpace it was now aſcended three digits; its height was 83.

Febr. 24.	Its height was	90
25.		97
26.		101
27.		105
28.		$107\frac{1}{2}$
March 1.		112

AND water ſeemed to be expreſſed out of the maſs.

March 2. } Its height was { 120
3. } { 121

4, & 5. It ſtayed at 121

March 8. THESE two or three laſt days, the froſt being diſſolved, the mercury ran up four digits: the height thereof was 125.

March 10. YESTERDAY the mercury perſiſted in the ſame height; but this day, mounting ſix digits, it ſtayed in 131.

March 21. BY reaſon of the long cold ſeaſon, no air was produced; but in the three laſt days the mercury aſcended ſeven digits, and ſtayed in 138.

April 4. YESTERDAY I perceived the mercury had aſcended, but I deferred exactly to meaſure the quantity till this day: but in this very night one of the iron-wires, that ſtraitned the receiver, was broken, and ſo the receiver was ejected to 4 or five foot diſtance.

FROM this experiment we may conjecture, that the compreſſion of the air did very much hinder the production thereof; for that is wont to be perfected in paſte in two or three days ſpace. Moreover, cold doth much hinder the ſame production.

EXPERIMENT V.

March 1, 1677. I included two ounces of raiſins of the ſun, with ſix ounces of vinegar, in an emptied receiver, and bubbles in a ſufficient quantity did break fourth: the raiſins were bruiſed.

March 2. THE mercury in 24 hours ſpace aſcended not to the height of half a digit: yet ſome bubbles ſtill appeared.

March 25. THE vinegar did always appear interſperſed amongſt ſome of the bubbles, yet the mercury aſcended not to the height of one digit.

BY

By this experiment it appears, that vinegar doth hinder the production of air and fermentation, seeing otherwise raisins are wont to afford much air.

EXPERIMENT VI.

April 7. I included 10 ounces of paste in a receiver capable of 22 ounces of water: afterwards I intruded air into it, as much as sufficed to sustain 128 digits of mercury, besides its accustomed height.

In six hours space the mercury mounted up four digits, and staid in 132.

April 8. In 16 hours the mercury ran up nine digits higher: it staid in 141.

Nine hours after the mercury was not changed.

April 9. This day, in the morning, I perceived some air had broke forth, and the mercury was depressed to 130 digits, and therefore with a screw I shut the receiver more closely, and thrust in 11 digits of new air: the height was 141.

April 10.	The height was	151
11.		158
12.		168
13.		176
14.		183
15.		183
16.		187
17.		191

April 27. For eight whole days the mercury kept its station in the same place, but these two last days it ascended seven digits, and stayed in 198 above its wonted height.

April 30. Perceiving the mercury to persist in the same height, I a little relaxed or eased the screw, that some air might break forth; and when I saw, that the mercury had so far descended, that it exceed d its accustomed height only 50 digits, I presently set the screw, that so I might know, whether that remission of the spring of the air would afford any place for new air to be generated; and truly in two or three minutes time I found the mercury to have ascended sensibly higher.

Three hours after, making an admeasurement, the mercury was found 12 digits higher; for it came to 62.

In five hours space it ascended one digit and ½ and no more.

May 1. In 15 hours the mercury gat higher only one digit.

May 3. Yesterday the mercury persisted in the same height, but this day it was higher by 1 ½, and remained in 66.

May 4. The mercury was not changed at all, and therefore I suffered all the air to escape: but something hindered, that I could not quickly set the screw; whence it is probable, that very much air, which at that time was produced, got out of the receiver; yet nevertheless, after the receiver was again straitly stopped, I perceived, that two digits of air and more had been produced in five or six minutes time.

May 7. The mercury, in three days, again mounted two digits.

May 8. The mercury was higher by ½ a digit.

May 11. Those two last days the mercury again ran up half a digit, and not much more. I included this mass, almost unfit, as it seemed, for producing of air *in vacuo*; and then, in five minutes space, the mercury ascended to the height of one digit.

May 21. It did not yet ascend quite three digits.

May 30. The mercury staid at the height of four digits and ½.

By this experiment it appears, that all the air, producible from paste, may be in a manner generated in a great compression; yet it is somewhat restrained by that hindrance, which at length, in a lesser compression, will break forth in a short time.

Moreover, we have a confirmation by this experiment, that air is producible by repeated turns and operations; also, that it is produced more slowly in compressed than in free air: for such a production, in free air, is wont to be perfected in two or three days time.

EXPE-

EXPERIMENT VII.

Artificial air.

July 30, 1677. I included plums and apricocks, many of them being cut asunder, in an empty receiver, and afterwards I immitted as much air, produced out of cherries, into the same receiver, as was sufficient to sustain 64 digits of mercury.

Aug. 1. OUR fruits had produced no air, but grew yellow, by reason of their overmuch ripeness, more than those which were in common air. *See next col.*

Aug. 3. THIS day I found the mercury a little higher, and that apricock, which remained whole, seemed to be full of some drops of water.

Aug. 7. THE whole apricock grew more and more soft; the mercury was 59 digits high above its wonted pressure.

Aug.		
8.	The height of it was	61
9.		65
10.		71
11.		74
13.		78
14.		80
15.		80

16. and the days following it abode at the same height.

24. The height of it was 77; though I certainly knew, that nothing had issued or escaped out of the receiver.

29. Seeing I found, that neither the fruits nor the height of the mercury were changed any more, I opened the receiver, and perceived, that the apricocks had kept their colour very well, but the flesh of them was spungeous, and their taste subacid; many bubbles had broke forth from them, at the time they were freed from the circumstant pressure.

Common air.

July 30, 1677. I included the half parts, cut off from the fruits aforesaid, in a receiver full of common air; and with them also some fruits of the same kind uncut.

July 31. I found the mercury had attained eight digits high.

Aug. 1. AT six o'clock in the evening the mercury was 21 digits high; in the other receiver it was not moved.

Aug. 3. OUR fruits kept their firmness much better than those, which were included with artificial air. The height of the mercury was 35 digits.

Aug. 4. THE height of the mercury was 42 digits.

Aug. 6. OUR whole apricock seemed not at all to be altered. The height of the mercury was 57.

Aug.		
7.	The height of it was	81
8.		95
9.		113
10.		124

THE colour of the whole apricock yesterday began, and now proceeded, to wax yellow. No moisture appeared.

Aug.		
11.	The height of it was	131
13.		157
14.		163
15.		171
16.		171

17. and the days following the same height remained.

27. The height was 182.

29. When I saw, that neither the fruit nor the height of the mercury were changed any more, I opened the receiver, and found the apricocks of a more acid and less acceptable taste, than the others in factitious air; yea, their pulp was of a very good colour, but spungy: they sent forth many bubbles, as the others did.

FROM this experiment made in two receivers together, it is probably collected, that the artificial air of the cherries was a great hindrance to the apricocks, that they could not produce air; yet, notwithstanding, it doth advance the alteration of their colour and firmneſs; and is alſo good to preſerve their taſte.

EXPERIMENT VIII.

Grapes without ſpirit of wine.

Octob. 10, 1677. I ſhut in an ounce and half of grapes, unripe and bruiſed, in a receiver, that would hold 10 ounces of water; I drew out no air.

Octob. 11. The mercury aſcended a little.
12. There was but a ſmall change.
13. The height was $\frac{1}{2}$ a digit.
17. The height was one digit.
18. The height 1 $\frac{1}{2}$.
19. The height almoſt four digits.
20. The height the ſame, but ſome finew or mouldineſs appeared in their ſuperficies.
21. The height was 4 $\frac{1}{2}$.
22. 23. 24. The height remained the ſame, but the mouldineſs or finew encreaſed.

26.	The height of it was	5 $\frac{1}{2}$
27.		6
30.		6 $\frac{1}{2}$
Nov. 2.		7 $\frac{1}{2}$
6.		9
8.		10
Nov. 9.		12
12.		15
14.		17
18.		23
21.		26
Dec. 8.		36 $\frac{1}{2}$
12.		39
27.		39

Jan. 6, 1678. The height was 36. The air broke out.

Grapes with ſpirit of wine.

Octob. 10, 1677. I made the ſame experiment in another receiver, obſerving the ſame circumſtances, ſave that here I mixed two drachms of ſpirit of wine with the grapes.

Octob. 11. The mercury was not changed.
12. There was no change.
13. The mercury was not moved.
17. It aſcended a little.
Octob. 18. The height of it was not yet a quarter of an inch.
19. It was moved but a very little.

Jan. 6. THE grapes, during all the time elapſed, had produced no air.

BY this experiment made in a double receiver, it appears, that ſpirit of wine doth hinder fermentation.

EXPERIMENT IX.

Octob. 17, 1677. I put one peach into an emptied receiver, with ſome quantity of ſpirit of wine, which yet could not touch the peach, unleſs it were elevated into vapours.

March 27, 1678. I drew out the peach, which had kept its colour, only it had loſt its firmneſs. Though the receiver was but ſmall; yet it was not filled with air, for when it was opened, the air ſeemed to ruſh into it: the peach being ſoftened, was ſo depreſſed, that the lower part of it did a little touch the ſpirit of wine; it alſo came to paſs, that the ſuperior part had contracted the taſte of the ſpirit of wine, as well as that, which was immerged in it.

EXPERIMENT X.

Air with ſpirit of wine.

Octob. 17. I included five peaches in an exhauſted receiver, and together with them, ſome ſpirit of wine, which could not touch the peaches, unleſs it were elevated in form of vapours.

Octob. 18. The mercury aſcended not at all.
20. The height of the mercury was 3 $\frac{1}{2}$.

21.	The height of it was	5 $\frac{1}{2}$
22.		7 $\frac{1}{2}$
23.		9
26.		9 $\frac{1}{2}$
Nov. 2.		12
6.		14
12.		16
Nov. 14. 16.	It kept the ſame height.	
Dec. 8.	The height of it was	18
16.		19 $\frac{1}{2}$
27.		20 $\frac{1}{2}$

Jan. 6, 1678. It was 23.
March 28, 1678. It was 31 ½.

Air without ſpirit of wine.

Octob. 17. I included five peaches in a receiver full of common air, without ſpirit of wine.

Octob. 18. The mercury aſcended not at all.

20. The height of the mercury was five digits.

21.	The height of it was	8
22.		10
23.		11
26.		12
Nov. 2.		15
6.		17 ½
12.		20
14.		20
16.		21
Dec. 8.		26
16.		26 ½
27.		28 ½

Jan. 6, 1678. The height was 32.

March 28, 1678. The height was 33 ½.

April 15. THE liquor in the lower part of the receiver had broke all out, and the air followed it; ſo that I took out the peaches.

BY this experiment we learn, that the very vapours of ſpirit of wine do ſomewhat hinder fermentation, yet much leſs than the ſpirit itſelf.

EXPERIMENT XI.

Paſte with leaven or ferment.

April. 27, 1678. I included an ounce and half of paſte mixed with leaven, with common air, in a receiver able to hold 23 ounces and a half of water.

April 28. The height of the mercury in the gage was 2 ½.

April 30, The height of it was 3 ¼.

May 4. The mercury was depreſſed, though no air broke forth, and the paſte was mouldy. The height of it was 2 ½.

May 6.	The height of it was	2 ¾
8.		3
10.		3 ½
14.		4

May 17.	The height of it was	4 ½
20.		5
24.		6
28.		8
June 2.		9
6.		10
14.		10 ½
July 5.		13 ½
19.		15

Paſte without leaven.

April 27, 1678. I included an ounce and half of paſte, without leaven, with common air, in a receiver capable of holding 23 ounces and a half of water.

April 29. HITHERTO the mercury had not aſcended; but this afternoon I found its height to be a quarter of a digit.

April 30. There was no change.

May 4. The mercury aſcended but very ſlowly, and the paſte was finewed or mouldy.

May 6. The height of the mercury was four digits.

8.	The height of it was	5 ½
10.		7 ½
14.		10 ½
17.		12 ½
20.		13 ½
24.		16
28.		18 ½
June 2.		20 ½
6.		21 ½
14.		25

BY this experiment, made in two receivers at once, it ſeems clear, that leaven doth rather hinder than help the production of air, if the paſte be not made in a place hot enough.

EXPERIMENT XII.

Paſte with ſpirit of wine.

May 23. I included an ounce and a half of paſte, without leaven, in a receiver capable of holding 25 ounces of water, and I poured ſpirit of wine on the paſte.

May 24. The mercury was one digit high.

26. It was almoſt two digits high.

27. It was 2 ½.

31. There was no change.

June 2.	The height of it was	3½
6.		4
10.		4½

July 19. No change.

Decem. 14. WHEN the height of the mercury was no more changed, I opened the receiver, and the paste affected my nostrils with a subacid smell.

Paste without spirit of wine.

May 23. I included one ounce and a half of paste, without leaven, in a receiver capable of holding 25 ounces of water; but I added no spirit of wine.

May 24. There was no ascension of the mercury.

May 26. It was three digits high.

27.	The height of it was	4½
28.		5½
29.		7
31.		9½
June 2.		12
6.		17
10.		22
July 4.		30

July 19. The mercury little exceeded 30 digits. This day I found, that the air had broke out, and therefore I set or straitned the screw.

Decem. 14. THE mercury came again to the height of 15 digits; but this day I opened the receiver, and found the paste very acid.

FROM these experiments, made with paste, in a four-fold receiver, at one and the same time, it seems to follow, that spirit of wine doth very much prejudice the production of air; and the rather, if the paste be wrought with ferment: besides, it is clear, that paste without ferment, in tract of time, will produce no less air than paste with ferment.

EXPERIMENT XIII.

Octob. 11. I included new ale in a receiver, exactly filled by the help of my pneumatick engine, that so no air might be left; and I included another quantity of the same ale in another receiver, wherein some room was allowed for the air.

Octob. 12. I this day found the cover of that receiver, in which I had left some air, to be broken, and therefore I transfused the same ale into another receiver, in which there was room large enough left for the air. In the receiver exactly full, the mercury ascended a little.

Octob. 13. IN the receiver exactly filled, the height of the mercury was 13 digits, though it had been shut up a shorter time, and a much larger space was left therein, in which the air newly produced might have been dilated.

Octob. 14. IN the full receiver the height was 13; in the other receiver, 18. Towards evening I found the full receiver to work with greater swiftness, for the height of the mercury in it was 22; and in the other 20.

Octob. 15. IN the full receiver the height of the mercury was 42 digits; in the other 26. Besides we must mark, that some bubbles of air, which in the full receiver had possessed its upper part, now did wholly vanish; and besides, the ale did occupy a long space in the mercurial gage, wherein before it was not found.

Octob. 16. IN the full receiver the height was 60 digits. In the other 30.
18. IN the full receiver the height was 90. In the other 40.
22. IN the full receiver the height was 90. In the other 42.
23. IN the full receiver the height was 103. In the other 50.
26. IN the full receiver the height was 108. In the other 60.
28. IN the full receiver the height was 134. In the other 63.

THE bubbles, which were vanished, appeared again, yet nothing flowed out.

Nov. 8. THE full receiver had lost much ale; wherefore I opened it, and thereupon all the ale seemed as if it would have vanished into froth, unless I had suddenly shut the little hole, which I had opened:

opened: I tried it many times, that if the hole were opened in the gage, the mercury preſently deſcended; but if the hole were again ſhut, it would ſpeedily aſcend; as if the compreſſion, being abated, had afforded ſome facility for the production of air. The ale had a moſt pungent taſte.

Nov. 9. I opened the other receiver, and obſerved in a manner the ſame circumſtances.

FROM this experiment it ſeems to follow, that ale, if the air be wholly excluded from the veſſel, will ferment more ſlowly, than if ſome air were left with it: yet, in tract of time, it makes a greater compreſſion, if no place be left for its dilatation.

EXPERIMENT XIV.

Peaſe with ſpirit of wine.

June 27. I put green peaſe into an emptied receiver with ſpirit of wine. Towards the evening the receiver ſeemed to admit the external air, and the mercury came to the height of 18 digits; and therefore I firmed the cover with turpentine.

June. 30. I perceived no more change in the height of the mercury.

July 7. No air was produced, even in the moſt vehement heat.

Peaſe without ſpirit of wine.

June 27. I put new peaſe into an emptied receiver without ſpirit of wine. The receiver and the quantity of the peaſe were the ſame, as in the laſt mentioned experiment.

June 28. THE receiver was full of air, for I think it was not exactly ſhut; and therefore I again included the ſame peaſe. Towards evening the height of the mercury was five digits.

June 29.	The height of it was	10
30.		16
July 1.		19
5.		26
7.		30

July 8. THE air got out of the receiver being too much filled.

FROM this experiment, made in two receivers at once, it appears, that ſpirit of wine doth alſo hinder the production of air in peaſe.

ARTICLE III.

The effects of artificial air are different from the effects of common air.

EXPERIMENT I.

June 19, 1677. I put cherries into an evacuated receiver. In ſix hours time the mercury came to the height of five digits and a half.

June 20. The aſcenſion of the mercury was 3½. Towards the evening it was two.

N. *The aſcenſions are always to be underſtood, as added to the former.*

June 21.	The aſcenſion was	1½
22.		1½
23.		2
24.		1½
25.		1½
26.		3
27.		3
28.		5
30.		1½
July 1.		3
2.		4
3		2
4.		2½
5.		3

The height was 48; but I tranſmitted the air into another receiver, and the mercury was depreſſed to the height of 25 digits.

July 6. The aſcenſion of the mercury four digits in one night's ſpace.

7. The aſcenſion of it was 5½ in 24 hours ſpace.

8. The aſcenſion of it was 5.

9. The aſcenſion of it was 5.

10. The aſcenſion of it was 6.

11. The aſcenſion of it was 12 in the ſpace of 34 hours.

12. The aſcenſion of it was 7.

13. The aſcenſion of the mercury was three, the height about 92 digits; but the air tranſmitted into

to another receiver, the mercury ſtaid in the height 50.

July 14. 15. 16. 17. } The aſcenſion was { 14 11 13 5

18. The aſcenſion of the mercury was nine, the height of it 102.

19. The height of the mercury 92. *viz.* becauſe I tranſmitted part of the air into another receiver.

20. The aſcenſion of the mercury was 15.

22. Some air got out, and the height of the mercury was 63 $\frac{1}{2}$.

23. The aſcenſion of it was 12 $\frac{1}{2}$.

24. The aſcenſion of the mercury was four, the height of it was 79 digits; but the air being tranſmitted into another receiver, the height ſtaid at 62.

25. 26. 27. 28. } The aſcenſion was { 8 9 4 5

30. The aſcenſion of it was 10, the height was 98. Part of the air being tranſmitted into another receiver, the height ſtaid at 64.

31. The aſcenſion was 6.

Aug. 1. The aſcenſion of the mercury was nine digits.

2. The aſcenſion of it was 4.

3. I tranſmitted the air into another receiver, and the mercury abode in the height 68.

4. I tranſmitted the air again into another receiver, and the mercury reſted in the height 54.

6. The aſcenſion of the mercury was 7.

7. The aſcenſion of it was 4.

8. There was no aſcenſion thereof.

9. The aſcenſion thereof was three digits.

THE receiver being opened, I found the cherries of a whitiſh colour, and of very little taſte; but the taſte they had was not ungrateful; their fleſh or pulp was ſpungy.

FROM this experiment it ſeems to follow, that cherries contain much air in them, and that they produce it very irregularly.

EXPERIMENT II.

July 13, 1677. I put cherries into an empty receiver, and then I tranſmitted into the ſame receiver as much air produced from other cherries, as was ſufficient to ſuſtain 50 digits of mercury.

July 15. YESTERDAY the mercury had not aſcended at all; but this day it was two digits higher, *viz.* in 22 above its wonted height.

July 16. The height of the mercury 23 $\frac{1}{2}$.

17. The height of it was 25.

26. The height of it was 43. Some air got out.

27. The height of the mercury was 45. Some more air made an eſcape.

30. The height of it was 52.

31. The height of it was 61 digits.

Aug. 1. THE height of the mercury perſiſts in manner the ſame, but the air brake out.

Aug. 27. THE air had all broke out for ſome time before: I took out the cherries, and found them not to have loſt their colour, as they had in the former experiment; and beſides they had contracted no putrefaction nor mouldineſs, but had a taſte a little more acid than they were wont to have; and being opened, there were many cavities in their pulp, like fermented paſte or dough, but not quite ſo thick.

FROM this experiment compared with the former it may probably be inferred, that in artificial air fruits do produce leſs air, and ſo they keep their colour and their taſte better; for the cherries, in the former experiment, remained included in a receiver, nor much longer than thoſe in this.

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EXPERIMENT III.

Common air.

Sept. 10, 1677. I put ſix ounces of unripe grapes into a receiver capable of containing 25 ounces of water; and I ſtopped it firmly, by the help of a ſcrew, with common air.

Sept. 11. The mercury aſcended not at all.

12. The mercury ſtopped a little below one digit.

	The height of it was
13.	3 ½
14.	7
15.	10
16.	12 ½
17.	14
18.	16
19.	18
20.	20
21.	22
22.	23 ½

23. The height of it was 27. The grapes were not altered.

24. The height was 30.

25. The height was 31. The grapes now began to be yellow.

	The height of it was
26.	32 ½
27.	34
29.	35
30.	35

Octob. 1. The height remained at 35.

2. The height was 36.

5. 6. The height ſtayed at 36.

10. The height was 35.

13. The height of it was 32 ½. The air got not forth, but the cold began to come on and encreaſe.

Nov. 9. The ſame remained.

Dec. 19. I found the air almoſt all to have made an eſcape.

Dec. 20. I took out the grapes, and I found, that, by their ſmell and their taſte, they had contracted ſome mouldineſs, though the ſame was not diſcernable by the eye. Their firmneſs was encreaſed.

Factitious air.

Sept. 10, 1677. I included two ounces of crude grapes in a receiver capable of holding eight ounces of water; and to the common air I ſuperadded air produced out of pears, until the mercury did ſtay 10 digits above its wonted preſſure.

Sept. 11. The mercury deſcended; its height was eight digits.

Sept. 12. The height of it was 11; the aſcenſion of it was 3.

	The height of it was
Sept. 13.	16
14.	20
15.	23
16.	24

17. The height was 28; the grapes turned yellow.

	The height of it was
18.	29
19.	30
20.	31
21.	33
22.	35
23.	20

Becauſe ſome air had broke out: the grapes were alſo of a yellow colour.

Sept. 24. The height of the mercury was 21 digits.

25. The height was 22.

26. The height almoſt the ſame.

Sept. 27. The height abode in 22.

29. The height was 27.

30. The height was 28.

Octob. 1 & 2. The height ſtayed at 28.

	The height of it was
5.	30
6.	31
10.	31 ½.
13.	31

Nov. 9. The height was 13. Some air had got out.

Decem. 19. The height of the mercury was 20 digits.

Dec. 20. I took out the grapes, and their ſmell and taſte were more grateful than of others, and their firmneſs was rather increaſed than diminiſhed.

BY this experiment, made in two receivers at once, we learn, that factitious air ſeems fit to alter colour, and to preſerve taſte; but the firmneſs might be increaſed here, as it is augmented in turpentine; *viz.* the ſpirits, in tract of time, being exhaled.

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EXPERIMENT IV.

July 18. I took two pieces of orange, and by the help of my ſcrew I ſtopped them in faſt in my receiver with common air, and then into the ſame receiver I put air, produced out of cherries, as much as was ſufficient to ſuſtain 12 digits of mercury. At the ſame time I put another piece of the ſame orange into another receiver, with common air alone, and that not compreſſed.

July 20. THE orange, in common air, began to contract mouldineſs; the other ſeemed not at all to be altered.

July 23. THE mouldineſs of the orange in the common air increaſed; the other remained ſound.

July 16. THE orange, in the common air, did not proceed to increaſe its mouldineſs, but ſeemed wholly rotten; the other alſo began to putrify, but remained free from mouldineſs.

Aug. 1. PERCEIVING, that the oranges were no more ſenſibly changed, I opened the receivers; and though the air, wherewith I had mingled artificial air, was ſo compreſſed in its receiver, that now it could not ſuſtain 26 digits of mercury above its wonted preſſure, yet the fruits were far better preſerved in it than in the other; only ſomething in the ſuperficies ſeemed to have loſt its juice, but all the inner parts, with the rind or pill, were very well coloured, well taſted, and firm: in the other receiver, the whole orange ſeemed almoſt rotten, not excepting the rind. In the *Exper.* X. of *Artic.* IV. the orange was more corrupted in the compreſſed air, becauſe, as it ſeems, no factitious air had been mixed with it.

HERE alſo it ſeems worthy our obſervation, that the ſame air, generated from cherries, is apt to produce different effects upon fruits of a different kind; for here it retarded the alteration of colour and firmneſs, which in *Exper.* VII. of *Artic.* II. where I included air with apricocks, it accelerated and haſtened.

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EXPERIMENT V.

Factitious air.

July 20, 1676. I included a ſmall piece of beef in an emptied receiver, and then I put air, produced from cherries, into the ſame receiver, as much as ſufficed to ſuſtain 27 digits of mercury.

July 21., 22., 23., 25. } The mercury perſiſted almoſt in the ſame height, and came not to its wonted preſſure.

July 26. THIS day the beef had removed the receiver from its cover; and becauſe it ſtunk very much, we threw it away.

Common air.

July 20, 1676. I put a piece of beef into a receiver full of common air, and I carefully ſtopped and firmed it in, by the help of the ſcrew.

July 21. The mercury had not at all aſcended in the gage.

22. The height of the mercury was one digit.

23. The height of it was $5\frac{1}{2}$

25. The height of it was $9\frac{1}{2}$

26. The height of it was $14\frac{1}{4}$. In the evening 18.

27. The height of it was $21\frac{1}{3}$. In the evening 25.

28. The ſcrew, not being firm enough, ſuffered the air to break forth.

BY this experiment, made in two receivers at once, it appears, that air, produced from cherries, is a great hindrance to the production of air from fleſh.

EXPERIMENT VI.

Common air.

March 14, 1676. I put two onions into a receiver, full of common air, with a mercurial gage; and I faſtened the ſtopple with a ſcrew, to ſee whether vegetation would increaſe the quantity of the air, or diminiſh it.

March 28. Two days after, the mercury ſeemed depreſſed $\frac{1}{4}$ of a digit; but

afterward

afterward it recovered its former height, and two digits more; and now the air brake forth, and the roots grew longer.

April 28. ABOUT 10 or 12 days ſince I perceived the roots to be corrupted; and indeed now they were wholly putrified.

May 9. THE mercury perſiſted in the ſame height, becauſe the air had broke forth; and therefore I took out the onions, and found their roots putrified, but they were not mouldy at all.

Factitious air.

March 17, 1676. I included two onions in an empty receiver, and afterward put air, produced from paſte, into the ſame receiver.

March 28. MY onions took root, at leaſt as well as thoſe, which I kept in the common air.

April 28. THE ends of the roots began to putrify, yet they were in far better caſe than thoſe, who are ſurrounded with common air. Perhaps the cauſe of this difference is to be fetched from hence, that a greater quantity of water was included with artificial air. The mercury mounted higher 9 or 10 digits.

May 18. HITHERTO the onions ſeemed not all to be corrupted, but this day I found one of them to have contracted ſome corruption, which may be called a ſyderation or planet-ſtriking, and differs from a mouldineſs.

FROM this experiment, made in two receivers at once, we may gather, that artificial air doth not at all hinder vegetation: it appears alſo thereby, that not only the ſenſible bigneſs of the body, but alſo the quantity of the air, is increaſed by vegetation.

EXPERIMENT VII.

Common air.

Auguſt 25. I included ſix ounces of unripe grapes in a receiver capable of holding 25 ounces of water, but I did not exhauſt the air.

Auguſt 26. The mercury aſcended a little.

27. The height of the mercury was one digit.

Auguſt 28. The height of it was 1 $\frac{1}{4}$.

29. The height of it was 1 $\frac{1}{4}$.

Auguſt 30. THE mercury ſeemed to have deſcended rather than aſcended. The colour of the grapes was leſs altered here, than in the receiver, into which air produced out of pears had been immitted.

Auguſt 31. THE receiver was broken, and I left the grapes expoſed to the free air.

Sept. 7. THE grapes being left in the free air, did ſtill keep their green colour, and were of a taſte grateful enough, though leſs pungent than before.

Factitious air.

Auguſt 25. I included two ounces of unripe grapes in a receiver capable of holding eight ounces and $\frac{1}{2}$ of water; and having ſtopped it cloſe with a ſcrew, I filled it further with air, which I immitted, produced from pears, as much as ſufficed to ſuſtain 15 digits of mercury.

Auguſt 26. SOME air eſcaped out, and therefore I immitted new air, produced out of the ſame pears, until the mercury ſtaid at 17 digits above its wonted preſſure.

Auguſt 27. THE mercury was depreſſed below the 16 digit; and yet no air had brake forth. Towards evening, I found the mercury had again aſcended to 17.

Aug. 28.	The height of it was	19
29.		21
30.		22
31.		23 $\frac{1}{2}$
Sept. 1.	The height of it was	24
2.		24

Sept. 4. THE ſame height continued at 24, and the grapes had all contracted a yellow colour.

Sept. 5. THE air broke out.

Sept. 7. THE air proceeding to get out by degrees, I took out the grapes, and found them very inſipid, and of an unacceptable taſte.

THIS experiment, made in two receivers at once, doth confirm to us the efficacy of artificial air, to alter the colour of fruits. It is alſo very obſervable, that in this experiment it did prejudice the preſer-

preservation of the taste, and promoted the production of the air, contrary to what had happened in the former experiments. It would be worth the while to try, whether the same success would evene with all unripe fruits.

EXPERIMENT VIII.

Factitious air.

August 2, 1676. I shut up one gilliflower in a receiver, with air produced from paste made with meal, and not mixed.

August 4. OUR flower began to change colour and to be moist.

August 9. THE gilliflower was little altered.

August 12. THE moisture increased by little and little, but no mouldiness appeared.

August 31. THE gilliflower was little altered, yet it was less fresh than those, which were kept *in vacuo.*

Common air.

August 2. I shut up one gilliflower in a receiver, with common air, not mixed.

August 4. OUR flower was not changed.

August 9. THE gilliflower was madid, and had almost lost all its colour.

August 12. Now a great mouldiness covered all the flower.

Vacuum.

August 2. I included two gilliflowers *in vacuo*; and took special care, that no humidity should be included with them.

August 4, 1676. ONE of the gilliflowers began to appear madid.

August 31, 1677. DURING the whole elapsed year, the gilliflowers had suffered no mutation.

BY this experiment, instituted in three receivers at once, it seems probable, that factitious air doth render the change of colour more speedy, yet it prevents mouldiness, even as *vacuum* doth the same.

EXPERIMENT IX.

Common air.

July 24. I put apricocks, and some plums, of which divers were cut in pieces, into a receiver full of common air, and stopped it firmly with a screw.

July 25. THE mercurial gage was spoiled, and therefore I could not by any means perceive the quantity of the air to be generated.

July 30. THE fruits seemed not at all to be altered, saving, that one of the dissected plums had contracted something of mouldiness.

August 2. I opened the receiver, and found all the fruits firm, of a good colour, and of a grateful taste.

Artificial air.

July 24. I made the same experiment in another receiver, with the same circumstances, save only, that into this last receiver I intruded air, produced from cherries, as much as was sufficient to sustain 22 digits of mercury.

July 25. I found the mercury to have descended three digits; it staid in 19. Toward the evening it recovered its former height; it staid in 22.

July 26.	The height of it was	28
27.		$34\frac{1}{2}$
28.		36
29.		40

July 30. The height was 44. The apricocks, which were cut, began to moisten, and to be dissolved into water.

31. The height was 51.

Aug. 1. The height was 60.

2. The height was 65. Towards evening, when I found some liquor had escaped out of the receiver, I screwed it more straitly; but one of the iron wires being broken, all the air got out. Wherefore I took out the fruits, and found them very soft, especially those whose lower parts were immerged in the water; for the rest they were a little more firm, but all of them retained a grateful taste.

FROM this experiment made in two receivers, it seems to be inferrible, that air produced from cherries doth promote the

the alteration both of colour, and alſo of firmneſs in apricocks.

IT appears alſo, that ſome part of ſuch air is deſtroyed in the beginning.

EXPERIMENT X.

July 30, 1676. I put plums, cut aſunder, into three receivers, of which one was full of artificial air, produced from gooſberries; the ſecond was full of common air; the third was vacuous.

Auguſt 2. IN the artificial air, the plums were not changed; in the common air, they began to be mouldy; but in the evacuated receiver, they retained their colour, but were ſoft.

Auguſt 5. IN the artificial air the plums had contracted a red colour, humidity, and ſoftneſs; in the common air they ſeemed black and mouldy, yet retaining their firmneſs; in the evacuated receiver they were almoſt melted or diſſolved.

Auguſt 7. IN the common air the plums now began to ſoften.

Auguſt 8. IN the common air, the plums ſeemed to have loſt their black colour, and to have contracted a red one; even as it happened three days ago to the plums in the artificial air.

IN this experiment, artificial air ſeems to have promoted alteration.

EXPERIMENT XI.

Sept. 24. I put five peaches in a receiver, with common air mixed with air produced from grapes; and I included the grapes themſelves in the ſame receiver, that the common air might be the better ſaturated with the artificial.

Sept. 25. THE height of the mercury was 21 digits.

Sept. 26.	The height of it was	23
27.		31
28.		39
29.		42
30.		45
Octob. 1.		48

2. THE ſame height continued.

3. THE height of it was $52\frac{1}{2}$.

5. THE height the ſame; but the peaches ſeemed ſomewhat madid.

6. THE height of it was 58.

7. THE height of it was the ſame.

8. THE height of it was 61.

11. THE mercury aſcended a little.

19. THE height of it was 65.

25. THE height of it was 16. The cold was ſharp.

27. THE cold abated, and the mercury aſcended.

30. THE height ſtaid at 61, and a little more.

Nov. 2. THE height of the mercury was 59. It was bitter cold weather.

6. THE height of it was 61. The froſt broke and was diſſolved.

7. THE mercury ſeemed ſomewhat higher.

9. THE mercury perſiſted in the ſame height.

Decem. 9. IN one month's ſpace the mercury aſcended by little and little; its height was 80 digits.

April. 1, 1678. THE mercury came to 96 digits above its wonted height. And I opened the receiver, and whilſt the air was breaking out, the peaches did emit many bubbles through their ſkin, not without violent noiſe, and the ſkin in ſome of them was broken: they had preſerved their taſte pleaſant enough, and the colour of their pulp was commendable, but they had loſt their firmneſs, as if they had been boiled: being left in the air for three hours ſpace, they were all rotten.

THIS experiment proves, that common air doth corrupt bodies, yet it doth ſo much leſs, if it be mixed with factitious air.

EXPERIMENT XII.

The firſt receiver.

Auguſt 4. I cut five pears, each of them into four parts, and I put one part of each into a receiver full of common air, and ſtopped it cloſe with a ſcrew.

August 6. THE colour of these fruits was altered little less than of others: the mercury ascended not at all.

August 7. THE pears were little altered; the mercury was higher by a little.

August 8. THE pears underwent no great mutation. The height of the mercury was four digits.

Aug. 9.	The height of it was	$4\frac{1}{2}$
10.		6
11.		10
13.		16
14.		20

The pears began to be softned.

15. The height of it was 21.
16. The height of it was 19. I believe the air had got out.
17. Now I found the air had escaped out.
18. When the air had almost all got out since yesterday in the evening, and I saw the fruits to look worse than before, I took them out, and found them putrified.

The second receiver.

Aug. 4. I took one quarter of each of the aforesaid pears, and included it after the same manner; and afterwards I immitted air, produced out of cherries, till the mercury possessed 23 digits above its wonted pressure.

August 6. THOSE fruits had altered nothing, but their colour a little.

August 7. THE pears, almost all, seemed rotten. The mercury persisted in the same height.

August 8. THE pears were not altered much more. Something hindered, that I could not see the mercury.

August 10. THE pears waxed more and more soft. Now looking upon the height of the mercury, it was 40 digits more than its wonted height.

August 11.	The height of it was	51
13.		61
14.		67
15.		73

16. The mercury descended; yet I know assuredly, that nothing had got out.
17. The mercury exceeded not 69 digits in height, yet the air could by no means escape out.
18. The mercury persisted at the same height, but I suffered the air to break forth; it affected my nostrils with a sharp odour: moreover, the taste of the fruits seemed very acid, and their pulp exceeding soft.

The third receiver.

August 4, 1677. I put a quarter of each of the foresaid pears into a receiver, not exactly shut.

August 6. THE pears seemed to change their colour.

August 7. ONE of our pieces of pears began to lose its firmness: but in the artificial air, another piece of the same pear did yesterday seem wholly rotten.

August 8. ONE piece was mouldy, the rest were soft.

August 9. THE pears grew more and more rotten.

August 11. THE pears were wholly mucid and rotten.

THIS receiver, compared with the first, shews, that corruption doth not begin in free air sooner than in included air; but when it is begun, it is much more, yea, and more speedily increased, *viz.* because the included air might be satiated.

The fourth receiver.

August 4, 1677. I included one quarter of each of the said pears *in vacuo*.

August 6. THE height of the mercury was 5.

August 7.	The height of it was	8
8.		10
9.		12
10.		14
11.		16
13.		20
14.		23
15.		25
17.		28

August 20. HITHERTO the pears had undergone no alteration, but this day began to be soft: the mercury ascended not.

August 26. NEITHER the pears, nor the height of the mercury were altered at all.

THIS production of the air seems very regular.

BY this experiment, made in 4 receivers at once, we find the aptitude of artificial air for the softning of fruits.

AND that the production of air was here promoted by artificial air, is very probable; yet it had succeeded otherwise with apricocks, *Art.* II. *Exper.* VII.

EXPERIMENT XIII.

The first receiver.

August 21, 1677. I divided six apricocks, each into four parts, and I put one piece of each into a receiver full of common air, and stopped it firmly with a screw.

August 22. THE apricocks seemed riper this day than yesterday; but no air was produced by them.

August 23. ONE piece, contiguous to the water, began to be mouldy; the rest inclined to putrefaction: the mercury seemed to have ascended a little.

August 24. A piece next the water was covered with a great deal of mouldiness; another piece, more remote from the water, was somewhat mouldy also; but all were rotten.

August 25. THE fruits contracted no more mouldiness; but the putrefaction more and more increased. The height of the mercury was seven digits.

August 26. The height of the mercury was 15 digits.
28. The height of it was 30.
29. The same height continued.
30. The height of it was 33. The fruits were almost all dissolved.
31. The height of it was 38.

Septem. 1. The height of the mercury was the same.
2. The same height still.
3. The mercury ascended a little.
4. 5. 7. 8. The height of it was 41, 43, 45, 46.
9. The same height continued.
22. Little or no change was made in the height of the mercury; but the fruits were almost melted into water.

Octob. 1. WHEN the mercury continued in the same height, and the fruits were almost all vanished, I opened the receiver, and found the apricocks very much impaired, and soft; yet they had retained a taste, not ungrateful, but subacid.

The second receiver.

August 21, 1677. I covered one quarter of each of the foresaid fruits, the receiver not being fortified against external air.

August 22. THE apricocks were flaccid or quailed, as if they had been dry or withered.

Aug. 23. MANY of our fruits appeared rotten and mouldy.

Aug. 24. THE apricocks wholly infected with putrefaction and mouldiness.

The third receiver.

Aug. 21. I included firmly, by the help of a screw, one quarter of each of the foresaid fruits, in an unexhausted receiver; to which I after added air produced from pears, as much as sufficed to sustain 20 digits of mercury.

Aug. 22. THE mercury ascended not at all; but the fruits seemed to have acquired a greater degree of maturity than those, which are included in common air.

Aug. 23. THESE fruits seemed less altered than they, which were in common air.

Aug. 24. THE fruits were not altered.
25. THE fruits did begin to produce air, but I could not discern the quantity.

Aug. 26. LITTLE alteration in the fruits.
28. THE apricocks began to moisten, yet they were far less altered than those which remain in common air.

Aug.

Aug. 30. THE mercury did this day emerge above the body by which it was hid. Its height above the wonted preſſure was 30 digits.

Aug. 31. The height of the mercury was 40 digits.

Sept. 1. The height of it was the ſame.
2. The ſame height continues.
3. The height thereof 45.
8. The height was little changed.
9. The height was 40, and yet no air got out.

Sept. 11. The height was 38.
12. The mercury continued to deſcend.

Sept. 13. The height of it was 33.

Sept. 14. The mercury was ſo depreſſed, that it appeared no more.

Sept. 22. The mercury did emerge again; its height was 33. The fruits were covered with a kind of mucor or finew.

Octob. 1. WHEN the height of the mercury, nor the apricocks, were any more altered, and the finew vaniſhed away, I opened the receiver, and found the apricocks not impaired, but of a colour laudable enough; but their pulp was ſpungy and ſoft, and of a ſubacid taſte.

The fourth receiver.

Aug. 21. I took a quarter of each of the aforeſaid fruits, and ſhut them up firmly with a ſcrew in an unexhauſted receiver, into which afterwards I intruded air, till the mercury came to 90 digits above its accuſtomed preſſure.

Aug. 22. OUR receiver broke into an hundred pieces, by the force of the air compreſſed within it: whereupon I put the fruits into another receiver, and added only ſuch a quantity of air, as was able to ſuſtain 60 digits of mercury.

Aug. 25. THE apricocks had contracted much mouldineſs; I added new air.

Aug. 26. THE apricocks were wholly infected with mouldineſs, and rottenneſs.

THIS receiver, if compared with the former, doth ſhew, that the quantity of corruption doth depend on the quantity of the air.

BY this experiment made in four receivers at once, we have a confirmation, that in factitious air, alteration is made quicker; but in tract of time, the corruption is far greater in common air.

ARTICLE IV.

The effects of compreſſed air are different from the effects of common air.

EXPERIMENT I.

March 21, 1677. I put two onions into a receiver, which was to be ſtopped cloſe with a ſcrew, and I intruded ſo much common air thereinto, that raiſed the mercury 60 digits above its wonted preſſure.

March 28. MY onions took root, as well as other onions, which I had included in common air at the ſame time.

April 28. THE onions included in common air eight days ago were covered with mouldineſs, though in the beginning they had put forth roots numerous enough: the onions in the other receiver began to contract corruption at the ends of their roots; but the compreſſed air 10 days before had found a gradual paſſage out, and now was almoſt all eſcaped: and therefore I put in new air, till the mercury had attained to the height of 60 digits above its accuſtomed preſſure.

April 29. THE onions in the compreſſed air were all over covered with mouldineſs.

FROM this experiment it ſeems to follow, that a little compreſſion doth not prejudice thoſe bodies, which are to be expanded by vegetation.

MOREOVER, the new air, which was intruded, ſeems to have promoted the mouldineſs; though, in the beginning, it is probable, that the compreſſion of the air did retard both the mouldineſs, and alſo the corruption.

EXPERIMENT II.

May 9. I put two equal quantities of tulips and lark-ſpurs into two receivers of an equal bigneſs, and ſtopped them up firmly

firmly with ſcrews: I left one of them with common air only; but I compreſſed the other with the intruſion of new air, till the mercury did exceed its wonted height by 70 digits.

May 11. Two tulips, in the common air, contracted mouldineſs; but all things remained unaltered in the compreſſed air.

May 12. A third tulip, in the common air, began to be finewed; but there was no ſuch thing in the compreſſed air.

May 14. THIS day I perceived one tulip in the compreſſed air to be infected with ſome mucor or finew; but thoſe, which remained in the common air, were all very mucid; and alſo one of the larkſpurs, in the common air, had contracted a mucor.

May 17. THREE of the tulips, in the compreſſed air, had indeed contracted a finew, but not half ſo much as tulips in the common air were covered with. And moreover, two of the larkſpurs in the common air appeared finewed alſo; but thoſe ſhut up in compreſſed air, were preſerved freſh, and wholly free from mouldineſs or finew.

May 21. THE flowers in the common air were all rotten and putrified; but the other in the compreſſed air, received no further alteration: and beſides, the tulips, which had contracted ſome finew, ſeemed rather to loſe that, than to acquire new.

May 30. WHEN the flowers, in the common air, being wholly putrid, were diſſolved into water, I took them out, and kept the liquor in the veſſel, to try whether any inſects would breed therein. In the compreſſed air the flowers ſuffered no more ſenſible alteration; and therefore I took them out, and found them madid, and infected with a ſubacid odour.

BY this experiment it ſeems plain, that compreſſed air doth hinder putrefaction and mouldineſs in ſome plants.

EXPERIMENT III.

May 21, 1677. I cut an orange into equal parts, and one of the halfs I ſtopped up in a receiver, with air ſo compreſſed, that it would ſuſtain 100 digits of mercury above is wonted preſſure; but I left the other half in another receiver, well ſhut, only with common air.

May 25. EACH half of the orange had contracted mouldineſs, but that which was in the common air, was much more mucid than the other.

May 26. THIS day I perceived that the compreſſed air had almoſt all got out, and therefore I put in new.

May 30. EVERY day I perceived ſome air had got forth, and therefore I made a daily ſupply by adding new. And it came to paſs that the orange, by receiving new air, ſo often admitted, had contracted a mucor, notwithſtanding the compreſſion, much more than the other piece of orange, that was always left in the ſame air without preſſure.

June 1. I took out the two half oranges, and that which remained in the compreſſed air, ſeemed to have contracted a corruption, at leaſt, three times greater than that, which had continued in the common air.

BY this experiment, the aptitude of compreſſed air, to retard corruption, is confirmed; yet, in progreſs of time, it is very probable, that the quantity of corruption doth depend upon the quantity of the air. See *Exper.* I.

EXPERIMENT IV.

May 31, 1677. I included two equal quantities of roſes in two receivers, which I ſtopped by the help of ſcrews, into one of which I intruded as much air as would ſuffice to ſuſtain 90 digits of mercury, beſides its accuſtomed preſſure; but I left the other only with common air.

June 11. THE roſes, in the common air, were free from mouldineſs, only they ſeemed to have loſt ſomething of their colour; but thoſe, which were ſhut up in the compreſſed air, had almoſt all contracted a yellow colour, as if they had withered in the open air, and yet they were not mucid or finewed.

June

June 18. THIS last week the flowers in the common air admitted not the least change; but those in the compressed air grew more and more yellow. I opened both receivers, and found the roses to have kept their smell, yet it was somewhat altered; neither of them were dry nor withered. I kept them apart in the open air, and found that the roses, taken out from the compressed air, were not so soon altered by the contact of new air, as those which had remained in the air not compressed.

FROM this experiment it seems to follow, that compressed air is sometimes fitter for the alteration of colour than common air. And perhaps it may not be unworthy of our notice, that roses so included contract not a mouldiness, but only a yellow colour; but in tulips and larkspurs, the matter succeeded otherwise. See *Exper.* II.

EXPERIMENT V.

June 1, 1677. I put the two halfs of the same orange in two receivers: in the one I increased the quantity of air till it sustained the mercury 100 digits above its wonted height; but I left the other uncompressed, only exactly shut.

June 6. EACH half of the orange was infected with mouldiness, especially that, whose ambient air was compressed. But note, that new air was every day to be supplied thereunto; for the compressed air in 24 hours space had almost all got out: but in *Exper.* III. it had remained very well shut in for six whole days.

June 11. THE orange in the common air contracted no more mouldiness; but in the compressed air, the mucor or mouldiness was more and more increased.

June 18. FINDING the mouldiness of the orange in the common air to be lessened, rather than increased, I took it out; and perceiving further, that in compressed air the orange was not more mucid, after I had ceased to intrude new air, I was willing to try, whether the new air did suppeditate new strength to the orange, to exert and thrust out its mouldiness; therefore I made the mercury in the gage, by reason of the air I intruded, to exceed its wonted height 80 digits.

June 20. Two days after I had intruded new air into the receiver, the mouldiness of the orange appeared to be manifestly augmented.

FROM this experiment we may gather, that the quantity of the mouldiness doth depend on the quantity of the air.

EXPERIMENT VI.

June 17, 1677. I put two shrew-mice into two receivers, of equal bigness, and stopped them up carefully: in one of them I left only common air; into the other I intruded air, till the mercury was higher than its wonted pressure 30 digits: but the mouse in the common air was included about 5 and 52′, 6′ after the other.

THE mouse in the compressed air seemed to lose his strength much sooner than the other, the motion of his breast being less frequent. Yet notwithstanding about 6 and 18′, the mouse in the common air, which seemed the stronger, fell into convulsive fits and died; but the mouse in the compressed air seemed then, and some time after, to be as well as it was an hour and half before.

ABOUT eleven of the clock, the mouse in the compressed air did as yet breathe, but about four in the morning he was found dead in the same posture, wherein he was seven hours before: whence we may conjecture, that he was free from convulsive fits.

I must not here omit to relate, that the mouse in the common air had consumed something of that air, so that the mercury stood at 29 digits, which, when the receiver opened, presently ascended to 30.

FROM this experiment we learn, that compressed air seems fitter than common air for the prolongation of life, seeing the one mouse lived 24′ and no more, but the other lived about 15 turns longer, though only a double quantity of air was included in his receiver.

EXPE-

EXPERIMENT VII.

June 13, 1677. I put four flies into a receiver, into which I afterwards intruded air, till the mercury did occupy 60 digits above its wonted height; and at the ſame time I included three other flies in another receiver, with common air not compreſſed.

June 14. THIS day in the morning all the flies were well. In the afternoon I found two of them dead in the compreſſed air, but in the common air they were all alive. About five of the clock one of the flies in the compreſſed air was alive, and three in the common air.

June 15. THIS morning I found all the flies in the common air dead; but that ſingle one, which remained alive in the compreſſed air, ſeemed ſtill to be very well, and being taken out of the receiver, flew ſpeedily away.

FROM this experiment it ſeems to follow, that flies are not very ſenſible of the compreſſion of the air; and that they die more for hunger than for default of air: for the flie, which was ſo long well, fed upon the carcaſſes of thoſe which were dead, ſo that ſhe ſeemed to be affected with no diſtemper. Yet I iterated the experiment. See *Exper.* VIII.

EXPERIMENT VIII.

June 15. I repeated the former experiment, only including four flies in each receiver, and compreſſing the air ſomewhat more.

June 16. THIS morning I found two of the flies in the common air dead, and but one in the compreſſed air.

ABOUT two in the afternoon the four flies in the common air ſeemed to be dead; but in the compreſſed air, the three were alive

June 17. ALL the flies died, except one in the compreſſed air.

FROM this, and the former experiment, a man may conjecture, that the compreſſion of the air is of ſmall conſequence to flies; and indeed they are not prejudiced by the rarefaction of the air, but with great difficulty, unleſs there be almoſt a complete *vacuum*.

EXPERIMENT IX.

June 18. I included two frogs in two receivers, and ſtopped them by the help of ſcrews; the one only with common air, the other with air compreſſed to ſuſtain 70 digits of mercury.

June 19. BOTH the frogs were alive; and the height of the mercury in both receivers remained the ſame.

June 20. NEITHER of the frogs were dead, and they ſeemed to me rather to diminiſh than increaſe the air, but the difference was ſo ſmall, that I dare not be poſitive therein.

June 21. IN the morning both the frogs were alive; but towards evening the frog in the common air was found dead.

June 22. AT evening the frog in the compreſſed air was alive.

June 23. IN the morning I found the frog dead.

IT muſt be found out by iterated experiments, whether the greater length of life was to be aſcribed to the compreſſion of the air, or to the diſpoſition of the frogs.

EXPERIMENT X.

June 18, 1677. I ſhut two half parts of the ſame orange in two receivers, and ſtopped them by the help of ſcrews; the one with common air, the other with air compreſſed to ſuſtain 90 digits of mercury.

June 22. THIS morning I found the orange in the common air to be infected with mouldineſs, but the other was ſound.

AT three of the clock in the afternoon, the orange in the compreſſed air ſeemed alſo to have contracted ſome mucor.

June 23. I found the orange in the common air far more mucid than the other.

June 24. THE orange in the common air did not increaſe his mouldineſs, but the other was covered all over with it.

June 28. THE mouldineſs produced in the common air was now wholly vaniſhed; in the other receiver, I ſaw no further alteration in the fruit.

June 30. PERCEIVING that the fruits perſiſted in the ſame ſtate, I took them out. The half orange, which was kept in common air, ſeemed half rotten; but the other beſides its finew, appeared wholly putrified.

BY this experiment we have a confirmation, that the quantity of the mouldineſs or finew doth depend upon the quantity of the air.

IT ſeems alſo worthy of obſervation, that the mouldineſs, or hoarineſs, did appear a little later in the compreſſed air than in the common, though afterwards it increaſed much more.

EXPERIMENT XI.

June 29, 1677. I included roſes in two receivers, ſtopped by the help of ſcrews; I left one with common air only, but I filled the other with ſo much air, intruded by force, that the mercury aſcended to 90 digits above its wonted preſſure.

July 14. FOUR or five days ago I found the roſes in the compreſſed air to wither and to degenerate into a yellow colour. There was not the leaſt alteration in the other receiver.

July 17. WHEN I perceived, that this preſent experiment proceeded after the ſame manner, as that mentioned p. 121. I took out the roſes. Thoſe kept in the compreſſed air, were very much corrupted and of a very ungrateful ſmell; but the others we little altered; and their ſmell not unpleaſant.

HENCE we have a further confirmation, that the quantity of corruption doth depend on the quantity of the air.

EXPERIMENT XII.

July 4. I cut a lemon aſunder, and put both halfs into two receivers, to be ſtopped by the help of ſcrews: the one I left with common air only, but the other I filled with ſo much compreſſed air, that it ſuſtained 90 digits of mercury above its wonted preſſure.

July 7. THIS day both parts of the lemon ſeemed to grow mouldy at the ſame time.

July 17. THE part of the lemon in the compreſſed air had contracted much more of hoar or finew, than the other: and perceiving no further alteration in them, I took them out, and found the lemon in the compreſſed air far more putrid than the other.

BY this experiment it is confirmed, that the quantity of corruption doth depend on the quantity of the air.

IT ſeems alſo, that a triple compreſſion of the air, in reſpect of a lemon, is too weak ſenſibly to retard the production of mouldineſs or finew.

EXPERIMENT XIII.

July 18, 1677. I included two parcels of gilliflowers, equal in number, in two equal receivers, and ſtopped them cloſe with ſcrews. I filled the one with compreſſed air, till it ſuſtained 100 digits of mercury above the wonted preſſure; but the other was left with common air alone.

July 23. IN the compreſſed air, the gilliflowers were bedewed with ſome hoarineſs or mould; the others appeared only moiſt: but the mercury exceeded its wonted height only 70 digits, becauſe ſome of the air had got forth.

July 25. IN the compreſſed air, the gilliflowers proceeded to be much more corrupted than the others: they had wholly loſt their colour.

July 26. IN the compreſſed air, the gilliflowers were wholly putrified, and covered with an hoary finew: the others were moiſt only in ſome places.

Aug. 1. PERCEIVING no further alteration in the gilliflowers, I took them out of their receivers: thoſe which were kept in compreſſed air were rotten, and did ſtink; but the other kept their colour, and their ſmell was not offenſive, but they were moiſt.

THIS

THIS experiment confirms, that the quantity of the air doth increaſe corruption.

WE may alſo obſerve, that the mouldineſs or hoarineſs is not produced, but in compreſſed air; neither is it probable, that this happened by chance, ſeeing, in each receiver, there were four gilliflowers included, or three at leaſt.

EXPERIMENT XIV.

July 21, 1677. I included a ſhrew-mouſe in a recipient, with common air, and ſhut it in firmly with a ſcrew, to try whether he would produce or conſume air.

AFTER two hours the mouſe died, and ſome air was conſumed, but a leſs quantity than in the experiment mentioned, p. 121.

July 24. HITHERTO I found no change in the height of the mercury.

TOWARDS evening it ſeemed a little higher.

July 25. THIS day in the morning much air was produced *de novo*.

July 26. THE quantity of the produced air increaſed more and more.

BY this experiment we have a confirmation, that living animals do conſume air, but dead ones produce new.

EXPERIMENT XV.

Compreſſed air.

Aug. 31. I put pears into a receiver, whereto, after it was well ſtopped, I added as much air, as ſufficed to ſuſtain 30 digits of mercury above the wonted preſſure.

Sept. 1. THE mercury was depreſſed, as it happened fol. 522.

Sept. 2. THE height of the mercury decreaſed: it exceeded not 25 digits.

Sept. 3. THIS day the mercury proceeded one digit higher: it ſtayed in 26.

Sept 4. THE height thereof was 28.

Sept. 8. BECAUSE the receiver did afford ſome efflux to the air, I therefore put in new: and this day, opening the receiver, to compare the taſte of theſe fruits with the taſte of the others, I found, that five of the pears had loſt their firmneſs, but two had retained it.

Common air.

Aug. 31. I included pears of the ſame kind in another receiver, with common air only, not compreſſed.

Sept. 1. THE mercury was a little depreſſed, as if it had been in compreſſed air: the cauſe whereof I judge attributable only to the cold.

Sept. 2. THE mercury was not changed.

Sept. 3. THE height of the mercury was one digit above the wonted preſſure.

Sept. 4.	The height of it was	4
5.		$6\frac{3}{4}$
6.		$6\frac{1}{3}$
7.		12

Sept. 8. THE height of the mercury was 20, The pears being taken out of the receiver, had preſerved their taſte much better than thoſe, which were included *in vacuo*. They alſo retained their firmneſs.

Vacuum.

Aug. 31. I included pears of the ſame ſort *in vacuo*; but ſome external air brake in, and the height of the mercury was one digit.

Sept. 1.	The height of it was	4
2.		8
3.		12
4		16
5.		19
6.		23
7.		27
8.		30

THE pears, being taken out, had kept their firmneſs, but had loſt much of their taſte.

FROM this experiment made in three receivers at once, it ſeems to follow, that in a greater compreſſion, a leſs quantity of air is produced.

EXPERIMENT XVI.

Dec. 7. I ſhut up a ſmall bird in a receiver, capable of holding twenty ounces of water. The bird began to be ill, before I had ſet the ſcrew; but after I had intruded ſo much air, as could ſuſtain 30 digits of mercury, above its wonted height, ſhe ſeemed to recover again; but in ſome ſpace of time after, ſhe began again to be ſick, and therefore I intruded air the ſecond time, till the mercury ſtaid in 45 digits above its wonted height, and then the bird was again reſtored to health, but a little time after ſhe began to gaſp again; then opening the receiver, after ſhe had ſtaid in it 28 minutes, ſhe got out, and was very well.

EXPERIMENT XVII.

Jan. 20, 1678. I put a ſhrew mouſe into the receiver of my wind-gun, whoſe elliptick aperture was ſituate in its upper part, the figure of it is ſet down p. 516, 517. Then as quick as I could, I ſo far condenſed the air there, till it was reduced to the twentieth part of its ſpace, or thereabouts; and then I preſently diſcharged that air, and the elliptick hole being opened, I ſuſpected, that the mouſe had been only a little convulſive; but when he was taken out, there were no ſigns of life in him. And therefore it is left to enquiry, whether the cauſe of his death were to be aſcribed to the narrowneſs of the receiver, or to the compreſſion of the air?

WHEREFORE I put another mouſe into the ſame receiver, and the air being reduced to a third or fourth part of its ſpace, I opened the receiver, but not ſo carefully as I had done in the former experiment; yet the mouſe taken out therefrom, was found to be very well.

I afterward repeated the ſame experiment, the air being about ſeven or eight times condenſed, and the mouſe ſeemed to ſuffer no inconvenience thereby.

I tried the ſame experiment again in air, compreſſed ſeven times, and left the mouſe included for 24 minutes; which time being elapſed, I diſcharged the air, and the hole being opened, I perceived the mouſe to fetch many deep groans, as it were; yet, being taken out, he could not recover his health again.

BY theſe experiment it is manifeſt, that a great compreſſion of air is noxious, yea mortiferous to animals.

EXPERIMENT XVIII.

Jan. 28, 1678. I put a ſhrew mouſe into a glaſs, to whoſe neck I tied a bladder ſtopping the orifice. Theſe things being thus prepared, I put them into a receiver for the compreſſing of the air. A little time after, when the mouſe began to be ſick, I compreſſed the air, and the bladder was ſtraitned, and ſo the mouſe was found in compreſſed air, though no new air could penetrate to him: then he ſeemed to be much better, and his heart did not pant ſo often; and opening the receiver, in a ſhort time was as well as ever.

I iterated the ſame experiment, and the mouſe was left there ſo long, that he could hardly breathe, whilſt I began to compreſs the air; and the compreſſion ſeemed again to abate his reſpiration; the receiver being opened, and ſo the mouſe expoſed to the air could not breath much more freely; but if I blew the air on him by bellows, he ſeemed to be ſomething relieved; but being again committed to the compreſſed air, he breathed leſs frequently, and at laſt died.

March 25. BECAUSE in the former experiment it was not clearly manifeſt, whether the air did enter through the ligature of the bladder, I uſed the inſtrument deſcribed p. 516. And when I perceived, that the mouſe was ſick, and breathed ſeldom, I intruded water into the receiver, ſo that the air was reduced to half of its ſpace, and then the mouſe breathed more rarely; but if, extracting the water, I left the whole ſpace entire for the air, his reſpiration ſeemed more vivid; and the air being thus many times contracted and dilated, the ſick mouſe ſeemed to me to breathe more lively in the common air, than in the compreſſed. Whence I conjectured, that the air is to animals like food, the quantity whereof ought to bear ſome proportion with their ſtrength: and that I might more certainly know it, I put the ſame mouſe into my pneumatick engine, and rarefied the air, ſo that it poſſeſſed more than double the ſpace it was wont; whilſt the air was rarefying, preſently

ſently the mouſe began to be better; yet a little while after he ſeemed to be ſick, and when the air was reſtored, it brought no ſenſible commodity or inconvenience to the mouſe. I thus repeated the rarefaction three times, and the ſame ſucceſs followed; but at laſt the mouſe died.

ARTICLE V.

The effects of artificial air upon animals.

EXPERIMENT I.

May 5, 1677. I put a bee, with vinegar diſtilled, and pulverized coral, into an emptied recipient; and the air being wholly exhauſted, I ordered the matter ſo, that the coral fell down into the glaſs of vinegar: but the air, produced from thence, did not reſtore any power or motion to the bee; but when ſhe was expoſed to the open air, in a little time after ſhe began to move herſelf.

HENCE a ſuſpicion doth ariſe, that artificial air is unfit for the life of animals.

EXPERIMENT II.

Aug. 12, 1676. I put two flies into a receiver, and exhauſting the common air, I ſubſtituted air, produced from gooſberries, in its place, as much as could ſuſtain 26 digits of mercury.

AFTERWARDS I put two other flies alſo *in vacuo*; but with this difference, that I reſtored common air to theſe latter flies, only in that quantity, as could ſuſtain 23 digits of mercury.

WITHIN a quarter of an hour, theſe latter flies, upon the reſtitution of the air, recovered that power of motion, which they had loſt *in vacuo*, and did flie in the rarefied air; but the former lay without any motion, though they had received a greater quantity of air.

Aug. 13. THE flies in the artificial air, ſeemed ſtill dead; but the others were luſty.

The flies taken out of the artificial air, and expoſed to the common air, remained ſo all this whole day, and yet did not recover any life.

Aug. 18. I renewed the ſame experiment, with the ſame ſucceſs, though I had reſtored a greater quantity of artificial air.

HENCE we have an high confirmation, that artificial air is noxious to the life of animals.

EXPERIMENT III.

June 22, 1677. I put paſte into three receivers, out of which I afterwards exhauſted the air.

June 23. WHEN my three receivers did this day regurgitate with air produced from the paſte, I kindled a perfumed cone, and thus kindled I put it into one of my receivers, which being preſently ſtopped, the fire, within one minute of time, went out. Then by blowing, I expelled the artificial air from the receiver, and put in fire to it, as before; and then it burned bright for a pretty long time, though I had ſhut the receiver as ſpeedily, and as accurately as before.

I tried another experiment, after the ſame manner, with a fly, and in the artificial air ſhe was preſently dead, as it were; but afterward, being expoſed to the ſun, ſhe, in a ſhort time, grew well again. Then I blowed in common air into the receiver, which being done, the fly included as before, ſuffered no inconvenience thereby.

I iterated the ſelf-ſame experiment with the ſame fly in our third receiver, being filled with artificial air, and the ſame ſucceſs followed; ſave only that this fly, when it was taken out from the artificial air, could not be reſtored to health, but in a longer time, *viz.* becauſe ſhe was left there longer.

BY theſe experiments it appears, that factitious air is prejudicial to fire, as well as to the life of animals.

EXPERIMENT IV.

June 25, 1677. I put paſte into four receivers, and exhauſting the air wholly from two of them, I pumped out only half the air from the other two.

June 26. I found the two receivers, which I had left half full with common air,

air, to be quite filled with air newly produced; neither dare I affert, whether they had, for fome time, regurgitated or no, fo that the quantity of common air was much diminifhed. However the matter was, I put two flies at once into one of the receivers, after the manner before defcribed; and they, as foon as they touched the bottom of the receiver, in a very little while after remained without motion. I put a third fly into a receiver, after the fame manner, and found fhe lived a little longer there than the former. A fourth fly being thruft in, maintained her life longeft of all, yet at laft, fuffering fome convulfion, fhe lay unmoved and refupine. All the flies, after fome ftay in the artificial air, being taken out from thence, and expofed to the common, grew well in a fhort time.

I made the fame experiments in another receiver half full of artificial air, and in a manner with the fame fuccefs; but the flies, in that receiver to which only common air was blown in, recovered the power of motion and their ftrength in a fhort time.

June 27. I found one of the receivers, which was wholly evacuated of common air, to be full of artificial air; but it being cafually thrown down upon the ground, ingrefs was thereby afforded to the external air; yet I put a frog into it, which feemed not to be very fick therein.

June 30. My fourth receiver, by the power of the produced air, feemed at length forced away from his cover. I put a frog into it, in manner aforefaid, and fhe fell into high convulfions for five minutes fpace, and then lay without motion. After four minutes were elapfed, I opened the receiver, and taking out the frog, for 46 minutes fhe remained without motion; but afterwards in four or five minutes more fhe grew very well.

By thefe experiments it is evident, that artificial air is very hurtful to the life of animals; but if it be mixed with common air, it doth not fo readily produce its effects.

EXPERIMENT V.

June 28, 1677. I put pafte into four receivers, three of which I caufed to be wholly exhaufted of common air, but the fourth was left half full of air.

June 29. One of the receivers, which were wholly exhaufted, was found full of air newly produced; and a frog being put into it for four or five minutes, had ftrong convulfive fits; then for one minute it lay ftill without motion, whereupon I took the frog out, and in five minutes fhe began to move, and a while after became well again.

I took another receiver, filled with artificial air, and putting a frog into it, feven minutes were elapfed before fhe ceafed to be convulfive. And afterward, when fhe had lain one minute there without motion, I opened the receiver, and taking out the frog, found, that fhe began to ftruggle and move; yet I judged thofe motions to be the relicks of her convulfions; for after that, fhe remained unmoved for a whole half hour and more; yet at laft fhe grew well again.

As for that receiver, from which I had exhaufted only half of the air, it had fo long regurgitated with produced air, that it is very credible much common air had got out together with it. A frog being caft into it, feemed to be vehemently moved, and convulfive for 10 minutes, as the reft did, and then fhe feemed quite dead; but after a full minute was elapfed, I opened the receiver, and the frog, being expofed to the open air, within a quarter of an hour began to recover motion again.

I put a frog into a recipient, full of common air, to try, whether, the pafte being now taken out, the frog would continue her life any longer time there?

July 1. In the afternoon, I found the frog dead; in the morning fhe was alive and breathed; fo that fhe lived about 48 hours.

June 30. I caft a frog into my fourth receiver, which was wholly filled with artificial air; for feven minutes and an half ſhe was vehemently convulſive, and at laſt died; then, after two minutes, ſhe was taken out of the recipient, and yet recovered no motion at all.

July 1. PERCEIVING the frog to remain in the ſame poſture, I threw her away.

WE have a confirmation by theſe experiments, that artificial air is ſo much the more hurtful to animals, by how much the freer it is from common air.

EXPERIMENT VI.

June 30. I included paſte in two receivers, and then I exhauſted the air.

July 4. I would have put a ſhrew-mouſe, being taken by the tail, into one of my receivers, filled with artificial air; but the little vermin, with his fore feet, did ſo catch at the edges of the receiver, that he could not then be thruſt into it; and by this means the receiver, being for a while open, afforded ingreſs to the external air; yet I ſhut it again, till I had bound the legs of the mouſe, and then he was eaſily put in, and there ſuffered vehement convulſions, and after the elapſe of one minute, died: I preſently took him out, and expoſed him to the common air; but his life being wholly gone, no power of motion could be recovered.

THEN I took the other receiver, and putting a ſnail into it, did with ſome wonder obſerve, that he continued to be moved very ſtrongly for a whole quarter of an hour; but afterwards his motion was ſlower, until, about another quarter of an hour being elapſed, he lay ſtill, as if he were dead; but then being taken out of the receiver, and expoſed to the air, in a ſhort time he grew well.

I put flies into the ſame receiver; but now it had admitted too great a quantity of external air, for the flies ſuffered no prejudice.

BY this experiment we gather, that artificial air doth kill animals by ſome venomous quality, and not only by the defection of common air; for the ſnails lived a longer time *in vacuo*. See *Art.* VI. *Exper.* III.

EXPERIMENT VII.

I took a receiver, filled with air produced from cherries, and then tranſmitted that air out of that into another receiver, full of common air, in which a frog was kept: matters were ſo ordered, that the water gave place only to the artificial air entering in, and the water itſelf flowed out; and thus the frog, being included in pure artificial air, for a quarter of an hour and more ſuffered convulſions, and at laſt lay ſtill without motion: yet being after taken forth, and expoſed to the open air, ſhe grew quickly well.

IT ſeems probable by this experiment, that air produced from cherries is leſs hurtful to frogs, than that produced from paſte. See *Exper.* V.

EXPERIMENT VIII.

July 9, 1677. I put gooſberries into three empty receivers.

July 20. I found one of my recipients ſevered from his cover by the force of the produced air; I caſt a flie into it, which died in one punctum of time; a ſecond flie being likewiſe caſt into the receiver, preſently alſo died: a third flie put into the ſame receiver, ſeemed a little while to be convulſive there; but leſs than a fourth flie, which I included there, which yet before one quarter of a minute was elapſed, lay unmoved. Afterward I diſpelled the artificial air out of the receiver, by blowing, and in a little time the flies grew well.

July 24. I took another receiver, filled with air produced from gooſberries, and putting a ſhrew-mouſe into it, found, that he died there in the ſpace of one half minute.

FROM this experiment it ſeems inferrible, that air produced from fruits is leſs hurtful to animals, than air produced from minerals. For the 20th day of *July* I tried, that a mouſe did not live above a quarter

quarter of a minute in air produced out of gunpowder.

EXPERIMENT IX.

July 5, 1677. I included paſte in four receivers, having the air exhauſted from them.

ONE of thoſe receivers, being filled with factitious air, was forced from its cover, which I again ſtopped, yet not ſo ſuddenly, but ſome common air might mix with the artificial: yet I put a ſhrew-mouſe into it, who was preſently highly convulſive, and after one minute and half remained unmoved; and being preſently taken out, he ſeemed to make ſome convulſive motions, but died notwithſtanding.

July 7. I took a ſecond receiver, filled with artificial air, and having put a little bird into it, I ſuddenly ſtopped it; ſhe preſently fell into convulſive motions, and within a quarter of a minute, or a little more, died; I took her out, but it was too late, for ſhe never ſtirred more.

I blew out the artificial air from the receiver, and then, another bird of the ſame kind, being put into it, was very well; yet ſhe ſtaid there four minutes.

July 9. I took a third receiver full of artificial air, and put that bird into it, which in the former experiment had continued well, and yet ſeemed to be lively and ſound; before ſhe had been there a full quarter of a minute, ſhe lay without motion, and being preſently taken out, there appeared no ſign of life in her.

IN the afternoon I put an adder into my fourth receiver, and within two minutes he began to be ill, and to gape and pant; yet he was not wholly drprived of motion till after 24 minutes. Then after ſix minutes more, which made up half an hour, I took the adder out of the receiver, motionleſs as he was, and expoſed him to the free air; yet he did not recover life.

July 10. THE adder remained in the ſame ſtate, and gave no hope of reviviſcence.

EXPERIMENT X.

July 12, 1678. I put a bird into a receiver full of air produced out of raiſins of the ſun; ſhe died in $\frac{1}{4}$ of a minute, and though I took her out preſently, yet ſhe never ſtirred more.

July 18. I likewiſe put a ſhrew-mouſe into a receiver full of air produced from raiſins of the ſun; but a thread, left on the edge of the receiver, hindered me from ſtopping it cloſe; yet the mouſe preſently began to be very ill, and after two minutes he lay, as it were, without any motion; yet being taken out, in two or three minutes time he was well again.

EXPERIMENT XI.

Octob. 1, 1678. ABOUT ten of the clock in the morning, I included a ſhrew-mouſe with common air, in a receiver fortified againſt the external air; about 11 the mouſe was brought to ſuch ſtraits, that he could hardly breathe. I threw in another ſtrong and luſty mouſe into the ſame receiver, and preſently put on the ſtopple again: but becauſe the firſt mouſe had conſumed ſome of the air, it came to paſs, that the external air was forcibly impelled into the receiver, and ſo was able to diſpel a great part of the air ſtagnant there: and indeed, when this was done, the firſt mouſe ſeemed to be much better, neither did it die much ſooner than the other; but both of them died about noon. About four in the afternoon, I thruſt a freſh ſtrong mouſe into the ſame receiver; and leſt the external air might again expel the included air, I put him in very ſlowly and leiſurely: the iſſue was, that this third mouſe lived not three minutes entire.

WHENCE we may conjecture, that that portion of air, which hath once ſerved the reſpiration of animals, as much as it could, is no longer uſeful for the reſpiration of another animal, at leaſt of the ſame kind.

EXPERIMENT XII.

THIS day in the morning I put ſo great a quantity of paſte into an empty receiver, that

that in the afternoon I found the receiver full of factitious air: whereupon I thrust down a snail into it, which presently frothed very much, and did very often expand and again contract itself; but at length after four minutes were elapsed, he ceased to move at all; yet I took him not forth, till he had staid in the receiver an whole quarter of an hour, and then, being extracted, he seemed as if he had been quite dead; for though he were pricked with a pin, yet he discovered no sign of life; yet after another quarter of an hour, being also pricked with a pin, he made a little motion.

I blew out the factitious air from my receiver, and then thrusting in another snail after the same manner as I did the former, he was very well in the receiver, and did not froth at all.

WE have a confirmation by this experiment, that factitious air is a greater enemy to animals, than a *vacuum* is.

EXPERIMENT XIII.

June 22, 1678. THIS day, in the morning, I put green pease into an empty receiver, and towards evening the mercury had almost attained to the height of 10 digits.

June 23. THE height of the mercury was almost 30 digits.

June 24. THE mercury did not as yet exceed 30 digits in height: the cover did no longer stick to the receiver yet hitherto nothing had escaped out of it.

June 26. I included the same pease in the same empty receiver.

June 29. WHEN I now found, that the receiver was filled with factitious air, I thrust a snail into it, who put forth much spume or froth, and did very often expand and contract his horns; but after six minutes were elapsed, he lay still, as if he had been dead, for two or three minutes; then the receiver being opened, and the snail taken out, moved himself a little, if he were pricked: whence it seems to follow, that air produced from pease is less prejudicial to snails than air from paste. See *Exper.* XII. I blew new air into the receiver, and a snail then put into it did very well.

IN this experiment it seems observable, that pease do quickly produce air *in vacuo*; but in the wonted compression of air they generate but little.

ARTICLE VI.

Animals in vacuo.

EXPERIMENT I.

June 22, 1676. I put a butterfly into an empty receiver, and it was almost three hours before she was wholly deprived of her faculty of motion: at length, perceiving him to lie unmoved, I let in the air into the receiver, and in a little time the butterfly recovered his motion. Then I bound him by one of his horns with a thread, and so hanged him in the receiver, and then he was carried very freely from one part of it unto the other, by clapping his wings; but after the air was extracted, the clapping of her wings was in vain, for she could not move the thread in the least, from being perpendicular.

EXPERIMENT II.

July 12, 1676. YESTERDAY I put two flies into a receiver, in which I left $\frac{1}{3}$ of air, *i. e.* as much as would sustain 10 digits of mercury: the biggest of the flies seemed to die presently, but the other, which was a small bodied one, lived almost twenty-four hours.

WHEN both the flies lay as if they were dead, I suffered some air to enter in, till the mercury was 15 digits high; and then the lesser fly began to move her feet, but the other continued still without motion.

HENCE it appears, that air highly rarefied may serve for insects to breath in, and that it doth not kill them so soon as artificial air.

EXPERIMENT III.

May 1. I put two snails into an emptied receiver, and for an whole hour they seemed

ſeemed to be well enough, and crept up to the top of the receiver; but in two hours time, they fell down from thence, and lay without motion.

SIX hours after they were firſt put in, I took them out *è vacuo*, and within half an hour they began to move a little. During the time they were included, they produced near as much air as ſufficed to ſuſtain the mercury in the height of $\frac{1}{4}$ of a digit.

THESE ſnails lived longer *in vacuo* than the others included in artificial air. *Art.* V. *Exper.* VI.

EXPERIMENT IV.

Auguſt 12, 1676. I put fly-blowings, or the eggs of flies, into an empty receiver, to try, whether they would produce worms there or no.

Auguſt 14. I ſaw the worms were formed, but the air had crept into the receiver, ſo that it could ſuſtain 15 digits of mercury.

HENCE it appears, that inſects may be produced, and may live, if not *in vacuo*, yet at leaſt in air very highly rarefied. See *Exper.* VI. and VIII.

EXPERIMENT V.

March 17, 1677. I put two equal quantities of frog-ſpawn into two veſſels of glaſs, of equal bigneſs; I left the one included in an empty receiver, expoſed to the ſun; but the other being in a receiver full of common air, I fortified againſt the acceſs of the external air. The frog-ſpawn *in vacuo* did all ſwell into bubbles.

May 2. No frogs were produced in either receiver, and that ſeed or ſpawn, which was kept *in vacuo*, remained ſtill full of bubbles; but about three days ago all the bubbles vaniſhed, and the ſpawn was changed into a certain green liquor.

July 2. OUR receivers remained in a window, expoſed to the noon-day ſun; and ſo ſome water, that was mixed with the frog-ſpawn, all *in vacuo*, and the very ſpawn itſelf, was elevated into vapours, and afterwards ſticking to the ſides of the receiver, out of its own veſſel, was there condenſed; but the veſſel kept in the common air, ſtill contained all its water, together with the ſeed or ſpawn.

EXPERIMENT VI.

Auguſt 16, 1677. I put flies-eggs into an empty receiver.

Auguſt 29. WHEN no worms were produced out of them, I gave admiſſion to the air to enter into the receiver, and left all things in the ſame poſture, to try, whether the eggs had loſt their faculty of producing worms.

Sept. 9. THE egges produced nothing.

THIS experiment, if it be compared with *Exper.* IV. ſeems to ſhew, that inſects may be generated, and may live in air highly rarefied, but not at all *in vacuo.*

EXPERIMENT VII.

June 15. I ſhut in a frog in an emptied receiver, at about ſeven of the clock in the evening; about nine the frog died.

June 16. I repeated the ſame experiment, and again perceived. that the dead frog, in two hours ſpace, had produced ſome air, rather than conſumed it.

June 18. THE frog, left hitherto *in vacuo*, was ſwollen very much; but the air now entering, made her far more flaccid and lank, than ſhe was wont to be.

WE are inſtructed by this experiment, that a receiver void of artificial air, is leſs hurtful to the life of ſuch kind of animals. See *Exper.* IV. and VIII. of *Art.* V.

EXPERIMENT VIII.

Auguſt 3, 1678. I put fly-blowings ſticking to fleſh, into an emptied receiver.

Auguſt 12. No worms were generated from them.

Auguſt 15. PERCEIVING no change in the eggs, I opened the receiver, to try, whether they would yet be generated in the free air.

Sept. 15. NOTHING was produced from them.

WE have a confirmation by this experiment, that animals, which may be generated

rated and live in highly rarefied air, yet are killed *in vacuo*. See *Exper.* IV.

EXPERIMENT IX.

August 22, 1678. I included vinegar full of small eels, or vinegar-worms, in an emptied receiver.

August 29. THE worms were still moved, yet they were fewer than in the beginning.

Sept. 6. YESTERDAY some of those worms did still move in our vinegar, but this day I could not see one; whereupon taking a microscope, I found them all dead: but in the vinegar, which I had left in the open air, the eels made as brisk motions as at the beginning.

HENCE it appears, that those, even very diminutive animals, are also affected with the presence and absence of the air.

ARTICLE VII.

Fire in compressed air.

EXPERIMENT I.

May 14. I took a perfumed cone, of that nature, that being once kindled in the free air, it is wont by degrees wholly to be consumed, and put it into a receiver firmly stopped with a screw; and I intruded air into it, till the mercury came to 120 digits above its wonted height, and then putting to my burning glass, I kindled the cone, which presently darkned all its receiver with smoke, and after some time $\frac{7}{8}$ parts of 1 digit thereof in length were reduced to ashes: yet taking out the cone, and blowing away ashes, I found only the superficies therefore consumed, but the inner parts were untouched.

I included another cone of the same sort in a much greater receiver, but I did not compress the air therein: the cone, fired by the same burning-glass, was not taken out, till all the fumes were abated and fallen down; yet much less of this cone was burnt than of the other.

EXPERIMENT II.

