AN
EXAMEN
OF
Mr. *T. HOBBES* his
Dialogus Physicus
De Naturâ Aëris.

As far as it concerns Mr. *R. BOYLE*'s Book of New Experiments touching the Spring of the Air, &c.

With an APPENDIX touching Mr. *Hobbes*'s Doctrine of Fluidity and Firmness.

By the Author of those Experiments.

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Diagnostic Practices

EXHIBITED

The Preface.

HAving, ſoon after I had begun the following *Examen* of Mr. *Hobbs*'s Dialogue, been diverted for a good while by divers urgent Avocations from purſuing it; I was in the mean time informed by learned men (ſome of whom keep great Correſpondences with the *Virtuoſi* abroad) that my publiſhing any thing againſt his Objections would not be neceſſary, nor was much expected: Whereupon I ſhould perhaps have declined reſuming an Employment, that to a perſon of my humour could not be delightful; but that beſides thoſe Inducements mentioned at the beginning of the following Treatiſe, it came into my mind that my Adverſary, not content to fall upon the Explications of my Experiments, has (by an Attempt, for ought I know, unexampled) endeavoured to diſparage unobvious Experiments themſelves, and to diſcourage others from making them. Which if he could by his Dialogue effect, I dare be bold to ſay, he would far more prejudice Philoſophy by this one Tract, then He (and that it may not ſeem ſaid to undervalue him, I ſhall adde, or any Man elſe) can promote it by all his other Writings. Wherefore, though his diſparaging of Experiments would probably have much more Authority (eſpe-

cially with considering men) if he had been the Author of considerable ones, or did appear to be more then ordinarily skilled in them: yet left for all this his Fame and Confident way of writing might prejudice Experimental Philosophy in the minds of those who are yet strangers to it, I thought it not amiss, both to go on with the Discourse I had begun, and to enlarge it beyond what I first designed; and accordingly, to the intended Vindication of the main points of our Doctrine, *The Weight and Spring of the Air*, which (if I mistake not) we have firmly established, we have added an *Examen*, that otherwise we should scarce have made, of the greatest part of the Physiological passages in Mr. *Hobbs*'s book, most of which I thought might be rationally *question'd*, and many of them clearly *disprov'd*. And in pursuance of this, though I did not perhaps alwayes think my self oblig'd to prosecute things further then the nature of my Design requir'd, or to forget that the Matters in dispute were not all of an equal weight; yet the Reflections I have employ'd will, I presume, be found sufficient to shew *both* that 'tis easie even for a great Wit frequently enough to mistake, and much more frequently to miss of clearly demonstrating what he pretends in matters Physical, for want of having sufficiently considered the Experiments he would be thought to despise; *and* that Mr. *Hobbs*'s Adversaries need not be much ashamed of the Name he is pleased to give them of *Experimentarian Philosophers*. It was also suggested to me, that the dangerous Opinions about some important, if not fundamental, Articles of Religion I had met with in his *Leviathan*, and some other of his Writings, having made but too great Impressions upon divers persons, (who, though said to be for the most part either of greater *Quality*, or of greater *Wit* then Learning, do yet divers of them deserve better Principles) these Errors being chiefly recommended by the Opinion they had of Mr. *Hobbs*'s demonstrative way of Philosophy; it might possibly prove some service to higher Truths then those in Controversie between him and me, to shew that in the Physicks themselves his Opinions,

Opinions, and even his Ratiocinations, have no ſuch great advantage over thoſe of ſome Orthodox Chriſtian Naturaliſts. But for all this, as little as I would grudge to write a much longer then the following Diſcourſe to do Religion the leaſt ſervice; yet thinking it fit to leave Controverſies of this kind to thoſe whom they more particularly concern, I ſhould ſcarce in the Introduction to a Diſpute about the Air have at all mentioned any thing of this nature, but that Mr. *Hobbs* in the Preface to his Dialogue is pleaſed (though I know not to what purpoſe in that place) to ſpeak without limitation or diſtinction (and conſequently unwarily enough at leaſt) of the things ſaid in the Books of Naturaliſts concerning immaterial ſubſtances, (and ſure ſome things *true*, (at leaſt *That there are ſuch Beings*) as well as ſome things *erroneous*, are there ſaid:) though he hath been by the learned Dr. *More* and others publickly accuſed to have taught, That 'tis abſurd to believe that there either are or can be any. Which yet methinks he ſhould not do, ſince elſewhere and in this very Dialogue he builds ſeveral things in his Philoſophy upon the Creation of the World, and an Infinite Power: And how a thing material can create matter and have an Infinite Power, I confeſs I do not underſtand.

— *Neq; hominis Philoſophi eſſe cenſeo corporum quorundam, ut ſolis & ſtellarum, mirabiles ſupponere magnitudines, contra vero mirabiles exiguitates non admittere, cum Virtutis ejuſdem infinitæ ſit utraq; creare, tam maxima quam minima.* Mr. *Hobbs* in his Dialogue of the Air, page 11. See alſo page 5. and elſewhere.

I doubt not but Criticall Readers will think I might have excepted againſt many more particulars in Mr. *Hobbs's* Book then I have Examin'd; and indeed about this I dare not contend with them. For beſides that I may through haſte and indiſpoſedneſſe to quarrel, have over-ſeen ſeverall things which an Eye either ſeverer or more attentive would have obſerv'd; I purpoſely paſt by divers things I did not altogether over-look; partly, becauſe I thought it needleſſe to queſtion them (having no want of other Objections) and partly, becauſe I could not do ſo in few words, and was loath to engage in needleſs

and

and long-winded Disputes: And perhaps I was too weary of my Employment to be willing to spend many words when I could safely spare them. And though others will possibly think it strange, that a Member of the Society he is so severe to should not take notice of such passages as these, *Nam conveniant* (sayes he, speaking of the *Virtuosi* that meet at *Gresham* Colledge) *Studia conferant, Experimenta faciant quantum volunt, nisi & principiis utantur meis, nihil proficient*; and again, *Næ illi quæ dicerent non videntur cogitasse, sed sortitos esse*; and elsewhere, *Conjicere hinc licet, quàm sint boni ratiocinatores, & quæ sit ab illis expectanda Philosophia Naturalis*; and (to trouble you no more) *Ad causas autem propter quas proficere ne paulum quidem potuistis nec poteritis, accedunt etiam alia, ut odium Hobbii, quia nimium liberè scripserat de Academiis veritatem: Nam ex eo tempore irati Physici & Mathematici veritatem ab eo venientem non recepturos se palam professi sunt:* Though, as I said, some may wonder I should silently pretermit such passages as these; yet besides what I elsewhere say by way of Account of my so doing, I shall here tell them, that I presume some sorts of Readers will more easily pardon me for neglecting such Expressions, then they will Mr. *Hobbs* for using them. And I confesse, I thought I should find it more easie to say nothing at all to such passages, then say any thing without saying somewhat that would offend a Person that could allow himself to say such things. Though I ignore not that divers Readers will much the lesse relish the following Discourse, for my having, perchance not altogether for want of knowing how to write otherwise, forborn to furnish it with quick and smart Expressions, which are wont to be employ'd in Disputes, to expose or depreciate an Adversaries Person or Cause, and which are usually not the least things that serve to amuse such Readers, and engage their attention. But I fear I have much lesse need to make Excuses for my Omissions, then for having in the following *Examen* been reduc'd by the Nature of my Task, to say so many things which Intelligent Readers need not be taught by

by me. And therefore ſuch ſhall have my conſent to skip, if they pleaſe, the whole Diſcourſe; which though I could ſcarce upon ſuch an Occaſion make a very Inſtructive one, yet if they will be pleas'd to forgive me its Barrenneſſe, I hope hereafter to avoid the like Temptations of writing again at the like rate.

And having ſaid thus much as to the *Reaſons* of my penning the following Diſcourſe, I muſt adde ſomething, though but little, touching the *Manner* of it: Wherein I hope I have not much, if at all, ſwerv'd from what I propos'd to my ſelf, namely, to give an Example of Diſputing in Print againſt a Provoking, though unprovoked, Adverſary, without Bitterneſs and Incivility, and without purſuing thoſe things which how much more ſoever they belong to the Perſon of an Antagoniſt then to his Cauſe, are wont to make up a great part (if not the greateſt) of Books divulg'd on ſuch Occaſions. But ſince I intend what I write for Intelligent and Ingenious Readers, I dare expect that my forbearing to inſiſt on ſuch things as I judg'd wholly extrinſecal to the Opinions and Arguments I examine will be aſcrib'd to the true Cauſe, That my Diſcourſe will not be thought to have the leſſe of Reaſon for having the leſs of Paſſion; and (eſpecially) That my ſilence as to thoſe things that are ſpoken to the Diſparagement of the Illuſtrious Company that meets at *Greſham* Colledge, will be look'd upon only as an effect of my judging it fit to leave them the full Liberty to right themſelves, if they think it worth while, by ſome better Pen then mine. And if Mr. *Hobbs* think fit to ſay any thing to the following Diſcourſe, it will not be amiſſe that his Reply be as inoffenſive as I have endevour'd to make my *Examen*. For having diſpatch'd as much as I think requiſite to ſay of this Controverſie my ſelf, and having other (and I hope better) Employments for my leaſure hours, if I can get any; I muſt leave the further Diſputes, if any ſhall ariſe, to be manag'd by others, who, if Mr. *Hobbs* refuſe to imitate my way of writing, will poſſibly make no ſcruple to imitate his,

and

and put him in mind of that Law of *Vespasian*, upon which himself would be thought to ground that heap of strange Titles he bestows upon the two Learned *Savilian* * Professors, *That it is unlawful to give ill Language first, but civil and lawfull to return it.* I have but one thing more to adde; which is, that I would not be so far mistaken, as to be upon the Account of what I have written against my two Adversaries, look'd upon as a Person wedded to his Opinions: For not having hitherto learn'd that either of their Books has yet made *Proselytes*, I presume it will not be wonder'd at that they have not made me one. And though the two Learned Authors I have answer'd, have given me no Cause to retract any of my Opinions; yet as 'tis not improbable that others reasoning upon better Principles may do what these have not done, so I am still of the same temper I was of when I us'd to propose my Thoughts but as Conjectures.

* So go your wayes (speaking to Doctor *J. Wallis* and Doctor *S. Ward*) you uncivil Ecclesiasticks, Inhumane Divines, Dedoctors of Morality, unasinous Collegues, Egregious pair of *Isachar's*, most wretched *Vindices* and *Indices Academiarum*; and remember *Vespasians* Law, &c. Mr. *Hobbs* Lesson 6. page 64.

AN

AN

EXAMEN

Of the greateſt Part of Mr. *Hobbs*'s *Dialogus Phyſicus De Natura Aëris.*

CHAP. I.

The Occaſion and Scope of the preſent Treatiſe.