May 19. I weighed a perfumed cone exactly, and then firmly included it in a receiver with common air, and I kindled it by the help of my burning-glass. When the fumes were condensed, I took the cone out of the receiver, and weighed it again; the loss of its weight was almost one grain. Then I got me many pieces of paper, each of them of the self-same weight, which I presume to call paper-grains.

AFTERWARDS the same cone, observing the same circumstances, was again included and kindled; but first I had intruded air into its receiver, as much as could sustain 90 digits of mercury: and thus, by means of a pair of scales, I found the loss of weight this time was four times more than of the former, for the cone was lighter by four paper-grains.

FROM this experiment it seems to follow, that the consumption of matter is so much the greater, by how much the greater quantity of air is contained in the receiver.

EXPERIMENT III.

May 17, 1677. I included a perfumed cone in a receiver firmly stopped by the help of a screw; and, the air being compressed to sustain 60 digits of mercury above its wonted pressure, I set fire to it with my burning-glass; the cone being afterwards taken out, had lost three paper-grains and an half in weight.

I repeated the same experiment, but in air so compressed, that the mercury reached to 120 digits above the wonted pressure: then the cone was 7 $\frac{3}{4}$ paper-grains lighter; and so, though the quantity of the air was not double, yet the consumption of the matter by the fire was more than twice as much as that was in the former experiment.

May 17. I iterated the same experiment in air compressed to sustain 97 digits of mercury, and then the loss of weight seemed to be six paper-grains.

By all these experiments we are taught, that the matter is so much the more consumed by the fire, by how much the compression of the air in the receiver is the greater; yea, the consumption seems to have a greater proportion to the consumption, than the compression hath to the compression.

May 18, 1677. I included a perfumed cone as before, in a receiver seven times larger than that, which I used in the former experiments, and I immitted no air at all into it. The cone kindled there lost $3\frac{1}{4}$ paper-grains of its weight, and no more; whereas, in the same quantity of air, if it had been reduced to a fifth part of its space, the cone would have lost 10 grains, *viz.* by observing the proportion of the consumption made before in air, sustaining mercury to 120 digits above its accustomed height, *(i. e.)* air reduced to a fifth part of its space.

From this experiment it seems to follow, that the same quantity of air, if it be reduced to less than its accustomed space, on that account alone causeth a greater consumption, than if it had remained in its wonted expansion.

Experiment IV.

May 19, 1677. I repeated the experiment last described, in the same receiver, closely stopped with a screw, that nothing might go out or in. The cone lost one paper-grain and quarter only of its weight; whence I suspect that it was not well kindled.

May 21. I made the same experiment, after the same manner. This day the cone was lighter by four paper-grains; whence I more certainly collected, that it was not well set on fire in the former experiment.

May 23. I repeated the same experiment twice, but do suspect, that the cone was not well kindled, seeing at one time it lost only $\frac{3}{4}$, and at another time one paper-grain of its weight.

May 24. I tried the same experiment again, and this day also the loss of weight was found only one paper-grain and a quarter. Then I opened my receiver, and having wiped and cleansed away the soot, I iterated the experiment, and then the cone took fire very well, for the loss of its weight amounted to six paper-grains and an half.

I tried the same experiment again in an uncleansed receiver, and then the cone lost only three paper-grains in weight.

May 25. I iterated the same experiment in a receiver well washed, and the cone was lighter by six paper-grains and an half.

I made the same experiment in the like manner, and in a well cleansed receiver, and the cone lost seven grains and an half of its weight.

I tried the same experiment again, in an unwashed receiver, and then I could not sufficiently kindle the cone.

May 26. I tried the same experiment in an unwashed receiver, about the middle of the day, the sun being clear and clouded with no mists; and I removed not my burning-glass from kindling the cone a long time, so that it took fire very well, and became eight paper-grains lighter.

By these experiments it is manifest, that the quantity of a cone to be consumed in the same quantity of air, is not fixed and certain, but sometimes lesser, as the cone shall be more or less kindled: besides, the imperfect mixture of the matter may cause some difference; yet, it seems certain, that fire is more easily kindled in compressed air, than in common; and the consumption will be the greater in a certain quantity of air, if that air be reduced into a narrower space, than if it enjoyed its wonted expansion.

Experiment V.

May 22. I put a perfumed cone into a receiver made for compressing the air; and intruding the air till the mercury staid in 30 digits above its wonted pressure I kindled the cone, and found its weight to be abated $1\frac{3}{4}$ of a paper-grain.

May

May 23. I made the same experiment again, after the same manner, and in effect, with the same success,

I tried the same experiment again, but the cone took not fire well. Whence we we have a confirmation, that fire is more easily kindled in air much compressed, than in common air, or that, which is but a little condensed.

I iterated the same experiment, and after I had removed my burning-glass from kindling the cone, whilst I was intent to see whether the cone would proceed to be consumed, the receiver brake into 100 pieces, some of which struck my head, and wounded it: which passage I mention, that so no man may be confident his glass will not break whilst he is about these experiments, because he hath found, that at other times it hath resisted a greater pressure. For this very glass of mine, had contained air four times more compressed, very well. See *Exper*. III. Yea in *Exper*. VI. of *Art*. II. it had resisted air sustaining 168 digits of mercury above its wonted height; yet, now it was broken by a pressure more than six times less: and therefore, whilst a man looks into such receivers, his head had need be fortified with some perforated or pellucid muniment and defence to preserve it from a blow.

ARTICLE VIII.

Fire used to produce air.

EXPERIMENT I.

June 4, 1676. I burnt paper, besmeared with sulphur *in vacuo*, and found, that it produced some air, which air was not at all diminished for two whole days.

THAT air is to be ascribed to the paper, for no air is produced out of sulphur alone.

EXPERIMENT II.

June 15. I burnt harts-horn *in vacuo*, and found, that the fumes issuing therefrom did contain some air in them.

THESE two last days, I iterated the same experiment, and always observed, that air produced from harts-horn, was in a short time in part destroyed; but that, which preserved the elastic nature of air for a full hour after the burning-glass was removed, seemed afterwards not to lose it at all.

June 19. I took the harts-horn out of the receiver, and found no volatile salt, but only a fœtid oil to be produced therefrom.

EXPERIMENT III.

June 21. I burnt amber *in vacuo*, and at first I could not find, that the fumes did ascend above the height of one digit; and yet in a receiver full of air, they would be carried up to the top of the receiver, and from thence be reflected downwards; yet afterwards, even in the vacuum itself, the fumes reached almost to the top of the receiver, but the mercury was not at all changed in its gage.

June 22. THIS night a great deal of that water, in which I had immersed the receiver, found a passage into it, though the cover was so well fitted to the aperture, that I never perceived any water to get in betwixt them before. Hence a suspicion arose in me, that some volatile salt had probably attracted (if I may so speak) the aqueous parts, by reason of the congruity betwixt them.

July 8. I still kept the receiver immerged in water, but no more water entered in, as if the salts being washed away, the external water, being destitute of assistance, could no longer creep in: but that agreement between the fumes of the amber and the parts of the water had need of a confirmation by a great many more experiments.

HENCE it appears, that amber produceth no air, no not though it be burnt.

EXPERIMENT IV.

Jan. 18, 1677. I put two drachms of camphire into an empty receiver, and the commiſſure of the cover with the receiver being fortified againſt external air, I put the camphire on a digeſting furnace.

Jan. 19. THE camphire was ſublimated into flowers, but no air was produced.

EXPERIMENT V.

May 24, 1676. I included *ſulphur vivum* in an exhauſted receiver, and melted it by the help of my burning glaſs, but found, that the fumes, produced therefrom, did contain no air in them, becauſe the mercury did aſcend to the aperture of its gage, as it uſeth to do while the receiver is evacuating: yet when the receiver was cooled, the mercury returned to its former height; and therefore I think that change proceeded only herefrom, becauſe the air, included in the ſealed leg of the gage, was rarefied, and drove the mercury into the other part.

EXPERIMENT VI.

July 19. HAVING included paſte nine days ago *in vacuo*, and perceiving, that it now contained no more air, I endeavoured to fire it with my burning-glaſs. The ſubſiding fumes had tinged the ſuperficies of the paſte with a curious yellow colour; and beſides I conjectured, that ſome air, was produced, becauſe the receiver, which before was ſtraitly joined to its cover, was now with eaſe plucked therefrom.

ARTICLE IX.

Concerning the production of air in vacuo.

EXPERIMENT I.

Sept. 9, 1676. I exhauſted the air out of a receiver half full of dried grapes, and fortified it againſt the external air.

Sept. 10. In 24 hours time the height of the mercury was $\frac{1}{2}$.

12. In two days time the aſcenſion of it was $\frac{1}{2}$.

14. The aſcenſion of the mercury was $\frac{3}{8}$.

17. The aſcenſion of it was $\frac{3}{8}$.

22. The aſcenſion of it was $\frac{5}{8}$.

27. The aſcenſion was $\frac{5}{8}$. The height three digits.

Octo. 11. The height of the mercury was now about ſix digits.

Sept. 9, 1676. I put dried figs into a receiver, and filled about half of it with them, and then I extracted the air, till the mercury ſtaid in the height of three digits.

Sept. 10. No air was produced.

Sept. 17. PERCEIVING no air to iſſue out of the figs, I opened the receiver.

BY this experiment we learn, that dried fruits, put into an exhauſted receiver, do produce very little air with regularity.

EXPERIMENT II.

Aug. 5, 1676. I included pears and apricocks *in vacuo*.

Aug. 6. IN 18 hours time the mercury reached two digits; in 10 hours more it reached the third digit. Its height was three digits.

Aug. 7. The height of it was five digits.

8. The height of it was 6 $\frac{1}{2}$.

9. In 14 hours ſpace the mercury mounted $\frac{3}{4}$. Its height was 7 $\frac{1}{4}$.

10.	The height of it was	8 $\frac{3}{4}$
11.		10 $\frac{3}{4}$
12.		12 $\frac{1}{4}$
13.		14 $\frac{1}{4}$
14.		16
15.		18
16.		20
18.		25
19.		29
20.		31 $\frac{1}{2}$
21.		32 $\frac{1}{2}$
22.		34
23.		35
26.		38 $\frac{1}{2}$

29. The height of the mercury was 41.

Sept. 1. The height of the mercury was 42 $\frac{1}{2}$.

4. The height of it was 44.

7. The three days laſt paſt being hotter than the foregoing, the aſcenſion of the mercury was 2 $\frac{1}{4}$. Its height was 46 $\frac{1}{4}$.

Sept.

Sept. 10. The height of the mercury was 47 $\frac{1}{2}$.

13. The mercury was depreſſed; its height was only 44 digits.

23. The mercury was by degrees again mounted to the 48th digit.

27. The height of the mercury was 50 $\frac{1}{2}$.

Nov. 5. The mercury aſcended by degrees to the height of 52 $\frac{1}{2}$.

28. The apricocks were reduced to water; the ſkin was ſevered from the pulp, yet no more air was produced.

Jan. 10, 1677. WHILST it was a very hard froſt, the mercury came to the height of 57 digits: but when the thaw came, it was depreſſed to 23. Whether the ſtrength of the froſt opened ſome way for the air to get out, I know not.

March 3. THE mercury could aſcend no higher, becauſe the air was got out. This day I found the receiver tumbled on the ground, and the apricocks, when the froſt was broke, were putrified, and had loſt their colour.

FROM this experiment it ſeems to follow, that apricocks do produce air almoſt as eaſily in their wonted preſſure, as *in vacuo*.

EXPERIMENT III.

June 20, 1676. I put four cherries into two empty receivers, and obſerved altogether the ſame circumſtances in them both; ſave, that in the one, the cherries were whole, in the other, cut aſunder. In two hours ſpace the whole cherries had impelled the mercury into the gage to the height of 10 lines; and the diſſected cherries to about 20.

June 21. IN 24 hours ſpace the mercury, which was in the receiver containing the whole cherries, came to the height of three digits; but in the other receiver the mercurial gage was ſpoiled.

June 26. THE whole cherries had not yet produced ſo much air, that could ſuſtain 15 digits of mercury; but the diſſected cherries had wholly filled their receiver with air.

July 9. THIS day the receiver of the whole cherries was removed from his cover: I did eat one of the cherries, and its taſte ſeemed pleaſant enough. I included the reſt again *in vacuo*; many of them were broke, and in one hours ſpace they impelled the mercury to aſcend to the height of about two digits.

July 10. THESE laſt 24 hours the mercury aſcended not; whether the gage was prejudiced, I am not certain.

July 15. THIS day I found the cover ſevered from his receiver, and ſo it was clear, that the gage was ſpoiled or hurt.

THIS experiment gives us a probable conſequent, that ſome diſſected fruits do ſooner produce their air than whole and undivided ones.

EXPERIMENT IV.

June 9, 1676. I put cherries (not acid ones) into an empty receiver, and within one hour I found as much air produced from them, as ſufficed to ſuſtain $\frac{1}{4}$ of a digit of mercury.

June 10. IN 18 hours the mercury ſeemed to have come to the height of 11 digits.

July 11. OUR fruits produced air leſs and leſs copiouſly; ſo that this day, towards the evening, they came not up to the height of 15 digits.

June 12. Now the mercury was a little higher than 25 digits.

13. The height of the mercury was 22 digits.

16. The mercury yet came not up to 30 digits.

18. Perceiving no more air to be produced from my fruits, I opened the receiver.

SUCH a ſmall production of air ſeemed very obſervable to me, becauſe I had found by experience, that fruits of the ſame kind in *France* had filled their receiver in two days time: it may probably come to paſs, that fruits of the ſame kind, in ſeveral countries,

countries, may differ much amongst themselves.

EXPERIMENT V.

June 12, 1676. I put cabbages cut in pieces into an empty recipient with a mercurial gage, and in one hours space the mercury had made one line.

June 13. The mercury was now come almost to the height of 10 digits.

17. The mercury was come almost to the top of its gage, and the receiver being opened, I found the cabbages little altered.

19. The cabbages being left two days in the open air, were wholly corrupted and blackish. I put them again *in vacuo*, to try, whether the putrefaction begun would promote, or else retard the production of air.

19. The mercury in half an hour ran up $\frac{1}{2}$ of a digit.

22. For three whole days the mercury got higher only 10 lines. Its height was 1 and $\frac{1}{3}$ of a digit.

23. Finding, that the cabbages produced no more air, I took them out of the receiver: their smell was very bad.

HENCE a suspicion arose within me, that bodies, when they putrefy, have already produced almost all their air.

EXPERIMENT VI.

May 29, 1676. I took pieces of orange weighing four ounces, and put them into a receiver capable of holding 10 ounces of water, and I exhausted the air.

June 10. THIS day the receiver was removed from his cover by the force of the produced air; so that I took out the oranges, and presently put them into another empty receiver capable of containing eight ounces of water, and the mercury, within half an hour, was elevated to the height of one half digit.

June 13. THAT sudden ascension of the mercury was not durable, for it yet came not to the height of two digits.

June 16. THE mercury, the last 24 hours, ascended about three lines.

June 21, THE mercury, these last 24 hours, did not ascend the space of one line.

July 18. I perceived no more alteration was made in the height of the mercury; but some mouldiness appeared, though I am certain, that no air from without had found any ingress into the receiver.

EXPERIMENT VII.

April 27, 1676. I put a tulip into an empty receiver with a mercurial gage; but before it was fortified against the external air, some air had got in, enough to sustain two digits of mercury.

May 2. THE tulip, which first seemed striped with sundry colours, was now wholly changed into a dark red, and was moist: it produced very little air.

EXPERIMENT VIII.

April 22, 1676. I put half of a lemon into an empty receiver, with a mercurial gage so short, that the mercury could run up the space of three digits.

April 24. In two days space the mercury came to the height of one digit and a half.

25. The mercury was now two digits high.

27. Yesterday the mercury made four lines, but this day only one.

29. The two last days the mercury mounted higher by one line.

May 3. In four days space the mercury ascended one line and a little more.

May 3, 1677. THE mercury came to the top of its gage, yet no air got out; but the lemon was little altered.

Jan.

Jan. 1, 1678. As yet no air eſcaped out of the receiver; but the lemon had contracted a yellow colour, and moiſture therewith.

EXPERIMENT IX.

March 16, 1677. I put two apples of the ſame ſort in two empty receivers; one of the apples began to putrify before, the other was only bruiſed with a few blows.

May 15, 1677. As yet the fruits were in very good caſe; but this day that apple, which was bruiſed, appeared wholly rotten, and the receiver was forced from his cover; the other apple remained without any change.

Aug. 20. 1677. THAT apple, which before began to be rotten, ſuffered no farther alteration; but this day finding, that the receiver was pulled from his cover, and fearing leſt the apple would be ſpeedily putrified, I took it out: its taſte was grateful, but ſubacid, as if it had been fermented; but the pulp inclined to the conſiſtence of meal.

FROM this experiment it ſeems to be confirmed, that fruits have produced the greateſt part of their air, when putrefaction begins to alter them; ſeeing the putrid apple did not fill its receiver but in a much longer time than the other apple. See *Exper.* V. of this article.

EXPERIMENT X.

May 17, 1676. I poured two equal quantities of milk into two glaſs receivers of equal bigneſs; the one I left in the free air, the other I included to be kept in an emptied veſſel with a mercurial gage.

May 18. THE cream did ſwim on the top of that milk, which was left in the free air; but that, which was *in vacuo*, was only covered with bubbles, and the gage was not changed at all.

May 19. THE bubbles ſwelled more and more, and the mercury in the gage was a little higher.

May 20. THE bubbles *in vacuo* ſwelled yet more, and that milk ſeemed curdled; but the other, in the free air, was manifeſtly curdled. The mercury *in vacuo* came almoſt to the top of its gage.

May 22. THE milk *in vacuo* proceeded to generate air more and more, and now it evidently appeared to be curdled; whence it is manifeſt, that the coagulation of milk, when the air is taken away, is retarded. Now almoſt all the bubbles were broke.

June 20. THE milk *in vacuo* was no longer covered with bubbles, and remained ſtill coagulated in the ſame ſtate. But the milk in the free air ſtank filthily, and was full of worms: when it was put on the engine, and the air extracted, it did emit many very great bubbles for a long time; and the worms did move themſelves very vehemently, but not one of them died in four hours ſpace.

May 19, 1677. THREE or four months ago, ſome whey *in vacuo* was poured out of a veſſel into a receiver, and it ſeemed clear and limpid like water; yet there was whey enough left in the veſſel to ſeparate the butyrous from the caſeous part at a ſufficient diſtance.

THIS day the milk ſtagnant in the receiver ſeemed to have got out of it; ſo that it is clear, that the air in the receiver was of greater force than the external air, for the cover alſo was forced from the receiver. Towards night I took that milk out of the receiver, and found it to be acid both in ſmell and taſte, yet it was not unacceptable to the palate; but after a ſhort time, the whey, which hitherto had remained limpid between the caſeous and butyrous part, began to diſappear, and to be blended with the reſt.

May 24. THIS day the butyrous part was wholly vaniſhed, though as yet it had ſuffered no ſenſible mutation; but the milk began to ſmell amiſs.

June 1. OUR milk had not yet contracted the worſt of ſmell, neither had it produced any worms, but it grew dry by degrees; and this night the mice eat it up, as perhaps they had done the butyrous part before.

THIS is the ſtory of my preſerved milk, in which theſe four things ſeem moſt obſervable. Firſt, that the coagulation of milk, when air is extracted thereform, is ſomewhat retarded. Secondly, the weight of butter, or of whey, or cheeſe, is not the ſame in the air, as it is *in vacuo*; for in the air they are mixed one with another confuſedly: but *in vacuo* one ſwims on the top of the other. Thirdly, the putrefaction of milk, when air is extracted, is hindered or very much retarded. Fourthly and laſtly, milk, by long continuance *in vacuo*, is made unfit to generate worms, even in common air.

EXPERIMENT XI.

Sept. 5, 1677. I took the ſame receiver and the ſame veſſel, which I uſed before to preſerve milk *in vacuo*, and I included urine therein, after the ſame manner as I had done milk before. The quantity of urine was three drachms, or thereabouts; and the receiver was only capable of holding 10 ounces of water.

Sept. 7. THE mercury reached to the height of almoſt two digits.

Sept. 8. THE mercury was this day ſomewhat higher than yeſterday.

Dec. 5. THE mercury aſcended not above three digits in height, and for the whole month paſt was not changed at all. The urine ſeemed not all to be altered.

Dec. 6. I ſet other urine under a receiver, not fortified againſt the external air.

Dec. 16. THE urine *in vacuo* ſtill kept unaltered, but the other in 10 days time ſeemed turbid, and to have contracted ſome mouldineſs in its ſuperficies.

THIS experiment, compared with the former, gives us a probable inference, that urine, which is an excrementitious humour, contains leſs air in it than milk, which is alimental.

MOREOVER the efficacy of the air to corrupt urine ſeems very obſervable.

EXPERIMENT XII.

May 19. I took paſte very much diluted, and without leaven, and put it in a glaſs veſſel into an empty receiver; and though the veſſel, which contained it, were not half full, before all the air was exhauſted, yet the paſte had ſwollen above the brims of the veſſel.

May 20. THE paſte continued to ſwell more and more, and was interſperſed with many cavities.

May 22. THIS day the paſte was much more tumid than before, and much air was generated therefrom.

May 23. THIS day in the morning I found the cover ſevered from his receiver, by the force of the produced air, and ſome of the paſte was ſpread above the edges of the receiver; yet its ſwelling was ſomewhat abated. In the afternoon, its tumidneſs was much more abated, yet it took up twice more room than it did before it was put into the receiver. The taſte of it was not acid, and therefore I think that bread, thus made, is very light.

EXPERIMENT XIII.

July 20, 1676. I took a quantity of beef, and put it into an exhauſted receiver fortified againſt the external air; and likewiſe I put another equal quantity of beef into a receiver, neither exhauſted nor cloſely ſtopped.

July 21. IN thirty hours ſpace, the exhauſted receiver was all filled with air, ſo that I ſuſpected ſome air had got in; and therefore I included the ſame beef again, and ſo cloſed it, that there was no fear of the ingreſs of any external air.

July 22. IN fourteen hours ſpace the mercury came to the height of 15 digits.

July 25. FOR three whole days and more, the beef did not produce ſo much air as would fill one half of the receiver.

July 29. THIS day the receiver was ſevered from his cover; and in one hour's ſpace, I perceived, that the beef, being again included *in vacuo*, had produced air, which ſufficed to ſuſtain 10 digits of mercury.

July 28. I found the receiver again filled with air, and re-exhauſted it, much air was in a ſhort time again produced from the beef.

July

July 30. The receiver being again filled, I included the beef again *in vacuo*, and found, that the air produced from it in one hour's ſpace was able to ſuſtain 10 digits of mercury.

Aug. 1. The receiver being this day filled again, the beef ſtank ſo filthily, that we threw it out of doors.

Hence it appears, that fleſh, whilſt it putrifies, doth produce much more air, than before it putrifies; but it is otherwiſe with fruits. See *Exp.* IX. of this Art.

EXPERIMENT XIV.

July 18, 1676. I put ſome gooſberries, which I had kept long in receivers to produce air, into a vacuous receiver.

Within half an hour the mercury aſcended to the height of one digit.

In an hour and half's time, the mercury mounted to another digit.

July 19. In twenty-four hours time the receiver was almoſt all filled with air.

July 20. The cover was forced from his receiver, and much juice had run out of the receiver.

July 29. I left the ſame gooſberries in a receiver, not hitherto fortified againſt the external air; but this day I included them again *in vacuo*, to try, whether they could produce any more air.

July 30. In ſixteen hours time, the gooſberries drave up the mercury a digit and $\frac{1}{2}$ into the gage.

July 30, 1677. The gooſberries could not wholly fill the receiver, and they always remained in the ſame ſtate; but a while ſince they had almoſt loſt their red colour, and inclined to white.

From this experiment it ſeems to follow, that theſe fruits, after they have produced all their air, admit very little alteration; as if that air itſelf were the cauſe of corruption.

EXPERIMENT XV.

Aug. 23. I put pears into a *vacuous* receiver with a mercurial gage; and before the receiver could be well fortified againſt the ingreſs of the air, the mercury was come to the height of one digit and an half.

In two hours ſpace the mercury aſcended four digits; its height was almoſt ſix.

Aug. 24. The height of the mercury twelve digits.

Aug. 25. The height thereof was ſixteen.

Aug. 26.	The height of it was	18
27.		21
28.		23
31.		30
Sept. 1.		32
2.		35
3.		$38\frac{1}{2}$
4.		$44\frac{1}{2}$
5.		$45\frac{1}{2}$
6.		50

Sept. 7. The height of it was the ſame, becauſe ſome air had eſcaped; but I prevented that for the future.

8. The height of the mercury was $53\frac{1}{2}$.

9. The height of it was $54\frac{1}{2}$.

10. The height of it was 58.

Sept. 12. Yeſterday the mercury perſiſted in the ſame height; but this day it ſeemed to be depreſſed: whence I conjecture, that ſome air had got out. The height of it was $53\frac{1}{2}$.

Sept. 13. I tranſmitted the air into another receiver: the height of it was $32\frac{1}{2}$.

Sept. 16. I perceived, that the air had got out; and opening the receiver, I found the pears very rotten.

These pears produced their air irregularly enough, ſometimes quicker, ſometimes more ſlowly.

EXPERIMENT XVI.

Sept. 17. I put dried plums into an evacuated-receiver.

Sept. 19. The mercury ſeemed to have aſcended a little.

Sept. 22. I perceived not that the height of the mercury was any more altered.

Nov. 8. When I ſaw, that the plumbs produced no more air, I opened the receiver.

By this experiment we have a confirmation, that dried fruits are very unfit to produce air.

EXPERIMENT XVII.

Sept. 28. I put freſh nut-kernels, cut into pieces, having thrown away their ſhells, into an evacuated receiver, with a mercurial gage.

Sept. 29. The mercury aſcended a little.
30. The height of it was two digits.

Octob. 5. The mercury proceeded to aſcend by degrees: the height of it exceeded ſix digits.

Octob. 15. The height thereof was ten digits.

Octob. 22. The height of it was fifteen.

Nov. 28. The mercury was come to the height of twenty digits, or a little more; but this day the receiver was caſt down and broken, and the nut-kernels thrown about: they were kept very well, both as to colour and taſte.

HENCE we may conjecture, that air without ſenſible putrefaction may be produced from fruits, even of an hard conſiſtence.

ARTICLE X.

Concerning the production of air above its wonted preſſure.

EXPERIMENT I.

I included new peaſe in a receiver, with a glaſs full of raiſins of the ſun bruiſed, and mixed with water; I did not exhauſt the air.

TOWARDS evening the mercury had mounted to twelve digits; but a great part of that air was produced from the raiſins, not from the peaſe.

June 23. The height of the mercury was 49.

June 24.	The height of it was	75
25.		90
26.		90
28.		100

The peaſe did, as it were, ſweat, and grow yellow.

June 30. The height of the mercury was 110.

July 1. The mercury aſcended not, yet no air eſcaped out.
4. The height of the mercury was 124.
7. The height of it was 140.

July 10. The height remained the ſame, but the liquor, which diſtilled or ſweat out from the peaſe, got out.
12. New liquor was produced from the peaſe, but the mercury continued in the ſame height.

July 13. The liquor got out of the receiver, and ſome air beſides: whereupon I ſet the ſcrew, and new liquor being in a ſhort time collected did fortify the cover within.

July 15. THIS day the receiver was broken in pieces; but the peaſe being ſofter than ordinary, were eaſily ſtript of their huſks, as if they had begun to be boiled: they kept their ordinary taſte.

EXPERIMENT II.

Sept. 15, 1676. I put unripe plumbs into a vacuated receiver; but before the receiver could be guarded againſt the external air, the mercury had already aſcended to the height of one digit.

Sept. 16. IN twenty-four hours time the mercury ran up five digits; its height was ſix digits.

Sept. 17. The height of the mercury was eight.

Sept. 18.	The height of it was	10
19.		12
20.		14
22.		18
23.		18
24.		19
26.		23
28.		26

Octob. 1. The height of the mercury was 30.
4. The height of it was 31: it was ſomewhat cold.

Octob. 5.	The height of it was	32
7.		33
9.		33 ½
11.		Same.

Octob.

Octob. 15. These two last days, the cold being abated, the mercury ascended more speedily: its height was 37.

Octob. 17.	38
19.	$39\frac{1}{2}$
22.	41
26. The height of it was	43
29.	45
Nov. 2.	46
5.	47
20.	53

In this experiment, the air seems to be produced sometimes regularly enough, and at other times anomalously.

EXPERIMENT III.

July 6, 1676. I put goosberries into an emptied receiver; but before it could be guarded against the external air, it had entered in, and impelled up the mercury to the height of half a digit; and afterwards in half an hour, the air produced from the goosberries had impelled it up to another semi digit.

In seven hours time the mercury ascended four digits higher: it staid in five.

July 7. In fourteen hours space the ascension of the mercury was two digits and $\frac{1}{2}$.

In ten hours space the ascension of it was $2\frac{1}{2}$.

July 8. In fourteen hours the ascension of the mercury was $1\frac{1}{2}$.

In ten hours the ascension of it was two digits.

July 9. In fourteen hours the ascension of the mercury was $2\frac{1}{2}$.

In ten hours its ascension was $1\frac{1}{4}$.

July 10. In fourteen hours the ascension of it was $1\frac{3}{4}$.

In ten hours the ascension of it was three.

July 11. In twenty-four hours the ascension of the mercury was four.

July 12. In twenty four hours the ascension of the mercury was four.

Now the mercury was brought to its wonted pressure.

July 13. This day in the morning, I found the cover to be broken; and because it was fastned by a screw, that it might not be severed from the receiver, I suspected, that it was broken by the force of the internal air: I substituted another cover in its place.

July 14, 15, 16, 17, 18. I perceived no change in the height of the mercury, because the cover was not exactly shut; and therefore I took out the fruits, and put some part of them into another evacuated receiver, and the rest I stopped up closely with common air, that nothing might get out.

In four hours the ascension of the mercury was four digits.

July 19. In fourteen hours the ascension of the mercury was $1\frac{1}{2}$; but suspecting the air to have escaped, I set the screw.

In nine hours the ascension of the mercury was eleven digits.

The cover was broke, and the air made an escape.

This experiment seems to prove, that goosberries contain much air in them, which, as soon as it is freed from the wonted pression of the air, doth more readily break forth, than when it is restrained by some ambient air, until the goosberries begin to be fermented, for then air is produced in a far larger quantity, even in a great compression.

EXPERIMENT IV.

I included paste in an exhausted receiver, and, before it was guarded against the external air, the mercury was come to the height of three digits, by reason of the air making an irruption from without; when it came to pass that the paste, which was much swollen, lost about the third part of its tumidity.

A little

A little while after it swelled again, and within half an hour the mercury mounted higher by two digits.

In one hour's time the ascension of the mercury was 2 $\frac{1}{2}$; and the paste continued to swell or rise more and more.

In another hour's space the ascension of the mercury was three digits and $\frac{1}{2}$.

In one hour's time the ascension of it was 4 $\frac{1}{2}$ digits: it staid in sixteen.

July 9. In 14 hours space, the ascension of it was 21 digits. The height of the mercury was 37. Moreover I suspected, that some air had got out. When I set the screw, the cover brake, and upon the ingress of the external air, the paste, which always did rise, now did abate about two digits of its tumidity, though it was now found in a less compression than before.

In five hours space the ascension of the mercury was 15 digits.

BUT when I again endeavoured to set the screw, the cover brake, so that the air escaped; the paste did presently somewhat pitch, and was depressed.

In four hours space the ascension of the mercury was 10 digits: the paste did again swell or rise, as before; but being willing to substitute a better screw in the place of the other, I permitted an egress to the air; yet this time the paste did not pitch or subside, as before it had done.

July 10. THIS night the paste rose again, yet it seemed to have produced no air.

In four hours space there was no ascension of the mercury.

In seven hours space the ascension of it was four digits.

July 12. I perceived no ascent of the mercury.

13. It seemed to have ascended a little.

17. Seeing no more air was produced, I took out the paste, and found it to be of a subacid smell.

THIS experiment seems to prove, that air may be produced out of paste, in compressed air, as well as *in vacuo*.

BUT the paste was twice depressed, because the compressed air suddenly finding out a way of eruption, was so much dilated, as it is wont to happen in all springs, when they are carried beyond their point of rest: but when that air was immediately repelled by the external air, the paste did pitch and was depressed.

EXPERIMENT V.

July 13, 1676. I included some beans, of that sort, which are given to horses for provender, *in vacuo*, with some water: some of them, which were bruised, seemed to swell much; but those, which were left whole, suffered no sensible alteration.

In two hours space I saw no air produced, though the beams continued to swell.

July 14. In 24 hours the ascension of the mercury was seven digits.

July 15. In 16 hours the ascension of the mercury was three digits and $\frac{1}{2}$.

In eight hours the ascension of it was 1 $\frac{1}{2}$; the height of it was 12.

July 16. In 14 hours the ascension of it was three.

17. In 26 hours the ascension of it was six.

18. In 24 hours the ascension of the mercury was almost nine.

19. I stopped the receiver firmly with a screw, because the air had got out. In nine hours space the ascension was one digit.

20. In 24 hours space, the ascension was 3 $\frac{1}{2}$.

21. In 24 hours space the ascension was 5 $\frac{1}{2}$.

22. In 14 hours the ascension of the mercury was two digits.

23. In 24 hours the ascension of the mercury was 18 digits.

24. In 14 hours the ascension of the mercury was almost five. The height of it was 35 above the wonted pressure.

25. The receiver could not sustain a greater pressure. I found the beans of a fœtid smell, not much unlike

unlike the ſmell of putrified fleſh.

FROM this experiment it ſeems to follow, that beans contain much air in them; and that that air is produced in a moderate preſſure, as well as *in vacuo*, ſometimes more ſpeedily, ſometimes more ſlowly.

ESPECIALLY, that great inequality, which happened *July* 23, is to be taken notice of.

EXPERIMENT VI.

July 23. I included gooſberries *in vacuo*, and fortified them very well againſt the external air.

IN two hours ſpace the mercury aſcended one digit.

July 24. The height of the mercury was ſeven digits $\frac{1}{2}$.

July 25.	The height of it was	12
26.		17
27.		20
28.		24 $\frac{1}{2}$

22. The height of it was almoſt 30.

30. The height of it was almoſt 31.
I tranſmitted ſome air out of this receiver into another evacuated receiver, and ſo the height of the mercury was 26.

31. The height of the mercury was 35.

Auguſt 1. THE height of the mercury was 32. But ſome air had eſcaped out; and going about to ſtop the receiver cloſe, I ſuffered ſome more air to get out.
The height of the mercury was 30.

2. The height of the mercury was 39. I tranſmitted ſome air into another receiver.
The height of the mercury was 31.

3. The height of the mercury was 39.

4. The height of the mercury was 41.

Aug. 5. The height of the mercury was 43. I tranſmitted the air into another receiver.
The height of the mercury was 30 digits.

6. The height of the mercury 43.

7. The height thereof was 47.

8. The height thereof was 48. But the air being tranſmitted into another receiver, the height of it was 36.

9. The height of the mercury was 41. Fourteen hours were paſt.

10. The height of the mercury was 47; the air being tranſmitted into another receiver, the height of it was 35: 24 hours were elapſed.

11. The height of the mercury was 38 $\frac{1}{2}$. Fourteen hours were elapſed.

12. The height of the mercury was 42; twenty-four hours were paſſed. I extracted the air, and the height of the mercury was 26.

13. The height of the mercury was 33: twenty-four hours were elapſed,

Aug. 14.	The height of it was	36	hours 24
15.		39	
16.		41 $\frac{1}{2}$	
17.		44	
18.		47	
19.		50	

I tranſmitted the air into another receiver, and the mercurial gage was ſpoiled. I took out the gooſberries, and found, that they had loſt their colour, and alſo almoſt all their acidity.

FROM this experiment we may infer, that gooſberries do produce their air regularly enough, unleſs ſomething be extracted out of the receiver; for then they acquire ſtrength to produce new air more ſpeedily.

EXPE-

EXPERIMENT VII.

Sept. 12. I put crude grapes into an emptied receiver; but before they could be fortified againſt the external air, ſome thereof had got in, as much as could ſuſtain three digits of mercury.

Sept. 13.	The height of it was	5
14.		10
16.		17
17.		19
19.		23
20.		25

Sept. 22. The height of the mercury was 30. I ſtopped the receiver with a ſcrew.

23. The height of the mercury was about $30\frac{1}{2}$.

24. The height thereof was 32.

Sept. 26.	The height of it was	$34\frac{1}{2}$
27.		$36\frac{1}{4}$
28.		$36\frac{1}{4}$
29.		$37\frac{1}{4}$
30.		$37\frac{3}{4}$
Octob. 2.		$39\frac{1}{2}$
4.		$39\frac{1}{2}$
5.		$40\frac{1}{2}$
7.		$41\frac{1}{2}$
9.		$42\frac{1}{2}$

Octob. 15. The height of the mercury was 46. It aſcended chiefly theſe two laſt days, when the froſt was diſſolved.

Nov. 2. The height of the mercury was 54 digits.

Nov. 5. The height 58.

Jan. 10, 1677. Now the mercury was come to the height of 70 digits; and yet I perceived no ſenſible mutation in the mercurial gage, even when the cold was moſt fierce, though the grapes and their juice were concreted into ice.

Sept. 21. HITHERTO the grapes ſeemed not altered: but the mercury had aſcended a little, becauſe the air had found a paſſage out. This day I opened the receiver, and when the air brake forth, many of the grains ſeemed to be contracted into wrinkles. The grapes had kept their taſte, but much more pungent; but their juice continued to be tinged with a curious red colour.

THIS experiment ſeems to inform us, that grapes produce not all the air, but in a long tract of time.

EXPERIMENT VIII.

Auguſt 10, 1677. I put pears, cut in two, into a vacuous receiver. Towards evening the mercury was come up to the height of 10 digits.

Aug. 11.	The height of it was	20
13.		38
14.		48
15.		55
16.		60
17.		68

The air being tranſmitted into another receiver, the height of the mercury remained at $53\frac{1}{2}$.

Aug. 18.	The height of it was	61
19.		64
20.		70
21.		72

The air being tranſmitted into another receiver, the mercury remained in the height of 61.

Aug. 22.	The height of it was	68
23.		74
24.		79
25.		81

The air being tranſmitted into another receiver, the mercury remained in the height of 61.

Auguſt 26. The height of the mercury was 56, becauſe ſome air had got out; yet I tranſmitted the air into another receiver, and the mercury remained in the height 52.

Aug. 27.	The height of it was	60
28.		68
29.		75
30.		83
31.		88
Sept. 1.		93

Sept. 2. The height of it was 100.

Sept. 3. The height of it was 89, becauſe ſome air had eſcaped out, which made me cautious to prevent the like for the future.

Sept. 4. The height of the mercury was 100.

5. The same height continued.

7. The same height still continued, though no air at all had any egress.

9. The height of the mercury was 107.

10. The height of the mercury was the same.

The air being transmitted into another receiver, the mercury staid in the height 99.

Sept. 11. The mercury moved not.

13. The height of the mercury was 105.

Octob. 8. I this day found, that the air had got out.

THIS experiment seems to inform us, that pears do produce their air, as it were, by paroxysms, or fits.

ARTICLE XI.

Various experiments.

EXPERIMENT I.

March 16. I melted down lead with a fire in a brass vessel, whose diameter was an inch and half; but before the lead was concreted by cold, I put it into a receiver, out of which I exhausted the air with great speed; whence it came to pass, that the figure of the concreted lead was concave, and the parts of it were so much the more depressed, by how much they were the nearer to the center: whereas, on the other side, lead congealed in common air doth exhibit a convex figure, except in the middle, where a little cavity doth appear.

I made the same experiment with tin, and had the same success; though both metals being liquid, and very hot, had remained long enough *in vacuo*, yet no bubbles seemed to emerge from either of them; whereas all other hot liquors do send forth numerous bubbles *in vacuo*.

EXPERIMENT II.

Sept. 2. I put water saturated with dissolved salt, *in vacuo*, to try, whether it would be there converted into chrystals, and the salt be carried above the plain, or superficies of the water, as it is wont to happen in the free air.

Sept. 15. THE water with the dissolved salt abiding in the same state, I opened the receiver: seeing no vapours could escape out of the evacuated receiver, it is consentaneous to reason to judge, that the salt could not there be converted into chrystals.

EXPERIMENT III.

August 8, 1676. I put air produced from gooseberries, into an evacuated recipient, furnished with a mercurial gage.

March 1, 167$\frac{6}{7}$. WHEN I perceived, that no change was made in the height of the mercury, I opened the receiver.

EXPERIMENT IV.

August 8. I took a phial, which was able to hold seven ounces five drachms and three grains of water, and exhausted the air out of it; and when, in a balance, it was suspended in an *æquilibrium* with another weight, I pierced the bladder, which covered the orifice, with a needle; and then the phial, being filled with air, appeared heavier by four grains and $\frac{1}{2}$, which latter weight to the former, is in the same proportion as 1 to 814: whence it follows, that water is about 800 times more ponderous than air of an equal bulk. Yea, it is probable, that the proportion is with the least, because this day the air was hot and clear, and besides, some air was always left in the receivers after the exhaustion.

EXPERIMENT V.

Jan. 19, 1677. I put *aqua fortis* with fixed nitre into a receiver; and having exhausted the air as much as I could, I poured in one of them on the other, and found much air produced. I marked the height of the mercury in the gage.

March 5. FINDING, that the produced air was not destroyed, and that the mercury persisted in the same height, I opened the receiver, and found nitre produced *in vacuo* from the mixture.

EXPERIMENT VI.

May 12, 1677. I filled a phial, of a long and very narrow neck, with oil up to the middle of the neck; and thus filled, I put it into a receiver firmly ſtopped by the help of a ſcrew, into which afterwards I intruded air till it could ſuſtain 120 digits of mercury above its wonted height; and the oil, in the neck of the phial, appeared depreſſed toward the phial about one quarter of an inch; the cauſe whereof I judge attributable to the compreſſion of the air: and yet having eaſed the ſcrew, and thereby ſuffered the air to break out and be dilated, the oil did not aſcend at all; ſo that I judge it was condenſed only by cold.

Auguſt 5. I made the ſame experiment after the ſame manner, only uſing water inſtead of oil; and yet I could perceive no change of the height of the water in the neck of the glaſs, though the heat being moderate might have produced a ſenſible effect.

Jan. 14, 1678. BECAUSE I found by ſome experiments, that compreſſed air did enter into the pores of the water, and did pierce even to the bottom, a ſuſpicion might ariſe, that the water was not condenſed by the compreſſed air, for this reaſon, becauſe the air entering into the pores did make the preſſion within equal to the preſſion from without. And to be ſure of this, I filled the glaſs aboveſaid with ſpirit of wine, leaving only the length of three digits in the top of the neck thereof, which was filled with air only. Then my hands being applied to the glaſs, the ſpirit of wine, being heated, in a ſhort time filled the whole neck even to the top. Then the glaſs being inverted into a veſſel full of mercury, I removed my hands; which being done, the ſpirit of wine being ſoon cooled, afforded ſpace to the mercury to fill three digits in height. I put the veſſel and the glaſs in that poſture into a receiver, into which I afterwards compreſſed the air, till the mercury exceeded its wonted height 90 digits; and yet there was no ſenſible condenſation of the ſpirit of wine, nor any aſcenſion of the mercury: however, it is certain, that no air had crept in, becauſe the mercury hindered it; and the receiver being opened, when the air, that compreſſed from without, was dilated, no bubbles appeared in the ſpirit of wine.

IN this experiment, it ſeems worthy our enquiry, how it comes to paſs, that ſpirit of wine was ſo ſenſibly condenſed by a moderate cold, and not at all by a great compreſſion of the air.

EXPERIMENT VII.

May 12, 1676. I poured ſpirit of wine into a glaſs veſſel, and ſuperadded ſome drops of oil of turpentine thereto, which ſwimming upon the ſpirit of wine, began to be whirled about by motion, hither and thither, as it is wont to come to paſs. I put the glaſs veſſel on the pneumatic engine, and covered it with a receiver; and yet the bubbles did not at all ceaſe to be moved up and down. Then I pumped out the air, till the ſpirit of wine did only not bubble; and it came to paſs, that the bubbles emerging from the ſpirit of wine did adhere to the drops of oil, and carried them with themſelves to the ſides of the veſſel, and there retained them; yet two drops, free from ſuch bubbles, proceeded to have further motion: afterwards I wholly exhauſted the receiver, and ſome drops were emitted to the top thereof, by the force of the bullient ſpirit of wine; but the remaining drops proceeded on to be moved a little, and in a little time after they reſted. The air being immitted, the drops began again to renew their motion, but it was a ſlow one, and it quickly ceaſed.

I iterated the ſame experiment with ſpirit of wine and oil of turpentine, cleanſed from air; and no ebullition was then made, yea, no bubble appeared at all, but the drops of the oil of turpentine were moved *in vacuo*, as in the open air.

HENCE

HENCE it seems to follow, that the cause of the motion of the drops is not to be ascribed to the dissolution; for all the dissolutions *in vacuo* have hitherto seemed to me to produce bubbles.

EXPERIMENT VIII.

May 19, 1676. I left yesterday two radishes *in vacuo*; one of them I hanged up, the root being upside down, the other in a contrary posture; both of them cut transversly did hang over a subjacent vessel, which contained red wine. All these being left a whole night *in vacuo* seemed well purged from their air. Opening the receiver, I added two other radishes to the former included ones, cut after the same manner, and from which I had further detracted their thick skin. Then exhausting the receiver, I immerged the cut part of all the radishes at once into the subjacent wine: and then many bubbles seemed to arise out from them, as it came to pass in those little glass-tubes of *Exper*. IX. yea, more bubbles were emitted from those radishes, which were purged from air the whole night, than from those, which had not remained above half an hour *in vacuo*; and from whom I had taken away their skin.

THIS experiment seems to afford us a confirmation, that bubbles are formed of particles of air swimming in water; and because in the skin there are some canals fit to retain parts of air, it came to pass, that the radishes, from which I had detracted their skin, afforded no opportunity for the forming of so many bubbles.

THE liquor ascended no less into those radishes, which hanged with their roots upwards, than into those of a contrary posture.

EXPERIMENT IX.

May 4, 1677. I immerged one end of a small glass-tube, open at both ends, into water stagnant *in vacuo*, and presently the water ascended up into it, as it is wont to do in common air, and even to the same height; but a little while after, many bubbles being formed there, lifted the water higher, and kept it suspended in three different places, disterminated by many bubbles; and many other bubbles seemed to pass out from that end, which was immersed in water.

THEN I sealed the other end of the tube hermetically; and so the experiment being made in common air, the water could not ascend up into the tube by the open end. But *in vacuo* the matter succeeded far otherwise; for the water ascended up into the tube no otherwise, than if it had been open at both ends; and many bubbles formed in a short time, did distinguish the water, contained in the tube, by great intervals, as before; whilst the mean time, many other bubbles seemed incessantly to pass out from the end of the tube immersed in water; yet in progress of time they appeared less frequent.

BUT this circumstance I much admired, that the water being suspended higher in the tube, seemed to be filled with no bubbles, whereas the end only did emit so many.

THEN I took out that end from the water, and no bubbles did any more appear, though that end was wholly filled with a cylinder of water.

May 5. I repeated the same experiment; but before I had immerged the end of the tube in water, a drop of water, which ran over from the superior aperture of the receiver, flowed down to the open end of the tube, and pierced up into it the height of two lines; neither was any bubble formed there in a full half hour's time: that being passed, I immitted the end of the tube into the water of the vessel, and not long after bubbles began to be formed, as before, of which some followed others within half a minute; yet afterwards they came forth less frequent. Furthermore, iterating this experiment many times, I perceived, that when the water was extracted from the tube, no bubbles appeared; but if it were immerged in water, bubbles would cleave to the end of it, either sooner or later.

May 6. I tried the fame experiment, with the infufion of nephritic-wood, and the fuccefs was wholly alike, but that the bubbles could emerge and pierce the liquor, before they had acquired any bignefs; for being yet very fmall, they pervaded the liquor contained in the tube, and were carried to the upper part thereof; whence we may conjecture, that that liquor is very thin, and hath no vifcofity to refift the pervading body.

May 10. I iterated the fame experiment with fpirit of wine, mixed with a certain oil made *per deliquium:* yet I found no new event, but that the afcenfion of the liquor into the tube was not fo high.

FROM thefe experiments it feems to follow, that the bubbles are formed, in the extremity of the tube, of aerial particles, fwimming in the water, which finding fome impediment at that end cannot pafs by, and fo, new ones coming upon them, they fwell into a bubble.

EXPERIMENT X.

July 18, 1676. Two days ago I took fome beans, fuch as are given to horfes for provender, and included them in an iron tube clofely ftopped; yet I firft affufed water on the compreffed beans, till the tube feemed wholly full, to try, whether the force of the fwelling beans would be enough to break the tube. This day the tube feemed not to be altered at all; but the ftopple being plucked back, fome quantity of air brake out, and much water fell upon the ground, which was not fucked up by the beans; then a certain noife, as it were, of bubbling water was heard for a whole hour and more.

July 25. I left the iron tube in the fame pofture; but this day one of the ends of it being unftopped, and fome beans taken out, the murmur of the bullient water was heard, as before.

FROM this experiment it feems to follow, that beans do contain air in them, which in a great compreffion cannot efcape out; but if it be freed from the force compreffing it, then it makes an eruption.

EXPERIMENT XI.

March 4, 1677. I put a glafs half full of fpirit of fal-armoniac and *limatura cupri*, into a receiver exhaufted as much as I could, and there ftopped it in. And it came to pafs, that in 15 minutes fpace the liquor had contracted a certain blue colour, but very much diluted; but the air being immitted, in three minutes the blue colour appeared vivid and thick. I put the liquor fo tinged again *in vacuo*, to try, whether in tract of time that colour would vanifh.

April 4. THE blue colour was almoft quite vanifhed, but upon the admiffion of the ai-, it quickly returned.

EXPERIMENT XII.

May 8. I put a certain oil made *per deliquium*, with fpirit of wine into an exhaufted receiver, and the fpirit always fwam on the top; now left the fpirit might be fpilt by bubbling above the edges of the veffel, I extracted the air by degrees, and in the beginning great bubbles came from the fpirit, and but very fmall ones from the oil; but after one hour's time, the oil did emit great bubbles, which, being fmall at bottom, in their afcent did fill the whole latitude of their veffel; and after another hour, fome bubbles brake out with fo great force, that they hit againft the top of the receiver.

May 9. I iterated the former experiment in a glafs fomewhat long and narrow, that I might the better perceive the motion of the bubbles; and then I faw the bubbles paffing out of the oil into the fpirit of wine, without any great increafe of their quantity; but being diftant only one quarter of an inch from the fuperficies, they were fuddenly expanded.

EXPERIMENT XIII.

May 3, 1676. I mixed a certain quantity of *aqua-fortis* with a quantity of fpirit of wine fomewhat greater; and then I diftributed that mixture equally into three glafs veffels, and put three equal pieces of iron

iron into them, to each veſſel one. This being done, I included one of the three veſſels *in vacuo*, and there many great ebullitions were made. Then after a quarter of an hour, I took out the veſſel, and found the liquor black and turbid; whereas the other two veſſels had their liquor not altered in colour, but only ſome black powder did appear in the bottom of the liquor.

Of theſe two veſſels I put one *in vacuo*, and then there aroſe ebullitions, great indeed, but much leſſer than the former: when one quarter of an hour was elapſed, I took the veſſel *è vacuo*, and found the liquor black indeed, yet ſomewhat leſs ſo than the former; but the liquor, which was left always in the air, did in a manner remain unchanged.

May 4. This day in the morning, the liquors in the two veſſels, put *in vacuo*, appeared cleanſed and green, and had no other operation.

But the liquor, which was not put *in vacuo*, did bubble more ſtrongly than yeſterday, and exhibited a red colour. I put the three veſſels together *in vacuo*, and perceived no eminent ebullition; only ſome bubbles appeared larger in the red liquor, than in the other two.

From this experiment, it ſeems to follow, that ſpirit of wine *in vacuo* doth accelerate ebullition.

Experiment XIV.

Jan. 21, 1678. I kept a glaſs half full of ſal armoniac and filings of copper, the hole thereof being ſo exactly ſtopped, that the blue colour, which was induced into that liquor, from the contact of the external air, (ſee *Philoſophical Tranſactions*, *Num.* 120.) did wholly now diſappear. The ſtopple was made of leather, prepared after a ſpecial way and manner.

I put that glaſs *in vacuo* with paſte not yet fermented.

I did it to this end, that the receiver, being full of air from the paſte, I might perforate the leather, that ſtopped the glaſs, with an iron wire prepared for that purpoſe; and that I might try, whether the contact of the air generated from the paſte, would alſo communicate ſome colour unto the liquor.

Jan. 22. There was no need to perforate the leather, for this day I found the liquor already tinged; ſo that it is probable, that air produced from paſte is endued with ſuch minute particles, that it can penetrate leather, which is impervious to common air.

Yet I will keep the glaſs, not touching its ligature, to try, whether that colour may vaniſh again.

Jan. 25. Now the liquor became almoſt colourleſs; whence it appears, that common air is too thick to penetrate all paſſages, which are pervious to air produced from paſte.

Feb. 2. I put the ſame phial *in vacuo*, but did not fortify the commiſſure of the receiver with the cover, with turpentine; ſo that the air, making a gradual ingreſs, in 24 hours filled the receiver, even as it was leiſurably filled with the air produced from paſte; yet the liquor remained ſtill colourleſs.

Febr. 15. I put the ſame glaſs again *in vacuo* with ſome quantity of paſte; but this time the air produced from thence did not pervade the leather, as it had done before, and the liquor was not tinged at all.

Experiment XV.

April 2, 1678. I put a ſhrew-mouſe into the engine deſcribed, *p.* 516. and when I perceived he was reduced to extremity, I began to ſtir the pump, that the air might penetrate, and be, as it were, filtrated through the water. The mouſe, a while after, ſeemed to be better, yet he could not be wholly reſtored to health. Now, becauſe he had been long kept faſting, I am uncertain, whether he died for want of aliment, or of new air.

April 12. I iterated the ſame experiment with a ſmall and weakly mouſe, that had been kept a long time faſting. And finding, that this experiment had the ſame ſucceſs with the former, I took out the mouſe

mouſe before he was dead; and though he then enjoyed the free air, yet he recovered not; ſo that we have need of more experiments, that we may attain to a certain knowledge of the effect of that filtration.

EXPERIMENT XVI.

May 2, 1678. SIX weeks ago, I included frog-ſpawn in three recipients; the firſt of which was vacuous; the ſecond contained common air; and into the third I intruded ſo much air, that the mercury ſtaid in 60 digits above its wonted height.

IN 15 days the mercury in the evacuated receiver came to the height of one digit.

The ſpawn in the common air ſeemed corrupted and of a blackiſh colour; but that in the compreſſed air remained unaltered in colour; but no frogs were generated.

AFTER an whole month was elapſed, the ſperm *in vacuo* had not changed its colour, excepting the black round ſpots, but ſeemed reduced into water: the colour of that in the common air was very black; but in the compreſſed air the ſpawn began to be reddiſh.

As yet no change was perceived, neither in that ſpawn *in vacuo*, nor that in the common air; but in the compreſſed air the ſpawn waxed more and more red.

May 22. THE ſperm *in vacuo* was not changed; in the compreſſed air it remained red; but in the common air it became again colourleſs.

June 23. THE ſperm *in vacuo* and in common air was tinged with no colour, but in the compreſſed air it inclined to greenneſs.

Octob. 15. I took out all the ſpawns; that which was kept *in vacuo* was almoſt exhaled out of its veſſel, and was ſtagnant in the receiver, like clear water: in the common air, the ſperm remained colourleſs; but that in the compreſſed air kept ſtill its red colour.

EXPERIMENT XVII.

May 9, 1678. SIX days ago, I included two pieces of the ſame orange in two receivers, not quite of equal bigneſs; but in the greater receiver there was left ſome quantity of water, ſo that no leſs ſpace was left for the air in that, than in the leſſer. The iſſue was, that the orange included with water, though it were not touched by it, yet was four times more mouldy than that, which was kept without water.

AND therefore in iterating this experiment, I put two pieces of the ſame orange into two receivers, but I filled the third part of one of them with water, yet ſo, that it did not reach the orange.

June 15. NEITHER of the oranges had contracted any mouldineſs.

May 16. I repeated the ſame experiment with the ſame ſucceſs; yet neither orange had acquired any mouldineſs in the ſpace of more than a month, though in former experiments all ſuch oranges would be mouldy.

THE cauſe of the difference ſeems to be attributable to ſome diſpoſition of the air.

EXPERIMENT XVIII.

June 1, 1678. I put a ſmall glaſs-tube, half full of Venice turpentine, into the wind-gun, deſcribed *p.* 516. and I had ſcarce reduced the air to the tenth part of its wonted ſpace, but the leather, ſpread over the elliptic valve, was driven out; ſo that the air having made an eſcape, I drew the glaſs-tube out of the engine, and found many bubbles formed in the ſuperficies of the turpentine; and therefore I ſuſpected that the air had pervaded the turpentine, and that it would have penetrated more deeply into it, if they had remained longer thus encloſed together: and therefore I re-immitted the ſame tube into the ſame gun, and there left it in air reduced to about the 15th part of its ſpace.

June 3. I opened the engine, and, taking out the tube, found the turpentine almoſt free from bubbles; yet by degrees many were formed therein, in the parts remote enough from the ſuperficies.

June 4. I threw away the former turpentine, and put new in the ſame tube, and included it *in vacuo*, that the turpentine might be the better purged from all air; then I poured water upon it, and ſhut up all in the wind-gun.

June 8. I opened the engine, and at firſt ſight both the water and the turpentine in the tube ſeemed to be very free from bubbles; but a little while after I perceived, that bubbles were formed in the turpentine, and that they aſcended by degrees; yea, ſome of them ſeemed to be made almoſt in the very bottom, about half an inch below the ſuperficies of the turpentine. So that we may conjecture, that all the water, and ſo great an height of the turpentine, were penetrated by the air, which formed thoſe bubbles.

EXPERIMENT XIX.

Aug. 11, 1678. I included ſpirit of ſal armoniack, with a mercurial gage, *in vacuo*; and after that the ſpirit ceaſed to emit any bubbles, I mixed filings of copper therewith, which cauſed many bubbles to break forth again; but they were ſo far from producing any air, that they contrariwiſe conſumed that, which was there before; as it hath been already obſerved in the *Philoſophical Tranſactions*, N. 120: but the liquor was made greeniſh and turbid.

Dec. 5. THE ſpirit was almoſt all exhaled out of the veſſel, in which it was contained, and being condenſed in the receiver, remained ſtill turbid, by reaſon of much filth, which was included there; but that, which was not exhaled out of the veſſel, appeared clear like water: alſo the mercury was wholly expelled out of the gage. Whence I conjecture, that the air in the receiver was more and more conſumed.

EXPERIMENT XX.

Sept. 2, 1678. I put two cylinders, one of tin, the other of lead, *in vacuo*, but their loweſt parts were immerſed in mercury; and at the ſame time I immerſed two other cylinders, like the former, after the ſame manner in mercury: but theſe latter were left in the free air.

Sept. 6. I opened the vacuous receiver, and the mercury, in the tin cylinder, was come to the height of four digits and a half above the ſuperficies of the ſtagnant mercury; and cutting the cylinder tranſverſly in the middle of that height, the amalgama ſeemed to have penetrated into the cylinder about half a line. And cutting the cylinder tranſverſly again, in that part, which was diſtant only one digit from the ſuperficies of the ſtagnant mercury, I found the thickneſs of the amalgama to equal one line.

IN the lead cylinder the mercury came to the height of two digits and $\frac{1}{2}$, but only as far as the ſuperficies; and that very part, which was immerſed in the mercury, was not penetrated by it to any ſenſible thickneſs.

Sept. 7. I took out the tin, left in the air, out of the mercury, in which it was immerged, and found the mercury to have aſcended to the height of five digits.

Sept. 10. THE ſame cylinder being left in the mercury, ſeemed to be beſmeared therewith up to the very top, ſix digits and more above the ſuperficies of the ſtagnant mercury. When the cylinder was tranſverſly cut in ſeveral places, it appeared, that the mercury had pierced ſo much the higher into the tin, by how much it came nearer to the ſtagnant mercury; ſo that in the part near to the foreſaid mercury, almoſt the whole diameter of the cylinder, three lines broad, was penetrated thereby.

IN the lead cylinder the mercury exceeded not the height of three digits and $\frac{1}{2}$, neither had it penetrated to any ſenſible thickneſs. Whence it appears, that the weight of the air contributes little or nothing to the aſcenſion of mercury into metals.

EXPERIMENT XXI.

Dec. 12, 1678. I took a ſmall whiting, and having cut off his head, I divided him tranſverſly

transversly into five pieces. The first whereof I included *in vacuo*. The second in common air. The third in air so compressed, that it could sustain mercury 50 digits above its wonted height. These three receivers were closely stopped with screws. The fourth piece was put into a receiver, full of air produced from paste, which was presently stopped. The fifth was left in the free air.

Dec. 15. THIS day in the morning, that part of the whiting, which was left in the free air, began to shine; and towards evening it sent forth somewhat a more vivid light.

Dec. 16. IN the morning, the whiting left in the free air, gave over shining; but towards evening it shone again.

Dec. 17. THIS morning the same part of the whiting shined a little, yet less than it did yesterday in the evening.

Dec. 18. IN the morning there appeared no light, though I fixed my eyes a long time upon the receiver in a dark place; but the night coming on, the light appeared again.

Dec. 20. HITHERTO the same part of the whiting, left in the air, proceeded to shine; but all the other parts did not yet begin to shine.

Dec. 22. YESTERDAY the light of the whiting, left in the air, had not quite ceased; but this day it appeared no more.

Dec. 24. THE part of the whiting in the free air gave over its shining quite; but that, which was included with common air, did yesterday send forth a faint light; but this day it proceeded not to shine.

Dec. 26. No shining appeared any more in the common air: but the three other pieces did not so much as begin to shine.

Jan. 26, 1676. I perceived no more shining in any one of the receivers.

ARTICLE XII.

Artificial air destroyed.

EXPERIMENT I.

Aug. 3, 1677. I transmitted air, produced from cherries, into a receiver full of common air, but so stopped with a screw, that the mercury ascended to 25 digits above its wonted pressure.

Aug. 4. THE mercury was depressed about two digits. The height of it this day was only 23 digits.

Aug. 6. The height thereof was reduced to 20 digits.

7. The height thereof was the same.

8. The mercury was somewhat depressed.

Aug. 10. THE height of it was $19\frac{1}{2}$ above its wonted height. When I perceived little or no alteration more, I opened the receiver.

FROM this experiment we have a confirmation, that air, produced from fruits at the beginning, is in part destroyed; but the rest can keep the form of air very long.

EXPERIMENT II.

May 26, 1676. I took six grains of sal armoniack, and put them into a receiver with a sufficient quantity of oil of vitriol: then the air being exhausted, I forced down the salt into the oil, whereupon a great ebullition presently followed, and the mercury ascended into the gage almost to its wonted height; but presently after it sunk again, and returned to its former state.

May 27. I repeated the same experiment; but this time the salt remained 10 hours *in vacuo* before it was put into the oil, but the ebullition followed as in the former experiment; yet the air was produced much more slowly, neither could it wholly be destroyed but in seven or eight hours time; yet at last the mercury descended to the very bottom.

May 29. I tried the same experiment again, leaving the materials 24 hours *in vacuo*: this time the ebullition seemed much less, and the air was produced both in a lesser quantity and more slowly than before. I observed also, that whilst the materials staid *in vacuo* before their mixture, that the mercury came nearer to the open end of the gage, as if some air had been either extracted or destroyed.

June 8. I put oil of vitriol alone into a receiver, in which I left only a fifth part of common air, to try, whether this oil, without ſal armoniack, would diminiſh the elaſtical force of the air: but it fell out contrary, that the force of the air was increaſed, and the mercury, in one hour's ſpace, ſeemed to have aſcended a little into the gage; but afterwards, for 24 hours ſpace, no change was made.

This experiment doth confirm, that ſome artificial airs may be deſtroyed; but why this deſtruction happens ſometimes ſooner, ſometimes ſlower, it may perhaps ſeem worthy of a further enquiry.

ARTICLE XIII.

Experiments concerning the different celerity of air produced in vacuo, *or in common air.*

Experiment I.

Common air.

July 10, 1676. I put paſte, kneaded two days before and ſouriſh, into a receiver, and ſtopped it firmly with a ſcrew.

In one hour's ſpace the height of the mercury was one digit.

In ſeven hours ſpace the height of it was ſix digits.

July 11. The height of it was 11 digits.
12. The height of the mercury was 24.
13. The height thereof was 30.
14. The height of the mercury was ſenſibly greater.
15. The mercury aſcended a little. Meaſuring its height exactly this day, I found it 38 digits.
19. No more air was produced from the paſte.

Vacuum.

July 10, 1676. I put another quantity of the ſame paſte, much leſs than the former, into an exhauſted receiver.

Though the quantity of the paſte was leſs, yet in one hour's time the height of the mercury was two digits.

In ſeven hours time the mercury came almoſt to the top of the gage, but it was a ſhort one.

July 19. The paſte was not able to remove the receiver from his cover, though at the beginning it had produced a greater quantity of air than the paſte in common air. I endeavoured to fire it with a burning-glaſs, and the fumes, elevated therefrom, afterward falling upon the paſte, did tinge the ſuperficies thereof with a pleaſant yellow colour; and that air was thus produced, I conjectured hereby, becauſe the cover was afterwards eaſily ſevered from its receiver.

From this experiment, made in two receivers at once, we learn, that air is ſometimes generated much more eaſily *in vacuo* than in common air.

Experiment II.

Common air.

Aug. 20, 1676. I put paſte, kept for 24 hours, into a receiver full of common air; to which I further added new air, ſo that the mercury exceeded its wonted height four digits and $\frac{1}{2}$.

In ſix hours ſpace the mercury gained almoſt four digits. Its height was eight digits.

Aug. 21. The aſcenſion of the mercury was four digits and $\frac{2}{3}$.
22. The aſcent of it was about one digit.
23. The aſcent of it was $\frac{1}{2}$ a digit.
26. For three whole days the aſcent of the mercury was only $\frac{1}{2}$ a digit.
27. There was no aſcent of it at all.
29. The paſte, taken out of the receiver, affected our noſtrils with an acid ſmell.

Vacuum.

Aug. 20. I put another quantity of the ſame paſte into an empty receiver, and kept the ſame proportion between the quantity of the paſte and the capacity of the veſſel, as in the former experiment.

THE mercury ſeemed to have aſcended in a ſhort time. Its height was two digits.

Aug. 21. The aſcent of the mercury was five digits.

22. The aſcent of it was three digits.

23. The aſcent of the mercury was one digit.

26. For three whole days the aſcent of it was two digits.

27. There was no aſcent of the mercury.

29. I took out the paſte, exhauſted of its air, from the receiver.

THIS experiment confirms to us, that air is ſometimes more eaſily produced *in vacuo* than in common air.

EXPERIMENT III.

Vacuum.

Sept. 4. 1677. I put the kernels of filberds into an exhauſted receiver.

Sept. 5. The height of the mercury was five digits.

6.	The height of it was	10
7.		10
8.		12
9.		15
11.		18
12.		23
13.		27
14.		29

15. The height of it was almoſt the ſame.

17. The height of it was 30.

18. This day the air began to eſcape out of the receiver; for ſome bubbles appeared in the turpentine, which ſtrengthened the commiſſure of the receiver and cover.

Common air.

Sept. 4. I put kernels of filberds into a receiver with common air.

IN the afternoon the quantity of air ſeemed to be leſſened.

Sept. 5. The height of the mercury was leſs than half a digit.

6. The height of it was the ſame.

Sept. 7. The height of it was one digit.

8. The ſame height ſtill continued.

18. The ſame height continued.

THIS experiment gives us a confirmation, that ſometimes air is produced much more eaſily *in vacuo* than in common air.

EXPERIMENT IV.

Sept. 15, 1677. I included eight ounces of raiſins of the ſun, bruiſed and diluted with a little water, in an exhauſted receiver able to hold 22 ounces of water.

Sept. 16. The height of the mercury was ſix digits.

17.	The height of it was	10
18.		15
19.		29
20.		29

Sept. 21. THIS day I found the receiver forced from his cover.

Sept. 24. I took out ſome of the raiſins; but thoſe, that remained, I encloſed in the ſame evacuated receiver.

Sept. 25. THE raiſins forced the receiver, now full of air, from his cover.

Sept. 15, 1677. I put eight ounces of raiſins of the ſun, bruiſed and diluted with a little water, into a receiver able to hold 22 ounces of water; but I did not exhauſt the air at all.

Sept. 16. The height of the mercury was $\frac{3}{4}$ of a digit above what was accuſtomed.

Sept. 17. The height of the mercury was $1\frac{1}{2}$.

18. The height of it was 3.

19.	The height of it was	5
20.		7
21.		9
22.		11
23.		12
24.		15

BEING about to put peaches into the receiver, I permitted the air to break forth; and then many bubbles did emerge from the raiſins.

THIS

This experiment doth further teach, that air is ſometimes much more eaſily produced *in vacuo* than in common air.

Experiment V.

Vacuum.

Feb. 17, 1677. I put three onions into an emptied receiver.

Feb. 19. The aſcenſion of the mercury was one digit.

21. The aſcent thereof was again one digit. The onions were not altered.

15. The whole aſcent of the mercury was nine digits. The onions not altered.

May 4. The onions had yet undergone no alteration.

18. Neither were they yet altered.

June 19. I this day found the receiver forced from his cover, and the onions rotten.

Rarefied air.

Feb. 17. I incloſed three onions in air ſo rarefied, that it could ſuſtain 10 digits of mercury.

Feb. 19. There was no aſcent of the mercury.

21. There was yet no aſcent thereof.

The onions did not germinate, but contracted a mouldineſs.

25. The aſcenſion of the mercury was about ſeven digits. The onions received no further alteration.

May 4. The onions were not altered.

18. The onions were not yet altered, but the receiver, by the force of the produced air, was removed from his cover.

Common air.

Feb. 17. I put three onions in a receiver not exactly ſhut.

Feb. 21. The onions contracted no mouldineſs, but did germinate.

25. The onions put forth root more and more.

May 4. The onions began to be mouldy.

This experiment gives us a likely proof, that ſome bodies do produce their air not much more eaſily *in vacuo* than in rarefied air.

And beſides it hereby appeareth, that vegetation is hindered, not only by the evacuation, but alſo by the rarefaction of the air.

It ſeems alſo worthy our obſervation, that the onions, as long as they emitted roots, did contract no mouldineſs.

Article XIV.

The difference betwixt whole or entire, and bruiſed fruits.

Experiment I.

Bruiſed fruits.

Aug. 23, 1677. I put pears bruiſed into a vacuous receiver with a mercurial gage.

Aug. 25. The height of the mercury was five digits.

Aug. 26.	The height of it was	10
27.		14
28.		18
29.		21
30.		25
31.		28

Sept. 1. The height of it was 30.

2. The receiver was found forced from his cover.

Whole or entire fruits.

Aug. 23. I put whole pears into a vacuous receiver, and I took care, that the quantity of the pears, and the capacity of the receiver, might be the ſame with thoſe, which I mentioned before.

Aug. 25. The height of the mercury was 11.

Aug. 26.	The height of it was	17
27		25
28.		28
29.		30

Aug. 30. The mercury aſcended no higher, becauſe the receiver was forced from his cover.

This experiment ſeems to prove, that bruiſed fruits do not produce air as ſoon as entire ones.

EXPERIMENT II.

Entire fruits.

Aug. 24. I enclosely whole apples *in vacuo* with a mercurial gage.

Aug. 25. The height of the mercury was five digits.

26.	The height of it was	9
27.		12
28.		15
29.		19
30.		25
31.		28

Sept. 1. The height of it was 29.

2. The height of it was 30.

3. The receiver was forced from his cover.

Bruised fruits.

Aug. 24. I put an equal quantity of bruised apples into a vacuated receiver of the same capacity with the former.

Aug. 25. The height of the mercury was one digit.

26. The height of it was three digits.

27. The height of it was four.

Sept. 3. The mercury continued in the same height.

25. The mercury ascended not at all.

THIS experiment seems to inform us, that bruised fruit do produce air slower than whole or entire ones.

EXPERIMENT III.

Bruised fruits.

Aug. 25, 1677. I put unripe grapes bruised into a vacuated recipient.

Aug. 26. The height of the mercury was one digit.

27. The height of it was two digits.

28. The height of it was two digits and an half.

29. The height of the mercury was the same.

Sept. 15. The mercury did not ascend at all, but its height remained at 2 $\frac{1}{2}$.

Whole fruits.

Aug. 25, 1677. I put unripe grapes, not bruised, into a vacuated receiver.

Aug. 26. The height of the mercury was three digits.

27. The height of the mercury was five digits.

28.	The height of it was	7
29.		10
30.		12
31.		13

Sept. 1. The height of the mercury was 15.

2. The height of it was 16.

3. The height of it was 18.

4. The height of it was the same.

Sept. 5. The height of the mercury continued the same; but all the grapes had almost contracted a yellow colour.

Sept. 7. The mercury rested in the same height; but all the grapes were yellow.

Sept. 15. The height of the mercury was 20.

THIS experiment gives us a further confirmation, that whole fruits do produce air more readily than bruised ones.

EXPERIMENT IV.

Fruits whole and entire.

Sept. 10, 1677. I put two ounces of ripe grapes, but not bruised, into a receiver able to hold 10 ounces of water.

Sept. 11. The height of the mercury was six digits.

12.	The height of it was	9
13.		12
14.		15
15.		20
16.		25
17.		28

18. The height of the mercury was 30. The grapes were not altered at all.

19. The height of the mercury was the same.

20. The receiver was not yet forced from his cover. The grapes were not altered, but appeared only a little riper.

21. The receiver was forced from his cover, though as yet nothing had made any eruption out.

22. This day in the morning I found the grapes begin to rot, and therefore I included them again *in vacuo*.

Sept. 23. The height of the mercury was five digits.

24.	The height of it was	9
25.		14
26.		17
27.		20
29.		27
30.		28

Octob. 10. The receiver was not forced from his cover till this day: the grapes by their colour ſeemed rotten, yet they had kept their firmneſs.

Bruiſed fruits.

Sept. 10, 1677. I included two ounces of ripe and bruiſed grapes in a receiver capable of holding 10 ounces of water.

11.	The height of it was	4
12.		7
13.		10
14.		12
15.		15
16.		18
17.		20
18.		25

Sept. 19. The grapes had ſevered the receiver from his cover, and much juice was ſpilt.

Sept. 20. I again put the ſame grapes into the ſame receiver; but becauſe they had ſpilt their juice by ebullition, I did not exhauſt all the air; but the mercury ſtaid in the height of five digits.

Sept. 21. This day in the morning, the receiver, being now full of air, did no longer ſtick to his cover; ſo that I took out the grapes, and tranſmitted them into another receiver, which I ſtopped cloſe with a ſcrew, but extracted no air from it.

Sept. 22. The height of the mercury was 11 digits, though the receiver was able to hold 26 ounces of water.

Sept. 23. The height of the mercury was 19.

24. The height of it was the ſame.

30. The height of it was 20.

Octob. 3. When the grapes produced no more air, I took them out, and found them of a bitter taſte, becauſe they were not yet come to their perfect ripeneſs.

This experiment, if you compare it with that, which I related before concerning unripe grapes, doth ſeem to intimate, that unripe grapes do produce leſs air when they are bruiſed, than when unbruiſed; but ripe grapes do the contrary.

Experiment V.

Nov. 29, 1678. I put apples into three vacuated receivers. In the firſt was a ſound apple; in the ſecond an apple bruiſed, and repoſited looſly in the open veſſel; in the third was alſo a bruiſed apple, and repoſited in the veſſel, but the cover was ſo fitted to the veſſel, that it did ſtraitly compreſs the parts of the apple; for I was deſirous to know, whether the bruiſed apple would produce air *in vacuo*, as well as the ſound one, provided his parts were narrowly conjoined: but the iſſue was, that in the exhauſting of the receiver, the air, formed between the parts of the apple, did expel all the juice.

Nov. 21. In the firſt receiver the height of the mercury was five digits; in the ſecond, three digits; in the third, none at all.

Nov. 23. In the firſt receiver the height of the mercury was ſeven; in the two others there was no change.

Dec. 7. In the firſt receiver the height of the mercury was 11 digits. There was no alteration in the other two.

Jan. 23. The firſt receiver was now ſevered from his cover by the force of the air produced anew. In the two others there was no air generated.

May 20, 1679. This day the third receiver was found forced from his cover; whereas the ſecond had produced no air.

This experiment informs us, that bruiſed fruits do produce leſs air *in vacuo* than ſound ones, contrary to what happens in common air. The reaſon whereof may perhaps be this, that fruits bruiſed are very much rarefied *in vacuo*, and ſo the

ſeveral

ſeveral principles, of which they conſiſt, cannot act upon one another: but unbruiſed fruits, by reaſon of the entireneſs of their ambient ſkin, undergo leſs rarefaction.

ARTICLE XV.

Air is ſometimes found unfit to produce mouldineſs.

EXPERIMENT I.

July 12, 1678. I put roſes into two receivers, which were to be ſtopped with ſcrews. One of them contained common air uncompreſſed; but I intruded ſo much air into the other, as ſuſtained the mercury 60 digits above its wonted height.

Aug. 2. THE roſes in the common air, four days ago, were turned into a yellow colour, as if they had been withered: but thoſe in the compreſſed air kept their colour very well.

Feb. 10, 1679. THE roſes, in the compreſſed air, as yet retained their freſh colour.

THIS experiment, compared with that, which was made the year before with roſes, doth inform us, that the air, at divers times, is diverſly affected; ſo that ſometimes it had a power to hinder corruption, and ſometimes to promote it. See *Artic.* IV. *Exper.* IV.

EXPERIMENT II.

May 22. FIFTEEN days ago I included two equal quantities of flowers in two receivers; into one of them I thruſt ſo much air, as ſuſtained the mercury 60 digits above its wonted height; but in the other I left common air incompreſſed. The flowers were tulips and larkſpurs.

SINCE that time no mouldineſs appeared, except only, that 10 days ago, one half of a tulip, being cut in two, in the common air ſeemed ſomewhat mouldy: but this day, the other half of the ſame tulip, in compreſſed air, ſeemed to be infected with ſome mouldineſs.

As for the flowers, ſome of them ſeemed as freſh as when they were firſt put in, eſpecially thoſe in the common air; for in the compreſſed air they ſeemed more moiſt.

June 22. No more mouldineſs appeared; whence we have a confirmation of the inference drawn from the former experiment, *viz.* that the air is ſometimes unfit to produce mouldineſs; ſeeing the year before all thoſe kind of flowers had contracted a great deal of mouldineſs.

ARTICLE XVI.

Experiments concerning the change of weight made by the beams of the ſun, even in veſſels ſealed hermetically.

EXPERIMENT I.

Sept. 4, 1678. I expoſed one drachm of minium in an open glaſs to the ſun beams, concentrated in a burning glaſs, and I found, that it had loſt $\frac{3}{4}$ of a grain of its weight, though much of the minium had not been touched by the ſolar rays.

EXPERIMENT II.

Sept. 6. I took coral, already calcined in fire, and endeavoured to calcine it further by the beams of the ſun in a ſealed glaſs, but I could ſcarce produce any good effect; yet the whiteneſs of the calx of the coral was ſomewhat increaſed.

Sept. 10. I expoſed the ſame coral again to the ſun-beams, in the ſame glaſs hermetically ſealed, for two whole hours; and weighing the glaſs, found, that the loſs of its weight was about $\frac{1}{16}$ part of a grain, ſince the time it was firſt ſealed.

EXPERIMENT III.

May 23. I put calx of tin in a light glaſs phial, ſealed hermetically, and weighed it exactly: afterwards I expoſed it to the beams of the ſun for a long time, by the help of a large burning-glaſs; then the glaſs, being again weighed, ſeemed to have loſt $\frac{1}{54}$ part of a grain of its weight.

May 29. I repeated the ſame experiment, only uſing minium inſtead of calx of tin, and the loſs of weight came to $\frac{1}{32}$ part of a grain.

May

May 30. I endeavoured to burn the ſame minium again; but ſuch plenty of air was produced, that the glaſs broke into an hundred pieces, and made a great noiſe at its diſſilition.

June 6. I tried the ſame experiment again with minium, and then $\frac{1}{34}$ part of a grain was abated of the weight.

WHEN I attempted again to burn the minium, the glaſs broke a ſecond time.

July 15. I took coals made of wood, for the ſame experiment; but the ſun did not affect them at all.

July 20. I expoſed vive ſulphur to the beams of the ſun, after the manner before deſcribed; and though it was eaſily melted, and did emit many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the ſame phial ſtill with the flower of ſulphur, and expoſed it often to the fire of my burning-glaſs, without danger of being broken, *viz.* becauſe ſulphur produceth no air; but the fumes were emitted as at the firſt, and the ſulphur bubbled up; but the weight ſeemed not to be changed.

ARTICLE XVII.

The preſervation of bodies in compreſſed liquors.

EXPERIMENT I.

Auguſt 3, 1678. I included two apricocks in two receivers, one of which was exactly filled with raiſins of the ſun bruiſed, and with water; but in the other there were only ſome raiſins encloſed, yet ſo, that the apricock was not touched, neither by the raiſins, nor by the water.