MEeting the other day with a Treatiſe then newly publiſht by Mr. *Hobbs*, and intituled *Dialogus Phyſicus De Natura Aeris*; The Name of the Author, the Subject of the Book, and the Information I had a good while before received from his Friends that he was writing againſt me, invited me to peruſe it as a Diſcourſe wherein I might probably find my ſelf concern'd: nor was I deceived in my Expectation. For having curſorily paſs'd through it, I readily found, that though I be not expreſly nam'd there, and though ſome things in the Title-Page, and ſome others in the Book it ſelf, ſeem to make the chief Deſign of it to be the Diſparagement of the Society that is wont to meet at *Greſham* Colledge; yet the Ar-

guments are for the moſt part levelled at ſome Writings of mine, publiſhed ſome of them the year before, and ſome of them this laſt Spring; As the Experiments, whoſe Explications he is pleaſed to cenſure, do all along declare. I confeſſe I was ſomewhat ſurpriz'd to find that Mr. *Hobbs*, whom if my Books have at all mentioned, it has been with reſpect, ſhould fall upon a perſon that had not provoked him, whileſt ſuch Mathematicians as Dr. *Wallis*, Dr. *Ward*, *Tacquet*, and *Moranus* (Men much too famous to be deſpicable Adverſaries) having a good while ſince profeſſedly and unchalleng'd written againſt him, he hath yet, the whole Diſcourſes of ſome, and ſo great a part of the Objections of the others, to reply to. And it ſomewhat added to my wonder, that a Writer of Politicks ſhould cauſeleſly and needleſly, for ought I can learn, fall upon a Society, whereof, beſides many other Perſons of Quality and men of Parts, his own great Patron, and my highly Honour'd and Learned Friend, *The Earl of Devonſhire* himſelf, is an Illuſtrious Member. And as for me, I ſhal not ſcruple to confeſs, that I could have been well enough contented Mr. *Hobbs* had ſpared this Dialogue, partly becauſe I have a natural Indiſpos'dneſs to Contention, partly becauſe I am at preſent diſtracted by ſtore of other Employments both of a Publick and a Private nature, (and particularly by the publiſhing of three or four Books of differing Subjects, and Printed in ſeveral places) partly becauſe Mr. *Hobbs*'s Objections are of ſuch a Nature, that perhaps my Replies, though as ſhort as (my Deſign mention'd in the Preface conſidered) I can conveniently make them, will amount to a longer Diſcourſe then moſt Readers will think the Objections needed; and partly too, becauſe Mr. *Hobbs* is pleas'd to write of divers Worthy and Learned Men in ſo depreciating, and of himſelf in ſo differing, a way, that I fear I ſhall find it ſomewhat uneaſie to retain (under ſuch Provocations to decline it) the Civility I am wont, and am deſirous to write with; and that I muſt almoſt deſpair of diſſenting without an abſolute Rupture from a Perſon, whoſe way of Diſcourſing is ſuch, that though

I ſhall not give it any Epithete, yet I confeſs it leaves me but little hope that I can oppoſe him without angering him.

But however, becauſe if I can (as I intend to do) ſo far comply with my Inclinations and my Cuſtom, as to wave perſonall and extrinſick Matters, and reſtrain my ſelf to the *Examen* of the Argumentative part of his Diſcourſe; My Reply will not need to be prolix; and becauſe he has vouchſafed rather to ſingle out a young Writer, whoſe Books (at leaſt of Matters Philoſophical) do but begin to appear in the World, then to defend himſelf againſt thoſe Illuſtrious Enemies, upon whom he might expect to gain much more Honour; and becauſe Mr. *Hobbs*'s Name may with ſome Readers give his Arguments an Efficacy which their own Nature could not confer on them; I muſt reſolve to ſubmit to what he and my Concern for the Truths he rejects impoſe upon me. But to ſhorten as much as I can a Work to which I can allow but very little time, it will be expedient before I deſcend to the Examination of Particulars, to premiſe three or four Advertiſements touching the Occaſion and the Nature of the Controverſie, that I might not be reduc'd to a frequent and unwelcome Inculcation of the ſame things.

CHAP. II.

Of ſome miſtakes of Mr. Hobbs *touching matters of fact, and the Author's Doctrine.*

AND firſt, whereas Mr. *Hobbs* is pleas'd to write as if the Explications and Experiments to be met with in the *Phyſico-Mechanical* Treatiſe he cenſures, were thoſe of the whole Society at *Greſham* Colledge; I muſt do them that Right to declare, that this way of Proceeding is manifeſtly grounded upon a Miſtake. I will not affirm that the Miſtake was wilful, that Mr. *Hobbs* might give himſelf a pretence to Quarrel with them, (who have hitherto ſuſpended the Declaring themſelves as a Society)

in the controverted Points. But there are ſome that think Mr. *Hobbs* might very eaſily have avoided this Miſtake: ſince the Book he cenſures was publiſhed (and perhaps taken notice of by moſt of the *Virtuoſi* here) ſome Months before the Society was begun. And the Experiments themſelves had been long before the Book came forth, not only ſeen and diſcourſed of, by divers Learned Men and Illuſtrious Perſons, but had the Honour to have our great *Monarch* of the *Virtuoſi*, as well as of *Great Britain*, for a Spectator. And though poſſibly divers of the Learned Members of our Aſſembly may have no unfavourable opinion of what I have delivered in that Book; yet the Aſſembly, as ſuch, has been ſo far from Adopting or Owning my Opinions as theirs, that it has with Approbation been propos'd among them, to repeat the Experiments, and take a review of the Explications, that upon a ſtrict *Examen* of the ſeveral Opinions, and the Objections that could be brought in againſt them, they might ſee what Judgement will be fit to be paſt on them. And although there be very few Philoſophers whoſe Parts may make their Judgement more formidable to me; yet to comply with their Deſign, whatſoever the event might be, I preſented them the Engine it ſelf, I had made uſe of and deſcrib'd in my Book; chuſing rather to undergo their Cenſures, then want their Inſtructions. By which it may appear, upon how little ground Mr. *Hobbs* has thought fit to impute to the Society thoſe Opinions which (how Erroneous ſoever he is pleas'd to think them,) I muſt own to be mine. And this Juſtice I the rather do It, becauſe 'tis all that I am to do in this Treatiſe on their Behalf, not only for the Reaſons above intimated, but becauſe the Vindication of ſuch an Aſſembly againſt Mr. *Hobbs* deſerves a better Pen then mine, though it doth not need it.

Secondly, undertaking then the Defence of my own Cauſe, without Intereſſing them in my Quarrel, I muſt next admoniſh the Reader, that whereas Mr. *Hobbs* writes, as if the new Experiments were deviſed, or at leaſt employ'd, to prove a *Vacuum*; he is in this likewiſe miſtaken. For neither has the Society declared

clared either for or againſt a *Vacuum*, nor have I: Nay I have not only forborn to profeſs my ſelf a Vacuiſt, or a Pleniſt, but I have in a fit place of my Epiſtle expreſly ſaid, that I reſerv'd the declaring of my own Opinion touching that Point to another Diſcourſe (which as yet is not publiſhed.) Wherefore Mr. *Hobbs* either injures or miſtakes thoſe, whom he will needs make his Adverſaries, when he repreſents the new Experiments as Demonſtrations alledg'd by profeſs'd Vacuiſts to diſprove the Fulneſs of the World. And though I ſhall be oblig'd in the following Diſcourſe to reject Mr. *Hobbs*'s Suppoſition of a *Plenum*; yet I intend not thereby to declare whether or no I do abſolutely allow a *Vacuum*. But that which I drive at, and which alone my preſent Work exacts, is to ſhew that I may reaſonably oppoſe the *Hypotheſis* of a *Plenum*, as it is ſtated by Mr. *Hobbs*: and conſequently, unleſs he had better prov'd it, I may very well refuſe to let *Him* take it for demonſtrated. But I intend not to queſtion whether or no other Pleniſts may not have better Arguments then his Principles have ſuggeſted to him: nor to deny but that the *Carteſians*, may without granting a *Vacuum*, give a more plauſible Account (whether true or no) of divers of the *Phænomena* of our Engine, if they will add, as ſome of them of late have done the Spring of the Air to their *Hypotheſis*, That the Celeſtial Matter of which the Air does in great part conſiſt, is ſubtile enough freely to paſs through the Pores of the cloſeſt Bodies, and even Glaſs it ſelf.

As for the Aſſertion *Non dari vacuum*, though, as I ſaid, I need not in this place declare my ſelf either for or againſt it, yet I confeſs I do not find that Mr. *Hobbs*, though all along this Diſcourſe he argues from this Principle againſt thoſe he thinks Vacuiſts, has demonſtrated it. For in his Book *De Corpore* (though a main part of it depend upon the Plenitude of the World) He has that, I remember, but one poſitive Argument (indeed he thinks that unanſwerable) to evince it. And that is drawn from this Experiment: *That if a Gardeners Watering-Pot be fill'd with Water, the hole at the top being;*

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being ſtopt, the Water will not flow out at any of the holes in the bottom: But if the finger be removed to let in the Air above, it will run out at them all; and, as ſoon as the finger is applied to it again, the Water will ſuddenly and totally be ſtayed again from running out. The cauſe whereof (ſubjoyns he) *ſeems to be no other but this, that the Water cannot by its natural endeavour to deſcend drive down the Air below it, becauſe there is no place for it to go into; unleſſe either by thruſting away the next contiguous Air it proceed by continual endeavour to the hole at the top, where it may enter and ſucceed in the place of the Water that floweth out; or elſe by reſiſting the endeavour of the Water downwards penetrate the ſame, and paſs up through it.*

But this Experiment, as an obvious one, and without dreaming that Mr. *Hobbs* had laid ſuch ſtreſs upon it, I have incidentally anſwer'd in what I ſay in two or three paſſages on the thirty third Experiment of my Epiſtle. But after found that it had been more fully anſwer'd (but upon Grounds ſome of which I do not need) by my Learned Friend Dr. *Ward*, with whom I thus take Mr. *Hobbs* his Argument to pieces. The Cauſe, according to Mr. *Hobbs*, of the Suſpenſion of the Water in the Veſſel is, that the Water cannot thruſt away the Air. 2. And it cannot thruſt that away unleſs Air ſucceed in its place. 3. But Air cannot ſucceed in its place, unleſs either by getting in at the upper Orifice, or at the Holes that perforate the bottom. By which view of the Argument it appears that the main force of it lies in the ſecond Propoſition; but neither doth he demonſtrate that (which omiſſion might excuſe us from any further Anſwer) nor indeed do I think it true. For if the Watering-Pot were tall enough, what Reaſon is there, why the Water ſhould not run out at the Holes of it? as *Monſieur Paſchall*'s Experiment mentioned in my Epiſtle manifeſts; That though in a Glaſs-Tube Hermetically ſealed at one end, and ſeveral times as long as a Watering-Pot, the Water will not fall down; yet it will, if the Tube exceed two or three and thirty foot, or thereabouts. And indeed the Suſpenſion or Deſcent of the Water depends upon

upon the Proportion betwixt the weight of the Aqueous Pillar that tends downwards, and the Reſiſtance or Preſſure of the Air that can come to bear againſt it. For as on the one ſide, when the height of that Pillar is ſo increas'd, as that it can outweigh the *Atmoſpherical Cylinder* that oppoſes its Deſcent, 'twill flow out till thoſe two Cylinders come to an *Æquilibrium*: ſo on the other ſide if inſtead of increaſing the length of the Cylinder of the Water, you leſſen the preſſure and reſiſtence of the Air, the Water will likewiſe deſcend, though the Pillar be very ſhort, as I have ſhewn in the nineteenth Experiment; where, by withdrawing ſome of the Air in the Receiver, and thereby weakning its Spring, the Water in a Tube Hermetically ſeal'd at one end of but about four foot long ſubſided about three foot, though That the ſpace relinquiſh'd by the Water was not full of Air, as Mr. *Hobbs* his Argumentation requires it ſhould be, may be prov'd by what is there added, *That by letting in the outward Air when the Water was ſunk ſo low it was immediately impell'd up again to the higher parts of the Tube.*

In ſome Dialogues of Heat and Flame.