Sept. 10. I took out the apricock, incloſed with the water; and whilſt the air did break forth, the fruit did bubble very much: the raiſins had loſt almoſt all their taſte, but the apricock had preſerved a pleaſant reliſh; yea, it ſeemed more pleaſant than the taſte of ſuch fruits bought at that time of the year uſeth to be.

Feb. 10, 1678. THE apricock, incloſed without water, as yet kept its colour and figure, only ſeemed to have loſt its firmneſs.

THIS experiment informs us, that the taſte of ſome fruits may be preſerved in an infuſion of raiſins of the ſun; at leaſt in veſſels, which are able to contain a great compreſſion of the air.

EXPERIMENT II.

Sept. 17, 1678. I included peaches, with an infuſion of raiſins, in two receivers, ſhut with a ſcrew.

Sept. 21. Too great a quantity of air, produced in one of my receivers, expelled ſome part of the liquor out of it. The other receiver as yet retained its liquor.

Sept. 25. THE receiver, out of which the liquor was expelled, loſt ſome more hereof, ſo that its fifth or ſixth part now ſeemed empty: but ſetting the ſcrew, the liquor was then preſerved. The other receiver was not altered.

Sept. 26. THE ſame receiver began again to leak and run over, ſo that I ſet the ſcrew again.

Nov. 27. OUR receiver ſeemed hitherto to be ſhut exactly enough, but this day I opened it, and, whilſt the air was getting out, the peaches bubbled very much; one of them, of the ſort of thoſe, to which the ſtone or kernel uſeth to ſtick, had preſerved its firmneſs, and afforded a taſte pleaſant enough; but the other, being of that ſort, which are of a yellow colour, was very ſoft, yet the taſte thereof ſeemed to be more pleaſant than the taſte of the other. The liquor was very pleaſant and grateful.

Dec. 28. As yet the other receiver ſeemed unaltered; but when I opened it; an innumerable company of bubbles did emerge from the liquor, and from the peach. The peach on one ſide had preſerved its firmneſs, on the other it had loſt it; but the whole peach was acceptable to the palate, yet ſomewhat ſharp.

THIS experiment ſeems to teach us, that liquors may grow ſour, though no ſpirits have evaporated from them.

EXPERIMENT III.

Sept. 20. I included peaches, with unripe grapes, in two receivers, and filled them exactly. In the one were apples bruised to the consistency of a pultis: in the other, an infusion of raisins of the sun.

Sept. 25. THE receiver filled with pulp of apples hitherto seemed unaltered; but in the other, the air which was generated had extruded the half of the contained liquor, and impelled the mercury into the gage, to the height of 100 digits; wherefore I opened the receiver, and the peach, whilst the air was getting out, was almost reduced to the consistency of a pultis; the taste of it was pleasant enough.

I put another peach into the same receiver, and substituted a new infusion of raisins of the sun, instead of that, which was lost.

Sept. 26. THE mercury was now come to 30 digits above its wonted height.

Sept. 27. The height of the mercury was 72.
28. The height of it was 90. The liquor did work out.
30. The same height remained, but the liquor was all gone out.

Octob. 1. I now perceived, that all the air had also escaped; wherefore opening the receiver, I found the peaches very soft, yet of a pleasant taste.

Octob. 3. THE receiver filled with the pulp of apples had as yet lost nothing; but this day I perceived, that almost all the juice of the apples had run out: I opened the receiver, and found all therein very much fermented. The peach was very soft, but in taste not unpleasant.

THIS experiment informs us, that fruits cannot be long kept in pulp of apples, by reason of the great production of air; though that happens a little later in the infusion of raisins.

EXPERIMENT IV.

Sept. 23, 1678. I included peaches with crude grapes in two receivers, one of which was exactly filled with pulp of apples, the other with unripe grapes bruised.

Octob. 1. THE receiver filled with pulp of apples seemed as yet to have received no alteration; but the other was this day found emptied of his wine: this therefore I opened, and found one of the peaches to have retained its firmness, and its taste; but the other had lost its firmness, yet retained a grateful taste.

Feb. 5, 1676. THE receiver containing the pulp of apples hitherto seemed unaltered; yet I opened it, and the great ebullition thereupon did manifest, that a mighty compression of the air was in it. The pulp of apples and the peach had kept a grateful taste, but somewhat more pungent than ordinary.

THIS experiment shews us, that juice of crude grapes cannot conveniently be used for the preservation of fruits, by reason of production of too much air.

EXPERIMENT V.

Sept. 25, 1678. I included two pears, called butter pears, in a receiver exactly filled with pulp of apples.

Sept. 28. HITHERTO I perceived no alteration in the height of the mercury.

Octob. 5. THE mercury was now come to the height of 15 digits.

Octob. 6. THE height of the mercury was 16 digits and more.

Octob. 12. THE mercury was not changed.

Octob. 20. THREE days ago the mercury was depressed, though nothing had escaped out.

Octob. 26. THIS day my receiver was found cracked, though I did not find, that the air was compressed within; but perhaps the screw was set too high. The pulp of the apples was of a very grateful taste; so were the pears, but they were very soft, and one of them seemed to incline to rottenness.

PERHAPS the crack in the receiver was the cause, why so little air was produced in this experiment.

EXPE-

EXPERIMENT VI.

Oct. 1, 1678. I inclosed peaches in two receivers, one of which was filled with pulp of apples, and the other with unripe grapes bruised.

Octob. 5. MUCH air was produced in the second receiver, yet some of the wine ran out. The height of the mercury was 64 digits.

Octob. 6. THE wine proceeds to run out: the height of the mercury was 70.

Octob. 8. Now the wine all run out of the receiver, and the height of the mercury was 86.

Octob. 12. THE height of the mercury abode at 86.

Octob. 18. THAT receiver, out of which all the wine was run, yet held the air very well; and the height of the mercury in it, staid at 86. The other receiver, filled with pulp of apples; had for these five last days suffered some juice to flow out.

Dec. 4. I opened the receiver filled with pulp of apples: and though all the juice was got out, yet it still contained the air, very much compressed; and many bubbles brake forth, not without some noise, after the receiver was quite opened. The peach was very soft, and of a pungent taste, like to that of inebriating wine.

Jan. 21, 1679. AFTER the effusion of the wine in the other receiver, the mercury staid in the same height. I opened the receiver; the peaches did emit many bubbles, and were wrinkled, but their colour was little changed: their sapor was most pungent, and inclining to acid.

THIS experiment doth confirm the conclusions of the former.

EXPERIMENT VII.

Octob. 4, 1678. I put peaches into three receivers; the first of which was filled with ale, or beer without hops; the second with beer hopped; the third with wine.

Octob. 5. The height of the mercury in the first receiver was 15 digits; in the second, 10; in the third, 9 digits.

Octob. 6. The height of it in the first receiver was 25 digits; in the second, 15; in the third, 20.

Octob. 8. The height of the mercury in the first receiver, was 35 digits; in the second, 15; in the third, 20.

Octob. 12. The height in the first receiver was 63 digits; in the second, 15; in the third, 28.

15. The height of the mercury in the first receiver was 81 digits; in the second, 15; in the third, 30.

16. There was no more change perceived in any of the three receivers.

18. The mercury rather descended than ascended, in all the three receivers.

22. In the wine only, the mercury ascended or descended according to the heat and the cold.

24. The height of the mercury in the first receiver was 96 digits; in the second, 15; in the third, 30.

30. The height in the first receiver was 115 digits; in the second, 20; in the third, 30.

Novem. 3. The height in the first receiver was 117 digits; in the second 20; in the third, 30.

6. The height in the first receiver was 120 digits; in the second 31; in the third, 31.

11. The height of the mercury in the first receiver was 105 digits; in the second, 31; in the third, 28. It was cold weather.

Novem. 16. The height of the mercury was the same. The peach, which hitherto was demersed, now mounted up to the upper part of the liquor in the second receiver; all the rest staid in the bottom.

Novem. 25. The height in the firſt receiver was 140 digits; in the ſecond, 47; in the third, 32.

Novem. 28. The height in the firſt receiver was 96 digits; in the ſecond, 36; in the third, 28. It was very cold weather.

Dec. 13. The height in the firſt receiver was 96 digits; in the ſecond, 47; in the third, 33. I opened the third receiver, and found the peach firm, and of a laudable colour; but it had contracted much of taſte from the wine, which yet was capable of being amended by ſugar, ſo that a very pleaſant and edible diſh might be made thereof. The wine alſo was grateful to the palate.

Dec. 30. The height of the mercury in the firſt receiver was 96 digits; in the ſecond, 47. I opened the firſt receiver, and the peaches, which had lain till then at the bottom of the liquor, did preſently emerge to the upper part thereof; they emitted many bubbles: the taſte of the ale, of which they had contracted much, was made pleaſant with ſugar.

THIS experiment informs us, that fermented liquors may be uſeful for the preſervation of fruits, as being unfit to produce air.

EXPERIMENT VIII.

Sept. 5, 1678. I included one peach not cut, with another cut into pieces, in a receiver; into which I after poured old wine, till it was exactly filled, and then ſhut it with a ſcrew. I hoped the iſſue would have been, that if the wine did extract any tincture from the peach, that the cut peach would eaſily ſupply it; and ſo the whole peach would keep its full taſte.

Nov. 20. As yet nothing ſeemed to be altered; but this day I perceived, that ſome of the wine did run out.

Nov. 30. THE third part of the wine was loſt.

Decem. 8. Seeing the wine begin again to run out, and that there was little of it left, I opened the receiver, and found the peaches very much fermented, yet endued with a grateful, but moſt pungent taſte. The wine alſo was pleaſant.

BY this experiment, if it be compared with the third receiver in the former experiment, we may conjecture, that wine doth hinder the fermentation of peaches, if it be in a ſufficient quantity; but here the wine was not ſufficient, becauſe the pieces of that peach, which was cut, did fill the whole receiver, ſo that no room was left for the wine, but in the interſtices.

EXPERIMENT IX.

Octob. 11, 1678. I put two peaches, one whole, the other cut in pieces, into a receiver filled with hopped and fermented beer.

Octob. 12. In one night's ſpace the mercury aſcended three digits.

Oct. 15. The height of the mercury was 15 digits.
16. The height of it was 15.
18. The height of it was 12. It was very cold.
20. The height of it remained at 12.
22. Now the mercury aſcended again. The cold abated.

Nov. 2. The height of the mercury was 20.
3. The mercury deſcended a little. It was cold weather.
6. The height of the mercury was 28. The weather grew hotter.
8. The height of it was 33.
11. The height of the mercury was 40.
12. The height remained at 40. Some of the beer wrought out.
16. The height of it was 46.
19. The height of it was 43. But much of the beer was loſt.
21. The mercury aſcended not, but the beer proceeded to work out.
23. When the beer was almoſt all wrought out, I opened the receiver,

receiver, and found the peaches very soft, yet of a grateful taste, though they had been kept nine hours in the free air, after the receiver was opened.

N. *These fruits were never quite ripe.*

FROM this experiment, if it be compared with the second receiver in *Exper.* VII. it may be inferred, that beer doth hinder the fermentation of peaches, and the production of air, if it be in a sufficient quantity: but here there was but a little beer contained in the interstices, which was not able to hinder the fermentation of the peaches.

EXPERIMENT X.

Octob. 19, 1678. I included raw beef in three receivers; the first of which was exactly filled with stale beer, forcibly intruded, so that the mercury exceeded its wonted height by 60 digits. The second was also exactly filled with stale beer, but here there was no compression made. The third was filled partly with the beef, and partly with common air.

Octob. 20. IN the first receiver the mercury was depressed to the twentieth digit beyond its usual height, though nothing at all had escaped out. In the second also, it descended a little; but in the third, it ascended somewhat.

Octob. 26. IN the first receiver the mercury did sometimes ascend, and then descend very irregularly; in the second it began to ascend slowly two days ago; in the third it was not moved at all.

Octob. 27. ONE piece of the same beef, which was left in the air, began to have an ill smell; and also the mercury in the third receiver began to ascend. In the second it proceeded to ascend by little and little; but in the first it seemed rather to descend.

Nov. 3. THE mercury in the first receiver ascended not; in the second, the height of it was 20 digits; in the third, it was 10 digits.

Nov. 5. I opened all the receivers, and the two first did not stink at all, yet they had contracted a smell from the beer; the flesh boiled in the same beer was found very tender, but its taste was bitter, perhaps by reason of the too great quantity of the beer. That beef, which was included with common air, when the receiver was opened, did presently affect the nostrils with a stinking smell; yet, when it was taken out, and accurately smelt to, it scarce seemed to stink. I included the same flesh in the same receiver, to try, whether new air being admitted would promote corruption.

Nov. 6. The height of the mercury was three digits.

11. The height of it was 9.

25. The height of it was 20 digits.

I opened the receiver; I found the flesh so stinking, that I was forced to throw it away.

FROM this experiment it seems to follow, that beer may be convenient for the preservation of flesh, especially if it be intruded by force into the receiver; but this compression is soon abated, because the air compressed in the same receiver is apt to enter into and pervade the pores of the beer by degrees.

EXPERIMENT XI.

Nov. 12. I included beef, as hardly as I was able to do it, in three receivers: into the first of them I poured water, mixed with one fortieth part of salt, which filled up all the interstices, which were left betwixt the parts of the flesh: in the second some salt water was in like sort contained; but it was intruded by force, so that the mercury in the gage ascended to 15 digits above its wonted height: into the third receiver I poured no water, and therefore those few interstices, which could not be possessed by the flesh, was left for the air.

Nov. 13. THE mercury descended in all the receivers, especially in the second, which had admitted the compressed liquor.

Nov. 18. THE two receivers, which were not compressed, did not repel the depressed mercury upward: but as for that, whose mercury had been impelled to 15 digits, and afterwards had descended most of all, it now returned almost to its former height. A piece of the same beef, being left in the air, began to have a bad smell.

Nov. 23. IN the three receivers air was produced a new; but this day in the second the mercury descended three digits; the height of it was 20: in the other two it was about 16. I opened the first receiver, and the flesh was not corrupted at all.

Nov. 30. I took the flesh out of the receiver, which was put in without salt; it did not stink at all; but being boiled, was very tender and of a pleasant taste.

Dec. 6. I opened the receiver, into which I had forcibly introduced salt water. The mercury exceeded its wonted height 25 digits. The smell of the flesh did strongly affect the nostrils, yet it did not stink. The flesh put *in vacuo* sent forth many bubbles, which ceased not but a pretty while after the receiver, in which it was included, was taken out of the pneumatic engine; yet the mercury, in one hour's space, came to the height of three or four digits. Afterwards I immersed the same receiver so exhausted in hot water, and the liquor contained therein did bubble very much, though the water from which it borrowed all its heat did not boil at all; but so great a quantity of air was produced, or else had entered from without, that the receiver was quickly full. Afterwards the liquor contained therein did not bubble or boil, though it were immerged in boiling water. I took out the flesh, and found it pleasant and tender, yet less so than I expected, perhaps because it was not yet boiled enough.

THIS experiment teacheth us, that water, as well as beer, may conduce to the preservation of flesh.

EXPERIMENT XII.

Nov. 29, 1678. I inclosed oysters in four receivers: in the first the oysters were without their shells, and filled the whole space as exactly as we could; in the second, the oysters, not taken out of their shells, were included with common air: in the third, the oysters also were included in their shells, and the remaining space of the receiver was exactly filled with salt water. All these three vessels were firmly closed with screws. The fourth receiver was exhausted of air, and it contained three oysters in their shells, and eight taken out of their shells. When the air was pumped out of this receiver, the oysters, which were taken out of their shells, did emit many bubbles, and those very great ones; but the three others underwent no sensible mutation, save that one of them did gape.

Nov. 30. IN the three recipients, which were stopped with screws, the air seemed to be consumed, rather than produced; but the mercury *in vacuo* ascended a little.

Dec. 4. WHILST the weather was cold, the mercury ascended not; but now when the cold began to abate, the height of the mercury in the first receiver was seven digits; in the second, none; in the third, three; in the fourth, three.

Dec. 5. THE height of the mercury in the first receiver was 20 digits; in the second, one digit; in the third, three; in the fourth, five.

Dec. 7. THE height of the mercury in the first receiver was 30 digits; in the second, one digit; in the third, three; in the fourth, eight. Other oysters, left at the same time in the air, had a bad smell.

Dec. 9. IN the first receiver the height was 30; in the fourth, 11. The rest were not changed.

Dec. 13. THERE was no change in the three first receivers, but in the fourth the height was 14 digits.

Dec. 20. IN the first receiver the height was 46 digits; in the fourth, 24; the rest were not changed.

Dec. 21. IN the first receiver the height was 52 digits; in the fourth, 25: in the rest no change.

Dec. 22. THE height of the mercury in the firſt receiver was 60; in the fourth, 27: no change in the reſt.

Dec. 27. IN the fourth receiver the height was 29; the reſt were not changed.

Jan. 1, 1679. THE oyſters in the third receiver had tinged the water with a black colour.

Jan. 25. THE mercury *in vacuo* ſeemed ſtill to remain almoſt in the ſame height. But this day ſome bubbles were formed in the turpentine, by the internal air, about the commiſſure of the cover with the receiver. Therefore I opened the receiver, and found the oyſters very ſtinking; I likewiſe opened the other receivers, and found the oyſters of a ſtinking ſmell, and turned to a kind of viſcous gelly.

THIS experiment ſeems to inform us, that fiſhes do produce leſs air than fleſh; and yet that they will be corrupted, though they are fortified againſt the air.

EXPERIMENT XIII.

Nov. 29, 1678. I exactly filled a glaſs veſſel with freſh butter, not at all ſalted, and then ſtopped it with a ſcrew. A mercurial gage was included in the ſame veſſel

Nov. 30. IN the night, the cold being very ſharp, the butter was condenſed, for the mercury came nearer to the aperture of its gage.

Dec. 2. THE mercury came nearer and nearer to the aperture of its gage, perhaps becauſe the cold did daily increaſe.

Dec. 5. THE cold being abated, the mercury returned almoſt to its former height; part of the ſame butter, being left in the air, began to have a very bad ſmell.

Dec. 7. THE cold again returning, the mercury did alſo again come to the top of its gage. The butter left in the air ſmelt worſe than before, notwithſtanding, as yet, it was edible.

Dec. 24. THE butter had produced no air; being taken out of the receiver, it was of a grateful taſte, except only a little of the ſuperficies, which was contiguous to the leather, that was ſpread over the cover.

FROM this experiment it follows, that butter may be kept a great while, if it be defended from the contact of the external air.

EXPERIMENT XIV.

Nov. 30, 1678. I filled two receivers with whitings; and that no air might be left in the vacant ſpaces, into the one I poured wine; into the other oyſters, with their juice, without their ſhells; ſo that both the receivers were exactly filled. When I had afterwards cloſed their covers with ſcrews, the air in the mercurial gages was compreſſed; but in three hours ſpace the mercury again returned to its former mark

Dec. 2. THE cold increaſing, the mercury came nearer to the aperture of its gage in both receivers.

Dec. 4. THE cold ceaſing, the mercury aſcended very much in that receiver wherein the oyſters were; but in the other receiver it was not moved.

Dec. 5. IN the receiver containing the oyſters, the height of the mercury was 20 digits; but in the other, it was not yet returned to its wonted height.

Dec. 7. IN the receiver with oyſters, the height of the mercury was 40 digits; in the other, it continued ſtill below its wonted height.

Dec. 9. THE mercury in both receivers was changed little or nothing.

Dec. 20. WHEN the mercury was changed no more, I opened the receivers, and both of them were found to be very ſtinking. And this ſeemed new to me in this experiment, that the receiver, in which the wine was, had admitted of corruption without production of air; for hitherto all bodies, whilſt they were corrupting, produced air.

EXPERIMENT XV.

Dec. 3, 1678. I put raw beef into two large receivers, with pepper and cloves; and

and that no air might be left in the interstices, I poured in beer upon them, and no long time after I found the pressure of the air in the receivers to be abated, the mercury in the gages coming to the open ends.

Dec. 8. THE mercury did not ascend in either of the receivers. I opened the one, that I might boil the flesh; it was endued with a sweet smell, contracted from the cloves; and the liquor contained in the same receiver, before it was boiled, did smell like hippocras.

Jan. 2, 1679. I opened the other receiver, and found no air produced therein; the flesh was not at all corrupted, and when I boiled it *in vacuo*, I observed, that if a more intense fire were kindled, the air, or some spirits, did make an eruption through the stop-cock, which was fastened to the top of the receiver. The receiver, being cooled all the night, the day after was found almost quite empty of air. The flesh was very tender, and well tasted, only it was a little over-boiled, for it had been kept on the fire six full hours.

WE have a confirmation by this experiment, that beer may be useful for the preservation of flesh, especially if the bitter taste thereof be corrected by some aromaticks.

EXPERIMENT XVI.

Dec. 4, 1678. I included two larks, with some beef, in a receiver, all whose spaces, unpossessed by the flesh, I filled with ale; and at the same time I filled another receiver with the same sort of beef, adding beer also, but no larks were put in with it.

Dec. 9. SOME pieces cut off from the larks, and exposed to the air, began to smell ill; but those included in the receiver, as yet had produced but little air; for the mercury was not yet come to five digits above its wonted height. In the other receiver it was not moved.

Dec. 19. IN the receiver, which contained the larks, the mercury ascended no higher; for the cover being broken, suffered the liquor to run out. Wherefore I opened the receiver, and boiled both the beef and the larks, which were not at all corrupted, but they seemed very acceptable to the palate; yea the beef had contracted a pleasant taste, partly from the larks, and partly from the beer.

Dec. 23. I opened the other receiver, and the boiled flesh seemed pleasant, yet not so pleasant as that, which was endued with a venison-like taste from the larks.

THIS experiment shews, that even tender birds may be preserved long by the help of beer or ale.

EXPERIMENT XVII.

Dec. 14. I included apples in four receivers; in the first was an whole apple, and all the spaces were filled with powdered sugar: in the second, an apple was cut in pieces, and the spaces filled with sugar, as before: in the third an apple was also cut, but the rest of the receiver was filled with water, wherewith $\frac{1}{10}$ part of sugar was mixed: in the fourth, the apple was also cut, and the spaces were likewise filled with a solution of one part sugar, and five parts of water.

Dec. 21. THIS day, in the first receiver, the mercury began a little to ascend, yet the sugar did not melt: in the second receiver all the sugar was melted, and the pieces of apple were shrivelled, also they produced much air when they were first put into the receiver. In the two other receivers the mercury began also to ascend; but in the third, the pieces of apple were very much corrupted, for their skin or rhine was taken off.

Dec. 22. AIR was produced in all the receivers, but the quantities of the air produced, did not bear the same proportion amongst themselves, as the quantities of the sugar: for in the second receiver much air was produced, but in the fourth the mercury ascended less than in the third; and besides, in the first some air was generated.

Dec.

Dec. 27. IN the three firſt receivers the height of the mercury was ten digits; but in the fourth it was only ſix digits.

Dec. 31. IN the firſt and ſecond receivers the height of the mercury was 13; in the third the height was 15; in the fourth it was only nine digits.

Jan. 2, 1679. IN the firſt and ſecond receivers the height of the mercury was almoſt 14; in the third 17; in the fourth 11.

Jan. 7. IN the ſecond receiver the height of the mercury was 16 digits; in the third 36; in the fourth the height of it was 15: but in the firſt the mercury had not aſcended, and ſomething had eſcaped out of the receiver, and therefore I eaſed the ſcrew, that I might diſpoſe of it the better; and then the air made an eſcape.

Jan. 9. IN the firſt receiver the height was ſix digits; in the ſecond 16; in the third 39; in the fourth 15.

Jan. 17. IN the firſt receiver the height was 13; in the ſecond 19; in the third 56; in the fourth 17.

Jan. 30. IN the third receiver the height of the mercury was 76 digits, and the liquor brake out, and therefore I opened it, and found the fruit to have loſt much of its taſte, but the water had contracted it, and was pleaſant enough to the palate. In the ſecond receiver the mercury aſcended no more. I opened this receiver alſo, and found the fruit much more pleaſant in this than the other; yet much of its taſte was imparted to the ambient ſugar, ſo that it was found changed into a very good ſyrup.

Feb. 16. THE height of the mercury in the firſt receiver was 22 digits; but in the fourth 33. I opened it, and found the fruit to have loſt much of its taſte, and that the ambient water had got it, and was thereby turned into a pleaſant drink.

Feb. 27. IN the firſt receiver the height of the mercury was 30 digits.

March 15. IN the firſt receiver the height of the mercury was not changed; but this day I found ſomething to eſcape out of the receiver, and therefore I opened it, and found the apple of a laudable colour; but the pulp was ſpungy, and had loſt much of its taſte.

THIS experiment ſeems to teach us, that ſugar is not ſo fit for the preſervation of fruits, as fermented liquors. See *Exper.* VII.

EXPERIMENT XVIII.

Dec. 23. I filled a glaſs veſſel with milk, and then ſtopped it with a ſcrew; and into another receiver I put a lark with milk, and ſtopped it cloſe.

Dec. 24 THIS evening I perceived, that the caſeous part was ſevered from the butyrous, in the cloſed receivers as well as in the milk, which, at the ſame time, I had left expoſed to the air.

Dec. 27. I found no air produced in the receiver, which held the lark; but in the other, the mercurial gage was ſpoiled.

Dec. 31. THE mercury aſcended in that receiver. which contained the lark; but the milk, that was left in the air at the ſame time, that I ſtopped the receivers, did ſtink three days ago.

Jan. 1, 1679. IN the receiver, wherein the lark was included, the height of the mercury was 10 digits.

Jan. 2. THE height of the mercury was $14\frac{1}{2}$. The milk ſtagnant below the butyrous part appeared of a red colour.

Jan. 4. THE height of the mercury was 19. Some white ſediment was concreted in the bottom of the milk.

Jan. 9. THE height of the mercury was 29 digits.

Jan. 25. I opened both receivers, and found the lark to affect the noſtrils with a ſtrong, though no fœtid ſmell, yet it had been kept 32 days; when it was boiled, it was of a pleaſant taſte. In the other receiver, the caſeous part of the milk was ſubacid and grateful, but the butyrous part was not ſour at all.

THIS experiment informs us, that ſometimes milk may be uſed with good ſucceſs for the preſervation of fleſh.

EXPE-

EXPERIMENT XIX.

Dec. 24, 1678. I put a lark into a ſmall receiver, and poured butter upon it, melted with a ſlow fire, till all the ſpaces were exactly filled; then I cloſed the cover with a ſcrew.

Dec. 27. THE mercury approached nearer to the aperture of its gage; but the butter ſeemed to be altered, for the loweſt part of it was more yellow, and the middle more white than it ſeemed before the incluſion thereof: the upper part was fluid.

Jan 5, 1679. THE mercury returned by little and little, to its wonted height.

Jan. 9. THE mercury was ſomewhat higher.

Jan. 28. THE mercury was little changed: I opened the receiver, and found that part of the butter, which was contiguous to the leather ſpread over the cover, to be white, and of a very unacceptable taſte. The butter, which was more remote from the leather, was yellow and ſomething graveolent, yet it was edible. But the lark being roaſted was grateful to the palate, though it had been kept 34 days.

THIS experiment ſeems to inform us, that butter melted and hot is not ſo ſucceſsfully uſed for the preſervation of fleſh.

EXPERIMENT XX.

Jan. 4, 1679. I included boiled fleſh *in vacuo*, in a receiver ſtopped with a ſcrew, and filled the interſtices exactly with broth of the ſame fleſh, which ſeemed a little too ſalt. Whilſt I ſet the ſcrew all the things in the receiver ſuffered a compreſſion, and the mercury aſcended to the height of ſix digits into the gage; but ſhortly after it returned to its wonted height.

Jan. 28. THE air was more and more conſumed, ſo that the mercury now deſcended to eight digits below its wonted height. I opened the receiver, and found the fleſh very ſweet and tender. The broth alſo had a ſubacid, but a very grateful taſte.

THIS experiment informs us, that fleſh, after it is boiled, may be kept long without prejudice, which is a great conveniency in long voyages at ſea; ſo that perhaps there will be no need of ſalted fleſh. For after the raw fleſh hath been kept ſo long in veſſels ſtopped with ſcrews, till experience ſhews, that there is no danger of its corruption, then it is to be taken out, and being perfectly boiled, is again to be included in the ſame receivers: and ſo without doubt it may be kept for a long time without ſalt. See *Exper.* XII.

EXPERIMENT XXI.

Jan. 30, 1679. I put raw fleſh into two receivers: to the firſt I added pepper and cloves; in the ſecond I mixed nothing, for I was willing to know, whether theſe ſpices would promote the production of air, or retard it.

Feb. 11. THE height of the mercury in the firſt receiver was three digits; in the ſecond the height of it was below $1\frac{1}{2}$.

Feb. 12. THE height of the mercury in the firſt receiver was $4\frac{1}{2}$; in the ſecond not above $1\frac{1}{2}$.

Feb. 13. IN the firſt receiver the height of the mercury was ſix digits and more; in the ſecond it was three digits. I boiled the fleſh of the firſt receiver, after the manner before deſcribed, and it was very pleaſant and tender.

Feb. 14. THE height of the mercury in the ſecond receiver, was five digits.

Feb. 19. THE height of the mercury in the ſecond receiver, was eight digits.

Feb. 20. THE height of the mercury in the ſecond receiver was 11 digits. I boiled the fleſh and found it very tender, though it had ſtaid over the fire *in balneo Mariæ* only for three quarters of an hour. I put ſome part of this fleſh, before it was boiled, into a receiver, and filled all the ſpaces as exactly as I could with the ſame fleſh, to try how long the fleſh might be preſerved when the air was ſo excluded.

Feb. 21. THE mercury aſcended very little.

March

March 20. THE height of the mercury was about 16 digits. I opened the receiver, and the flesh seemed of a pleasant taste, yet inclining to corruption.

EXPERIMENT XXII.

Feb. 10. I put raw beef into three receivers: in the first, the beef was seasoned with pepper and cloves; in the second it was encompassed with salt water; in the third I put neither salt nor spice.

Feb. 19. FOUR days ago the mercury ascended in the third receiver; in the first also it began to ascend; but in the second it was not moved at all.

Feb. 21. IN the first receiver the height of the mercury was four digits and $\frac{1}{2}$; in the third 10 digits; but in the second there was no ascent at all.

Feb. 25. THE height of the mercury in the first receiver was six digits; in the third 19 digits; in the second half a digit.

Feb. 26. THIS night there was no ascension of the mercury in all the receivers. I opened the third receiver, and the flesh, after boiling, and was found very good.

THE former experiment seems to teach us, that spices do hinder the production of air; but the present experiment proves the contrary. Whence this contrariety should proceed, I know not; unless it be, because, perhaps, I had left a space large enough for the air in these receivers; but in the former experiment I filled all as exactly as I could with flesh.

March 9. THE height of the mercury in the first receiver was eight digits; in the second none.

March 12. THE height of the mercury in the first receiver was 12 digits; in the second one digit.

April 3. THE height of the mercury in the first receiver was 11 digits: but in the second it exceeded not one digit. I opened the receiver, and boiling the flesh, after my accustomed manner, I found it very tender, and of an excellent taste.

THE corollary from this experiment seems to be, that the saltness of water, included with flesh, doth hinder the production of air; but because there was so small a quantity of water, compared with the quantity of flesh, I do rather incline to think, that less air was produced in the second receiver, because it was more exactly filled. And indeed, if fresh water had been used instead of salt, the matter succeeds after the same sort; but the chief art to preserve flesh without salt consists herein, that all air be excluded from it, and that there be a great compression in the receiver.

ALL these experiments about the preservation of aliments, what great use they may be of for the transporting of fruits, venison, or other flesh, from places far remote to great cities, and for the affording better nourishment to mariners, I leave to the reader to judge.

ARTICLE XVIII.

Experiments concerning elixation and distillation in vacuo.

EXPERIMENT I.

Dec. 12, 1678. I put two ounces and six drachms of beef into an empty receiver, which was able to hold 22 ounces of water. Then I put it into boiling water for three hours; which being done, I exposed it to the air to be cooled for a whole night; afterwards, using my pneumatic engine, I perceived, that the air formed in the receiver could scarce sustain three digits of mercury; and so deducting from the calculation, a man may easily find, that flesh, whilst it is boiled, cannot form air enough to make an entire pressure in a receiver capable of holding a double weight of water: that is, if you include one pound of flesh in an emptied receiver, able to hold two pounds of water, it will not generate air, that can remove the cover from the receiver, unless heat do confer much to produce the effect; but I confess, that our flesh was not boiled enough.

See the description of a vessel to boil and distil in vacuo, p. 515.

EXPERIMENT II.

Dec. 23. I inclofed three ounces of raw beef in a receiver able to hold 32 ounces of water; and when it boiled, having been long on the fire, the cover was forced from its receiver, and fo fuffered the vapours to pafs out; but becaufe it was prefently fhut again, the fire being removed, the receiver foon loft its internal preffure, fo that being fet again to the fire, it was a long time before it could force away the cover the fecond time. I tried this again and again; yea, unlefs the receiver had been expofed to a very ftrong fire, the cover could never have been removed; but if the fire be kindled enough, fweet exhalations continually pafs out.

Dec. 24. THE receiver, having been cooled during the whole night, was this day, by the ufe of the pneumatic engine, almoft wholly evacuated. Whence we feem to have a confirmation, that the divulfion of the cover is not made by that air, which can keep the form of air, but from the fteams exhaling from the flefh, and fubfiding again therein, if they be hindred from egrefs, which may eafily be performed, if we ufe not too fierce a fire in the empty receiver, and fo the lofs of thofe fweet fmelling vapours may be eafily avoided.

EXPERIMENT III.

Jan. 21, 1679. I put pafte, without leaven, into an exhaufted receiver; and alfo I included another part of the fame pafte in another receiver, full of common air. I enclofed thefe two receivers *in balneo Mariæ*, ftopped with a fcrew; and when they had ftaid there for three hours, having been expofed to a moderate fire, I opened the receivers: the pafte *in vacuo* I found reddifh, as far as the fuperficies; but the other had admitted water; and the pafte was not boiled enough, and therefore I put both receivers again *in balneo Mariæ*, where they ftaid an whole night.

Jan. 22. THIS day in the morning, I found the *balneum Mariæ* quite cold; and the pafte, when it was taken out, was boiled enough, but it was covered with no cruft, That, which was included *in vacuo*, was interfperfed with many cavities, but it feemed too infipid; the other contained no cavities, but afforded a more pleafant tafte. Both the receivers were found almoft wholly emptied of air.

EXPERIMENT IV.

Feb. 3, 1679. I enclofed pafte kneaded with leaven *in vacuo*; and, as foon as it had filled its receiver with factitious air, I tranfmitted it into that receiver, which I am accuftomed to ufe to boil flefh *in balneo Mariæ*; but when the pafte was thus removed out of one receiver into another, it pitched or funk very much; yet when it had remained for three hours in a fervid *balneo Mariæ*, the bread made of it was interfperfed with many cavities, but it was covered with no cruft.

Feb. 5. I iterated the fame experiment; but this time the pafte was included *in vacuo*, in the fame receiver, which was afterwards put *in balneo Mariæ*, and therefore there was no need to remove the pafte, and expofe it to the air. Hence it came to pafs, that the bread made thereof was much lighter than the former.

EXPERIMENT V.

Feb. 12. I included rofemary with water in the veffel defcribed p. 515. and when the air was pumped out, I put the veffel *in balneo arenæ*, and there came forth a water endued with a very fweet fmell; yea, and fome drops of effential oil, fmelling very fweet alfo, and affected with no *empyreuma*. But when I opened the ftop cock for to let in the air, the noife did fo foon ceafe, that I judged much air was produced from the rofemary.

Feb. 13. I put the fame rofemary into the fame evacuated veffel, and adminiftred a more intenfe fire thereunto; yet I could extract no oil, neither fweet nor ftinking; and befides, the water was lefs fragrant than the former.

EXPE-

EXPERIMENT VI.

Feb. 10, 1679. I boiled one pound of flesh *in vacuo*, in the vessel described p. 515, which could contain almost four pounds of water: the upper part thereof, which was made of glass, did hold the mercurial gage, by the help whereof I perceived, that the mercury had not ascended to the height of three digits, though the flesh had boiled for three hours and more. It was not boiled enough, and its taste was ungrateful; and moreover the liquor, which was formed of the condensed vapours, afforded also an unpleasant taste.

Feb. 11. I iterated the former experiment, but this time I sprinkled the flesh with pepper and cloves; the issue was, that the mercury ascended to the height of six digits, though the flesh was boiled no longer than the other: it seemed very grateful to the palate, and the liquor formed from the vapours afforded a most pungent taste of pepper; but it had contracted nothing ungrateful from the flesh, as was done in the former experiment.

FROM these experiments made about elixation and distillation *in vacuo*, the corollary seems to be, that such vessels may be very useful for the distilling and boiling of such bodies, which do contain thin and very volatile spirits; for all things will be preserved by their help, and nothing will avolate or fly away.

ARTICLE XIX.

Concerning elixation in vessels stopped with screws, by the help whereof, even hartshorn, and the bones of fishes, and four-footed Creatures may be softned.

EXPERIMENT I.

Jan. 29. EIGHT days ago I filled a vessel, stopped with a screw, with beef and water together, and when it had continued exposed to a moderate fire, for eight or nine hours *in balneo Mariæ*, stopped also with a screw, I took the flesh out of it; but it was boiled a great deal too much, and the taste of it was very unpleasant. Afterwards, I boiled new beef in the same vessel, and after the same manner, save that this was seasoned with pepper and cloves, and remained exposed to the fire only for three hours. The issue was, that this flesh preserved a most pleasant taste: wherefore that I might know, whether the excellency of this flesh above the other, did proceed from the spices, or from a shorter time of boiling, I boiled other flesh without spices for three hours, in the same manner: when the flesh was taken out, it was of a good taste. Whence I conjectured, that the cause of spoiling the first flesh was to be chiefly ascribed to the over-boiling: yet I think, that the spices may be convenient to correct some part of the ungrateful taste; for I left a place for the condensing of the vapours, in the top of the vessel, and found, that the liquor there formed was of an unpleasant taste; but when the flesh was seasoned with pepper and cloves, no such thing was found.

EXPERIMENT II.

Jan. 29. I boiled apples, after the same manner as I did the flesh before described; but I mixed no water with them. They were set upon a moderate fire almost for two hours. They were very soft, and of a very good taste, but some pieces, which were laid in the upper part of the receiver, where the vapours ascending from the inferior part were condensed, were found of an unpleasant taste; and also the drops, formed from the same vapours, did affect the nostrils with an ungrateful odour.

EXPERIMENT III.

Feb. 4. I inclosed flesh, with pepper and cloves, in a receiver stopped with a screw, but poured no water in to fill the interstices; only I compressed the flesh, as much as I could, and then I put the receiver *in balneo Mariæ*, already hot, and stopped it with a screw; and when it had remained there, over a moderate fire, for a whole hour, the flesh was rather over-boiled than under-boiled: but when I opened the *balneum Mariæ*, all the water brake out of it with a great force, *viz.* the liquor being hot, and hitherto incarcerated, now having freedom given, at length did shew its strength.

Feb. 5. I inclosed some part of this flesh in a receiver stopped with a screw.

March 12. THE flesh, which was included five weeks ago, was this day found very good. I do not doubt, but that perfect elixation was able to contribute something to its preservation, *viz.* because the sundry principles of which flesh consisteth, had, whilst the heat continued, exerted their strength upon one another, far better than if the flesh, being less boiled, by reason of the great avolation of parts, had been to be removed from the fire, as it happens in ordinary coctions. And indeed, by experiments made about other bodies, I have found that elixation, the perfecter it is, doth so much the more hinder fermentation. See *Artic.* XVII. *Exper* XII, XX.

EXPERIMENT IV.

Feb. 10. I boiled an ox-foot or cow-heel, after the same manner as I had done the flesh above-mentioned, but I left the cow heel for four hours or more, upon a moderate fire. That time being elapsed, and the vessels unstopped, the flesh was excellently well boiled, and the bones were so soft, that they might be cut with a knife, and eaten like cheese.

Feb. 12. I repeated the same experiment, but the vessels remained exposed to the fire for twelve hours space; and though the water of the *balneum Mariæ* did every where secure the vessel demersed in it, yet the flesh had contracted a taste and a smell very *empyreumatical*; but the juice, which in the former experiment did concrete into a very firm gelly, in this latter could not be congealed at all.

BY these experiments it appears, that many bones and hard tendons, which we daily cast away as unprofitable, by the help of *balneum Mariæ*, stopped with a screw, may be converted into good nourishment.

EXPERIMENT V.

Feb. 10. I boiled a fish, after the same manner as was described above, *in balneo Mariæ* stopped with a screw, but I mixed no water therewith. The fish staid upon the fire two hours only; then the vessel being cooled and opened, the fish was found of a very good taste, and his bones were so soft, that they yielded to the pressure of one's finger, and the head of it could be eaten like its flesh. The juice of it, in a short time, did concrete into a gelly of an hard consistence.

THIS experiment is very useful for the boiling of fish, which are full of bones.

EXPERIMENT VI.

Feb. 15. I put harts-horn into a receiver, which was to be stopped with a screw, and filled the intervals with water. I included the receiver, thus stopped, *in balneo Mariæ* stopped also with a screw, and so exposed it for four hours to a moderate fire; when that time was passed and the vessels opened, the harts-horn was as soft as cheese, and the juice did soon concrete into a very firm gelly.

Feb. 17. I repeated the same experiment, but no water was included with the harts-horn, and the fire lasted six hours under the *balneum Mariæ*: when this was done, the harts-horn was found very soft, but a little juice had excreted out of it, and that did adhere to the external parts of the harts-horn in the form of drops of gelly.

THE excellency of this *balneum Mariæ* is confirmed by this experiment: for seeing harts-horn itself can be boiled by the help thereof, without the mixture of water, there is no doubt, but all fresh water, which is wont to be spent in ships to boil flesh, may be preserved for other uses of the mariners. Furthermore, if we add what we have tried about the preservation of raw flesh, and after of that, which

which is boiled, (See *Exper.* III) doubtless we may conceive great hope, that many inconveniences, which are wont to prejudice mariners, both by reason of the saltness of their meat, and the putrefaction of their water, will be almost wholly remedied and prevented. Neither let any man object, that so many vessels, and so exactly stopped, are very difficult to be procured; for daily experience doth evince, that very many mechanical instruments, far more difficult, may, in a little time, become very easy for use, and as easily procurable.

A LETTER *of Mr.* BOYLE'*s to the learned Dr.* JOHN BEAL, *Fellow of the Royal Society, concerning Fresh Water made out of Sea Water, printed at the desire of the patentees, in a tract intitled,* Salt Water sweetned, *by* R. FITZGERALD, *London* 1683, *in Quarto.*

SIR,

TO give you a short account (suitable to the little time I have to do it in) of the transaction, which I suppose must have given the rise to the mention made of my name in the publick gazette, I must inform you, that one of my nearest relatives, (Captain *Fitzgerald*) and some other worthy gentlemen, having acquainted his majesty, that they had an invention for making sea water sweet and wholsome, in great quantity and with small charge, and that I had examined, and did approve the water so prepared; his majesty was pleased, with very gracious expressions, to command me to attend him with a further and more particular information. Having readily obeyed this order, and been made acquainted with the objections the king thought fit to make against the practicableness of the invention, which (though a private man had urged them) I should think the most judicious, that have been framed against it; I humbly represented to him, that I looked upon this invention as comprising two different things; a *mechanical* part, which related to the engine itself, and the use of it a ship-board, and a *physical* part, which concerns the potableness, and wholesomeness of the liquor. About the former of these I did not pretend to clear the difficulties, especially such strong ones as his majesty had proposed, but left it to the patentees to give him satisfaction, which they were in a readiness to offer: but as to the wholsomeness of the prepared water, I had made some trials upon that liquor, which gave me no just grounds of suspecting it to be unwholsome, but several motives to believe it well conditioned, and of great use to navigators, and not to them only. And having hereupon briefly acquainted his majesty with the chief trials I had made to examine this sweetned water, he was pleased to look upon them as satisfactory; and vouchsafed, on that occasion, to discourse as a virtuoso of the sea, and brackish waters, and gave me some new, as well as instructive observations about them; and, in conclusion, dismissed the patentees with a gracious promise of his royal protection and peculiar favour.

To this short narrative it now remains, that I briefly subjoin the chief things, that persuaded me of the salubrity of this water, whence may be justly and easily inferred the utility the publick may receive by a cheap and easy way of preparing it. First then, I considered, that almost all the rain water, that falls from the clouds on the main

main ocean, and which (except perhaps in very few places in torrid climates) is unqueſtionably received as wholſome, muſt be afforded by the ſea, and conſequently be but ſea water, freed from its ſalt; (according to the famous motto, *redit agmine dulci.*) Next I found, as his majeſty himſelf had done, that the liquor was well taſted, and without any ſenſible brackiſhneſs; and ſo ſome of it continued for between four and five months in a large chryſtal bottle, that I purpoſely kept unſtopped, and for the moſt part in a ſouth window, where it neither did, nor probably in a long time will, putrify, nor ſo much as appear troubled, or leſs tranſparent; during which time it was, with approbation, taſted and ſmelled by ſeveral learned phyſicians of the famous college of *London*. Thirdly, I found it laver very well, which moſt pump waters, and many others, that have ſome little (though unperceived) common ſalt in them, will not do. Fourthly, this water will boil peaſe tender, which, amongſt ſeamen, is one of the principal ſigns of good water. Fifthly, in very good balances, with an inſtrument that I purpoſely cauſed to be made for the nice weighing of liquors, I found this water far leſs heavy than one would expect; for if it differed at all in weight from the like quantity of undiſtilled water, (I ſpeak with an *if*, becauſe it is far more difficult to be exact in ſuch nice trials, than the unpractiſed will imagine) the difference was not conſiderable, being but between the 400 and 500th part of the weight of the common water; and that difference is very ſmall, in compariſon of that, which navigators and learned authors relate to be obſervable in natural waters, all of them good and potable. I might tell you on this occaſion, that the laſt great duke of *Tuſcany*, who was an eminent virtuoſo, and the patron of the celebrated academy of Lyncean philoſophers, is affirmed, among other prudent courſes, that he took for his health, whereof he was very ſollicitous, to have conſtantly made uſe of diſtilled water for his own drinking. And I could add other things, favourable enough to the patentees water, if haſte, and perhaps diſcretion too, did not oblige me to leave them yet unmentioned, that I may now have time to ſay ſomewhat of the main thing of all, that convinced me of the ſaltleſſneſs of the water I ſpeak of. I conſidered then, ſixthly, that the thing, that was aimed at by thoſe ingenious men, that, at differing times and in ſeveral countries, have attempted to make ſea water ſweet; and the thing, that was required, by propoſing recompences, or otherwiſe to encourage the makers of ſuch attempts, was to free the ſea water from brackiſhneſs, without any noxious additament: ſo that on all ſides it was taken for granted, that the only thing, that kept the ſea water from being ſafely potable, was its brackiſhneſs; from which reflection it was natural for me to infer a concluſion very favourable to our prepared water. For having long ſince written a ſhort diſcourſe of the ſaltneſs of the ſea, I had been induſtrious to deviſe ways of comparing water in point of brackiſhneſs: and by theſe I found the patentees water to be more free from common ſalt, than waters, that are uſually drank here in *London* of which I remember I ſhewed thoſe gentlemen an experiment, that ſurpriſed as well as convinced them; and that, which more ſatisfied me myſelf, was a trial, that I carefully made by a way, which having mentioned, but not yet, for want of opportunity, diſcloſed to his majeſty, the reſpect I owe him forbids me to impart it without his leave: on which account, I hope, you will be content to be at preſent aſſured of theſe two things; one, that, by this way of trial, I found what poſſibly you will think ſtrange, that if there were in water ſo much as one grain of ſalt in above two ounces of water, I could readily diſcover it; and the other, that even by this critical examen, I could not detect ſo much as a thouſandth part of ſalt in our prepared water; whereas I found, by trials purpoſely and carefully made, that our Engliſh ſea water contained a 44th or 45th part of good dry ſalt, or which is all one, that 44 pints, or near ſo many pounds of marine water, would yield about one pound of dry common ſalt.

THUS,

THUS, Sir, you have a ſhort and artleſs account, ſuch as my haſte will permit, and the nature of the ſubject requires, of my part in promoting this profitable invention, to which I own myſelf a great well-wiſher, not out of any private intereſt, (though that were obligingly proffered me by the patentees) but as I think the bringing it into general uſe may prove a real ſervice to mankind, upon the ſcore of divers utilities and advantages, which yet, though I had leiſure, I ſhould think very needleſs to enumerate to ſo diſcerning a perſon as Dr. BEAL; to whom I ſhall therefore haſten to ſubſcribe myſelf

An affectionate friend and ſervant,

R. BOYLE.

MEMOIRS for the NATURAL HISTORY of HUMAN BLOOD, eſpecially the Spirit of that Liquor. With an APPENDIX.

The PREFACE introductory, addreſſed to the very ingenious and learned Dr. *J. L.*

I WILLINGLY acknowledge, that divers phyſicians have amply and learnedly, and ſome of them very eloquently, ſet forth the praiſes of the blood, and manifeſted how noble and excellent a liquor it is. But I muſt beg their pardon, if I doubt, whether their writings have not better celebrated its praiſes, than diſcovered to us its nature. For, though the laudable curioſity of the moderns has acquainted us with ſeveral things not delivered to us by the antients; yet, if I miſtake not, what is generally known of human blood, is as yet imperfect enough, and conſiſts much more of obſervations than experiments; being ſuggeſted far more by the phænomena, that nature herſelf has afforded phyſicians, than by trials induſtriouſly made to find what ſhe will not, unſollicited by art, diſcover. I will not be ſo raſh as to ſay, that to mind (as too many anatomiſts have done) the ſolid parts of the body, and overlook enquiries into the fluids, and eſpecially the blood, were little leſs improper in a phyſician, than it would be in a vintner to be very ſollicitous about the ſtructure of his caſk, and neglect the conſideration of the wine contained in it. But though I will not make ſo bold a compariſon, yet, when I conſider how important a part of the human body the blood is; and that, as when it is well conſtituted and does orderly move, it conveys nouriſhment, and vigour, and motion, and in a word health, to the reſt of the living engine; ſo the maſs of blood being either vitiated, or (which is very often the effect of that depravation) diſorderly moved, is the ſeat of divers, and the cauſe of moſt diſeaſes, whoſe cure conſequently depends mainly upon the rectifying of the blood: when I ſay, I conſider theſe things, I cannot but think it an omiſſion, that ſo important a ſubject has not been more ſkilfully and induſtriouſly enquired into. But, I hope, you were not in earneſt, when you ſollicited me to repair that omiſſion; for, you know I have not the vanity to pretend to be a phyſician; and being none, I muſt want both the ſkill and many opportunities, wherewith a man, that were profeſſedly ſo, would be advantaged. And though I deny not, that many years ago I propounded to ſome ingenious phyſicians a hiſtory of the fluid parts of the body, ſuch as the humours and other juices, and alſo the ſpirits of it, and did particularly draw up a ſet of enquiries, and make divers experiments in reference to the blood; yet those papers being since lost, and a long tract of time, and studies of a quite other nature, having made me lose the memory of most of the particulars, I find myself unable to contribute any thing considerable to your laudable design. And as all the search your commands obliged me to make after my papers, has hitherto proved fruitless, so they having been written, when I had more health, vigour, and leisure, than I now have, and when my thoughts were much more conversant with medicinal subjects; any thing, that I shall now present you about the blood, will not only be extremely short of what ought to be said, but will also be short even of what, if I mistake not, I did say of it. But yet all this is said, not to excuse me from obeying you at all, but to excuse me for obeying you so unskilfully. For, since you will have me set down what I can retrieve about human blood, you shall receive it in the following paper, which consists of four parts: the first whereof contains a set of titles (which I call of the first order, for reasons to be given you in the advertisements about them) towards the natural and medical history of human blood, which may direct those, that want better guides, what enquiries to make, and to what heads to refer what they have found by observation or trial. But because this part contains but bare titles (whose system, yet, perhaps I look upon as likely to make the usefullest part of the ensuing papers) and because I have neither leisure nor materials to answer all or most of the titles; I thought fit, in a couple of subjects, namely, the serum of human blood, which is a natural, and the spirit, which is a factitious part of it, to give some instances of what I had thoughts to do on others; and propose some example to those, that may be more unpractised in drawing up natural histories, than the general design and course of my studies of natural things permitted me to be: and what is said on these two subjects, makes the third and fourth part of these papers. As for the other titles (of the history of the blood) I contented myself, in compliance with my haste, to set down what occurred to me in the casual order, wherein they offered themselves; without scrupling to mingle here and there, among the historical notes, some experiments, that I formerly but designed as trials, that might prove luciferous, whatever the event should be. This rhapsody of my own observations makes one of the four parts, and the second in order, of what your commands embolden me to offer you at this time. And I shall be very glad to be so happy as to find, that by doing a thing, that I am wont to do so delightfully as to obey you, I have, by breaking the ice, contributed something to so noble and useful a work as the history of human blood. About which, that I may not make the porch much too great for the building, I shall add to this preamble nothing but these two advertisements; of which the first shall be, that it is not my design in these papers, to treat of my subject, as it may be considered (to borrow a school phrase) *in fieri*, which would have obliged me to trace the progress from the reception of aliments at the mouth, to the full elaboration, which were to write the history of sanguification as well as that of blood; but to treat of this liquor, as it is compleatly elaborated, and that too, not as it is formed in the vessels of a living body, but as it is extravasated, and let out by the lancet; such blood alone being that, on which I had some opportunity to make trials. And to this first advertisement I shall subjoin as the second, that in the following papers I have, as the title intimates, treated but of such human blood, as was taken from sound persons; both because, being no professed physician, I had not the opportunities of examining that of sick persons molested with particular diseases, (which yet would much conduce to a complete history of the blood;) and because the knowledge of the nature of the blood, when it is rightly conditioned, is necessary to those, that would discern, in what particulars, and how far it deviates in the sick, according to that generally received axiom,

Rectum

Rectum est index sui & obliqui: on which account the ſcheme of titles drawn up for the hiſtory of healthy blood may ſerve for a direction to any, that would write the hiſtory of morbid or depraved blood in any particular diſeaſe, as a pleuriſy, a quartan ague, the dropſy, the ſcurvy, *&c.* For having compared the qualities and accidents of this vitiated blood, with thoſe of the blood of ſound men delivered in the fore-mentioned ſyſtem of titles, it will not be difficult for a phyſician to find, to what heads he is to refer thoſe things, that conſiderably recede from ſuch as belong to healthy blood. And theſe receſſions or depravations, with perhaps a few additions of ſome peculiarities, if any occur, will make up the hiſtory of the blood, as it is wont to be vitiated in that particular diſeaſe; one general admonition ſufficing (if that itſelf be not unneceſſary) to make the reader take notice, that in all other points the blood of perſons ſick of that diſeaſe is not unlike that of thoſe, that are healthy.

The NATURAL HISTORY of HUMAN BLOOD.

PART I.

Containing a liſt of titles for the hiſtory of human blood. To which are premiſed ſome advertiſements about them.

THAT the ſcope and meaning of the enſuing ſcheme of titles, (and divers others, that I drew up for differing ſubjects) may be the more clearly underſtood, I muſt lay down in this place ſome paſſages borrowed from the (unpubliſhed) eſſay or letter I wrote to Mr. *Oldenburg*, ſecretary to the Royal Society, about the way of compiling a natural hiſtory. I propoſed then in that tract three ſorts of heads, to which the particulars, that might occur, and properly belong to the hiſtory of the ſubject to be treated of, whether a body, or a quality, or an operation, or a proceſs, (that is, a progreſſive change) might conveniently be referred. Theſe diſtinct ſets of topics or enquiries I call orders, ranks, or claſſes; and becauſe to each of theſe ſets it was found by experience, that things of ſomewhat differing nature were to be referred, as queries more properly ſo called, propoſitions either affirmative or negative, and other heads of natural hiſtory, that are leſs fit to be reduced to either of the two former ſorts than to be looked upon as ſubjects to be treated of; for this reaſon, I ſay, among others, I thought fit to compriſe all theſe ſorts of particular topics, or articles of inquiry (to uſe our illuſtrious *Verulam*'s phraſe) under the general and comprehenſive name of titles.

THE firſt order or claſſis of theſe titles, I would have to conſiſt of ſuch, as occurred readily enough to my thoughts upon the firſt deliberate view, or general ſurvey, of the ſubject to be treated of. For it is ſcarce to be expected, that at the firſt attempt a man ſhould be ſo clear ſighted, or ſo happy, as to pitch, or light upon as direct and compendious ways of indagation, and as good methods of digeſting and delivering what is diſcovered, as when a ſtudious enquiry has furniſhed him with better informations about the ſubject he is to treat of: and therefore it may ſuffice for the firſt time, that the mind do, as it were, walk round the object it is to contemplate, and view it on every ſide, obſerving what differing proſpects it will that way afford, (as when a painter, or an anatomiſt, looks upon a man's body, firſt when the face and belly are towards him, then when the back and other hinder parts are ſo) and that it takes notice of the limits and boundaries of it, and of the moſt eſſential and conſiderable parts or other things that belong to it. Wherefore in the firſt claſſis of titles, one need not be too scrupulous about the enumerating and marshalling the particulars referable to it, but may be more sollicitous, that the titles should be various and comprehensive enough, than that they should be nicely methodical; and much less than that they should be accommodated to any particular hypothesis. And because, even at the first deliberate view, some (though perhaps very few) of these titles may appear considerable and fertile enough to deserve, that there should to each of them be referred two or a greater number of subordinate and more particular topics; I thought fit, for method's sake, to call the capital titles, that is, those of the first classis, primary titles, and the subordinate, secondary ones. [Of which distinction a notable instance will be met with in what is hereafter delivered, about the spirit of human blood; all the sixteen titles, together with the appendix, contained in that epistolary discourse, being secondary titles referrible to that primary one, which is the eighteenth in the first classis of the history of human blood.]

When by reading, conference, meditation, and (which is here mainly to be considered) the trials suggested in the topics of the first classis or order, the naturalist has received the best and fullest information he can procure of all, that belongs to the subject he is to treat of, he may then proceed to frame another set of titles, which may be called the second; or (if no other interposed) the last order or classis of them, which, if he have been diligent and any thing prosperous, will be much more copious and better ranged than the first.

For now divers things will in likelihood appear to belong to the subject of the history, which were not at first taken notice of to do so, yea, perhaps were not at all thought of; and the further discovery made of the nature of the thing treated of may direct the historian to range his topics, or titles, in a better order, and more natural method, than those of the first classis. And, which is a thing of far greater moment, divers, and perhaps most, of the particular titles will appear to be of greater extent, or more comprehensive, than they were formerly conceived to be: so that a particular title may well be thought fit to be branched into many subordinate topics, or articles (which we lately called secondary titles) some one of which may perhaps comprise as many experiments, or observations, as it was at first guessed would appertain to the prime or more general title itself. And from the materials orderly drawn together under this last set or classis of titles, with some requisite changes in point of method and connexions, and some additional things, as transitions, &c. by the help, I say, of such alterations and additions, the particulars whereof the last order or classis consists, may be digested and framed into an inchoate natural history of the subject they have relation to; I said, an inchoate history, to intimate, that even after all that has been already done, I think it too probable, that the history will hereafter appear to have been rather begun then compleated; the nature of things, and the industry of skilful men, being so very fertile, that the knowledge of the subject of the history will from time to time be increased; and so the history itself may be enlarged and corrected, but will not, I fear, in many ages, if ever it be at all, be made absolutely perfect.

And on this occasion I must add, that when the subject to be treated of is very comprehensive or very difficult, as the generation of living creatures, magnetism, fermentation, gravity, &c. it may be very useful, if not almost necessary, to interpose, between the titles of the last and those of the first classis, a set of titles, that may be called of the middle order, or classis. For the framing whereof, the historian is considerately and narrowly to resurvey the nature of the subject, and make a heedful collation of that, and of the several notices attained by his endeavours to furnish the differing titles of the first classis with a competent number of particulars. For, by this collation, there will in all likelihood be suggested to him many new topics of enquiry, and

and hints, which, added to the former, will deferve to have a new claffis framed confifting of articles more copious and various than the firft, and fit to be ranged in another order. It may perhaps illuftrate what I have been faying, and am going to fay, about the feveral claffis of titles, if on this occafion I fhall add, that a natural fubject being propofed to be hiftorically treated of, there may occur fomething like what happened to the *Ifraelities* in reference to the land of *Canaan.* For at their firft entrance into it, *Jofhua* and the other fpies took a tranfient view of the country, and could bring back but an unaccurate account of it, together with a little of the moft remarkable fruit: but upon a fecond expedition, the fpies were furnifhed with fuller inftructions, and ordered to direct their refearches to the anfwering of a great many particular articles of enquiry; their induftry to anfwer which produced, in methodical tables or fchemes, a far more copious and diftinct chorography and furvey of the fruitful land of *Canaan.*

It is fcarce to be expected, that at the very firft time the titles, whether primary or fecondary, of a natural hiftory fhould be made fo comprehenfive, and be fo fkilfully bounded, as not to need to be either enlarged or reformed by fecond thoughts, and a further progrefs in the practical knowledge of the fubject treated of. I therefore thought it neceffary, or at leaft ufeful, to fubjoin to the firft edition of the titles of each of the natural hiftories I delineated, a mantiffa or appendix, that fhould confift of two forts of particulars, *viz.* Paralipomena and other Addenda; whereof the firft fhould contain fuch things as may be properly referrible to fome one or more of the titles, (either primary or fecondary,) diftinctly enumerated in the fcheme of the hiftory, and were but by hafte or overfight kept from having place among them: the other confifts of new particulars, that, after the hiftory was written, were fuggefted by further difcoveries; whether thefe particulars did directly belong to any of the preceding titles, or might only in a general way contribute fomewhat to the knowledge, or illuftration of the fubject.

TITLES of the firft ORDER.

For the natural hiftory of human blood of healthy men.

1. OF the colours of human blood arterial and venal.
2. Of the tafte of human blood.
3. Of the odours of human blood.
4. Of the heat of frefhly emitted human blood.
5. Of the inflammability, and fome other qualities of human blood.
6. Of the aerial particles naturally mixed with human blood, and alfo found in its diftinct parts.
7. Of the fpecific gravity of human blood entire.
8. Of the fpecific gravity of the two obvious parts of human blood, the red (and fibrous) and the ferous.
9. Of the confiftence of entire human blood.
10. Of the difpofition of human blood to concretion, and the time wherein it is performed.
11. Of the liquors and falts, that coagulate human blood.
12. Of the liquors and falts, that impede or diffolve its coagulation.
13. Of the liquors, &c. that preferve human blood.
14. Of the mixtures, that human blood may admit from aliments.
15. Of the fpontaneous or natural analyfis of human blood into a ferous and a fibrous part.

16. OF the respective quantities of the serous and fibrous part of human blood.
17. OF the differences between the serous and the red part of human blood.
18. OF the artificial or chymical analysis of human blood, and first of its spirit.
19. OF the volatile salt of human blood, and its figures.
20. OF the phlegm of distilled human blood.
21. OF the two oils of human blood.
22. OF the fixt salt of human blood.
23. OF the *Terra Damnata* of human blood.
24. OF the proportion of the differing substances chymically obtained from human blood.
25. OF the fermentation or putrefaction of human blood, and its phænomena.
26. OF the mechanical uses of human blood, as in husbandry, *&c.*
27. OF the chymical uses of human blood.
28. OF the medicinal uses of human blood.
29. OF the difference between human blood, as it is found in persons differingly constituted and circumstantiated, as men, women, (when menstruous, and when not) children, Moors, *&c.*
30. OF the affinity and difference between the blood of men, and that of divers other animals, as quadrupeds, birds, fishes, and sanguineous insects.

An APPENDIX, *containing*,

1. PARALIPOMENA, relating to the history of human blood.
2. MISCELLANEOUS observations, experiments and enquiries about human blood (to be added to the history of it.)

I do not think it unlikely, that some of the titles of our intended history of blood, and a greater number of the particulars, that you will meet with in it, may seem frivolous to you at the first perusal. But, perhaps, in process of time, these very things will not appear impertinent, nor be found useless: for it is a matter, as of more difficulty, so of more utility, than men are wont at first to discern, to find out, and bring into a narrow compass, a considerable number of particulars relating to one subject, and present them, as it were, at one view to the intellect to act upon. And there is many a particular experiment or observation, which, upon the first, or perhaps the second reading, may seem but slight or superfluous, which afterwards is found capable of being made good use of by those, who seriously intend, and endeavour to attain, not a maimed or a superficial, but a deep and solid knowledge of the subject of their enquiry. And to such indagators many particulars, that at first were past by unregarded, because there appeared no direct use or obvious application of them, will be found serviceable to hint new hypotheses or theories, or to illustrate them; to examine those of others, and, if they be true, to confirm them, and, if erroneous, to confute them. For, to be short, the knowledge of matters of fact cannot but be some way or other, and probably more ways than one, serviceable to a naturalist, that has sagacity and judgment to make a right use of them.

HAVING already advertised you, that the following papers treat of none but extravasated blood, since I had no other at command, to make my trials upon; I presume you will not wonder, that you find not in the scheme of titles such as these.

OF the process of sanguification, or the series of changes, that the aliment successively undergoes, from its being taken in at the mouth, till it be turned into blood.

OF the motions of the mass of blood, and particularly its circulation.

OF the chyle, lympha, and other liquors, that are supposed to enter and mingle with the blood.

WHETHER the humours, phlegm, gall, and melancholy, be really contained in the blood, as conftituent parts of it.

WHETHER fome other fubftances may not with as much reafon be admitted into the compofition of the blood.

THESE, as I was faying, and perhaps fome other titles fhould have been added, if my defign had reached further than to treat of blood feparated from the body; and I wifh that you, who by your abilities and profeffion are far better qualified than I for fuch a work, would fill up thefe titles, and add to them, fome as preliminaries, and others as appendices, to the hiftory of blood I have adventured to begin.

PERHAPS it may not be altogether impertinent to add, that I had once fome thoughts of a defignation of a natural hiftory of other liquors of a human body, as well as the blood; I mean fuch as the gall, the lympha, the *Succus Pancreaticus*, fpittle, urine, milk, *&c.* But I quickly perceived it was fit for me to refign fuch tafks to phyficians; only I fhall here fubjoin, as a fmall fpecimen, a fet of titles for the hiftory of urine, which, though, by reafon of its affinity in many regards to blood, it muft have many titles in common with it, yet fome will be differing according to the nature of the fubject; which (liquor) I therefore pitch upon, becaufe I dare own to you, and I do it not without premeditation, and having wrought on urine longer than on blood itfelf, that I think urine to be a liquor, which, as much defpifed as it is by others, deferves to be follicitoufly enquired into by phyficians, naturalifts, and upon fpecial accounts by chymifts; who will perhaps be excited to feek and hope for great matters, both for medicine and alchymy, from this liquor fkilfully handled, when they confider that the phofphorus, of which I have elfewhere related fo many new, and fome of them furprizing, phænomena, is made, at leaft according to my way, of meer urine by a fimple diftillation.

TITLES of the firft CLASSIS.

For the natural hiftory of human urine emitted by healthy men.

1. OF the colours of human urine.
2. OF the tafte of human urine.
3. OF the odours of human urine frefh and putrified.
4. OF the heat and cold of human urine.
5. OF the fpecific gravity of human urine.
6. OF the confiftence of human urine, as to denfity, vifcofity, *&c.*
7. OF the aerial particles contained in human urine.
8. WHETHER human urine is a fit liquor for fermentation properly fo called.
9. OF the differences between frefh and ftale human urine.
10. OF the fermentation or putrefaction of human urine, and the time it requires.
11. OF the fpontaneous feparation of parts in human urine.
12. OF the vulgar analyfis of human urine by diftillation.
13. OF fome other ways of diftilling human urine.
14. OF the proportion of the principles, or ingredients of human urine.
15. OF the fpirits of human urine.
16. OF the phlegm of human urine.
17. OF the volatile falt of human urine.
18. OF the fixt falt of human urine.
19. OF the compounded falt of human urine.
20. OF the fhining fubftances obtainable from human urine.
21. OF the falt, that is predominant in human urine.

22. OF

16. Of the respective quantities of the serous and fibrous part of human blood.

17. Of the differences between the serous and the red part of human blood.

18. Of the artificial or chymical analysis of human blood, and first of its spirit.

19. Of the volatile salt of human blood, and its figures.

20. Of the phlegm of distilled human blood.

21. Of the two oils of human blood.

22. Of the fixt salt of human blood.

23. Of the *Terra Damnata* of human blood.

24. Of the proportion of the differing substances chymically obtained from human blood.

25. Of the fermentation or putrefaction of human blood, and its phænomena.

26. Of the mechanical uses of human blood, as in husbandry, *&c.*

27. Of the chymical uses of human blood.

28. Of the medicinal uses of human blood.

29. Of the difference between human blood, as it is found in persons differingly constituted and circumstantiated, as men, women, (when menstruous, and when not) children, Moors, *&c.*

30. Of the affinity and difference between the blood of men, and that of divers other animals, as quadrupeds, birds, fishes, and sanguineous insects.

An APPENDIX, *containing*,

1. Paralipomena, relating to the history of human blood.

2. Miscellaneous observations, experiments and enquiries about human blood (to be added to the history of it.)

I do not think it unlikely, that some of the titles of our intended history of blood, and a greater number of the particulars, that you will meet with in it, may seem frivolous to you at the first perusal. But, perhaps, in process of time, these very things will not appear impertinent, nor be found useless: for it is a matter, as of more difficulty, so of more utility, than men are wont at first to discern, to find out, and bring into a narrow compass, a considerable number of particulars relating to one subject, and present them, as it were, at one view to the intellect to act upon. And there is many a particular experiment or observation, which, upon the first, or perhaps the second reading, may seem but slight or superfluous, which afterwards is found capable of being made good use of by those, who seriously intend, and endeavour to attain, not a maimed or a superficial, but a deep and solid knowledge of the subject of their enquiry. And to such indagators many particulars, that at first were past by unregarded, because there appeared no direct use or obvious application of them, will be found serviceable to hint new hypotheses or theories, or to illustrate them; to examine those of others, and, if they be true, to confirm them, and, if erroneous, to confute them. For, to be short, the knowledge of matters of fact cannot but be some way or other, and probably more ways than one, serviceable to a naturalist, that has sagacity and judgment to make a right use of them.

Having already advertised you, that the following papers treat of none but extravasated blood, since I had no other at command, to make my trials upon; I presume you will not wonder, that you find not in the scheme of titles such as these.

Of the process of sanguification, or the series of changes, that the aliment successively undergoes, from its being taken in at the mouth, till it be turned into blood.

Of the motions of the mass of blood, and particularly its circulation.

Of the chyle, lympha, and other liquors, that are supposed to enter and mingle with the blood.

 WHETHER

WHETHER the humours, phlegm, gall, and melancholy, be really contained in the blood, as conſtituent parts of it.

WHETHER ſome other ſubſtances may not with as much reaſon be admitted into the compoſition of the blood.

THESE, as I was ſaying, and perhaps ſome other titles ſhould have been added, if my deſign had reached further than to treat of blood ſeparated from the body; and I wiſh that you, who by your abilities and profeſſion are far better qualified than I for ſuch a work, would fill up theſe titles, and add to them, ſome as preliminaries, and others as appendices, to the hiſtory of blood I have adventured to begin.

PERHAPS it may not be altogether impertinent to add, that I had once ſome thoughts of a deſignation of a natural hiſtory of other liquors of a human body, as well as the blood; I mean ſuch as the gall, the lympha, the *Succus Pancreaticus*, ſpittle, urine, milk, *&c.* But I quickly perceived it was fit for me to reſign ſuch taſks to phyſicians; only I ſhall here ſubjoin, as a ſmall ſpecimen, a ſet of titles for the hiſtory of urine, which, though, by reaſon of its affinity in many regards to blood, it muſt have many titles in common with it, yet ſome will be differing according to the nature of the ſubject; which (liquor) I therefore pitch upon, becauſe I dare own to you, and I do it not without premeditation, and having wrought on urine longer than on blood itſelf, that I think urine to be a liquor, which, as much deſpiſed as it is by others, deſerves to be ſollicitouſly enquired into by phyſicians, naturaliſts, and upon ſpecial accounts by chymiſts; who will perhaps be excited to ſeek and hope for great matters, both for medicine and alchymy, from this liquor ſkilfully handled, when they conſider that the phoſphorus, of which I have elſewhere related ſo many new, and ſome of them ſurprizing, phænomena, is made, at leaſt according to my way, of meer urine by a ſimple diſtillation.

TITLES of the firſt CLASSIS.

For the natural hiſtory of human urine emitted by healthy men.

1. OF the colours of human urine.
2. OF the taſte of human urine.
3. OF the odours of human urine freſh and putrified.
4. OF the heat and cold of human urine.
5. OF the ſpecific gravity of human urine.
6. OF the conſiſtence of human urine, as to denſity, viſcoſity, *&c.*
7. OF the aerial particles contained in human urine.
8. WHETHER human urine is a fit liquor for fermentation properly ſo called.
9. OF the differences between freſh and ſtale human urine.
10. OF the fermentation or putrefaction of human urine, and the time it requires.
11. OF the ſpontaneous ſeparation of parts in human urine.
12. OF the vulgar analyſis of human urine by diſtillation.
13. OF ſome other ways of diſtilling human urine.
14. OF the proportion of the principles, or ingredients of human urine.
15. OF the ſpirits of human urine.
16. OF the phlegm of human urine.
17. OF the volatile ſalt of human urine.
18. OF the fixt ſalt of human urine.
19. OF the compounded ſalt of human urine.
20. OF the ſhining ſubſtances obtainable from human urine.
21. OF the ſalt, that is predominant in human urine.

22. OF

22. Of the empyreumatical oil, or oils of urine.

23. Of the mellago, or rob of human urine, and its uſes.

24. Of the *Terra Damnata* of human urine.

25. Of ſome accidental differences of human urine, as it is emitted in the morning, or at certain diſtances from meat, or after the uſe of certain aliments, or medicaments, as ſparagus, turpentine, &c. or at differing ſeaſons of the year, as winter, ſummer, &c.

26. Of the affinity of human urine with divers other bodies, eſpecially vegetable and mineral.

27. Of the hoſtility of human urine with acid, &c.

28. Of the affinity and difference between urine, blood, gall, milk, &c. and divers other liquors, or juices belonging to the animal kingdom, particularly of the compariſon between human urine and that of beaſts.

29. Of the mechanical uſes of human urine.

30. Of the chymical uſes of human urine, and its parts, eſpecially as menſtruums.

31. Of the medicinal uſes of human urine, external and internal.

An APPENDIX, *containing*

1. Paralipomena relating to the hiſtory of human urine.

2. Promiscuous obſervations, experiments, and inquiries about human urine (to be added to the Hiſtory of it.)

PART II.

Containing miſcellaneous experiments and obſervations about human blood.

IF I were furniſhed with all the former experiments, obſervations, and papers, that at ſeveral times I made and wrote about human blood, or were ſupplied with materials and opportunities to repair the want of them, (as poſſibly, God aſſiſting, I may hereafter be,) this ſecond part of our work would perhaps appear much leſs maimed, and jejune, than it will now be found. But I am ſo ſenſible of the diſadvantage, that the want of thoſe requiſite helps muſt have brought to this rhapſody of unconnected notes, (written at differing times, and on differing occaſions) that I was more than once inclined totally to omit it. And it is the importance of the ſubject, upon which even mean experiments may ſometimes prove of good uſe, that keeps me from ſuppreſſing it; which I thus early give notice of, that nothing more than looſe experiments, and thoſe referrible but to ſome of the titles of the hiſtory of human blood, (divers others being left untouched) may in the ſecond part of our memoirs be expected.

To the fourth (primary) Title of the History of Human Blood.

EXPERIMENT I.

Having for ſome reaſons, that need not here be mentioned, been induced to inquire of more than one perſon, that has uſed to let many men blood, whether they did not obſerve, that ſome perſons found a manifeſt and conſiderable change in the heat of the blood, as it came to iſſue out firſt or laſt? I was anſwered affirmatively, and told, that ſeveral perſons, that had no fever, ſaid, that after their blood had run out a while, they found it come ſenſibly hotter than before; and ſome of them complained, that it came with a degree of heat, that was troubleſome, and, as they fancied, ready to ſcald them.

To the ſame Title of the HISTORY of HUMAN BLOOD.

EXPERIMENT II.

I got a chirurgeon to put a ſealed weather-glaſs, adjuſted by the ſtandard of *Greſham* college, into the porringer, wherein he was going to bleed a young gentlewoman, that, as the blood ran out of the open vein, it might fall upon the ball of the inſtrument, in which the liquor was made by the warmth to aſcend a good way, but not much (if at all) nearer, than about an inch to the ſmaller upper ball of the thermoſcope.

To the ſame TITLE.

EXPERIMENT III.

BUT, within leſs than an hour before this time, having procured a man of middle age (that ſeemed healthy enough, and was let blood in the ſame ſhop by the ſame chirurgeon) to bleed upon the ſame weather-glaſs, the tincted ſpirit of wine aſcended above all the marks belonging to the ſtem, and from the top of the ſtem expanded itſelf to a conſiderable quantity in the ſmall upper ball of the ſtem, (for the chirurgeon told me it was a fourth part of the height of the ball;) ſo that, though we could not determine, how high it would have riſen, if the ſtem had been long enough, yet it ſeemed manifeſt, that the warmth, that made it riſe, did conſiderably exceed the uſual warmth of the air in the dog-days; theſe gaged thermoſcopes being wont to be ſo framed, as to keep the liquor in the ſtem all the year long, without ſinking quite into the greater ball in winter, or aſcending into the leſſer in ſummer.

WE employed alſo, when a young woman was blooded, a ſealed thermometer, that was not gaged, but was much ſhorter than the other; and in this the tincted ſpirit was raiſed almoſt to the top, which argued no inconſiderable degree of heat.

To the ſame TITLE.

EXPERIMENT IV.

I know not, whether it may be worth while to take notice on this occaſion, that a porringer, whereinto a healthy man had been let blood, having been brought from the chirurgeon's houſe to my lodging; though the blood was already coagulated, yet when I thruſt into it the ball of the forementioned gaged thermoſcope, it appeared to have retained warmth enough to make the ſpirit of wine aſcend, by my gueſs, at leaſt three or four fingers breadth above its former ſtation.

To the fifth Title of the HISTORY.

SINCE human blood does, in diſtillation, afford a not inconſiderable quantity of oil, one may well ſuppoſe it to be a combuſtible body: but every one will not think it ſo inflammable, as, upon trial purpoſely made, I found it to be. For having taken a piece of human blood dried till it was almoſt pulverable, and held it in the flame of a candle, it would take fire, and afford a flame much like that, which excited it, burning with a crackling noiſe (much like that of ſea-ſalt caſt into the fire) and here and there melting. But the inflammableneſs of ſuch dried blood did much better appear, when putting together four or five throughly kindled coals, we laid on them a piece of dried blood of the bigneſs of a ſmall nutmeg, or thereabouts; for this yielded a copious and very yellow flame, and if it were ſeaſonably and warily blown from time to time, as the effluvia degenerated into ſmoak, it would, by theſe frequent re-accenſions continue to yield clear and yellow flames of no contemptible bigneſs (in proportion to the body that yielded them) much longer than one would expect. And during a good part of this deflagration, the blood appeared, as it were, to fry upon the coals, and in good part to melt into a black ſubſtance almoſt like pitch. There was alſo a crackling noiſe produced, like that, which chymiſts obſerve, when they decrepitate common ſalt.

THESE experiments for the ſubſtance were repeated. But I ſhewed another inſtance of the inflammableneſs of blood, that was ſomewhat ſurpriſing. For, having cauſed ſome human blood (being part of the ſame, that was made uſe of in the foregoing trials) to be ſo far dried, that it was reducible to fine powder, I took ſome of this powder, that had paſt through a fine ſearch, and caſting it through the flame of a good candle, the grains, in their quick paſſage through it, took fire, and the powder flaſhed, not without noiſe, as if it had been roſin. This experiment was reiterated with ſucceſs.

To the ſeventh Title of the HISTORY.

THE ſpecific gravity of human blood is more difficult to be determined than one would readily imagine: for the gravity of blood may differ ſenſibly in ſeveral perſons, according to their ſex, age, conſtitution, *&c.* and in the ſame perſon it may be varied by the time of the year, and of the day, and by being drawn at a greater or leſſer diſtance from a meal, and by divers other circumſtances. But beſides all theſe things, there is a mechanical difficulty, if I may ſo call it, that attends the work we are ſpeaking of: for the blood begins to coagulate ſo ſoon after it is emitted, that it is ſcarce a practicable thing to weigh it hydroſtatically, either by immerſing into it a ſolid body heavier than itſelf, or by weighing the whole blood in water; the former way being oppoſed by the fibrous part of the blood, and the latter by the ſerum: and upon the ſame account it is ſomewhat (though not ſo much) difficult to compare with any accurateneſs, the weight of blood with that of water in a glaſs; as alſo for other reaſons, which he, that ſhall conſiderately go about to try it, will quickly find. But however, ſince it may be a thing of conſiderable uſe, to have ſome tolerable eſtimate, though not an exact one, of the difference in gravity between water and human blood, by which ſo many parts of the body, conſiſtent as well as fluid, are by various changes of texture conſtituted and nouriſhed; I ſhall ſubjoin a trial, that this conſideration invited me to make as well as I could. We took the blood of a ſound man emitted all at one time, and put the whole maſs of it, as well the ſerous as the fibrous part, into an oblong glaſs, of the fitteſt ſize and ſhape we could light on amongſt ſeveral; and having ſuffered the blood to reſt, till all was ſettled, and the many bubbles vaniſhed, we carefully marked with a diamond that narrower part of the glaſs, which the upper ſurface of the blood reached to: then we weighed the glaſs and the blood in a very good balance, and having poured out the blood (for other uſes) and waſhed the glaſs, it was filled with common water to the lately mentioned mark, and then weighed again in the ſame balance; afterwards the water being poured out, the glaſs alone was counterpoiſed in the ſame ſcales, and its weight being deducted from each of the two formerly mentioned weights, the water was found to have weighed ℥ix. ʒvi. 50 *gr.* and the blood (equal to it in bulk) to have weighed ℥x. ʒij. 4 *gr.* ſo that the difference between them being ʒiij. 14 *gr.* the blood was heavier than ſo much water, but about the 25th part (for I omit the fraction) of its own weight. But this experiment, for the reaſons above intimated, deſerves to be reiterated more than once.