And indeed (as I elſewhere diſcourſe) it ſeems to me a difficult matter for thoſe that reject, as Mr. *Hobbs* juſtly does, that Conceit of Natures Abhorring a *Vacuum*, and making it, as it were, her Buſineſs to hinder it, to prove there can be no *Vacuum* at all by any particular Experiment. For if the Fulneſs of the World be not made neceſſary either by the Nature of Body in general, or by the Deſign of the Author of the Univerſe, it can ſcarce be eaſie to prove by a particular Experiment, that no Humane Force or Art can contrive a way of overcoming at leaſt for ſome time, and as to ſome ſpace, either the Gravity of fluid Bodies, or whatever other Quality of the Air or Water it is by which the Contiguity of the neighbouring Parts of the World is wont to be maintain'd. As we ſee the Water that will not deſcend even in a Tube of thirty foot, (and thereby has made men think it will never deſcend whileſt the Air is not permitted to ſucceed it,) may by our Engine be brought to ſubſide in a Tube of about a

foot

foot long. And I ſhall here add this out of my (yet unpubliſht) Dialogues *of Flame and Heat*; That whileſt only particular Experiments are brought to aſſert the Impoſſibility of a *Vacuum*, perhaps the Vacuiſts will have the Advantage on their ſide. For a thouſand Experiments are not of that force to prove univerſally that a thing cannot be effected, as one that ſhews it may be, is to prove the contrary. And the Vacuiſts have as well as the Pleniſts ſtore of Experiments on their ſide that ſeem to favour their *Hypotheſis*, according to which, were it true, I ſee not why they may not ſolve the Objections drawn from either the aſcenſion of Liquors upon Suction, or the non-deſcenſion of Liquors in Watering-Pots clos'd at the top, or from any of the like Experiments I have yet met with, in caſe the Weight and Spring of the Air be taken in to ſolve the *Phænomena*. And the Vacuiſts will have this Advantage, that if Mr. *Hobbs* ſhall ſay that it is as lawful for him to aſſume a *Plenum* as for others to aſſume a *Vacuum*; not only it may be anſwer'd, 'tis alſo as lawful for them to aſſume the contrary; and he but Barely Aſſuming, not Proving a *Plenum*, his Doctrine will ſtill remain queſtionable. But I think I could ſay more in favour of the Vacuiſts Experiments; namely, That whereas in ſome *Phænomena* of the *Torrecellian* Experiment, and in many of thoſe of our Engine, Mr. *Hobbs* proves the ſpace deſerted by the Quickſilver or the Air to have no Vacuity, becauſe according to his Suppoſition the World is full; and not by any ſenſible *Phænomena* that prove the Space in Queſtion to be perfectly full: (For no leſs Fulneſs is requiſite to the truth of his *Hypotheſis*:) The Vacuiſts on the other ſide need not go about to prove that thoſe Spaces are not full by their *Hypotheſis*. But they prove it by this, that it appears by ſenſible *Phænomena*, that the Quickſilver deſerts the upper part of the Tube; and that much Air is pump'd out of our Receiver. (The firſt of which is evident to the Eye; and ſo is the other too, when the Pump is kept under Water.) But it does not appear by the like *Phænomena*, that the Air (as Mr. *Hobbs* would have it in his Elements) does Succeed to fill, I ſay, perfectly

perfectly to fill the deserted space; which also they will confirm from hence, that in the *Torrecellian* Experiment by inclining the Tube the relinquish'd space may be again readily fill'd with *Mercury*; and if our exhausted Receiver be plung'd under Water, that Liquor, when access is given it to the Cavity, violently rushes into it, and almost fills it up.

From all which it seems probably deducible, That 'tis a very hard thing, by Mr. *Hobbs*'s way of managing the Controversie, to prove that there can be no *Vacuum*. But as for the *Cartesian*'s more subtile and plausible way of asserting a *Plenum*, it concerns me not here to Dispute against it, or Declare for it.

I will add this, and but this, on the occasion of Mr. *Hobbs*'s Building a great part of his Philosophy upon no surer a ground, That we may hence learn how little Reason there is to blame me, as he is pleas'd to do, for making Elaborate Experiments; and that though (as I have elsewhere purposely and amply discours'd) obvious Experiments are by no means to be despis'd; yet 'tis not safe in all Cases to content ones self with such: Especially when there is Reason to suspect that the *Phænomenon* they exhibit may proceed from more Causes then one, and to expect that a more Artificial Trial may determine which of them is the true.

Thirdly, whereas Mr. *Hobbs* is pleas'd to find much fault with the Society, and me, for not assigning the Cause of Springs in general; that Omission seeming to him very unworthy of Philosophers: I answer, that the Society having hitherto, for weighty Reasons, forborn to determine the particular Causes of Things, there was no Reason they should alter their Method, for Experiments that were not made or published by Them or by their Order. And as for me, the Title of my Book promises some *Experiments* touching the Spring of the Air and its Effects, not Speculations of the Causes of Springs in general. My avow'd Intention was candidly to communicate with the Curious some Experiments which I thought their Novelty would render acceptable to them, wherein I have the good luck not to have

been miſtaken; nor can I be juſtly cenſur'd for not performing what I did not undertake, nor was oblig'd to. And perhaps Mr. *Hobbs* would more prejudice the Commonwealth of Learning by his ſeverity, then he has yet Advantag'd it by any other way, if he could obtain, that none ſhould publiſh an Experiment or Obſervation that cannot by deduction from the Firſt and Catholick Principles of Philoſophy aſſign the true Cauſe of it. But when I take upon me to write, as Mr. *Hobbs* has done, Elements of Philoſophy, then perhaps I ſhall be able to give an Account of Springs, not much more unſatisfactory then others think his. For though he referre us to his Explication given of the Motion of Reſtitution in his Book *De Corpore*; yet in the 22. Chapter and 30. Section, which profeſſedly contains his Theory of it, after having premis'd, (what rightly interpreted may be true enough,) that the Cauſe of the Reſtitution proceeds not from the taking away the force by which they were compreſſed or extended (*the removing of Impediments not having the Efficacy of a Cauſe*) that which follows to the end of the Section is only this: *The Cauſe therefore of their Reſtitution is ſome Motion either of the Parts of the Ambient, or of the Parts of the Body compreſs'd or extended. But the Parts of the Ambient have no endeavour which contributes to their Compreſsion or Extenſion, nor to the ſetting them at Liberty or Reſtitution. It remains therefore, that from the time of their Compreſsion or Extenſion, there be left ſome endeavour (or Motion) by which the Impediment being remov'd, every Part reſumes its former place; that is to ſay, the Whole reſtores it ſelf.* Now this notwithſtanding, I am ſo dull, or ſo wary, that though I had met with this paſſage, and all the Praiſes the Author in his Dialogue gives it, yet I ſhould have made ſome ſcruple to undertake the aſſigning the true Cauſe of Springs in general. For firſt, the Learned *Gaſſendus*, and the *Epicureans* both Ancient and Modern, together with divers other Naturaliſts, do not admit what Mr. *Hobbs* ſuppoſes a few lines before, that *That which is at reſt cannot be mov'd but by a moved and contiguous Movent.* For they think Motion, or at leaſt

least *conatus ad motum*, an unlooseable Property, congenit to Matter. And, by the way, whatever exceptions I have to this Opinion, yet I am not satisfied with that Principle of Mr. *Hobbs*, though it be the Fundamental one of his Philosophy; unless it be more warily propos'd. For to assert universally and without exception, as he does in his *Elements*, that nothing can be moved but by a Body contiguous and moved; I do not take to be true, nor consistent with his other Assumptions. This I elsewhere (in a Discourse against another then Mr. *Hobbs*, about the Christian Religion) prove more at large: But now it will suffice to represent that Mr. *Hobbs* not only admitting, but making use in his Philosophy of the Creation of the World, either he must allow that Motion is Natural to some, if not all Parts of Matter; or that God put them into a Motion not included in their Nature. From which it will necessarily follow, that at least some Bodies may have Motion though it be not given them by any Body contiguous and moved, as an attentive considerer may easily discern. But to return to the Cause of Springs. Secondly, whereas Mr. *Hobbs* assumes that the Parts of the Ambient have no endeavour which contributes to their Compression or Extension, nor to the setting of them at Liberty or Restitution: He saies this indeed, but does not go about to prove it. And I should the less have made this precarious Assertion, because that after the celebrated *Des Cartes* himself, the *Cartesian* Philosophers generally ascribed the Motion of Restitution to the passage of a subtile Ethereal Substance (and an *Æther* Mr. *Hobbs* also admits) through the Pores of the Springy Body, which striving to obtain its wonted Freedom of passage, restores them to the shape and bigness from which they were forced. Nay, I shall have occasion to shew anon that Mr. *Hobbs* himself, whatever he say in this place, does elsewhere ascribe a Motion of their own to multitudes of Terrestrial Corpuscles. And I might add, that elsewhere he speaks of the re-kindling of the Fire taken out of the Receiver after this manner.

Quanquam vis illius motus in Recipiente (*ut loquimini*) Pag. 18.

evacuato diminuta sit, oppressa ab Aëris intus commoti consistentia, non tamen extinguitur; & propterea levata oppressione, satis habebit virium ad excitandum phantasiam lucis quanquam debiliorem. But I shall rather subjoyn, That yet, thirdly, I do not think it improbable what the Learned *Gassendus* had taught, and what Mr. *Hobbs* here teaches, that the Restitution of bent Springs may proceed from a certain Endeavour or Motion in their internal Parts (left from the time of their Compression or Extension) which when the Impediment is remov'd, makes every Part resume its former place, and thereby makes the Whole restore it self. But this notwithstanding, I fear'd others might be as Inquisitive as my self, and might expect from him that would undertake to settle a general Theory of the Motion of Restitution, the clear and distinct Explication of several *Phænomena* that I had met with, which are not touch'd, nor perhaps were, some of them, thought upon, by Mr. *Hobbs*. As first, why such a determinate Temper of Iron and Steel is requisite to make it Elastical; so that if after having been hardned and gradually heated it be suddenly cool'd at an inconvenient point of time, it will be brittle, and fit to make Gravers and other rigid Tools, not Springs. Next, why Bows and other Elastical Bodies, if they be kept too long bent, lose in process of time almost all their Elastical Power, and continue crooked. Thirdly, why not only divers solid Bodies as well as Lead and Gold, which before tryal, one would think as likely as many Springy ones to have their Parts put into a due Motion by the force that bends them, should be devoid of an Elastical Power. Fourthly, what kind of Motion, and what kind of Texture it is, by virtue whereof, the Parts of a Body being for a very short time put into Motion, do some Months, perhaps some Years, retain in great part a smart Motion, without in so long a time communicating it to the Ambient Bodies, to some or other of which multitudes of them are perpetually contiguous, and thereby losing it themselves. Why upon such a bare and inartificial change made in the Texture of a Body as is scarce at all discernable to the Eye, it

ſhould acquire a ſtrong Spring that it had not before (as I have try'd upon Silver and Copper, which though flex'ble before they were hammer'd, yet being beaten into thin Plates obtain'd a notable Spring :) And why (which may ſeem more ſtrange, upon another light change of Texture) the acquired Spring may preſently be loſt again; as I have try'd in Silver, that Chymiſts teach us loſes nothing in the fire, which having by being hammer'd acquir'd a ſtrong Spring, we have preſently made flexible again as before, by only heating it red-hot, without ſo much as melting it; which argues that in Springs, Texture is as well to be conſidered as Motion. To theſe I might add other Particulars that I had either made or obſerv'd (and mention in another Treatiſe) concerning Springs; all which *Phænomena* perhaps every one that has read what we have lately recited out of Mr. *Hobbs*, will not preſently be able ſatisfactorily to explicate. So that I hope the equitable Reader will not think it a fault that (contenting my ſelf to propoſe the two Explications of Springs, I ſaw moſt lik'd among the Curious; to which I ſhould have added Mr. *Hobbs*'s, if I had found it as much eſteemed) I declin'd engaging my ſelf in Controverſies about the Origine of Motion, and ſuch other high Speculations, as, had my Abilities enabled me, neither my Deſign exacted, nor my leiſure permitted that I ſhould proſecute. And though Mr. *Hobbs* be pleaſed to ſpeak thus of his Notion concerning the Reſtitution of Bodies; *Sine qua Hypotheſi quantuſcunque labor, ars, ſumptus, ad rerum naturalium inviſibiles cauſas inveniendas adhibetur, fruſtra erit:* Yet whether that bold Aſſertion ſhould paſſe for an Argument, for an Hyperbole, or for a Complement to himſelf, I am content to let the Reader judge. Pag. 8.

Fourthly, Mr. *Hobbs* in divers paſſages wherein he diſputes againſt me, ſeems to have miſapprehended my Notion of the Air. For when I ſay, that the Air has Gravity and an Elaſtical Power, or that the Air is, in great part, pump'd out of the Receiver, 'tis plain enough that I take the Air in the obvious Acception of the word, for part of the *Atmoſphere* which we breath,

breath, and wherein we move. Nor do I find that any other of my Readers do otherwise understand me. But Mr. *Hobbs* seems to think he has sufficiently confuted me, if in some cases he have prov'd (which whether he have done well or no is not here to be examin'd) that there is a subtile substance, which he calls *Æther* (but which I wish he had better explain'd) in some places which I take not to be fill'd with Air; and that the *Æther* has or has not some Accidents which I deny or ascribe to the Air. Whereas I deny not but that the *Atmosphere* or fluid Body that surrounds the *terraqueous* Globe, *may*, besides the grosser and more solid Corpuscles wherewith it abounds, consist of a thinner Matter, which for distinction sake I also now and then call Ethereal. And therefore though I did not think my self oblig'd to declare against either the *Atomical* or the *Cartesian Hypothesis* touching the Nature of the Air, yet I propos'd the later too as probable (which as it excludes a *Vacuum*, so it makes the Air consist in great part of a Celestial Matter.) And my incidental Explications of the Rarefaction and Condensation of the Air, together with my comparing it to a Fleece of Wooll, sufficiently declare that I take it not to be a Homogeneous Body; and though there be Air intercepted betwixt the Hairs of Wooll, yet in case I should prove that a Box were not so full of Wooll as before, because the most part of the Hairs had been taken out, I should not think he argued well against me, that should onely prove that the Box contain'd as much of Matter, consisting of Air and Wooll together, afterwards as before. Nor do I think Mr. *Hobbs* has in divers passages, wherein he supposes he disputes against me, much more directly contradicted what I teach concerning the Air, if that word be rightly and in my sense understood. And on this occasion I must crave leave to add, that whereas he is pleas'd to intimate that I misrepresent the *Cartesian Hypothesis*, ascribing that to the Air which *Des Cartes* does to Water; If the Reader think it worth while to compare the Summary Account I give of that *Hypothesis*, with what *Des Cartes* himself has taught in his 45, 46, & 47. Articles of the

fourth

fourth Part of his Principles, wherein that Author compriſes his Doctrine of the Nature of the Air, he will quickly find, that whether or no Mr. *Hobbs* be miſtaken, I am not, unleſs it be in eſtimating his *Hypotheſis* by what he teaches in his Principles, which were publiſhed after his *Meteors*, and more elaborately written. And as for that particular, which alone Mr. *Hobbs* alledges, namely, that he makes not the Parts of Air but of Water ſo flexible: *Des Cartes*'s Words in the 46. Article are theſe; *Cum ejus Particulæ ferè omnes ſint flexiles inſtar mollium plumularum vel tenuium funiculorum, &c.* And as for what Mr. *Hobbs* ſubjoyns, *Sed quiſquis talis ſuppoſitionis Author fuit, parum refert. Nam ipſa Hypotheſis, in qua motus ſupponitur materiæ ſubtilis ſine cauſa velociſſimus, & præterea Corpuſculorum innumerabiles vertigines diverſæ ab illius Materiæ unico motu generatæ, vix ſani hominis eſt.* I cannot but in Gratitude to ſuch a Perſonage declare my Diſlike, to find him upon ſo ſlight an Occaſion ſo courſly us'd for an Opinion the Cenſurer of it does no better confute, and which is thought to be in ſome particulars not ſo unlike his own. And perhaps I ſhould be afear'd that Mr. *Hobbs*'s ſpeaking ſo ſeverely of one that was at leaſt a famous Geometrician, might reflect upon the Engliſh Civility in the opinion of Strangers, if I did not hope that thoſe who have read Doctor *Ward*'s Exercitation will look upon this cenſure of the *Carteſian* Doctrine by Mr. *Hobbs*, as provoked by that ſevere Judgment of *Des Cartes* mentioned by the Doctor in theſe words; *Nempe hoc eſt quod alicubi admiratus eſt magnus Carteſius; nuſquam eum, ſive verum, ſive falſum poſuerit, rectè aliquid ex ſuppoſitionibus ratiocinando inferre.*

Wardi Exer. in Philoſoph. Hobbian. pag. 188.

CHAP. III.

Wherein the Weight and Spring of the Air are asserted against Mr. Hobbs.

HAving thus dispatch'd those general Considerations I thought expedient to premise, my proposed Method leads me in the next place to consider that Mr. *Hobbs* does not, that I remember, deny the truth of any of the matters of fact I have deliver'd. Nor does he, if my memory fail me not, labour to prove that the Explications I have given of my Experiments, are not agreeable to the Doctrine I propos'd: But rather thinks fit to reject our two grand *Hypotheses* themselves, The Weight, and the Spring of the Air. And therefore it will suffice us in this Chapter, briefly, but not slightly, to prove what he is unwilling to grant.

And first, that the Air (in the sense wherein we take the word) is not devoid of Weight, we have prov'd by divers Experiments: which having more fully deliver'd in the Book it selfe, it may in this place suffice now to name them.

One then of these Experiments that prove the Air's Gravity, is, that we found a blown Bladder carefully weighed in an exact pair of Scales, manifestly heavier when full of Air, then when the Air was let out.

Next, it has been observ'd in our 36. Experiment, that an *Æolipile*, being well heated, and the little hole left at the top of the Pipe being stopt, when it was thus hot; upon the opening of that hole, when the *Æolipile* was grown cold again, the external Air rushing in with a whistling noise at the forementi-on'd Orifice, made the *Æolipile* weigh so much more then it did just before the external Air got in, that it amounted, by computation, to near a thousandth part of the Weight of an equal bulk of Water. And though some difficulty may perhaps be mov'd touching the accurateness of the proportion this way found out,

betwixt

betwixt the gravities of thoſe Bodies; yet that the one as wel as the other is actually heavy (which is all that we here need contend for) the Experiment ſufficiently manifeſts.

Thirdly, in the *Magdeburgick* Experiment, (mention'd at the beginning of our Epiſtle) the ingenious makers of it found, that, having before weighed the great Receiver they were to exhauſt, and having done the like after the extraction of the Air, they found it to weigh one whole Ounce and $\frac{1}{10}$; *quod ſane* (ſayes the learned Publiſher, though a Peripatetick) *luculentiſsimum eſt argumentum gravitatis aëris.* *Exper. Magdeburgicum apud Schotum*, pag. 446.

Fourthly, in our 36. Experiment we relate our having weighed the Air, and that ſhut up in Bodies in our exhauſted Receiver, wherein of two Bodies of differing Natures (the one a blown Bladder, and the other a Glaſs Bubble) that were æquiponderant each to a more ſolid Weight before the Air was pumpt out, that which included a good quantity of Air did manifeſtly preponderate after the exhauſtion.

And to theſe four we might adde other proofs to the ſame purpoſe; But that theſe contain in them ſuch a variety of Caſes, that I think it would be ſuperfluous.

But now let us ſee what Mr. *Hobbs* objects againſt the newly-mention'd Experiment of the Bladder weighed in the exhauſted Receiver, (for the others he quarrels not with,) *Quod quidem lanx* (ſayes he) *in qua eſt veſica, magis deprimitur quam altera, certi eſſe poſſunt, oculis teſtibus: Quod autem id à gravitate aëris naturali accidit, certi eſſe non poſſunt; præſertim ſi quæ ſit gravitatis cauſa efficiens neſciunt.* Pag. 15. But I know not whom Mr. *Hobbs* will perſwade, that a man cannot be ſure that Lead is *in Specie* heavier then Cork, unleſs he knows what is the efficient cauſe of Gravity. And Mr. *Hobbs* ſpeaks in his 30. Chapter (where he expreſly treats of that Subject) as if that had not been explain'd by any man, and conſequently not by any Writer of Staticks: (and perhaps I am therein ſomewhat of his mind) And yet ſure all theſe Writers, treating of the Proportion of Heavy

Bodies, did not write they knew not what. And, though he mentions his own *Hypotheſis*, as that *then which nothing is more likely*; yet I think I could frame Objections againſt it, that would not eaſily be anſwer'd, if my preſent task requir'd it; or if I found his opinion, in this point, embrac'd, as yet, by men of Note. Wherefore I ſhall now ſay no more of it then he himſelf doth; namely, that according to his Doctrine, *It may well be thought to determine (for it is a certain conſequent) that heavy Bodies deſcend with leſs and leſs velocity, as they are more and more remote from the Æquator; and that at the Poles themſelves they will either not deſcend at all, or not deſcend by the Axis: which whether it be true or falſe, Experience muſt determine.* Which till it have done in his favour (an event I do not expect) I hope he will allow me to diſtruſt his *Hypotheſis*.