To the eleventh Title of the HISTORY.

THOUGH rectified ſpirit of wine be a menſtruum conſiſting of very ſubtle parts, and upon that account be a good diſſolvent of divers vegetable ſubſtances, and, as experience

experience has aſſured me, of ſome metalline ones too, that ſeem to be more ſolid than the fibrous part of human blood; yet looking upon this body as of a very differing texture from thoſe, I thought ſpirit of wine might have a very differing operation upon it. And accordingly, having ſeparated from the ſerum a clot of blood, that was coagulated, but ſoft enough, as the fibrous part uſes to be before it is dried, I kept it for divers hours in a very well dephlegmed vinous ſpirit, from whence I afterwards took it out as hard, as if it had been well dried by the fire.

To the nineteenth Title of the HISTORY.

EXPERIMENT I.

THE volatile ſalt of human blood, as fugitive as it is, is yet ſo fuſible, that, if it be dextrouſly handled, one part of it may be brought to melt, and, as I have tried, even to boil, whilſt the reſt is flying away. The like I have tried with ſome other volatile ſalts, and I preſume the obſervation will hold in moſt, if not all of them.

To the ſame TITLE.

EXPERIMENT II.

THOUGH the volatile ſalt of human blood, when it is by ſublimation made white and clean, ſeems to be a very homogeneous ſubſtance, and according to the principles of the chymiſts ought to be ſo; yet I am apt to ſuſpect, either that its ſubſtance is not altogether ſimilar, or that the corpuſcles, that compoſe it, are of ſizes, if not alſo of ſhapes, differing enough. For having weighed out ſome grains of a reſublimed ſalt of human blood, that ſeemed very pure, the odour was ſo ſtrong and diffuſive, that one would have expected the whole ſalt, being but ſix grains, ſhould in a few hours evaporate away, eſpecially being left in a ſouth window expoſed to the air in a flat piece of glaſs. And yet ſeveral days after, if I miſtake not, ſeven or eight, I found the ſalt ſo little diminiſhed, as to its ſenſible bulk, (for I did not think fit to weigh it) that it ſeemed to have waſted but little; and yet what remained had ſcarce any odour at all, that I (whoſe organs of ſmelling are acute enough) could well perceive, notwithſtanding which, this white body retained a ſaline taſte; and a little of it being for trial's ſake put upon a ſolution of common ſublimate in fair water, readily turned it white. So that it ſeemed, that the penetrant and diffuſive odour of the volatile ſalt of blood proceeded from ſome particles, much more ſubtile and fugitive than the other parts, that compoſed it. But this experiment ought to be reiterated with differing quantities of ſalt, by which means, perhaps, a heedful obſerver may diſcover, whether the comparative fixity of the ſalt, that remains after the odorous particles are (at leaſt for the moſt part) flown away, may not ariſe from their coalition with ſome acid corpuſcles, that are wont to rove up and down in the air, and adhere to bodies diſpoſed to admit their action.

To the ſame TITLE.

EXPERIMENT III.

A dram of volatile ſalt of human blood, ſublimed in a lamp furnace, was put into as much common water, as in a narrow cylindrical glaſs ſerved to cover the whole ball of our ſtandard or gaged thermoſcope; and when, after this had ſtood a while in the water to be brought to its temper, we put in the above-mentioned ſalt, the tincted ſpirit of wine manifeſtly ſubſided about two tenth parts of an inch, and probably would have fallen lower, if there had been more water in the veſſel to make a ſeaſonable ſolution of the ſalt, whereof a conſiderable part lay undiſſolved at the bottom.

To the ſame TITLE.

EXPERIMENT IV.

WHEN we perceived the liquor to ſubſide no more, we put to it by degrees ſome ſtrong ſpirit of nitre, till it would no longer make any manifeſt conflict with the diſſolved ſalt. The event of which trial was, that the liquor in the thermoſcope began preſently to mount, and continued to do ſo as long as the conflict laſted; at the end of which, we found by meaſure, that it had aſcended more than three inches and a half above the ſtation it reſted at when the ebullition began.

To the ſame TITLE.

EXPERIMENT V.

THE figuration of the volatile ſalt of human blood may be conſidered, either in regard of the ſingle grains, or of that aggregate of them, which, when they are made to aſcend to the top of the glaſs, may be called its ſublimate. The latter of theſe may may be beſt obſerved, when the ſaline exhalations firſt aſcend and faſten themſelves to the inſide of the blind head, or other glaſs, that is ſet to receive them. For, though towards the end of the operation the corpuſcles lie ſo thick and confuſed, as to leave no diſtinct figures, yet at firſt one may often obſerve the little ſaline concretions to lie in rows, ſometimes ſtrait enough, and ſometimes more or leſs crooked, with differing coherencies and interferings; ſo that though ſometimes theſe rows of concretions may, eſpecially if a little befriended by the ſpectator's fancy, repreſent either trees, or their branches, or hartſhorn, &c. yet theſe ſeem not to be conſtant repreſentations depending upon the particular nature of human blood, but caſual figurations, that depend upon ſeveral accidental cauſes and circumſtances, ſuch as the degree of fire employed to ſublime the ſalt, the plenty or paucity of the aſcending matter, the capacity and figure of the veſſel, that receives it, beſides ſeveral others not needful to be here enumerated. Nor is the ſalt of human blood the only volatile one, among whoſe elevated concretions I have obſerved the above named circumſtances to produce diverſity of configurations. But as to ſingle grains of the volatile ſalt of blood, I diſcerned a good many of them to be finely ſhaped; but whether it were accidental or not, further trial muſt inform me. I could not, that I remember, obſerve theſe handſome figures in the concretions, that compoſed the ſublimate, that was obtained by rectifying or elevating again the ſalt, that firſt came over, but in the grains, that in the firſt diſtillation faſten themſelves to the upper part and ſides of the receiver; for of theſe divers were of conſiderable bigneſs and ſolidity: and though they were not all of the ſame ſhape, ſome of them being not unlike to cubes, others to parallelopipeds, others to octoedrons, being almoſt like grains of allum; yet moſt of them were prettily ſhaped, being comprehended by planes ſmooth, finely figured, and aptly terminating in ſolid angles, as if the concretions had been cut and poliſhed by a jeweller.

To the ſame TITLE.

EXPERIMENT VI.

THERE is another way, that I have uſed to obſerve the figures of the ſalt of blood, which was to rectify the ſpirit of blood, ſo as it may be fully ſatiated with the ſalt, whilſt

whilst the liquor (in the receiver) continued yet somewhat warm. For then, setting aside this over impregnated liquor when it came to be refrigerated (which should be done very slowly) there appeared at the bottom of the vial a good number of saline concretions of differing sizes; several of which, as far as the rest would suffer me to see them were shot into crystalline plates, very smooth, and prettily figured, having, to the best of my conjecture, their broad and parallel surfaces of a hexagonal on an octagonal figure regular enough.

To the same TITLE.

EXPERIMENT VII.

ACCORDING to the hypothesis of divers learned naturalists and physicians, I supposed it would be thought considerable to know, what would happen upon putting together the volatile salt of human blood, and the spirit of nitre, with the more fugitive parts of which salt they conceive the air to be plentifully, and some of them to be vitally impregnated.

To gratify some of these philosophers, we took a dram of dry volatile salt of blood, (which we made choice of rather than spirit, because we had a mind to know what quantity of acid salt it would retain;) and having dissolved it in some distilled water, we dropped into it good spirit of nitre, till the two liquors, though they were shaken, would no longer manifestly act upon one another: the conflict being ceased, we slowly evaporated the superfluous moisture, which steamed almost all away before the saline part would coagulate. At length it came to drines, and then the middlemost part appeared in the form of thin crystals, not unlike those of saltpetre; but the rest, which was by much the greater part of the concretion, seemed to be a confused mass without any distinct figure. This mass weighed but 12 gr. more than a drachm. So that, as far as this single experiment can inform us, the volatile salt of blood may be satiated by so little as a fifth part of its weight of the saline corpuscles of spirit of nitre. This compounded salt being laid in a window, did appear to be very prone to be resolved by the moisture of the air, or, in the chymist's phrase, to run *per deliquium*. A little of the same salt being put upon a well-kindled coal, readily melted, and seemed to boil, and towards the latter end made a noise, and afforded a flame very like common nitre, save that its colour was more yellow. The strong smell, that accompanied this deflagration, was like that, which is peculiar to spirit of nitre.

To the twenty-first Title of the HISTORY.

EXPERIMENT I.

HUMAN blood, as most of the other subjects of the animal kingdom, that I have had occasion to examine, afforded by distillation in a retort an empyreumatical and very fetid oil, whose colour was almost black; but that seemed to me to proceed only from the intense and opacous rednes of the liquor, since some portions of it being purposely looked on against the light, when they were spread very thin upon glass, appeared of a deep yellow, or of a reddish colour, as they chanced to lie more or less thick upon the glass.

EXPERIMENT II.

WHEN the blood was well dried, before it was committed to distillation, I found it to afford a greater quantity of oil, in proportion to the weight of the dry body, than was at first expected. Once out of a pound of not over-well dried blood, we had near an ounce and a half of oil; and from another parcel we had it in a far greater proportion to the quantity of blood, that afforded it.

EXPERIMENT III.

I remember, that having many years ago had the curiofity to prepare blood by a very convenient digeftion, and to rectify very carefully the diftilled liquors, that came over, with the flame of a lamp; I obtained among other things two oils of very differing colours, the one being of a yellow or pale amber colour, and the other of a deep red. But that, which furprized even ingenious fpectators, was, that though thefe oils were both of them afforded by the fame blood, and were clear and pure enough, yet they would not only fwim in diftinct maffes one over another, but if they were confounded, by being fhaken together, would little by little feparate again, as common oil and water are wont to do. Whether the difference in fpecific gravity between thefe two oils could keep them from permanently mixing, when they were mingled, as well as it kept their maffes diftinct before they were fhaken; or whether this feeming antipathy proceeded from fome particular incongruity in the textures of thefe liquors, I fhall not now ftay to difpute.

EXPERIMENT IV.

It may be of fome ufe, efpecially to thofe, that aim at making medicinal ufes of human blood; to know, that having had a fufpicion, that the oil of blood might contain or conceal divers faline particles capable of being feparated from it; we took a parcel of unrectified oil, and having put to it a convenient quantity of diftilled water (I fuppofe rain-water would have done as well, though common water would not) we diligently confounded thefe liquors by frequent agitation, that the water might rob the oil of its feparable faline corpufcles. Of which trial the event was, that after the liquors were well fettled, the water (whereof we purpofely forbore to employ too much) was found impregnated with faline corpufcles, that it had by diffolution obtained from the oil, by virtue of which it was endowed with a moderately brifk tafte, and would readily turn fyrup of violets green, and precipitate out of a folution of common fublimate a white powder, to name now no other of its refemblances to weak fpirit of human blood. And this operation I the more willingly relate on this occafion, that you may be invited to try, what the like method will do on other empyreumatical oils, as of hartfhorn, urine, *&c.* drawn from bodies; that belong to the animal kingdom.

EXPERIMENT V.

To examine a conjecture, whofe grounds I cannot ftay to fet down, we put fome unrectified oil of human blood into a concave piece of glafs; and then having dropt into it as much oil of vitriol, as might by guefs amount to a fourth or third part of the fetid oil, we ftirred them well together with a flender piece of folid glafs; by which means the mixture was made to fend up good ftore of whitifh fumes, or fmoke, and grew, as was expected, confiderably hot, it being indeed fo hot, that though it amounted not to above a fpoonful, yet I was not able, without pain and inconvenience, to hold my finger underneath that part of the glafs, that contained the incalefcent liquors.

EXPERIMENT VI.

It may be worth while to relate, that what I have elfewhere obferved about fome other empyreumatical oils, holds true in that of human blood: for having fome of this liquor unrectified, though in that ftate it appeared grofs, and dark, and muddy, yet it would readily, even in the cold, diffolve in, or mingle with highly rectified vinous ſpirits, to which it communicated a reddiſh colour deep enough, agreeably to what I formerly noted touching the colour of this oil.

To the twenty-ſecond Title of the HISTORY.

I do not remember to have met with, in any author, an account of the qualities of the fixed ſalt of human blood; and I know not whether any have had the curioſity to prepare it; whereat I do not much wonder, ſince to obtain ſo much as ℥j. of it, there is requiſite a conſiderable quantity, perhaps ſome pounds of blood; and the calcination requires ſo obſtinate a fire, that a man's patience may eaſily be tired before the operation be perfected; or by the ſmall appearance of calcination, that the *caput mortuum* will afford him after having been kept three or four hours in the fire, he may be induced to conclude, that all the ſalt of blood is volatile in a good fire, and conſequently, that it will yield no fixed ſalt.

But having by an obſtinate calcination obtained between three and four drachms of this ſalt, I found not, that it was a fixed alcaly or a lixiviate ſalt, but rather, as I expected, of the nature of common or ſea-ſalt, though not without ſome little diverſity, which diſcovered itſelf by ſome nice trials. But as to the main, our ſalt was ſcarce diſtinguiſhable from marine ſalt, for it taſted very like it; a ſtrong ſolution of it did not readily (for I was not at leiſure to wait long for the event) turn ſyrup of violets green or greeniſh, nor (which was more) precipitate a brick colour or browniſh yellow, no more than a white powder, out of ſolution of ſublimate. I alſo found by trial, that the ſpirit of ſalt did not diſſolve it as an alcaly. And to theſe ways of examining it I added three others, that I had not known uſed for ſuch a purpoſe, and which had all three of them ſuch events as were expected: for having put ſome oil of vitriol upon a little of our dry ſalt, it did immediately, as I had divers times obſerved it to do upon common ſalt, corrode it with great violence, and with much foam and ſmoke. We alſo dropped a little of our fixed ſalt, diſſolved in diſtilled water, upon a ſolution of fine ſilver made in *aqua fortis*, whereupon immediately enſued a precipitation of a copious white powder. And laſtly, for further trial, having put ſome leaf gold into *aqua fortis*, which would not (as will eaſily be believed) work upon it; whilſt it was ſwimming there without being ſo much as diſcoloured, I put a little of our powdered ſalt into the liquor, which being thereby turned into a kind of *aqua regia*, did in a trice, without the aſſiſtance of heat, totally diſſolve it.

To the twenty-third Title of the HISTORY of HUMAN BLOOD.

THERE is a far greater calcination, than one would expect, required to obtain the *caput mortuum* of human blood, which affords but very little of it. For from ℥xxiv. of dried blood, (which perhaps was but the third or fourth part in weight of the entire blood, that afforded it) we could get after two days calcination but ʒij. 9 gr. of earth. And though this were ſo carefully made, that it may very probably be ſuppoſed to deſerve the name of *terra damnata*, better then moſt ſubſtances, to which chymiſts are wont to give that appellation; yet one may ſuſpect, that this itſelf was not pure elementary earth, ſince it had a red colour, very like that of colcothar of vitriol.

To the twenty-fourth Title of the HISTORY.

THE quantities of the principles, or rather of the ſeveral differing ſubſtances. obtained by diſtillation from human blood, may ſeem eaſy, but is indeed very difficult, if at all poſſible, to be determined; not only becauſe of the ſometimes great diſparity, as to proportion, that may be met with of the fibrous, or concreted part, to the ſerum, in the blood of differing perſons, and even of the ſame perſons according to differing circumſtances; but alſo, becauſe it is more difficult to diſtill even the dried and pulverable part of blood without addition, than thoſe, that have not tried, will eaſily judge; and I doubt, that few have tried it well, becauſe I have not met with any, that take notice of the neceſſity of ſhifting the retort, to gain as much volatile ſuſtance as may be obtained, and leave as little as may be in the *caput mortuum*. For when we diſtilled a ſomewhat conſiderable quantity of dried blood, though it was warily done by an expert artiſt, yet the ſame heat, that made the lower part of the blood paſs in the form of exhalations into the receiver, made the matter ſo ſwell, that it heaved up to the upper part of the veſſel a conſiderable quantity of black matter, which an ordinary diſtiller would have taken and thrown aſide for *caput mortuum*, but which an heedful eye might eaſily diſcern to be much of the ſame nature with what it was, when it was firſt put in, though it were blackened by the aſcending fumes. Wherefore we took it out, and mixing it with the remaining ſubſtance, that was leſs remote from the nature of a true *caput mortuum*, it was again in another retort committed to diſtillation, whereby we obtained more oil, *&c.* And perceiving, that even this ſeeming *caput mortuum* had at the top of it a pretty deal of matter, that I did not think ſufficiently diſpirited, if I may ſo ſpeak, I cauſed it to be taken out, and diſtilled in a freſh retort, in which it afforded a not contemptible quantity of volatile matter.

HAVING thus prepared you not to expect any thing of accurateneſs, in the determination of the quantities of the differing ſubſtances obtainable even from dried human blood; that I may aſſiſt you to make ſome gueſs at it, that may approach ſomewhat near the truth, I will inform you, that having thus in three retorts diſtilled 24 ounces of dried human blood, we obtained of volatile ſubſtances, I mean ſpirit, together with a little phlegm, white ſalt, and very high coloured oil, ℥xiij. and a drachm, beſides ſeveral parcels of thick oil, that ſtuck to the retorts and the receiver, which we eſtimated at ſeven drachms more. So that the whole volatile part amounted to fourteen ounces, of which we found the oil to be about ℥iij. + ʒvj. And the clear liquor (which, though probably not without ſome phlegm, may deſerve the name of ſpirit, becauſe it was fully ſatiated with ſaline and ſpirituous parts) to be ℥vi + ʒiijſs, beſides the volatile ſalt, which, when the ſpirit was drained from it, appeared white, but wet: for which reaſon it was not poſſible to determine exactly, neither how much liquor it yet retained, nor conſequently how much itſelf weighed; but you may gueſs pretty near the truth, when I ſhall have told you, that having carefully ſublimed the ſalt, there remained in the glaſs ʒij and about five grains of phlegmatic liquor, which was not judged devoid of ſalt, though it could not by that operation be ſeparated; and of volatile ſalt in a dry form we obtained ℥j + ʒijſs. The *caput mortuum* amounted to ℥viijſs, and ſomewhat better, which being calcined for two days together, afforded not white, but only browniſh red aſhes, whence we obtained ʒvij and a quarter of white and fixed, but not truly lixiviate, ſalt; and (as was lately noted to another purpoſe, under the next foregoing title) ʒij, and nine grains of earth. In this troubleſome experiment there occurred ſo many neceſſary operations, in each of which we could ſcarce poſſibly avoid loſing ſome, and now and then a conſiderable portion of the matters we handled, that if you had been preſent at the trials, perhaps you would not think it ſtrange, that I ſhould write, (as I did a little above) that I think it a very difficult thing in practice, to determine exactly the proportions of the differing ſubſtances, that may be chymically obtained, by vulgarly known operations, from a propoſed parcel of human blood; eſpecially ſince I think, that it is without ſufficient grounds, that chymiſts do univerſally take it for granted, that in diſtillations carefully made, the matter, that paſſes into the receiver, or at leaſt aſcends, together with the remains, or *caput mortuum*, amount to juſt the weight, that

Of this ſee the notes referred to the 22d title.

hat the entire body had before diſtillation: which paradox I endeavour to make highly probable, if not certain, in another paper, that belongs not to the preſent collection.

PART III.

Containing promiſcuous experiments and obſervations about the ſerum of healthy man's blood;

(Whereof the firſt may be referred to the ſixteenth, and moſt of the reſt to the ſeventeenth of the titles of the firſt order.)

SINCE the diviſion, that nature herſelf makes of human blood, when being let out of the veins it is ſuffered to refrigerate and ſettle, is, into a fluid or ſerous, and a conſiſtent or fibrous part; and ſince it is found, that oftentimes the former of theſe parts either equals or exceeds the latter in quantity; I thought it might probably much conduce to the better diſcovery of the nature of the blood, to make ſome trials upon the ſerum by itſelf; of which, it will not, I hope, be uſeleſs to give a ſummary account in the following promiſcuous obſervations, that were made only upon the ſerum or whey of the blood of perſons preſumed to be ſound.

1. HAVING ſeparately weighed the ſerum and the conſiſtent part of a parcel of human blood, obtained at once by a ſingle phlebotomy, we found the latter to weigh ℥iv + ʒvjſs, and the former ℥iij + ʒvj: and having made the like trial with another parcel of blood drawn from another perſon, the fibrous part weighed ℥iv + ʒv and the ſerum four ounces: but though in both theſe trials the weight of ſerum, that appeared in one maſs, was inferior to that of the fibrous part; yet it would not be ſafely inferred, that, abſolutely ſpeaking, the fibrous part of either of theſe parcels of blood exceeded the other, ſince we weighed only the ſerum, that we found in a diſtinct maſs; whereas a multitude of ſerous particles may well be ſuppoſed to be lodged between the parts of the conſiſtent maſs or portion of the blood; ſince, beſides, that it is, probably upon the account of the interſperſed ſeroſity, very ſoft, it affords a great deal of aqueous liquor.

2. THIS may ſufficiently appear by the following experiment, which was purpoſely made to examine this conjecture.

WE took a porringer of blood, wherein the ſerum was ſeparated from the fibrous portion, that was coagulated into one conſiſtent maſs; and having carefully poured off all the fluid part, we put the remaining maſs (which weighed ℥iv. + ʒv + 34 gr.) into a ſmall head and body, and diſtilled it in the digeſtive furnace, till the matter left in the bottom of the cucurbite was quite dry, which it did appear to be long before it was ſo indeed. Then taking out the ſeparated parts of this red maſs, the dried portion was found to weigh but ℥i + ʒiij + 34 gr. whereas the ſerous liquor, that paſſed into the receiver, and was limpid and aqueous without any ſhew of ſalt or oil, amounted to ℥iij + 53 gr.

FOR further ſatisfaction we repeated this experiment with the fibrous part of another parcel of human blood, and found the dry maſs remaining in the cucurbite, to weigh but ℥j + ʒvj + 50 gr. whereas the phlegmatick liquor diſtilled from it amounted to ℥vij, that is, to more than three times and a half as much as the dry part.

3. HAVING hydroſtatically examined the ſerum of human blood, we found it heavier than common water: for a piece of red ſealing-wax being ſuſpended in a good balance by a horſe-hair, was found in the air to weigh ʒj + 56 gr. and in the water 35 gr. but did in the ſerum weigh but 33 gr.

THIS trial was confirmed by a more exact one, made with an inſtrument, that I purpoſely cauſed to be made for weighing liquors nicely, in which, when common

water

water weighed 253 grains, an equal bulk of ſerum weighed 302: and becauſe I ſuppoſed, that all ſerums of human blood would not be of equal ſpecific gravity; I thought fit to try that of the blood of another perſon in the ſame inſtrument, and found it to weigh two grains leſs, that is, 300 grains in all.

4. WE once employed ſome ſerum, that could not be (or at leaſt was not) poured off ſo clear, but that it appeared of a reddiſh colour; and though we filtered it through cap-paper, yet a good number of the tinging corpuſcles were ſo thoroughly mingled with it, that the liquor paſſed through the filtre of a yellow colour.

5. To try whether acids would coagulate our ſerum, as I had found they would ſome other animal liquors, I dropt into it ſome ſpirit of ſalt, which did immediately produce with it ſome white concretions, that quickly ſubſided to the bottom, and there (when there was a pretty quantity of them) appeared like a very light and tender cheeſe-curd.

THE like operation, but more powerful, had oil of vitriol upon another parcel of our ſerum.

6. WE dropt into ſome of our liquor good ſpirit of ſal-armoniac; which, as we expected, rather made it more fluid, than did appear to coagulate it, as the acid liquors had done.

7. To try whether theſe precipitations did not more proceed from the coalition and texture of the acid ſalts and the ſerum, than barely from the peculiar action of thoſe ſalts, as acids; we dropt into another portion of our ſerum, a ſtrong alcaliſate ſalt, *viz.* oil of tartar *per deliquium*, which inſtantly produced a white curd, as the ſpirit of ſalt had done, but not, as it ſeemed to us, ſo copiouſly.

8. WE poured alſo upon ſome ſerum highly rectified ſpirit of wine, which, as we expected, did preſently coagulate ſome part of it into a white curd, that was copious enough, but appeared much lighter than either of the former, ſince it would not like them ſubſide, but kept at the top of the liquor.

9. To try alſo, what a ſalt compounded with a metal would do upon our ſerum, we put to it a little ſtrong ſolution of ſublimate, with which it preſently afforded a white and curdled ſubſtance.

WE put ſome of our ſerum upon ſome filings of *Mars*; but, by reaſon of the colour of the liquor itſelf, we could not ſatisfy ourſelves about the event: and though we afterwards put another parcel of ſerum upon filings of the ſame metal, yet neither did this give us ſatisfaction, in regard the vial having been miſlaid, was not looked upon again till many days after; at which time the liquor was grown ſo thick and muddy, that we could not well diſcern any more of the colour, than that it was ſomewhat dark, but not either black or blackiſh: yet, by a trial or two, that we made with a little of this liquor, it ſeemed to have made a ſolution of ſome part of the ſteel; for putting it to ſome freſh infuſion of galls, made with water, it preſently afforded a copious precipitate; but this was ſo far from being inky, that it was not ſo much as dark coloured, but rather whitiſh; at which ſome analogous experiment (mentioned in another treatiſe) that I formerly made, kept me from wondering. Yet I ſhall not omit to add, on this occaſion, that having mixed with ſome of our impregnated ſerum a convenient quantity of infuſion of galls, made in a highly rectified vinous ſpirit, the two liquors did not only afford a kind of coagulum, or precipitate; but, being left together for ſome hours, aſſociated into a conſiſtent body, wherein the eye diſcovered no diſtinct liquor at all.

10. BUT expecting more clear ſucceſs, by putting ſome of our liquor upon filings of copper, which when wrought upon by bodies, that have in them any thing of urinous ſalt, are wont to give a conſpicuous tincture; we accordingly found, that the metal

metal had in a very few hours discoloured the menstruum; and afterwards (the vial being left unstopped, that the air might have access to the liquor) it began by degrees to grow more and more blue, and within a day after was of a deep ceruleous colour.

11. AND, to be confirmed in our conjecture, that this tincture proceeded from some particles of volatile salt latent in the liquor, we mixed some of it with a convenient quantity of syrup of violets, and thereby obtained what we looked for; namely, a colour, which, by reason of the action of those particles upon the syrup, appeared of a fine green.

12. THE blue tincture or solution of copper (mentioned number the 10th) I thought fit to keep for some time, to try, whether the metalline particles would, as it were, embalm the serum they were dispersed through, and preserve the liquor from putrefaction: and in effect, though the vial was left unstopped in a window in my bedchamber for many weeks, yet I (whose organs of smelling are very tender, and who did often put the vial to my nose) did not perceive the liquor to grow at all stinking.

13. ABOUT ℥ij, by guess, of serum of human blood, were left in an unstopped vial (which they more than half filled) for twenty days or three weeks; and though the glass usually stood in a south window, and in the month of *July*, yet, somewhat to our wonder, the serum did not by the smell appear putrified, and yet had let fall a considerable quantity of whitish sediment: but within three or four days after this, the liquor was found to stink offensively. Wherefore, we tried, whether this more than incipient putrefaction was accompanied with any acidity; but could not perceive, that it was, since it would not so much as take off the blue colour of the infusion of lignum nephriticum, or our succedaneum, to it: when it was in this state, we put it to distil in a low cucurbite with a gentle fire, to try, if from this fætid liquor, as is usual from putrified urine, the spirit would first ascend; but we found the liquor, that first came over, to be so little spirituous, or saline, that it would not in an hour's time turn syrup of violets green: but yet we judged it not quite destitute of volatile alcali, because having let fall some of it into a good solution of sublimate, it presently made a white precipitate.

14. WE took some ounces of serum of human blood, filtred through cap-paper to free it from all concreted substance; and having committed it to distillation, in a small retort placed in a sand furnace, we obtained only a few large drops of a darkish red oil, some of which subsided to the bottom of the other liquor, but the greater part swam upon it. We obtained in this first distillation no volatile salt in a dry form, but after a pretty deal of insipid phlegm had been drawn off, there came over a good proportion of spirituous liquor, which smelled almost like the spirit of blood, and contained a pretty deal of volatile alcaly; so that it would readily turn syrup of violets green, and make a white precipitate in the solution of sublimate, and a great ebullition with spirit of salt: this spirit being rectified in a small head and body, there was left, in the bottom of the glass, a greater quantity than was expected of a substance thick like honey, and which was for the most part of a dark red, and seemed to contain more oil than appeared upon the first distillation. The liquor, that came over the helm, seemed more pure, but not very much stronger than the first spirit. Yet, having put it into a glass egg with a slender neck, and given the vessel a convenient situation in hot sand, we obtained a volatile alcali, that sublimed into the neck in the form of a white salt. If this trial be reiterated with a success like that I have now recited, it will seem to argue, that the serous, or fluid part of the blood, affords the same elementary principles, or similar substances, both as to number and kind, that the fibrous and consistent part does, though not as to quantity; that of the oil and dry salt

being lefs in a determinate portion of ferum, than they would be in a like quantity or weight of the concreted part of the blood.

Having long fince obferved, that though the fpirituous parts of man's urine are wont to require, that the liquor be digefted or putrified about fix weeks, to loofen them from the more fluggifh parts, and make them afcend before the phlegm; yet if frefh urine be poured upon a due proportion of quicklime, a good part of the fpirit will prefently be untied, and made capable of afcending in diftillation; I thought it worth while to try, what would be afforded by the ferum of human blood, if it were put upon quicklime, before we diftilled it. In purfuit of this enquiry, we put thefe two bodies together, upon whofe commixture there enfued (but not prefently) a fenfible, but tranfient heat. This compounded body, being committed to diftillation, afforded firft a kind of phlegm in a gentle fire, and then in a ftronger, a moderate quantity of liquor, that was thought to fmell manifeftly of the lime, but had not a brifk tafte. This was accompanied with fomewhat more of high coloured and fætid oil, than was expected. The other liquor being flowly rectified, the fpirit, that firft came over, had a ftrong and piercing fmell, but lefs rank than that of human blood, drawn the ordinary way. Its tafte alfo was not only quick, but fomewhat fiery. Being dropt upon fyrup of violets, it prefently turned it green; with a ftrong folution of fublimate in water, and another of quickfilver in aqua-fortis, it immediately made two white precipitates; and being mingled with fome good fpirit of fea-falt, though, upon their being confounded, there appeared a thick, but whitifh fmoke, there was not produced any vifible conflict or bubbles, yet the colour of the fpirit of falt, appeared much heightened by this operation. But here I muft, though not in due place, take notice, that having put the lately mentioned mixture of the fpirit of ferum and of falt, to evaporate, that we might obferve, whether it would afford a falt much figured like fal-armoniac, we found, that it did not; but that the colour produced in the mixture, whilft fluid, was fo heightened in the concretion we fpeak of, that it appeared of a blood-red colour; but for the fhape, it was fo confufed, that we could not reduce it to any known kind of falt. But all which phœnomena, this fpirit of the ferous part of blood feems to be very near of kin to that of the concreted part of blood, elfewhere by us defcribed. Becaufe quicklime is wont to be fufpected by phyficians, by reafon of its cauftick fretting quality, I thought fit to try, whether the fixed falt of pot-afhes (which is a lixiviate alcali as well as lime) being fubftituted in the room of it, would in diftillation have the fame effect upon ferum of human blood. Wherefore, to four parts of the liquor we put one of the falt, and having diftilled them flowly in a glafs head and body, we obtained good ftore of a liquor, which was not judged any thing near fo ftrong, as that formerly mentioned to have been drawn off from quicklime: and having put this weak liquor, afforded by our ferum, to rectify with a gentle heat, we found, that even the two fpoonfuls of liquor, that firft afcended, were not fpirituous, but very phlegmatick; nor would it well turn fyrup of violets green, though it afforded fome little and light precipitate, when it was put upon a folution of fublimate.

This may feem fomewhat the more remarkable, if I add, on this occafion, an experiment, that may be fometimes of practical ufe, efpecially in phyfick, and may afford much light to thofe that are ftudious, to know the nature and preparations of fo very ufeful a fubject, as human urine. We took three parts of frefh urine, (that was not many hours old: and having put into it one part of falt of pot-afhes, (becaufe that was at hand, for elfe I prefume the fixt falt of tartar, or even of common wood-afhes, would have ferved the turn) and having flowly diftilled them in a head and body, there firft afcended a liquor fpirituous enough; which being fet afide, we continued the diftillation

lation (after having poured the mixture into a retort) till the remains appeared dry. In this operation it is to be noted, that we obtained not one drop of oil, and that (perhaps for that reason) this ſpirit of urine was not near ſo fætid, as, being made the common way, it is wont to be: and that the liquor, that came over toward the latter end of the diſtillation, was ſo unlike that, which the ſerum of blood afforded us, that it was not only conſiderably ſtrong, and manifeſtly ſtronger than that, which firſt aſcended, but had a penetrating and fiery taſte, which left a laſting impreſſion upon the tongue; and, with good ſpirit of ſalt, made a notable ebullition, which I remember not, that upon trials purpoſely made, I found the ſpirit of urine, drawn from quicklime, to have done. And whereas, with this laſt mentioned liquor, I never (that I remember) found any volatile ſalt to aſcend (in a dry form;) in the operation made by the help of ſalt of pot-aſhes, there came up, without rectification, divers grains of volatile ſalt, one of which was chryſtalline, and conſiderably large; ſo that we could, with pleaſure, obſerve it to be like a plate curiouſly figured: but becauſe of ſome leſſer corns of ſalt, that hid one part of it, I could not clearly diſcern, whether it were hexagonal or octagonal.

But here I muſt not conceal, that having, for greater certainty, reiterated this experiment, it had not ſo good ſucceſs; the liquor, that came over, appearing much more phlegmatick than that, which the former trial afforded us; though we both times employed ſalt of pot-aſhes taken out of the ſame veſſel, and the urine of the ſame perſon. So that what the reaſon of the difference may be, does not yet occur to me; but perhaps will, upon further trials: yet this liquor, that appeared ſo weak at its firſt coming over, being rectified *per ſe*, afforded more than was expected of a briſk ſaline ſpirit, from which we eaſily obtained a pretty quantity (in proportion to the liquor) of volatile ſalt in a dry form, and of a very white colour.

We took between two and three ounces of ſerum of human blood; and having put it into a bolthead, capable, by our gueſs, of containing about four times as much liquor, and having ſealed the glaſs hermetically, ſet it by, as well to obſerve, whether any manifeſt changes would appear in it within a week or two, (of which none in that time occurred to us) as for ſome other purpoſes, that may be gueſſed at by the following account of the event.

1. After we had kept the liquor ſealed up above a whole year, it did not appear to be at all coagulated, nor to have let fall any manifeſt reſidence; but ſeemed to be as fluid as when it was firſt put in.

2. It did not appear to have bred any the leaſt worm or maggot. And this I the rather take notice of, becauſe it agrees very well with what I have elſewhere alledged, in diſfavour of their opinion, that think all the fluid and ſoft parts of human bodies do naturally, and of themſelves, in no long time, breed worms, or ſome ſuch inſects; which, for my part, I never obſerved to be generated in blood itſelf, though very long kept and putrified, provided it were freſh enough when put into the glaſs, and, by an exact cloſure, kept from being any way blown upon by flies, or impregnated by ſeminal particles, that may be unſuſpectedly conveyed to it by the air.

3. Nor did there appear to the eye any mother, as they call it, or recrementitious ſubſtance, that is ſuppoſed, in liquors, always to accompany and betoken putrefaction.

4. One of my deſigns, in our experiment, being to try, whether the ſerum would, by the mutual action of the parts upon one another, or by that of ſome catholick permeating fluid, afford ſo much air, as would either crack, or more violently break the glaſs; the tip, at which the bolt-head was ſealed, was warily taken off with a key,

 whereupon

whereupon there rushed out a pretty deal of air, with a considerable noise: and I doubted not that this generated (or at least extricated) air had been considerably compressed, whilst it was pent up; when casting my eyes on the liquor, to discover what change this eruption had made there, I perceived, on the upper surface of the liquor, a multitude of small bubbles, such as are wont to be seen in drink a little bottled, upon the opening of the vessel; and also in divers liquors, after the air has been pent up with them, when the glasses come to be unstopped. And I also the less wondered at this, because I remembered what formerly happened to me, after having sealed up some sheep's blood, and kept it for several days in a gentle warmth; for, though the glass it was inclosed in, were far larger than this, that contained our serum; yet, after some time, when no body offered any violence to it, or was near enough to stir it, it was suddenly blown up with a surprising noise, by the aereal or elastical corpuscles, that were produced, or set free by the putrefaction we discovered to have been made.

5. THE smell of our serum was strong, but not cadaverous, but rather resembled that of the tincture of sulphur, made with salt of tartar and spirit of wine, or of some such sulphureous preparation.

6. ONE of the chief aims I had in keeping our serum so long sealed up, was to try, whether by a digestion, or putrefaction for some months, the serum of blood would, like urine, (which is commonly thought to be a liquor made of it, and of very near cognation to it) afford a saline spirit, or an alcali volatile enough to ascend before the phlegm. And, in pursuit of this enquiry, we committed our serum to distillation in a small glass head and body, and in a digestive furnace, being careful to take the first spoonful, or thereabouts, of spirit, that passed into the receiver: but we found, that, though this liquor at first smelt strong enough, (I say at first, because the odour soon after grew fainter) yet the taste was not at all brisk nor spirituous, like that wont to be obtained by distillation from putrified urine; nor did our liquor, being dropped into a little syrup of violets, give it presently any manifest greenness. But yet, because I found it not insipid, I thought fit to examine it a little more critically, and dropt a convenient quantity of it into a clear and staturate solution of sublimate in common water, by which means there was produced a whiteness like that (but not near so dense) which spirit of urine, or volatile salt would have produced. And by this I was invited to mix some of it with a little syrup of violets, upon a piece of white paper, and also to wet with the same (distilled) liquor some small fillings of copper spread upon another piece of paper, and to leave them both all night in the open air, that the liquor might have time enough to work upon the syrup and the metal: by which course we found, in the morning, that the former was turned green, and the latter was so far dissolved, as to leave a large blueish stain upon the paper. I mention these things the rather, because, according to the opinion of some learned men, this degenerated serum should have been of an acid, not an alcalisate nature.

7. THE near cognation, that, according to some learned physicians, there is between milk, and the more serous part of the blood, invited me to try, whether, (according to an experiment made on new milk, that I have heard ascribed to the famous *Sylvius*) our serum of human blood would grow red, by being kept continually stirring over a moderate heat, with a competent quantity of salt of tartar; but in two trials, we found not any redness produced, though one of them was made in a vessel of refined silver, with an eighth part of the salt in reference to the serum, which was the same proportion, that we had used, when we made the experiment succeed well in milk.

8. PERHAPS it will be needless to take notice, that the serum of human blood will, by heat, be in a short time coagulated into a kind of gelly, or rather, as far as I have observed,

observed, into a substance like a custard, as to consistence, though not as to colour. And therefore I shall now add, that having found, that acid spirits also would coagulate serum, I thought fit to try, whether alcalies would not oppose, or retard its coagulation. Of which trial the event was, that having put spirit of human blood to a convenient quantity of serum, and caused them to be kept stirring over a very gentle fire; though the volatile alcali did not hinder the coagulation, yet it seemed to make it both more slow, and more soft or lax: and this effect was yet more considerable, when we tried another parcel of serum with salt of tartar, instead of spirit of blood.

PART IV.

Containing the history of the spirit of human blood, begun in an epistolary discourse to the very learned Dr. J. L.

SIR,

HAVING, by want of leisure and opportunity, been reduced to treat of the history of human blood in so imperfect and desultory a way, that several of the titles have been left wholly untouched, and others have been but transiently and jejunely treated of; I thought fit to handle more fully some one of the primary titles, and branch it into its several subordinate or secondary titles: and for this purpose I pitched upon the spirit of human blood, being willing, on so noble a subject, to give a specimen of what might have been done to illustrate the other primary titles, if some requisites had not been wanting. And since the spirit of human blood is, at least, one of the noblest of urinous or volatile alcalies, so that most of the things, that shall be taught concerning that, may, with some little variation, be applied to spirit of urine, hartshorn, sal-armoniac, soot, *&c.* I thought fit to lay down a scheme of subordinate titles, whose heads (which amount to above half the number of the primary ones, that belong to the whole history of blood) should be so numerous and comprehensive, that this paper may pass not only for an example, but for a kind of summary of the history of volatile salts in general, and so supply the loss of a paper, that I once begun on that subject.

AND now I should, without further preamble, proceed to the intended history, but that I think it requisite to premise three or four short advertisements: whereof the first shall be, that the spirit I employed in making the following trials and observations, was drawn from human blood without any sand, clay, or other additament, (save perhaps, that, by a mistake, that could do no mischief, a small parcel had some vinous spirit put to it to preserve it a while) and that the first distillations (which I so call to distinguish them from rectifications) were performed in retorts placed in sand, (and not with a naked fire) care being taken, that the vessels were not too much filled, because blood [*N. B.*] if it be not well dried, is apt to swell much, and pass into the neck of the retort, if not into the receiver.

SECONDLY, I desire to give notice, that the blood we made use of, was drawn from persons, that parted with it out of custom, or for prevention; which was the main reason, why I was so scantily furnished with blood, that of sound persons being, in the place I resided in, very difficult to be procured in quantity, and that of sick persons being unfit for my purpose.

THIRDLY, It may not be amiss, for obviating of some scruples, to advertise, that there being so great a cognation between the spirit and volatile salt of human blood, that, as we shall see anon, it is probable, that the latter is little other than the spirit in a dry form, and the former than the salt united with phlegm enough to give it a liquid form;

form; it is prefumed, that it may be allowable to confider the volatile falt of blood as its dry fpirit.

LASTLY, To the three foregoing it will be fit to add this fourth advertifement, that though, in comparifon of the particulars thrown in to the fecond and third part of thofe memoirs, the enfuing fourth part is methodically written; yet you are not to expect to find in the method any thing of accuratenefs; fince the experiments and obfervations, whereof this fourth part confifts, were written in loofe papers at diftant times and on differing occafions; and, becaufe of this and of my hafte, will be found, without any regular dependence or connexion, referred to the titles, under which they are ranged, in that order, or rather diforder, wherein they chanced to come to hand.

A LIST of the fecondary Titles concerning the SPIRIT of HUMAN BLOOD.

a.

1. WHETHER human blood may be fo ordered by fermentation or putrefaction, as that, in diftillation, a fpirit, either urinous or vinous, may afcend before the phlegm?

b.

2. WHETHER fpirit of human blood be really any thing but the volatile falt and phlegm well commixed?

c.

3. OF the fpecies of faline bodies to which fpirit of human blood is to be referred.

d.

4. WHETHER fpirit of human blood be differing from fpirit of urine, and other fpirits, that are called volatile alcalies?.

e.

5. OF the quantity of fpirit contained in human blood: whether accompanied with its ferum or dried?

f.

6. OF the fpecifick gravity of fpirit of human blood.

g.

7. OF the odour, tafte, colour, tranfparence and confiftence of the fpirit of human blood.

h.

8. OF the diffolutive power of the fpirit of human blood.

i.

9. OF the tinctures, that may be drawn with fpirit of human blood.

k.

10. OF the coagulating power of the fpirit of human blood.

l.

11. OF the precipitating power of the fpirit of human blood.

m.

12. OF the affinity between fpirit of human blood and fome chymical oils and vinous fpirits.

n.

13. OF the relation between fpirit of human blood and the air.

o.

14. OF the hoftility of the fpirit of human blood with acids, whether they be in the form of liquors or of fumes.

p.

15. OF the medicinal vertues of fpirit of human blood outwardly applied.

q. 16. OF the medicinal vertues of ſpirit of human blood inwardly uſed in pleuriſies, head-achs, coughs, fevers, ſcurvies, cachexies, dropſies, fits of the mother, *&c.*

App. AN appendix, containing Paralipomena, and promiſcuous experiments and obſervations concerning the ſpirit of human blood.

The firſt (ſecondary) TITLE.

Whether human blood may be ſo ordered by fermentation or putrefaction, as that, in diſtillation, a ſpirit, either urinous or vinous, may aſcend before the phlegm?

IT is not unlike, that you will think the queſtion propoſed in this title more curious than neceſſary; and I ſhall not quarrel with you, if you do ſo. But that you may not think it groundleſs, I deſire two things may be conſidered; firſt, how ordinary it is, eſpecially ſince the learned Dr. *Willis*'s writings came to be applauded, to look upon fevers as inordinate fermentations of the blood: and the ſecond, that though human urine, which has a great cognation with the human blood, will not, whilſt freſh, afford by diſtillation a ſpirit or volatile ſalt, till the phlegm be firſt drawn off, and then requires a good fire to make it riſe; yet, if it be kept for a competent time (which uſually amounts to divers weeks) in fermentation, (as chymiſts commonly call that, which, in this caſe, I would rather ſtile putrefaction,) the ſpirit and volatile ſalt will with a gentle fire aſcend, before much, if not before any phlegm.

THESE two conſiderations, as I was intimating, may keep that from being thought a groundleſs queſtion, which has been above propoſed. And, though I more incline to the negative than to the affirmative, at leaſt as to the firſt part or member of the queſtion, yet I thought it well deſerved to be determined, if it may be, by experiment; but for want of a ſufficient quantity of blood, and good luck in making trials with that I could procure, I muſt ſuſpend my judgment, till further experience reſolve me one way or other. By what I have yet tried, I am not much encouraged to expect from human blood a vinous or ardent ſpirit, though that be the uſual product of fermentation in liquors: and I am the leſs encouraged to expect this, becauſe I am not ſure, that there is any fermentation, truly and properly ſo called, in human blood, either within or out of the body; having never yet found any thing in the blood or urine, that convinced me, that either of thoſe liquors would afford an ardent ſpirit. I remember I once kept human blood for a year together in a glaſs very carefully, and, if I miſtake not, hermetically cloſed, with a purpoſe to try, whether any ſpirits would firſt aſcend: but when the blood came to be expoſed to the contact of the air, the ſtink was ſo great and offenſive, eſpecially to ſome ladies, that lived in the houſe, that we were fain to have it haſtily thrown away. Another time, having cauſed ſome ſheep's blood to be digeſted in a pretty large vial, hermetically ſealed, after it had continued a good while in the digeſtive furnace, upon a ſudden, though no body touched it, it broke with a ſurprizing noiſe, and blew off the long neck of the vial. Two or three almoſt like miſchances I had with attempts made on human blood, which I was the more troubled at, becauſe I thought it not very improbable, that by putrefaction the texture of blood, like that of urine, may be ſo looſened or otherwiſe altered, that a volatile ſalt or ſpirit may, in a ſlow diſtillation, aſcend before the phlegm. But, as I ſaid before, it is only from further experience, that I muſt expect ſatisfaction in theſe enquiries.

YET in the mean time I ſhall add on this occaſion, that the ill ſucceſs I had in my attempts to draw a ſpirit from entire portions of blood, without ſeparating any part from it, or adding any foreign body to it, did not hinder, but rather invite, me to try, whether

whether I could not make ſome experiment, of affinity to thoſe above-mentioned, upon whoſe ſucceſs I might ground ſome kind of conjecture, what would have been the events of thoſe trials in caſe they had not miſcarried. Wherefore looking upon the ſerum of blood as the likelieſt part of it, as well as much more likely than the entire blood to concur to a fermentation properly ſo called; we took ſome ounces of this ſerum, and put to it about a fourth part of raiſins (of the ſun) well bruiſed, and kept them in a glaſs, whereof a conſiderable part was left empty; and having cloſed the veſſel, we kept it in a warm room for many days. The event of this trial was, that within few days the raiſins began to emerge, and afterwards continued to float; and there was produced or extricated a conſiderable quantity of permanent and ſpringy air, as by a certain contrivance deſcribed in another paper, did manifeſtly appear: both which phænomena ſeemed plainly to argue, that there had been ſome degree of fermentation produced in the mixture. But yet when we came to diſtill the thus altered ſerum, though it did not ſtink, as if it had putrified it would have done, yet the liquor, that firſt aſcended, even with a gentle heat, did not taſte or ſmell like a vinous ſpirit, though it was differing from meer phlegm. If I had been furniſhed with a greater quantity of ſerum, perhaps the reiterated experiment would have given more ſatisfaction; and, in making it, I would have been careful to obſerve, whether the produced fermentation might not be ſuſpected to proceed, not ſo much from the whole ſerum, as ſuch, as from the aqueous particles, in diſtinction from the others, that concurred with them to compoſe it.

As for the ſecond queſtion intimated in the preſent firſt title, namely, whether blood will, by digeſtion or putrefaction, be ſo opened, as that, when it is diſtilled, the ſpirit will aſcend before the phlegm? I likewiſe endeavour to try that, with the ſerous part of the blood poured off from the fibrous or coagulated, as ſuppoſing it in this ſeparated ſtate more proper for our trial than the entire blood: and having kept a pretty quantity of this ſerum above four times as long, as I had obſerved to have been ſufficient to make urine in diſtillation part with its ſpirit before its phlegm, we diſtilled this long kept liquor with a very gentle fire, that few, or none, beſides the fugitive parts might at firſt aſcend. But we found the liquor, that came over, to have but little ſtrength, either as to ſmell or taſte, nor would it readily turn ſyrup of violets green: I ſay readily, becauſe after they had been ſome hours together it would; but yet as a volatile alcali, it would preſently turn a ſtrong ſolution, made of common ſublimate in fair water, into a white opacous, and almoſt milky liquor.

The ſecond (ſecondary) TITLE.

Whether ſpirit of human blood be really any thing but the volatile ſalt and phlegm well commixed?

SINCE the queſtion moved in this title may be alſo propounded concerning other alcaliſate ſpirits, as thoſe of urine, hartſhorn, ſoot, *&c.* it is upon that account the more important: and for this reaſon, as well as for the difficulty of determining it by cogent proofs, I may think myſelf obliged to forbear taking upon me to decide it peremptorily, till further experience ſhall have furniſhed me with fuller information. So that for the preſent, about this difficult queſtion, I ſhall venture to ſay no more than this; that what has hitherto occurred to me, inclines me to think, that the ſpirit of human blood is totally compoſed of volatile ſalt and phlegm; if by phlegm we underſtand, not ſimple or elementary water, but a liquor, that, although it paſs among chymiſts for phlegm, and deſerves that name better than any other liquor afforded by human blood, yet in the ſtricteſt acception it is not that; for when the ſpirit,

ſpirit, volatile ſalt, and oil, are ſeparated from it by diſtillation and ſublimation, as far as they are wont to be in chymical preparations of volatile alcalies, the remaining liquor, which paſſes for phlegm, will yet be impregnated with ſome particles of oil, and perhaps alſo with ſome few of volatile ſalt, that are too minute to be diſtinguiſhable by the naked eye. But whether frequent rectifications may ſo accurately ſeparate theſe heterogeneous parts, as perfectly to free the aqueous ones from them, and thereby reduce the phlegm to ſimple or elementary water, I am content, at leaſt till I ſhall have had ſufficient quantities of diſtilled blood for making the requiſite trials, to leave as a problem: and this the rather, becauſe I am not ſure, but that, by frequent diſtillations, ſome particles of the fire may, from time to time, ſubſtantially be aſſociated with thoſe of the liquor; nor yet, but that, even in the firſt diſtillation of human blood, the fire may have either ſeparated or produced a liquor, that, though almoſt ſtrengthleſs, and not juſtly referrible to either of the received principles or ingredients, oil, ſalt and earth, is not yet phlegm truly ſo called, but a liquor as yet anonymous; as I have elſewhere ſhewn, that woods, and many other bodies, afford by diſtillation a liquor, that is not an oil, and is neither acid nor alcaliſate, and yet is no true phlegm, but, as I have there ſtiled it, an adiaphorous ſpirit.

It will probably be thought material, if on this occaſion I add, in favour of the opinion or conjecture, to which I lately owned myſelf inclined, that conſidering, that the knowledge of the compoſition of a body may be ſometimes as well, if not better, inveſtigated by the way of generating or producing of it, as by that of analyſing or reſolving it; I made for trial's ſake the following experiment. We diſſolved in diſtilled water as much volatile ſalt of human blood, as the liquor would take up; and then having carefully diſtilled it in a conveniently ſhaped veſſel, with a regulated degree of heat, the diſtillation afforded us ſuch a liquor as was deſired; namely, one, that by ſmell, taſte, and divers operations, appeared to be a good briſk ſpirit of human blood. This experiment, for the main, was made another time with the like ſucceſs.

The third (ſecondary) TITLE.

Of the ſpecies of ſaline bodies, to which the ſpirit of human blood is to be referred.

I need not ſpend much time to declare a thing, that is now ſo well known to many phyſicians and chymiſts, of this, and ſome of the neighbouring countries, as it is, that, of late years, ſaline ſpirits, obtained by diſtillation, have been obſerved to be of two ſorts: but becauſe there are many, even of the learned, eſpecially in the remoter parts of *Europe*, that are not well acquainted with this diſtinction; leſt ſome, to whom you may ſhew this paper, ſhould chance to be of that number, it may not be amiſs to intimate in two or three words, that the ſaline ſpirits, that aſcend in diſtillation, are ſome of them acid in taſte, as ſpirit of nitre, ſpirit of vitriol, *&c.* and ſome others have taſtes very differing from that, being rather ſomewhat like common ſalt, or like lixiviate ſalts. And the difference is greater in their operations, than in their taſtes; for being put together, there will preſently enſue a manifeſt conflict between them, and uſually (for I have not found it to hold in all caſes) the one will precipitate the bodies, that the other hath diſſolved. And it is neceſſary to add, that among the ſalts called alcalies, ſome are fixed in conſiderable degrees of fire, and others not; for which reaſon, divers modern ſpagyriſts and phyſicians, that take acid and alcali for the true principles of mixed bodies, call the one fixed and the other volatile alcalies. And, though I have elſewhere queſtioned this doctrine, and given my reaſons, why I approve neither it nor the appellations newly mentioned, and often call the ſalts, made by combuſtion, ſimply alcalies or elſe lixiviate ſalts, and thoſe, that aſcend, ſometimes urinous, and sometimes volatile salts and spirits; yet, since the names of fixt alcalies and volatile ones are much now in request, I shall comply with custom, and oftentimes (though not always) make use of them in the sense of those, that employ them.

THESE things being premised, I may now seasonably propound this important question; to what species of saline bodies the spirit of human blood is to be referred? I say, of saline bodies, because, though the spirit of blood be a liquor, yet its more efficacious operations seem almost (if not more than almost) totally to depend upon the fugitive salt wherewith it abounds. The ground of the foregoing question may be twofold; the one, that I have elsewhere proved against the general supposition, that some volatile salts, that arise even in a dry form, may not be of an alcalisate, but acid nature; and the other, that not only *Helmont* and his disciples, but a great part of the modern chymists, and physicians too, ascribe digestion to an acid ferment or menstruum in the stomach; whence, one may suspect, that store of acid corpuscles may pass into the mass of blood, and impregnate it, as I elsewhere shew, that particles of differing natures may be even by the senses discovered to do.

BUT, notwithstanding this, I shall not scruple to say, in answer to the propounded question, that, as far as I have hitherto been able to observe, the spirit of human blood is manifestly referrible to that classis, that many call volatile alcalies (and I often call urinous spirits;) for I find spirit of blood capable of doing those things, the performance of which has been looked on, almost ever since I publickly proposed them, as the touchstone to know volatile alcalies, and distinguish them from the other sorts of saline bodies. For the spirit of human blood will make a great conflict with divers acid spirits, as spirit of salt, aqua-fortis, *&c.* It will immediately turn syrup of violets from its blue colour into a fair green; it will precipitate a solution of sublimate in common water into a white powder; and, in short, I found it to perform those other things, that may be expected from volatile alcalies, as such, as often as I had occasion to make trial of it, sometimes on one body, and sometimes on another.

IF I were sure (as for reasons elsewhere declared I am not) that the digestion of aliments were made by an acid ferment or juice, whencesoever the stomach is furnished with it, I should be prone to suspect, that some acid particles may be mingled with the blood: but however, that would not hinder me from referring the spirit of human blood to volatile alcalies; because so few acid particles would be either destroyed by the alcalisate ones, that are so abundant in the spirit, or at least these would be so very much predominant, as to allow us very warrantably to give on their account a denomination to the mixture. And if a few drops of spirit of vinegar were mixed with some pints or pounds of stale urine, they would either be deprived of their acidity by some corpuscles of a contrary nature, that they would meet with in the liquor; or they would be so obscured and overpowered by the fugitive salts it abounds with, that the acetous corpuscles would not hinder the spirituous liquor, drawn from the mixture by distillation, to be justly referrible to the classis of volatile, urinous salts.

The fourth (secondary) TITLE.

Whether spirit of human blood be differing from spirit of urine, and other spirits, that are called volatile alcalies?

THE question, whether there be any difference between the spirit of human blood, and other volatile alcalies, as spirit of urine, harts-horn, *&c?* Seems to me very difficult to be decided; because two bodies may agree in many qualities, and perhaps, in all of those, that are the most obvious, and yet may on some third body, or in some cases, manifest distinct powers, and have their peculiar operations. Nor do I yet see any

any certain way, by which the affirmative part of the queſtion, though it ſhould be true, can be clearly demonſtrated. Therefore, leaving the peremptory deciſion of this queſtion to thoſe, that ſhall think themſelves qualified to make it, I ſhall (at leaſt till I be further informed) content myſelf to make a couple of remarks, in reference to the propoſed inquiry.

AND firſt, I think, there may be a great difference between volatile ſalts or ſpirits, as they are ordinarily prepared for medicinal uſes, and as they may, by reiterated rectifications, and other ways of depuration, be brought to as great a ſimplicity or purity, as a dextrous chymiſt can bring them to. I thus expreſs myſelf, becauſe as to an exquiſite or elementary ſimplicity, though ſome eminent artiſts pretend to it, I am not ſure, that chymiſts can attain it; eſpecially conſidering what I elſewhere ſhew of the unheeded commixtures, that may (at leaſt ſometimes) be made by the corpuſcles of the fire, with thoſe of the bodies it works on.

MY other remark is, that whether or no, if the ſpirit of human blood, and other liquors abounding like it in volatile alcalies, were reduced to as great a purity as they can by art be brought to, they would be altogether alike in their nature and qualities; yet, if we conſider them (as men uſe to do) in that ſtate, wherein they are wont to be thought pure enough for medicinal uſes, and are accordingly employed by phyſicians and chymiſts; I think it very probable, that there is ſome difference between the ſpirit of human blood and ſome other volatile alcalies, and particularly thoſe afforded by urine and by harts-horn. For, though to me the bad ſmells of all theſe liquors ſeemed to be much alike, yet divers ladies, and thoſe of very differing ages, affirm they find a manifeſt difference between theſe ſmells, and do abhor the odour of ſpirit of blood as a ſtink, though they will with pleaſure hold their noſes a great while over the ſpirit of harts-horn, and even that of (vulgar or European) ſal armoniac (which is in effect a ſpirit of man's urine) and affirm themſelves to be much refreſhed by it. And, whereas, with ſpirit of urine or of ſal-armoniac joined in a due proportion with ſpirit of ſalt, I have uſually (as I have long ſince noted in another paper *(a)* been able to make a ſalt, that ſhoots into the peculiar figure of ſal-armoniac, which figure is very differing from that of ſea-ſalt, nitre, *&c.* I have ſeldom, if ever, obtained (at leaſt in any quantity) a ſalt of that ſhape, by the commixture of the ſpirit of human blood with that of common ſalt; for, though their ſaline corpuſcles upon the evaporation of the ſuperfluous moiſture, would coagulate together, yet the concretion ſeemed confuſed, and either all or a great part of it was deſtitute of that neat and diſtinct ſhape, that I had ſeveral times obſerved in concretions made by the mixture of the ſpirit of ſea-ſalt with urinous ſpirits. And, as to the medicinal virtues of ſpirit of blood, though I have not had opportunity to make compariſons experimentally, and therefore ſhall forbear to affirm any thing myſelf; yet, if we credit the famous *Helmont*, there is a conſiderable difference between the ſpirit of human blood and that of human urine, ſince he ſomewhere expreſly notes, (though I remember not the place, nor have his book at hand) that the ſpirit of human blood cures epilepſies, which is a thing the ſpirit of urine will not do.

(a) The uſefulneſs of experimental philoſophy.

The fifth (ſecondary) TITLE.

Of the quantity of ſpirit contained in human blood, whether accompanied with its ſerum or dried.

IT is not eaſy to determine the exact proportion of that liquor, which, when by diſtillation obtained from human blood, the chymiſts call its ſpirit, in reference to the other principles or ingredients, whereof the blood conſiſts. For ſome men's blood may be much more phlegmatic or ſerous than that of others, which itſelf may be more leſs ſpirituous, according to the complexion, age, ſex, &c. of the perſon that bleeds. But, to make ſome eſtimate, that will not probably much recede from what may be ordinarily found, I ſhall inform you, that twelve ounces of healthy human blood afforded us ſeven ounces and a half of phlegm, and conſequently about four ounces and half of dried ſtuff: and then, I ſhall add, that having committed to diſtillation, in a retort ſand furnace, ſeven ounces of well dried (but not ſcorched) blood, we obtained about ſeven drachms, that is, about an eighth part of ſpirit, to which, though it were not rectified, that name may well enough be given; becauſe it was ſo very rich in ſpirituous and ſaline parts, that it left, in the receiver and in the vial I kept it in, a good deal of volatile ſalt undiſſolved, which a phlegmatic liquor would not have done: and if that be admitted for a truth, that was above propoſed as a very likely conjecture; namely, that ſpirit of blood is but ſalt and phlegm united, we may well ſuppoſe, that human blood yields a far greater proportion of ſpirit than this; ſince from the ſeven ounces of dried blood laſt mentioned, we obtained about five drachms of volatile ſalt, which if we had by diſtillations united with a fit quantity of phlegm, would probably have afforded us near two ounces of a liquor deſerving the name of ſpirit.

The ſixth (ſecondary) TITLE.

Of the conſiſtence and ſpecific gravity of the ſpirit of human blood.

To the conſiſtence of the ſpirit of human blood, taken in the more lax ſenſe of the word conſiſtence, one may refer its ſpecific gravity, (as that is uſually proportionate to the denſity of bodies,) the greater or leſſer degree of fluidity, that belongs to the liquor as a maſs, and the greater or leſſer ſubtilty of the minute parts, whereof it is compoſed, or wherein it abounds.

And as to the firſt of the three attributes, we have noted to be referrible to the conſiſtence of our ſpirit; gravity is a quality, that is ſo radicated, if I may ſo ſpeak, in the nature of viſible fluids or liquors, and does ſo obſtinately accompany them, that I durſt not omit to examine the ſpecific gravity (that is, the gravity in proportion to the bulk) of ſpirit of human blood; though by reaſon of the ſmall quantity I had of it I could not make uſe of the ſame inſtruments, that I was wont to employ in hydroſtatical trials, where I was not ſo ſtinted in the liquor to be examined, But, however, I made a ſhift to make a trial of this kind, by which I found, that a compact body weighing fifty eight grains in the air, and in water ſix grains and three fourth parts, weighed in rectified ſpirit of human blood but five grains and one fourth part: and on this occaſion I ſhall tell you, what I preſume you did not expect, which is, that notwithſtanding the volatility of our ſpirit of blood, I found, that a pretty large piece of amber being put into it, did not, as moſt men would confidently expect, fall to the bottom of the liquor, but kept itſelf floating at the upper part of it, and, if plunged into it, would emerge.

The next quality we referred to the conſiſtence of our ſpirit of blood, is the degree of its fluidity, or, if you pleaſe, its greater or leſſer immunity from tenaciouſneſs or viſcoſity, which ſome modern philoſophers (whoſe opinion needs not here be diſcuſſed) think to belong to all liquors as ſuch. Now, one may be the more inclined to expect a manifeſt degree of tenacity in the ſpirit of human blood; becauſe among many modern chymiſts it paſſes for an alcali, and we know, that divers other alcaliſate liquors, as oil of tartar *per deliquium*, fixed nitre reſolved the ſame way, ſolution of pot-aſhes, &c. are ſenſibly unctuous, and but languidly fluid: but yet, I did not obſerve, that ſome rectified ſpirit of human blood, that I purpoſely tried between my fingers, did feel more

more unctuous than common water. And whereas, those, that sell brandy, or spirit of wine, are wont to shake it, till it afford some froth, and then, by the stay this makes on the surface, to judge of the tenacity or tenuity of the liquor, esteeming that to be the most unctuous, whereon the bubbles make the longest stay, and that the finest, on which they soonest disappear; I thought fit, by the same method, to examine spirit of human blood, and found, that the froth would last very little on the surface of it, the bubbles breaking or vanishing, almost (if not quite) as nimbly, as if the liquor had been good spirit of wine. And I likewise observed, that when I warily let fall some of our well rectified spirit of blood upon some other body, it seemed to me, that the single drops were manifestly smaller, than those of water, and of several other liquors, would have been: which will be much confirmed by one passage of what I have to say, about the third quality referrible to the consistence of the spirit we treat of.

BECAUSE it may be a thing of some importance, as well as curiosity, to know, how subtil the active parts of spirit of human blood are, and how disposed and fitted to disperse or diffuse themselves through other liquors of convenient textures; to make a visible discovery of this, I bethought myself of a method, that having formerly devised for several purposes, I thought fitly applicable to my present design. For having looked upon it as a great defect, that men have lazily contented themselves to say in general, that such a body is of subtile, or of very subtile parts, without troubling themselves to find out any way of making more particular and less indeterminate estimates of that subtilty; I was invited to find out and practise a way, that might, on divers occasions, somewhat supply that defect: but having delivered this easy method in another paper, I shall forbear to repeat a tedious account of it in this; since it may here suffice to tell you in short, what will perhaps surprize you; namely, that according to the forementioned way, we so prepared common water by infusions made in it without heat, that by putting one single drop of our rectified spirit of human blood into ℥iv. + ℈iv. (which make 2000 grains) of the prepared water, and lightly shaking the vial, there appeared throughout the liquor a manifest colour, whereof no degree at all was discernible in it just before: which sufficiently argues a wonderful subtilty of parts in the spirit we employed; since that a single drop of it could disperse its corpuscles, so as to diffuse itself through, and mingle with two thousand times as much water, and yet retain so much activity, as to make their presence not only sensible, but conspicuous, by a manifest change of colour they produced. I confess, this computation is made upon supposition, that a drop of water weighs about a grain, and that a drop of our spirit of blood was of the same weight with a drop of water.

THE former supposition is commonly made; and though I have not found it to be exactly true, but that a drop of water weighed a tantillum more than a grain; yet, that difference is much more than recompensed by that, which we found between the weight of a drop of water, and the weight of one of spirit of human blood. For having, in a very good and carefully adjusted balance, let fall ten drops of common water, and as many of our rectified spirit of human blood, (as judging it a safer way to make an estimate, by comparing so many drops of each liquor than one alone;) we found, as we might well expect, that a drop of this last named liquor, as it was manifestly lesser, so it was far lighter, than a drop of water, insomuch, that the whole ten drops did not amount to four grains: so that we may safely judge the drop of spirit to have manifestly diffused itself, and acted upon above 4000 times so much water in weight, (and perhaps in bulk too;) since indeed the proportion extended a good way towards that of one to 5000, and so may be said to be, as that of one to between 4000 and 5000; which, though it may seem incredible to those, that are unacquainted with the great subtilty of nature and art, in the comminutions they can make of bodies;

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may be much more phlegmatic or ferous than that of others, which itfelf may be more lefs fpirituous, according to the complexion, age, fex, *&c.* of the perfon that bleeds. But, to make fome eftimate, that will not probably much recede from what may be ordinarily found, I fhall inform you, that twelve ounces of healthy human blood afforded us feven ounces and a half of phlegm, and confequently about four ounces and half of dried ftuff: and then, I fhall add, that having committed to diftillation, in a retort fand furnace, feven ounces of well dried (but not fcorched) blood, we obtained about feven drachms, that is, about an eighth part of fpirit, to which, though it were not rectified, that name may well enough be given; becaufe it was fo very rich in fpirituous and faline parts, that it left, in the receiver and in the vial I kept it in, a good deal of volatile falt undiffolved, which a phlegmatic liquor would not have done: and if that be admitted for a truth, that was above propofed as a very likely conjecture; namely, that fpirit of blood is but falt and phlegm united, we may well fuppofe, that human blood yields a far greater proportion of fpirit than this; fince from the feven ounces of dried blood laft mentioned, we obtained about five drachms of volatile falt, which if we had by diftillations united with a fit quantity of phlegm, would probably have afforded us near two ounces of a liquor deferving the name of fpirit.

The fixth (fecondary) TITLE.

Of the confiftence and fpecific gravity of the fpirit of human blood.

To the confiftence of the fpirit of human blood, taken in the more lax fenfe of the word confiftence, one may refer its fpecific gravity, (as that is ufually proportionate to the denfity of bodies,) the greater or leffer degree of fluidity, that belongs to the liquor as a mafs, and the greater or leffer fubtilty of the minute parts, whereof it is compofed, or wherein it abounds.

And as to the firft of the three attributes, we have noted to be referrible to the confiftence of our fpirit; gravity is a quality, that is fo radicated, if I may fo fpeak, in the nature of vifible fluids or liquors, and does fo obftinately accompany them, that I durft not omit to examine the fpecific gravity (that is, the gravity in proportion to the bulk) of fpirit of human blood; though by reafon of the fmall quantity I had of it I could not make ufe of the fame inftruments, that I was wont to employ in hydroftatical trials, where I was not fo ftinted in the liquor to be examined, But, however, I made a fhift to make a trial of this kind, by which I found, that a compact body weighing fifty eight grains in the air, and in water fix grains and three fourth parts, weighed in rectified fpirit of human blood but five grains and one fourth part: and on this occafion I fhall tell you, what I prefume you did not expect, which is, that notwithftanding the volatility of our fpirit of blood, I found, that a pretty large piece of amber being put into it, did not, as moft men would confidently expect, fall to the bottom of the liquor, but kept itfelf floating at the upper part of it, and, if plunged into it, would emerge.

The next quality we referred to the confiftence of our fpirit of blood, is the degree of its fluidity, or, if you pleafe, its greater or leffer immunity from tenacioufnefs or vifcofity, which fome modern philofophers (whofe opinion needs not here be difcuffed) think to belong to all liquors as fuch. Now, one may be the more inclined to expect a manifeft degree of tenacity in the fpirit of human blood; becaufe among many modern chymifts it paffes for an alcali, and we know, that divers other alcalifate liquors, as oil of tartar *per deliquium*, fixed nitre refolved the fame way, folution of pot-afhes, *&c.* are fenfibly unctuous, and but languidly fluid: but yet, I did not obferve, that fome rectified fpirit of human blood, that I purpofely tried between my fingers, did feel more

more unctuous than common water. And whereas, those, that sell brandy, or spirit of wine, are wont to shake it, till it afford some froth, and then, by the stay this makes on the surface, to judge of the tenacity or tenuity of the liquor, esteeming that to be the most unctuous, whereon the bubbles make the longest stay, and that the finest, on which they soonest disappear; I thought fit, by the same method, to examine spirit of human blood, and found, that the froth would last very little on the surface of it, the bubbles breaking or vanishing, almost (if not quite) as nimbly, as if the liquor had been good spirit of wine. And I likewise observed, that when I warily let fall some of our well rectified spirit of blood upon some other body, it seemed to me, that the single drops were manifestly smaller, than those of water, and of several other liquors, would have been: which will be much confirmed by one passage of what I have to say, about the third quality referrible to the consistence of the spirit we treat of.

BECAUSE it may be a thing of some importance, as well as curiosity, to know, how subtil the active parts of spirit of human blood are, and how disposed and fitted to disperse or diffuse themselves through other liquors of convenient textures; to make a visible discovery of this, I bethought myself of a method, that having formerly devised for several purposes, I thought fitly applicable to my present design. For having looked upon it as a great defect, that men have lazily contented themselves to say in general, that such a body is of subtile, or of very subtile parts, without troubling themselves to find out any way of making more particular and less indeterminate estimates of that subtilty; I was invited to find out and practise a way, that might, on divers occasions, somewhat supply that defect: but having delivered this easy method in another paper, I shall forbear to repeat a tedious account of it in this; since it may here suffice to tell you in short, what will perhaps surprize you; namely, that according to the forementioned way, we so prepared common water by infusions made in it without heat, that by putting one single drop of our rectified spirit of human blood into ℥iv. + ℈iv. (which make 2000 grains) of the prepared water, and lightly shaking the vial, there appeared throughout the liquor a manifest colour, whereof no degree at all was discernible in it just before: which sufficiently argues a wonderful subtilty of parts in the spirit we employed; since that a single drop of it could disperse its corpuscles, so as to diffuse itself through, and mingle with two thousand times as much water, and yet retain so much activity, as to make their presence not only sensible, but conspicuous, by a manifest change of colour they produced. I confess, this computation is made upon supposition, that a drop of water weighs about a grain, and that a drop of our spirit of blood was of the same weight with a drop of water.

THE former supposition is commonly made; and though I have not found it to be exactly true, but that a drop of water weighed a tantillum more than a grain; yet, that difference is much more than recompensed by that, which we found between the weight of a drop of water, and the weight of one of spirit of human blood. For having, in a very good and carefully adjusted balance, let fall ten drops of common water, and as many of our rectified spirit of human blood, (as judging it a safer way to make an estimate, by comparing so many drops of each liquor than one alone;) we found, as we might well expect, that a drop of this last named liquor, as it was manifestly lesser, so it was far lighter, than a drop of water, insomuch, that the whole ten drops did not amount to four grains: so that we may safely judge the drop of spirit to have manifestly diffused itself, and acted upon above 4000 times so much water in weight, (and perhaps in bulk too;) since indeed the proportion extended a good way towards that of one to 5000, and so may be said to be, as that of one to between 4000 and 5000; which, though it may seem incredible to those, that are unacquainted with the great subtilty of nature and art, in the comminutions they can make of bodies;

yet,

yet, I can, by repeating the experiment, easily convince a doubter in less than a quarter of an hour. And this subtilty of the parts of blood will appear yet greater, if it be considered, (what I think I can evince,) that no contemptible part of the single drop I employed was phlegm, useless to the change produced, the operation being due to the energy of the saline spirits of the little drop.

The seventh (secondary) TITLE.

Of the odour, taste, colour, and transparence of the spirit of human blood.

THOSE qualities, that, in my opinion, more generally than deservedly are called first, do not any of them belong to the spirit of human blood, in such manner, as to oblige me to say any thing of them in relation to it; and therefore I shall content myself to have made this transient mention of them, to keep it from being thought, that through forgetfulness I had overlooked them: yet, something there is, that may not inconveniently be referred to the heat or coldness of spirit of human blood; in regard that physicians, as well as philosophers, distinguish these qualities into actual and potential; for it seems, that the spirit of human blood is, in reference to some liquors, potentially cold, since it refrigerates them; and, in reference to some others, potentially hot, since, being mingled with them, the mixture becomes actually hot. Of this last, I shall here set down the ensuing instance.

INTO a slender cylindrical glass we put the lower part of an hermetically sealed thermoscope; which, in this paper and elsewhere, I usually call the gaged one, because it was adjusted according to the standard of such instruments kept at *Gresham* college. Into this cylindrical glass we poured as much moderately strong spirit of blood, as would cover the ball of the thermometer, and then dropped on that liquor some good spirit of salt, upon whose mingling with it there was produced a conflict, accompanied with noise and bubbles, and a heat, which nimbly enough made the spirit of wine ascend two inches and a half. This experiment is therefore the more considerable, because there are divers volatile alcalies, that being confounded with acid spirits, though they seem to make a true effervescence, yet do really produce a notable degree of coldness. And that, which to me seemed considerable on this occasion, was, that whereas I had several times found by trial, that the spirit of verdigrease (which some call the spirit of *Venus*) would with the volatile salt of sal-armoniac, or of urine, produce a seeming effervescence, but a real coldness; this spirit of verdigrease itself, being mixed in the forementioned small cylindrical glass, with but moderately strong spirit of blood, did not only produce a hissing noise and store of bubbles, but an actual heat, whereby the spirit of wine in the thermoscope was made quickly to ascend above an inch and a half, though the liquors employed amounted not both together to two spoonfuls.

The eighth (secondary) TITLE.

Of the dissolutive power of spirit of human blood.

IT will not only serve to manifest the subtilty and penetrancy of the spirit of human blood, but it may be also of some use to physicians, if it be made appear by experiments, that this spirit is by itself not only a good medicine for several diseases, (as will be hereafter shewn,) but may be also employed as a menstruum, to dissolve several bodies, and even some metalline ones. And because these last mentioned are the most unlikely to be readily dissoluble, by a substance belonging to the animal kingdom, as chymists speak; I shall subjoin two trials, that I made to evince this dissolutive power of the spirit of blood.

AND

AND firft, we took crude copper in filings, (which if they be very fmall, are fo much the fitter for our purpofe) and having poured on them fome highly rectified fpirit of human blood, we fhook them together; and in about a quarter of an hour or lefs, perceived the menftruum to begin to look a little bluifh, which argued its operation to have already begun: and this colour grew higher and higher, till, after fome hours, the menftruum had diffolved copper enough to make it deeply ceruleous. Some other, and fomewhat differing trials on the fame metal, will be met with in their proper place. In the mean time, I fhall here take notice, that, in fome circumftances, the fpirit of blood has fuch an operation upon copper, whofe quicknefs is furprifing: for having made a coined piece of that metal clean and bright, (that no greafe or foulnefs might hinder the effect of the liquor,) and put a drop or two of our fpirit upon it, within about half a minute of an hour, (obferved by a watch, that fhewed feconds) the verge of the moiftened part of the furface appeared bluifh, and, almoft prefently after, the reft of the wetted part acquired a fine azure colour.

WE alfo took filings of zink, or (as in the fhops they call it) fpelter; and having poured on them very well rectified fpirit of blood, we obferved, that even in the cold it quickly began to work manifeftly, though not vigoroufly: but being affifted with a little heat, it diffolved the zink brifkly, and not without producing ftore of bubbles, being alfo a little difcoloured by the operation. Of this experiment fome ufe is made in another place, and therefore need not be delivered in this.

ON this occafion I fhall add, that for curiofity's fake I took a piece of coagulated blood, but not dried, fomewhat bigger than a large pea, having a care to take it from the lower part of the lump of blood, that it might be black, the fuperficial part of fibrous blood, that lies next the air, being ufually red. This clot of blood we put into a flender vial of clear glafs, that the colour might be the better difcerned; and then poured on it a little rectified fpirit of human blood, and fhook the glafs a little; whereupon, in a trice, the colour of (at leaft) the fuperficial part of the blood, was, as I had conjectured, manifeftly changed, the blacknefs quite difappearing, and being fucceeded by a very florid colour, like that of fine fcarlet. The liquor alfo was tinged, but not with near fo deep or fo fair a red; and by the little bubbles, that from time to time paft out of the clot into it, it feemed to work like a menftruum: and yet, foon after coming to look upon this lump of blood again, I found it to have much degenerated from its former colour, to one lefs fair and more dark,

WE took alfo another clot of blood like the former, fave that one part of it, which had lain next the air, was not black; and having, in a vial like the former, poured on it fome fpirit of blood, taken out of the fame vial, whence I took the firft parcel, the reddifh colour feemed prefently to be much improved, and made more fair, and like true fcarlet: but the black was not fo altered, as to be deprived of its blacknefs, but retained a dark and dirty colour. So that this fecond experiment requires a further trial, when there fhall be conveniency to make it; and it will the rather deferve one, becaufe what has been already recited of the operation of the fpirit upon the two parcels of blood, may fuggeft uncommon reflections to fpeculative wits.

AND here, on this occafion, it will be proper to relate to you, that having a confufed remembrance, that I had a great while before put up fome human blood, with a certain quantity of volatile fpirit, to keep it fluid and preferve it, without diftinctly remembering what volatile alcali I had employed; I found among other glaffes, that had been laid afide, one bolt-head with a long neck, to which was tied a label, importing, that at fuch a time twelve drams of human blood were put up with two drams of fpirit of human blood. By the date of this paper it appeared, that this blood had been preferved much above a whole twelve month; and yet it appeared, through the glafs,

of

of a fine florid colour, and feemed to be little lefs than totally fluid. And indeed when we came to open the veffel, which was carefully ftopt with a good cork and hard fealing-wax, we found no ill fcent or other fign of putrefaction in the mixture; and but a very fmall portion of blood lightly clotted at the bottom; the reft paffing readily through a rag. So that the fpirit of human blood feems to have a great embalming virtue; fince it was able fo long and well to preferve fix times its weight of a body fo apt to concrete and putrify, as human blood is known to be, and probably would have preferved it much longer, if we had thought fit to profecute the experiment. To this account of our trial I know not whether it will be worth while to add, that having broken it off, that we might diftill the above-mentioned mixture with a very gentle heat; the firft liquor, that afcended, was not a fpirit, but a kind of phlegm, though afterwards there came up, befides a fpirituous liquor, a volatile falt in a dry form.