Elem. of Phil. Ch 30. Sect. 4

But to return to our Experiment. The Account he gives why the Bladder does *propend* (for ſo he loves to ſpeak) is this, *Quod veſica ſive follibus ſive flatu oris diſtenta ſit, gravior ſit quam eadem veſica non diſtenta, negare nolo, propter majorem quantitatem Atomorum follibus, vel Corpuſculorum fuligineorum ab halitu inflatorum. Ab experimento autem quod fit à veſica inflata nihil colligunt quod ſit ſatis certum. Oportuit lancibus imponere duo vaſa pondere æqualia, quorum alterum eſſet accuratè clauſum, alterum apertum: Sic enim non inflatus ſed incluſus tantum aër ponderatus eſſet. Quando igitur aërem ſic ponderatum videbis, meditabimur poſtea quid dicendum ſit de Phænomeno quod retuleris.*

Pag. 16.

But, as to the Firſt part of this paſſage, it does not deny the gravity of what we call the Air; but only endeavours to ſhew what Parts they are that make it heavy. And as to the Second, he ſeems to miſtake the preſent Caſe. For, there is no need that the Air in the Bladder be, before the exhauſtion of the Receiver, (in which the foregoing (fifteenth) Page declares he ſuppoſes the Experiment to be made) heavier then the outward Air. Wherefore when he ſubjoyns that from this Experiment we collect nothing *quod ſit ſatis certum*, the Affirmation is not an

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Inference, but Precarious. And as for the annexed way whereby he would wish to have an Experiment made fit to infer the gravity of the Air, if he had not over-look'd what I have delivered in the beginning of the 36. Experiment, he would easily have perceiv'd that we did make a Trial much of the same nature with that he desires. For we weighed in our Receiver the Air, in a Glass Hermetically sealed; wherein it was not (to use his Expression) *inflated*, but only included. This is what he here objects against the gravity of the Air in the other place (*Pag.* 8. & 9.) where he saies something to this Controversie; he inculcates also that we should first explicate what is Gravity, and then adds, *Quod Atmosphæræ insunt permistæ corpori Æthereo multæ tum aquæ tum etiam terræ particulæ, facile persuadeor; sed quod in medio Æthere, sursum, deorsum, quaquaversum motæ, nec semper alteræ alteris innitentes gravitent, inconceptibile est.* To which he adds two or three Reflexions, whose *Examen* being here unnecessary, would require more time then perhaps it would (in reference to the present Controversie) deserve: for we are now enquiring not how the Air comes to gravitate, but whether or no it have gravity. And since in his *Elements of Philosophy* he grants, and gives his Reason for it, That *if Air be blown into a hollow Cylinder, or into a Bladder, it will encrease the Weight of either of them a little:* and since here he likewise confesses (as we have just now seen) that there are mingled with the *Æther* many aqueous and earthly (and consequently heavy) Particles: he confesses that which we labour to evince; namely, that the Air is not devoid of Weight. And it concerns us no more then himself, to shew how the Corpuscles, upon whose account the Air is heavy, make it so. And this being what Mr. *Hobbs* in several places thinks fit to object against the Gravity of the Air; the Reader will, I suppose, easily take notice that he has left the Experiment of the *Æolipile*, and some others, unanswered. Though these alone prove that the Air has a manifest Weight even when it is not compress, but retains its laxity.

Pag. 9.

Chap. 30. Sect. 10.

Having ſaid thus much to evince againſt Mr. *Hobbs* the Gravity of the Air, let us now examine whether it have not alſo a Spring (in the ſenſe we take that word in.) This though Mr. *Hobbs* be pleas'd to call (as he alſo does the Weight of the Air) *a Dream*; yet he does himſelf grant, in effect, as much as is requiſite to prove the Spring of the Air, in the ſenſe I contend for it. For taking upon him to give account (how good an one we ſhall ſee anon) of that known Experiment wherein the Air is compreſt in a Glaſs Bottle by the forcible injection of Water, which Water, when the Glaſs is unſtop't, the Air does again throw out in recovering its former Dimenſions; of this Experiment (I ſay) he gives this account (Pag. 24.) *Aër quo ab initio sphæra plenus erat à Corpuſculis illis terreis motus motu circulari ſimplice, vi injectionis coactus, qui quidem purus eſt exit (aquam injectam penetrans) in aërem extrinſecum, locum relinquens aquæ; ſequitur ergo Corpuſculis illis terreis minus relinqui loci in quo motum ſuum naturalem exercere poſſint: itaque in ſe mutuo impingentes aquam urgent ad egreſſum; egredientem aër externus (quia univerſum ſupponitur eſſe plenum) penetrat, locumque egredientis aëris ſucceſsivè occupat, donec Corpuſcula, quantitate aëris eadem reſtituta, libertatem motui ſuo naturalem recipiant.*

Utraque enim illa phantaſia, tum gravitatis Atmoſphæræ, tum vis Elaſticæ ſive antitupiæ aëris, ſomniumerat, Dial. p. 21.

But how little this comes ſhort of granting as much Spring to the Air as the *Carteſians* do, and as I need require, may eaſily be judg'd by divers paſſages in our Book; and particularly by our propoſing as not improbable, the *Carteſian* way of explicating the Spring of the Air; according to which the Corpuſcles that ſwim in the *Æther*, being each hindred by the neighbouring ones from the freedom of its motion, they beat off one another (which Mr. *Hobbs* would have them do:) whence it comes to paſs that, in any aſſign'd portion of Air here below, the Corpuſcles that compoſe that portion, beaten off by one another, do make the whole portion tend to obtain (though not exactly to fill up) more room, and conſequently to emulate a Spring,

Spring, like that which we ſcruple not to aſcribe to a compreſt Fleece of Wooll, becauſe of a like endeavour to expand it ſelf.

We may enforce this by another paſſage of Mr. *Hobbs*'s, that ſpeaks expreſly enough to our preſent purpoſe, where he gives this Reaſon of one of the *Phænomena* of our Engine, Pag. 11.

Quoniã per ſuctoris retractionem aër purus impulſus erat, partes autem terreæ impulſæ non erant; major erat ratio particularum terrearum quæ extra Cylindrum ſuctori contiguæ erant, ad aërem purum, in quo motum ſuum exercebant, poſt revulſionem quam ante: quare particulæ illæ motæ minus habentes loci ad motum ſuum naturalem exercendum, aliæ aliis impingebant, & propellebant: neceſſe ergo erat, at particulæ quæ ſuctoris ſuperficiei contiguæ erant ſuctorem propellerent. To which we may adde, that Mr. *Hobbs* himſelf ſeems rather to reject other mens wayes of propoſing the Spring of the Air, then reſolutely to deny the thing it ſelf. For, *Vidiſti* (ſayes he) *jam Elaſtrum illud aëris quod ſupponunt, aut impoſsibile eſſe, aut recurrendum eſſe ad Hypotheſin Hobbianam.* Pag. 8.

The Motions therefore of thoſe ſmall Bodies (ſpeaking of the Earthy Particles in the Air) will be leſs and leſs free, by how much the quantity of the injected Water is greater and greater: ſo that by their Motion falling upon one another, the ſame Bodies will mutually compreſs each other, and have a perpetual endeavour of regaining their liberty, and of depreſſing the Water that hinders them. *Elem. Chap.* 30. *Sect.* 9.

But beſides Mr. *Hobbs*'s Conceſſions in the paſſages newly recited, and ſome others; we can prove the Spring of the Air by many of the *Phænomena* of our Engine, which we have deduc'd from it, and of which he does not offer any other way of Explication. Wherefore we ſhall now content our ſelves to prove the Spring of the Air by two Experiments: The one not mention'd in our Epiſtle, and the other much oppos'd by Mr. *Hobbs*.

And Firſt, if you make the *Torricellian* Experiment in a Tube of between two foot and half and three foot in length, and if, when the *Mercury* reſts at its wonted Station, you dexterouſly ſtop the Orifice of the Tube with your Finger (that Orifice being lifted up as near the ſurface of the reſtagnant *Mercury* as it can

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be, without giving admiſſion to the external Air) and if then you quite lift up the Tube thus ſtopt into the free Air, you ſhall feel upon your Finger little or no gravitation or preſſure from the Weight of the *Mercurial Cylinder*, diſtinct from the Weight of the Tube: becauſe (as we have more fully explicated this *Phænomenon* elſewhere) the gravity of the Quickſilver is balanc'd by that of the outward Air that thruſts the Finger againſt it. But if you invert the Tube, and having let in the Air at the Orifice, ſtop it again with your Finger, and again let the *Mercurial Cylinder* lean upon that Finger; you ſhall then find your Finger ſtrongly preſt, and endeavour'd to be thruſt away: which new preſſure, ſince it cannot come from the *Mercury*, that being the very ſame that was in the Tube before, nor from the Weight of the admitted Air, which perhaps may not amount to ſo much as a grain, to what can we rationally aſcribe it but to the Spring of the included Air, whoſe force will be as well manifeſt to the Eye as the Finger, if the Tube be unſtopt under the ſurface of the reſtagnant *Mercury*; for then that in the Glaſs will not reſt as before at the uſual Station, but be depreſt beneath it a good way, perhaps ſome Inches? And if you make the *Torrecellian* Experiment in a ſhort Tube ſeal'd at neither end, but ſtopt above and below with your Fingers, you ſhall find, upon the unſtopping of the upper Orifice, a new and forcible preſſure upon the Finger that keeps the lower Orifice ſtopt, made by the gravitation of the external Air, which was before kept off from leaning upon the *Mercurial Cylinder* by the upper Finger; the Pulp of which Finger by that gravitating Air was before thruſt into the deſerted Cavity of the Tube (as we have elſewhere diſcourſt, in a fuller meaſure, of theſe Experiments.) Which will evince againſt Mr. *Hobbs*, both the Spring of the Air and Gravity of the *Atmoſphere*; ſince he is as little as I for aſcribing theſe *Phænomena* to the efficacy or abſence of my other Antagoniſt's imaginary *Funiculus*.

In our Defence againſt L nus.

The other Experiment I ſhall mention is the Fourth in our Epiſtle; namely, that of the ſwelling and ſhrinking of a Bladder

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hung in our Receiver, according as the ambient Air, and consequently its pressure, is withdrawn or suffer'd to return. But though this Experiment be so congruous to our *Hypothesis*, that 'tis generally acquiesc'd in by those Ingenious men that have hitherto seen it; yet Mr. *Hobbs* is pleas'd to reject our Explication, and substitute another in these Words, which are all he has concern'ing this matter. *Quia Cuticula omnis ex filiculis constat, quæ propter figuras, contactum per omnia puncta accuratum habere non possunt, pervia ergo est vesica, cum sit cuticula, nec aëri tantum, sed etiam aquæ, qualis est sudor. Eadem ergo aëris per vim incussi est compressio intra vesicam quæ extra, cujus conatus, propter viam motuum undiquaq; discussatam, tendit undiquaque ad superficiem vesicæ concavam. Quare necessarium est ut undiquaq; intumescat, & crescente conatûs vehementiâ tandem lacereturr.* Pag. 19. But, if this be a sufficient Answer to such an Experiment, I confess I fear it will be harder then we are yet aware of to prove any thing by Experiments.

For First, how unlike is it to be true what he affirms, and what his Reply supposes, namely, that such Bladders as we us'd are readily pervious to the Air; when easie Experience shewes us, that by leasurely compressing such blown Bladders betwixt our hands, we shall rather break them (as we have try'd) then squeeze out the Air at the Pores? So that the rest of his Answer being built upon what is so repugnant to common Experience, will not need a particular Confutation: but however *ex abundanti* we will adde, that in our 36. Experiment, we relate that by the exhaustion of the Air we likewise broke a Glass Hermetically seal'd; and to say that Glass also is pervious to Air, were to affirm what the greatest part of his Book supposes to be false. Besides, whereas there is not any sensible and unquestionable *Phænomenon* to prove that the Receiver is full of any such *aër per vim incussus* as he would have, we see plainly that when the Air does manifestly get into the Receiver, the Bladder is not thereby made to swell, but strangely to shrink. Moreover since (according to Mr. *Hobbs*) the Bladder is pervious to

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the Air, and the Air within the Receiver is universally compreſt, as well that which is within the Bladder as that which is without it; how comes it to paſs, that the Air that bears against the Convex Surface of the Bladder does not reſiſt that which is contiguous to the Concave Superficies of the ſame? And at leaſt how comes the Bladder to be broken by the Air, which, according to Mr. *Hobbs*, can get in and out at pleaſure? And laſtly, to ſhew that to the ſwelling of the Bladder there needs nothing but the Spring of the included Air, and not ſuch vehement agitation of the ambient Air as Mr. *Hobbs* ſuppoſes to be made in our Engine; It appears by the elſewhere-mention'd Experiment of *Monſieur Paſchal*, that in the free and ordinary Air a Foot-ball half blown up will ſwell more and more the nearer it is carried to the top of an high Mountain; where the Incumbent *Cylinder* of the *Atmoſphere* is ſhorter, and its Weight lighter: and will, for the contrary Reaſon, grow more and more flag'd, the nearer it approches again to the foot of the Mountain.

Though I doubt not but the Arguments employ'd in this Chapter will be ſufficient to convince impartial Readers; yet I ſhall adde by way of Inforcement, that whereas Mr. *Hobbs* aſcribes the Weight of the Air in Bladders to the earthy Corpuſcles intruded by him that blows them up; and attributes the Spring of the Air in the Wind-gun and in the *Phænomena* of our Engine, to the violent Motion the Air is put into by the vehement impulſes of the Rammer or Sucker: our Doctrine may be evinc'd by Experiment, wherein the Air in its natural and wonted ſtate operates without being forcibly compreſt or put into motion by us. This may appear by the two ſorts of Experiments to be made upon high Mountains, which we have mention'd and urg'd in the *Second Part* of our *Defence againſt the Learned Linus*. Wherefore referring the Reader thither, we ſhall now only in very few words mention the ſubſtance of them.

The Firſt Experiment is, That it has been found upon Tryal, both formerly in *France* and ſince in *England*, that the Quick-

ſilver

ſilver in the *Torricellian* Experiment falls notably lower at the top of a Mountain then at the foot, (by Monſieur *Paſchall's* obſervation upon a Hill (far higher then thoſe the Experiment was try'd on here) the difference was ſo great, as to amount, as the moſt ingenious *Pecquet*, a happy promoter of Experimental Learning, informs us, to above three Inches) which we ſay is cauſed by this, that the Atmoſpherical Cylinder is much lighter, as well as ſhorter, at the top of the Mountain then at the bottom: and Mr. *Hobbs* diſallows not the Experiment, but yet gives onely this account of it, *Sed & particulæ illæ quæ interſperſæ aëri ita moventur ut ſuppoſuimus, magis confertæ ſunt ad radicem montis quàm in ſummo, nam hoc quoq; ſuppoſuimus.* But what then? how does the plenty of theſe interſpers'd Particles hinder the *Mercurial* Cylinder from deſcending at the bottom of the Hill as much as at the top, unleſs by their gravity or preſſure? And 'tis very unlikely that the Earthy Atomes, contiguous to the reſtagnant *Mercury* at the bottom of the Hill, ſhould be able by their weight to keep ſuſpended a Cylinder of *Mercury* of above three Inches, unleſs the contiguous Air were gravitated upon by the weight of other incumbent parts of the Atmoſphere. Pag. 12.

The other of the two mentioned Experiments is briefly this, That a Termoſcope being carried from the bottom to the top of a Hill, the included Air, inſtead of ſhrinking in that colder Region, manifeſtly dilated it ſelf, and notably depreſs'd the water. An effect which I ſee not to what it can well be attributed but to the ſpring of the included Air, which having not near ſo great a preſſure againſt it from the Atmoſphere incumbent on the reſtagnant and ſuſpended water, was able to make it ſelf more room then before it could; and ſince that preſſure of the Atmoſphere depends for ought appears upon its gravity, the ſame Experiment may argue both the ſpring of the Air and its weight.

And this may ſuffice for our third Chapter, wherein having evinc'd againſt Mr. *Hobbs* our grand *Hypotheſis* of the *weight* and *ſpring* of the Air, I hope we have diſpatched the chief part of our

work; since as for the particular Explications we deduce from these *Hypotheses*, there are but very few, if any, that he endeavours to prove incongruous to them. Yet after we shall have (in the following Chapter) consider'd upon what grounds he prefers his Doctrine before ours, we shall (God permitting) in two or three other Chapters gather up the things that he objects against some particular Opinions and Explications by us delivered, and examine them.

CHAP. IV.

Wherein Mr. Hobbs's *principal Explications of the* Phænomena *of the Authors Engine are examined.*

OF the *Hypotheses* that Mr. *Hobbs* assumes to explicate the *Phænomena* of our Engine, himself gives us a summary in this passage, (*pag.* 10.) *Intellexti ergo Hypotheses meas,* 1. *Quod aëri interspersæ sunt particulæ multæ terreæ præditæ motu circulari simplice, naturæ congenito.* 2. *Quod major est quantitas earum particularum in aëre propè ad terram quàm in aëre à terra remotiore.*

Now here I might at the beginning take notice, that there are *other* things which he takes for granted. As, first, *Non dari Vacuum*, which as we have already seen he has not well evinced, nor I think easily will upon the grounds he proceeds on. Next, that our common Air is chiefly cómposed of an Æthereal substance, which methinks he should have proved; since for the most part the Vacuists (and such he will needs have his Adversaries to be) admit not that pure Air of his. Thirdly, that the Air, at least the pure Air, is easily divisible into parts alwayes fluid and alwayes Air. Indeed he sayes of this Assumption,* *Nec suppono tantùm, sed credo*; but neither to *suppose* nor to *believe*, is to *prove*. And what he adds, † *Neq; est qui hactenus*

* Pag. 4.
† *Ibid.*

hactenus ullam adduxit rationem, quare ita esse non potest; if it were true, would conclude little, since many things have not been, and perhaps cannot be, proved to be true; of whose not being possible no proof has been given. We might, I say, mention and examine these *other* Assumptions of our Author, but for brevities sake we will consider those two lately recited from him.

And as for the second of them, bating the peculiar motion he is pleased to ascribe to the earthy Particles, I shall not contend with him about that *Hypothesis*; and therefore shall now onely consider the other. The *Motus circularis simplex* it self, which he imagines in the Sun and the terrestrial Globe, I shall not need to examine, since Dr. *Ward* (a person whom, without disparagement to a famous man, I may affirm to be at least as esteemed for Astronomy as Mr. *Hobbs*) has expresly endeavoured to confute it, and that not without some derision, (which yet I willingly forbear to imitate) by Arguments that I cannot learn Mr. *Hobbs* has yet answered. And I am informed that the learned Dr. *Wallis*, and others, intend some Animadversions on this Motion. But restraining our present consideration to what this Dialogue suggests to me, This Assumption to me seems very precarious, since I know not any unquestionable Example or Experiment whereby it can be made out, that any small parcel of matter has such a *motus circularis simplex* as he ascribes to each of these innumerable earthy and (as himself adds in the same page) aqueous Particles. The onely Argument he brings in that page to prove that each Atome would have this motion, if all the rest of the earth were annihilated, does not to me seem clear. For, not to mention that it is still by many learned men doubted whether the Terrestrial Globe it self have it; nor to examine whether or no he assigns a good Natural cause of it; it is not alwayes true, that each minute part of a Homogeneous body (which yet 'twill be hard to prove the Terrestrial Globe to be) has in every respect the same qualities with the whole: As the roundness which a small drop of Water or Quicksilver is com-

monly obſerved to have when it leans upon a dry or greaſie plain, is not to be met with in great portions of either of thoſe Liquors, though placed upon the ſame plain. And Mr. *Hobbs* as well as we makes the terrene Atoms in the Air to have *gravity*, which yet is a quality that does not properly belong to the whole Globe of the Earth; nor is it manifeſt why, becauſe the Terreſtrial Globe moves in a vaſt Circle about the Sun, each particular Atom of it muſt deſcribe a ſmall Circle in the Air about I know not what Centre. And ſince he teaches in his ſecond *Hypotheſis*, and a few lines before it, That the Air near the Earth abounds with ſuch Terrene Corpuſcles, 'tis not likely they ſhould be permitted to exerciſe ſuch a regular motion as he attributes to them; but hitting againſt one another, they muſt in probability be put into almoſt as various and confuſed a motion as *Des-Cartes* aſcribes to his Terreſtial Particles ſwimming in the Atmoſphere. But that which ſome will, I doubt not, peculiarly wonder at in Mr. *Hobbs*'s *Hypotheſis* is, that he makes this regular motion of each Atom *naturæ ſuæ congenitus*: For Philoſophers that are known to wiſh very well to Religion, and to have done it good ſervice, have been very ſhy of having recourſe, as he has, to Creation, for the explaining of particular *Phænomena*. And the *Carteſians* will think it at leaſt as allowable for them to ſuppoſe the Motion he will not grant in their *Materia ſubtilis*, as for Mr. *Hobbs* to aſſume it in his *Particulæ terreæ*: eſpecially ſince he ſeems to make each ſuch Atom put into and kept in a regular motion; whereas they aſſume but the having of one general impulſe given to the whole maſs of Matter. Thoſe likewiſe that fancie a Spring properly ſo called in particular Aerial Corpuſcles, will hence perhaps take occaſion to think they may ſuppoſe an ingenite motion fit for their turn, as well as he an ingenite *motus circularis ſimplex*. How well likewiſe this *Hypotheſis* will agree with his Fundamental Doctrine, That *Nihil movetur niſi à corpore contiguo & moto*, I leave to him to conſider. As alſo whether or no *Gaſſendus*, and thoſe other Atomiſts that admit Creation, may not hence countenance their

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grand ſuppoſition of the congenite motion of Atoms, which granted would deſtroy the beſt part of Mr. *Hobbs*'s Philoſophy. But whatever become of this *motus circularis ſimplex*, I need not be much ſolicitous, having formerly ſhewn, that the admiſſion of it would not diſprove what I have delivered concerning the Spring of the Air: and therefore leaving Mr. *Hobbs* to diſpute it out, if he think fit, with his other Adverſaries, I will proceed to the main Explications, wherein Mr. *Hobbs* endeavours to prefer his Doctrine about the *Phænomena* of our Engine before ours. And theſe I find to be the four that enſue.