ON this occafion I fhall fubjoin the following trial, long fince made with a fpirit, that I fuppofed to have been weaker than that, with which the lately mentioned experiments were made.

IN order to a defign, that need not here be mentioned, I caufed fome filings of mars to be purpofely made, that, being prefently employed, they might not contract any ruft, whereby the operation of our liquor might be made doubtful. On thefe we poured fome of our fpirit, and having kept them together a while in digeftion, we found, as we expected, that the liquor had wrought on the metal, and produced a confiderable quantity of a light fubftance, in colour almoft like crocus, but fomething paler: and we alfo found more than we expected; for there appeared in the liquor good ftore of thin plates, like a kind of *terra foliata*, (as the chymifts fpeak) which, after a very flight agitation, being held againft the fun-beams, exhibited the colours of the rainbow in fo vivid a manner, as did not a little delight, as well as furprife the fpectators; but I did not perceive, that the tafte of the liquor was confiderably martial.

The ninth (fecondary) TITLE.

Of the tinctures, that may be drawn with fpirit of human blood.

MOST of thofe extractions the chymifts call tinctures, being, as I have elfewhere fhewn, partial folutions of the bodies from which they are obtained, it will, I prefume, be eafily granted, that fince the fpirit of blood is able (as in the foregoing title it has appeared to be) to diffolve copper and zink, that are folid and metaline bodies, it will be able to extract tinctures out of divers others: but, that this power of our *menftruum* may be rather proved than fuppofed, it will not be amifs to add a few inftances of it.

SPIRIT of blood being put upon Englifh faffron, did foon acquire upon it a fine yellow colour.

SPIRIT of blood being put upon powdered *curcuma*, or, as tradefmen are wont to call it, turmerick, did, in the cold, extract from it a lovely tincture, like a rich folution of gold; which probably (to intimate that upon the by) may prove a good deobftruent medicine, particularly in the jaundife; in which difeafe turmerick, that is taken to be a kind of Eaft-Indian faffron, is, upon experience, commended, and in this our tincture is united with fpirit of human blood, which is very near of kin to fpirit of urine, and probably at leaft as efficacious; with which liquor, when well rectified, I have had more than ordinary fuccefs in the jaundife.

To make fome trial of the extracting power of the fpirit of blood upon fubftances, that have belonged to animals, I thought it might particularly conduce to fome medical purpofes, to try, what it would do upon the folid part of human blood itfelf flowly dried,

dried, so as not to be burned, but only to be reducible, with some pains, to fine powder. Accordingly, upon this well-sifted powder of blood, we put some moderately strong spirit of the same subject, on which the liquor began very soon to colour itself, even in the cold; and within no long time after, it appeared as red as ordinary French claret wine. This extraction made me suspect, that the phlegm, that was not carefully separated from the spirit I then employed, might hasten the coloration of the *menstruum*: for which reason I put upon another portion of the same powder some rectified spirit of blood, so well dephlegmed, that it would not dissolve a grain of the volatile salt of blood; and I found indeed, as I suspected, that this *menstruum* did not any thing near so soon draw a tincture, as the other had done; for after divers hours, the colour it obtained was but brown; but, after some hours longer, the colour appeared to be heightened into redness, but yet manifestly inferior to that of the somewhat phlegmatick spirit above-mentioned, whereto it did yet, in a longer time, grow almost equal. By this means we may not only disguise the spirit of blood, but impregnate it with the finer parts of the unanalysed solid body, which may possibly make the spirit a remedy more proper for some diseases or constitutions: and this medicine I sometimes call the intire tincture of human blood, because it consists of nothing else but such blood.

To shew, at length, that the spirit of human blood may extract tinctures out of some of the hardest bodies, I made the following experiment.

WE took some choice filings of steel (for such are those, that are saved by the needle-makers) and having put them into a small egg, we poured on them some highly rectified spirit of blood, and kept them all night in digestion in a moderate heat. The next day (but not early) we found the *menstruum* turned of a brownish red colour, that was deep enough. And some of the filings, that chanced to stick to the sides of the glass, but were higher than the liquor could reach in its gross body, seemed to have been, either by exhalations from the *menstruum*, or perhaps by the transient contact of it, as it was pouring in, turned into a kind of yellow crocus martis. I must not here forget, that having kept the *menstruum* and the filings together in the forementioned egg, for some days longer, the colour was grown opacous, and appeared to be black, when it was looked on in any considerable bulk; this last expression I employ, because it had another appearance, when it was somewhat thinly spread upon white paper.

PERHAPS it may be a remark not altogether useless to physicians, among many of whom chalybeate remedies are in very great request, if I add, that for reasons not needful to be mentioned here, having a suspicion, that our spirit would work upon steel, in another manner than the acid solvents wont to be used by chymists and physicians; we poured some of our tincture, drawn from filings of steel, upon a freshly drawn tincture of galls, (infused in common water) and did not find, that this liquor would, with the infusion, make any inky mixture, nor that the precipitate, that was quickly produced, was of a black, much less of a true inky colour; though I have found means to produce, in a trice, a black mixture, with other martial solutions and tinctures, which, for curiosity's sake, I sometimes made green, sometimes red, sometimes yellow, and sometimes, if I mistake not, of neither of those colours.

I have been the more express, in setting down the particulars above delivered, because, I hope, they may be somewhat helpful to rectify the judgment of divers very ingenious modern physicians, especially among the cultivaters of chymistry, who build much upon a supposition, which, though I deny not to be specious, I doubt is not solid, and I fear may be of ill consequence. For, by the above recited trials, it may appear, that it is unsafe either to suppose, that if chalybeates be dissolved in the body, it must be by some acid juice; or, to conclude, that if the steel be dissolved by the

 liquors

of a fine florid colour, and ſeemed to be little leſs than totally fluid. And indeed when we came to open the veſſel, which was carefully ſtopt with a good cork and hard ſealing-wax, we found no ill ſcent or other ſign of putrefaction in the mixture; and but a very ſmall portion of blood lightly clotted at the bottom; the reſt paſſing readily through a rag. So that the ſpirit of human blood ſeems to have a great embalming virtue; ſince it was able ſo long and well to preſerve ſix times its weight of a body ſo apt to concrete and putrify, as human blood is known to be, and probably would have preſerved it much longer, if we had thought fit to proſecute the experiment. To this account of our trial I know not whether it will be worth while to add, that having broken it off, that we might diſtill the above-mentioned mixture with a very gentle heat; the firſt liquor, that aſcended, was not a ſpirit, but a kind of phlegm, though afterwards there came up, beſides a ſpirituous liquor, a volatile ſalt in a dry form.

On this occaſion I ſhall ſubjoin the following trial, long ſince made with a ſpirit, that I ſuppoſed to have been weaker than that, with which the lately mentioned experiments were made.

In order to a deſign, that need not here be mentioned, I cauſed ſome filings of mars to be purpoſely made, that, being preſently employed, they might not contract any ruſt, whereby the operation of our liquor might be made doubtful. On theſe we poured ſome of our ſpirit, and having kept them together a while in digeſtion, we found, as we expected, that the liquor had wrought on the metal, and produced a conſiderable quantity of a light ſubſtance, in colour almoſt like crocus, but ſomething paler: and we alſo found more than we expected; for there appeared in the liquor good ſtore of thin plates, like a kind of *terra foliata*, (as the chymiſts ſpeak) which, after a very ſlight agitation, being held againſt the ſun-beams, exhibited the colours of the rainbow in ſo vivid a manner, as did not a little delight, as well as ſurpriſe the ſpectators; but I did not perceive, that the taſte of the liquor was conſiderably martial.

The ninth (ſecondary) TITLE.

Of the tinctures, that may be drawn with ſpirit of human blood.

Most of thoſe extractions the chymiſts call tinctures, being, as I have elſewhere ſhewn, partial ſolutions of the bodies from which they are obtained, it will, I preſume, be eaſily granted, that ſince the ſpirit of blood is able (as in the foregoing title it has appeared to be) to diſſolve copper and zink, that are ſolid and metaline bodies, it will be able to extract tinctures out of divers others: but, that this power of our *menſtruum* may be rather proved than ſuppoſed, it will not be amiſs to add a few inſtances of it.

Spirit of blood being put upon Engliſh ſaffron, did ſoon acquire upon it a fine yellow colour.

Spirit of blood being put upon powdered *curcuma*, or, as tradeſmen are wont to call it, turmerick, did, in the cold, extract from it a lovely tincture, like a rich ſolution of gold; which probably (to intimate that upon the by) may prove a good deobſtruent medicine, particularly in the jaundiſe; in which diſeaſe turmerick, that is taken to be a kind of Eaſt-Indian ſaffron, is, upon experience, commended, and in this our tincture is united with ſpirit of human blood, which is very near of kin to ſpirit of urine, and probably at leaſt as efficacious; with which liquor, when well rectified, I have had more than ordinary ſucceſs in the jaundiſe.

To make ſome trial of the extracting power of the ſpirit of blood upon ſubſtances, that have belonged to animals, I thought it might particularly conduce to ſome medical purpoſes, to try, what it would do upon the ſolid part of human blood itſelf ſlowly dried, ſo as not to be burned, but only to be reducible, with ſome pains, to fine powder. Accordingly, upon this well-ſifted powder of blood, we put ſome moderately ſtrong ſpirit of the ſame ſubject, on which the liquor began very ſoon to colour itſelf, even in the cold; and within no long time after, it appeared as red as ordinary French claret wine. This extraction made me ſuſpect, that the phlegm, that was not carefully ſeparated from the ſpirit I then employed, might haſten the coloration of the *menſtruum:* for which reaſon I put upon another portion of the ſame powder ſome rectified ſpirit of blood, ſo well dephlegmed, that it would not diſſolve a grain of the volatile ſalt of blood; and I found indeed, as I ſuſpected, that this *menſtruum* did not any thing near ſo ſoon draw a tincture, as the other had done; for after divers hours, the colour it obtained was but brown; but, after ſome hours longer, the colour appeared to be heightened into redneſs, but yet manifeſtly inferior to that of the ſomewhat phlegmatick ſpirit above-mentioned, whereto it did yet, in a longer time, grow almoſt equal. By this means we may not only diſguiſe the ſpirit of blood, but impregnate it with the finer parts of the unanalyſed ſolid body, which may poſſibly make the ſpirit a remedy more proper for ſome diſeaſes or conſtitutions: and this medicine I ſometimes call the intire tincture of human blood, becauſe it conſiſts of nothing elſe but ſuch blood.

To ſhew, at length, that the ſpirit of human blood may extract tinctures out of ſome of the hardeſt bodies, I made the following experiment.

WE took ſome choice filings of ſteel (for ſuch are thoſe, that are ſaved by the needle-makers) and having put them into a ſmall egg, we poured on them ſome highly rectified ſpirit of blood, and kept them all night in digeſtion in a moderate heat. The next day (but not early) we found the *menſtruum* turned of a browniſh red colour, that was deep enough. And ſome of the filings, that chanced to ſtick to the ſides of the glaſs, but were higher than the liquor could reach in its groſs body, ſeemed to have been, either by exhalations from the *menſtruum*, or perhaps by the tranſient contact of it, as it was pouring in, turned into a kind of yellow crocus martis. I muſt not here forget, that having kept the *menſtruum* and the filings together in the forementioned egg, for ſome days longer, the colour was grown opacous, and appeared to be black, when it was looked on in any conſiderable bulk; this laſt expreſſion I employ, becauſe it had another appearance, when it was ſomewhat thinly ſpread upon white paper.

PERHAPS it may be a remark not altogether uſeleſs to phyſicians, among many of whom chalybeate remedies are in very great requeſt, if I add, that for reaſons not needful to be mentioned here, having a ſuſpicion, that our ſpirit would work upon ſteel, in another manner than the acid ſolvents wont to be uſed by chymiſts and phyſicians; we poured ſome of our tincture, drawn from filings of ſteel, upon a freſhly drawn tincture of galls, (infuſed in common water) and did not find, that this liquor would, with the infuſion, make any inky mixture, nor that the precipitate, that was quickly produced, was of a black, much leſs of a true inky colour; though I have found means to produce, in a trice, a black mixture, with other martial ſolutions and tinctures, which, for curioſity's ſake, I ſometimes made green, ſometimes red, ſometimes yellow, and ſometimes, if I miſtake not, of neither of thoſe colours.

I have been the more expreſs, in ſetting down the particulars above delivered, becauſe, I hope, they may be ſomewhat helpful to rectify the judgment of divers very ingenious modern phyſicians, eſpecially among the cultivaters of chymiſtry, who build much upon a ſuppoſition, which, though I deny not to be ſpecious, I doubt is not ſolid, and I fear may be of ill conſequence. For, by the above recited trials, it may appear, that it is unſafe either to ſuppoſe, that if chalybeates be diſſolved in the body, it muſt be by ſome acid juice; or, to conclude, that if the ſteel be diſſolved by the liquors of the body, it muſt be, *ex prædominio*, (as they ſpeak) alcaliſate; ſince a liquor, that exerciſes a great hoſtility againſt acids, diſſolves it: and, by parity of reaſon, one may probably infer the quite contrary of what they ſuppoſe; in regard that ſteel, in our experiment, was (partially at leaſt) diſſolved by what they call an alcali; and conſequently ought to be, *ex prædominio*, of an acid nature. But of this hypotheſis we elſewhere purpoſely diſcourſe, and therefore ſhall here add nothing concerning it, but leave it to be conſidered, whether it would not be requiſite to ſeek out ſome other way, than phyſicians have hitherto pitched on, to explicate the manner of operation of chalybeate medicines in the human body; and whether ſome uſe may not be made in medicine of martial remedies prepared by volatile alcalies, inſtead of acids.

I put ſome ſpirit of human blood upon powdered amber, ſifted through a fine ſieve, and kept it in digeſtion for ſome days, giving it a pretty degree of heat; but we obtained not hereby any tincture at all conſiderable; whether it was, that the ſpirit was not yet highly enough rectified, or that the amber (which was of a finer ſort of white amber) was not ſo proper to yield its tincture, as I have ſeveral times found courſer, but deeper coloured amber, to be.

To this (ninth) title may be referred the event, that followed, upon our having put ſome ſpirit of human blood upon that ſort of gum *laccæ*, that comes out of the *Eaſt-Indies* in grains, and (for that reaſon) is commonly called ſeed-lace: for the ſpirit we put upon this, though this be a reſinous gum, and of no eaſy ſolution, ſoon became tincted; which I expected it ſhould, becauſe I conjectured, that the redneſs, wont to appear in many of the ſeed-like grains, is but ſuperficial, and proceeds from ſome adhering blood of little (winged) inſects, that, by their bitings, occaſion the production of this gum upon the twigs of the tree where the lac is found; on which twigs I have more than once ſeen ſtore of theſe gummous grains. So that the tincture ſeems not to be drawn from the lac itſelf, but rather to be afforded by the blood of theſe little animals, which the ſpirit of human blood, that will draw tinctures from dried man's blood, diſſolves; and this tincture may probably be a good medicine, ſince moſt of the inſects, uſed in phyſick, as *millepedes*, lice, bees, ants, *&c.* even in our colder climates, afford medicines of very ſubtil and piercing parts, and of conſiderable efficacy.

The tenth (ſecondary) TITLE.

Of the coagulating power of the ſpirit of human blood.

THOUGH the ſpirit of human blood have ſuch a diſſolving power, as we have mentioned, in reference to ſome bodies, yet, upon ſome others, it ſeems to have a quite contrary operation. I ſay *ſeems*, becauſe it may be queſtioned, (and I am not now minded to diſpute it) whether the effect I am going to ſpeak of be a coagulation, properly ſo called, that one body makes of another, or a coalition of particles fitted, when they chance to meet one another, (in a convenient manner) to ſtick together. But whatever name ought to be properly given to the thing I am about to ſpeak of, I have found, by trial purpoſely made, that the highly rectified ſpirit of human blood being well mingled, by ſhaking, with a convenient quantity (which ſhould be at leaſt equal) of vinous ſpirits, that will burn all away, (for if either of the liquors be phlegmatic, the experiment ſucceeds either not at all, or not ſo well) there will preſently enſue a coagulation or concretion, either of the whole mixture, or a great portion of it, into corpuſcles of a ſaline form, that cohering looſly together, make up a maſs, that has conſiſtence enough not to be fluid, though it be very ſoft: and in this form it may remain, as far as I have yet tried, for a good while, perhaps ſeveral months, or weeks at leaſt, if it be kept in a cool place.

The

The eleventh (ſecondary) TITLE.

Of the precipitating power of ſpirit of human blood.

OF the precipitating power of ſpirit of human blood, I have yet obſerved nothing, that is peculiar; and therefore it may ſuffice to ſay in general, that, as far as I have had occaſion to try, it has, in common with thoſe other volatile ſpirits, which I elſewhere call urinous, a power of precipitating moſt bodies, that are diſſolved in acid *menſtruums.* I ſay *moſt*, becauſe (as I have elſewhere more fully ſhewn) it is an error, though a vulgar one, to ſuppoſe (as chymiſts and phyſicians are wont to do) that whatever is diſſolved by an acid, will be precipitated by an alcali, as ſuch, whether fixed or volatile, which latter ſort they take the ſpirits of urine, blood, *&c.* to be of: for there is no neceſſity this rule ſhould hold, when the body is of ſuch a nature, that it may be diſſolved as well by an alcali, as by an acid. And, though the hypotheſis of *alcali* and *acidum* allowed them not to think there were any ſuch bodies, yet I have in another paper experimentally evinced, that there are ſo: and it may be proved, without going very far, ſince we lately obſerved, * that good ſpirit of human blood would, in the cold, diſſolve both copper and zink, which are bodies, that will each of them be readily diſſolved by aqua-fortis and ſome other acid menſtruums.

* See the eighth Title.

BATING ſuch bodies as thoſe I have been ſpeaking of, I have not found, but that ſpirit of human blood precipitates other bodies diſſolved in acid menſtruums, much after the ſame manner, that ſpirit of urine and other ſuch volatile alcalies are wont to do. Of this, among other inſtances, I remember, that I made trial upon red-lead, or minium, diſſolved in the acid ſalt of vinegar, ſilver in aqua-fortis, gold in aqua-regia, and tin diſſolved in an appropriated menſtruum. I alſo, with our ſpirit, precipitated the ſolutions of divers other bodies, which need not here be named. But in regard of the great and frequent uſe, that men make of ſea ſalt, in preſerving and ſeaſoning what they eat, it may not be amiſs particularly to mention, that out of a ſolution of common ſalt, made in common water, we could readily precipitate, with the ſpirit of blood, a ſubſtance, that looked like a white earth; and ſuch a ſubſtance I obtained in far greater quantity from that, which the ſalt-makers call bittern, which uſually remains in their ſalt pans, after they have taken out as much, or near as much ſalt, as would coagulate in figured grains.

THE ſpirit of human blood does alſo make a precipitation of Dantzick vitriol diſſolved in water, but not, that I have obſerved, a total one; which you need not wonder at, becauſe it will diſſolve copper, which is one of the ingredients of blue vitriol.

The twelfth (ſecondary) TITLE.

Of the affinity between ſpirit of human blood, and ſome chymical oils, and vinous ſpirits.

THOUGH in another † paper I declare myſelf, for reaſons there expreſſed, diſſatisfied with the vulgar notions of ſympathy, antipathy, friendſhip, affinity, hoſtility, *&c.* that are preſumed to be found among inanimate bodies, yet, in this place, nothing forbids to employ the terms affinity, cognation, and hoſtility, in the lax and popular ſenſe, wherein they are uſed, not only by the vulgar, but by ſchool-philoſophers and chymiſts.

† About the mechanical origine or production of qualities.

IT ſeems then, according to this acception of the word affinity, that there is ſuch a thing between rectified ſpirit of human blood and pure ſpirit of wine; ſince we have formerly (under the tenth title) obſerved, that, being put together, they will readily concoagulate, and continue united a long time. It is very probable, that the like aſſociation may be alſo made with other ardent ſpirit prepared by fermentation.

WE have likewiſe formerly noted, that our ſpirit will make a ſolution of the finer parts of human blood well dried ; which inſtance I mention, on this occaſion, becauſe it ſeems to be the effect of ſome affinity or cognation, (as moſt men would call, what I would call mechanical congruity) between the ſpirit and the body it works on, in regard I found, by more than one trial purpoſely made, that a highly rectified vinous ſpirit (for if it be phlegmatic, the water may diſſolve ſome of the blood) would not (at leaſt in divers hours, that my trials laſted) draw any tincture from it.

WITH lixiviate liquors, ſuch as are made of ſalt of tartar, fixed nitre, &c reſolved in the air, or otherwiſe, the chymiſt will expect, that the ſpirit of blood ſhould have an affinity, ſince they eſteem all theſe liquors alcalies, though this be volatile, and thoſe be fixed. But though theſe liquors comport well with one another, yet we find not, that they ſtrictly aſſociate by concoagulation, as we lately obſerved the ſpirit of blood to do with ſpirit of wine.

See the producibility of chymical principles.

THE ſame ſpirit of blood mingles readily with that ſpirit of vegetables, that I have elſewhere given a large account of under the title of adiaphorous ſpirit; which argues, that there is ſome affinity between them, or rather, that there is not any manifeſt hoſtility or contrariety.

THE like relation may be found between ſpirit of blood and many other liquors, which it were needleſs and tedious to enumerate. It may better deſerve the conſideration of a chymiſt, that though there is manifeſtly a near cognation between the ſpirit of human blood and the oil, ſince they both proceed immediately from the ſame body, yet even dephlegmed ſpirit of blood, being ſhaken, and thereby confounded with its oil, will quickly ſeparate again from it; though with ſpirit of wine (which is, according to the chymiſts, a liquid ſulphur, as well as the oil) it will permanently unite, notwithſtanding, that theſe two liquors do (to ſpeak in their language) belong even to differing kingdoms, the one to the animal, and the other to the vegetable.

WITH the eſſential oils (as chymiſts call them) of aromatic vegetables, or at leaſt with ſome of them, the well rectified ſpirit of human blood ſeems to have a greater affinity. For having taken a dram of this liquor, and an equal weight of oil of anniſeeds, drawn in a limbeck, [*per veſicam*] and ſhaken them well together, they made a ſoft, or ſemi fluid white coagulum, that continued in that form for a day or two, and probably would have longer done ſo, if I had not had occaſion to proceed further with it.

IT may not be impertinent, on this occaſion, to take notice, that becauſe I preſumed, that, though ſpirit of blood would not totally mix with eſſential oils, (as chymiſts call them) it might either communicate ſome ſaline parts to them, or work a change in them ; I digeſted a while, in a glaſs with a long neck, ſome rectified ſpirit of human blood, with a convenient quantity of oil of anniſeeds drawn in a limbeck, and found, as I expected, that the oil grew coloured of a high yellow, and afterwards attained to a redneſs; which experiment I the rather mention, becauſe it may poſſibly afford you a hint about the cauſe of ſome changes of colour, that are produced in ſome of the liquors of the body.

UPON the forementioned affinity, or congruity, of the ſpirit of blood, with that of wine, and with (ſome) eſſential oils, I founded a way of taking off the offenſive ſmell of ſpirit of human blood, which is the only thing, that is likely to keep the more delicate ſort of patients from employing ſo uſeful a medicine, as this will hereafter appear to be. But, to deal with a philoſophical candour, I muſt not conceal from you, that, till experience ſhall be duly conſulted, I ſhall retain a doubt, whether the way employed

ployed to deprive our ſpirit of its ſtink, will not alſo deprive it of part of its efficacy: but, on the other ſide, I conſider it as a thing probable enough, that theſe aromatiſed ſpirits may, by being impregnated with many of the finer parts of the oils employed to correct their odour, be likewiſe endowed with the virtues of thoſe oils, which are liquors, that chymiſts not improbably believe, to conſiſt of the nobleſt parts of the vegetables, that afford them.

To aromatiſe the ſpirit of human blood, we employed two differing ways, the firſt whereof was this: we took a convenient quantity of well rectified ſpirit of blood, and having put it into a glaſs egg, we added to it as much, or (what may in many caſes more than ſuffice) half as much eſſential oil of anniſeeds, for inſtance; and having ſhaken theſe liquors together to mingle them very well, we placed the glaſs in a fit poſture in a furnace, where it ſhould not have too great a heat: by which means, the ſlight texture of the coagulum being diſſolved, part of the oil (ſometimes a great portion of it) appeared by itſelf floating at the top of the ſpirit; whence being ſeparated by a tunnel or otherwiſe, the remaining liquor was whitiſh and without any ſtink; the ſmell predominant in it being that of the anniſeeds, of which it taſted ſtrongly, though the ſaline ſpirituous part of the blood did in this liquor retain a not inconſiderable degree of their briſk and penetrant taſte.

THE other way I thought of to aromatiſe our ſpirit of blood, was by employing a medium to unite it with eſſential oils: for which purpoſe, in a vinous ſpirit ſo dephlegmed, that in a ſilver ſpoon it would totally burn away, we diſſolved, by ſhaking a convenient proportion, as an eighth part, or a far leſs (according to the ſtrength of the oil) of an eſſential oil (of anniſeeds, for inſtance;) and to this ſolution we added an equal quantity, or ſome other convenient one, of our rectified ſpirit of blood; and having, by ſhaking them, mixed them as well as we could, we ſuffered the expected coagulum (which was ſoft and not uniform) to reſt for ſome time, after which it appeared, that ſome of the oil was revived, and ſwam in drops diſtinct from the other liquor, which conſiſted of a mixture of the two ſpirits, impregnated with the particles of the oil they had intercepted and detained. This liquor abounded with little concretions, made by the con-coagulation of the ſanguineous and vinous ſpirits, and theſe, with a very gentle heat, ſublimed in the form of a volatile ſalt, to the upper part of the glaſs; which ſalt ſeemed to have a much leſs penetrating odour, than the meer volatile ſalt of human blood, but had quite loſt its ſtink, and yet retained a conſiderable quickneſs, and ſomewhat of the ſcent of the anniſeeds; the remaining liquor alſo was deprived of its ill ſmell, and moderately imbued with that of the oil.

I thought it worth trying, whether there would be any affinity between our ſpirit (which I perceived contained in it many latent particles of an oleaginous nature) and the highly rectified oil of petroleum, which is a mineral bitumen: and having ſhaken together a convenient quantity of theſe two liquors in a new vial, they preſently turned into a white mixture. And though, after it had for many hours been left to ſettle, the greater part of the oil ſwam above the ſpirit, yet there appeared betwixt the two liquors a good quantity of a whitiſh matter; which ſeemed to be ſomething, that had been produced by the precipitation or union of many particles of the ſpirit and oil, that were more diſpoſed than the reſt to combine with one another.

The thirteenth (ſecondary) TITLE.

Of the relation between ſpirit of human blood and the air.

THAT the contact of the air has a ſpeedy and a manifeſt operation upon human blood, is elſewhere ſhewn by ſome experiments of an Italian virtuoſo, ſignior —— and ſome of mine. But whether, after human blood has had its texture ſo much altered,

tered, as it uſes to be by diſtillation, it will retain any peculiar relation to the air, I have not been able to make trials enough to determine; but, however, it will not be amiſs to ſet down the chief experiments I made on this occaſion, becauſe they may be conſiderable, as parts of our hiſtory, though they ſhould not be ſo, as arguments deciſive of our controverſy.

THE firſt experiment was quickly made, by thinly ſpreading upon a piece of white paper, (which ought to be cloſe, that it may not ſoak up the liquor) ſome ſmall filings of copper, and wetting them well, without covering them quite over, with a few drops of ſpirit of blood; for, by this means, being very much expoſed to the free air, the action of the liquor was ſo much promoted, that within a minute or two it did, even in the cold, begin to acquire a bluiſh colour, and in fewer minutes, than one would have expected, that colour was ſo heightened, as to become ceruleous: but, when I put another parcel of the ſame filings into a vial, and covered them with ſpirit of blood, and then ſtopt the vial, to keep it from intercourſe with the external air, the liquor would not in ſome hours acquire ſo deep a colour.

THE other experiment we made, in order to the late propoſed inquiry, was the ſame for ſubſtance, that I had formerly made, (and have elſewhere at large delivered) with the ſpirit of urine, and with that of ſal-armoniac, ſave that, to ſpare our ſpirit of blood, we employed a far leſs quantity of it, than we did of either of the forementioned liquors. For, having, in a clear cylindrical vial of about an inch diameter, put more filings of copper than were requiſite to cover the bottom, we poured upon it but ſo much ſpirit of human blood, as ſerved to ſwim a finger's breadth, or about an inch, above them. This liquor, becauſe of the quantity of air, that was contained in the vial, did within few hours acquire a rich blue colour; and this, after a day or two, began to grow more faint, and continued to do ſo more and more, till it came to be almoſt loſt; but, yet the liquor was not altogether lympid, or colourleſs, as I have often had it with ſpirit of urine, or of ſal armoniac; which remains of bluiſhneſs I was apt to attribute to the great quantity of air, that was included in the vial with ſo ſmall a quantity of liquor: and though I thought it not impoſſible, but that length of time might deſtroy theſe remains of bluiſhneſs alſo, yet, not having leiſure to wait ſo long, I unſtopped the vial, and perceived, as I expected, that in a very ſhort time, perhaps about two minutes of an hour, the ſurface of the liquor, where it was touched by the newly entered air, became ceruleous, and in a ſhort time after, perhaps leſs than a quarter of an hour, the whole body of the liquor had attained a deeper colour than that of the ſky; which colour, the vial being ſeaſonably and carefully ſtopped, began in two or three days to grow paler again.

THESE experiments would, I queſtion not, to many ſeem manifeſtly to infer a great cognation or affinity (for I know not well what name to give it) between the ſpirit of human blood and the air. But, though I ſhall not deny the concluſion, as it is an aſſertion, I dare not rely on the validity of the inference; becauſe, I have, for curioſity's ſake, made the like experiments ſucceed with other ſpirits abounding with volatile ſalt. I foreſee it may very ſpeciouſly be pretended, that thoſe trials ſucceeded upon the account of ſome ſpirituous parts of the blood, ſince ſpirit of urine is made of a liquor ſeparated from the blood; and that, though the ſal-armoniac that is made in the *East*, may conſiſt in great part of camel's urine, yet, that, which is made in *Europe*, (where camels are rarities) and is commonly ſold in our ſhops, is made of man's urine, and conſequently its ſpirit may well be preſumed to be impregnated with ſpirit of human blood. And I confeſs, that when this conſideration came firſt into my mind, it appeared ſo probable, that I ſhould perhaps have acquieſced in it, if it were not for what I am going to ſubjoin; namely, that I found by trial, carefully made, that with another

another volatile ſpirit, made without any ſubſtance, that is afforded by the body of man, I could, with filings of copper, make an experiment very analogous to that above related. But, becauſe, in this trial the reiterated contact of the air produced, in the liquor, not a ceruleous, but a green colour, I am willing to ſuſpend my judgment about the problem lately propoſed, till experience ſhall have further informed me.

I know not, whether it will be worth while to relate, that having, in an unſtopped glaſs, put ſome ſpirit of human blood into a receiver placed upon our pneumatic engine, and withdrawn the incumbent air by pumping; the ſpirit of blood ſeemed to afford leſſer and fewer aerial bubbles, than ſuch a quantity of common water itſelf would probably have done: but, as I lately intimated, I know not, whether this obſervation be conſiderable, becauſe, being not willing to weaken, by expoſing it, a freſh parcel of ſpirit, I know not, whether the paucity of air, obſerved in that lately mentioned, were accidental or not.

The fourteenth (ſecondary) TITLE.

Of the hoſtility of the ſpirit of human blood to acids, whether they be in the form of liquors or fumes.

THAT there is in the ſpirit of human blood ſuch a thing, as a chymiſt or a vulgar philoſopher would call hoſtility, or an antipathy in reference to acids, has been plainly enough, though very briefly, intimated in a paſſage belonging to the third of the precedent titles. But, yet, it may not be impertinent to add in this place, that our ſpirit of human blood exerciſes this hoſtility againſt more than one ſort of acid ſpirits, though perhaps they differ not a little from one another, as ſpirit of ſalt, ſpirit of nitre, ſpirit and oil of vitriol, *aqua-fortis*, *aqua-regia*, *&c.* and not only againſt factitious acids, but againſt natural ones too, the ſpirit of human blood may diſcover a manifeſt hoſtility, as I found by the conflict it would make with newly expreſſed juice of lemons, which it would put into a confuſed agitation, accompanied with bubbles. And this was yet the more evident, when I employed the volatile ſalt of blood, that is, the ſpirit in a dry form: for having ſqueezed upon a parcel of this ſome juice of lemons, there was preſently excited a great commotion, accompanied not only with froth, but with noiſe. But (to return to the ſtrongly acid liquors made by diſtillation) whether the great commotion, and froth, and hiſſing noiſe, that uſually follows upon the mixture of ſpirit of human blood with any of theſe menſtruums, do proceed from a true hoſtility, or an antipathy deſervedly ſo called, or elſe by a motion to coaleſcence or union; or an effect of the diſturbed motions proper to the differing, but now confounded, liquors; or laſtly, a conſequent of ſome impediment, which the new texture of the mingled liquors gives to the free paſſage of ſome æthereal or other ſubtil permeating matter or fluid, I ſhall not take upon me to determine; but rather, to what I lately told you of the at leaſt ſeeming contrariety of the ſpirit of human blood to acid ſpirits, I ſhall add, (what perhaps you did not expect) that this hoſtility extends even to the inviſible effluvia or emanations of theſe liquors, as may be readily ſeen in the following way, that I long ſince pitched upon to make it not only viſible but manifeſt.

THIS is eaſily done by putting any ſtrong acid ſpirit, as of ſalt, or of nitre, *&c.* into a vial ſomewhat wide-mouthed, and ſome well dephlegmed ſpirit of blood into another; for when I purpoſely inclined theſe glaſſes ſo towards one another, that their lips did almoſt touch, and their reſpective liquors were ready to run out, though neither of the liqnors did at all viſibly fume whilſt they were kept aſunder, though the glaſſes were unſtopped, yet, as ſoon as the liquors came to be approached in the way juſt now mentioned, the fumes meeting each other in the air would make coalitions, which would be manifestly visible in the form of ascending smoke, which was wont at first to surprize the delighted spectators; and this production of smoke would continue a good while, if the vials were not severed to make it cease, which, upon their remove, it would presently do. I have divers times practised a more easy way of making these fumes conspicuous; but it belongs more to another paper, and what has been now delivered may suffice for my present purpose.

YET, it may not be improper to take this occasion to acquaint you with an experiment, that I made to observe what the contrary salts, that abound in our spirit of blood and in some acid liquors, would produce, when they were combined and brought into a dry form. I shall therefore annex a transcript of the experiment I speak of as I find it registred in one of my note-books.

[WE took some pure volatile salt of human blood, and having just satiated it with spirit of nitre, we slowly evaporated away the superfluous moisture, that the acid and urinous salts might be united into a dry concretion, from which my design was to separate them again, the salt of blood in its pristine form, and the spirit of nitre in the form of salt-petre. To effect this, we put the compounded salt into a small bolt head with a long and slender neck, and then added to it a convenient quantity of salt of tartar, and as much distilled water, as would suffice to make the mixture somewhat liquid, to promote the action of the contrary salt, upon one another. By which mutual actions we supposed, that the saline spirits of nitre, being more congruous to the fixed salt than to the volatile, would forsake the salt of blood, (which it detained before from flying away,) and give it leave to sublime; and accordingly, having kept the glass, wherein the mixture was made, for a competent time in a convenient heat, we obtained what we looked for, since a good proportion of fine volatile salt ascended, in a dry form, into the neck.]

HAVING put to some of the spirit of human blood a small quantity of exceeding strong spirit of nitre, there was, upon the conflict of the two liquors, excited so great a quantity of thick white fumes, that I could not but wonder at it, having never seen any thing of that kind comparable to it. And these fumes circulating long in the cavity of the glass, whereof perhaps a tenth part was full of liquor, did many of them, though the vessel were wide-mouthed, fall back and run down the sides of the glass into the stagnant mixture, as if they had composed streams of a milky liquor. And when at length, after these fumes had disappeared, we dropt in a little more of the same smoking spirit of nitre, the like strange plenty of white exhalations did presently ensue, and continue to circulate a great while in the open glass, the mixture, in the mean while, appearing reddish. Being settled, and seeming to have been so discoloured by a fattish substance, we put to it a little rain or distilled water, and having by filtration separated it from the fæces, and slowly evaporated the thus clarified liquor, the saline parts shot into crystals much of the shape, and crossing one another much after the manner, of striæ of salt-petre; but their colour after a while appeared yellow, as if some oily substance were yet mixed with them.

N. B. THOUGH on several occasions the spirit of blood appeared thus oily, yet, I remember, I had not long since some distilled from another parcel of blood, which, after having been kept a year, was limpid and colourless, like an ordinary vegetable spirit.

SOME of the forementioned crystals being put upon well kindled charcoals, did presently melt and burn away, with a noise not unlike salt-petre; but the flame seemed not quite so halituous, and was more differing in colour, being not at all blue but very yellow. After the deflagration was quite past, I was curious to see, if any fixt substance was left upon the coals, and found it to be somewhat odd; for it was not of a light

light colour, nor of an incoherent body, like ashes, but a little lump of a dirty coloured matter, in which I could not perceive an alcalisate taste, and indeed scarce any at all: and this brittle substance (for such it was) being held in the flame, became red hot, without appearing destroyed by that ignition, no more than afterwards it did by being a good while kept upon a glowing coal.

The fifteenth (secondary) TITLE.

Of the medicinal virtues of spirit of human blood outwardly applied.

HAVING resided for many years last past in a place so well furnished with learned physicians as *London* is, I was careful to decline the occasions of entrenching upon their profession. And though that care did not always secure me quiet, yet, I did it so far, as that you, to whom my circumstances are not unknown, will not, I hope, expect, that I should say much upon my own experience, of the medicinal virtues of spirit of human blood; yet, since I had some few opportunities to get trials made by practitioners in physic, (who were pleased very willingly to make them for me,) that I may not leave this subject wholly untouched, I will subjoin what occurs, either to my memory, or to my thoughts, about it.

WHEN I consider, that, as far as I have observed, we do not meet regularly with any acid substance, (except perhaps in the *succus pancreaticus)* in a sound human body; (for the fixt salt of blood does itself much resemble sea-salt, whether its spirit be acid or no;) whereas the several parts of it, whether solid, as bones, or liquid, as blood, afford in distillation store of liquor impregnated with volatile salt; I am induced to think it probable, that the spirit of human blood, wherein such a salt abounds, and whereof it is the main and predominant ingredient, is like to have notable operations upon the human body, and afford medicines of great efficacy in many of its diseases. And though against most of these it is to be internally given, yet there are some, against which it may be succesful, when but externally administred.

FOR, as well rectified spirit of human blood abounds with very subtle particles, which in point of taste, odour, diffusiveness and penetrancy, do much resemble those of strong spirits of urine, of hartshorn, and of sal-armoniac; so one may very probably expect to find the same vertues in the spirit of blood, that experience has manifested to belong to those other spirituous liquors.

I have seldom, if ever, seen any medicine operate so nimbly in fits of the mother, as a well dephlegmed spirit of sal-armoniac; which, as I formerly noted, is in effect mainly a spirit of urine, which itself is granted to be a liquor separated from blood: for this spirit being held to the noses of hysterical women, has often in a trice, to the wonder of the by-standers, fetched them out of their fits. Nor is this the considerablest effect, that I have had of this spirit, for sometimes it has, with a strange quickness, brought to themselves patients, that were fallen to the ground, and either really were, or were judged to be, epileptical: and even in agonizing persons, where it could not recover them, it would frequently for the time bring them out of their swoons, and make them know and understand the assistants, and perhaps speak to them too; of which, if it were needful, I could give more than one instance. But I shall rather add, that if nature be not quite spent, and the case wholly desperate, this may be of great advantage, because it allows the physician some (though perhaps but little) time, and a good opportunity to administer other remedies, which the patient, unless excited and brought to himself, would not be made to take. Of which I shall give you a memorable instance in a patient of the very learned Dr. *Willis*'s who being in the fit of an apoplexy, when he was necessitated to go from her out of the town, and leave her would be manifeſtly viſible in the form of aſcending ſmoke, which was wont at firſt to ſurprize the delighted ſpectators; and this production of ſmoke would continue a good while, if the vials were not ſevered to make it ceaſe, which, upon their remove, it would preſently do. I have divers times practiſed a more eaſy way of making theſe fumes conſpicuous; but it belongs more to another paper, and what has been now delivered may ſuffice for my preſent purpoſe.

YET, it may not be improper to take this occaſion to acquaint you with an experiment, that I made to obſerve what the contrary ſalts, that abound in our ſpirit of blood and in ſome acid liquors, would produce, when they were combined and brought into a dry form. I ſhall therefore annex a tranſcript of the experiment I ſpeak of as I find it regiſtred in one of my note-books.

[WE took ſome pure volatile ſalt of human blood, and having juſt ſatiated it with ſpirit of nitre, we ſlowly evaporated away the ſuperfluous moiſture, that the acid and urinous ſalts might be united into a dry concretion, from which my deſign was to ſeparate them again, the ſalt of blood in its priſtine form, and the ſpirit of nitre in the form of ſalt-petre. To effect this, we put the compounded ſalt into a ſmall bolt head with a long and ſlender neck, and then added to it a convenient quantity of ſalt of tartar, and as much diſtilled water, as would ſuffice to make the mixture ſomewhat liquid, to promote the action of the contrary ſalt, upon one another. By which mutual actions we ſuppoſed, that the ſaline ſpirits of nitre, being more congruous to the fixed ſalt than to the volatile, would forſake the ſalt of blood, (which it detained before from flying away,) and give it leave to ſublime; and accordingly, having kept the glaſs, wherein the mixture was made, for a competent time in a convenient heat, we obtained what we looked for, ſince a good proportion of fine volatile ſalt aſcended, in a dry form, into the neck.]

HAVING put to ſome of the ſpirit of human blood a ſmall quantity of exceeding ſtrong ſpirit of nitre, there was, upon the conflict of the two liquors, excited ſo great a quantity of thick white fumes, that I could not but wonder at it, having never ſeen any thing of that kind comparable to it. And theſe fumes circulating long in the cavity of the glaſs, whereof perhaps a tenth part was full of liquor, did many of them, though the veſſel were wide-mouthed, fall back and run down the ſides of the glaſs into the ſtagnant mixture, as if they had compoſed ſtreams of a milky liquor. And when at length, after theſe fumes had diſappeared, we dropt in a little more of the ſame ſmoking ſpirit of nitre, the like ſtrange plenty of white exhalations did preſently enſue, and continue to circulate a great while in the open glaſs, the mixture, in the mean while, appearing reddiſh. Being ſettled, and ſeeming to have been ſo diſcoloured by a fattiſh ſubſtance, we put to it a little rain or diſtilled water, and having by filtration ſeparated it from the fæces, and ſlowly evaporated the thus clarified liquor, the ſaline parts ſhot into cryſtals much of the ſhape, and croſſing one another much after the manner, of ſtriæ of ſalt-petre; but their colour after a while appeared yellow, as if ſome oily ſubſtance were yet mixed with them.

N. B. THOUGH on ſeveral occaſions the ſpirit of blood appeared thus oily, yet, I remember, I had not long ſince ſome diſtilled from another parcel of blood, which, after having been kept a year, was limpid and colourleſs, like an ordinary vegetable ſpirit.

SOME of the forementioned cryſtals being put upon well kindled charcoals, did preſently melt and burn away, with a noiſe not unlike ſalt-petre; but the flame ſeemed not quite ſo halituous, and was more differing in colour, being not at all blue but very yellow. After the deflagration was quite paſt, I was curious to ſee, if any fixt ſubſtance was left upon the coals, and found it to be ſomewhat odd; for it was not of a light

light colour, nor of an incoherent body, like ashes, but a little lump of a dirty coloured matter, in which I could not perceive an alcalisate taste, and indeed scarce any at all: and this brittle substance (for such it was) being held in the flame, became red hot, without appearing destroyed by that ignition, no more than afterwards it did by being a good while kept upon a glowing coal.

The fifteenth (secondary) TITLE.

Of the medicinal virtues of spirit of human blood outwardly applied.

HAVING resided for many years last past in a place so well furnished with learned physicians as *London* is, I was careful to decline the occasions of entrenching upon their profession. And though that care did not always secure me quiet, yet, I did it so far, as that you, to whom my circumstances are not unknown, will not, I hope, expect, that I should say much upon my own experience, of the medicinal virtues of spirit of human blood; yet, since I had some few opportunities to get trials made by practitioners in physic, (who were pleased very willingly to make them for me,) that I may not leave this subject wholly untouched, I will subjoin what occurs, either to my memory, or to my thoughts, about it.

WHEN I consider, that, as far as I have observed, we do not meet regularly with any acid substance, (except perhaps in the *succus pancreaticus)* in a sound human body; (for the fixt salt of blood does itself much resemble sea-salt, whether its spirit be acid or no;) whereas the several parts of it, whether solid, as bones, or liquid, as blood, afford in distillation store of liquor impregnated with volatile salt; I am induced to think it probable, that the spirit of human blood, wherein such a salt abounds, and whereof it is the main and predominant ingredient, is like to have notable operations upon the human body, and afford medicines of great efficacy in many of its diseases. And though against most of these it is to be internally given, yet there are some, against which it may be successful, when but externally administred.

FOR, as well rectified spirit of human blood abounds with very subtle particles, which in point of taste, odour, diffusiveness and penetrancy, do much resemble those of strong spirits of urine, of hartshorn, and of sal-armoniac; so one may very probably expect to find the same vertues in the spirit of blood, that experience has manifested to belong to those other spirituous liquors.

I have seldom, if ever, seen any medicine operate so nimbly in fits of the mother, as a well dephlegmed spirit of sal-armoniac; which, as I formerly noted, is in effect mainly a spirit of urine, which itself is granted to be a liquor separated from blood: for this spirit being held to the noses of hysterical women, has often in a trice, to the wonder of the by-standers, fetched them out of their fits. Nor is this the considerablest effect, that I have had of this spirit, for sometimes it has, with a strange quickness, brought to themselves patients, that were fallen to the ground, and either really were, or were judged to be, epileptical: and even in agonizing persons, where it could not recover them, it would frequently for the time bring them out of their swoons, and make them know and understand the assistants, and perhaps speak to them too; of which, if it were needful, I could give more than one instance. But I shall rather add, that if nature be not quite spent, and the case wholly desperate, this may be of great advantage, because it allows the physician some (though perhaps but little) time, and a good opportunity to administer other remedies, which the patient, unless excited and brought to himself, would not be made to take. Of which I shall give you a memorable instance in a patient of the very learned Dr. *Willis*'s who being in the fit of an apoplexy, when he was necessitated to go from her out of the town, and leave her in that condition, he committed her to the care of a very ingenious phyſician, who (whether by his direction or no, I remember not) came to me to acquaint me with it, complaining, that they could not hope for any ſuceſs of their remedies, in regard ſhe was ſo ſtupid, and had ſhut her mouth ſo cloſe, that they could not get any down: whereupon I gave him, and told him the uſe of, a very ſubtile ſpirit, that I had by me for ſuch caſes, though I remember not, whether it were of ſal-armoniac, or ſome other volatile or liquid alcali; by applying which to her noſe, the phyſician found he could preſently make her open her eyes, and in part come to herſelf; but then ſhe would again, when the glaſs was removed, ſoon relapſe into her former condition. Wherefore, having by thoſe frequent viciſſitudes gained ſome time, and got a medicine for his purpoſe, he then held the glaſs to her noſe for a good while together; by which means ſhe ſo recovered her ſenſes, that ſhe knew the by ſtanders, and being exhorted to take a medicine, that was offered her, which they told her would do her much good, ſhe underſtood them, and ſwallowed it; and though afterwards, upon the removal of the vial, ſhe relapſed into a ſenſeleſs ſtate, yet by the help of the urinous ſpirit they kept her alive, till the very briſk medicine ſhe had taken began to act its part, and make a copious evacuation, which did not only rouſe her, but little by little relieve her: ſo that in a ſhort time ſhe happily eſcaped a danger, that was judged to be very hardly, if at all, ſuperable by any medicines. But here I muſt give you notice, that in ſuch difficult and deſperate caſes I am not content, that a vial, with a ſomewhat long neck, be held to the noſe, but ſometimes order, that little pellets of lint or cotton, or of thin rags, be dipt into the ſpirit and thruſt up into the noſtrils. And the ſame thing I would adviſe, if need ſhould require it, in the adminiſtration of ſpirit of human blood. And as, for external uſes, I make a particular preparation of ſpirit of ſal-armoniac, or of urine, that is more ſtrong and penetrant than that, which is made the more ordinary way: ſo, if I had been furniſhed with ſtore of ſpirit of blood, I would have handled it in a not very unlike manner. And, however, with the little I had, I made the following experiment for trial's ſake. We took ſome dried volatile ſalt of human blood, (being then better able to ſpare that than ſpirit,) and put to it as much ſpirit of nitre, as would juſt ſerve to ſatiate it; and then by evaporation we obtained thence an anomalous kind or compounded ſalt, which afterwards, becauſe we deſired a medicine in a dry form, we ſublimed from a convenient quantity of a well choſen fixed alcali, (if I miſtake not, we took an equal weight of ſalt of tartar,) fit to retain not only the phlegmatic parts, but the oleaginous too, which oftentimes lie concealed in volatile ſalts and liquors, wherein they do not at all at firſt appear, and unto which the greateſt part of the fœtid or offenſive ſmell may probably be imputed: by this means we obtained a dry white ſalt of a very piercing ſmell. But I had no opportunity to try this ſublimed ſalt upon diſeaſed perſons; for whoſe ſake I alſo made uſe of another way to bring over the ſaline part of blood in a liquid form, (which, for the uſe of ſmelling, I for the moſt part prefer to the dry:) for which purpoſe we mixed two parts of dried human blood with three parts of lime, and then diſtilled them with a pretty ſtrong fire; by which means we obtained, as we expected, a pretty deal of ſpirit, unaccompanied with any volatile ſalt in a dry form: which ſpirit ſeemed, even without rectification, to have a ſtronger ſmell, and a more fiery taſte, than other ſpirit of blood after a rectification. And I gueſſed, that if we had taken more or ſtronger lime, we ſhould have had leſs oil and a more piercing ſpirit, ſince the lime would probably have retained moſt of the oil, and perhaps all the ſuperfluous moiſture.

I have likewiſe often found, that ſlighter head-achs have been cured in leſs (and perhaps much leſs) time than a quarter of an hour, by the bare ſmell of ſome of theſe well depurated volatile alcalies; and if I miſ-remember not, I have been relieved, particu-

larly

larly by that of *H. B.* and I have very rarely for these many years used, or (thanks be to God) needed any other medicine to free myself from pains of the head. And even violent and durable pains of that part have been, if not quite removed, yet much lessened by the same remedy often reiterated, which I have likewise observed to be usually enough very effectual in faintings, especially those of hysterical and hypochondriacal women; which makes it probable, that our spirit of human blood, which is a liquor, that in many qualities manifestly resembles other volatile alcalies, (and perhaps surpasses them,) and which, when well freed from its oil, can by few, if by any, be distinguished from other urinous spirits, may, by its odour, be available in the forementioned maladies. I expect you should tell me, that the ill scent of spirit of blood will hinder that sex from using it externally, to divers of whose distempers it is the most proper. To this it may be answered, that most of those, that find themselves in pain or danger, would be content to be eased or rescued by an unpleasant medicine: for we may apply to health, what *Vespasian* said of the tax, that was paid him upon the score of urine, *Lucri bonus odor ex re qualibet*. And accordingly we see, that ladies themselves ordinarily make use in such cases of burnt feathers, and in these and some others of *Castoreum*, *Galbanum*, and *Asa fœtida*, whose smells are offensive enough to men. But for the more delicate and nauseous patients, one may much lessen the offensive odour of our spirit by long digestions, or by reiterated or skilful rectification. And if even then they cannot be reconciled to the odour of so good a remedy, that odour may (as was formerly intimated on another occasion) be corrected by uniting it with a convenient quantity of highly rectified spirit of wine; by which means it may perhaps (for I am not sure of it) lose somewhat of its penetrancy, as well as of its urinous odour, but yet may remain subtile and active enough for divers good purposes. And if you would not only correct the smell of the spirit of blood, but make it afford a fragrant one, you may do it by dissolving in the spirit of wine a convenient quantity of some aromatic, or other well scented chymical oil, whose proportion may be found by letting it fall drop after drop into the vial, and frequently shaking it to mingle the liquors well, till you find by your smell, that the offensive odour of the spirit of blood is sufficiently obscured; or (if you will not only correct it, but perfume the liquor) that the mixture is sufficiently imbued with the grateful odour of the oil, wherewith you compounded it.

I shall add on this occasion, that, if we aim chiefly at correcting or changing the smell of spirit of blood, we may usefully employ a chymical oil, more mild or temperate than the aromatic ones of cinnamon or cloves. For trial purposely made has informed me, that if the oil of *Rhodium* (which is much esteemed by perfumers) be sincerely and skilfully made, (which I fear it is not over frequently) a very few drops of it will make an ounce of alcohol of wine so fragrant, that this solution being shaken together with a convenient quantity (perhaps much less than an equal one) of well rectified spirit of human blood, there will emerge a mixture, that I found to have a scent brisk enough, and yet to be not only free from stink, but imbued, though not strongly, with the odoriferous particles of the *Rhodium*.

I must not here omit, that divers happy practitioners, as well physicians as professed chymists, do highly extol the oil of amber against convulsion fits and other distempers of the brain and *genus nervosum*: and indeed experience has so recommended some medicines of amber to me, that, in some cases, there are few, that I more willingly give or take. And besides the great character, that *Helmont* has left of amber dissolved in spirit of wine, experience has brought such credit to it in divers cases, (for there are some cases and constitutions, wherein I suspect it of too much heat) that many patients, as well women as men, had much rather endure the smell, than deny themselves the benefit of the tincture or the oil. And if you have any such patients, perhaps you will not be ill pleased to be advertised, that you may, according to the formerly mentioned way, employ the high tincture of amber taken with spirit of wine, to correct the odour, and increase (at least in number) the vertues of spirit of human blood. And because it requires some skill, and not seldom a pretty deal of time, to draw this tincture from crude amber, though finely powdered, I bethought myself of the following way to draw speedily a strong tincture from the oil itself; for, though this oil will not, even by long shaking, dissolve throughly in spirit of wine, as the aromatick and other oils lately mentioned will do; yet I found, that, by well shaking those two liquors together, and leaving them to settle at leisure, though they would separate into distinct masses, yet the spirit of wine would, even in the cold, extract from the oil a fine tincture of a high yellow colour, little, if at all, different from that of the oil itself: of which tincture I afterwards mixed as much with spirit of blood, as sufficed to obscure the urinous smell, and make that of the oil of amber somewhat predominant, and as we judged, more subtile and brisk than it was before.

THREE things more I have to intimate concerning the external use of our spirit of blood. The first is, that by what has been said of the good effects it may have, when (after it has been, by the lately mentioned or other preparations, imbued with chymical oils) it is smelt to, I would by no means be thought to deny, that it is, after these changes, fit to be also inwardly employed, as I shall have, e'er long, occasion more particularly to declare. My second admonition shall be, that, whereas in some mixtures it will be hard to hit upon the proportion of the chymical oil, or other things employed to correct the smell of the spirit of blood, so exactly, but that, after the mixture has had some time to settle, a separation of some oleaginous parts will be made; the bulk of the mixture may be freed from it, by pouring all into a glass tunnel somewhat sharp at the bottom, after the manner used among chymists to separate oils from other liquors, and then the mixture, that will run through before the oil, may be kept close stopt in a vial by itself, and the fragrant oil (unless it be of cinnamon or cloves) reserved for other uses. And whereas frequently, if not most commonly, if the vinous spirit were sufficiently rectified, there will, by the concoagulation of the saline and urinous particles, be produced a kind of salt; you may either pour the liquid part from it into another vial, and use each of them separately without more ado, or else, without thus separating them, you may sublime, with a very gentle warmth, as much as will ascend from the rest of the mixture in a very dry form. And this *sal volatile oleosum* of spirit of blood, when it was duly prepared, I found to be deprived of its former bad scent, and perhaps endowed with a fragrant one, and yet to have an odour more subtile, brisk, and piercing, than I had thought it reasonable to expect. The third and last thing I would advertise, is, that besides those medicinal uses, that may be made of the odours of spirit of blood simple or compounded, it may have considerable vertues, applied in substance as a liquor, by way of fomentation or otherwise; which I think the more likely, because the spirit of sal-armoniac has been much commended for mitigating the sharp pains of the gout, and is said to have been succesfully used in the *Erysipelas*. And when I consider, that our liquor is very spirituous and penetrating, and so fit to strengthen and resolve, and also of an alcalisate nature, which fits it to mortify acidities; it seems very probable, that, by vertue of these and other friendly qualities, it may, by being applied in its liquid form, prove good in divers cases, where the chirurgeon's or physician's help is wont to be required.

BUT it is high time for me to proceed from the external to the internal uses of the spirit of human blood.

The

The fixteenth (fecondary) TITLE.

Of the medicinal vertues of the fpirit of human blood inwardly ufed.

I have long been prone to think, that it is not neceffary the number of fpecifically different morbific matters, (as phyficians call actually noxious humours or other fubftances) in the human body fhould be near fo great, as that of the difeafes it is [illegible]oxious to; and confequently, that every difeafe, that has a diftinct name af[illegible] to it, does not always require a diftinct fort of peccant matter to produce it; but that the fame hurtful humour, or other agent, may produce fickneffes, that pafs for different ones, (and accordingly have diftinct denominations) only as the fame morbific agent's bad effects are diverfified, partly by its own greater or leffer quantity, and more or lefs active qualities, and partly (and indeed chiefly) by the particular natures, or ftructures and fituations of the parts, that it invades. To this opinion I have been led by divers inducements, that I fhall not now ftay to fet down; efpecially, fince the probability of it may be eafily deduced, from what frequently enough occurs among fick perfons, of the metaftafes of morbific matters; the fame acid or fharp humour, for inftance, producing fometimes a cholic, fometimes after that a palfey, fometimes a cough, fometimes a flux of the belly, fometimes an ophthalmia, fometimes a violent head-ach, fometimes convulfions, and fometimes other diftempers; as the peccant humour, or other noxious matter, happens primarily to invade, or afterwards to be tranflated to, this or that particular part of the body. And to the hitherto propofed notion it is very agreeable, that one remedy, by being capable victorioufly to oppugn one or two of the principal kinds of morbific matter, may be able to cure differing difeafes, efpecially if it be endowed with any variety of active vertues. And upon this ground I am apt to think, that the fpirit of human blood, fkilfully prepared and adminiftred, may be a good remedy in no fmall number of internal affections of the human body. And indeed volatile alcalies in general have been in *England* fo profperoufly made ufe of in phyfick, fince the year 1656, (about which time I had the good fortune to contribute fo to introduce them, as to bring them by degrees into requeft, by divulging eafy ways of making them, as well as by declaring their virtues) that I fee fmall caufe to doubt, but that they will hereafter be more generally efteemed and employed, than yet they are, and will little by little invite phyficians to prefer them to a great many vulgar remedies, that, for want of better, are yet in common ufe, though they clog or weaken the patient, and want divers advantageous qualities, that may be found in volatile alcalies. For (to apply what has been faid to our prefent fubject, as an inftance, that may ferve for other urinous fpirits) the fpirit of human blood is endowed with divers qualities, that are both active and medicinal. For it mortifies acid falts, which are the caufes of feveral difeafes, and if I miftake not, of fome, that are not wont to be imputed to them. It is a great refolvent, and, on that fcore, fit to open obftructions, that produce more than a few difeafes. It is both diaphoretick and diuretick, and, on both thefe accounts, fit to affift nature to difcharge divers noxious falts, and expel divers contagious or malignant corpufcles, that offend her. It refifts putrefaction and coagulation of the blood, gives it a brifknefs and fpirituofity, that promotes the free circulation of the blood, to which it is congeneal; by which means (though not perhaps by thefe only) it becomes a good cordial, and probably againft fome poyfons an antidote. And, which is none of the leaft, nor leaft extenfive, vertues, it is very friendly to the *genus nervofum*, and upon that account is like to be very proper in fits of the mother (as they are called,) convulfions, fome forts of head-achs, palfies, incipient apoplexies, fome fort of afthmas, *&c.* It is alfo balfamical in fome circumftances, and may have divers other virtues, that have not yet been obſerved. For a medicine, that does not weaken, nor cauſe great evacuations, nor clog the ſtomach, nor is blemiſhed with the exceſs of any manifeſt quality, but has in itſelf a complex of ſo many uſeful powers, may reaſonably be ſuppoſed, likely to be available in more than a few diſeaſes: ſince a good part of thoſe, that human bodies are liable to, may be powerfully oppugned by ſome of thoſe excellent qualities, one or more, whoſe confluence may be found in the ſpirit of human blood.

I preſume therefore, that one may rationally propoſe it, as likely to be a good remedy in many diſtempers, eſpecially wherein either ſpirit of urine, or the urinous ſpirit of ſal-armoniac, have been found ſucceſsful medicines; ſuch as hyſterical fits, pleuriſies, coughs, ſome ſcorbutick diſtempers, convulſions, apoplexies, ſome kinds of fevers, head-achs, the jaundice, *&c.* But I formerly prepared you not to expect, that I ſhould ſay much of the virtues of the ſpirit of human blood (inwardly given,) upon my own perſonal experience. And therefore I ſhall not ſcruple to tell you, that *Helmont* himſelf, as little as he is apt to praiſe other than his own, or the Paracelſian, arcana, more than once commends the ſpirit of cruor, (though that be, in his ſenſe of the word, not yet fully elaborated human blood) againſt the epilepſy, which, he ſays, it will cure even in adult perſons, which is a virtue he expreſsly denies the ſpirit of urine. And a famous writer about the hermetick phyſick (but, if I miſtake not, better verſed in divers other parts of learning, than in chymical arcana,) though he ſo far depretiates ſpagyrical preparatious, as to commend the utility but of a very few of them, is pleaſed to put the diſtilled liquor of blood into the number of thoſe very few, that he vouchſafes a good character to.

Spiritus vitæ, N. 16. Pag. m. 122.

I am the more inclined to give credit to theſe praiſes of ſpirit of blood, becauſe, as I remember, this was the medicine, that I made uſe of in the following caſe. A young lady, in whoſe family the conſumption was an hereditary diſeaſe, was moleſted with a violent and ſtubborn cough, that was judged conſumptive, and looked upon by thoſe, that gave her phyſick as not to be cured by any other way, than a ſeaſonable remove from *London* into the French air; but ſhe was already ſo far gone and weakened, and there remained ſo much of the winter, that it was judged ſhe would die before the ſeaſon would make it any way fit for her to undertake ſo long and troubleſome a journey; but if ſhe could be kept alive till the end of the ſpring, there would be ſome hopes ſhe might in *France* recover. On this occaſion being ſollicited by ſome friends of hers and mine, to try what I could do to preſerve her, I ſent her ſome ſpirit of human blood very carefully prepared and rectified, (to which I gave ſome name, that I do not well remember,) upon the uſe of which ſhe manifeſtly mended, notwithſtanding the unfriendlineſs of the ſeaſon; inſomuch, that about the end of *February*, ſhe had gained relief and ſtrength enough to venture to croſs the ſeas, and make a journey to *Montpellier*, whence in autumn ſhe brought home good looks and recovery. If I much miſ-remember not, the ſame ſpirit of blood, made very pure and ſubtile by the help of a lamp furnace, was the medicine, that I put into the hands of an ingenious and ſucceſsful phyſician, who complained to me, that he had a patient, that had quite puzzled him, as well as baffled the endeavours of other eminent doctors, whom the difficulty of the caſe had invited at ſeveral times to try their ſkill upon him. This man was frequently obnoxious to ſuch violent and tormenting fits of the head-ach, that he could not endure the light, and was offended with almoſt every noiſe or motion, that reached his ears; inſomuch, that he was forced to give over his profeſſion, which was that of a taylor: but upon the conſtant uſe of the before-mentioned ſpirit of blood, (for the other medicines he took were much inferior to it, and had not before been available) he received ſuch relief, as made him with great joy and thankfulneſs return

to

to the exerciſe of his trade; and the phyſician, to whom I gave the remedy for him, told me one circumſtance too conſiderable to be here omitted; namely, that the patient having, by our famous *Harvey*'s advice, been uſed to bleed once in two or three months, the phyſician counſelled him, notwithſtanding his recovery, not abruptly to break off this ancient cuſtom; and the patient thereupon ſent for the ſame chirurgeon, that had been formerly wont to let him blood, and to complain of the great badneſs of his blood; but when this chirurgeon, who knew not what had been done to the patient, came to open a vein again, and perceived what kind of blood it afforded, ſo he was ſurprized, that he ſtopped the operation, and aſked the man with wonder, how he came by ſuch florid blood, adding, that it was pity to deprive him of ſo well conditioned a liquor.

The medicinal vertues, hitherto mentioned, belong to the ſpirit of human blood, as it is pure and ſimple: but it is not improbable, that it may acquire other, and perhaps nobler, faculties, if it be dexterouſly corrected, diverſified, or united with fit ingredients, that is, in a word, ſkilfully altered or compounded.

These things may be performed ſeveral ways; for they may be done, either by uniting, as well as one can, by long digeſtion, or frequent cohobations, the ſpirit of human blood with the oils, ſalt, and (if need be) phlegm, of the ſame concrete, into ſuch a kind of mixture, as ſome chymiſts call *Clyſſus*.

Or, ſecondly, by uniting the ſpirit of blood with acids, as with ſpirit of nitre, ſpirit of vinegar, ſpirit of verdegreaſe, oil of vitriol, *&c.* and employing theſe mixtures, either in their liquid form, or reduced, by evaporation, into chryſtals, or other ſalts; and making uſe of theſe, either as they are, or after a kind of analyſis of them.

Or, thirdly, by uniting our ſpirit with metalline ſolutions, as of gold, ſilver, mercury, and with ſolution of minium, made with ſpirit of vinegar, by mixture of which liquor with ſpirit of blood, and a ſlow evaporation of them, I remember I have had pretty ſtore of finely figured chryſtals.

Or, fourthly, by diſſolving in ſpirit of blood, carefully dephlegmed, ſulphur opened with ſalt of tartar.

Or, elſe, by diſſolving in it ſome metalline bodies, as copper, zink, and iron, which laſt will afford a martial liquor, that, differing much from other preparations of ſteel, that are wont to be made with acids, may probably have ſome virtues, diſtinct from thoſe of the known remedies made of that metal.

But I cannot ſtay to enumerate the ſeveral ways, whereby the ſpirit of human blood may be made ſerviceable to the medicinal art. Yet one preparation there is, which though I have already taken notice of in the foregoing title, and therefore can ſcarce mention without ſome repetition, yet I think I ought not to pretermit it on this occaſion; partly becauſe, whereas it was formerly propoſed with reſpect only to the outward uſes of it, I ſhall now conſider it with reference to the inward; and partly becauſe, by this way of proceeding, we may, at once, correct, diverſify, and compound our ſpirit of blood.

This operation may be performed two ways, whereof the former is more ſimple than the latter. The firſt is, to add to well rectified ſpirit of blood a double weight, or about an equal one, (as the liquors, eſpecially the volatile alcali, are more or leſs ſtrong) of alcohol of wine: for theſe liquors, being well ſhaken together, will, in very great part, coagulate into ſalt, which, with a very gentle heat, will ſublime in a dry form, in which I found it to have loſt almoſt all its offenſive ſmell. And though, againſt this way of proceeding I know it may be objected, (as was formerly intimated) that the efficacy of the medicine may, as well as the urinous ſmell, be much weakened by this preparation; yet I found this ſalt to retain a conſiderable degree of quickneſs and

and penetrancy, which its volatility kept me from thinking ſtrange. And experience has perſuaded me, that divers of theſe compounded, or, if I may ſo ſtile them, reſulting ſalts, (which ſome chymiſts call *ſalia enixa*, for all agree not in the ſenſe of that name) though they ſeem to have their activity clogged, may have conſiderable operations both in chymiſtry and phyſic. And why the emergent ſalt we ſpeak of, may not be of that number, I ſee no ſufficient cauſe; [*N. B.*] eſpecially ſince ſuch a kind of mixture, though made with another urinous ſpirit, has had ſuch effects in fevers, as I thought extraordinary. Nor is the liquor, that our compounded ſalt leaves behind, to be thrown away, ſince, if it be dephlegmed, it may afford a not deſpicable liquor, both for medical and mechanical uſes, of which it may here ſuffice to have given you in general this hint.

AND if the more ſimple way of altering the ſpirit of human blood, be carried on a little further, by diſſolving in the alcohol of wine, before the conjunction of the two ſpirits be made, a convenient proportion (as perhaps a twentieth, or twenty-fourth part) of an eſſential chymical oil, as of cloves, anniſeeds, marjoram, *&c.* the volatile ſalt, that will be ſublimed from this mixture, will not only be deprived of its ſtink, but endowed with the ſmell and the reliſh of the oil; which, by being thus united with a ſalt very ſubtile and friendly to nature, will leſs over-power and offend the brain and ſtomach, than mere chymical oils are wont to do; and being aſſociated with ſuch agile and penetrating corpuſcles, will with them gain admiſſion into the more inward receſſes of the body, and there exerciſe the virtues, that belong to the vegetables, that afforded the oils, or, at leaſt, to the oils themſelves. In theſe odoriferous aromatic mixtures, the oleaginous particles are, by the intervention of the ſaline ones, brought to mix readily with other liquors, and even with aqueous vehicles, and to continue long enough mixed for the patient to take them commodiouſly. And thus, by this one method, there may be a multitude of *ſalia volatilia oleoſa*, that is, of pleaſing, ſubtile and efficacious remedies for inward uſes, prepared, even as many as the phyſician or chymiſt ſhall pleaſe to make eſſential oils, (or others, that will diſſolve in alcohol of wine;) and if theſe be drawn from cephalick plants, as marjoram, roſemary, lavender, *&c.* or from cephalick ſpices, as nutmegs, cinnamon, *&c.* they will probably afford very briſk and grateful medicines, to relieve and comfort the brain and ſpirits; as they may the heart, liver, and other *viſcera*, if, in the ſublimation, the ſaline particles of blood be aſſociated with thoſe of oils drawn from vegetables, whoſe virtues do peculiarly reſpect thoſe parts.

OTHER ways might be here propoſed of making remedies, whereof the ſpirit of blood ſhould be the main ingredient. But I willingly leave that work to yourſelf, and thoſe of your profeſſion, if you think fit to proſecute it; ſince my preſent taſk does not require, that I ſhould write like, what I am not, a profeſſed phyſician, but like what I endeavour to be, a diligent natural hiſtorian. And for the ſame reaſon I purpoſely forbear to inſert here ſome chymical proceſſes, that I have met with, of remedies, that admit of diſtilled blood, though I have alſo declined the mention of them, for two other reaſons; one, that the authors do not recommend them upon their own experience; and the other, that theſe medicines being much more compounded, than thoſe I lately propoſed, wherein our ſpirit is mingled but with ſome one chymical oil or other diluted with alcohol of wine, their preparations are leſs fit for my deſign, which leads me to conſider the effects of human blood upon patients, leſs as they are ſanative, than as they are ſigns of qualities, whoſe knowledge tends to the diſcovery of the nature of ſpirit of human blood, and ſo of that blood itſelf.

AND this, Sir, it may ſuffice to have, at preſent, ſet down, touching the hiſtory of the ſpirit of human blood; of which, and of the other parts conſtituting that red body, or obtainable from it, I might have given you a far less incomplete account, if I had had more leisure; and if, for want of materials to make experiments upon the entire liquor, and the concreted and serous parts of it distinctly, and especially to afford a sufficient quantity of the spirit, I had not been so straitened, that I was fain to leave many things untried, and to try some others in much less quantities, and much more unaccurately than otherwise should have been done by, Sir,

Yours, &c.

An APPENDIX to the MEMOIRS for the HISTORY of HUMAN BLOOD.

HAVING elsewhere mentioned the reasons, that moved me to think it fit to subjoin an appendix to each of the natural histories, that I drew up, or designed, of particular subjects; it would be needless to trouble you with them in this place, where it may therefore suffice to advertise you, that the following particulars I have thrown together, as they occured to me, to be annexed to the foregoing history of human blood, are made up of two sorts: some, which through haste, or otherwise, were prætermitted, when they should have been ranged under one or other of the foregoing titles, and so are answerable to those, that in the first part of these memoirs were called *paralipomena*; and others, that are, for the most part, of kin to those, that are there stiled *addenda*; though some of them may be judged to deserve better the name of supernumerary, which yet I thought fit to let pass among the rest, because, though they do not directly belong to any of the distinct titles of our history, yet they may obliquely be referred to one or other of them, or are at least capable of being made some way subservient to the general design of the history itself. But the paucity of the particulars, that I am at present furnished with makes me fear it may savour of ostentation, if in so much penury of matter, I should curiously refer the particulars, that now occur to me, to the differing titles, primary and subordinate, that have been enumerated in the schemes of our intended histories. And therefore, till I be better stocked with materials, I shall forbear to make scrupulous references of them, or so much as constantly distinguish the *paralipomena*, from the other *addenda*; contenting myself to refer some of them in a general way, and in the order they chance to come to hand, to that part of the memoirs, whether the second, the third, or the fourth, to which they respectively seem most to belong.

IT is hoped, that neither connection nor stile will be expected in loose notes, hastily set down, at several times, to secure the matters of fact, then fresh in memory, from being, as to any necessary circumstances, forgotten.

SOME trials may seem to have been made extravagantly, and quite at random, which perhaps would be otherwise thought of, but that I judged it not worth while, especially writing in haste, to spend time in setting down the inducements I had to make them, or the aims I had in them.

I am well aware, that some few of the following trials may seem but repetitions of others, recited in the body of the history. But these were added on purpose, that where the event of both trials was the same, they might confirm one another, which, where the subject has lain uncultivated, is oftentimes a desirable thing; and where they disagree in any considerable circumstances, their difference may occasion further trials, and in the mean time keep us from building dogmatical conclusions upon the circumstances, wherein they differ.

Particulars referrible to the ſecond part of the HISTOY.

EXPERIMENT I.

THE proportion of the ſubſtances obtainable from dried human blood being, as I formerly noted, very difficult to be determined, becauſe of that difficulty, and the importance of the inquiry, I thought fit to employ ſome blood, that I made a ſhift to collect, ſince the writing of the ſecond part of the foregoing hiſtory, in making another experiment, that we may make the nearer and ſafer eſtimate, of the quantities of the diſtinct ſubſtances ſought after. For this end I cauſed twelve ounces of dried blood to be carefully diſtilled by an expert laborant, well admoniſhed of the difficulty of his taſk, and the exactneſs he was to aim at in performing it. The diſtillation being ended, the ſubſtances obtained were brought me, with this note of their quantities. Twelve ounces of dried human blood yielded, of volatile ſalt and ſpirit together, five ounces, of which we poured off from the wet ſalt ʒxiij +. 54 *gr.* ſo that there remained ℥iij +. ʒij +. 6 *gr.* of volatile ſalt; of fœtid oil there were two ounces; of *caput mortuum* four ounces and two drams: ſo that, in ſpite of all his care, there was loſt, by ſticking to the retorts and other glaſſes, (which, I preſumed, retained little elſe than the more viſcous oil and phlegm) and by avolation of ſome more ſubtile parts, (eſpecially upon pouring the liquors from veſſel to veſſel) about ſix drams. The four ounces and two drams of *caput mortuum*, being diligently calcined, afforded but ſix drams and a half of aſhes: of which very great decrement, the accenſion and conſumption of the more fixed oleaginous part ſeems to be the cauſe. And if it be ſo, we may ſuppoſe, that there is a far greater proportion of oil in human blood, than has been hitherto taken notice of. Theſe aſhes were not white or grey, as thoſe of other bodies uſe to be, but of a reddiſh colour, much like that of bricks; and yet the watchful laborant affirmed he could eaſily know them to be true aſhes, becauſe, that whilſt there remained any thing oily or combuſtible in the *caput mortuum*, it would look like a throughly kindled charcoal (which it would continue to do far longer than one could expect:) but when that combuſtible ſubſtance was quite waſted, the remaining *caput mortuum* would look, in the fire, like dead and ordinary aſhes, though, when they were cold, they appeared and continued red. Theſe aſhes, being carefully elixiviated, afforded five ſcruples of white fixed ſalt, beſides a little, which being caſually got into the contiguous ſand, and thence recovered by water, and reduced to the like white ſalt, amounted to about a ſcruple more: ſo that there remained for the *terra damnata* thirteen ſcruples and about a half, that is, a good deal above twice the weight of the ſalt; whence it appears, that according to this analyſis, the pure fixed ſalt of human blood is but between the 57th and 58th part even of dried blood, and therefore probably, amounts but to the 150th, or perhaps to the 170th part (in weight) of blood, as it flows from the vein opened by a lancet; and the fixed earth, or *terra damnata*, is to the dried blood, that affords it, as 19 and about a half to one.

EXPERIMENT II.

IN regard the foregoing experiment, and another of the like nature formerly mentioned, were made with dried and pulverable blood of ſeveral perſons put together, though I knew it would be ſcarce poſſible, in ſo ſmall a quantity of blood, as I could obtain, at once, from one perſon, to find out, with any accurateneſs, the quantities of the ſeveral ſubſtances it was capable of affording; yet, to be able to make ſome tolerable eſtimate grounded upon experience, I was invited to make a trial, whoſe ſucceſs, though in one part of it unlucky, was regiſtered as follows:

An entire parcel of human blood weighing ten ounces and 73 *gr.* being flowly diftilled to drynefs in a head and body, on a digeftive furnace, afforded of phlegmatick liquor ℥vij +, ʒij +. 47 *gr.* and of *caput mortuum*, or rather of dry fubftance ℥ij +. ʒij. This pulverable matter being beaten and put into a retort, and diftilled in fand, by degrees of fire, afforded ʒj +. 48 *gr.* of oil. But there happened an unlucky miftake about the falt and fpirit; for after the latter was poured off, which weighed but 48 *gr.* the wet falt, which ftuck in good quantity to the lateral and upper parts of the receiver, inftead of having been wafhed out, as it fhould have been, with the phlegm of the fame blood, was wafhed out with diftilled water; whence we obtained, by fublimation into the neck of a glafs egg, ʒj +. 5 *gr.* of dry falt: but, by the tafte of the diftilled water, whence it was fublimed, it appeared, that all the falt had not been raifed; which invited me to put to it as much good fpirit of falt, as I fuppofed to be at leaft fufficient to fatiate it with defign to try, whether, by evaporating this mixture to drynefs, and fubliming the falt, by the help of an alcali, we might not recover all, or almoft all the volatile falt, that had been fomewhat fixed by the acid fpirit.

The retort being cut, that the *caput mortuum* might be taken out, it was found to weigh ʒvj +. 12 *gr.* which, being carefully calcined, yielded but two fcruples and four grains of afhes, which the laborant faid were red. Thefe, being elixiviated, afforded 18 grains of falt, befides the remaining earth or terreftrial fubftance, which I keep by me, becaufe, notwithftanding all the violence of fire it has undergone, it is of a red colour, which feems to fome to have an eye of purple in it.

EXPERIMENT III.

Spirit of vinegar being put upon the florid fuperficies of a parcel of human blood, did very quickly deprive it of its frefh fcarlet colour, and make it of a dark or dirty colour.

EXPERIMENT IV.

The juice of a lemon fqueezed upon the florid furface of blood, did prefently fomewhat impair the colour, but did not appear to alter it any thing near fo much, as the fpirit of vinegar had done.

EXPERIMENT V.

Juice of orange changed the colour of the florid furface of blood, lefs than juice of lemons had done.

EXPERIMENT VI.

The black, or lower part of a portion of human blood being turned uppermoft, and thereby expofed to the air, within half or three quarters of an hour, (fomewhat more or lefs) acquired, by the contact of it, a pleafant and florid colour.

EXPERIMENT VII.

But if upon the black furface of the blood fome good urinous fpirit (as that of falarmoniac) were dropped, there would be an alteration produced in a trice; and a pleafant red colour, though perhaps fomewhat inferior to that produced by the contact of the air, would prefently appear on the furface of the blood.

EXPERIMENT VIII.

Fixed alcalies, or lixiviate falts refolved *per deliquium*, did likewife alter the black fuperficies of the blood to a red colour, but not fo florid or pleafant, as that produced by the urinous fpirit abovementioned.

EXPERIMENT IX.

THE freshly drawn juice of the leaves of ſcurvy-glaſs being dropped upon the black ſuperficies of a lump of human blood, ſeemed preſently to make ſome change in the colour of it, making us judge it ſomewhat reddiſh and inclinable to floridneſs.

THE ſeven foregoing notes ſuppoſe it to be already known, that when healthy blood is ſuffered to ſettle in a porringer, that ſurface of the concreted part, which is expoſed to the air, will be adorned with a fine red colour; and if the ſame maſs be turned upſide down, that, which before was the lower ſurface of it, will appear of a very dark and blackiſh colour.

EXPERIMENT X.

HAVING, for trial's ſake, almoſt filled a vial, capable of containing by gueſs near a pound of human blood, with a mixture of that liquor, and ſome rectified ſpirit of wine, whoſe proportion I cannot remember, but gueſs it was a fourth or eighth part; at the end of above three years, looking upon the ſame glaſs, ſtopped with nothing but a cork, we found it coagulated, or to ſpeak more warily, in a conſiſtent form. And the veſſel being unſtopped, there appeared no ſign of putrefaction in the blood; and having ſmelt to it, we could not perceive, that it did at all ſtink: ſo balſamick a virtue has dephlegmed ſpirit of wine to preſerve human blood.