The firſt and principal of theſe is that wherein he ſtrives to prove, That by the Exhauſtion of our Cylinder no *Vacuum* is produced, and to give of the Experiment it ſelf a very differing account from ours. This he does in the following paſſage; which, by reaſon of its importance in our préſent Controverſie, we ſhall ſet down *verbatim*: *Dum Suctor* (ſayes he) *retrahitur, quanto relictus locus major fit, tanto minus loci relinquitur aëri externo, qui retruſus à Suctore moto verſus externa, proximum ſibi aërem ſimiliter movet, & hic alium, & ſic continuè ità ut neceſſe ſit aërem tandem compelli in locum deſertum à Suctore, & intrare inter ſuperficiem Suctoris convexam & Cylindri concavam: ſuppoſito enim aëris partes eſſe infinitè ſubtiles, impoſſibile eſt ut viâ illâ qua retrahitur Suctor, illæ non ſe inſinuent. Primò enim, contactus ſuperficierum iſtarum per omnia puncta perfectus eſſe non poteſt, quia ipſæ ſuperficies fieri infinitè læves non poſſunt. Deinde vis illa quæ ad Suctorem revellendum adhibetur, cavitatem Cylindri aliquantulum diſtendit. Poſtremò, ſi in confinio duarum dictarum ſuperficierum ingrediatur una tantum atomus dura, aër purus eâ viâ ingreditur conatu quantumvis debili. Poteram etiam computaſſe aërem illum qui propter eandem cauſam inſinuaſſet ſe per Cylindri valvulam. Sublatam ergo vides conſequentiam à retractione Suctoris ad locum vacuum. Sequuturam hoc quoq; eſt, aërem illum qui eſt in locum à Suctore deſertum impulſus, quia magnâ vi impulſus eſt, motu valde celeri & per circuitum inter ſummum & imum in Cylindro moveri; cum nondum ſit quod motum ejus poſſit debilitare: Scis autem:* Pag. 5.

autem nihil esse quod sibi motum aut impertiri posit aut diminuere. But this Ratiocination containeth divers things lyable to exceptions; and in order to the examining of it I must premise, That I know not why Mr. *Hobbs* should here confine his discourse to the Pump without taking notice of the Glass, for whose evacuation 'twas designed. Wherefore for easier considerations sake we will consider, how this discourse will account for the Exhaustion of the Receiver, as well as for the Cylinder; for we usually empty them both in the same tryals. And he being obliged to explicate the Exhaustion of the one as well as the other, it will be convenient to take into consideration the Receiver also, because that being of Glass and transparent, we can better see what happens in it then in the opacous Cylinder. This premised, we may now proceed to the Exceptions themselves. And, first, I do not clearly see by this Explication how he avoids a *Vacuum*: For, according to his first words, the external Air is displaced by the motion of the Sucker outward, and this displac'd Air must move that which is next to it, and that the next, and so onward, (whether *in infinitum* or no he declares not;) so that at length *(tandem)* the Air must be compell'd into the place deserted by the Sucker: so that till this returning Air get in betwixt the Sucker and the Cylinder, how appears it from this Discourse, that the deserted space was not empty for some little while? For, certainly, all these motions of the Air forward and backward could not be perform'd in an instant; as may appear by the motion of Sounds and Echo's, whose Velocity is reducible to measure. Secondly, though he takes his Adversaries to be Vacuists, yet (to give an account of these *Phænomena)* he supposes the Plenitude of the World; as may appear both by expres passages in his Dialogue, and by his here rendring no other probable cause of the Airs getting into the room relinquish'd by the Sucker. But, because I have not here taken upon me the person of a Vacuist, I shall offer some other Considerations. I wish then, thirdly, that Mr. *Hobbs* had declared from whence the regress of the Airs impulsion should begin; for that may well

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be required from one that, making the World full, and for ought appears (the Celestial Globes excepted) fluid, allows us to believe it infinite, if the Magistrate shall please to enjoyn us that belief. Fourthly, I demand what necessity there is there should be such a forcible return of the impulse, as is requisite to thrust in the Air at so narrow a passage as that between the Sucker and Cylinder. For, why may not that impulse, when diffused in the vast ambient *Medium*, be so communicated and blended among the differing motions of the other parts of it, as not to return again from whence it begun? As we see that a Voice, though strong, will not move the Air beyond a certain distance smartly enough to be reflected in an Echo to the Speaker: and a stone cast into a Lake will have the Waves it makes diverted from returning to the place they began at. Fifthly, I do not likewise see that 'tis prov'd, or probable, what Mr. *Hobbs* affirms of so thick a Cylinder as ours, that it should be distended by the depressing of the Sucker. But this I insist not on; for the main thing that is peculiar in Mr. *Hobbs*'s Explication is, That as much Air as is driven away by the Sucker, gets presently in again betwixt it and the Cylinder: wherefore let us examine that a little. I say then, that by the Air which is so supposed to get in, he either means, in the usual sense and in ours, the Common Air, such as we live and breath in; or he does not.

If he *do* speak of such Air, I can plainly prove by several Experiments, that our Engine is in great part devoid of it.

For, first, if there be a contrivance made, whereby the whole Pump may be covered with Water, one may, as we have tryed, plainly see the Air that is drawn out of the Receiver, at each reciprocation of the Sucker, pass in great bubbles out of the Valve through the water.

Next, it appears by the *Magdeburg* Experiment formerly mentioned, that by reason of the recess of the Air, the Globe of Glass, whence it went out, was diminish'd in weight above an Ounce. Thirdly, the same truth may be proved by the Experiments formerly mentioned of the swelling of a Bladder, and

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the breaking of an Hermetically-seal'd Glass upon the recess of the ambient Air: these Experiments having been already vindicated from Mr. *Hobbs*'s very improbable Explications of them. Fourthly, the same may be prov'd by the breaking of weak or ill-figur'd Receivers inwards; of which in our *Hypothesis* the reason is clear, but not in Mr. *Hobbs*'s. But, fifthly, (not to multiply Instances, though that were easie for me) what I contend for may be sufficiently proved by this one *Phænomenon*, That though, if the Receiver being full of common Air the Key be turned under water, the water will not at all be spurted up at the open Orifice: yet the like being done after the Exhaustion of the Receiver, we have had divers Gallons of water violently impell'd into the cavity of the Glass; which could not happen if it were full of Air, both in regard there can be no probable cause assigned why the water should be thus spurted up; and because the Receiver being already full of Air, either two bodies must be contained in one place, and so we must allow *Penetration* of *Dimensions*; or else common Air, to which Glass is impervious, must pass through the water, which we conclude it does not, because no such bubbles are made in the external water, as would appear if common Air past through it: Nay, so little of this common Air was sometimes left in the Globe us'd at *Magdeburg*, that when the water was suffered to rush in, it reduced the Air into less by the beholders estimate then the thousandth part of the capacity of the Globe: And even if our Receiver be unstopt, not under water, but in the open Air, the ambient Air will violently press in with a noise great and lasting enough to argue that the Glass was far from being full of such Air before.

And thus we may argue against Mr. *Hobbs*, if he would have the Engine, when we call it exhausted, fill'd with common Air, as his words in the recited passage (where he talks of the external Air, and that impell'd into the Cylinder, without differencing them) seem to intimate. But because by some other passages of this Dialogue he may be favourably thought to mean, that the pure Air (as he speaks) is that which gets in by the sides of

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the Sucker into the Pump, and ſo into the Receiver; let us conſider his Explication in this ſenſe alſo. And not to urge, that it had not been amiſs if, to avoid ambiguity, he had more clearly expreſt himſelf, and named that Other here, as well as he elſewhere calls it ſo: not to urge this, I ſay, I deſire it may be taken notice of, that if Mr. *Hobbs* take the Air in this ſecond Notion, he oppoſes not what I have delivered; the Air I pretend to be pumpt out of the Receiver being the common Air, which conſiſts in great part of groſſer Corpuſcles then the Æthereal ſubſtance; and therefore I might ſafely paſs on to another ſubject. But I conſider further, that even this Explication of Mr. *Hobbs*'s will be lyable to the two firſt Inconveniences lately objected againſt the other in favour of the Vacuiſts; and to divers of thoſe things beſides, that are objected in the following parts of that Diſcourſe. Next I obſerve again, that though the Pump be all the while kept under water, yet the Exhauſtion of the Cylinder and Receiver will be made as well as in the open Air: I demand then of Mr. *Hebbs*, how the pure Air gets in by the ſides of the Sucker that is immers'd in water. I preſume that for want of a more plauſible Anſwer he will here ſay, (as he elſewhere does in an almoſt parallel caſe) That the Air paſſes thorough the body of the water to fill up that deſerted ſpace, that muſt otherwiſe be void: But then I appeal to any rational man, whether I am obliged to believe ſo unlikely a thing upon Mr. *Hobbs*'s bare affirmation. If I be, I muſt almoſt deſpair to prove things by Experiments; and if he will allow me to expect from him as much as he ſeems to do from me, I ſhall ſcarce deſpair to maintain almoſt any *Hypotheſis* I pleaſe: For, beſides that he does not ſo much as pretend by any *Phænomenon* to countenance this bold aſſertion, there are *Phænomena* that make againſt it. For I know not how many Experiments ſhew us, that when Air paſſes through Water, it makes bubbles there, which in our caſe do not appear. And beſides, I ſee not why the outward Air ſhould not rather impell the water (as we ſee it frequently does in ſuch caſes) then be ſuppoſed to dive ſo ſtrangely and unper-

ceivedly through it. When also the diligently-exhausted Receiver is unstopt under water, he that observes how the water rushes in with a stream as big as the passage will give leave, will hardly imagine that at the self-same time as much Air as there gets in water can passe through the same hole without being perceiv'd. But it may by Plenists be said in Mr. *Hobb*'s behalf, and it seems the most that can be said, that either his Explication or a *Vacuum* must be admitted. To which I reply, First, that he has not evinc'd there can be no *Vacuum*; having endeavoured to prove it but by a single Experiment, which at best does not more strongly plead against a *Vacuum* then this does for it. Next, that we have lately made it probable, that by his Explication he does not avoid the necessity of a *Vacuum*. And thirdly, that a Plenist without having recourse to Mr. *Hobb's* precarious *diving* of the Air, may more probably decline the necessity of yielding a *Vacuum* by saying, according to the Principles of the *Cartesian*, (the subtilest and wariest Champions for a *Plenum* I have yet met with) that the *Æther* is by the impulse of the depressed Sucker and the resistance of the ambient Bodies squeez'd in at the pores of the Glass or Cylinder into the cavity of the vessel, as fast as room is there made for it. And I confesse, I somewhat wonder at Mr. *Hobb*'s being averse to this way of salving the objected Difficultie, since (a little above the middle of that passage of his we have so long been examining) he supposes the parts of the Air to be infinitely subtile; which if they are, I know not what pores can be too narrow for them to insinuate themselves into. But, to presse this no further, I must here take notice, that whether the cavity of the Receiver be resolved to be (totally or in part) empty or full of Mr. *Hobb*'s Ætheriall Body, or the *Cartesians* Celestiall matter; the violent rushing in of the water, when the vessel is unstopt under that Liquor, and divers other *Phænomena* which will not be ascrib'd to the subtil matter within (to which they attribute not any attraction) sufficiently argue that there is in the External Air a far greater power of pressing inwards, then there