EXPERIMENT XI.

WE took a piece of fibrous or concreted blood, of the bigneſs of a large bean (or thereabouts) and having put it into a ſmall glaſs veſſel with a flattiſh bottom, we poured on it as much highly rectified vinous ſpirit, as might ſerve to cover it, though it had been twice thicker than it was; then we lightly covered this open-mouthed glaſs with another, and ſet the veſſel in a quiet place, that the vinous ſpirit might have leiſure to imbibe the ſerous or aqueous parts of the blood, and thereby harden that yet ſoft ſubſtance; and, in effect, it quickly ſeemed to have gained a ſuperficial cruſt, but the internal parts continuing yet ſoft, we left the liquor upon the blood for a day or two longer, and then we found, that the action of the liquor had quite penetrated the lump of blood, and made it moderately hard and friable.

THIS experiment having been made in the cold, may much confirm a trial elſewhere mentioned to have been made to the ſame purpoſe; and both of them together induced me to fear, that two or three ingenious writers, that, in their chymical receipts preſcribe ſolutions and tinctures of concreted blood in ſpirit of wine, have ſet down the pompous proceſſes, wherein theſe operations are preſcribed, rather according to conjectures than experience.

EXPERIMENT XII.

IT may be of ſome uſe to the ſpeculative to know, how much volatile ſalt of blood is diſſoluble in water or phlegm: and therefore, having cauſed an ounce of diſtilled water (for common water, becauſe of ſome ſaltiſhneſs, that uſually accompanies it, would not have been ſo proper on this occaſion) to be carefully weighed out, we put into it, little by little, ſome dry and white volatile ſalt of blood, and ſhook it well into the liquor, to make it diſperſe the better: we allowed it alſo a competent time for ſolution; and by this means, we found, that ℥i. of water would diſſolve at leaſt ʒij. that is, a fourth part of its weight of dry ſalt, and that in the cold; for, afterwards, by the help of heat, we made the ſame liquor diſſolve near five and 20 grains more. In which laſt part of the experiment I had a further aim, which was to try, whether, upon the refrigeration of the liquor, the diſſolved ſalt would not ſhoot into cryſtals of obſervable

obſervable figures; but the event anſwered not at that time my deſire, yet left me not without ſome intention to reiterate the experiment, if I ſhall get another opportunity.

POSTSCRIPT.

EXPERIMENT XIII.

We put the abovementioned ſolution into a retort, to be drawn off with a pretty quick heat, (which on this occaſion we perferred to a much ſlower one) and thereby obtained a diſtilled liquor, that contained all the volatile ſalt, ſave a little, that eſcaped in a dry form; which liquor taſted ſtrong enough to paſs for quite, or at leaſt almoſt, as briſk a liquor, as moderate ſpirit of blood drawn the common way, and conſequently diſcovered near enough, what proportion ſhould be taken of the aqueous ingredient to the ſaline, when one would make ſuch a ſpirit. The knowledge of which proportion may probably eaſe us of ſome trials, that would otherwiſe be neceſſary to find it out, when we are (as we may often be) leſs ſtored with ſpirit than with volatile ſalt, and deſire to employ this in a liquid form; in which, we are wont to call it, for diſtinction's ſake, the aqueous (not the phlegmatic) ſpirit of blood.

If opportunity had not been wanting, we would have tried, whether, by repeating the diſtillation twice or thrice, a better or ſtricter union of the ſalt and liquor would not have been effected: and this, the rather, becauſe having ordered the vial, that contained this aqueous ſpirit, in which the water had been, if I may ſo ſpeak, ſuperonerated, to be kept ſtopt during a froſty night, we perceived at the bottom of the glaſs (what we had miſſed of before) a pretty deal of volatile ſalt coagulated or ſhot into cryſtals, though the cryſtals, that were this way obtained, were fine and clear, and ſome of them larger than ſpangles, yet being much more numerous than we deſired, by adhering cloſely and confuſedly enough to one another, they kept us from being able to diſcover the figure of particular grains, and made me ſomewhat doubt, whether the ſingle cryſtals were all of them of the ſame ſhape; all that I could clearly diſcern being, that divers of thoſe concretions were flat thin plates, with fine rectilinear angles, that inclined us to think, that if the whole plains could have been perfectly diſcovered, their broadeſt ſurface would have been found hexagonal, or of ſome polygon figures very near of kin to that.

EXPERIMENT XIV.

We put an ounce of diſtilled water, wherein we diſſolved as much volatile ſalt of human blood, as it would well take up, into a glaſs egg, and expoſed it during a froſty night to congeal; which we did with deſign to diſcover, whether, as the ſaltneſs, that is in ſea water keeps it here in *England* from freezing, (at leaſt or ordinary winters,) ſo the volatile ſalt of human blood, which much reſembles the other in taſte, would have the like effect upon water, eſpecially if it were fully impregnated therewith. To this conjecture the event was anſwerable, the froſt having produced no ice in our ſolution, nor having ſo much as made any of the ſalt manifeſtly ſhoot, (as I wiſhed it had done, hoping thereby to diſcover ſomewhat about the figuration of the ſalt of human blood.)

And, though afterwards we removed it into a frigorific mixture, that would probably have frozen beer and ale, and perhaps the weaker ſort of French wine; yet, we did not perceive it to glaciate any part of our ſolution, but only made it let fall a pretty deal of matter, that ſeemed to be feculent, (for what it was, I had not opportunity to examine.)

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EXPERIMENT XV.

Sea-salt diſſolved in water renders that liquor much more difficult to be frozen, than it was before; and yet being joined with ice or ſnow, the other ingredient of our frigorific mixture, it does, when outwardly applied, very much conduce to the artificial congelation of it, which uſually would not ſucceed without it. Wherefore, to try whether, as volatile ſalt of human blood being diſſolved in water, did, as was formerly noted, hinder it from freezing, ſo it would, outwardly applied, highly promote its glaciation; we mixed by gueſs about a ſcruple of this ſalt with a convenient quantity of beaten ice, and having put into this mixture a ſomewhat ſlender pipe of glaſs with common water in it, we found, after a while, the water, that lay in the lower part of the glaſs veſſel, and was ſurrounded by the mixture, was turned into ice.

EXPERIMENT XVI.

To try ſome ſuſpicions I had about the ſaline and aqueous parts, I thought might lie concealed in the fibrous or conſiſtent part of human blood, I cauſed ſome of it to be in an open and ſhallow glaſs expoſed to the air in a froſty night, and the next morning found it to be lightly frozen, and the ſurface of the ice prettily figured with reſemblances of combs, with teeth on both ſides or edges; on which account theſe figures did not ill reſemble thoſe, that I have oftentimes obtained, by ſlowly coagulating into ſalt a ſolution of ſal-armoniac made in common water.

In the ſecond part of the foregoing memoirs I have not ſaid any thing of the medicinal virtues of human blood itſelf, (for thoſe of the ſpirit belong to the fourth part) and though I might now, if I thought fit, ſay ſomething not impertinent to that ſubject in this appendix, both out of ſome printed books and my own obſervations; yet, I now forbear to do it, not only for a reaſon, that it is not neceſſary I ſhould here declare, but becauſe four to five proceſſes, that I have met with about human blood in *Paracelſus*, *Burgavius*, (famous for his biolychnium made of that ſubject) and one or two more, about the tranſplantation of diſeaſes by means of the patient's blood, are ſuch, as either I do not well underſtand, becauſe of their being (probably on purpoſe) obſcurely pen'd, or ſeem in themſelves unlikely; of which ſort is the boilychnium, or lamp of life, in which it is pretended, that the blood is ſo prepared, that the ſtate of the health of the perſon, whoſe it is, may be diſcovered by the manner of the burning of the flame it affords, (though he be perhaps at a great diſtance from it,) and his death by its extinction. Beſides that, as I have elſewhere noted, ſome circumſtances relating to the aſhes of human blood, make me doubt, whether ſome of theſe proceſſes were not rather the products of fancy than experience. And, though, I think thoſe medicines leſs improbable, that, without much deſtroying the texture of the blood by fire, aim at tranſplanting diſeaſes by its intervention, yet I thought fit to decline tranſcribing the forementioned medicines, till experience ſhall warrant me to do it. And I ſhall alſo at preſent forbear to ſet down my own trials, becauſe I have not yet ſeen the event of them: but, yet, I ſhall invite you to endeavour with me to prepare two, that, if they ſucceed, may afford, eſpecially the laſt of them, conſiderable medicines. The firſt medicine, that I attempted, was, by putting to ſalt of tartar oil of human blood, inſtead of oil of turpentine; and by keeping them long, and ſtirring them frequently, in the open air, to make ſuch a ſaponary concretion, as is not unknown to many in *London* by the name of *Matthews*'s corrector, which, as he made it with common oil of turpentine, though it ſeem but a ſlight compoſition, is yet eſteemed and employed with good ſucceſs by ſome doctors of phyſic and other practitioners in *London*. To make the other medicine, we endeavoured to unite, by long digeſtion, the ſalt, ſpirit,

ſpirit, and oil of human blood, into a mixture, which ſome chymiſts (for their terms are not by all of them uſed in the ſame ſenſe) call a clyſſus: but, having begun th s, without having had time to finiſh it, we ſhall ſay no more of it, but that divers chymiſts may not improbably look upon this ſort of compoſitions, as one of the nobleſt ſort of preparations, that many a drug is capable of.

Particulars referrible to the third part of the HISTORY.

EXPERIMENT I.

A YOUNG man having bled into a porringer, and the blood having been kept ſeveral hours, that a ſufficient ſeparation might be made of the coagulated or conſiſtent part and the fluid, the fibrous portion and the ſerum were ſeparately weighed; and the difference of the two maſſes, in point of weight, was not ſo great as one would have expected, the curdled part of the blood weighing about ſix ounces, and the ſerous part not many drams from that weight. This trial is here ſet down, that, by comparing it with ſome others, one may ſee what difference there is between the bloods of ſound perſons, as to the proportion of the ſerum, and the concreted part.

EXPERIMENT II.

HUMAN urine having firſt (that I know of) by the very ingenious Mr, *Hook*, and oftentimes by me, been obſerved, when frozen, to have on the ſurface of the ice figures not ill reſembling combs or feathers; the great affinity generally ſuppoſed to be betwixt urine and the ſerum of blood made me think fit to try at once, whether this laſt named liquor would freeze with ſuch a degree of cold, as would eaſily, and yet not very eaſily glaciate water, and whether, in caſe it ſhould freeze, the ice would have a ſurface figured like that of frozen urine. But, having for this purpoſe expoſed ſome ſerum of human blood to cold air, in two freezing nights conſecutively, the ſerum was not found to congeal, though ſome grumous parts of the ſame blood did, as has formerly been noted; yet I ſcarce doubted, but an exceeding hard froſt would have produced at leaſt a thin plate of ice upon the ſurface of our liquor. And to confirm this conjecture, we took the ſame ſerum, and having ſtrained it through a linen cloth, to ſeparate the liquor, as much as by that way we could, from any clotted or fibrous parts, that might have lain concealed in it, we put it into a ſhallow concave glaſs, and laid that upon ſome of our frigorific mixture made of ice and ſalt, which we have deſcribed and often made uſe of in the hiſtory of cold. By this means the expoſed ſerum, being frozen from the bottom upwards, there appeared here and there upon the ice contiguous to the air certain figures, that did not ill reſemble thoſe of conglaciated urine.

EXPERIMENT III.

HAVING formerly had occaſion to obſerve, that man's urine would tolerably well ſerve for what they call an inviſible ink; and hving conſidered (when I remembred this) the great affinity, that is ſuppoſed to be between urine and the ſerum of blood, I thought fit to try, whether the latter might not be employed like the former to make a kind of inviſible ink. To this effect we took ſome ſerum of human blood, and having dipt a new pen in it, we traced ſome characters upon a piece of white paper, and having ſuffered them to dry on, we held the unwritten ſide of the paper over the flame of a candle, keeping it always ſtirring, that it might not take fire: by which means, the letters, that had been written, appeared on the upper ſurface of the paper, being, though not of an inky blackneſs, yet of a colour dark enough to be eaſily legible, and very

very like to ſome others, that, having been purpoſely written with freſh urine, and made viſible, by heat, were compared with them.

Particulars referrible to the fourth part of the HISTORY.

EXPERIMENT I.

I confeſs the defectiveneſs of our hiſtorical knowledge of human blood extravaſated has been ſuch, that among the authors I have had occaſion to peruſe, I have met with ſo few matters of fact delivered upon their own knowledge, that the things I have thought fit to tranſcribe out of their books into this little tract, do ſcarce all of them together amount to half a ſheet of paper: but yet I would not impute this penury, either to the lazineſs or the ignorance of writers, but rather to this, that they wanted ſome perſon, exerciſed in deſigning natural hiſtories to excite their curioſity, and direct their attention; there being many, that would enquire, if they knew what queſtions were fit to be aſked about a propoſed ſubject, as for inſtance human blood, and what reſearches ought to be made to diſcover its nature. Upon this account I hope, that after ſome time the foregoing ſcheme of titles, and the papers, that refer to it, will give occaſion to a great many more experiments and obſervations about the blood, (and perhaps other liquors of the human body) than hitherto have been publiſhed by others, or are now imparted by me. Which laſt words I ſet down, becauſe I would not be thought guilty of the vanity of pretending to have near exhauſted the ſubject I have treated of; ſince, beſides other deficiencies, I now perceive, that I wholly omitted a conſiderable title, which might either have been referred to the primary ones of the firſt order, or employed as a kind of preliminary to the ſecondary titles of the hiſtory of the ſpirit of blood. This pretermitted title ſhould have been, *of the ſeveral ways of diſtilling human blood*; ſince, according to theſe, the produced ſpirit, ſalt, *&c.* may be conſiderably diverſified.

UPON this account I thought fit to diſtill three portions of dried blood, each with a differing additament. The firſt with a mineral alcali, quicklime: the next with a vegetable alcali, calcined tartar: and the third with a ſulphureous acid, oil of vitriol. And, though ſome accidents kept me from proſecuting the trials as I deſired, yet the firſt having ſuceeded indifferent well, and the others not having wholly miſcarried, I ſhall ſubjoin the accounts of all three, as they were ſet down in my notes.

HAVING obſerved (*Exper.* I.) that divers bodies, when they were diſtilled with quicklime, afforded liquors differing from thoſe they would have yielded, if they had been diſtilled, either *per ſe*, or with ſome vulgar additaments; we took ℥v. of concreted, but not dried human blood, and having mixed it with an equal weight of quicklime, (ſuch as I could procure, but not ſo ſtrong as I have often ſeen,) we diſtilled it by degrees of fire in a retort placed in ſand, by which means we obtained a large proportion of reddiſh ſpirituous liquor, which did not ſeem conſiderably phlegmatic, together, with ſome oil, which was but in very ſmall quantity, the reſt being probably kept back; (and perhaps ſome of it deſtroyed) by the lime: and of this little oil, that did come over, there was a ſmall portion, that ſunk in the ſpirit, the reſt ſwimming upon it.

THE abovementioned ſpirit being put into a ſmall head and body, was ſet into a digeſtive furnace, to rectify at leiſure with a very gentle heat, and the receiver was three or four times ſhifted, that we might obſerve, what difference, if any, there would be betwixt the ſucceſſively aſcending portions of liquor. The firſt ſpirit, that came over, did not ſmell near ſo rank, as that is wont to do, that is diſtilled *per ſe*. This obſervation belongs alſo to the three or four ſucceeding portions of liquor; probably, becauſe

because the lime better freed the spirit of the first distillation from the fætid oil, many of whose particles are wont, though unperceivedly, to mingle with it, when it is drawn over without additament. The rectified spirit, which was clear and colourless, had a taste much stronger than its smell; for a small drop of it upon the tongue had something of fieryness, that was surprizing, and lasted longer than one would wish; which made me doubt, whether the spirituous part of the blood had not carried up with it some of the fiery parts of the quick-lime; which doubt, if future trials resolve in the affirmative, one may expect some uncommon effects from such a spirit, which, in this case, would be enriched with a kind of volatilised alcali, a thing much desired by many chymists and physicians. Upon occasion of this suspicion, we dropped a little of it into a strong solution of sublimate in fair water, and it seemed at the first contact to make a precipitate a little inclining to yellow, (as I have observed the saline parts of quick-lime to do in a greater measure,) though afterwards the precipitate appeared white, like that made with ordinary volatile liquors of an urinous nature.

But, because I expected, that our alcalisate spirit of blood, if I may so call it, would have some peculiar qualities, discriminating it from the spirit drawn without addition; I thought fit to make a few trials with it, whose event justified my conjectures. For having put into a glass egg with a slender neck some of our well rectified spirit, it did not then afford any volatile salt in a dry form, (though afterwards, if I mistook not, by another trial, we at length obtained a little;) and having continued the trial somewhat obstinately, we found the spirit to have, by the action of the fire, lost its limpidness, and to have been made muddy or troubled.

Having mingled another portion of it with a highly rectified ardent spirit, and kept them all night in the cold, no coagulation ensued, nor could we perceive any after it had been kept divers hours in a moderate heat: but the mixture acquired a yellow colour, and let fall, somewhat to our surprize, a pretty deal of darkish powder, though not enough to invite us to make any trials upon it.

We put to another parcel of our spirit some good spirit of salt; but, though they smoked much at their meeting, yet we observed no noise nor bubbles upon their commixture.

And having mingled another portion with oil of vitriol, though there was produced a very great smoke, and besides that an intense degree of heat, (the quantity of the matter considered,) yet there was no visible ebullition, nor any noise or bubbles produced, but the colour of the oil of vitriol was very much heightned, the mixture growing almost red.

From these, and the like phænomena, one may gather, that our alcalisate spirit of blood is in several things differing from the simple. Whether this disparity will make it a more potent medicine, or make it, by too much participation of the fiery parts of the lime, a less safe remedy, future experience must discover. But it seems not improbable, that either as a medicine, or as a menstruum, if not in both capacities, it may be a not inconsiderable liquor; for which reason I have made my account of it the more circumstantial.

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 the

the calcinatum of nitre and tartar afforded us, at the very firſt diſtillation, a ſpirit leſs ſtrong; but withal, ſo much volatile ſalt as covered almoſt all the inſide of the receiver, not now to mention the difference of their reſpective *caput mortuums*. And, though the ſtrong ſaline ſpirit of blood made with quick-lime did not, as we lately noted, make an effervecence with acid ſpirits, yet this volatile ſalt readily did it upon the affuſion of ſpirit of ſalt.

EXPERIMENT III.

BESIDES the fixt alcaliſate additaments, with which I diſtilled the dried blood of men, I thought fit to add to it a very acid additament, *viz.* oil of vitriol; and this, the rather, becauſe I had long ſince found by trial, (and, if I miſ-remember not, have elſewhere related) that this liquor being mixed with ſome other bodies, particularly with ſome belonging to the animal kingdom, did in an odd manner mingle its own ſubſtances (for I take it not to be a ſimple body) with them, and notably diverſify the products of the diſtillation. We put therefore upon ℥iij. of powdered human blood an equal weight of oil of vitriol, and left them for ſome time together, to try, if by the action of this corroſive menſtruum, though upon a body not of a mineral nature, ſome heat would not be excited; and accordingly we found, that after a while, though not at the very firſt, the mixture grew ſenſibly warm. Then we removed the retort into a ſand furnace, and diſtilling it by degrees of fire, we had a ſpirit, which was preceded by a pretty deal of phlegmatic liquor of an odd ſulphureous ſmell, but ſo ſtrong and laſting, that I could not but wonder at it. The *caput mortuum* I was fain to let alone, becauſe I had ſome inducements to ſuppoſe, that it was of ſo compounded a nature, that I ſhould not, in my preſent circumſtances, have the opportunity to examine it thoroughly. But, it ſeemed remarkable, that, notwithſtanding the great acidity of oil of vitriol, and the fixative power it exerciſes on many bodies wherewith it is committed to diſtillation, our experiment afforded us a pretty quantity of volatile matter in the form of a white ſalt. But, indeed the ſmell and taſte of it were ſo uncommon, that I was troubled I had not then conveniency to examine it carefully, much leſs, to try, whether it had any peculiar virtues or operations in phyſic; though I had then by me a glaſs inſtrument, that I purpoſely provided to obviate the great inconvenience, that is uſually met with, and has been often complained of by me as well as others, in the way chymiſts are wont to imploy, when they are put to make repeated ſublimations of volatile ſalts, whether alone or with additaments. Of this inſtrument I cannot now ſtay to give you an account; but if it continue to appear as uſeful as expeditious, I may hereafter do it by preſenting you one ready made.

HAVING ſet down theſe preliminaries, I ſhall proceed to

EXPETIMENT I.

To ſome naturaliſts and phyſicians, that delight to frame hypotheſes, perhaps it may not be unwelcome to know, that, for curioſity's ſake, we attempted to make *aurum fulminans*, by precipitating a ſolution of gold (made in *aqua-regia)* with ſpirit of human blood, by dulcifying the precipitate with common water, and then drying it leiſurely; and that, by this means, we ſucceeded in the attempt.

EXPERIMENT II.

HAVING into a wide-mouthed glaſs put as much ſpirit of blood, as would more than cover the ball of a ſmall ſealed weather-glaſs, and ſuffered this inſtrument to ſtay a while, that the ambient liquor and the included might be reduced to the ſame temper, as to heat and cold; we poured on ſome ſpirit of verdigreeſe made *per ſe*, and obſerved,

ſerved, that, though this ſpirit, with ſome other volatile ſaline liquors, had a very differing operation, yet working on our ſpirit of blood, with which it made a conflict, and excited bubbles, there was produced in the mixture a degree of warmth, that was not inſenſible on the outſide of the glaſs, but was much more ſenſible in the thermoſcope, whoſe liquor, being hereby rarified, aſcended to a conſiderable height above the former ſtation, towards which, when the conflict of the two liquors was over, it began, though but ſlowly, to return.

EXPERIMENT III.

HAVING by degrees mixed our ſpirit of blood with as much good ſpirit of nitre, as it would manifeſtly work on, there was, not without noiſe, produced great ſtore of bubbles by their mutual conflict; which being kept in a quiet place, till after the liquors had quite ceaſed to work on one another, it began to appear, that, notwithſtanding all our care to free the ſpirit of blood from oil, ſomething of oleaginous, that had been concealed in it, had been manifeſted, and partly ſeparated by this operation; ſince not only a ſomewhat red colour was produced by it, but, after a while, the ſurface of the liquor was covered with a film, ſuch as I have often obſerved in ſaline liquors copiouſly impregnated with antimony or other ſulphureous bodies. And this thin membrane had its ſuperficies ſo diſpoſed, that looking upon it with eyes placed conveniently in reference to it and the light, it did to me, and other perſons, that did not at all look on it from the ſame place, appear adorned with vivid colours of the rainbow, as red, yellow, blue, and green; and, as I remember, in the ſame order, that theſe colours are to be ſeen in the clouds.

EXPERIMENT IV.

HAVING unexpectedly found, amongſt ſome other long neglected glaſſes, a vial, that was written upon above twelve years before, and inſcribed ſpirit of human blood, it appeared to have been, by I know not what accident, very looſely ſtopt, and yet not ſo, as to give me cauſe to think, that the liquor was much waſted: but notwithſtanding this, and that the liquor had acquired a deep colour, almoſt like that of red wine; yet it was ſo diſpirited and ſtrengthleſs, that it appeared to be very little other than nauſeous phlegm. Which obſervation I therefore think not unworthy to be preſerved, becauſe by it we may gueſs, how little a portion of the noble and genuine ſpirit or ſalt may ſuffice to make a liquor paſs for ſpirit of human blood.

EXPERIMENT V.

IN a froſty ſeaſon we expoſed, late at night, two or three ſpoonfuls by gueſs of ſpirit of human blood, that was not of the beſt, being, at the utmoſt, but moderately ſtrong. And though the cold of that ſeaſon had thoroughly frozen a vial almoſt full of oil of vitriol, and the night, wherein our ſpirit was expoſed, was (at leaſt) moderately froſty; yet, the next morning, we did not find ſo much as any ſuperficial ice upon it: but having removed the vial into a mixture of powdered ice and common ſalt, we found, in no very long time, that moſt part of the ſpirit was turned into thin plates of ice, which joined cloſe together, and had their edges upwards, like thoſe of the leaves of a book, when it is held with its back downwards.

EXPERIMENT VI.

To make a further trial of that imperfect one mentioned in the eighth ſubordinate title, we took a clot of human blood of the bigneſs of a bean, or thereabouts, and having put it into a vial in ſuch manner, that that part, which before was contiguous to to the air, and for that reafon was florid, was now the undermoft, and the other, which was blackifh, lay now uppermoft, we made hafte to pour upon it as much fpirit of human blood, as was more than fufficient to cover it, and perceived, that the contact of it prefently began to leffen the blacknefs of the furface of the blood, and bring it to a confiderable degree of floridnefs: and to try whether that would continue, we ftopt the vial, and fet it by till the next morning, (for it was then night,) when looking upon it, we found the fuperficial colour not to be black, but ftill red.

EXPERIMENT VII.

UPON the powder of dried human blood we put (in a fmall vial) fome of the rectified fpirit of human blood, which quickly diffolved part of it, and acquired a deep and pleafant colour. But highly rectified fpirit of wine being put upon fome of the fame powder in a like glafs, did not in many hours acquire any manifeft tincture, and got but a pale yellow one, even after having been for a longer time kept in a moderate heat. And yet common water, being put upon another portion of the fame powder, did quickly enough appear, by the colour it acquired, to have diffolved a pretty deal of it.

EXPERIMENT VIII.

SOME of our fpirit of human blood being put upon fome curious vitriol, that I had as a rarity (if I miftake not) from the *Eaft-Indies*, part whereof was in lumps, and part beaten to powder; that liquor, which was put upon the former, being able to diffolve it but flowly, made little or no froth; but the fpirit, that was put upon the latter, by haftily working on it, produced a manifeft one. And the folutions made of both parcels of vitriol were of a deeper and more lovely blue, than the mineral itfelf had been: nor did I obferve in them any precipitate of a dark colour, as I have done upon the mixture of fpirit of urine and ordinary vitriol.

EXPERIMENT IX.

HAVING with a clean pen drawn fome letters upon white paper with fpirit of human blood, and, as foon as it was dry, moved the unwritten fide over the flame of a candle, we found, that this liquor may, for a need, be employed as an invifible ink, that feemed to be fomewhat better, than thofe formerly mentioned to have been afforded us by ferum and urine.

EXPERIMENT X.

HAVING found by trial, that divers falts, fome that are volatile, and fome that are not, being put in powder into water, will, whilft they are diffolving, fenfibly refrigerate it; and on the other fide, that fome very fubtile fpirits actually cold, being put into cold water, will quickly produce in it a fenfible warmth, I thought it would not be amifs to try, what fpirit of human blood would do, when employed after the fame manner. Having therefore placed a fealed thermofcope in an open-mouthed glafs, furnifhed with as much diftilled water as would cover the ball of the inftrument, we left it there for a while, to bring the internal liquor and the external to the fame degree of coldnefs. Then we poured upon the immerfed ball two or three fpoonfuls of fpirit of human blood (which was all we could fpare for this trial) but perceived very little alteration to enfue in the thermofcope, only that it feemed the fpirit of wine in the ftem did a little, and but a very little, fubfide; which effect (though it had been much more manifeft) I fhould not have been furprized at, partly becaufe I found fpirit of urine to have a like, or fomewhat more confiderable effect, and partly becaufe I remembered, what

what I elfewhere relate about the operation of the pure falt of human blood upon diftilled water; which liquor I therefore make ufe of in thefe and many other experiments, becaufe in our common pump-water or well-water, and in moft other common waters, I have obferved a kind of common falt, which, though in very fmall quantity, makes it apt to coagulate with, or precipitate, fome kind of faline corpufcles, whether more fimple, or compounded. But before I quite difmifs the lately recited experiment, I muft acknowledge, that I dare not acquiefce in it; fince probably the effect of the fpirit of blood would have been more confiderable, if I had been furnifhed with a fufficient quantity of it to pour into the water.

EXPERIMENT XI.

INTO a flender cylindrical vial we put filings of copper, more than enough to cover the bottom, and then pouring on fome fpirit of human blood, till it reached about an inch above the filings, we ftopt the glafs clofe; and, as we expected, the menftruum diffolved fome of the metal, and acquired upon it a deep ceruleous colour, which, by keeping the veffel in a quiet place for fome days, did by degrees difappear, and left the liquor like water. And then the glafs being unftopt, there did, as was expected, appear a fine blue furface on the confines of the air and the liquor, in a minute of an hour or lefs; and this fine colour extending itfelf downwards, was in no long time diffufed through the whole body of the liquor, and that fo plentifully, as to render it almoft opacous. But, though I kept the glafs many days after well ftopt, yet (whether it were, that there was too much air left in the vial, or for fome other reafon) the colour did not difappear, as was expected, but continued very intenfe. This may confirm and diverfify an experiment related in the thirteenth title of the fourth part of the memoirs.

EXPERIMENT XII.

IT is not only upon copper, in its perfect metalline form, but by nature itfelf embrionated in, or blended with, ftony matter, that our fpirit of human blood did manifeftly work: for having poured fome upon well powdered *lapis Armenus*, the liquor did, even in the cold, and in no long time, (for it exceeded not a few hours) acquire a deep and lovely blue, almoft like the folution of filings of crude copper made with the fame menftruum.

The POSTCRIPT.

AND here, Sir, I fhall at length difmifs a fubject, about which I now perceive I have already entertained you much longer, than at firft I imagined. And yet, if I prevail with you, your trouble is not quite at an end; fince I exhort you to take the pains, for your own fatisfaction and mine, to try over again fuch of the foregoing experiments, as you fhall judge likely to be of a contingent nature. For, though I hope you will do me the right to believe, that I have as faithfully as plainly delivered matters of fact, without being biaffed by hypothefes, or aiming at elegance, yet my exhortation may be reafonable: for I have obferved human blood to be a thing fo diverfifiable, by various circumftances, and efpecially by the habitual conftitution of the perfon, that bleeds, and his prefent condition at the very time of phlebotomy, that I dare not undertake, that every repeater of the like experiments with mine will always find the events to be juft fuch, as I have recited mine to have had. Nay, I dare not promife myfelf an exact uniformity of fucceffes, even when I myfelf fhall reiterate ſome (of the nicer) of my own trials; eſpecially if I can do it, as I deſire, with greater quantities of blood than (for want of them) the firſt were made with.

To the particulars already delivered in order to the hiſtory of human blood, I could now, Sir, add ſome others, if time and diſcretion would permit me to do it. For, as little cultivated as the ſubject has been, I found it not ſo barren, but that, whilſt I was delivering ſome trials concerning it, the conſideration of thoſe, and of the nature of the thing, ſuggeſted new ones to me. But it is high time I ſhould break off an appendix, that being but a rhapſody of the notes and other things, that have occured to me ſince the memoirs were written, may, I fear, ſeem already too prolix, as well as confuſed. I do not forget, that the two laſt ſubordinate titles of the fourth part of the memoirs concern the external and internal uſe of the ſpirit of blood in phyſic; and that therefore perchance it may be expected, that I ſhould here add ſome experiments or obſervations relating to thoſe titles. But I hope the lately mentioned reaſons, and my juſt backwardneſs to part with ſome of them, becauſe they are not yet finiſhed, will make you eaſily excuſe my laying them aſide; which I am like to do long, unleſs you and your learned friends ſhall peremptorily require them of me in a fitter ſeaſon than this, in which ſome occaſions, that I cannot diſpenſe with, call me off to other employments, and oblige me to leave a further inquiry into this ſubject to yourſelf, and thoſe able profeſſed phyſicians, who have, as well more obligation, as more ability than I, to purſue it effectually. This I may well hope, that you and they will do, ſince, upon a curſory review of a part only of what I have written, ſo many things ſprang up, even in my thoughts, as original trials, if I may ſo call them, or as other things fit to be further conſidered, that I perceived it would not be difficult to increaſe the appendix, by two ſorts of particulars; the one made up of *deſigned* experiments, that is, ſuch as have not yet been tried, and yet ſeem worthy to be ſo, (to which it is probable our excellent *Verulam*, would have given the title of *hiſtorio deſignata:)* the other ſhould conſiſt of ſuch trials, as I call ſuccedaneous experiments, that is, ſuch as I intended ſhould be made upon the blood of beaſts, in ſuch caſes and circumſtances, wherein the blood of men either cannot be had, or ought not to be procured. When I ſhall next have the happineſs to converſe with you, you may command a ſight of what I have drawn up of this kind. And, if GOD ſhall pleaſe to vouchſafe me health and conveniency, I may perhaps (for I muſt not abſolutely promiſe it) offer you what *addenda* have occured to me, as things not unfit to make way for a more copious, and leſs unaccurate ſcheme of titles, ſuch as thoſe, that in the firſt part (of the memoirs) are called *titles of the ſecond claſſis, or order:* for which ſcheme I was the rather invited to think it fit materials ſhould be by ſome body provided, becauſe ſecond thoughts made me ſenſible, that the particulars compiled in this ſmall book come far ſhort (as I lately acknowledged, and you will eaſily believe) of comprizing all, that ſhould and may be known of ſo noble and uſeful a ſubject, as I have ventured to treat of. And I will freely confeſs to you, on this occaſion, that, for my part, in the proſpect I have of the future advancement of human knowledge, I think moſt of thoſe virtuoſi, that now live, muſt content themſelves with the ſatisfaction of having employed their intellects on worthy objects, and of having induſtriouſly endeavoured, by promoting uſeful knowledge, to glorify GOD and ſerve mankind. For, I preſume, that our enlightened poſterity will arrive at ſuch attainments, that the diſcoveries and performances, upon which the preſent age moſt values itſelf, will appear ſo eaſy, or ſo inconſiderable to them, that they will be tempted to wonder, that things to them ſo obvious, ſhould lye ſo long concealed to us, or be ſo much prized by us; whom they will, perhaps, look upon with ſome kind of diſdainful pity, unleſs they have either the equity to conſider, as well the ſmallneſs of our helps, as that of our attainments; or the generous gratitude

tude to remember the difficulties this age furmounted, in breaking the ice, and fmoothing the way for them, and thereby contributing to thofe advantages, that have enabled them fo much to furpafs us. And fince I fcruple not to fay this of thofe fhining wits, and happy inquirers, that illuftrate and enoble this learned age, I hope you will not think, that I, who own myfelf to be more fit to celebrate than rival them, would diffuade you from improving and furpaffing the flight performances, that are, in this little tract, fubmitted to your judgment by,

SIR,

Knightfbridge,
Dec. 22, 1683.

Your very humble fervant.

EXPERIMENTS and CONSIDERATIONS about the Porofity of BODIES, in Two ESSAYS.

To the READER.

THE reader is to be advertifed, not to expect, in the following effay, a regular, or fo much as a coherent difcourfe: for it was intended only as a collection of loofe experiments and obfervations, about the porofity of the parts of bodies belonging (as chymifts fpeak) to the animal kingdom, and laid (not to fay thrown) together, in order to what I had thoughts of offering towards an intelligible account of occult qualities. I am not ignorant, that even one of the moft antient and famous of phyficians hath faid, that a man's body is (almoft) every where perfpirable. But I judged, that a doctrine of fuch moment, and which divers things in the theory and practife of after phyficians may make one think they either difbelieved or difregarded, did not deferve to be flightly delivered, and in general terms; but to be narrowly confidered, and likewife made out by particular inftances, whofe appliablenefs and ufefulnefs to explain divers obfcure phænomena may hereafter appear much greater, than, perchance at the firft fight, they will be thought.

AND the foregoing advertifement, with a light change, which, it is prefumed, the reader may eafily make of himfelf, is to be extended to the effay tacked to this about the pores of folid bodies, and fo may excufe the abfence of a diftinct preface to it.

Of the POROUSNESS of ANIMAL BODIES.

AS the moft numerous part of the pores of bodies is too minute to be feen, fo the contemplation of them has been thought too inconfiderable to be regarded. But when I confider, how much moft of the qualities of bodies, and confequently their operations, depend upon the ftructure of their minute, and fingly invifible particles, and that to this latent contexture, the bignefs, the figure, and the collocation of the intervals and pores do neceffarily concur with the fize, fhape and difpofition, or contrivance of the fubftantial parts, I cannot but think the doctrine of the fmall pores of bodies of no fmall importance to natural philofophy. And I fcarce doubt, but if fuch little things had not efcaped the fight of our illuftrious *Verulam*, he would have afforded a good porology (if I may fo call it) a place, (and perhaps not the loweft neither) among his *Defiderata*.

AND, though other employments and avocations hinder me attempting to treat of this fubject, as amply and particularly as it deferveth, or even as I had defigned in a fcheme drawn divers years fince, and feen by fome virtuofi; yet, not to leave a part of phyficks, that feems to me fo curious and important, altogether as uncultivated, as I found it, I fhall prefent you as many of the notes I had drawn together, about this fubject, as I can conveniently, (for I do not pretend to do it methodically) reduced to three heads: whereof the firft, which will challenge to itfelf this prefent effay, is *The porofity of animal bodies*, about which I fhall not be follicitous to marfhal my obfervations, fince they all confpire to fhew but this one thing; that the parts of animals, efpecially whilft thefe are alive, are furnifhed with numerous pores.

THOSE parts of the bodies of animals, wherein their porofity may be beft fhewn, feem to be their membranes, or fkins, the bones, the flefh, and coagmentations of membranes, flefh and juices; and therefore it would be proper enough to treat of thefe heads diftinctly, and give inftances of each of them in particular. But yet, I think, it will be more convenient to fet down, in order, the principal fountains, whence the poroufnefs of the fubftances belonging to the animal kingdom (as the chymifts fpeak) may be derived, and to annex to each of thefe the experiments and obfervations, upon which I argue from it, and which it will be eafy to refer, if that be thought fit, to this or that of the parts above-mentioned, (namely the membranes, bones, *&c.*) whereto they fhall (refpectively) appear the moft properly to belong.

CHAP. I.

THE firft thing, from which I will deduce the porofity we have been fpeaking of, is the frame or conftitution of the ftable parts of the bodies of animals. For the body of an animal being not a rude and indigefted lump of matter, but a curious engine, admirably framed and contrived for the exercife of feveral functions, as nutrition, generation, fenfation, and many differing local motions; it was neceffary, that it fhould be furnifhed with variety of diffimilar, and organical parts, not only very fkilfully, but very differingly contrived, congruous to the feveral ufes, for which they were defigned, or, if you pleafe, to the feveral functions they were to perform. And becaufe it will be eafily granted, that the corpufcles, that are fkilfully brought together, for fuch purpofes, muft be fo contexed, as not to touch one another exactly every where, it will readily follow, that they muft leave little intervals or pores between them; and that, confidering the multitude of particles, that muft go to the making up the body of the animal, and the great difference and variety, in point of bignefs and figure of the corpufcles, that are requifite to contex fuch differing parts, as membranes, fibres, bones, griftles, ligaments, veins, arteries, nerves, *&c.* both the number and the variety of the pores cannot but be very great. This argument will be much confirmed, by what there will be occafion to fay further to the fame purpofe, in the effay touching the porofity of even folid bodies. Wherefore I fhall now proceed to the fecond thing, whence we may derive that of animal fubftances.

CHAP. II.

THIS is afforded us, by confidering the nutrition of animals: for there being continually a great wafte made of their fubftance, partly by the exclufion of vifible excrements, and partly by the avolation of invifible fteams, this great lofs muft neceffarily, from time to time, be repaired by the fupplies afforded by nutrition, of which the beft, if not the only intelligible way of giving an account, is, to conceive, that the alimental juice, prepared chiefly in the ftomach, is impelled or attracted (for, to our prefent purpofe, it matters not which) to the parts of the body, that are to be nourifhed nouriſhed by it, and the corpuſcles of the juice inſinuate themſelves at thoſe pores they find commenſurate to their bigneſs and ſhape; and thoſe, that are moſt congruous, being aſſimilated, add to the ſubſtance of the part, wherein they ſettle, and ſo make amends for the conſumption of thoſe, that were loſt by that part before. This may be illuſtrated by what happens in plants, and eſpecially trees; in which, of the various corpuſcles, that are to be found in the liquors, that moiſten the earth, and are agitated by the heat of the ſun and the air, thoſe, that happen to be commenſurate to the pores of the root, are, by their intervention, impelled into it, or imbibed by it, and thence conveyed to the other parts of the tree, in the form of ſap, which paſſing through new ſtrainers, (whereby its corpuſcles are ſeparated and prepared, or fitted to be detained in ſeveral parts) receives the alterations requiſite to the being turned into wood, bark, leaves, *bloſſom, fruit, &c.* But to return to animals, our argument from their nutrition will be much confirmed, by conſidering, that in children, and in other young animals, that have not yet attained their due ſtature and bulk, the nutrition is ſo copious, as to amount to a continued augmentation: for as it is evident, that animals grow in all their parts, and each part according to all its dimenſions, in ſo much, that even the cavities of bones increaſe; ſo we cannot well conceive, how this can be done, unleſs the nutritive liquor be diſtributed through the whole body of the part, that is to be nouriſhed and augmented: and to this diſtribution it is requiſite, that the body abound with pores, into which the congruous particles of the juice may be intimately admitted, and, penetrating even into the innermoſt receſſes, may place or lodge themſelves in the manner, that is moſt convenient for the natural increaſe of the part. But the more particular declaration of this proceſs I leave to anatomiſts and phyſicians.

C H A P. III.

HAVING premiſed, once for all, that in this eſſay I often uſe the word ſkin, in the lax and popular ſenſe of it, without nicely diſtinguiſhing the *epidermis*, or *cuticula*, called in Engliſh the *ſcarf-ſkin*, from the *cutis* it inveſts, and ſticks cloſely to, I ſhall now proceed to another topic, whence the porouſneſs of animals may be argued; namely, the great plenty of matter, that is daily carried off by ſweat, and inſenſible tranſpiration. For it is confeſſed, that ſweat is diſcharged at the pores of the ſkin; and, ſince there is no penetration of dimenſions, we may ſafely conclude, that the matter, that is not waſted by ſweat, or by any other ſenſible way of evacuation, muſt have ſmall pores, or out-lets in the ſkin, at which it may iſſue in the form of ſteams; though no thing hinders, but that inviſible effluvia alſo may evaporate at the ſame pores with the ſweat, though for want of plenty, or groſsneſs, or a fit diſpoſition in the ambient, thoſe effluvia be not, at the orifices of thoſe pores, brought into little drops, ſuch as thoſe of ſweat.

THAT therefore the ſkins of a multitude of animals, though they ſeem cloſe to the eye, may be porous, may (as we have been ſaying) be argued in many of them from their ſweating: but becauſe all of them have not been obſerved to ſweat, as is wont to be particularly affirmed of dogs, we ſhall add ſome other inſtances to make it probable.

WE may, ſometimes, in the ſmooth ſkin of a living man diſcern pores, with good microſcopes; and, with one, that is none of the beſt, we may eaſily, on the inſide of gloves, which are made but of ſkins dreſſed, diſcern good ſtore of theſe little out-lets, ſometimes orderly enough ranged to make the ſight not unpleaſant. And though ſome of them may, I think be ſuſpected to have been made by the hairs, that grew on the ſkin before it was dreſſed, yet that greater numbers of them, than can be ſuppoſed to come from thence, are perforations that paſs quite through the leather, may, not improbably, be ſhewn, by the uſual practice of chymiſts, to purify quickſilver, by tying it up ſtrictly in a piece of kid's, or ſheep's leather, and wringing it hard to force it out; by which means the lower ſurface of the leather will be covered with a mercurial dew or ſweat, which will fall down and fly out, as the pores happen to open this, or that way, in a thick ſhower of globules, leaving the droſs behind in the leather. And though when a man's ſkin is tanned, it is of a greater thickneſs than one would expect, and that, which I employed, ſeemed almoſt as thick as a buckſkin glove; yet having had the curioſity to try the ſame experiment with the ſkin of a man's arm, I found the quickſilver would be ſqueezed out at the pores of that alſo. It is not neceſſary, that I ſhould here enquire, whether the little holes, unperceived by the naked eye, at which the ſweat is diſcharged, and perhaps the matter, that the body looſes by inſenſible tranſpiration, gets out, be not, at leaſt moſt of them, the orifices of ſmall excretory veſſels belonging to thoſe very numerous glandules, which the excellent anatomiſts *Steno* and *Malpighi* are ſaid to have diſcovered beneath the *cuticula*, and which, for their ſmallneſs and ſhape, have been called *glandulæ milliares:* I need not, I ſay, engage in this enquiry, ſince, according to this ingenious opinion alſo, the ſkin muſt be allowed a multitude of ſmall perforations or pores, and that is ſufficient for my purpoſe, from whenceſoever this poroſity proceeds in a man's ſkin. For the next obſervation will ſhew, that ſome membranes of animals may give paſſage to tranſpired matter, without being perforated by the excretory veſſels of glandules.

The membranes, or ſkins, under the ſhells of hens eggs, though they be very thin, are of a contexture very fine and cloſe, as may be confirmed by their reſiſting the ſharp corpuſcles of vinegar; and yet, that not only theſe ſkins, but the ſhells that cover them, are porous, may be inferred from the experiments I made of keeping them ſuſpended for a good while, and carefully counterpoiſed in good ſcales; for by theſe it appeared, that the eggs did, from time to time, manifeſtly loſe in weight; which could not reaſonably be imputed but to an inviſible tranſpiration, the rather, becauſe uſually in eggs, that have been kept long, there will be, at one end, a cavity, which is wont to increaſe with their age, and is made by the ſhrinking of the membrane from the ſhell, to accommodate itſelf to the diminiſhed quantity of matter, that remains to be involved by it.

When I conſider the plenty of matter, that is wont to be diſcharged daily by inſenſible perſpiration, eſpecially in healthful men, that exerciſe themſelves moderately, I cannot but think it probable, that the minute pores, that ſuffice for the carrying off ſo much matter, are very numerous, and are much more ſo than even, by the multitude of drops of ſweat, that ſerve to wet the ſkin, men are wont to imagine. For *Sanctorius*, in his excellent little tract *de medicina ſtatica*, affirms, that what is barely carried off by inſenſible tranſpiration does ordinarily amount to more, that is, diminiſhes more the weight of a man's body, than all the viſible excrements (whether groſs or liquid) put together, *Aph.* vi. He adds, if the meat and drink, taken in one day, amount to the weight of eight pound, the inſenſible tranſpiration ordinarily amounts to five pounds, or thereabouts: and elſewhere ſays, that ſometimes, in the ſpace of twenty-four hours, in the winter time, a healthy body may exhale fifty ounces, or more. And ſome trials, that I have carefully made upon myſelf, added to ſome others of a very curious as well as great prince, that made uſe of a like inſtrument, and did me the honour to acquaint me with the events, gave me no cauſe to reject *Sanctorius*'s obſervations, conſidering the difference, in point of heat, between the climate of *Italy*, where he writ, and that of *England*, where ours were made: only I fear there has been committed

committed an overſight by thoſe many, that aſcribe all the decrement of weight, that is not referrible to the groſſer excrements, to what tranſpires at the pores of the viſible parts of the ſkin, without taking notice of that great plenty of ſteams, that is in expirations diſcharged through the wind-pipe by the lungs, and appear manifeſt to the eye itſelf, in froſty weather; though they may be preſumed to be then leſs copious, than thoſe inviſible ones, that are emitted in ſummer, when the ambient air is much warmer. But though I look upon the wind-pipe as the great chimney of the body, in compariſon of thoſe little chimnies (if I may ſo call them) in the ſkin, at which the matter, that is waſted by perſpiration, is emitted; yet the number of theſe little vents is ſo very great, that the fuliginous exhalations, that ſteal out at them, cannot but be very conſiderable. Beſides that thoſe, that are diſcharged at the *aſpera arteria*, do probably, at leaſt for the moſt part, iſſue out at the latent pores of the membranes, that inveſt the lungs; which membranes may be looked upon as external parts of the body, in reference to the air, though not in reference to our ſight. But, to return to our eggs, we may ſafely allow a very great evacuation to be made at the pores of the ſkin in man, who is a ſanguineous and hot animal, ſince we ſee, that even eggs, that are ſtill actually cold, tranſpire. And I elſewhere mention the copious tranſpiration even of frogs, that are always cold to the touch, and the decrement in weight of ſome animals, ſoon after they are ſtrangled or ſuffocated, when their vital heat being extinct, no more fumes are emitted by expirations at the wind-pipe. To which ſigns may be added the trivial experiment of holding, in warm weather, the pulp of one's finger, as near as one can without contact, to ſome cold and ſolid ſmooth body, as to a piece of poliſhed ſteel or ſilver; for you will oftentimes ſee this body preſently ſullied or over-caſt with the inviſible ſteams, that iſſue out of the pores of the finger, and are, by the cold and ſmooth ſurface of the body, condenſed into viſible ſteams, that do, as it were, cloud that ſurface, but, upon the removal of the finger, quickly fly off, and leave it bright again.

THE perviouſneſs of the ſkin outwards may, not improbably, be argued from the quickneſs wherewith ſome medicines take away ſome black and blue diſcolorations of the ſkin, that happen upon ſome lighter ſtroke, or other contuſions. For, ſince theſe preternatural and unſightly colours are wont, by phyſicians, to be imputed to ſome ſmall portions of blood, that, upon the contuſion, is forced out of the capillary veſſels, that lye beneath the ſurface of it, and, being extravaſated, are obliged to ſtagnate there; it ſeems very likely, that if a powerful medicine do quickly remove the diſcoloration, that work is performed by attenuating, and diſſolving and agitating the matter, and diſpoſing it to tranſpire through the cutaneous pores, though perhaps, when it is thus changed, ſome part of it may be imbibed again by the capillary veſſels, and ſo, by the circulation, carried into the maſs of blood. Now, that there are medicines, that will ſpeedily work upon ſuch black and blue marks, the books and practice of phyſicians and chirurgeons will oblige us to admit. *Helmont* talks much of the great virtue of white briony root, in ſuch caſes; and a notable experiment made a while ago by a learned acquaintance of mine, in an odd caſe, did not give *Helmont* the lye. And I know an eminent perſon, who having, ſome while ſince, received from a ſtroke by a kick of an horſe on his leg, a very threatning contuſion, which made the part look black and frightful, he was, in a few hours, cured of the pain of the hurt, and freed from the black part of the diſcoloration, by the bare applicatian of the chopt leaves of hiſſop, mixed with freſh butter, into the form of a pultiſs.

NOR is it only the ſkin, that covers the viſible parts of the body, that we judge to be thus porous; but in the membranes, that inveſt the internal parts, we may reaſonably ſuppoſe both numerous and very various pores, according to the exigency of their peculiar and different functions or offices. For the two first causes of porosity, mentioned in this essay, are as well applicable to the membranes, that cover the internal parts, as the liver, the spleen, *&c.* as to the external skin, or membrane, that covers the limbs; and, in some respects, the transpiration through such pores seems more advantaged, than that through the pores of the surface of the body; since the parts that environ the spleen, liver, kidneys, *&c.* in man, are hot, in comparison of the ambient air, and being also wet, which the air is not, the laxity of the pores of the internal parts is doubly befriended. And perhaps it may be allowable to conceive, both the skin, that covers the limbs, and the membranes, that invest the internal parts of the body, to be like worsted stockings, waistcoats, *&c.* which, in their ordinary state, have a kind of continuity, but, upon occasion, can have their pores every way enlarged and stretched, in this or that manner, as the agents, that work on them determine them to be. This may be confirmed by what we manifestly see in the finer sort of leather, as that of kid or lamb, and by the latent pores, that may be opened in sheep's-leather, and man's leather, by the pressure of included quicksilver.

THIS porosity of a living man's skin, and other membranes, though internal ones, will the more easily be assented to, if it appear, that such thick and gross membranes, as the urinary bladders of dead animals, are porous, and penetrable even by water. This we tried, by putting some salt of tartar in a clean well dried bladder (which ought to be afterwards tied up close in the neck, lest the effect should be ascribed to the moist air) and leaving the lower part of the bladder, as far as the salt reached, immersed in common water, whose particles, by degrees, insinuated themselves into the pores of the bladder in plenty enough to resolve the salt of tartar into a liquor. And, that it may not be said, that the acrimony of the salt, by fretting the bladder, made way for the corpuscles of the water, I shall add, that the experiment succeeded, but much more slowly, when we tried it with sugar instead of salt of tartar. And there are some, who pretend that certain syrups made this slovenly way, which they would have pass for a secret, are very much preferable to those made of common water.

THAT the films, that line the shells of eggs, are of a very close texture, seems probable, as by other things, so by their resisting some liquors sharp enough to corrode the shell; and yet, that such membranes are pervious to liquors, that are none of the most subtile of all, we found by the ensuing experiment. This was made by taking an ordinary hen's egg, and keeping it for two or three days in distilled vinegar, or in strong crude vinegar; for then taking it out of the liquor, and wiping it well, it was visibly, and not inconsiderably, swelled, which I concluded to be from the ingress of some particles of the liquors at the pores of the skins, that invest the white of the egg; for we found nothing broken, though we made the trial more than once. And to be satisfied, that the manifest expansion proceeded from some other cause, than the mere dilatation of the white, or yolk, or both, we compared the weight of the egg, after it was taken out and well wiped, with that, which had been taken before it was put into menstruum, and found the egg, notwithstanding the loss of the shell, to be considerably heavier, than it was before its immersion.

I shall add, on this occasion, that by a more unlikely way, than that newly recited, both the egg, shell and lining of an egg, may be penetrated: for, notwithstanding the fine and close contexture of the membranes, that invest the eggs, the *Chinese* have a way of salting them in the shell, as I have been assured both by *English* and *Dutch* merchants trading to the *East-Indies.* And in one of the *Dutch* journals sent by the council of *Batavia* to their principals in *Holland*, and intercepted by an *English* man of war, I met with divers accounts of great numbers of salted eggs, that were such or such a day of such a month brought in by sea to *Batavia* or other ports. Long after which

which time meeting with an ingenious phyſician, that lived in *Batavia*, I learned by enquiry from him, that it is very true, that ſuch eggs are frequently met with in thoſe parts; he having divers times eaten of them there, ſome, that he judged to have been eitherboiled or roaſted before they were ſalted, and others, that were raw, when they came to be dreſſed for him, but yet retained a briny taſte. And, though the merchants I enquired of could not tell me, what way the *Chineſe* employed to ſalt their eggs, without making them unfit for common uſe; yet, by a trial, made with clay and brine, in which I kept the eggs for a competent time, I was perſwaded, that it was poſſible the *Chineſe* ſhould have the art aſcribed to them: for upon the breaking of an egg coated with clay, after it had lain for a competent time in brine, I found its taſte conſiderably ſalt, but was, by I know not what accident, hindered from proſecuting the experiment and endeavouring to make it more practicable and uſeful.

I knew a phyſician of more learning than virtue, who, being tormented with a violent and obſtinate cholic of a peculiar kind, was wont to relieve himſelf by clyſters of ſack; though he uſually found, that, not long after he had taken any of them, they would make him giddy, and fuddle him, as he himſelf confeſſed to me. But, upon this inſtance I lay not much weight, and leſs upon what was anſwered me by a great chirurgeon, who having practiſed his art in the *Weſt-Indies*, and being aſked by me, whether he had not dreſſed wounds and ulcers with the recent juice of tobacco (a plant I uſe to keep growing in my garden for its excellent virtues in cuts, burns, and tumors,) and whether, if he employed it, he did not find it emetic; he told me, among other things, that having divers times dreſſed with this juice a ſmall ulcer in a woman's leg, the patient ſoon after the application would grow ſick, and have her ſtomack turned, or actually vomit. But, as I was ſaying, on this inſtance I lay no ſtreſs, becauſe the corpuſcles of the tobacco might probably enough get in at the ſmall orifices of ſome corroded veſſels, and ſo be conveyed inwards, rather by the help of the circulation of the blood, than on the account of the porouſneſs of the parts. And therefore, I ſhall rather mention what has been related to me by an eminent phyſician of the famous college of *London*; namely, that he had divers times given himſelf a vomit, by a certain application of decocted tobacco to his wriſts and ſome other external parts: which brings into my mind, what is affirmed to have been obſerved in ſome children, that have ſcabbed heads, who have been made drunk by the application of clothes or ſpunges wetted in infuſion of tobacco, or of ſtrong liquors, and applied to the part affected; though, in this caſe, the inebriating particles may be ſuſpected to have got in, not at the meer pores, but rather at the orifices of the capillary veſſels, that were made acceſſible by ſuch little ſolutions of continuity, as are ſeldom wanting in ſcabby heads.

THAT children may be purged by outward applications, is aſſerted by ſome phyſicians; and an experienced perſon of that number has affirmed to me, that he can almoſt conſtantly do it by a plaiſter. Bnt it is more conſiderable, what was related to me by an eminent virtuoſo, who being indiſpoſed to believe ſuch things, a while before he told me the ſtory, was deſired by a curious perſon to ſhew him his hand; which the relator having done, the other took it in his hand, which was moiſtened (as was afterwards confeſſed) with a kind of ſubtile chymical oil, but ſo ſlightly, that the relator ſcarce minded it; till ſome time after, when he found himſelf preſſed with a motion like that, which a purge would have given him; for the other thereupon ſmiling, my acquaintance began to ſuſpect what the matter might be, and was in a ſhort time purged four times, without griping, or other pain or diſcompoſure. But, to return to the porouſneſs of membranes, it may ſerve to make way for your admitting it, to obſerve, that though lute-ſtrings be but ropes of fibres (which are at leaſt the chief parts, that membranes conſiſt of) dead, cold and ſtiff; yet, when the lute is in tune,

tune, they will ſometimes in wet weather ſwell ſo forcibly, as with noiſe and violence to break; which proceeds from the copious ingreſs of moiſt vapours into their pores, whereby they are not only ſhortened, but, as I have tried in nice ſcales, made manifeſtly heavier.

THE poroſity of the internal parts of animals by both the forementioned ways, (*viz.* of emiſſion and reception of corpuſcles,) may be confirmed by the things, that happen in ſome of the metaſtaſes or tranſlations (as the phyſicians call them) of the morbific matter in diſeaſed bodies. It is known to them, that are any thing converſant with hoſpitals, or the obſervations of phyſicians, that there do not ſeldom occur in diſeaſes ſudden removes of the matter, that cauſed them, from one part to another, according to the nature and functions of which, there may emerge a new diſeaſe, more or leſs dangerous than the former, as the invaded part is more or leſs noble. Thus oftentimes the matter, which in the ſanguiferous veſſels produced a fever, being diſcharged upon ſome internal parts of the head, produces a delirium or phrenitis; in the latter of which I have ſomewhat wondered to ſee the patient's water ſo like that of a perſon without a fever: the ſame febrile matter, either by a deviation of nature, or medicines improper or unſkilfully given, is diſcharged ſometimes upon the pleura, or membrane, that lines the ſides of the cheſt, ſometimes upon the throat, ſometimes upon the guts; and cauſes in the firſt caſe a pleuriſy, in the ſecond a ſquinancy, and in the third a flux for the moſt part dyſenterical. But, becauſe I ſuppoſe, that many, if not moſt, of theſe tranſlations of peccant humours are made by the help of the circulation of the blood, I forbore at the beginning of this ſection to ſpeak in general terms, when I mentioned them in reference to the porouſneſs of the internal parts of the body, and contented myſelf to intimate, that ſome of them may ſerve to confirm that poroſity.

THIS will not, perhaps, ſeem improbable, if we conſider, that it is in effect already proved by the ſame arguments, by which we have ſhewn, that both the ſkin and the internal membranes are furniſhed with pores, permeable by particles, whoſe ſhape and ſize are correſpondent to them. For we may thence probably deduce, that when a morbific matter, whether in the form of liquor, or of exhalations, chances to have corpuſcles ſuited to the pores of this or that part of the body, it may, by a concourſe of circumſtances, be determined to invade it, and ſo diſlodge from its former receptacle, and excite diſorders in the part it removes to.

CHAP. IV.

ANOTHER thing, whence the poroſity of animals may be argued, is their taking in of effluvia from without: for theſe cannot get into the internal parts of the body, to perform their operations there, without penetrating the ſkin, and conſequently entering the pores of it.

Now, that things outwardly applied to the body may without wounding the ſkin, be conveyed to the internal parts, there are many things, that argue.

AND firſt, it has been obſerved in ſome perſons, (for all are not equally diſpoſed to admit the action of particular poiſons) that cantharides being externally applied by chirurgeons or phyſicians, may ſoon, and before they break the ſkin, produce great diſorders in the urinary paſſages, and ſometimes cauſe bloody water. And, I remember, that having once had a bliſtering plaiſter applied by a ſkilful chirurgeon between my ſhoulders, though I knew not, that there were any cantharides at all mixed with the other ingredients, yet it gave me about the neck of my bladder one of the ſenſibleſt pains I had ever felt, and forced me to ſend for help at a very unſeaſonable time of night.

THE porousness of the skin may be also argued from divers of the effects, even of milder plaisters. For, though some plaisters may operate, as they closely stick to the skin, and hinder perspiration from within, and fence the part from the external cold; yet, it will scarce be denied, that many of them have notable effects upon other accounts, whereof none is so likely and considerable, as the copious ingress of the corpuscles of the plaister, that enter at the pores of the skin, and being once got in, act according to their respective natures and virtues. The like may be said of ointments, whose operations, especially on children (whose skin is ordinarily more soft and lax) are sometimes very notable. And I have known considerable things performed by them, in an internal disease of grown men, where I should scarce have expected a vegetable ointment should perform so much: I say, a vegetable ointment, for it is vulgarly known, that by mercurial ointments salivation may be excited; and sometimes, against the physician's will, the corpuscles of the quicksilver get so far into the body, that he is not able to get them out again.

WHAT we lately said of plaisters may be applied to those, that physicians call *pericarpia*, or wrist-bands; the better sort of which, though sometimes ineffectual, are oftentimes successful in stopping fits of agues, as I have frequently found in a mixture, elsewhere mentioned, of currans, hops, and bay-salt well beaten together; by which, by God's blessing, many, and I among others, have been freed from simple tertians, and either double tertians or quotidians.

THE argument of the porosity of animals, drawn from those things, that get in through their skins without breaking or wounding them, may be much strengthned, if it can be made appear, that those physicians do not deceive us, who ascribe sensible operations and virtues to things externally applied in so loose a way, that they do not so much as stick to the skin, or perhaps immediately touch it; such as some call *Periapta* and *Appensa*; divers of which are best known among us by the name of amulets; such as are the quills containing quicksilver or arsenick, that some hang about their necks, and wear under their shirts, against the plague and other contagious diseases; and the bloodstones, that others wear against hæmorrhages; and the stone, which the women use in the *East-Indies*, for a quite contrary effect, in *obstructione mensium*. That many of these external medicines answer not the promises of those, that extol them, having some of them no sensible operation at all, and others no considerable one, experience has assured judicious observers; but that some of them, especially on some patients, may have considerable, not to say admirable, operations, I confess, myself, by other motives, as well as authority, to be persuaded. Having been one summer frequently subject to bleed at the nose, and reduced to employ several remedies to check that distemper; that, which I found the most effectual to stanch the blood, was some moss of a dead man's skull (sent for a present out of *Ireland*, where it is far less rare, than in most other countries) though it did but touch my skin, till the herb was a little warmed by it. And, though I remember not, that I have known any great matter done to stop hæmorrhages by the bare outward application of other blood-stones; yet, of one, that looked almost like an agate, I admired the effects, especially upon a young and extraordinary sanguine person. To which I shall add a memorable thing, communicated to the experienced *Zwelfer* by the chief physician of the states of *Moravia*: for this learned man, whom he extols for a great physician and philosopher, assures him, that having prepared some trochiscks of toads, according to *Helmont*'s way, (which I remember I also was follicituous to prepare, but had not occasion to make trial of their virtue,) he not only found, that, being worn as amulets, they preserved him and all his domestics and friends from the plague (though he daily visited the infected) but that having caused these trochiscks to be put upon the plague sores of

If one would see this [passage] at large, he may find it at the end of [the essay].

ſeveral perſons, none of them died, but the venom of the peſtilential carbuncles was thereby ſo weakened, that the ulcers were afterward eaſily cured by vulgar remedies.

AND now, as to the difficulty, which I acknowledge not to be ſmall, to conceive, how bodies, actually cold, can emit effluvia, capable of penetrating (without moiſtening it) a membrane of ſo cloſe a contexture, as a man's ſkin; I ſuppoſe it will be much leſſened in the objector's opinion, by what he will meet with hereafter about the pores of bodies, and the figures of corpuſcles. For, ſuppoſing theſe to be congruous, it will not ſeem incredible, that the effluvia of amulets ſhould, in tract of time, get paſſage through the pores of the ſkin of a living body. And, to make this the more probable, I will give an inſtance in the ſkin of a dead animal. And, becauſe this requires a liquor I much employed in theſe trials about porology, though I have many years ſince, in another tract, taught how to make it for another purpoſe; yet I ſhall here repeat, that it is made by exactly mingling flower of brimſtone, powdered ſal-armoniac, and good quick-lime, in equal quantities, ſave that, if the quicklime, be not very dry and good, a fourth or fifth part muſt be ſuperadded; for theſe being nimbly mixed, and diſtilled by degrees of fire in a retort, till the ſand be at length brought to be almoſt red hot, there will come over a ſmoking ſpirit, which muſt be kept very carefully ſtopt, and which, for diſtinction's ſake, I alſo uſe to call the permeating menſtruum or liquor, and its expirations the penetrant, or permeating fumes.

AND now you will eaſily underſtand the experiment I was about to mention, which was this; we took a very clean piece of poliſhed copper, in want of which one of ſilver will ſerve the turn, and having lapt it up in a piece of either lamb's or ſheep's leather, ſo that it was every way incloſed, we then held it over the orifice of the vial, that contained the ſpirit, at a pretty diſtance from the liquor, whoſe fumes, nevertheleſs, did quickly (perhaps in a minute of an hour, or leſs) pervade the pores of the leather, and operate upon the included metal, as appeared by the deep and laſting tincture it had given to the lower ſurface of it, though the interpoſed leather itſelf was not deprived of its whiteneſs, nor at all ſenſibly diſcoloured; however, it ſmelt of the ſulphureous ſteams, that had invaded it. And if I miſ-remember not, the ſame experiment ſucceeded, though ſomewhat more ſlowly, when a double leather was interpoſed between the fumes and a new piece of copper coin. This will be thought the leſs ſtrange, when I ſhall come to ſome other inſtances of the penetrancy of theſe ſpirits. In the mean while I leave it to be conſidered, whether this may not ſuggeſt ſome conjecture at that ſtrange phænomenon, which is recorded by authors of good repute, that ſometimes in great thunders the lightening, among other operations, has been found to have manifeſtly diſcoloured men's money, without burning the purſes or pockets wherein it lay. For in our experiment, the ſteams, that in a trice pervaded the leather, the moſt uſual matter whereof purſes are made, were ſulphureous, as the ſmell argues, that thoſe, which accompany the fulmen, are wont to be; and whereas theſe, when they invade bodies, are uſually very hot, ours operated, when the liquor, that emitted them, was actually cold. And, if it be ſaid, that ſometimes their money has been found diſcoloured in their pockets, who were not ſtruck by the fulmen, but had it only paſs near them, it may be objected, that though the intire body, whether fluid or ſolid, if there be any of this latter kind, that is in Latin called fulmen (for our Engliſh word, thunderbolt, ſeems not ſo applicable to a fluid) did not touch them, yet it might ſcatter ſteams enough round about it, to cauſe the phænomenon. For confirmation of which I ſhall take notice, that a conſiderable perſon of my acquaintance having had the curioſity to aſcend a burning mountain in *America*, till the ſulphureous ſteams grew too offenſive to him, he told me, that, among other operations he obſerved them to have upon him, one was, that he found the money he had about him turned of a black and dirty colour,

lour, ſuch as I have obſerved our ſulphureous ſteams often give both to copper and to ſilver coins. But whether or no our ſpirits will juſtify the conjecture they invited me to mention, at leaſt their ſo eaſily pervading the ſkin of a dead animal may make it probable, that the ſkin of a living man may be eaſily penetrated by external ſteams, whoſe approach the eye does not perceive, and whoſe operations, though not inconſiderable, may therefore be ſuſpected. I leave to phyſicians to conſider, what uſe may be made of this obſervation, in reference to the propagation of contagious diſeaſes by the contact of infected air, diſtinct from the reſpiration of it, and by the penetration of the ſteams, that, iſſuing from divers bodies, invade the ſkin, and may perhaps be capable of operations, either hurtful or friendly, that are not uſually ſuſpected to proceed from ſuch cauſes, and are therefore miſ-aſcribed to others. And, on this occaſion, it will not be impertinent to add, that by hanging up ſheep's leather or lamb's leather in the free air, the vapours of it would ſo inſinuate themſelves into the pores in wet weather, that a moderate degree of moiſture in the air would add to it a not inconſiderable weight, of which dry weather, whether hot or cold, would deprive it.

CHAP. V.

I MUST not in this place omit ſome inſtances very proper to manifeſt the penetrableneſs of membranes to fumes themſelves, if they be ſubtile enough for their pores, or correſpondent enough to them.

AMONG the obſervations publiſhed by phyſicians, I have met with ſome, by which it appears, that cantharides may have great effects upon the internal parts of the body, though they do not ſo much as touch the ſkin, but are placed at ſome diſtance from it, ſo that their effluvia muſt be tranſmitted through other bodies before they can penetrate that. The learned *Michael Paſchalius* mentions a chirurgeon, who was twice brought to void much blood with his urine, by ſome Spaniſh flies, that he carried about in a purſe or bag. And another doctor of note relates of another perſon, that came to complain to him that he piſſed blood, having carried about with him cantharides, though in his pocket; and adds, that a like caſe was recounted to him by *Helidæus*, whom he calls an eminent *Bolognian* phyſician.

Schenkii obſervation. lib. 7. obſ. 37.

WE ſee, that in linnen cloth, the finer and more ſlender the threads are, the cloſer and leſs porous, *cæteris paribus*, the linnen is: by analogy to which one may eſteem the thin film, that lines the ſhell of an egg, to be of an exceeding cloſe contexture; and yet, that even this film is not impervious to ſome fumes, I have found by the following trial.

To make this, we ſlowly and warily picked off a ſufficient part of the ſhell of an hen's egg from the ſkin, that lay juſt beneath it, and is wont to ſtick ſo cloſe to it, that their ſeparation, without injuring the membrane, is not eaſy. In this ſkin, being wiped, we wrapt up a flat piece of copper, whoſe ſurface was made bright, that the change of colour might be the better ſeen; and having kept this covered bit of plate over the fumes of our ſmoking liquor, lately mentioned, for a minute or two by our gueſs, we unfolded the ſkin, and found, as we expected, that the lower ſurface of the copper, which was it, that had been held over the fumes, was turned of a dark colour, which manifeſted, that even ſo fine and cloſely contexed a membrane was not only, as we have formerly ſhewn, penetrable by liquors, but readily pervious to our ſulphureous exhalations, though theſe were probably but faintly emitted, ſince the liquor they came from was then actually cold. But, in making the trial, it is fit to hold (as we did in that newly recited) the membrane againſt the light, to ſee if it be intire, and have eſcaped all thoſe little lacerations, that are hardly avoidable in ſevering it from the ſhell it ſticks ſo cloſe to. If this caution be neglected, it is eaſy to be impoſed on, by overlooking ſome little holes, that are not eaſily diſcerned when one looks down upon the ſkin, and yet may be ſufficient to make the experiment deceitful: but, though, when it is well made, it is a notable confirmation of the doctrine endeavoured to be eſtabliſhed in this paper, yet, I ſhall now ſubjoin a more conſiderable inſtance to the ſame purpoſe.

The porouſneſs of the internal membranes of the body will be more eaſily granted, if it be conſidered, that either the liquors, or the moiſt exhalations, whoſe action is promoted by the natural heat of the parts, keeps them conſtantly wet or moiſt, and thereby renders them more lax, and more penetrable by ſubtle ſpirits or other corpuſcles. In favour of this reflection I made the following experiment. We took a piece of a dried urinary bladder, which was judged to have been a calf's; and having lapt it about a new piece of ſilver coin, ſo that the bladder was ſingle, where it covered the lower ſide of the piece, we kept it for divers minutes, by gueſs, over the ſpirituous fumes of our often mentioned permeating liquor, but could not perceive, that the coin was thereby at all affected or tarniſhed: whence we concluded, that the pores of the dry bladder were too cloſe and narrow, to give paſſage to the expirations of the menſtruum. But preſuming, that moiſture would ſomewhat relax them, with another piece of the ſame bladder, made limber, by being a little wetted in common water, we lapt up another like piece of new coin, as we had done the former, and kept it at the ſame diſtance, as before, from the liquor, but not for ſo long a time: for after about two minutes, by gueſs, we removed and took out the piece, and, as we expected, found much of its lower ſurface (that regarded the liquor) deeply diſcoloured. Which experiment will not only juſtify, what I lately ſaid, of the greater laxity of moiſt than of dry membranes, but will be thought no mean confirmation of what is in this eſſay delivered about the poroſity of membranes; ſince the urinary bladder does, as anatomiſts well know, conſiſt of more than one membrane, though they ſtick ſo cloſe together, as to appear but one to the eye. And this bladder was ſpeedily penetrated by the fumes, that our liquor emitted in exceeding cold and froſty weather, though the bladder itſelf was not in the warm body of the live animal, but had been ſo long kept dried and cold, that probably the moiſture it introduced in ſcarce one minute of an hour could not reſtore it to the laxity it had, whilſt it was a part of the living calf.

One of the notableſt inſtances I ever met with of the poroſity of the internal membranes of the human body, was afforded me by that *Britiſh* nobleman, of whom our famous *Harvey* tells a memorable, not to ſay matchleſs ſtory. This gentleman, having in his youth by an accident, which that doctor relates, had a great and laſting perforation made in his thorax, at which the motion of his heart could be directly perceived, did not only out-live the accident, but grew a ſtrong and ſomewhat corpulent man; and ſo robuſt, as well as gallant, that he afterwards was a ſoldier, and had the honour to command a body of an army for the king. This earl of *Mount-Alexander* (for that was his laſt title) having married one of my neareſt kinſwomen, and having been told, that I was very deſirous to ſee, what I had heard ſuch ſtrange things of, very obligingly came, at a fit time to give me that ſatisfaction. In order to which he removed that, which covered the wide orifice of his hurt, and gave me the opportunity of looking into his thorax, and of diſcerning there the motions of the cone, as they call it, or mucro of the heart. But theſe things I mention but upon the bye, and becauſe of the ſtrangeneſs of the fact; the thing I principally intended relates to my preſent argument. Having then made ſeveral inquiries fit for my purpoſe, his lordſhip told me, that, when he did, as he was wont to do from time to time, (though not every day) inject with a ſyringe ſome actually warm medicated liquor into his thorax, to cleanſe and cheriſh the parts, he ſhould quickly and plainly find in his mouth the taſte

and

and ſmell of the drugs, wherewith the liquor had been impregnated. And I further learned, that, whereas he conſtantly wore, upon the uncloſed part of his cheſt, a ſilken quilt ſtuffed with aromatic and odoriferous powders, to defend the neighbouring parts and keep them warm; when he came, as he uſed to do after ſome weeks, to employ a new quilt, the fragrant effluvia of it would mingle with his breath in expiration, and very ſenſibly perfume it, not, as I declared I ſuſpected, upon the ſcore of the pleaſing exhalations, that might get up between his cloaths and his body, but that got into the organs of reſpiration, and came out with his breath at his mouth, as was confirmed to me by a grave and judicious ſtateſman, that happened to be then preſent, and knew this general very well. Other circumſtances I might add, but that I dare not truſt my memory for them, and unhappily loſt the paper, wherein the oddneſs of the things invited me to ſet them down, for fear of forgetting them.

THAT part of this narrative, which relates to injections, may be much confirmed by what is delivered by *Galen* himſelf, who ſays, that mulſum, or honeyed water, being injected at the orifice of wounds penetrating into the cavity of the thorax, has been obſerved to be in part received into the lungs, and diſcharged out of the *Aſpera Arteria* by coughing. And this he mentions, as a known thing, employing it as a medium, whereby to prove another.

THE mention, that has been made of the poroſity of membranes, brings into my mind, what I once obſerved at the diſſection, made by ſome phyſicians and anatomiſts, of a luſty ſoldier, that was hanged for I know not what crime. This man, though otherwiſe young and ſound, was obſerved to have been long moleſted with what they call a ſhort dry cough, which made us expect to find ſomething much amiſs in his lungs. But meeting with nothing there, we were at a loſs for the cauſe of this cough, till coming to conſider the internal part of the cheſt, we perceived ſomething on one of the ſides, by tracing of which we diſcovered, that between the pleura and the ſubſtance of the intercoſtal muſcles there was lodged a certain matter, of the breadth of a ſilver crown-piece, or thereabouts, of a roundiſh figure, and of the conſiſtence and almoſt colour of new, ſoft cheeſe; which odd ſtuff was concluded to have been the remains of ſome ill cured pleuriſy, and to have tranſmitted through the pores of the pleura, though that be a very cloſe membrane, ſome noxious effluvia, which ever and anon irritated the lungs into an irregular and troubleſome motion, and ſo produced the cough the malefactor had been moleſted with.

C H A P. VI.

I AM well aware, that it is far leſs difficult, to prove the permeableneſs of ſingle membranes, than that of ſuch a part of the body as ſeems to be an aggregate of ſeveral parts, which, in regard of their cloſe adheſion, are looked upon but as one part, to which, on that account, men commonly give a diſtinct name: but yet there are ſome phænomena, that ſeem to argue, that even ſuch compounded or reſulting parts, if I may ſo call them, are not deſtitute of pores; which, whether they be not ſome of them the orifices of exceeding ſlender and therefore unobſerved capillary veſſels, I muſt not now ſtay to inquire.

WHEN the cavity of the abdomen in thoſe hydropical perſons, that are troubled with an aſcites, is filled with water, or rather with a liquor, that I have found to be much more viſcous, it juſtly appears ſtrange, that by an hydragogue, or ſome appropriated purging medicine, great quantities of this groſs liquor ſhould in a ſhort time be carried off by ſiege, and perhaps alſo by urine; though to get into the cavity of the guts, or that of either of the kidneys, it ſeems neceſſary, that it permeate the tunicles, and other component parts, of the viſcera it gets into.

I know not whether, I may, on this occasion, take notice of what physicians observe to occur now and then in empyemas, that follow ill conditioned pleurisies. For it has several times been observed, that upon the bursting of such imposthumes into the cavity of the chest, the purulent matter hath been voided by siege and urine. I hesitate, as I was saying, whether I should alledge this phænomenon, as a proof of what I now contend for, till it be determined, whether this metastasis be made by transudation properly so called, or by the ingress of the pus into the imperfectly closed orifices of the vessels of the lungs; where being once admitted and mingled with the blood, they may with this circulating liquor arrive at the kidneys, or any other parts fitted to make a secretion of this heterogeneous matter.

BUT whatever be the reason or manner of it, we find, that the lungs do sometimes oddly convey things to distant parts of the body. And if I may here mention a thing, *cui honos præfationis est*, I shall add, that I have several times observed in myself, that when I had been an actor, or an assistant, in the dissection of a living dog, especially if his blood or body were rankly scented, I should divers hours after plainly find that odour in the excrements I voided by siege.

A famous chirurgeon and anatomist relates, that one, who was very ill of a dropsy judged to arise from a scirrhus of the spleen, coming to ask his counsel and assistance, though he judged the patient's case desperate, yet to content him, he ordered him to dip a very large spunge in good quicklime water, and having squeezed out the superfluous liquor, to bind it upon the region of the spleen, only shifting it from time to time. He adds, that after some months he was much surprized to receive a visit from this patient, with solemn thanks for his recovery; the outward medicine having, it seems, resolved the schirrhus, and concurred with nature to evacuate the hydropical humour. For the resolution of which hard tumour it seems necessary, that the sanative corpuscles of the external remedy should at length penetrate, not only the epidermis, and the true cutis, but the muscles themselves of the abdomen, and some other interposed parts.

THESE instances may be strengthened by an eminent observation of *Galen*, who takes notice, that bones being sometimes broken, without piercing the skin, that covers the part they belong to, when the callus is making, and the broken parts of the bone begin to be conglutinated together, a portion of that blood, which had flowed to the part affected, is carried to the skin, and permeates that, so as to wet and foul the dressings or bandages, that are kept upon the limb affected by the fracture.

CHAP. VII.

BONES, horns, and parts of the like substance, being those, that are granted to be the most solid of the bodies of animals, I come, in the last place, to shew by particular experiments, that these also have their pores: I say, by particular experiments, because, in a general way, their porosity has been already proved by the same arguments from their original texture, nutrition, augmentation, *&c.* that have been employed to manifest the porousness of animal substances in general.

THAT the nails of men, as well as their skins, are porous, may be gathered from their being easily and permanently tinged with divers metalline solutions, and particularly with those of silver in *aqua-fortis*, and gold in *aqua-regia*; the former of which solutions, though cold, will but too easily tinge the skin and nails it chances to touch, and make some little stay upon, with a dark and blackish colour; which I found not, that I could wash out with water, or, even with a far more penetrating and abstersive liquor. The like durableness I found in the purple spots, that I sometimes purposely made

made on my nails by letting ſome little drops of the ſolution of gold in *aqua regia* dry upon them, which I now and then did, to obſerve the way of the nails growth; for if the ſtain were made near the root of the nail, it would be ſtill, though very ſlowly, thruſt on by the new matter, till, after ſome weeks, it arrived to the further end of the nail, and was fit to be pared off with it. But this only upon the by. It is more to our purpoſe to take notice, that, though the menſtruums employed in the recited experiments be of themſelves very acid and corroſive, yet they are ſo changed by the metals they have diſſolved, that they are acid no more; the ſolution of ſilver being rather extreamly bitter, and that of gold of a kind of ſtiptic taſte, almoſt like that, which ſloes growing in the hedges are wont to be of.

IVORY is a thing too well known to need to be deſcribed. And, ſince it is generally looked upon (for I have had no opportunity to compare it with the bones) as the ſolideſt part of the vaſteſt of terreſtrial animals, experiments proving its poroſity, will be ſtrong preſumptions for that of the hardeſt parts of other animals. And the porouſneſs of ivory may be argued from the ſeveral ways of dying it with permanent colours; for in theſe colorations the tinctures, that make them, muſt penetrate into, and be lodged in the ſubſtance of the ivory, eſpecially when the ſubſtance remains ſmooth and glaſſy, as I have divers times made it do, when I employed fit menſtruums and metalline pigments. The ſolution I formerly mentioned of ſilver in *aqua fortis*, being laid upon ivory, will ſoon give it a dark and blackiſh ſtain, which is not, that I have found, to be waſhed off. I remember alſo, that I many years ſince taught ſome ingenious artificers, to adorn ivory with a fine purple colour, by moiſtening it with, and ſuffering leiſurely to dry on it a ſolution of gold made in *aqua-regia*, and, if occaſion required, allayed with water; nor needs either of theſe ſolutions be applied hot, any more than the ivory needs to be heated. Both which circumſtances favour the porouſneſs of the ſolid body.

COPPER diſſolved in *aqua-fortis* ſtains ivory with a bluiſh colour, as I have ſometimes ſeen in the hafts of knives, about whoſe coloration, nevertheleſs, another way is alſo employed. But I remember, that, without making uſe of any acid or corroſive menſtruum, I have even in the cold ſtained ivory with a fine and permanent blue like a turquois, by ſuffering to dry upon it as deep a ſolution, as I could make, of crude copper, in an urinous ſpirit, as that of ſal-armoniac.

BUT, now to return to bones, their growth, in all their dimenſions, does, as I lately noted, argue their poroſity; and the marrow, that is found in the great hollow bones, whether it nouriſh them or no, muſt itſelf be ſupplied by ſome alimental juice, that ſoaks, or otherways penetrates, into the cavities, that contain it.

NOR does it ſeem at all improbable, that blood itſelf may through ſmall veſſels be conveyed into the very ſubſtance of the bone, ſo as that the veſſels reach at leaſt a little way in it, though perhaps the liquor they carry may afterwards, by imbibition, be brought to the more internal parts of the bone. For not to urge, that we manifeſtly ſee, that on each ſide of the lower jaw nature has been careful to perforate the bones, and make a channel in the ſubſtance of it; which channel receives not only a larger nerve, but a vein and artery, to bring in and carry back blood for the nouriſhment of the teeth, by diſtinct ſprigs ſent from the great branch to the particular teeth: not to urge this, I ſay, (which I mention but to ſhew, that the opinion lately propoſed is agreeable to a known practice of nature) I have been aſſured by eminent anatomiſts, whom I purpoſely conſulted, that they have obſerved blood-veſſels to enter a great way into the ſubſtance of the larger bones; and one of them affirmed, that he had traced a veſſel even to the great cavity of the bone: which I the leſs ſcrupled to admit, becauſe it has been obſerved, that in younger animals the cavity is in great part furniſhed with

with blood, as well as marrow, and in thoſe larger pores, whereof many are found in the more ſpungy ſubſtance of divers bones, blood has been obſerved to have been lodged, as alſo in the ſpungy part of the ſkull, that lies between the two tables, as I have been aſſured by ſkilful eye-witneſſes.

The blackneſs alſo, that bones acquire, when put into a competent heat, and a peculiar kind of fatneſs, which they may by heat be made to afford, ſhew, that they harbour, even in their internal parts, ſtore of unctuous particles, ſeparable from the ſolid ſubſtance, (which ſtill retains its ſhape and continues ſolid) in whoſe pores they may thereby be argued to have been lodged. The lightneſs of bones, even when their cavity is acceſſible to air and water, is alſo a great ſign of their poroſity: and ſo is their being corroded by tinging liquors, if they be penetrative and well applied. I know not, whether I ſhould add on this occaſion, that having taken calcined and pulverized bones, ſuch as we uſed to make our cupels of and, after having given them a good heat, kept them for ſome time in the air, but in a well covered place, I found the imbibed moiſture of the air to have manifeſtly increaſed their weight; and that, I alſo obſerved, in a curious ſkeleton, where the bones were kept together by wires, inſtead of other ligaments, that though I kept it in a well covered place, not far from a kitchen fire, yet in very moiſt weather the bones ſeemed to ſwell, ſince thoſe joints, that were eaſy to be bent in dry weather, and that after ſeveral manners, would grow ſtiff and refractory, and indiſpoſed to be put into ſuch motions, when the weather was conſiderably wet. Theſe particulars (as I was ſaying) I am ſomewhat doubtful, whether I ſhould here inſert, becauſe one may ſuſpect the phænomena may proceed rather from ſomewhat elſe, than the imbibed moiſture of the air; and yet, I would not omit to mention theſe obſervations, becauſe I do not yet ſee any cauſe, to which they may more probably (or indeed ſo probably) be aſſigned.

And, on this occaſion, I ſhall ſubjoin ſome obſervations made on large and ſolid ox bones, which in one of my note books I find thus regiſtred, *Nov.* 15. We weighed two [entire or unbroken] marrow bones, and found the one to weigh ℥xxix. + ʒſs. and the other ℥xxiv. + ʒiv. + 30 gr. *Nov.* 24. the former weighed ℥xxix. + ʒvi. and the latter ℥xxv. + ʒi. + 30 gr. *Dec.* 28. the former weighed ℥xxix. + ʒiij. 55 gr. and the latter ℥xxiv. + ʒvij + 39 gr. *June* 7. of the following year, the former weighed ℥xxix. + ʒij. and the latter ℥xxiv. + ʒvij. But which obſervations, purpoſely made at differing times of the year, and in very good ſcales, it ſeems, that bones do plentifully enough imbibe the exhalations of the air, and emit them again, together with ſome of their own, according as the ambient happens to be diſpoſed. And theſe alterations argue the bones to abound with pores, ſince the external ſteams muſt have pores to receive them, and the effluvia muſt, upon their receſs, leave pores behind them.

I confeſs, that to think (as, with ſome anatomiſts, I lately ſeemed to do) that bones themſelves admit into their ſubſtance veſſels capable of conveying a nutritive liquor, we muſt ſuppoſe thoſe veſſels extremely ſlender: but, that it is not only poſſible, but ſomewhat credible there may be ſuch, I am induced to think by what is known to happen in that diſeaſe, which, from the country it moſt infeſts, is called the *Plica Polonica.* For, though one would think, that the hairs of men are much too ſlender to have cavities in them capable of viſible liquors; and though I have found it very difficult, even with a good microſcope, to perceive any cavities in the hair of a man tranſverſly cut; yet, not only ſome other writers of good note, but the judicious *Sennertus* himſelf, deliver, that in this diſeaſe (of which he particularly treats) it has been obſerved, that if the patients cauſe the intangled hair to be cut, as they ſometimes do, by reaſon of its naſtineſs or unſightlineſs, they are not only thereby endangered, but ſometimes the ſingle hairs will actually bleed, where the ends have been cut off; ſo that ſo thick a liquor, as blood, may be conveyed through veſſels, that can at moſt be but in a proper ſenſe capillary, and muſt be far leſs than hairs, if their perforations be like thoſe, by which many plants have their nouriſhment conveyed to them, or ſuch as are obvious in divers canes, which, being cut quite through tranſverſly, diſcover a multitude of diſtinct pores, that, by ſome experiments one may be induced to gueſs, reach all along, and make the cane like a cylindrical bundle of minute pipes; or rather a multitude of ſmall cavities, that perforate from end to end the parenchyma, or ſubſtance analogous to it, that gives them ſtability. And for the preſent this ſort of veſſels ſeem to me the more likely to be thoſe, that convey the blood to the extreme parts of the hair; becauſe even in horſe hairs, which yet are nouriſhed and grow, I am not yet ſure, that I have diſcovered with my microſcopes any cavity, and therefore ſuſpect there may be divers imperceptible ones, for the hair is nouriſhed and grows, which it is not like it ſhould do, if the body were ſolid; and if there were but a ſingle cavity in it, as in the lower part of a quill, it is like the microſcope I uſed would have diſcovered it, ſince with one much inferior I could eaſily ſee, that ſeveral little ſhort hairs, that grow upon the animal, that yeilds muſk, had each of them a cavity in it like that of the lower part of a quill.

To the things, that have already been ſaid about the poroſity of bones, I ſhall now add an obſervation of a very learned phyſician, that is very remakable to our preſent purpoſe, becauſe it argues, that even bodies not ſaline, nor actually moiſt, may from without get into the pores and cavities of human bones. Divers phyſicians have complained of the miſchiefs done to the bones by mercury employed to ſalivate in venereal diſeaſes: whereof I remember I have read a very notable inſtance, in a learned book (which I have not now by me) of an eminent Roman profeſſor of phyſic, who had the opportunity of making ſeveral curious obſervations in the famous hoſpital of the *incurabili* at *Rome*, and is therefore the more to be credited; where he relates, that in the cavity of at leaſt one pocky man's bones, there was found real quickſilver, that had penetrated thither. And this brings into my mind a memorable obſervation of an antient and experienced phyſician, who being famous for the cure of venereal diſeaſes, was aſked by me, what inſtances he had found of the penetration of quickſilver, either outwardly or inwardly adminiſtred, into the bones of men? To this he anſwered, that he could not ſay he had himſelf taken notice of any quickſilver in the cavities of greater bones; but that ſome other practitioners had told him, that they had met with ſuch inſtances, as I inquired after. But for himſelf, he only remembered, that a patient, who had been terribly fluxed with mercurial inunctions, coming afterwards to have one of the grinders of his lower jaw pulled out, becauſe of the raging pain it had put him to; my relator had the curioſity to view narrowly this great tooth, and found, to his wonder, a little drop of true mercury in that ſlender cavity of the root, that admits the ſmall veſſels, which convey nouriſhment and ſenſe to the tooth, in more than one of whoſe three roots he affirmed to me, that he found true, though but exceeding little, quickſilver. But a full teſtimony, to my preſent purpoſe, is afforded me by the experienced phyſician *Euſtachius Rudius*, who relates, that he ſaw himſelf, and that others alſo obſerved, ſome bodies diſſected, of thoſe, that had been anointed for the venereal pox, in the cavities of whoſe bones no ſmall quantity of quickſilver was got together, which, yet, (to add that upon the by) he ſays, did not hinder ſome of them from living many years, ſurviving thoſe inunctions.

Euſtach Rudius (apud Schenckium) lib. 5. de morbis acutis cap. 15.

C H A P.

CHAP. VIII.

I AM not ignorant, that among the particulars laid together in the foregoing eſſay, there are ſome, that are not abſolutely neceſſary, to prove the porouſneſs of the bodies of animals. But I thought it not impertinent to mention them, becauſe I hoped, that they, in conjunction with the reſt, may be of ſome uſe to naturaliſts, in giving an account of ſeveral things, that paſs in a human body, whether ſound or ſick, eſpecially if it be of a topical diſeaſe, and may remove, or much leſſen that great prejudice, that makes many (and ſome of them otherwiſe learned) phyſicians deſpiſe the uſe of all amulets pericarpia, and other external medicines in diſtempers of the inward parts, upon a confident, but not well grounded ſuppoſition, that theſe remedies immediately touching but the outſide of the ſkin, cannot exerciſe any conſiderable operations upon the internal parts of the body.

BUT though I have thus acknowledged ſome paſſages of the foregoing eſſay to be ſupernumerary, yet, I muſt not diſmiſs it without intimating, that I might from one topic more have fetched a probable, though not a demonſtrative argument, in favour of the porouſneſs of animals. For this may be very probably argued from hence, that even inanimate, ſolid, and ponderous bodies, that in all likelihood muſt be of a far cloſer texture, than the living bodies of animals (whoſe various functions require a greater number and diverſity of pores in their differing organs) are not devoid of pores, and have ſome of them very numerous ones, as will be ſufficiently made out in the following eſſay, to which I ſhall therefore haſten.

N. B. The following paper is that, which is referred to in the 767th page of this eſſay.

Pharmacopœiæ Regiæ claſſis xiii. pa. 614, 615.

Hujus rei veritatem comprobat doctiſſimus ac celeberrimus medicus & philoſophus D. Johannes Chryſoſtomus *Irmbler, ſtatuum Moraviæ marchionatûs protomedicus, bis verbis ad me ſcribens: Et revera paravi ego, anno* MDCLV. *quo tempore inter infectos verſabar quotidie, trochiſcos bufonios, eóſque, ut cætera Helmontii, indefeſſi veritatis indagandæ, & ex puteo opinionum veterum noſtram credulitatem excæcantium eruendæ, nati philoſophi, experimenta ſuas laudes ſuſtinere comperi: inter viginti autem bufones vix unum quidem, jucundo ſane ſpectaculo, vidi vermiculos, per nares & oculos egreſſuros, manu repellere, quamdiu poterat, donec elanguerit bufo: ſed trochiſcos ex vermiculis unà cum pulvere emortui buſonis, & materiâ per anum (nondum vidi per vomitum;) ſcilicet alis, pedibus, capitibus, ventribus ſcarabæorum viridibus, auratiſve & nigris, quos bufo cum terra in eſcam venatur, ejectâ, cerea patinâ exceptis, cum tragacantho roſato formatos, pluribus perſonis ſuper authraces opponi feci, atque nullum eorum mortuum eſſe dicere poſſum, ſed & meorum domeſticorum, ut & aliorum, quibus dedi, amicorum nullus, quod ſcio, infectus eſt. Sic comperi non tantùm hiſce trochiſcis enervari virus peſtilens in carbunculo jam admiſſum, ut dein vulgaribus chirurgicis remediis ulcus facili negotio fuerit curatum, ſed etiam ad ſiniſtram mammam ligatos, mihi meiſque accurſui & occurſui infectorum expoſitis, animoſitatem quandam indicibilem conferre, atque ita miaſmata & effluvia peſtilentialia abarcere. Hucuſque excel. medicus Moraviæ.*

An ESSAY of the POROUSNESS of SOLID BODIES.

CHAP. I.

AS it will, with far leſs difficulty be allowed, that animals and vegetables, and ſuch bodies as have belonged to either, abound with pores, than that inanimate, ſolid, and even ponderous bodies are not deſtitute of them; ſo it is far leſs difficult to make out the former, than the latter of theſe propoſitions: and therefore *Pyrophilus*, I hope you will not expect, that I ſhould give you as many proofs of the one, as I have of the other: however I deſpair not, that thoſe I ſhall preſent you will appear ſufficient for my purpoſe, though they be not numerous enough to make me careful to marſhal them in any exact order.

OF the reaſons, that induce me to think, that even ſolid bodies are not deſtitute of pores, there are ſome, that have a greater affinity with thoſe arguments, that the ſchools are wont to call *à priori*, becauſe they require more unobvious ratiocinations upon phyſical principles; and others, which reſemble, and indeed are, ſuch proofs, as are uſually named *à poſteriori*, being ſuggeſted by the phænomena afforded us by experience, without the help of any difficult ratiocinations.

OF the firſt ſort of reaſons I ſhall propoſe to you three; and begin with that, which may be drawn from the origin and formation of divers hard bodies. For I have elſewhere endeavoured, and I hope not unſucceſsfully, to ſhew, both that divers ſtones, and even gems themſelves, and that ſeveral metalline, and other mineral bodies, were once either viſible liquors, or, at leaſt, very ſoft ſubſtances. And I have elſewhere proved, that both theſe kinds of bodies do conſiſt of, (which is the caſe of liquors) or abound in (which is the caſe of ſoft and moiſt bodies) minute particles of determinate ſizes and ſhapes: from whence, I think, one may very probably conclude, that ſuch gems and other mineral bodies, notwithſtanding any hardneſs they afterwards come to acquire, are not deſtitute of pores; ſince it is no way likely, that corpuſcles of various and very irregular figures, ſuch as thoſe of moſt liquors of the terreſtrial globe are wont to be, can be ſo brought together, eſpecially by chance, cold, or any other ſuch agents, as not to intercept little intervals or pores between them.

See the tract of the origine and virtues of gems, and the notes about the mechanical production of hardneſs.

CHAP. II.

ANOTHER thing, which makes me think the poroſity of the moſt part even of ſolid bodies to be great, is the conſideration of the great diſparity, that may be found in the ſpecific gravities of ſuch bodies, as the eye does not perceive to be porous: for, though water be a body of that kind, and though its parts be ſo cloſe packed together, that the attempts of ingenious men to make a manifeſt compreſſion of that liquor, by outward violence, have not hitherto proved ſucceſsful; yet we find, that ſtones, clays, metals, and even ſome woods, and a multitude of other kinds of ſolids, will readily ſink in water, and by conſequence are ſpecifically heavier than it; which greater gravity ſeems not any way explicable, without ſuppoſing, or at leaſt ſo well as by ſuppoſing, that the corpuſcles, whereof ſuch ſinking bodies conſiſt, do either lye cloſer together, or are ſeparately more ſolid, than thoſe of water; which liquor muſt conſequently be porous, though neither the eye, nor the great force, that has been ſeveral ways employed to compreſs it, can diſcover any pores in it. Upon the ſame ground I further conclude, that ſolid ſtones themſelves, as marble, flints, &c. are not free from poroſity: for whereas, as far as ſeveral trials, purpoſely made, can inform me, I have found, that ſuch of theſe as have nothing metalline in them, do ſeldom or never reach to treble the weight of an equal bulk of water, they will, upon the former grounds, appear to be conſiderably porous; ſince the lighteſt metals, which are tin and iron, are above twice heavier in ſpecie, that is, the bulks being equal, than marble, flint, chryſtal, *&c.* and, by the ſame reaſon, I alſo infer the great poroſity, even of the ſolid body of iron, which is as well heavier, as very much harder than tin. For though copper be a good deal more ponderous than iron or ſteel, yet I have divers times found fine gold to be more than twice as heavy in ſpecie as copper; ſince, whereas this metal, whether it be European, or brought from *Japan*, (for of that alſo I made trial) is about nine times as heavy as ſo much water; I found refined gold to be about nineteen times as heavy as water equal to it in bulk; by which it ſeems highly probable, that ſo ſolid and heavy a body, as iron or ſteel itſelf, may be ſo porous, that metalline matter, equal to it in weight, may naturally be contained in much leſs than half the dimenſions, that metal poſſeſſes. And that gold itſelf, which is the moſt compact and ſolid body we know of, is not deſtitute of pores, may appear by the diſſolution of it in quick-ſilver, of which I ſhall ſpeak a little below. And if any ſhould pretend, that hardneſs may be a greater argument of the compactneſs of a body, and its immunity from pores, than its ſpecifick weight can be; I ſhall add, that though I have found, that emery, which is the body employed to cut ſteel, and load-ſtones, and cryſtal, and the moſt of gems, being indeed much harder than marble or flints, be far heavier than thrice its bulk of water; yet that ponderouſneſs proceeds, as I elſewhere intimated, from the mixture of a metalline ſubſtance, which I have ſeparated from it. And diamonds, though much harder bodies than emery, and indeed the hardeſt we know of in nature, are ſo far from being, as ſome of late have written, the moſt ponderous of bodies, that having examined them hydroſtatically, by a way elſewhere mentioned, I found them not much heavier than either cryſtal, or fine glaſs, and not half ſo heavy as the lighteſt metals.

CHAP. III.

THE next thing, from which the porouſneſs of ſolid bodies, and even thoſe, that belong to the mineral kingdom, (as the chymiſts ſpeak) may be deduced, is the ſame with the firſt of thoſe, from which we formerly argued the poroſity of ſubſtances belonging to the animal kingdom; namely, the very frame and conſtitution of ſuch bodies: for the ſolideſt bodies themſelves reſulting from the convention or coalition of a great number of particles of ſeveral bigneſſes and ſhapes, we cannot reaſonably ſuppoſe, (eſpecially in thoſe concretes, wherein they are not ranged by a ſeminal principle) that they ſhould be contexed ſo, as to touch one another exactly every where; and therefore they muſt, of neceſſity, leave ſome little intervals and pores between them.

THIS reaſon will, I hope, appear clear enough of itſelf to him, that ſhall attentively conſider it; eſpecially if we know, that it has been geometrically demonſtrated, that there are but very few figures, that will, (as they ſpeak) *implere ſpatium*, that is, which being adjuſted to one another will ſo exactly touch, that there is not the leaſt unfilled ſpace within the circumference or circuit, if the figures be plain, or within the ambient ſuperficies, if they be ſolid; ſo that, conſidering the vaſt variety of other figures, which made *Epicurus*, and other anatomiſts, pronounce it incomprehenſible, it is very obvious to conceive, that corpuſcles of ſuch differing ſhapes being put together, will leave multitudes of little pores intercepted between thoſe parts, that do not every where touch one another. And even the mathematical figures, lately ſpoken of, may be ſaid to fill ſpace rather in a geometrical, than a phyſical ſenſe. For, if ſuch portions

tions of matter, as are required to conſtitute, for inſtance, a cube, were actually put together, they would not exactly fill the ſpace comprehended within the ambient ſurface of the body they compoſe; becauſe the component bodies, being phyſical, conſiſt of corpuſcles of their own particular ſhapes, which we never find mathematically exquiſite. As if, for example, the cube were of marble, no art could poliſh the ſides of a component body ſo, as that they ſhould be perfectly ſmoothed; ſince (as, if I miſtake not, the learned *Gaſſendus* well obſerves) emery, pumice-ſtone, and even puttee, or other powders, that are employed to poliſh, do themſelves conſiſt of little hard angular corpuſcles, that leave ſmall ſcratches, like ſo many little furrows, on their ſurfaces, which muſt needs hinder the perfect contact of the whole ſurfaces of two contiguous bodies, and conſequently leave here and there intervals or pores between thoſe ſurfaces: to which I ſhall add, that marble itſelf, as it is marble, abounds with internal pores, as will e'er long appear by experience, and as may be rationally conjectured from the ſpecifick levity of it, in compariſon of gold and lead.

CHAP. IV.

HAVING diſpatched the arguments *à priori*, that may be employed to ſhew the porouſneſs of ſolid bodies, it will be now ſeaſonable to propoſe ſome experiments and obſervations, that may (as it were) *à poſteriori*, either evince or confirm the ſame doctrine.

OF theſe inſtances ſome relate to ſolid bodies, that are of leſs ſpecifick gravity; ſome to foſſiles, preſumed to be devoid of metalline parts; ſome to minerals, that are thought to participate of a metalline nature; ſome to metals themſelves, and ſome to glaſs.

To begin with the firſt ſort of theſe inſtances; that wood is porous, there are many things, that argue, ſome of which are elſewhere mentioned: but few would ſuſpect, that quickſilver, which is ſo unapt to enter the pores of bodies much leſs compact, ſhould permeate pieces of wood of a conſiderable thickneſs; and yet, that we have made it do by the following experiment. We took a wooden trunk, ſuch as is employed to ſhoot pellets at birds with ſtrength enough to kill them, and having cloſely ſtopped one end of it, we poured in quickſilver at the other, till it reached to a good height in the cylindrical cavity of the inſtrument; and then the lower parts of the metalline liquor being aſſiſted by the weight of the incumbent ones, (not to mention that of the air) to preſs into the pores of the wood, they iſſued out at them on all ſides, in great numbers of minute drops, much after the manner of quickſilver ſtrained through leather out of amalgams; which was a phænomenon not unpleaſant to behold. But till I have opportunity to repeat this experiment with differing circumſtances, I ſhall not think it fit to lay much ſtreſs upon it, for want of knowing, what intereſt the great weight of the quickſilver may have had in the event.

AND this caution may perchance be applicable to the following experiment; namely, that having, by the help of my pneumatical engine, withdrawn the air from one ſide of a round piece of board, the air, on the oppoſite ſide, not having its preſſure any longer reſiſted by that, which it uſed to meet with from the withdrawn air, preſſed ſo ſtrongly againſt the ſurface of the wood expoſed to it, as to make itſelf way through the pores of it, and get copiouſly enough into the cavity, whence the other air had been pumped out; (the weight of the incumbent atmoſphere doing, on this occaſion, what the weight of the quickſilver did on that laſt recited:) which was a ſurpriſing ſpectacle to the by-ſtanders, becauſe the board, that was thus permeated, was of ſtrong wood, and of conſiderable thickneſs.

I SHOULD here ſubjoin ſeveral other arguments of the porouſneſs of wood, if I could diſplay them without more words, than I am willing to allow them; and, I preſume, it may here ſuffice, if I let you ſee, by ſome ſurpriſing effects, that when wood is reduced to that thinneſs, that its cloſeneſs or poroſity may conveniently be examined, it will eaſily enough give paſſage, even unto viſible, odorable, and tinging corpuſcles, though they invade it not in the form of a liquor, but of dry exhalations, ſo they be not incommenſurate to its pores. This, I ſuppoſe, you will not ſcruple to infer from the following trials, as they were long ſince ſet down in one of my note books.

1. THE fumes of our ſmoaking liquor [deſcribed in the foregoing eſſay] tinged a copper halfpenny through a broad thin ſhaving of deal, that did not, when held againſt the window, diſcover any perforation; tinged it, I ſay, very deeply in about a quarter of a minute and ſomewhat leſs.

2. THE ſame fumes tinged manifeſtly, but not ſo notably, the ſame halfpenny, firſt cleanſed, through two ſuch ſhavings of deal laid one upon another, in ſomewhat leſs than one minute.

3. AND, in about one minute, the ſame fumes tinged the cleanſed halfpenny, through three ſuch ſhavings of deal very viſibly, but not ſo conſpicuouſly, as through the two forementioned. Theſe trials were made without the help of heat to promote the operation of the fumes.

CHAP. V.

FROM the conſideration of woods, let us now proceed to give ſome inſtances of the porouſneſs of bodies made of cloſe and compacted, and, perhaps, well-baked clays, or other earths.

THAT earthen veſſels, though ſtrong and well-baked, are many of them porous enough, may be argued not only from what has been lately recited, but from hence, that ſome of them will ſuffer themſelves to be ſoaked through by oil, others by ſolutions of nitre, and ſome other ſalts: and there are very few of them, without excepting Haſſian crucibles themſelves, that will long keep ſalt of tartar, and ſuch like fixed alcalies, in fuſion, without being penetrated by them. I have heard diſtillers complain, that when they have diſtilled corroſive materials, as vitriol and ſalt-petre, with ſtrong fires in thoſe earthen veſſels, that are commonly made uſe of in *London*, (eſpecially by refiners) inſtead of retorts, though their necks be ſtraight and long, (upon which account they are called long-necks) a conſiderable quantity of the fineſt ſpirits make their eſcape quite thorough the veſſel; ſo that in the retort and receiver many ounces are found wanting of the firſt weight of the matter to be diſtilled; and this ſometimes, when the vitriol has been previouſly calcined, and a reaſonable allowance has been made for what may have eſcaped thorough the lute, that joined together the long neck and receiver. And though I have obſerved of our bottles, made of the ſame earth with juggs, that they are hard enough to ſtrike fire with good ſteel; yet a good experimenter upon ſuch veſſels, of whom I made inquiry, has aſſured me, that theſe, as compact as they are, may, even without external heat, have their pores pervaded by the finer parts of ſpirituous liquors.

To this purpoſe I remember, that meeting once with a virtuoſi, that was curious about the ways of making cyder as briſk and ſpirituous a liquor as he could; I inquired of him, whether he was able to keep in the ſubtile ſpirit of this ſkilfully fermented liquor in thoſe earthen bottles, that, by reaſon of the ſolidity they acquire by the vehement coction of the fire, are commonly called ſtone bottles: to which he replied, that he he often found, to his trouble, that the liquor would permeate the compact substance of the bottles: and when I objected, that the spirits might either escape out at the cork, which I have made several spirits of divers kinds, that would readily permeate; he replied, that what he had said appeared by the outside of the bottles: to which, when I further objected, that the sight of dew on the surface of the bottles would not convince me, without tasting, whether it were vinous, because I had divers times observed, that brisk liquors would produce a dew on the outside of the vessels, that contained them, not by any transudation, (for I have made trial of it in glasses) but by condensing the aqueous vapours dispersed through the neighbouring part of the ambient air; he replied, that, besides what his taste had informed him of the quality of this dew, he found, that the included liquor, though exactly stopped, wasted in not very many months so considerably, as sometimes to lose a sixth, or even a fifth part; and this escape, or percolation, of the liquor through the substance of the vessels, he affirmed himself to have observed not only in one or two bottles, but in very many; and the like observation, for the main, was confirmed to me, upon his own experience, by an eminent physician, who, being a great lover of brisk cyder, used to bottle it up early and carefully.

Though good Hassian crucibles be very closely compacted, as well as thoroughly baked bodies, and, upon that account, are able to keep silver and divers other metals long in fusion, without letting them at all run out; yet having dissolved silver in aqua fortis, I observed, that though the salts were, by this operation, so changed, that this horn-like silver did dissolve neither in aqua fortis, nor in the aqua regia, that I put it into; yet when I kept it a while in fusion, (which it is easily brought to be) among quick coals, it would, without cracking or perforating the crucible, soak into it, and permeate the pores of it in I know not how many places, as I convinced some curious persons, by shewing them, on the outside of the vessel, a multitude of minute globules of pure silver, like so many little drops, that were got thither, as it were, by transudation.

CHAP. VI.

From baked earths, that are designed in point of hardness to emulate stones, we will proceed to give some instances of the porousness of natural stones themselves.

There goes a tradition, that in some part of the *West Indies* they have a stone, of which they make large vessels, wherein they put water to percolate, as it were, through a strainer. Of these vessels I had one sent me for a present, whereof being hereafter to give some account in a more opportune place, I shall now only take notice, that I found, that water would (though slowly) soak through the vessel, though it were considerably thick.

If, as many of the ancients and most of the modern corpuscular philosophers have conceived, the transparency and opacity of bodies proceeds from a rectitude or crookedness of pores, which makes them fit or unfit to transmit the light, that tends to pervade them in physically straight lines: if this hypothesis, I say, be allowed, we may draw a very probable argument, that stones may be porous from the phænomena of that odd gem, that is best known by the name of *oculus mundi*. For this small stone (at least that, which I made my observations of) when it is dry, and is kept in the air, is opacous, almost like a polished piece of white amber, and so it continues, as long as it is kept dry; but if you put it into fair water, it will in no long time become, by degrees, quite transparent; and that, which I made trial of, looked then not unlike a piece of clear yellow amber, which, by degrees, does in the free air lose its transparency,

rency, and turn to be opacous as before. Now, according to the above-mentioned corpuscular hypothesis, the pellucidness, which the stone acquires in water, may be accounted for, by saying, that the liquor, getting in at the crooked pores of the stone, does for the time rectify them, and make them pervious to the straight beams of light; as we see, that white paper, being wetted with water, or, which does far better, being made to imbibe oil, has its pores so changed and rectified, that the water much lessens its opacity, and makes it almost semi-diaphanous, and the oil, if it be fine and well soaked up, makes it transparent: but upon the recess or evaporation of the imbibed particles of water, the pores of the little stone, becoming crooked again, reflect the rays of light they should transmit. Which explication will be the better allowed of, if my memory do not misinform me, when it tells me, that a learned member of the Royal Society found the *oculus mundi* to weigh more in a nice ballance, when it was taken out of the water and well wiped, than before it was put in. This stone, which very few of the writers about gems take notice of, is so rare and difficult to be got, that I had not opportunity to make upon it all the trials I desired; and therefore, though the subject be curious, I may, I hope, be excused, if I hasten from it to another.

There is so much difference in many qualities betwixt stones and metals, that it is very probable, that when the corpuscles of both come to be brought together into one mass, they will not touch one another so close, as not to leave store of little intervals or pores between them. And upon this ground I have been apt to think, that divers very hard stones, diaphanous and opacous, are not devoid of porosity. For I have elsewhere delivered a way, by which I have obtained good store of metalline parts, both from American granats, and from emery; though this last be so exceeding hard a stone, that it is usually employed by artificers, to work upon iron and steel, and to cut not only rock crystal, but divers gems, that are harder than either that or steel.

Upon the same ground one may probably infer the porosity of many artificial gems made by fusion; for, to give these the colour of sapphirs, topazes, amethysts, &c. we are wont to add to the vitrifiable matter either some prepared metal, as calcined copper, calx of gold, &c. or else some mineral, as *Zaffora* and *Manganeze* (as the glass-men call *Magnesia*) that abounds in metalline parts. Nay, I remember, I have sometimes given the colour to the vitrified substance, by employing natural gems, as granats; though to shew, that the coloration, which the mass received from these, proceeded from the metalline corpuscles, that lay hid in the tinging matter, the colour produced was not that, which was conspicuous in the gem itself, but one very different from it, and such as the metal, which, upon other accounts, I supposed the gem to partake of, ought, according to the grounds I proceeded upon, to produce in the vitrifiable matter. And this very experiment makes it also highly probable, that even natural transparent gems, (divers of which are much harder than marble, iron, and even steel) are themselves porous; since, notwithstanding their transparency, and seeming homogeneity, they are made up of ingredients of such differing natures, as are stony and metalline corpuscles.

From the same ground we may likewise deduce the porosity of marcasites; many of which I have observed to be not only hard enough plentifully to strike fire, by collision with steel, but more ponderous, than even divers ores, that were rich enough in metal to be wrought with good profit: and yet these hard and heavy (mineral) stones are very far from being homogeneous; since I have met with few inanimate bodies, produced by nature herself, so compounded, as several marcasites, that I have seen: for these are wont to contain more or less copper, and iron too; and they abound in combustible sulphur, a corrosive salt, and a certain fixed substance, which I found to

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differ from true earth, but of whoſe nature the trials I have hitherto made on it have but little ſatisfied me.

I MIGHT here deduce the poroſity of the load ſtone, as hard and ſolid a body as it is, partly from the effluvia it emits and admits, and partly from the heterogeneity I have, by chymical trials, found to be in it: but theſe things belong more properly to a paper about magnetical bodies, for which I the more willingly reſerve them, becauſe other experiments will keep them from being needful to be here inſiſted on.

THE poroſity of marble and divers other ſtones of like contexture may with probability be deduced from this, that they are liable to be diſſolved by divers of the corroſive menſtruums of the chymiſts, ſuch as aqua fortis, ſpirit of ſalt, &c. and ſome of them even by vegetable liquors of nature's own preparing, as the juice of lemons, and that of barberries. But a more noble and ſatisfactory inſtance may be afforded, by the invention of ſtaining or colouring white marble, without employing any fretting liquor, or ſpoiling the texture of it. This way being caſually lighted on by an ingenious ſtone-cutter in *Oxford*, who gained by it both credit and money, he long ſince thought fit to acquaint me with it, upon condition of ſecreſy, (which I have to this day inviolably kept) and of my aſſiſting him to improve his invention, by making it practicable with other colours than red. Theſe circumſtances I mention, to ſignify, that I write not by gueſs of this matter, having both ſeen the experiment tried, and made it myſelf. But though I found it far leſs improvable to other uſes, than one would expect, yet, as to our preſent purpoſe, it is very appoſite; for by this way an excellent red colour may be made to ſoak into a piece of white marble, almoſt as oil will do into leather, without impairing, that I obſerved, the ſolidity of the ſtone, which, after being dyed, will be capable of a fine gloſs as before. Some other colours (yet but few fair ones) would, by this way, be brought to ſoak into marble, on which one may with them ſo define and limit the colorations, that I remember the artificer, when I brought him to kiſs the king's hand, preſented his majeſty with an andromeda, whoſe colours were ſo vivid, that this ſkilful judge of curious things, was pleaſed to honour it with a place among his rarities. And to ſatisfy his majeſty, that the fine red was not, as ſome ſuſpected, a mere varniſh, I purpoſely broke a plate of marble, in whoſe fragments he ſaw, that the pigment had ſunk to a conſiderable depth into the very ſubſtance of the ſtone; and I doubt not but it might have been made eaſily enough to ſink much deeper, if it had been thought neceſſary. A fine plate of ſuch white marble, with the penetrating pictures of coloured flowers drawn upon it, I yet keep by me to ſatisfy the curious; and ſome utenſils, as hafts of knives, ſalt-ſellers, &c. I have known to have laſted ſeveral years.

THERE is an experiment, that ſeems much ſtronger for the porouſneſs of ſolid bodies, than that itſelf (which was lately recited) of ſtaining marble: for in *Italy* ſome goldſmiths have a way of imbuing fragments of rock-cryſtal, which is a body much harder than marble, with divers colours; which do ſometimes ſo embelliſh them, that having ground off thoſe parts, that would not receive the ſame tincture, they ſet ſome of them in gold rings, and ſell them with profit. When I was informed of this, I thought of a compoſition, that I hoped might perform the ſame thing, and perhaps better than that, which was employed by them, who either knew not, or, for aught I could perceive, uſed not, ſome minerals, that I thought fit for the purpoſe. Upon this preſumption we carefully cemented ſome clear fragments of native cryſtal with a compoſition of ſome volatile minerals, together with a ſalt or two; and having ſuffered the crucibles to cool leiſurely, we had divers of the fragments ſtained here and there, ſome with one colour and ſome with another, as differing fumes happened to invade them; and of theſe colours ſome were dark or dull, and ſome vivid enough. But having

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considered the ſtained pieces, and the progreſs of the operation, more attentively, I began to doubt, whether theſe adventitious colours were really produced by the bare penetrating of the mineral fumes into the pores of the cryſtal itſelf: for I thought it poſſible, and not very unprobable, that the great heat of the fire and the ambient mixture might cleave or flaw, in many places, ſome of the cryſtalline fragments; and that the finer parts of the minerals, being vehemently agitated, might inſinuate themſelves into theſe thin flaws, which, upon the ſlow refrigeration of the ſtones, ſhutting themſelves cloſe again, might lock up the tinging particles, without appearing diſcontinued, eſpecially to the eyes of perſons, that were not made uſe of with a more than ordinary attention excited by diſtruſt. This ſuſpicion was not removed by the apparent entireneſs of each little piece of cryſtal; for having taken more than once a lump of that ſtone, and ſlowly brought it to be red hot in the fire, I found, that if I warily quenched it in water, though it would thereby acquire a multitude of little cracks or flaws, which deſtroyed its former tranſparency and make it look whitiſh, yet it continued ſtill an entire body, notwithſtanding the diſadvantageous haſte, wherewith the operation had been performed. And having, after this ſuſpicion, inquired of an ingenious lapidary, that belonged to a great prince, whether, in poliſhing of gems upon a wheel, he had taken notice, that the heat would flaw them; he anſwered me, that now and then he had obſerved, that ſome ſtones, eſpecially, if I miſremember not, rubies, when they were very much heated by the ſwift motion of the engine he employed to poliſh them, did cleave, as it ſeemed to him, and gape, ſo as at firſt to make him fear the ſtones were ſpoiled; and yet afterwards they cloſed ſo perfectly, that no flaw at all could be perceived in them. I have mentioned the foregoing experiment of tinging cryſtal, to comply with the dictates of philoſophical candor, which forbids me to lay much ſtreſs upon a proof, whoſe validity I myſelf diſtruſt. But perhaps my ſuſpicion may, by further trial, which I have not now conveniency to make, appear not to have been well grounded; and in that caſe the tinging of cryſtal, as well inwardly as outwardly, by fumes, will be a noble argument for the poroſity of ſolid bodies, rock-cryſtal being harder, and probably cloſer, not only than marble, but even than glaſs.

CHAP. VII.

THAT metals, though the heavieſt of bodies, are not deſtitute of pores, may be, with probability, proved in a general way by this; that they are all diſſoluble in their appropriate menſtruums, as gold in *aqua regia*, and all the reſt in *aqua fortis*, except tin, which yet itſelf will be corroded by that menſtruum, though not well kept up in a fluid form, as it may be by another menſtruum, which I elſewhere teach: and ſometimes the ſame metal may be diſſolved by very differing menſtruums, as lead by *aqua fortis* and ſpirit of vinegar; and copper by *aqua fortis*, *aqua regia*, ſpirit of vinegar, ſpirit of ſalt, and ſome other ſolvents, that, upon trial, I have found ſufficient for that purpoſe.

BUT it will, I preſume, be thought more conſiderable to my preſent argument, if it be ſhewn, that bodies, that appear groſs, and which in their natural ſtate are not fluid, and are conſeſſed to be of a compounded nature, will penetrate metals quite through, even without melting them.

THIS we have divers times effected, by a cementation of copper plates with common ſulphur (much a kin to a way preſcribed by ſome alchymiſts to make *vitriolum Veneris*) which we warily performed much after this manner. We took good copper, laminated to the thickneſs of a ſhilling or thereabouts, and having cut it into ſmall pieces, that they might the more eaſily be put into a crucible or cementing pot, we ſtrewed at the

bottom

bottom of the veſſel ſome bea en ſulphur, and then covered it pretty well with ſome of theſe plates, which were laid on flat-wiſe. Upon theſe we ſtrewed another bed of powdered brimſtone, and covered that alſo with plates, upon which we put more ſulphur; and ſo continued making one lair of brimſtone, and another of metal, till we had employed all our plates, or filled the crucible, being careful, that the uppermoſt bed, as well as the loweſt, ſhould be of ſulphur. This done, we luted on an earthen cover to the veſſel, to keep the ſulphur from taking fire; and afterwards having placed the pot amongſt coals kindled at a good diſtance from it, that it might be heated by degrees, we kept it for ſome few hours (perhaps two or three) in ſuch a degree of fire, as was ſufficient to keep the ſulphur melted (which is eaſily enough done) without bringing the metal to fuſion; the pot being cold. we took off the cover, and found the plates quite altered in colour and texture, ſome of them having a dark and dirty colour, whilſt others looked of a fine violet or blue: they were generally ſo brittle, that it was very eaſy to break them with ones finger, and reduce them to powder. And (now to add ſuch circumſtances as a chymiſt would not take notice of) many of the plates, when they were broken, appeared to have been (by the contiguous beds of ſulphur above and below) horizontally divided each of them into two plates, divers of which in ſome places had a manifeſt diſtance or cavity between them: and it was obſervable, that when I conſidered one or other of theſe plates attentively in the parts, that had been contiguous before I broke it, I could plainly diſcern a multitude, as it were, of fibres, reaching from one of the flat ſides of the plate to the other, and running many of them, as to ſenſe, parallel to one another. Theſe circumſtances may ſufficiently argue, that the plates were pierced quite through by the brimſtone: but for confirmation of this, and to ſhew too, that the ſulphur does, as it were, ſoak into the body of the metal, and in a groſs manner lodge itſelf there; I ſhall add, that not only to the eye the plates appeared much ſwelled, or thicker than when they were put in, but having weighed them before the operation was begun, and after it was quite ended, the copper, though it needed not to be freed from externally adhering ſulphur, was found to have a conſiderable increaſe of weight by the acceſſion of the ſulphur, which (to add that circumſtance) though it appeared not to the eye, yet if a plate were laid upon quick coals, and blown, would oftentimes diſcover itſelf by a blue flame.

By making the like experiment for the main, we found, that refined ſilver, though a more heavy and compact body than copper, and not diſſoluble by moſt of the menſtruums, that work on this metal, is penetrable by the body of ſulphur, which will alſo calcine tin and lead, and (eſpecially) iron.

Nor is ſulphur the only conſiſtent body, that has this ingreſs into metals; for we have found them penetrable by prepared arſenick. But becauſe theſe operations are not ſo eaſy, and the ſubject is not eaſily handled without danger, I forbear the mention of them in this place, where, after what has been recited, it is not neceſſary.

Another experiment there is, which does more advantageouſly, than that made with brimſtone, diſcover the poroſity of copper. For there is a way, by which, without the help of ſalts, ſulphur or arſenick, one may make a ſolid and heavy body ſoak into the pores of that metal, and give it a durable colour, I ſhall not mention the way, becauſe of the bad uſe, that may be made of it. But having had the curioſity more than once to try it upon a new copaer farthing, the event was, that one part of it, which I purpoſely forbore to tinge, remained common copper ſtill; but the other part acquired a yellow, that differed very little, if at all, from a golden colour, the former ſtamp, that was impreſſed upon the coin, continuing viſible. And to convince the ſcrupulous, that the pigment did really ſink, and, as it were, ſoak into the body of the metal, and did not meerly colour the ſuperficies, I made them take notice, that the farthing was not melted, and yet, by filing off a wide gap from the edge of the coin inwards, it plainly appeared, that the yellow or golden colour had penetrated a pretty way beneath the ſurrace of the farthing; ſo that it looked there, as if two thin plates, the one yellow, and the other reddiſh, did, without any interval between them, lie upon one another.

If bodies be not to be pervaded, or deeply pierced into, by corpuſcles, but only to have their more ſuperficial pores, if I may ſo call them penetrated thereby, it is poſſible, that bodies, which are either much harder, or much cloſer, than marble, alabaſter, or the like bodies, may have their pores poſſeſſed even by odorous corpuſcles; I ſay, even by ſuch, becauſe they are moſt of them groſs enough to be kept from exhaling by bodies much leſs compact than earthen bottles, and are far from being of the fineſt particles, that nature affords. But that ſuch odorous corpuſcles may lodge themſelves in the exterior pores of very cloſe bodies, I have been inclined to think, not only by the obſtinately adhering odour, which I found, by trial, that ſome ſubtile and ſpirituous parts, ſuch as the chymiſts would perhaps call, in their aggregates, the eſſence of muſk, amber, ambergreece, *&c.* notwithſtanding the waſhing of the glaſſes, that had long contained ſuch liquors; but by what has been aſſured me by a phyſician of great experience, who travelled and lived much in the eaſt. For having told him, that I had been informed, that in ſome places, leſs famous than *Damaſcus* for curioſity in making fine ſword-blades, there was a way found and practiſed of making them richly ſcented, without injury to their gloſs, I deſired to know of him, if at *Damaſcus*, or elſewhere, he had ſeen any of them: to which he replied, that he did not remember he had, but yet made no doubt the information might be true; for he himſelf had in *Europe*, and kept for divers years, a watch, whoſe metalline caſe was richly perfumed: and when I aſked him, whether there were not ſome thin varniſh, or ſome outward caſe of perfumed leather, or chagreen, or ſomewhat elſe, from whence the odour proceeded; he aſſured me, that his obſervations had prevented and removed that and divers other ſcruples, and that the caſe being clean and gloſſy, he could not perceive, that the odour proceeded from any thing elſe than ſome odoriferous thing or other, that was inviſibly lodged in the pores, or porous ſubſtance of the metal itſelf. And indeed, ſince both arſenick, and even common ſulphur, may by art be, as it were, incorporated with ſome metals, and even with ſilver, I ſee not why it ſhould be impoſſible, that ſome pleaſingly ſcented ſubſtances ſhould be admitted into the pores of metalline bodies, and be volatile enough to have their ſubtiler parts fly off in odorous exhalations, eſpecially if they be a little exeited, as the watch-caſe lately mentioned was, by a gentle heat, ſuch as was that of the wearer's pocket. And on this occaſion I remember to have made a certain metalline compoſition, which looked like gold, and of which I cauſed a ring to be caſt; and yet this metal retained ſo many unperceived mercurial corpuſcles in it, that an ingenious perſon, to whom I diſcovered the compoſition of it, found, after trial, as he aſſured me, that being worn as a ring, it had in ſome diſtempers, particularly of the eyes, manifeſt operations, that evidently enough ſeemed to flow, at leaſt in great part, from its participation of the mercury we employed in preparing the factitious metal.

Since the writing of the former part of this eſſay, having met with an inquiſitive nobleman, who had lived in ſeveral parts of *Africk*, and was governor of the beſt town the *Europeans* have on that continent, I diſcourſed with him, among other things, about the ſkill, that ſome aſcribe to the African Moors, of making excellent weapons, whereof I knew his excellency was very curious. Upon which occaſion he told me, that ſome of the off-ſpring of the Granadine Moors were indeed the beſt at making arms,

arms, that ever he met with; and that he had ſeen ſome weapons of their forging and tempering, that he preferred even to thoſe of *Damaſco*. And when I aſked him, whether any of them had the art of perfuming their weapons? he anſwered me, that ſome of them did it admirably well, and inſtanced in a blade, which he kept for ſome years, and found it ſtill to retain the perfumed ſcent, which he ſuppoſed to be, as it were, incorporated with the ſteel, whereof the blade was made. When I told him I ſuſpected, that the ſcabbard might have been well perfumed, and communicate its odour to the contained blade, he allowed the objection to be plauſible, but replied, that it was not concluding, ſince miſliking the ſcabbard, as not handſome and faſhionable enough, he cauſed a new one to be made, wherein he afterwards kept it. And the ſame lord further told me, that he had alſo a fowling piece, whoſe barrel was perfumed. And when I objected, that perhaps the odoriferous ſcent proceeded from the ſtock, and not from the metal; he anſwered, that the gun not being, when it came into his poſſeſſion, ſkilfully and handſomely mounted, he cauſed the barrel to be fitted with a new ſtock, notwithſtanding which, it continued to ſmell fragrantly. And when I further aſked, whether he ever cauſed the gun to be waſhed or ſcoured after it was grown foul by having been often ſhot in? he anſwered me, that he had, and, as far as I can remember, ſubjoined, that after it was made clean, it did (notwithſtanding the ill ſcent, that the ſoot of the powder had given it) retain a pleaſing ſmell, but fainter than before.

CHAP. VIII.

SINCE the ſubject of this eſſay is the porouſneſs of ſolid bodies, and ſince there is no body, that is generally reputed ſo cloſe and compact a glaſs, it will be pertinent to this diſcourſe, and probably will be expected, that I ſhould here ſay ſomething about the queſtion, whether glaſs be, or be not, devoid of pores.

BUT before I acquaint you with my trials, or my thoughts, about this problem, I think it requiſite to clear the ſenſe, in which I mean to handle it, that I may not, as ſome others have done, for want of diſtinctly ſtating the queſtion, ſpeak confuſedly and erroneouſly of it.

I ſhall then here obſerve, to prevent miſtakes, that the poroſity of glaſs may admit of two acceptions: for it may be ſaid to be quite pervious to fluids, as a boot is to water, or only to be capable of having its ſuperficial parts further and further diſſolved or corroded thereby, as a ſilver cup is porous in reference to *aqua-fortis*, which cannot ſweat through it, as water does through a boot, but eat its way through it, by diſſolving the texture of the veſſel.

ANOTHER thing requiſite to be premiſed, to prevent ambiguity, is, that glaſs itſelf is not all of one ſort, as men unacquainted with chymiſtry are wont to preſume; for glaſs of antimony, for inſtance, and that of lead, both of them made *per ſe*, do manifeſtly differ, uſually in colour, and conſtantly in weight, and alſo in their operations upon human bodies; and both theſe ſorts of glaſs do in ſeveral points differ from common glaſs, under which name, for brevity's ſake, I comprehend both white or cryſtalline glaſs, as it is called in the ſhops, and that courſer ſort, which they uſually call green glaſs; both which ſorts I here conſider under one notion, becauſe both are made of fixt alcalies, and other fit ingredients, as ſand, earth, aſhes, pebbles, or flints, colliquated by a ſtrong and laſting operation of the fire: and it is of this common glaſs, in the ſenſe now declared, that I ſhall conſider the poroſity in the remaining part of this essay; in which, to proceed with some method, I shall digest what I have to say, into the ensuing propositions, and the observations annext to them.

Prop. 1. *It is very probable, that glass may be pierced into to some distance, even by visible and tangible bodies.*

I know, that this will seem a paradox to many, and repugnant to common experience, which shews, that glass vessels will contain very subtile and even highly corrosive liquors, as the spirit of hartshorn, of urine, and that of nitre; as also those potent menstruums, as *aqua-fortis*, *aqua-regis*, and oil of vitriol, which not only are not observed to pierce into it, but are unable to make any sensible alteration, so much as on the superficial parts, even in those vials, wherein they have been long kept.

BUT, notwithstanding all this, I presume you will not condemn the lately proposed paradox, when you have considered what may be said to justify it. For besides that it may be made probable *à priori*, by the arguments, whence we have formerly proved the porousness of solid bodies in general; there are two sorts of experiments, from whence one may argue, that glass, in particular, is not devoid of pores in the sense, wherein we are now speaking of them.

AND first, I remember, that having kept for a good while in a vial a quantity of a certain spirit of salt, that I had reserved in a cool place, I found, when I came to use it, that the glass was crackt, and most of the liquor was run out; but, before this happened, it had so far corroded the inside of the glass, that in some places it was eaten almost as thin as a piece of paper; and this part, which yet continued glass, was lined with a much thicker white substance, that stuck to the sides of it, and looked and tasted like a kind of odd salt; so that it invited me to conjecture, that it proceeded from the substance of the glass, which, you know, consists of an alcali, as well as of sand corroded by the saline spirits of the menstruum, and coagulated with them into this odd kind of concrete; and it was remarkable in our vessel, that the upper part of the vial, to which the menstruum did not reach, was not corroded nor altered, though the operation of the liquor reached as high as the liquor itself. And I remember, that when I related all this to some experienced chymists, one of them, that was a more heedful observer, assured me the like had once or twice happened to him, as since that time it hath likewise done to me.

I had also, if I misremember not, another vial, corroded by a distilled liquor of vitriol, that had in it more of the phlegm than of the oil; which you will somewhat the less wonder at, if you consider, that some corrosive menstruums will scarce work on some bodies, if they be too well dephlegmed, or at least will not corrode them so readily and powerfully, if they are very strong, as when they are diluted with a convenient quantity of water. And, as to oil of vitriol itself, which is the menstruum I am speaking of, when we employ it to make *vitriolum Martis*, we are wont to weaken it with water, that it may the better dissolve that metal. And, perhaps, you will suspect, that vitriol has some peculiar faculty of penetrating and fretting glass, when to the experiment newly recited I shall add that, which follows, as I find it registred among my notes.

[A pound of Dantzick vitriol and a pound of sea salt, after the former had been very lightly calcined, and the latter decrepitated, that they might not boil in, or crack the vessel, we caused to be distilled in a well coated retort by degrees of fire, giving at length a very strong one; then taking off the vessel, we were not much surprized to find, that the heat had here and there melted it, and that the fluxed *caput mortuum* had corroded the glass, fetching off, as it were films from it; and those parts, which did not appear to the eye thus manifestly wasted, seemed yet, by their great brittleness, to have

have been penetrated, ſo that their texture was ſpoiled by the ſaline and vitriolate corpuſcles.]

Prop. 2. *Common glaſs is not ordinarily permeable by chymical liquors, though ſtrong and ſubtile, nor by the directly viſible or odorable expirations of bodies, though, abſolutely ſpeaking, it be pervious to ſome corporeal ſubſtances.*

THIS propoſition conſiſting of two parts, we ſhall allow each of them its diſtinct proofs.

AND as for the firſt part, it is manifeſtly agreeable to the common experience of chymiſts, who daily find, that in well ſtopt vials, or at leaſt in hermetically ſealed glaſſes, they can preſerve their ſubtileſt and moſt piercing menſtruums, as ſpirit of nitre, *aqua-fortis*, ſpirit of ſalt, ſpirit of vinegar, and oil of vitriol. And this they found to be true, not only as acid and corroſive liquors, like thoſe I have newly named, but alſo in thoſe ſpirits, that abound with fugitive ſalts, as the ſpirit of urine, of blood, and of ſal-armoniac; and in the moſt ſubtile and highly rectified ſpirit of wine; as alſo in the ethereal oil, or, as many call it, ſpirit, of turpentine; as likewiſe in the liquors of ſalt of tartar, and other fixt alcalies reſolved by deliquium.

THE reſult of theſe obſervations may be much confirmed by conſidering, how often it happens in the diſtillation of more wild and fugitive ſpirits, as of nitre, tartar, and ſugar, that, though they are much agitated, and perhaps ſubtilized, by heat, yet if the lute, that joins the receiver to the retort, be very firm and cloſe, the receivers, though large, are often broken in pieces; which probably would not happen, if the ſpirits could inſinuate and croud themſelves through the pores of glaſs.

BUT, whereas it may be pretended that ſuch veſſels are ſtrong and thick, I ſhall add, that I have had the curioſity to cauſe very fine bubbles to be blown at the flame of a lamp, purpoſely that they may be made extremely thin, and of but a ſmall part of the thickneſs we meet with in the veſſels made at the glaſs houſe; and ſome of theſe I cauſed to be exactly ſtopt, and others to be hermetically ſealed; but could not find, that either dry ſalt of tartar would relent in one, that was kept a good while under water, or that ſtrong ſpirit of ſal-armoniac, which is one of the ſubtileſt ſpirits, that we know, would penetrate one of theſe thin films of glaſs, which we kept a great while immerſed in it, though to diſcover, whether it would at all penetrate the thinneſt glaſſes, we employed ſome, which were of that fine ſort, that is called eſſence vials.

THESE and ſome other trials have, I confeſs, made me very diffident of the experiments, that have been delivered by ſome men of note, and built upon by others, of the permeableneſs of ordinary glaſs veſſels to chymical liquors; as, that mercury and *aqua fortis*, being digeſted together in a bolt-head, may, by rubbing the outſide of the glaſs, be made viſibly and palpably to tranſudate: which experiment (if my memory do not much deceive me,) I purpoſely tried with care, but without ſucceſs.

BUT, after all this, I muſt deſire, that it may be remembered, that in wording the propoſition of the imperviouſneſs of glaſs, I intimated, that I would have it underſtood of what ordinarily happens; for in ſome extraordinary caſes, which I take to be exceeding rare, I do not abſolutely deny, but that the general rule may admit of exceptions. And, if it be lawful to conjecture, theſe exceptions are likelieſt to take place, when the peculiar texture of this or that glaſs is more ſlight or lax than ordinary; or, when the bodies, that are to pervade it, are vehemently agitated by heat; or when, beſides a great ſubtlety, and perhaps degree of heat too, their particles chance to have a ſpecial congruity to the relaxed pores of that particular glaſs they are to paſs through. I remember I have ſeen, not without ſome wonder, a ſort of glaſs of ſo ſoft and reſoluble a texture, that veſſels of it of a competent thickneſs would be manifeſtly prejudiced and wrought upon by liquors, that were not confiderably fharp or corrofive, if they were put in very hot. I have alfo heard of another fort of glaffes, made in a certain foreft, complained of by a diftiller, as fubject to be fometimes injured by corrofive liquors. I once knew a doctor of phyfic, that by divers credulous alchymifts was fufpected to have, what they call the philofopher's ftone, becaufe of a certain book, ingenious enough, that he was fuppofed to have written on that fubject. But when, after fome acquaintance, I happened to debate his principles freely with him, he confeffed to me, that he had been miftaken; and to invite me to give him my thoughts upon fuch like works, he frankly made me an ingenious relation of his proceedings, wherein the main thing, that dazzled him, and kept him from feeing his error, was, that he had reduced the matter he wrought on, which was real gold, to that degree of fufiblenefs and fubtlety, that when he gave too ftrong a fire, as miftake or curiofity made him feveral times do, the finer part of the metal would fweat through his glaffes, and ftick fometimes to the outfide of them, and fometimes to the neighbouring bodies. And when I objected, that he might be miftaken in this, and that what he thought had come forth by tranfudation, rather iffued out at fome fmall unheeded crack; he replied, that he had made the obfervation fo often, and with fuch care, that he was fully fatisfied it was a real penetration of the glafs by the attenuated metal, which he was to have convinced me of by trial. But, before he could come to make it, by an error of his own he unhappily died.

BUT, whatever be judged of this penetrating gold, I elfewhere relate, that I having upon a time diftilled fpirit of hartfhorn with a very ftrong fire, into a receiver, that was large and thick enough, but of a courfe kind of glafs, it did appear, upon my beft examination, that the glafs itfelf was penetrated by fome vehemently agitated fumes, or fome fubtile liquor, that fettled in ftrongly fcented drops on the outfide of the receiver. But fuch inftances being very rare, and happening but in fome cafes or conjunctures of circumftances, that are not like to be at all frequent, they cannot hinder the firft part of our propofition to be true in the fenfe, wherein it is laid down.

AND as to the fecond part of the propofition, which *afferts glafs to be pervious to fome corporeal fubftances*, it may be proved *ad hominem* againft any Epicurean, that fhould deny it, (and the Cartefians muft not) by the free ingrefs and egrefs, which our fealed thermofcopes fhew, that the atoms or corpufcles of cold and heat are allowed, through the pores of the glafs, that contains the rifing or falling tincture, or other liquor. And without proceeding upon the peculiar principles of the Epicureans, we may give more certain proofs of the permeablenefs of glafs by certain bodies: for I have elfewhere manifeftly evinced, that the effluvia of a load-ftone will attract and invigorate fteel, though inclofed in hermetically fealed glaffes; nay, I have alfo fhewn by experiment, that the effluvia of fo grofs and dull a body as the earth are readily tranfmitted through glafs, and will operate on iron, in veffels hermetically fealed. If light be, as probably it is, either a fubtile and rapidly moving body, or at leaft require fuch an one for its vehicle, it muft not be denied, that it is poffible for a body without difficulty to pafs through the pores of glafs; fince it is by its help, that we can clearly fee the dimenfions, fhapes, and colours of bodies included in glaffes. To this I fhall add, that far lefs fubtile bodies, than thofe, that conftitute or convey light, may be made to permeate glafs, if their figures being congruous enough to the pores of it, their penetration be affifted by an impetuous motion, or a brifk impulfe; as I have found by the increafe of weight in fome metals expofed for divers hours in hermetically fealed glaffes to the action of a flame: on which occafion I remember, that having fome years ago tried the fame experiment with fome filings of copper, they had indeed their colour much altered, being beautified with exceeding vivid dyes, which they yet retain, but did not evidently appear to be increafed in weight, as if becaufe they were not of a texture

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loose enough to be melted, the igneous particles could not pierce them enough to ſtick faſt in them, at leaſt in numbers great enough to amount to a ſenſible weight.

BUT without the help of fire, or any ſenſible heat, I think it not impoſſible, that glaſs ſhould be freely penetrated by ſome kind of corpuſcles, (though I do not yet know of what ſort they are) that ſometimes happen to rove about in the air. This you will probably be ſurprized to read, but, perhaps, no more than I was at the phænomena, that induce me to write it, But becauſe theſe are very unuſual, and can ſcarce be diſcourſed of without ſome odd reflections hinted by them, I thought fit to ſet down a circumſtantial account of them in another paper, to which it more directly belongs, than to this eſſay; and therefore ſhall now only tell you, what may be ſufficient for my preſent purpoſe: namely, that having in two or three vials cloſely ſtopt, kept a certain limpid and colourleſs liquor, it would by fits acquire and loſe a high colour, though I could not reaſonably impute the changes to any manifeſt ones in the air, nor to any other cauſe ſo probable, as the ingreſs and receſs of ſome very ſubtile and uncommon particles, which at that time happened to ſwim to and fro in the air, and now and then to invade, and ſometimes to deſert, the liquor.

THERE is another ſort of experiments relating to the poroſity of glaſs, to ſhew, that it may be pierced into by bodies, that are not corroſive in taſte, and are not liquors; but only have a forced and temporary fluidity, if they have ſo much as that.

THESE experiments may be drawn from ſome of the ways of colouring panes of glaſs for the windows of churches and other buildings; I ſay ſome of the ways, becauſe, to deal candidly with you, I think, and ſo I preſume will you e'er long, that in divers of thoſe glaſſes the colour doth not pierce at all deep into the glaſs, but is produced by the cloſe adheſion of a deep red, but thin and tranſparent pigment to the ſurface of a glaſſy plate, through both which the beams of light paſſing to the eye, receive in their paſſage the colour of the pigment.

BUT as, by ſome operations, the glaſs is rather painted, or externally enamelled, than tinged, ſo in ſome others the pigment or dying ſtuff appears to pierce a little beneath the very ſuperficies of the glaſs, and the yellow colour will not only go further or deeper, but ſometimes ſeems (for I do not yet poſitively affirm it) to penetrate the whole glaſs from ſide to ſide.

THE methods of painting and ſtaining glaſs having been hitherto the practices of a particular trade, that is gainful enough and known but to few, the artificers are wont to be ſhy of communicating their ſecrets; though we know in general, that glaſs is ſtained, by having the plates covered with mineral pigments, laid on beds of beaten lime, or ſome other convenient powder, and kept for divers hours in a ſtrong fire, but yet not ſtrong enough to make the plates melt down; by which means the pores of the glaſs being much opened by the heat, and the pigments being likewiſe agitated, and ſome of them, as it were, vitrified with it, they are made either to pierce into the plate, or at leaſt to ſtick very cloſely and firmly to it. But, becauſe the practices of glaſs painters require, beſides ſkill and experience, a particular furnace and divers implements, I ſhall add, that to try, whether glaſs may not, without ſo much ado, be ſo ſtained, as to ſhew it to be porous, we took prepared ſilver, (that metal having, of all the minerals I have tried, the beſt ingreſs into glaſs) and having laid it upon a piece of glaſs, not thick, nor yet ſo thin as to melt very eaſily, we laid this glaſs (with the pigment uppermoſt) warily upon a few quick-coals; and having ſuffered it to neal a while, we gave it about ſuch a degree of heat, as might make and keep it red-hot, without bringing it to compleat fuſion; and then, ſuffering it to cool by degrees, we found, as we expected, that the glaſs had acquired a yellow, and almoſt golden colour, which was not to be waſhed off, or to be taken away, without ſuch ſcraping as would injure or

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ſpoil the glaſs itſelf. The way of preparing ſilver for this operation, is not always the ſame: the glaſs painters commonly add to the calcined ſilver ſome mineral bodies, as antimony, yellow oker, or the like. But I, who take the penetration of the colour to proceed from the ſilver itſelf, do ſometimes employ only ſome thin piece of ſilver, ſuch as an old groat, upon which a little ſulphur being put, and kindled in the open air, the metal is preſently calcined, and the powder made uſe of. And this itſelf I do not ſo much out of neceſſity, as becauſe the calcination reduces the metal into ſmall parts, and gives it a form, that makes it more eaſy to be laid on, as one thinks fit. For, otherwiſe, going upon this my ſuppoſition, that the ſilver was the true pigment of the glaſs, I have more than once made glaſs yellow by leaf ſilver laid flat on the ſurface of it, and a little moiſtened, to keep ſo light a body from being blown off. And (to note that upon the by) it is pretty, that if the fire be made too ſtrong, which it is hard to avoid doing, when we will make it ſtrong enough without the help of a furnace, it has ſeveral times happened to me, that the died glaſs, though when held againſt the light it appeared of a golden or yellow colour, yet when held from the light it appeared blue; ſo that here we have in a mineral ſomewhat, that is very like that we admire in the tincture of *lignum nephriticum*, which ſhews almoſt the like difference of colour, as it is held againſt or from the light; which may ſerve for a confirmation of what I have elſewhere ſaid, to ſhew, that colours may be derived from mechanical principles: but that only upon the by. Whether the gold colour produced by ſilver do favour the hopes of thoſe alchymiſts, that work on that metal, upon preſumption that it is but unripe gold, it is improper here to examine. But ſince yellow is not the colour of ſilver, it ſeems the yellowneſs acquired by our glaſs plates argues, that there has been ſome ingreſs of the ſubſtance of the particles of the ſilver into the glaſs, there appearing no way ſo ready to give an account of the change of colours, as by ſuppoſing the particles of the ſilver to be wrought on by the fixt ſalts, and other fine parts, of the glaſs; ſince we know, that metals may afford differing colours, according to the ſaline and other bodies, that work upon them, as copper with ſpirit of urine, which abounds in volatile ſalt, gives a deep blue; with ſpirit of ſalt, a fair green; and with *aqua-fortis*, a colour, that participates of both. And in the making of glaſs of lead with minium and white-ſand, or cryſtal, the glaſs itſelf, if well made, is uſually of an amethyſtine colour: but, if you put a due proportion, (which it a very ſmall one,) of calcined copper to it, this metal will not communicate to the glaſs its own reddiſhneſs, but be ſo changed by it, as to give it a good green, aud ſometimes ſo good an one, that pieces of this glaſs, ſuch as we have cut and cauſed to be ſet in rings, might, among thoſe, that judge of ſtones but by the eye, paſs for no bad emeralds.

ON this occaſion it is likely it will be aſked, whether there be any way of tinging glaſſes quite through, with a true and beautiful red, and whether the art of dying plates of glaſs, which the windows of many old churches ſhew to have formerly been practiſed, be now (as it is commonly ſuppoſed) altogether loſt?

THIS queſtion, conſiſting of two parts, I ſhall quickly diſpatch: the former, by anſwering it without heſitancy in the affirmative; yet, adding withal, that the red tincture being communicated to glaſs, not properly by mere penetration of the pigment, but by the incorporation of it with glaſs or its materials, by the help of fuſion, I think the experiment of no ſuch great uſe in our preſent inquiry, as to hinder me from reſerving what I have obſerved about it to a more opportune place. And as to the ſecond part of the inquiry, it being rather a hiſtorical, than a philoſophical queſtion, I ſhall not here meddle with it; only I ſhall wiſh the queſtion may be cautiouſly ſtated. For, upon the burning the famous cathedral of St. *Paul's* church in *Londou*, many pieces of the red glaſs, that adorned the windows, were found broken and ſcattered about, ſome of of which I procured from a chymift, that had carefully preferved them, defigning to retrieve the loft invention of making the like: but when I came to examine them narrowly, I was confirmed in the fufpicion I had, that the rednefs did not penetrate the whole glafs, but proceeded from a diaphanous pigment very artificially laid on; for though in other poftures no fuch thing could be difcerned, yet, when I fo held it, according to my cuftom in examining painted glaffes, that the furfaces of the plate lay in the fame level with my eye between it and the window, fo that a broken edge was next my eye, I could plainly fee, and made the chymift himfelf fee, the lower part of the plate to be of ordinary uncoloured glafs, upon which there lay a very thin plate or bed of a diaphanous red pigment, which, though it were not eafily, was not impoffible to be here and there fcraped off.

BUT to return to thofe colorations, that feem to pierce into the pores of glafs, I remember, that I had once occafion to diftill in a fmall retort fome gold amalgamed with fuch a fine and fubtile mercury, that being (without the addition of any falt) put to the gold in the cold, they prefently grew hot together. And in the diftillation of this uncommon mixture, I found the matter had, before it flew away, permanently dyed or ftained, about an inch in diameter of the bottom of the glafs, with a colour, that, looked on from the light, was like that of the better fort of turquoifes; but beheld, when it was interpofed between the window and the eye, appeared of a fomewhat golden colour. And this glafs, with fome others oddly coloured, I have yet by me to fatisfy the curious, though I cannot but give advertifement, that the colorations of glafs may be much better performed with fuch plates, and in fuch furnaces, as the glafs-painters ufe, than without them.

SINCE the writing of the foregoing paragraph, I was vifited by an induftrious perfon, much addicted to fome chymical operations, who had formerly advifed with me about a procefs, of which I had had fome experience, that he conceived might be ufeful to him. I then acquainted him with fome of my thoughts about it, and he having afterwards united gold with quickfilver, (which by its effects, will be eafily concluded not to have been in common) he kept them in digeftion for fome months; and afterwards coming to me with a melancholy look, told me, that the fire having been once immoderately increafed in his abfence, the fealed glafs burft with an affrighting noife, and the included amalgam was fo ftrangely diffipated, that fcarce the leaft fragment of it could be retrieved. But the decoction having continued fo long a time, it feems the matter was fubtiliated enough to have a notable operation upon the glafs: for, though the upper part of the bolt glafs were blown off, and fhattered into many pieces, yet the lower part efcaped well enough; and when he brought it me to obferve, what change had been made in it, I took notice with much delight, that the glafs feemed to be tinged throughout with fo fine and glorious a red colour, that I have feen feveral rubies themfelves, in that point, inferior to it.

Short MEMOIRS for the Natural Experimental HISTORY of MINERAL WATERS. Addreſſed by way of LETTER to a FRIEND.

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I FIND by ſome diſcourſe I lately had with the author, that his deſign, in drawing up his memoirs, being to ſet down what had occured to him of his own obſervation and experiments, he purpoſely forbore to conſult the authors, that have profeſſedly written upon medical waters: he would by no means have it thought, that he undervalued thoſe learned writers, that he forbore to cite, becauſe he had them not at hand, as well as becauſe his deſign did not require he ſhould tranſcribe from them. And therefore he deſires, that his readers ſhould not be kept, by any thing he has written, from conſulting other writers, that have treated of mineral waters, eſpecially the late ingenious exercitations of the learned Dr. *Liſter de Fontibus medicatis Angliæ* (after mentioned by our author,) and the curious little tract of the *French* mineral waters, that was brought our author in *Engliſh*, after his memoirs were come to him from the preſs, publiſhed by the virtuoſi of the famous Royal Academy of Sciences at *Paris*, eſpecially where they curiouſly examine the ſaline and earthly reſidences of waters, which our author has not done to the remains of our *Engliſh Acidulæ*, of which liquors he had for the moſt part ſuch incompetent quantities, as concurred with another reaſon to diſcourage him from publiſhing his trials on them. Yet I may ſafely ſay, what he offers here to the reader is far beyond any thing, that has been publiſhed in this kind; for the virtuoſi, as well as the water-drinkers, may reap no ſmall benefit by the peruſal of this learned treatiſe, as containing a great number both of uſeful obſervations and unuſual experiments.

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THE author of the following papers had thoughts of reviewing and enlarging them before he parted with them; and, at leaſt, of annexing notes to ſeveral of thoſe titles of the hiſtorical platform, that are yet left untouched. But, beſides his want of health and leiſure, he was, by the ſupervening of ſome urgent occaſions, obliged abruptly enough to lay aſide this work he was about, and apply himſelf to others, that concerned him more, than the ſcrutiny of mineral waters could. Wherefore conſidering, that he had already made annotations, though but ſhort ones, upon moſt of the conſiderableſt titles or topicks of inquiry enumerated in the ſecond and principal part of his ſchemes above them, he was content to give the enſuing writing, unfiniſhed as it was, to the ſollicitations of ſome virtuoſi, who rather than tarry, till he ſhould have an opportunity, which he knows not how long he ſhall want, were deſirous to take what they found ready, with all its imperfections. Which preſſingneſs of theirs he could not deny to be the more excuſable on this occaſion, becauſe the communicated writing is not pretended to be a full and methodical hiſtory of mineral waters, but only a bundle of ſhort memoirs contributed towards the compiling of ſuch a work.

THESE, that they may be the more conveniently cited or referred to, I thought fit to divide into ſix ſections; whereof the firſt is introductory, and contains ſome general conſiderations about the occaſion, the ſubject, and ſome other things relating to thoſe memoirs. The ſecond contains only a ſet of titles for the firſt part of the propoſed work,

work, becauſe urgent occaſions kept me from making, I as intended, ſome marginal notes upon ſeveral of the particular articles. The third exhibits a ſcheme of titles for the ſecond part of the propoſed work, *viz.* the way of experimentally exploring portions of a mineral water ſevered from the ſpring or receptacle. And becauſe the ſecond part is that, which I mainly deſigned, I have referred to it two other ſections, one, which is the fourth, containing a collection of experiments and obſervations relating to the uſual way of examining mineral waters by galls, as a ſpecimen given on the 13th title of larger annotations on the titles of the ſecond part; and the other conſiſting of leſs copious annotations, and ſometimes much ſhorter notes on divers other articles of the ſame ſecond part. To which, laſtly, is ſubjoined the ſixth ſection, conſiſting only of a ſet of articles referrible to the medicinal uſe of mineral waters; together with a concluſion addreſſed to the ingenious Doctor that ſet me upon this taſk. In proſecuting of which, I deſire it may not be thought ſtrange, that I have not cited authors, that have written of *Thermæ*, or of *Acidulæ*; for in the diſadvantageous circumſtances, wherein I wrote, I ſhould have been kept from conſulting them, if I had had them at hand; and I thought it enough for me, at that time, to impart to my friends, what my own experiments and thoughts had furniſhed me with, how little or mean ſoever that was. Which advertiſement is therefore the more fit to be here given, that I may not divert any from ſtudying thoſe more elaborate pieces, that have within no long time been publiſhed by ſkilful men, and eſpecially by the very learned Dr. LISTER.

Short MEMOIRS for the Natural and Experimental Hiſtory of particular MINERAL WATERS.

SECT. I.

SO many years, Sir, have paſt, ſince I had occaſion to conſider mineral waters, and opportunity to make trials on them with any application of mind, that, though ſince that time ſome virtuoſi have been pleaſed publickly to declare, that they found ſome directions they received from me not unuſeful to the examen of ſuch waters; yet, having forgotten many of my paſt thoughts, and loſt or miſlaid moſt of my memorials about matters of fact relating to thoſe liquors, I fear I ſhall not be able to ſatisfy either you, or myſelf, by what I now write about them. But however, ſince you will needs have me ſay ſomething upon this ſubject; ſince it is a noble one, as that wherein the health of thouſands is concerned; ſince it is of late grown to be more prized and diſcourſed of, than ever; and ſince, I have obſerved men's curioſity about it to have been confined to very narrow limits, moſt men contenting themſelves with the diſcoveries they can make by the infuſion of galls (or their body,) and perhaps a ſlightly improved evaporation: ſince, I ſay, I have theſe invitations to obey you, I am content to offer you my advices, ſuch as they are, for the drawing up of ſuch a natural hiſtory of a mineral water, propoſed, as being comprehenſive of many inquiries and ways of indagation, that even phyſicians have either not known or overlooked, may probably afford a more reaching notice, and inlarged knowledge of the ſubject treated of. Upon which account, I have, I confeſs, a deſire and an aim, though no great hope, that this rude eſſay may, by your improvements, and thoſe of your learned friends, be made of ſome ſervice to the public.

2. BUT here I muſt ingeniouſly own to you, that notwithſtanding the many ways I propoſe of diſcovering the natures or qualities of mineral waters, yet, I think the ſureſt way of knowing them, is a long and ſufficient experience of their good and bad

 effects.

effects. For, I strongly suspect, and it may be partly known, that there are, beneath the surface of the earth, divers mineral substances, some fixed, and some volatile, some in the form of hard bodies, some of soft ones, some of liquors, and some of fumes, divers of which the generality, even of learned men, are altogether strangers to; besides those, that, though some men may chance to have seen, have their natures so little known, that they have not so much as names assigned to them. So that when I consider, that of the ingredients we are unacquainted with (to pass by all the rest, that the earth may conceal) the proportions, wherein they are mingled, may be numberless, and the qualities resulting from these commixtures may be very differing from those of the separate ingredients; I am apt to look upon the difficulty of securely determining the effects of mineral waters *à priori*, as little, if at all less than insuperable to human understandings.

3. BUT this difficulty is not such, as ought to make us think it useless to have a good project of the natural history of a mineral water. For it is no small advantage, to know what particulars are fit for our inquiry, to be furnished with a set of heads, to which one may conveniently refer whatever he tries, or observes, about the subject proposed: and (which is yet more considerable) to be furnished with variety of methods or ways, to make trials fit for investigating the nature, or examining the qualities, of the proposed water; since by the number and variety of purposely and fitly devised experiments, he that makes them may, as it were, view his subject on all sides, and be much assisted to conjecture, what saline, or other minerals known to us, and what quantities of them, do impregnate the water he examines, and consequently what effects they are like to produce in human bodies.

4. THOUGH there be three sorts of things fit to be taken notice of by him, that would give an historical account of a mineral water, whether cold or hot, yet contenting myself to treat but very cursorily of those, that belong to the first and to the third of the three sorts, I have made a more full and particular enumeration of the titles, that peculiarly belong to the second sort of observables; namely, those, that mention the various trials, chymical and mechanical, that are to be made with the water after it is taken out of the spring. This I purposely did, chiefly because it was only of this sort of particulars, that you desired my thoughts, and partly also because they are most wanted and desired by naturalists and physicians, and are like to prove the most instructive to them; having also this to recommend them, that to make the greatest part of them by far, it is not necessary, that a man repair to the place, where the spring rises, but he may at leisure examine the water at home, where he may be accommodated with furnaces, vessels, and other conveniences, to make his trials upon it.

5. A much less discerning reader, than you, may perceive, that in forming the insuing project of a natural history, I aimed much more to assist practical physicians to find the virtues and effects of mineral waters, than to inform speculative naturalists of their causes and manner of being generated. But yet a heedful peruser may find, that I have so endeavoured to gratify physicians, that I have not been altogether wanting [especially in the first part, which is almost wholly mineralogical,] to the curiosity of philosophers, as it relates to all sorts of mineral waters; though you may easily enough discern, and readily confess it, that the following paper does much more regard those cold ones, that from the acid taste, that is found in most of them, are called *Acidulæ*, than those other waters, that, from their heat, are commonly stiled *Thermæ*; because the former sort of mineral waters is that, which I have had the opportunity to be the more conversant in, as well as that, about which alone you have desired my observations.

6. I had

6. I had once thoughts of illuſtrating the following ſets of titles with a kind of *rationale*, briefly declaring the reaſon of their order and their number (for both theſe were conſiderately pitched upon, not lighted on by chance :) but I was obliged to omit it, when I found (as I quickly did) that I had too little leiſure and health, to employ much of either upon a troubleſome work of no greater importance. And therefore, knowing your perſpicacity to be more than ſufficient to make you diſcern ſome reaſon for the order, wherein I have marſhalled the articles of the laſt ſet of titles, which fall under the cognizance of your own profeſſion, I have not been ſollicitous to aſſign that reaſon. And I preſume it will be no great harm, if my haſte have made me alſo omit to perform at preſent the intention I had to make here and there ſome brief marginal notes upon ſome of the articles of the firſt part. And I thought it ſufficient (if not alſo capable of making ſome amends for the newly mentioned omiſſions) to make them ſomewhat numerous, and ſome of them large annotations upon the titles or articles of the ſecond part; this being indeed the chief, that I deſigned to inſiſt on, and preſent you.

7. I expect it will be wondered at, that ſo many inquiries ſhould be propoſed, and ſo many things directed to be taken notice of, about a ſubject, that hath been thought ſo barren, that men are wont to think their curioſity great enough, if they inquire, what colours the mineral water will ſtrike with galls, or oaken leaves; and do obſerve, what kind and quantity of ſalt will remain after the evaporation of the liquor, and I much fear, that ſome, even of your profeſſion, will think I cut them out a great deal too much work, by ſo many troubleſome queries and trials. But I confeſs, that nature, or long experience, having made me, though not a ſceptical, yet a ſuſpicious and diffident philoſophiſer, I think myſelf obliged, on difficult occaſions, to aſk more than ten queſtions, before I preſume to anſwer one. Nor do I think, that the ſlightneſs of another's curioſity diſpenſes me from induſtriouſly exerciſing mine. I might on this occaſion repreſent, that though the greateſt naturaliſts and phyſicians among the antients did not only mention, but admire and diſcourſe of the load-ſtone; yet our *Gilbert* thought fit to examine it further, and was thereby able to diſcover far more numerous phænomena, than all them put together had taken notice of; and I might add other inſtances to the ſame purpoſe. But, to anſwer more cloſely and directly, I ſay, that to diſcover the nature of mineral waters being a thing far more difficult, than thoſe, that have not tried, do imagine, I think we ought to view the ſubject in as many differing lights as we can expoſe it to, and take in as many helps to diſcovery, as we can; ſince a great many particulars, that ſingly, or at the firſt view, ſeem not very pertinent, if they be conveyed in conjunction, and be ſkilfully applied, may much conduce to the deſired end, And, perhaps, hereafter, it will be found uſeful, if not neceſſary, to make large additions to the topics, whoſe number is now thought redundant: for the more qualities and other particulars we are acquainted with in any ſubject, the better grounded and the more enlarged knowledge we have of it. As for the trouble it may coſt to make the propoſed inquiries and trials, it may be ſaid, 1. that they are not all neceſſary (though uſeful) nor yet of equal moment; and therefore the omiſſion of ſome, that are leſs important, may not diſappoint the main ſearches. 2. I have purpoſely made moſt of the trials as eaſy and ſhort, as the matter and ſcope will permit; and thoſe, that will not undergo ſome trouble in ſeeking an uſeful truth, do not deſerve to find it, eſpecially ſince, in the chace of noble diſcoveries, as in hunting the nobler game, the toil oftentimes makes a part of the pleaſure. And I have made the leſs ſcruple to be ſomewhat ample in the inquiries I propound, becauſe divers obſervations have perſuaded me, that phyſicians ought to conſider very well both the nature of the waters they ordain, and to what perſons, for what diſeaſes, and in what manner they

they prefcribe the ufe of them: for though many look upon them as fuch innocent medicines, as, if they do no good, can at leaft do no harm; yet the effects, that have too often infued the unfkilful ufe of them, efpecially when it was long continued, allow me not to look upon the drinking of mineral waters as a flight thing, that may fafely be plaid with, but as that, whereby we have feen, as very much good, fo a great deal of mifchief, done, efpecially fome time after the operation is thought to be quite over, and perhaps almoft forgotten.

8. I look upon the examen of the properties and other qualities of mineral waters, as a thing, that is therefore of the greater importance, becaufe, I am apt to think, upon probable grounds, that, by a diligent inquiry, there may be difcovered in *England* (and in divers other countries too) a far greater number, than is yet imagined, of mineral waters, efpecially ferruginous ones; which I therefore guefs will be found very numerous, becaufe, by fome uncommon ways of trial, that I have employed, I have found, that divers minerals, that either men knew not what to make of, or, by reafon of their paffing under other names, did not fufpect to be martial, did yet partake of, and perhaps abound with, parts of a martial nature. And I fhew, in another paper [*about the magnetifm of the Earth*] that kindly provident nature, or rather its divine author, has, under various difguifes, furnifhed our globe with a far greater plenty and variety of iron ores and minerals, that partake of that metal, the moft ufeful by far to mankind, than of any other metal. And as martial minerals do thus abound in the earth, fo they are more difpofed, than one would fufpect fuch hard bodies could be, to impregnate even fuch liquors, as are not manifeftly acid, and feem unlikely to be able to work upon minerals far lefs hard than they. To make this probable, we took not iron ore, or embryonated mars, but pure fteel itfelf, the fame as needles were made of; and upon the minute filings of it, we put fome tincture of galls made with common water, and filtered through cap-paper, that the prefent colour of the liquor, and the change we expected to be made in it, might the better appear: and by this trial we found, that in lefs than an hour the tranfparent infufion of galls was fo altered, as to be grown not only opacous, but of a dark and almoft inky colour, which it retained even after filtration; and this, though the vial, that contained it, was very flender. A not unlike effect was produced by fmall filings of fteel, but fomewhat flowlier, in the red tincture of brazil, and that of logwood, made with common water.

9. I know not, whether it may not be fit to be reprefented on this occafion, that in countries manifeftly abounding with metalline and other minerals, it may perhaps be worth while, that men's curiofity defcend much lower than the fuperficies or turf of the ground, and make fearch both after fubterraneal fprings and wells, and their operations upon human bodies. For I have upon inquiry been affured by thofe, that in feveral places have vifited mines, that they have met with in them, and fometimes at very great depths, running, as well as ftagnant, waters, of differing taftes, and fometimes other qualities; and that the diggers venturing to make ufe of them to quench their thirft, as they found fome of them mifchievous (as corrofive, petrific, *&c.*) fo they met with others, that were not only innocently potable, but medicinal. Of both thefe forts we have inftances, in our tin-mines of *Cornwal* and *Devonfhire:* and of the latter fort I received from an ingenious gentleman, that has the overfight of fome *Cornifh* water-works, this memorable anfwer to an inquiry I fent him. The ftrangeft account, fays he, of mineral waters, that I have yet had, was of that in the bottom of a tin-work called *Karnkey*, wrought above 60 fathom [that is 360 foot deep;] the mineral being a mixture of tin and iron, and the water red and puddle, yet drank was cool and not naufeous, and would pafs by urine near as red as it was drank, as I have been informed by thofe, that drank of it whilft it [the mine] was working, being now ftruck

ſtruck out, [that is, the vein of ore being degenerated, or loſt]. However, I believe experiments might yet be made with water much of the ſame nature. Thus far he, from whom, notwithſtanding the remoteneſs of the place he lives in, I hope to get ſome of this liquor to make trial of; which if I do, I deſign you an account of the effects.

I could enlarge upon the ſubjects of theſe two laſt (the 8th and the 9th) numbers; but, after ſo long an introduction to ſhort memoirs, it is high time, that I come at length to ſet down the topics themſelves, that I deſign to propoſe.

SECT. II.

TITLES

For the natural hiſtory of a mineral water propoſed, conſidered as being yet in its channel or receptacles: (being the firſt, or mineralogical, part of the deſigned work.)

HE, that would draw up the hiſtory of a mineral water, [to have its qualities, ſome examined and ſome inveſtigated,] ſhould, in my opinion, make three ſorts of obſervations about it. For firſt, he ought to take notice of thoſe particulars, that relate to it whilſt it is yet under ground, or in its native receptacles: next, he is to examine the properties and other qualities of it, when it is drawn up by men at the ſpring-head, or other receptacle: laſtly, he is to conſider the operations and effects of it upon human bodies, whether ſick or ſound, according to the ſeveral ways and circumſtances made uſe of in adminiſtring it.

To the firſt of theſe three ſorts of obſervations may be referred ſuch heads or titles, as theſe.

1. IN what climate and parallel, or in what degree of latitude, the mineral water does ſpring up, or ſtagnate?

2. WHETHER the ſpring-head, or other receptacle, do chiefly regard the Eaſt, the Weſt, the North, or the South?

3. WHETHER the water be found in a plain or valley? And if not, whether it ariſe in a hillock, a hill, or a mountain?

4. AND whether it be found at or near the top, the middle, or the bottom, of the riſing ground?

5. WHETHER the waters leave any recrement, or other unuſual ſubſtance, upon the ſtones, or other bodies, that lie in the channels they paſs through as they glide along, or the receptacles that contain them?

6. WHETHER there be beneath or near the medicinal water, any ſubterraneal fire, that hath manifeſt chimneys or vents, and viſibly (by night only, or alſo by day,) burns or ſmokes, either conſtantly, or at certain periods of time?

7. WHETHER at or near the mouth, or orifice, of the abovementioned chimneys or vents, there be found either flowers of brimſtone, or a ſalt like ſal-armoniac, or ſome other mineral exhalations in a dry form?

8. WHETHER there be under or near the courſe or channel of the water, any ſubterraneal æſtuary, or latent maſs, of hot, but not actually, or at leaſt viſibly, burning matters? And whether ſuch æſtuary afford an uniform heat, as to ſenſe, or have periodical hot fits, as it were; and if ſo, whether theſe come at certain and ſtated times, or uncertainly or irregularly?

9. WHETHER it be obſerved, that over the æſtuary, or in ſome other neighbouring part of the place, where the mineral water ſprings, there ariſe any viſible mineral fumes or ſmoak, (which, when they do appear, are wont to do it early in the morning, or

late in the evening,) and if ſuch fumes aſcend, how plentiful they are, of what colour, and of what ſmell?

10. WHAT is the more obvious nature of the not manifeſtly metalline, nor marcaſitical part of the ſoil, which the medicinal water paſſes through or touches? And what are the qualities of the neighbouring ſoil, and the adjacent country? As whether it be rocky, ſtony, clayiſh, ſandy, chalky, *&c.*

11. WHETHER there be any ores, marcaſites, or earths, (eſpecially highly coloured ones) impregnated with mineral juices, to be met with in the courſe of the medicinal ſpring, or in the receptacle of the ſame water ſtagnant? And what theſe minerals are, whether copperiſh, ferrugineous, marcaſitical, *&c.* and whether the ores do, or do not, abound in the metalline portion? As alſo with what other ingredient, as ſpar, cauke, ſulphur, orpiment, arſenick, *&c.* (whether innocent or hurtful) they are mingled, or elſe compacted together?

12. WHETHER it can be diſcovered, that the ſpring of the medicinal water was common water before it came to ſuch a place or part of the ſoil it runs through, and there begins to be manifeſtly impregnated with mineral bodies?

13. AND whether, in this caſe, it makes any efferveſcence, or other conflict, with the mineral it imbibes, or with any other water or liquor, that it meets with in its way; and whether the conflict produce any manifeſt heat or no?

14. WHETHER, if the mineral water propoſed be manifeſtly hot, or extraordinarily cold, the ſprings it flows out at, or the receptacle it ſtagnates in, have near it (and if, it have, how near) a ſpring, or well of water, of a contrary quality, as it is obſerved in very neighbouring ſprings in ſome few places of *France*, and elſewhere?

15. WHETHER, when the water appears in the ſpring or receptacle, there appear alſo, either floating at the top, or lying at the bottom, or ſwimming between both, any drops or greater quantity of oil, (like naphtha or petroleum,) or ſome other bituminous and inflammable ſubſtance?

16. WHETHER the water be conſiderably altered, in quantity or quality, by the different ſeaſons of the year, as ſummer, winter, *&c.* by the much varying temperatures of the air, as to heat, coldneſs, drought, *&c.* by the plenty, or paucity, frequency, or unfrequency, of falling rains, or ſnows: and what may be the bounds and meaſures of theſe alterations of the mineral water?

17. WHETHER any thing conſiderable can be certainly diſcovered, or any very probable conjecture made, of the nature and qualities of the ſubſtances, that impregnate the water, by chymically and mechanically examining the mineral earths, through which it flows, or in which it ſtagnates? And particularly, by obſerving their colour, whether native or acquired, by being kept in the fire; their ſpecific gravity; their affording, or not affording, any ſalt, or other ſoluble ſubſtance, by decoction; their being ſoluble, or indiſſoluble, in particular chymical menſtruums of ſeveral ſorts, as *aqua fortis*, ſpirit of ſalt, *&c.* and their being committed to diſtillation in veſſels of differing ſorts, and various degrees of fire, with care to receive ſeparately the differing ſubſtances they afford, whether in the form of liquors, or of flowers; and by examining theſe ſubſtances by fit and proper ways, as alſo the *cap. mort* by calcination, elixiviation, and (if it will bear ſuch a fire) vitrification?

SECT. III.

TITLES

For the natural history of a mineral water proposed, considered as being drawn out of its spring or receptacle: (being the second, or physico-chymical, part of the designed work.)

THAT this scheme of titles may be the better understood, and the more instructive and useful, though I have not time to write an ample comment upon it all, yet I thought fit to illustrate most of its particular articles by such notes, as may either explicate the meaning of what is but briefly couched, or deliver some of the practical ways of trial, that I make use of, on occasion of the subject mentioned in the title or article, whereto the notes belong. These being divers of them too large to be conveniently placed in the margin, are all of them set down together after this set of titles.

TITLE.

1. OF the actual coldness or heat of the mineral water proposed.
2. OF the specific gravity of the mineral water proposed.
3. OF the transparency, the muddiness, or the opacity of the mineral water.
4. WHETHER the mineral water will, by standing for a competent time, let fall of itself any oker, or other earthy substance, especially though the liquor be kept from the air?
5. WHETHER any thing, and if any thing, what can be discovered in the mineral water by the help of the best microscopes adapted to view liquors?
6. OF the colour or colourness of the mineral water.
7. OF the odour of the mineral water, as acetous, winy, sulphureous, bituminous, &c.
8. OF the taste of the mineral water, as acid, ferruginous, vitriolate, lixivial, sulphureous, &c.
9. WHETHER any change will be produced in the transparency, colour, odour, or taste of the mineral water, by its being taken up at the spring-head or other receptacle; or removed to some distance, by its being kept stopped or unstopped for a greater or lesser space of time; and, by its being much warmed or refrigerated, and also, by naturally or artificially produced cold, turned into ice, and thawed again?
10. OF the thinness or viscosity of the mineral water.
11. WHETHER the mineral water be more easy to be heated and cooled, and to be dilated and condensed, than common water?
12. WHETHER the mineral water will of itself putrify, and, if it will, whether sooner or later, than common water, and with what kind or degree of stink and other phænomena?
13. OF the change of colours producible in the mineral water by astringent drugs, as galls, pomgranate-peels, balaustium, red roses, myrobolans, oaken leaves, &c. as also by some liquors or juices of the body.
14. WHETHER any thing will be precipitated out of the mineral waters by salts or saline liquors, whether they be acid, as spirit of salt, of nitre, *aqua fortis*, &c. or volatile alcalies, as strong spirit of urine, sal-armoniac, &c. or lixiviate salts, as oil of tartar *per deliquium*, fixt nitre, &c?
15. How to examine with evaporation, whether the mineral water contain common salt, and, if it do, whether it contains but little or much?
16. How to examine, without evaporation, whether the mineral water have any acidity, though it be but very little?

17. OF the liquor or liquors afforded by the mineral water by diſtillation *in balneo*, and other ways.

18. OF the reſidence, *cap. mort.* of the mineral water, when the liquor is totally evaporated or diſtilled off; and whether the *cap. mort.* be the ſame in quantity and quality, if produced by either of thoſe ways?

19. WHETHER the propoſed water being, in glaſs veſſels exactly luted together, ſlowly and warily abſtracted to a thickiſh ſubſtance; this being reconjoined to the diſtilled liquor, the mineral water will be redintegrated, and have again the ſame texture and qualities it had at firſt?

20. WHETHER a glaſs full of mineral water being hermetically ſealed, and boiled in common water, deep enough to keep it always covered, will have its texture ſo altered, as to ſuffer an obſervable change in any of its manifeſt qualities? And if it do, in what qualities, and to what degree of alteration?

21. OF the proportion of the dry *cap. mort.* to the mineral water, that affords it.

22. OF the diviſion of the *cap. mort.* into ſaline and terreſtrial, and other parts not diſſoluble in water, in caſe it contain both or more ſorts.

23. OF the proportion of the ſaline part of the *cap. mort.* to the terreſtrial.

24. OF the fixity or volatility of the ſaline part in ſtrong fires.

25. WHETHER the ſaline part will ſhoot into cryſtals or no? And, if it will, what figure the grains will be of? And, if it will not, whether being combined with a ſalt, that will (as purified ſea ſalt-petre, *&c.*) it will then cryſtallize; and if it do, into what figures it will ſhoot, eſpecially if any of them be reducible to thoſe of any ſpecies of ſalt known to us?

26. To examine, whether the ſaline part be, *ex prædominio*, acid, alcaliſate, or adiaphorous?

27. OF the obſervables in the terreſtrial portion of the *cap. mort.* as, beſides its quantity in reference to the ſaline, its colour, odour, volatility or fixity in ſtrong fire; its being ſoluble, or not diſſoluble by divers menſtruums, as ſpirit of vinegar, ſpirit of urine, oil of tartar, *&c.*

28. WHETHER, and (if any thing) how much the mineral water's earth looſes by ſtrong and laſting ignition? What changes of colour, *&c.* it thereby receives? Whether it be capable of vitrification *per ſe?* And what colour, (if any,) it will impart to fine and well powdered Venice glaſs, if they be exactly mixed and fluxed into a tranſparent glaſs?

29. OF the oeconomical and mechanical uſes of the mineral water, as in brewing, baking, waſhing of linnen, tanning of leather, or dying of cloth, callicoes, ſilks, *&c.* as theſe may aſſiſt in diſcovering the ingredients and qualities of the liquor propoſed.

30. OF the imitation of natural medicinal waters, by chymical and other artificial ways, as that may help the phyſicians to gueſs at the quality and quantity of the ingredients, that impregnate the natural water propoſed.

An APPENDIX. Containing,

1. PARALIPOMENA, or things directly belonging to the hiſtory and pretermitted in it.

2. A chaos of obſervations and experiments, remotely or indirectly referrible either to one or more of the foregoing titles, or to the common ſubject of them all.

SECT.

SECT. IV.

Experimental remarks upon the (usual) way of examining mineral waters by the help of galls: delivered by way of larger annotations upon the thirteenth article of the second part.

SINCE the change of colour, that mineral waters produce in the infusion or tincture of galls, is the most usual way, that many physicians, and the almost only, that some of them endeavour to discover or examine mineral waters by, it may be worth while, in this place, to set down some remarks, that I have made about this way of probation; and the rather, because it may, *mutatis mutandis*, be not unusefully applied to the exploring the qualities of mineral waters by colorations, though made with other materials than galls. First then it may be observed, that one need not make an infusion or tincture of galls in common water, to try if, by their means, a new colour will be produced: for I am wont to beat them to powder, and keep them in a glass (not too big) exactly stopped, by which means I have them always in readiness to mingle with the mineral water, and alter the colour of it, if galls be able to do it, almost in a trice; whereas, to draw the tincture of galls with simple water, often takes up several hours, and the tinging parts are much weakened by being diluted by the menstruum. If you would have a tincture, the powder of galls, tied up close in a rag, and with it hung in the liquor, makes the infusion less muddy. If you be in haste, and have none of the powder at hand, you may scrape as much of a gall-apple, as you need, into the mineral water.

2. I have observed those parts of the infusion of galls, (especially if made by heat) that produce the new colour with ferruginous waters, to be more apt to fly away than one would think, the infusion becoming often unfit to alter the colour of the martial waters, whilst yet itself appears sufficiently high coloured. Upon which account I chuse to make a tincture of galls not long belong I mind to use it; and, if I employ dry galls, to take powder, that is not stale.

3. IT is no safe way, and may be very erroneous, that is usually taken in mixing galls, or their infusion with the water to be explored, so carelesly, as is wont to be done. For those, that are curious to make good ink, will easily believe, that much of the deepness of the colour depends upon the proportion of galls to the other ingredient; and accordingly, that by putting a much greater, or a much lesser, quantity of galls into such a quantity of the mineral water, the resulting colour may be more or less intense. To obviate which inconvenience, I take this course, when the occasion deserves it; I make my infusion of galls with a certain weight of the powder in a determinate weight of water. As for instance, I put about five gr. of powdered galls, to steep for so many hours in an ounce of water: but if I make use of the dry powder, then I am wont to put three or four grains into an ounce of the liquor to be examined; which is a way far more certain, than the common, wherein the ingredients are estimated but by guess. I have mentioned various proportions of powdered galls to the same quantity of liquor, because I have observed, that there is really a great inequality among the mineral waters, in which it may be put; and I have found by trial, that in an ounce of the *German* Spa, a single grain of powder would immediately produce a sufficiently deep purple colour.

IT is an inconvenience, that not only galls, but the other drugs hereafter to be mentioned, impart a high tincture of their own to the common water they are infused in; and therefore it were to be wished, and is fit to be endeavoured, that we had some drug, that, without imparting a colour to the common water it impregnates, would afford an infusion fit to strike a blackish or a purple colour with martial waters.

THOUGH it be ufeful, yet it is not neceffary, to employ galls to produce a colour in the mineral water propofed: for befides that it is known, that ufually, (though not always, as I have tried) the fame thing may be done, but fomewhat more faintly, with oaken leaves, we may fuccefsfully enough fubftitute, for the fame purpofe, fome other aftringent vegetables, as dried red-rofe leaves, the peel, and (as we have tried) the juice of pomegranates; and (what I find to be a notable ftiptick) the bloffoms of the fame plant, (which are vulgarly called in the fhops *ballauftium:)* to which may be added myrobolans, logwood, and fome others, that need not now be mentioned, whofe ftrong infufions have yielded me a tincture very dark and blackifh with fome martial liquors.

6. IN regard that the galls, or other drugs, to be infufed in common water, are not always of the fame goodnefs or ftrength, it is adviseable not fo to truft to any determinate proportion of the pigment to the water, as not to take in the help of the eye to judge, by the colour of the tincture, whether the liquor be duly (and not too much or too little) impregnated.

8. WHEREAS there is an intimation in the clofe of this thirteenth article of the prefent fet of titles, that animal liquors may be employed to produce new colours with mineral waters, I gave that hint, not only becaufe it is ufually obferved in martial waters, fuch as thofe of *Tunbridge*, the *Spa*, *&c.* that the grofs excrements of the lower belly are blackened by a commixture of their metalline parts; but in *Tunbridge* waters particularly I have obferved that after the drinking of larger dofes of them, the root of the tongue, and perhaps fome neighbouring parts, would alfo acquire a dark colour, by the operation of the tranfient liquor.

THOUGH the way of trying mineral waters by the change of colours, that galls produce in them, be ufeful and recommended by being eafy, cheap, and expeditious, yet I do not take it to be either of that extent, or of that certainty, that it is vulgarly prefumed to be of: for its main, if not only confiderable ufe is, to difcover by ftriking, or not affording, a black or blackifh, or at leaft a purple or purplifh colour, with a mineral water, to manifeft the liquor to be, or not to be, either of a vitriolate, or a ferruginous nature. But there are divers metalline ores, and other mineral bodies, which, not participating of iron, will not, by this way, be difcoverable, and yet may ftrongly impregnate the water propofed: as for example, to try, whether if arfenic were mingled with water, galls would difcover it by producing with it a dark colour, I put fome of the powder of them into a decoction of arfenic, but did not perceive, that it gave the liquor any deeper colour, than it would have done to common water. And as the extent of this explorer of waters is not very great, fo neither do I find the informations it gives us to be fo certain, as they are prefumed: for, if I much mifremember not, I long fince found, upon trial purpofely made, that another body of a metalline nature, and that did not partake of iron, would, with infufion of galls, afford a very dark colour, that might eafily, among ordinary beholders, pafs for the colour produced by a martial water: and I do fomewhat doubt, whether fo much as all liquors impregnated with iron will be difcovered to be fo by the colour they afford with galls; for I have fometimes made fuch a liquor with no mineral fubftance in it, fave fteel or iron, but I did not find it would turn the infufion of galls either blackifh or purple; which made me fufpect, that thefe colours are afforded only by fuch martial waters, as have been wrought upon, more or lefs, by fome acid falts or fumes.

9. UNTO thefe things I fhall add, that I found that to be a miftake, which is generally taken for granted, *viz.* that the infufion of galls will certainly difcover, by becoming black, (or purple) if a mineral water, that is mixed with it, be vitriolate; for tho' it be true, that if in the vitriolated water iron, be the only or predominant mineral, or be

be at leaſt conſiderably participated by the liquor, yet if the diſſolved vitriol be altogether copperiſh, I found, by ſeveral trials purpoſely made with a ſtrong ſolution of Roman vitriol, (wherein copper is affirmed to be the only, or to be very much the predominant metal) that it would not, with infuſion or tincture of galls, afford either a black or a blackiſh colour, but only a thick and muddy one, that was not ſo much purpliſh.

It comes into my mind, upon this occaſion, that from one of the northern countries of *England*, where there are divers mineral waters, there was brought me by a virtuoſi a good quantity of very whitiſh earth, which he ſuſpected to be of a peculiar nature, but could not tell of what. This odd earth being examined, I concluded it to contain a conſiderable proportion of lead ore, corroded by ſome mineral ſalts, and embodied with the ſoil; ſo that if it had been in a place, where people had ſought for mineral waters, it is probable, that, finding ſome peculiarity in the taſte of thoſe, that paſſed through this earth, they would have taken it for a mineral water, but had been at a great loſs to determine what mineral it did partake of; and perhaps, in endeavouring to reſolve the doubt, by drinking it, they would have found very bad effects of it. But probably the ſulphureous ſpirit, to be e'er long deſcribed in this paper, would have informed them, that the water was impregnated with a body of the nature of vitriol, but not of common vitriol: for though galls do not give a black, or very blackiſh colour with a ſolution of *ſaccharum ſaturni* (which is indeed the vitriol of lead) reſolved in diſtilled or rain water; yet I found by trial, that this volatile ſulphur did manifeſtly and preſently do it; which trial I was fain to take up with, becauſe, when I had occaſion to conſider this matter, I had not at hand the ores of lead, copper, &c. and therefore was fain to content myſelf with the ſolutions of the metals themſelves in their proper menſtruums, it being probable, that the metalline parts of the ores would have afforded either the ſame ſolutions, or ſome very like them, in the ſame menſtruums; which conſiſting of niter, ſea-ſalt and vitriol, bodies that abound in diverſe places of the earth through which ſprings flow, the impregnated water would afford phænomena of the ſame kind. I made trials alſo upon a ſomewhat fine ſolution of refined gold made in an aqua regalis, and upon a ſolution of common running mercury made with aqua-fortis, and in a clear ſolution of tin made, not with either of the foregoing menſtruums, (for I have not found them to diſſolve it genuinely) but in a peculiar ſolvent, (which I have communicated in another paper) that does not only diſſolve it readily, but keep it permanently diſſolved, as aqua-fortis does ſilver, but not tin. To theſe ſolutions I put galls, without obtaining any blackiſh colour, except from that, which contained gold. But with our ſulphureous liquor we produced notable changes of colour, and thoſe, in all the ſolutions but one, a dark one, or tending to blackneſs; and though, for that reaſon, a careleſs eye might judge them indiſcriminately to be blackiſh; yet, ſince I well remember, that the degrees, or ſome other modification, of the ſame dark colour ſeemed plainly enough not to be the ſame in all of them, I do not think it impoſſible, but that a very heedful beholder (which, when I made thoſe trials, I had no great motive to be) may diſcern between thoſe obſcure colours ſome little differences, that may much aſſiſt him to gueſs, what metalline ſubſtance is contained in the liquor, or at leaſt is predominant in it, if it be a compounded one. And I particularly remember, that the colour that, ſprang from our ſulphureous liquor and ſolution of tin, was manifeſtly diſtinguiſhable from thoſe produced in that of any of the other ſolutions, being not black or blackiſh, nor ſo much as purple, but of a kind of browniſh yellow.

Though I am content, that the things I come from mentioning, ſhould make men cautious and diffident, yet not only I do not deſpiſe or ſlight the uſe of galls, &c. even as it is vulgarly practiſed, but I am apt to think, that the way of exploring mineral waters by the changes of colour, that may be produced in them or by them, when they

they are mingled with convenient drugs or additaments, may be made of greater extent and uſe, than he, that has read what I have written in the foregoing number, will perhaps be forward to expect. But to make the way of exploring mineral waters by colorations, of ſomewhat more general uſe and leſs uncertainty, I would recommend theſe things to the experimenter,

(1.) IT ſeems very fit, if not neceſſary, that he look upon the change of colours, both while it is producing, and when it is produced, in a good light, and with a heedful eye: for by this means he may diſcover ſeveral ſhades or varieties of the more principal colours, and ſome other circumſtances, that he could not elſe take notice of; and which yet may afford good hints (in reference to other minerals, as well as martial ones) to a ſagacious obſerver. And I have ſometimes fancied, that there may be a kind of phyſiognomy of many, if not moſt other natural bodies, as well as of human faces, whereby an attentive and experienced conſiderer may himſelf diſcern in them many inſtructive things, that he cannot ſo declare to another man, as to make him diſcern them too.

(2.) THE attention here encouraged may perhaps be made more inſtructive by a way, that I have ſometimes practiſed to vary the ſhades and other phænomena of colours produced with mineral liquors. This way conſiſts chiefly in preparing ſheets of white paper, by drenching them in a ſtrong infuſion of braſil, logwood, or ſome other convenient dying ſtuff, and then letting them dry leiſurely in the air, which may give ſome of them, as I have obſerved, a colour differing enough from that of the liquor looked upon in a vial or drinking-glaſs. Upon this dry paper ye may let fall, but not all on the ſame place, ſome drops of the mineral liquor to be examined, eſpecially if it be of a ſaline nature; and by the changes of colour effected by theſe drops on the parts of the paper they fell and ſpread themſelves upon, a heedful obſerver may be aſſiſted to gueſs, what kind of mineral impregnates the liquor, and how much it does ſo; eſpecially if, on the ſame ſheet of paper, ſome other fit mineral water, or idoneous liquor, be likewiſe dropped, that the changes of colours produced by the two fluids may be ſurveyed and compared together. I alſo practiſed another way ſomewhat differing from this; as the main part of which we prepared white paper, by rubbing well upon it, with a hare's foot, or ſome ſuch thing, ſome idoneous powders, eſpecially that of vitriol (whereof, for this purpoſe, Engliſh ſeemed the beſt) lightly calcined in a gentle heat, till it became of a greyiſh colour and friable between the fingers. By this means it was eaſy to make the paper fit for our turn: for the finer parts having lodged themſelves in its pores, without much diſcolouring it, when the ſuperfluous duſt was ſtruck off, it became capable of affording a variety of colours, or rather ſhades, ſome deeper and ſome fainter, when I let fall on it ſome drops of differing martial liquors. But of the examen of the *materia medica*, by the changes of colour produced in it or by it, more is ſaid in another paper; and therefore, inſtead of transferring that hither, I ſhall here briefly intimate, that divers variations of colour may be made, either by infuſion or otherwiſe mixing, as I have ſometimes done, ſomething in the mineral water before the tinging ſtuff be put to it; or by putting ſomewhat in the infuſion or powder of galls, before it be mixed with the mineral water; or elſe by dropping fit liquors (ſuch as ſpirit of ſalt firſt, and then ſpirit of urine, or oil of tartar) into the blackiſh or purple mixture of galls, and the medicinal water to be examined: for by theſe means diverſe variations of colours may be obſerved, which, together with ſome other ways, that I have made uſe of to multiply them, I have not now leiſure to ſet down.

(3.) IT is not convenient to confine one's ſelf to the uſe either of galls or oaken leaves, but to make uſe alſo of red roſes, *balauſtium*, logwood, braſil, and other aſtringent vegetable pigments. For though ſome of theſe give a deeper tincture than galls,

galls, yet, by the diverſity of colours produced by them in mineral waters, an attentive beholder may, as was lately intimated, where I mentioned diverſity of lights and ſhades, diſcover ſome things, that he would not be informed of, or receive any hints of, by the help of galls or oaken leaves alone. Nay, I would not have our experimenter employ none but vegetable ſubſtances about his colorations, but ſometimes make uſe of animal ones, and (more often) of minerals; ſince, by this means, he may much diverſify his trials, and increaſe the number of phænomena, ſome of which he may probably find inſtructive. Beſides aſtringent plants, I have found, and ſometimes deviſed, other ſubſtances, that will turn black, as well as galls, with vitriolated water; and that not only with thoſe, that are richly impregnated with iron, but alſo with thoſe, wherein copper alone abounds, as in Roman vitriol. And though, for certain reaſons, I muſt not now ſet down a way I have, to diſcover in a trice both theſe vitriols, without any liquor or tangible body, yet I ſhall ſubjoin, as a kind of *ſuccedaneum,* that may ſuffice for the preſent occaſion, the way of making a liquor, that will preſently turn black with a ſolution either of martial or cupreous vitriol.

"Take equal parts of pure ſalt of tartar, and either flowers of ſulphur, or at leaſt " ſulphur finely powdered, and good ſal-armoniac; reduce the firſt and the laſt to " powder ſeparately, melt the ſulphur over a gentle fire, and by degrees put to it the " ſalt of tartar, ſtirring them well, to make them incorporate and grow red, (or red-" diſh:) then put this mixture pulveriſed into a glaſs retort, or a cucurbite, and pour " on it the ſal-armoniac diſſolved in fair water; and cloſing well the junctures, diſtill " all in ſand by degrees of a moderate fire, ſhifting the receiver once or twice, becauſe " the liquors will be differingly tinged and ſtrong; and that, which aſcends laſt, may " bring over but very little of the ſulphur, whoſe volatile tincture is yet the main thing " we aim at in this operation."

(4.) I do not deſpair, but that he, who were able to make a ſkilful uſe of the ſeveral drugs and other bodies, vegetable, animal and mineral, that may produce new colours in, or with, mineral waters, (or, in ſome caſes, with the ſubſtances, that impregnate them) may, by their means, be alſo enabled to diſcover the preſence or inexiſtence of divers other minerals, ſome of them ſalubrious, or at leaſt ſafe, and ſome others either hurtful, or at leaſt dangerous, that are not taken notice of by thoſe, that content themſelves to employ galls and oaken leaves in the exploration of the waters they examine: for ſome of theſe liquors contain ſalts, that, having not corroded either martial or cupreous ores, or marcaſites, do not betray themſelves by producing either an inky or a fainter degree of blackneſs, or elſe a purple, with the drugs made uſe of to change their colours. Of theſe ſalts I have met with more than one ſort, which may be more properly taken notice of, when we conſider the mineral water, and its contents.

12. I think it likewiſe very poſſible, that induſtrious men ſhould find ways to diſcover, by the help of the change of colours, whether orpiment or native arſenick, or the like poiſonous minerals, do ſo impregnate the water propoſed, as to make it very hurtful or dangerous, though not abſolutely pernicious. And as for ſulphur, there may be ſeveral waters, that partake of it, without being taken notice of to do ſo. For I remember, that I have ſometimes purpoſely made a liquor, that was limpid and colourleſs like ſpring water, and which would totally fly up, even with a gentle heat; and yet this liquor was richly impregnated with a mineral ſulphur, as I convinced ſeveral virtuoſi by manifeſt and ocular proofs. So that if ſulphur chanced to be combined with any ſalt or mineral, of thoſe many ſubterraneal ones, that nature hath hid from us, that can ſuppreſs or diſguiſe its peculiar odour, the water may be conſiderably, and yet unobſervedly, impregnated with it. And yet it is like, this may eaſily be diſcovered

covered by the change of colour producible in such a sulphureous liquor by vitriolate bodies, and other appropriate additaments: which may be thought the more probable, because, though the spirit lately described be very transparent and totally volatile in the form of a liquor sometimes pale enough, yet common English vitriol, as also that of *Dantzick*, which is venereal, will presently turn it of a black or very dark colour. And to add here somewhat more difficult to be performed, I have devised a way, which I elsewhere deliver, whereby it may appear, that even copper, that hath been melted into a body, may be so subtilized and disguised, as to have a multitude of its metalline parts made to ascend with others in the form of a transparent liquor like common water; and yet by putting to it a little of another substance, as volatile and colourless as itself, it would presently disclose the copper it contained, by turning blue as a sapphire.

A Paper referred to certain observations about the salubrity and insalubrity of the air, under whose 4th proposition this process is ranged.

13. BECAUSE arsenic is a very pernicious drug, and yet has been suspected to be clandestinely mingled with some mineral waters, which I thought the less improbable, because some of the marcasitical bodies, by which some minerals pass, are judged not to be devoid of arsenic; for these reasons, I say, and for this other, which makes the mention of it pertinent in this place, that galls did not (as I elsewhere note) discover at all the inexistence of this poisonous drug in water, though the liquor were copiously impregnated with it, I thought fit to make some trials, that seemed to me likely to discover at once the inexistence of arsenic in water, and somewhat of the nature of that dangerous mineral.

HAPPENING some years ago to taste arsenic, not without some little danger and inconvenience, the taste of it did not seem to me to favour the vulgar supposition, that its poisonous nature consists in a highly acid salt; whereas its taste agrees well with my conjecture, who suspect it to be of an exceeding corroding or fretting nature, but whose corrosiveness is *sui generis*, that is, of a peculiar kind. Having then made a strong solution of arsenic in common water, [which does not without some skill easily dissolve it,] we mixed a small proportion of it with the German Spa-water, and then dropping into this mixture some highly dephlegmed spirit of urine, we perceived a light lactescence to be produced, and a whitish precipitate very slowly to subside.

WE found also, that a little (excellent) oil of tartar *per deliquium*, being dropped into some of the lately mentioned solution of arsenic, produced a heavy whitish cloud, which presently settled at the lower part of the glass. We also put oil of vitriol, as one of the strongest acids we know, into the solution of arsenic, but did not perceive, that the oil made a precipitation, or wrought much otherwise on it, than it would have done upon common water. And by these three trials one would suspect, that arsenic is, at least *ex prædominio*, an acid body.

BUT not content with these, we put some of the arsenical liquor upon some syrup of violets, and found it to change the syrup, though but slowly, rather to a green than a red or purple colour.

WE put to another portion of the same liquor some of our volatile sulphureous spirit, but took notice of no precipitation, that ensued.

FOR a severer examen we imployed a trial, that we successfully make use of (and have delivered in another paper) to discover such slight degrees of acidity in liquors, as by ordinary trials are not discoverable; but we could not, by this way, discern the least acidity in our arsenical solution, but rather a manifest token of an urinous or lixiviate quality.

WITH the former experiment agreed very well that, which we afterwards made, by putting some of the arsenical liquor into a strong solution of common sublimate made in fair water. For by this means we had a copious precipitate, such as might have been expected from an alcaline precipitant; and this was not brick-coloured, as fixed alcalies produce with dissolved sublimate, but white, such as urinous or volatile alcalies (as they call them) are wont to make with the same liquor.

THE foregoing trials having been made at one time, when I was in haste, and not at all fond of having to do with arsenic, (for which reason I caused the solution to be presently thrown away to prevent dangerous mistakes;) though what I have hitherto tried seems very favourable to our proposed conjecture; "That though arsenic be a "very corrosive body, and perhaps upon that score poisonous, yet its deleterious na-"ture does not consist only, or mainly, in a transcendently acid, nor in a lixiviate "caustick quality, but in a corrosiveness *sui generis*, I mean peculiar and distinct:" yet I shall forbear to be positive in this conjecture, till further trial, pretending only, by what has been said, to shew the need of examining the vulgar supposition by further inquiries, and to give some hints towards the finding of antidotes against this cruel poison.

I shall now add, that for the sake of water-drinkers I cast about in my thoughts for some way, that might be of use, though of no certainty, in examining a mineral water suspected to contain arsenic. To which purpose, for reasons, which haste forbids me to mention, I pitched upon vitriolate bodies, and found, that if a little solution of *Dantzick* vitriol were put to a convenient quantity of arsenical liquor, there would presently ensue a great change of colour, and a dark substance would by degrees precipitate itself, and settle in the lower part of the glass. The like effect we found, when we put English vitriol, which (having not copper added in the making, as that of *Dantzick* has,) is either altogether, or almost totally, martial, into a considerable proportion of the arsenical solution.

I fear I shall be thought to have dwelt by far too long upon this one (13) article of our set of titles: but I was tempted to do it, partly, because I thought the subject seemed both to merit and to need it; partly, because I thought fit to give an instance, that may shew, that even that part of the exploration of mineral waters, that is judged to be the most cultivated, hath been but superficially enough considered; and partly too, because my want of health, and my preingagement to some subjects, that I am more concerned for, than I am for that I now treat of, permitting me to make few other than shorter notes upon the particular articles and clauses of this scheme of titles; I thought it not amiss, by referring all the foregoing observations and trials to the same topick, to give one specimen (though but an imperfect one) of those, that, for distinction's sake, I stile Large Annotations. And though the title these belong to be the thirteenth in the scheme (of the II part,) yet I thought fit to promise these notes to all the rest, though divers of them be on titles antecedent to the thirteenth; because one or other of the many particulars referred to this last named title may probably be of use to you, in considering many of the other articles of this scheme, whether they follow the thirteenth, or precede it.

MARGINAL NOTES for the II. or physico-chymical part of the natural history of a mineral water proposed.

Notes on the first title.

I. 1. THE article mentions actual coldness and heat, because we do not here consider that, which the schools call potential.

2. THE knowledge of the degree of coldness in the water, especially if it be extraordinary, may somewhat assist the examiner to guess, whether the spring come from

ſome notable depth under ground before it aſcends, or whether it runs through a ſoil abounding with ſalt-petre or ſal-armoniac, or ſome ſuch very refrigerating ſubſtance.

3. THE degree of coldneſs or heat may be eſtimated ſeveral ways; as, if the water be cold, by its having, or not having, the power to coagulate eſſential oil of anniſeeds or that of fennel-ſeed; and if it be that, by its being, or not being, able to melt bodies of ſomewhat differing diſpoſitions to fuſion, as butter, tallow, bees-wax, *&c.* or to coagulate the whites of eggs, or to boil eggs in the ſhell, *&c.* But the beſt way is to plunge into the water propoſed at leaſt the whole ball or globulous part of a good hermetically ſealed thermoſcope, whereon the degrees of cold and heat are carefully marked.

Notes on the ſecond title.

II. THE knowledge of the ſpecifick gravity of a mineral water may be of great uſe to him, that endeavours to diſcover its nature, not only as this knowledge enables him to diſtinguiſh the propoſed water from others, but becauſe it may afford him a conſiderable and double information. For, by comparing the weight of the propoſed liquor with that of common water, he may be, in caſe the former be heavier, (as it uſually happens to be,) aſſiſted to eſtimate, what proportion of ſalt, or martial, or other mineral ſubſtance, it is impregnated with. And if it be very light, and much more if it be lighter than common water, he may probably conclude, that the ſubſtance, that impregnates it, is either very ſmall in quantity or proportion, or is not near ſo groſs, as is to be found in other mineral waters, but of a ſpirituous and volatile nature, which is a diſcovery of no ſmall moment in this affair. And though that may ſeem a paradox, which I here ſuppoſe, that a water impregnated with a metalline or mineral ſubſtance ſhould be as light, or even lighter than common water; yet upon trial carefully made I have found ſome mineral waters, as particularly that of *Tunbridge* well taken up, and (though they be ſomewhat leſs light) that of the German Spa, and of ſome of the *Iſlington* ſprings, to be manifeſtly lighter, than common water; and ſome taken up at *Tunbridge* I found to be lighter, than common water, even purified by diſtillation.

AND though it be very hard to conceive, yet I think it not impoſſible, that a ſubterraneal ſubſtance, that impregnates water, ſhould be lighter *in ſpecie* than it; but yet I would not refer this ſurprizing levity, in all caſes, nor all of it in moſt caſes, to the admixture of lighter corpuſcles; becauſe ſome trials juſtified the ſuſpicion I had, that much of the comparative lightneſs proceeded from this, that the mineral water was imbued with a ſmaller quantity of vulgar or culinary ſalt, than common water uſes to contain. But yet theſe trials did not ſatisfy me, that this paucity of common ſalt was the ſole or adequate cauſe of the lightneſs of the mentioned waters.

BUT, to diſcover ſuch minute differences, one muſt have good inſtruments; and indeed, to ſpeak freely, there are few, upon whoſe reports I durſt confidently rely for the ſpecific gravity of mineral waters; for to weigh liquors any thing exactly, there is requiſite more heedfulneſs, and more ſkill, and better inſtruments, than are eaſy to be met with together, and than we uſually imagine. And when phyſicians and others weigh mineral waters, they are wont to do it in ſome apothecary's or other tradeſman's ſhop, where, if the balances be ſmall, the veſſels and the water are commonly too heavy for them, and oftentimes wrong them, And if, as is uſual, the bottles or other veſſels be great, they require far better balances, than are uſually employed in the ſhops of apothecaries or grocers, whoſe balances a critical examiner will too often find to be far from being accurate; inſomuch that uſually, without at all altering the weights, though perhaps not great ones, he may eaſily make which ſcale he pleaſes manifeſtly preponderate, and continue in that poſition, and may as eaſily afterwards give the other ſcale the ſame advantage. The diligent and experienced methematician *Merſennus* much

much complains of the difficulty he found to weigh liquors exactly, even by the help of his nicer inſtruments, The accurateſt way I know, is by comparing the differing weights, that the ſame ſinking body has in common water and in the liquor propoſed. But this way (which I elſewhere circumſtantially deliver) requiring, beſides good inſtruments, ſkill in hydroſtaticks, is practicable but by few. And the way of comparing waters, by the greater or leſſer ſinking of the ſame cylinder or other ſwimming body into them, is ſcarce accurate enough: wherefore, I choſe to make a very ſkilful artiſt blow, at the flame of a great lamp, a thin round vial with a flattiſh bottom, that it might ſtand upright, and be very light; and this was furniſhed with a neck as large as a gooſe quill, drawn very even into a hollow cylinder of above three inches long, and fitted at the top with a little gap, that hindered the water from aſcending above the due height.

THIS glaſs contained ℥iijſs. and 43 grains of common water, and yet, when empty, weighed but ʒvi + 42 grains: ſo that I could uſe it, when full of liquor, in ſuch a balance, that the addition or detraction of half a grain, or leſs, would make either ſcale preponderate. The length and evenneſs of the ſtem was deſigned for uſes not needful to be mentioned here, where it may ſuffice for my purpoſe to ſay, that this glaſs was judged capable of holding water enough for not uncurious trials, and yet not to be, though well filled, too heavy for a tender balance. In this veſſel therefore we carefully weighed ſeveral liquors (whoſe gravity belongs not to this place) and among them divers mineral waters, ſome of which, at leaſt known here at *London*, were found to be of the annexed weights.

THE glaſs being filled with ſeveral liquors to the ſame height, and weighed in the ſame balances;

	Ounces	dr.	gr.
Common water was found to weigh	3	4	43
Common water diſtilled	3	4	41
Acton water	3	4	48 ½
Epſom water	3	4	51
Dulwich water	2	4	54
Straton water	3	4	55
Barnet water	3	4	52
North-hall water	3	4	50
The German Spa-water	3	4	40
Tunbridge water	3	4	38
Iſlington water from the muſic houſe	3	4	36
Iſlington water from the vault with ſteps	3	4	39
Iſlington water from the cellar	3	4	39

BY this ſhort account it may appear, that, as divers mineral waters (that contain ſalts in them) are conſiderably heavier, than common water, ſo ſome, eſpecially ferruginous waters, are impregnated with ſo fine a ſubſtance, as to be lighter than common water.

Notes on the fourth title.

IV. THIS article may, in divers caſes, give ſome light to the diſcovery of the kind of ſoil, through which the water has paſſed; and is alſo uſeful to diſtinguiſh the ſpontaneous reſidence, if I may ſo call it, that the liquor lets fall by meer ſtanding, from that, which they call the *caput mortuum*, that remains after the total evaporation of the water; by which means alſo the weight of this laſt reſidence may be more truly known. Beſides ſome other mineral waters, I found, that the German Spa waters, brought very well ſtopt to *London*, afforded, by long ſtanding, a pretty quantity of terreſtrial ſubſtance, that looked almoſt like yellow oker, and perhaps was of great affinity to it in nature. 3. That clauſe in the article, ***though the liquor be kept from the air***, was therefore ſet down, becauſe I had found by trials, that ſome liquors, by being expoſed to the free air, would have copious, and ſometimes ſurprizing ſubſtances, ſeparated from them; as if the air contained ſome precipitating ſalts fit to work on the liquors, ſo as to make in them ſuch notable ſeparations.

Notes on the fifth title.

V. AN accidental weakneſs I had in my eyes, when I had the beſt opportunity to endeavour ſatisfying myſelf about this inquiry, forced me to leave the proſecution of it to others. Only two things I ſhall take notice of on this occaſion: one is, that having cauſed one, that had young eyes, and was accuſtomed to make uſe of ſuch microſcopes, as are mentioned in the article, to look upon ſome mineral waters through them, he ſaid he could diſcern no difference between them and common water: notwithſtanding which, the trial ought to be repeated by various perſons, on ſeveral waters, with differing engyſcopes, and in differing lights, and other circumſtances. The other is, that whereas it is by divers learned men objected againſt the goodneſs of theſe magnifying glaſſes we now make uſe of to look on liquors, that the little bodies, that the ingenious Mr. *Lewenhoeck*, and, ſince him, divers other virtuoſi have obſerved in water, wherein pepper has been infuſed, are not, as he pretends, living creatures, but little inanimate concretions, that are caſually formed, and carried to and fro in the liquor; to convince theſe doubters, of whoſe number I was myſelf at firſt inclined to be, I deviſed the following experiment: having laid upon the magnifying glaſs a part of a drop of water, wherein I could ſee ſtore of theſe little animals friſking up and down, we put to the liquor, with a briſtle or ſome ſuch very ſlender thing, part of a drop of ſpirit of ſalt, which, as was expected, preſently killed theſe little tender creatures, and, depriving them of their animal motion, left them to be carried ſo ſlowly to and fro in the liquor, as to make it viſible, that they were then dead, and had been before alive.

Notes on the ſeventh title.

VII. 1. THE odours of divers mineral waters are beſt judged of at the ſpring-head or other receptacle, whence ſome of them being removed ſcarce afford any odour at all (perceptible by us men.)

2. PERHAPS the ſulphureous ſcent, that is ſometimes perceived in *Tunbridge* and ſome other waters in their ſources, may in part proceed from looſe exhalations, that caſually happen to be mingled with the waters, but do not conſtantly belong to it.

3. THE winy odour is mentioned among others; becauſe I am credibly informed, that in *France* there is a mineral ſpring, if not more or leſs than one, that has ſuch a ſmell.

4. I MENTION the bituminous odour diſtinctly from the ſulphureous, becauſe men are too apt to confound them, and take all ſtinking mineral waters for ſulphureous, whereas divers are manifeſtly bituminous; as may be gathered, to omit other ſigns, not only from their proper odours, but from more or fewer drops of petroleum, or a kind of coarſe naphtha, that are found ſwimming upon the water.

5. I THINK it alſo not unlikely, that ſometimes a ſpring may partake both of ſulphur and bitumen, mingled together by the ſubterraneal heat, ſince I have found, that I could eaſily melt and incorporate theſe two ſubſtances here above ground.

Notes on the ninth title.

IX. 1. THIS is an almoſt neceſſary article, becauſe many perſons, that drink mineral waters, cannot well, either for want of ſtrength or conveniency repair immediately to the ſpring-head, but are obliged to drink them in their beds or their lodgings, and

and perhaps to have them transported to a great distance, or even to another country. 2. Many purging waters are found to retain their laxative virtue, and that perhaps for a considerable time, though they be transported to places distant from those they rise in. 3. In such ferruginous waters, as are lighter than common water, I found a manifest difference in reference to transportation; for most of them, even such as will bear removing, have something of freshness and quickness at the spring head, (perhaps from some spirituous and fugitive exhalations, that there arise with them, but presently vanish,) that they have not any where else; and some do not only lose this briskness by being removed, though in vessels well stopt, but they lose also the power of producing, with the powder of galls, a purple colour, as I found by trial purposely made in more than one of these mineral waters; which, to prevent fraud, I sent for to the springs themselves by servants of my own: for though these carried their glass bottles along with them, and had no other errand there but to fill and stop them carefully, yet, by being transported less than one league, I found them so altered, that they would no longer make a purplish colour with powdered galls, but a deep reddish one; whereas the German Spa-waters did almost always here in *London* afford me, with the same powder of galls, a rich purple colour; and Tunbridge waters afforded me the like, but not so deep a one, when I received them at *London* very well stopt. 4. This last clause was not to be omitted, because the exact or negligent closing of the vessels, wherein such waters are transported, is a circumstance of great moment. For more than once I received at *London* waters sent me from *Tunbridge* by physicians themselves, (who used at least a moderate care in putting them up,) which yet would by no means afford with galls a purplish colour. And I found, that even the German Spa-water would almost presently lose its capacity of being made purple by galls, if it were considerably heated. 5. But the same Spa-water being, in summer time, kept all night in an open vessel, did the next morning, till it was late, if not till noon, retain a disposition to be made purple by the admixture of galls; but that disposition it lost before the next day.

Notes on the fifteenth title.

XV. 1. BECAUSE it often happens, that men have not the leisure and the conveniency totally to evaporate the proposed mineral water, it may be an useful thing to be able, without evaporation, to discover, whether it contain any common salt; and, if it do, to make some estimate, how copiously or sparingly the liquor is impregnated with it. This might easily be done with nicety enough, if I were not by very just reasons restrained, for a while, from communicating that way of examining the saltness and freshness of waters; of which, I did, by the king's command, shew his majesty some proofs, whereof mention was presently after made in the printed Gazettes. But till it be free for me to impart that way to the public, I shall only intimate, that some guess may be made at the saltness of waters, by observing, whether they will lather with wash-balls or soap, and, if they will not, what quantity of curdled matter they will produce; as also, whether the waters will serve for washing of linnen, and will boil peas tender? Which two are the most usual ways, that many seamen take to examine the goodness of unknown waters by. In divers purging waters this way may be difficult to be practised with certainty, because of other salts, that may be predominant in them; but in the examen of lightly ferruginous springs it may be more relied upon. 2. It may not be unworthy observation, that, when I made use of my own way of examining the saltness of mineral springs, I did not find even the lightest sort of them devoid of common salt; which I found, but not in equal proportions, to be contained not only in the several waters of Islington, Hampstead-water, and, if I misremember not, in some others, but also particularly in Tunbridge-waters, and those of the German

man Spa; which I did not much wonder at, becauſe I had long known, that more or leſs of common ſalt is very uſually harboured, though not obſerved, in many ſoils, through which all ſorts of ſprings, and conſequently mineral ones, have their courſe.

Notes on the ſixteenth title.

XVI. THOUGH acidity be ſo uſually a manifeſt quality of mineral waters, that authors are wont to divide them into *acidulæ* and *thermæ*; yet I have found by ſeveral trials, that it is not near ſo eaſy, as men preſume, to find a manifeſt acidity in all mineral waters, that are not ſulphureous or hot. For ſeveral ferruginous waters having probably ſpent the acidity they had upon the iron ore, which they diſſolved in their paſſage, retain ſo little acidity, that it is hard to diſcover they have any, either by their working upon coral, or by any conflict with ſpirit of urine, or the like, or by mixing them with ſyrup of violets, to change the colour of it; inſomuch, that ſometimes I ſhould have concluded ſome ſuch waters to have no acidity at all, if I had not had a way of diſcovering a far leſs degree of it, than I could diſcern it to have by other trials. The circumſtances, that made this way of examining ſo critical, will coſt me too many words to ſet down here, and I have done it in another paper expreſly written of the way of diſcovering the qualities of divers bodies, by changes of colour made in or with them: and therefore I ſhall here but briefly tell you, that I diſcover the acidity of liquors by their operation upon the colours of an infuſion of lignum nephriticum made in limpid water, (and ordered after a certain manner.) By this means I found the German Spa to retain a little acidity, even here at *London*; but more than one of our own ferruginous ſprings did not, even upon this trial, appear to have any: and (which ſome may think ſtrange) I did not find even ſome of the purging ſprings, particularly that of *Acton*, to have any diſcernible acidity.

Notes on the twentieth title.

XX. THE ſcope of this inquiry was twofold: the firſt, to diſcover, whether a change of texture would notably alter the qualities of the liquor, when the hermetical ſeal hindered the avolation of any ſaline, ferruginous, or ſpirituous parts: and the other was to ſee, whether ſuch an agitation by heat, as in the open air would, as I had found, deprive the Spa-water of the virtue of making a purple colour with galls, would cauſe any manifeſt ſeparation of parts in the liquor, and make any groſſer ſubſtance to precipitate or ſubſide. But though we did twice (not without difficulty) make the experiment with Spa-water, yet we made it without ſucceſs; for the firſt time the glaſs broke at the bottom, before the water we immerſed it in was near boiling hot. And though the other glaſs reſiſted longer, and endured a greater heat, yet in not very many minutes that alſo broke at the bottom; which diſappointment a faithful hiſtorian ought as little to conceal as better ſucceſſes. And I chuſe to leave this 20th article of inquiry in its place, among the reſt of the titles, becauſe poſſibly ſome other may be more happy, than I was, in endeavouring to anſwer it. And I hold it not amiſs, in drawing up platforms of natural hiſtory, to ſet down what queſtions we think fit to be propoſed to nature; becauſe we cannot be ſure, before endeavours for trial be uſed, whether the thing to be attempted be practically performable, or not.

Notes on the twenty-ſixth title.

XXVI. 1. DIVERS ways may be propounded to diſcover, which of the qualities, mentioned in this article, is predominant in the ſalt to be examined; but, I confeſs, I ſomewhat doubt, whether theſe ways of trial be ſo certain, as many will be forward to think them. 2. If acidity be gueſſed to be predominant in the ſalt propoſed, it will probably appear by ſuch ways as theſe: by the taſte, odour, or both: by working upon coral or crab's eyes finely powdered: by curdling of milk: by making ſyrup of violets reddiſh: by the power of deſtroying the blue colour of infuſion of lignum nephriticum:

nephriticum; by not being precipitable by potent acid liquors, as oil of vitriol, ſpirit of ſalt; and by being precipitable by oil of tartar *per deliquium*, as alſo by ſtrong ſpirit of urine, and other volatile alcalies, as they are called. But, as I was noting above, I doubt whether theſe proofs be abſolutely certain; for, if I miſtake not, I found ſome purging mineral waters, that would not give even ſo ſlight a proof of acidity, as to deſtroy the blueneſs of the nephritic tincture, which yet would curdle milk, and turn it to a kind of poſſet; and, on the contrary, I found, that ſome German Spa-waters would not curdle milk, and yet would readily deprive the newly mentioned tincture of its ceruleous colour; which yet I did not find, that ſome of our Engliſh ferruginous waters were, at leaſt when brought me to *London*, able to do. 3. The predominancy of an alcali, in the ſalt of a mineral water, may be probably diſcovered by ſuch ways as theſe: by the lixiviate taſte, ſmell, or both; the former of which may be obſerved in the true nitre of the antients, (which I have had brought me from *Ægypt*, and a neighbouring country, whoſe name I do not now remember:) by the turning of ſyrup of violets green: by the precipitation of ſolution of ſublimate made in ſpring-water: by an efferveſcence or conflict with ſome potent acid, as *aqua-fortis*, or well dephlegmed ſpirit of ſalt: by heightning the red tincture of logwood or Brazil, drawn with common water; to which may be added a nicer way or two, that I have elſewhere mentioned. But I propoſe theſe ways but as appearing rational, upon the ſcore of my having ſucceſsfully tried them with other ſaline bodies, that were alcaliſate: for as to thoſe mineral waters I have had occaſion to examine, I do not remember I have yet met with any, wherein an alcali was predominant. 4. But, perhaps, farther inquiry will diſcover to others here in *England*, what I have not yet met with; and I doubt not, but that there are, in divers places of the earth, ſalts of an alcaliſate nature. And I preſume, that if the Egyptians were any thing curious of ſuch things, they would find, among their ſprings or wells, divers waters impregnated with them: for I found by trials purpoſely made upon latron, as ſome knowing men call the true Egyptian nitre, preſented me by an inquiſitive ambaſſador, who came out of the Eaſt, that the native ſalt exhibited divers of the ſame phænomena, that other factitious alcalies do. And ſome ſalt, afforded by the famous waters of *Bourbon* in *France*, being brought me thence, with a deſire, that I would examine it, I found it to be evidently alcaliſate; inſomuch that it would make a conflict with acids, and preſently turn ſyrup of violets green. 5. If we ſuſpect vitriol to be much predominant in the ſaline part of a mineral water, we may endeavour to diſcover it by ſuch ways as theſe. By its blackning a ſolution of galls: by its vomitive operation upon the drinkers, though this may ſometimes be an uncertain way, eſpecially becauſe an inviſible permixture of arſenic, or perhaps arſenical fumes, may give the water they impregnate an emetic quality: by putting alcalies to a ſtrong ſolution of the ſuppoſed vitriol, and obſerving, whether it will afford a yellow or yellowiſh precipitate, if ſalt of tartar or ſpirit of urine be dropt into it. By taking notice, whether a ſulphureous ſpirit, eſpecially ſuch an one as I formerly told I had made, though not here deſcribed, will make a blackiſh or a very dark colour with it, as I firſt gueſſed, and then found it would do with ſeveral vitriolate liquors, and even with one, to make which we had diſſolved but one grain of a natural vitriolate ſubſtance in above four or five thouſand times its weight of ſyrup or water. But in the parts about *London* I remember not, that in any of the waters I have made trials on, I have found vitriol to be predominant, or to be ſo much as a manifeſt ingredient; which ſeemed to me the more remarkable, becauſe ſeveral parts about this city are not deſtitute of marcaſites, the parents or wombs of vitriol. Since the writing of theſe papers, being caſually viſited by a diſcerning ſtranger, who had a particular occaſion to take notice of the reſidences of many of the mineral waters of *France*, his native country; he anſwered me, that he never met with any, that was manifeſtly vitriolate; and he ſeemed to be of opinion, that no vitriolate ſpring had yet been diſcovered among the many mineral ones, that are known to be in that country. 7. Since we ſo rarely meet with either manifeſtly acid or evidently alcaliſate ſalts in our Engliſh mineral waters, it may deſerve a ſerious inquiry, what other ſalts they may be impregnated with; and eſpecially from what ſalts the purgative virtue, that is found to belong to many of them, as *Epſom*, *Barnet*, *Acton*, *&c.* does proceed? Common ſalt indeed, as is already noted, I have found tokens of in the German Spa-water, and in all the Engliſh mineral waters I had occaſion to try, not one that I remember excepted. But I did not find, that common ſalt was ſo copious in any of them, as to diſcloſe itſelf by cryſtallizing in cubical grains. And the way I made uſe of to examine the ſaltneſs of the water without cryſtallization, is not equally certain in all ſorts of them. And becauſe I had not ſtore enough of theſe liquors, to evaporate them in large quantities, though I could not diſcern in the clear ſalts they afforded, either vitriol, or ſaltpetre, or allum, or even common ſalt, by their peculiar and genuine figures; yet I dare not confidently ſay, that none of our Engliſh mineral ſprings abounds with any of thoſe ſalts. But as far as I can gueſs, by the trials, that I have hitherto had opportunity to make, I am apt to think, that the ſalt, that is found in our purgative waters, and in ſome of them copiouſly enough, does not belong to any one known ſort of ſalts, but is either of a ſort, for which, as for many other minerals, we have yet no name; or, which ſeems more probable, is a ſalt of a compounded nature, made up by the coalitions of ſome or all of the ſalts above-mentioned, and perhaps of ſome other, as yet nameleſs, ſubterraneal ſalt, that the ſpring diſſolves in its paſſage. That two bodies, which are neither of them cathartic, may, by a change of texture wrought in one another, compoſe a third body, that is briſkly purgative, I have ſhewn in another paper. Beſides, having formerly had occaſion, in order to the reſolution of a certain doubt I had entertained, to burn ſalt of tartar with about a double weight of common ſulphur; I thence obtained, as I expected, a neutral ſalt, that had peculiar qualities differing from thoſe of the bodies employed to make it up: and talking of this ſalt with an ingenious empirick, he told me it had a quality I had not mentioned, and that a very uſeful one; ſince in the doſe of half a dram, or in ſome bodies, being taken in wine or broth, it would conſiderably, and yet gently, and without gripings, purge. And without the help of ſalt of tartar I have ſometimes made out of common ſulphur, a chryſtalline ſalt of a ſomewhat vitriolate taſte, the like to which might poſſibly be made under ground, where there are ſubterraneal fires, though perhaps not obſerved nor ſuſpected, ſince we made this ſalt without adding any thing to the ſulphur, only by the help of fire and common water. And I remember, that a great virtuoſo, ſeveral years ago, brought me, in order to an examen he deſired I ſhould make of it, a certain ſalt afforded by a ſpring in or near his land, which I remember was in the weſt of *England*, though I have forgot the name of the county; which ſalt nobody knew what to make of; but I quickly told him I took it to be of the nature of the *ſal mirabile Glauberi*, and predicted, that in ſuch trials it would afford ſuch and ſuch phænomena, which accordingly came to paſs. And I thought, that, if opportunity had not been wanting, this ſalt would have appeared purgative, as ſome factitious ſalts, that reſemble it in tranſparency, colourleſneſs, and figure, have been obſerved to be.

Notes on the twenty-ſeventh title.

1. IT is ſurpriſing to obſerve, how great an inequality one may ſometimes meet with in the proportion, that the ſame quantity of two differing mineral waters bear to the *caput mortuum* they reſpectively afford: for a pound, for inſtance, of one may, after evaporation, leave behind it perhaps more drams of dry ſubſtance, than a pound of the

the other will leave behind it grains. But becaufe I have no notes of the confiderableft inftances of this kind, that came to my knowledge, I fhall add only by and by the product of a more recent trial. 2. As far as I have hitherto obferved, thofe ferruginous waters, that are not heavier than common water, and in moft drinkers prove but diuretick, afford but very little *caput mortuum*, or dry fubftance upon the total evaporation of the liquor; whereas mineral waters, that are purging, and manifeftly more ponderous in fpecie than common water, leave, upon evaporation, a confiderable quantity of refidence, though fome far lefs than others. 3. At once to explain, and partly prove, what I have been faying, I fhall here recite, that from a pound of *Barnet* water (which is known to be purgative) flowly evaporated, we obtained a dram of white powder: but from the like quantity of *Tunbridge* water, we obtained but about one grain of *caput mortuum*; and, if I mifremember not, we had but about a grain and a half from 25 ounces of the German Spa-water. 4. It may feem fcarce credible to many, that fo fmall a quantity of matter, of which perhaps not one half is faline, or metalline, (the reft being terreftrial) fhould impart a manifeft virtue to fo great a proportion of water. But this difficulty did not much trouble me, who have purpofely made divers experiments, to difcover, how fmall a proportion of mineral matter may fuffice, when diffolved, to impregnate common water. I remember I took one grain of iron ftone, cafually found near the fprings at *Iflington*, (from which mineral it is probable thofe waters derive their virtue;) this being opened by the fire, and diffolved as far as it would be in a little fpirit of falt, we let fall a drop or two of the yellowifh folution into a great proportion of infufion of galls, to which it prefently gave a deeper colour than *Tunbridge* water, or even the German Spa water, was wont to give here at *London* with the powder of galls: fo that we gueffed, that, if we had then had at hand a competent quantity of the infufion, the remaining part of the martial folution would have been able to colour ten times a greater quantity of the infufion, than our trial was made upon. This will be eafily believed by him, that fhall confider an experiment we afterwards made to the fame purpofe, which was this: we diffolved a half grain of a good marcafite, taken up not far from *London*, in a fmall quantity of fpirit of nitre, (which for a certain reafon I made choice of, though other acid menftruums, as aqua-fortis, and fpirit of falt, would have diffolved the mineral;) this fmall folution we put into a pound of pretty high tincture of galls, made by infufing them in common water, and finding, as we expected, that this mixture grew very dark, we filled a vial with it, and emptying that vial into a larger glafs, we filled the fame vial three times with common water to dilute it; notwithftanding which, this new mixture, being put into one of our ufual glaffes, appeared of a colour much deeper than that, which the water of *Tunbridge*, or the German Spa, had formerly given with the powder of galls: fo that probably if another vial of common water had been added, it would yet have afforded a purple colour, if not a deeper; fo that one part of diffolved marcafite communicated a tincture to (61440) fixty one thoufand four hundred and forty parts of infufion of galls. And that, which makes this experiment more confiderable is, that this fmall quantity of marcafite was not itfelf all martial or metalline: for from our *Englifh* marcafites, as well as others, I have obtained a pretty quantity of fulphur, like common fulphur; befides that they afford a not defpicable quantity of terreftrial fubftance, about whofe nature I have not yet fatisfied myfelf. 5. I fhall now add this reflection, that fince the marcafite impregnated fo much water with its corporeal parts, if I may fo call them, obtained by bare diffolution, it feems highly probable, that the fame quantity of liquor may be impregnated by a far lefs quantity of mineral matter, attenuated into a kind of fpirituous ftate by being raifed in the form of fumes or exhalations; and that imperfect or embryonated iron may be fo, will fcarce be denied by them, that confider the way, that I have, in another paper, delivered to make iron manifeftly emit copious fumes, without the help of external fire. And if it be with fome fuch fpirituous and volatile exhalations, that a mineral water, as that of *Tunbridge* or of *Iflington*, is impregnated, it is not hard to conceive, that they may eafily lofe their chief virtue, by the avolation of moft or many of their fugitive parts, upon their being removed to a diftance from the fpring head. And to make it probable, that vitriolate corpufcles may be made to afcend, without lofing their nature, I fhall here mention an experiment, that I devifed to give fome light in this matter. I had often found by trial, that a fpirit richly impregnated with volatilized fulphur would with vitriol, whether in the form of a powder or a folution, produce in a trice a very dark or blackifh colour: and gueffing, that in mercury, turned by the addition of falt and vitriol into corrofive fublimate, many of the vitriolate corpufcles might afcend with the mercurial ones, I took fuch a volatile fulphureous tincture, as I have been mentioning, (which for this purpofe ought to be deep) and having dropped it upon good fublimate, I found it turn prefently of a very opacous colour. To fhew alfo, that, to make a great dilatation or difperfion of the martial corpufcles of an ore or mineral, there needs no fpirit of falt, or the like diftilled menftruum, I procured from a copperas-work, (or place where vitriol is made by art) fome of the liquor they employ before they caft in iron, that being corroded by it, it may increafe the weight, and give folidity and fome other qualities to the defigned vitriol. Now though this liquor be made, without any chymical menftruum, barely by rain or fnow-water, that impregnates itfelf with faline or metalline particles in its paffage through beds of marcafites, that lye expofed to the fun and air; yet in this water fuch numbers of martial corpufcles are difperfed, that having fhaken four drops of it into 12 ounces and a half of common water, this liquor, as I expected, was thereby fo impregnated, that with powder of galls it prefently produced as deep a colour, as good *Tunbridge* water would have done. So that, fuppofing a drop of this liquor to weigh about a grain, (as by fome trials purpofely made, we found it to do) it appears, that one part of the vitriolate water was able manifeftly to impregnate 1500 parts of common water. And yet of thefe four drops or grains of vitriolate liquor a confiderable part may very probably be concluded, from the way of its production, to have been rain water; as will eafily be granted, when I fhall have added, that, to examine this fuppofition or conjecture, we flowly evaporated fome ounces of the vitriolate liquor, and found, that the remaining dry fubftance did not fully amount to the fourth part of the weight of the whole, at which rate it was eafy to conclude, that one grain of vitriolate fubftance would have been found capable of fo impregnating fix thoufand times its weight of common water, as to make it fit to produce, with galls, a purple tincture. We afterwards found, upon trial purpofely and warily made, that the experiment will hold, though the proportion of the water to the grain of tinging fubftance fhould exceed that lately mentioned, by the weight of fome hundreds of grains.

TITLES for the natural hiftory of a mineral water propofed; confidered as a medicine, (being the third part of the defigned work.)

SECT. VI.

THOUGH the effects of a mineral water upon human bodies, as well as upon other fubjects, may challenge a place in the natural hiftory of it; yet becaufe the titles of this third part of this fcheme, for the moft part, directly regard the cure or prevention of difeafes, which are held to be the proper offices of phyficians as fuch, I forbore to make any comments upon the particular titles of this part of our hiſtorical idea, contenting myſelf, for the ſake of thoſe, that are ſtrangers to platforms of natural hiſtory, to have ſet down a ſeries of titles, which may point out to them, what particulars may be fit for their inquiry, and furniſh them with heads, whereto they may refer, and receptacles, wherein they may lodge what, upon trials or otherwiſe, they ſhall meet with worthy of obſervation. And ſo the accounts, that ſhall be given on theſe ſubjects, may be ſomewhat more diſtinct, and leſs incomplete.

To what temperaments and conſtitutions the mineral water propoſed is the moſt proper, to what leſs proper, and to what noxious or inconvenient?

In what ſtated diſeaſes, and in what particular caſes, the mineral water is proper, or to be ſuſpected of being dangerous, if not certainly hurtful?

What difference there is, if any, between the water taken up and preſently drank at the ſpring itſelf, or other receptacle, and that, which is carried to ſome diſtance off, whether in open, or in well ſtopped veſſels?

Of the manifeſt operations of the water in thoſe, that take it, whether it be by vomit, by ſiege, by urine, by ſeveral, or by two, or all of theſe ways.

Whether any occult virtues, or other hidden qualities, can be diſcovered in the mineral water? and, if any, what?

What variation in the effects of the mineral water proceeds from its being drank all of it quite cold, or hot, or lukewarm; or one part, when it is in one of thoſe tempers, and the reſt, when in another.

Of promoting or facilitating the operation of the water, in ſome by taking it in bed, and in others by moderate exerciſe.

What aſſiſtance may be given to the operation of the water, by giving with it, eſpecially in the firſt draught, ſomething to make it paſs the better, or to correct its crudity, or to ſtrengthen the ſtomach and bowels?

What advantages may accrue from preparing the patient's body before he enters upon his courſe of drinking the waters? and what inconveniences may attend the neglect of ſuch preparation, eſpecially in groſs, foul, or much obſtructed bodies?

Of the aſſiſtance the water may receive by gently purging medicines, diſcreetly given from time to time.

Of the beſt doſe, or quantity of the water, to be taken at once; of the compaſs of time, wherein it ſhould be all drank; and of the gradual increaſing and leſſening the doſe at the beginning, and ſometimes before the end, of the whole ſpace of time appointed for the taking it.

How much the greater or leſſer length of time ſpent in taking the water conduces to its good effects? and what is the fitteſt meaſure of time to continue the drinking of it, reſpect being had to the patient's ſtrength, diſeaſe, the time of the year, the accidental temperature of the air, and other conſiderable circumſtances?

Whether the drinking of the mineral water, for ſeveral years together, be found almoſt neceſſary, or more beneficial, than to intermit it ſometimes for a year or two, or perhaps longer, and then to return to the uſe of it?

Of the diet, as to meat, drink, exerciſe, ſleep, *&c.* that ought to be obſerved by thoſe, that take the water; and of the inconveniences that are wont to follow the neglect of it.

Of the ſigns, that declare the water to work kindly and effectually, and of the tokens of not doing ſo, and thoſe of its being already hurtful or likely to prove ſo.

Of the inconveniences or unwelcome accidents (if there be any, as uſually there is) that have been obſerved to happen, during, or ſome time after, the drinking of the

mineral

mineral water, especially to persons of such constitutions, or that are in such and such circumstances, and of the ways to prevent or remedy such inconveniences.

WHETHER there be any necessity, or great use, of taking physick, after one has done drinking the water? and if there be, what are the fittest times and medicines to be employed for the prevention of any bad effects of it, and what is the danger of neglect to make use of them?

WHETHER, and how, the mineral water may be usefully given by being simply commixed with other liquors or bodies, as by boiling meat in it; or by receiving, together with the additament, a further preparation, as when the water is mingled with wine, or some other drink; when with milk it is made into posset drink; when brewed with malt alone, or with that and hops, it is turned into ale or beer?

WHETHER any such saline, or other substance may by evaporation, inspissation, calcination, *&c.* be extracted or obtained from the mineral water, as, being given in a small dose, may be substituted as a *succedaneum* to large quantities of the water as nature affords it?

OF what uses (if of any) the mineral water is, when outwardly applied, as by washing sore eyes or ulcers, bathing in it, *&c.* and whether the mud, or sediment it leaves, where it passes or stagnates, being externally applied, have the same or other medicinal virtues; and, if so, how the mud is to be administred to make it exert them.

OF some mechanico-medical trials, that may be made upon animals, to help us to guess at the qualities of the mineral waters; as by injecting it into the veins of a dog, to try, whether it will coagulate his blood, or make it more fluid, or operate powerfully by vomit, siege or urine; as also by keeping a dog very long, without allowing him any other drink at all than the mineral water.

BUT I propose such particulars, as mentioned in this article, but as analogous experiments, or *succedaneums* to trials, that should, but cannot well, because of the worthiness of the subject, be tried in living human bodies. And indeed all the titles of this third part of our designed history belong properly to physicians; many of whom (at least if they resemble you) are far better qualified to cultivate this medicinal subject, than I, who being as little desirous, as fit, to incroach upon their province, shall not inlarge upon this third member of our history, but willingly resign it into their, and especially into your own more skilful hands.

The CONCLUSION.

AND now, Sir, it may be seasonable to put an end, at least for the present, to this rhapsody of papers, by telling you, that the foregoing idea, or platform of a history of mineral waters, being a draught of, or a first essay upon, so difficult and uncultivated a subject, as I have ventured to treat of; as I know you are too judicious to expect any thing of exactness and compleatness, in what I now present you, so I hope you will be so equitable, or so favourable, a reader, as to forgive those omissions and other imperfections, that I cannot doubt but you, (and even I myself upon a review) shall discover in the first edition of the foregoing papers. And though, if hereafter they shall be thought worthy of a second, I may possibly be able, if GOD will be pleased to grant me health and leisure, to rectify some oversights, and supply some omissions; yet, to deal freely with you, I much fear, that it will be very difficult for far skilfuller pens, than mine, to deliver such histories of mineral waters, as the curious would wish, and those criticks, that have never made trial of the difficulty of attempts of this nature, will be forward to require. And this difficulty will, I presume, be found a great one, not only, (as I have already noted) by him, that shall undertake

take to give a good account of mineral waters *à priori*, but to him alſo, that ſhall take in all the help he can obtain *à poſteriori:* for there are ſo many circumſtances of ſeaſons, weather, place, and a multitude of contingencies, that may vary the phænomena and effects of mineral waters, that it is extremely difficult, either to comprize ſo many different things at once, and, as it were, ſurvey them at one view, or without having ſuch a comprehenſion and multitude of various regards, to be able to pronounce with certainty about the nature, the medicinal operations, and the other effects of a ſubject, that may be influenced and diverſified by ſo many cauſes and accidents, as a mineral water may. And therefore, 'till further diſquiſitions and trials ſhall have better cleared up the ſubject, I ſhall, without pretending to more, think the paſt diſcourſe not altogether uſeleſs, if it can well perform the office of the *virgula divinatoria*; which, (ſuppoſing the truth of what many chymiſts and metalliſts deliver,) of how little value ſoever it be of itſelf, is fit to point at mineral treaſures, and ſhew men the places, where they are to ſeek for them. *Farewell.*

This belongeth to the 16th TITLE of the firſt part.

IT is known, that the drinking of ferruginous waters, ſuch as thoſe of the German Spa and our *Tunbridge*, is uſually preſcribed for many weeks, during which time it often enough happens, that the fall of rains makes men doubt, whether the mineral water be not ſo much diluted, as to be ſpoiled in its medicinal capacity. And indeed I have more than once obſerved, that ſome ſuch martial waters, after conſiderable rains, loſt their power of producing the wonted colour with galls. And therefore it may in ſome caſes be of good uſe to be aſſiſted to conjecture, whether or no the rain have made the mineral water unfit for drinking? In order to this I ſhall take notice, that uſually a ſmall rain does little or no harm to the medicinal ſpring; and ſometimes even a moderate rain, eſpecially after a long drought, may, inſtead of weakning it, increaſe its virtue, by waſhing down into its channel ſome ſalts, that, during the dry weather, were concreted in the pores of the ground, and perhaps alſo by heightning the water in the channel, ſo as to diſſolve ſome ſalts concreted there, which it could not reach before. But if the rain have long continued, the eſtimate may beſt be made, partly by the greater or leſſer depth of the ſpring beneath the ſurface of the ground, and partly, and indeed chiefly, by the peculiar nature or ſtrength of the mineral water; for ſome ſprings are much more copiouſly impregnated than others, and therefore will bear a greater dilution by rain-water, of which I ſhall give you this notable inſtance; that whereas I found (as I lately noted) that more than one of our Engliſh martial ſprings, eſpecially thoſe near *London*, were too much weakned by the water, that rained into them, I had the curioſity to try, how much of that kind of liquor ſome German Spa-water, that came to me to *London* very well conditioned, would bear; in purſuit of which deſign I warily made ſome trials, which ſhewed, (what probably will be thought ſtrange,) that when the mineral water was diluted with no leſs than thrice its weight of rain-water, it yet retained ſtrength enough to produce with newly powdered galls a purpliſh colour.

END of the FOURTH VOLUME.