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is within of resisting; and consequently such a Weight or Spring in that Air as my Epistle challenges to it. I had almost forgot to Answer the last lines of Mr. *Hobbs*'s so often mentioned passage, where he would have the Air that he supposes to be impell'd into the Sucker, to move very swiftly betwixt the top and bottom of it. And so elsewhere he would have the same Air, when it gets into the cavity of the Receiver. But having tired my self, as I fear I have you, by dwelling so long upon one passage; I will skip somewhat that I here approve not in the Ratiocination, and only say, that when a light Bladder is suspended in the cavity of the Receiver, it betrays no such motion as is here imagined; nay, the flame of a Taper, as our Epistle mentions, was not blown out, nor (for ought appeared) stir'd by this suppos'd wind: to which I shall adde, that Smoake produced in the Receiver whilst it remained exhausted, was not by this vehement motion of the Air blown about the Receiver, as is particularly set down in our *Appendix*, promised by the Translator of the newly-mentioned Treatise. But if you let in the Common externall Air at the Stop-cock, that indeed will rush in with Noyse and Violence, and whirle about the Bladder that hung quietly enough before.

Having thus examined Mr. *Hobbs*'s First solemn Explication, I now proceed to the next, wherein he undertakes to give an account by his *Hypothesis* of the Reason, Why in our Engine, if the *Manubrium* of the depress Sucker happens to slip out of the Pumpers hand, the Sucker is carryed up again toward the top of the Cylinder. But since this Explication is such, that though he mentions it as his first, presently after the Recital of his two *Hypotheses*, he himself is pleas'd to confess in the last page of his Book, that 'tis Erroneous; I shall forbear to vex it, thinking such acknowledgements more fit to be imitated whenever there is the like occasion, then to be discouraged. But as for the Explication which at the end of his Dialogues he substitutes for his Retracted one, I confess to me 'tis so obscure, that I know not well what to make of it. But, as far as upon consideration

I can understand it, it is coincident with that, which in our Method will be call'd his Fourth Explication; with which that it may the better agree, seems one of the chief Reasons of his altering it from what he had proposed at first. Wherefore we should presently fall upon examining them both together; but that between them I meet with an Explication (which in our reckoning is the Third) of the *Torricellian* Experiment.

And here he spends many words to prove the Opinion he had whether proposed or adopted in his Elements of Philosophy; namely, that the place deserted by the suspended *Mercury* is not empty, but full of Air. But because this Exposition assumes what he has not yet Demonstrated, *viz. Non dari Vacuum*, and because the *Torricellian* Experiment as 'tis wont to be made is none of the *Phænomena* of our Engine; I shall refer you to what those Learned men Doctor *Ward* and *Moranus* have professedly, and the first of them largely enough, written against Mr. *Hobbs*'s Explication, (yet without making all that either of them teaches mine:) Because, for my part, it will suffice me to argue, as I did before, that if he takes the Air in the common sense of the Word (and that wherein his Readers generally understand him) his conceipt is manifestly Repugnant to severall such *Phænomena* as these. *That* if the Experiment be very well made, we may by inclining the Tube impell the *Mercury* from its wonted station to the top of the Tube; which will not happen in case the Air were before Inclination let into that deserted space. *That* if when the *Mercury* is setled at its wonted station, the Tube be lifted up out of the Restagnant Quicksilver, the outward Air will drive up the heavy *Mercuriall* Cylinder oftentimes with force enough to beat out the sealed end. To which we shall adde onely this Experiment: The Quicksilver resting at its wonted station, if you carefully stop the lower Orifice under the Surface of the Restagnant Quicksilver, and then lifting up the Tube (that which we us'd was about three or four foot long) into the Air, keep it well stopt, if, I say, you first depresse one end and then the other, you shall find the

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Quickſilver fall againſt the depreſt extream of the Tube with ſuch ſwiftneſs and force, as will perhaps ſurprize you, and make you apprehend that the Tube will be either beaten out of your hand or broken: Whereas if unſtopping the Tube whilſt the ſelf-ſame quantity of *Mercury* remains in it, you let the outward Air into the cavity unpoſſeſt by the *Mercury*, and then if you again ſtop the Orifice with your finger, and proceed as formerly, you ſhall perceive the motion of the included Liquor to be very much ſlower and leſſe violent then formerly, by reaſon of the reſiſtance of the admitted Air: which will alſo manifeſtly diſcloſe it ſelf by the conflict and bubbles that will be produc'd betwixt the Air and Quickſilver in their haſty paſſing by one another to the oppoſite ends of the Tube.

If any friend of Mr. *Hobbs*'s, ſeeing the manifeſt inconveniences of this opinion, ſhall on his behalf pretend that 'tis what he calls the pure Air, that paſſes through the body of the Quickſilver to the deſerted part of the Glaſs Cane; the anſwer is ready, that Mr. *Hobbs*'s expreſſions look ſo much another way, that his Readers (for ought I have found) do generally underſtand him of ſuch common Air as is diſplac'd by the deſcent of the *Mercury*. And therefore I had reaſon enough to argue againſt what he wrote, as I have newly done; and however, this aſſertion is clearly precarious, and lyable to the Objections formerly alledg'd againſt the paſſing of the Air through the water. To which we may adde this circumſtance, that in our preſent caſe it muſt deſcend into a far heavier and cloſer Liquor then water. But perhaps it will be thought, I have already ſaid more then needed againſt an opinion which has been rejected as well by Pleniſts as Vacuiſts; and though mention'd as to the main by ſeveral Writers, as well before Mr. *Hobbs* aſſerted it as afterwards, has been thought ſo unlikely, as not to have been (that I know of) approv'd by any man, even before the diſcovery of the *Phænomena* of our Engine. Which laſt words I adde, becauſe that Mr. *Hobbs* not pretending that any attraction intervenes in the caſe, I ſee not how he can poſſibly make out, to omit other *Phænomena*,

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the deſcent of the *Mercury* in the Tube further and further beneath its wonted ſtation, upon the Exhauſtion of the Receiver, and the re-aſcenſion of the ſame *Mercury* in the ſame Tube, as we pleaſe to let in more or leſs of the outward Air; without admitting as much of Spring or Preſſure in the Air as I need contend to have here allowed me. The weight of the Terrene Particles, by which, at the end of the third Expoſition, he is reduc'd to endeavour the Solution of the Quickſilvers falling lower at the top then at the bottom of a Hill, (for I am willing to think that is his meaning, and that 'tis by the Tranſcribers fault rather then his, that he reſolutely affirms the quite contrary) will by no means ſerve his turn: It being utterly improbable to imagine, that the Air contain'd in ſo little a veſſell as one of our Receivers can by its weight counter-balance ſo ponderous a Cylinder of Quickſilver: Whence we may be allowed to argue that the Air ſuſtains it by ſuch a Preſſure or Spring as we plead for, whether that proceed from the Texture of the Aeriall Particles, or from their Motion, or from both.

The Fourth and laſt of Mr. *Hobbs*'s principal Expoſitions is of that Experiment of ours, wherein 100. and odde pound weight being hung at the depreſſed Sucker, the Sucker was notwithſtanding impell'd up again by the Air to the top of the Cylinder. Of this *Phænomenon* (which has not hitherto prov'd unwelcome to the Vertuous) Mr *Hobbs* gives us the following account.

Page 22. *Hærent hic noſtri*: (which why he ſayes I know not) *quomodo hæc expedies tu?*

A. Expedivi ante. Aër enim à retractione Suctoris retrò pulſus, nec locum in mundo (ut ſupponimus pleno) quò ſe recipiat inveniens, niſi quem ipſe, corpora contigua ſuis locis pellens, ſibi faceret, perpetuâ pulſione in Cylindrum tandem cogitur, tantâ velocitate inter Cylindri concavam & Suctoris convexam ſuperficiem, quanta reſpondere ſolet viribus illis magnis quas ad Suctorem revellendum neceſſarias experti eſtis. Aër autem ille, quâ velocitate ingreditur, eandem ingreſſus retinet, ſimulq; latera Cylindri æneï (vi elaſticâ præditi) undiquaq; diſtinct. Conatur ergo aër in Cylindro vehementer motus

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contra omnes partes ſuperficiei Cylindri concavæ; fruſtra quidem dum Suctor retrahitur: ſed quamprimum Sucter manu emiſſus Aërem impellere ceſſat, Aër ille qui ante incuſſus erat, propter conatum in omne punctum ſuperficiei Cylindri internæ & vim Aëris elaſticam, inſinuabit ſe inter eaſdem ſuperficies eâdem velocitate quâ impulſus fuerat, id eſt, eâ velocitate quæ reſpondet viribus impulſionis. Si ergo tanta ponderis vis Suctori appendatur quanta manuum vis erat quâ impellebatur, velocitas quâ idem Aër è Cylindro exit, locum in mundo pleno nullam habens quò ſe recipiat, Suctorem rurſus ad Cylindri ſummitatem impellet, propter eandem cauſam quæ effecit ut Suctor paulo ante impulerit Aërem. Thus far our Authors paſſage: againſt whoſe ſolution 'tis eaſie to draw divers Arguments from what we have diſcourſt againſt the firſt of his four Explications. But though we refer you thither, yet we will here alſo obſerve, that this whole conceipt of the Aires running in and out with ſtrange velocity between the Sucker and the Cylinder is precarious; nor does he propoſe any one *Phænomenon* to countenance it. To which generall Advertiſement I ſhall adde the three following particulars. *Firſt*, that in an Engine ſo contrived, that the Pump lay cover'd with water, when the Sucker was retracted, the Atmoſphere would ſtrongly preſſe the water againſt it; and if the *Manubrium* were let go, would ſwiftly enough repell up the Sucker into the deſerted cavity of the Cylinder. Which being a caſe Parallel to that under conſideration, let any unbiaſs'd Perſon judge how likely it is, that the Air could perform all theſe Excurſions without exciting bubbles, notwithſtanding the Waters conſtant interpoſition betwixt it and the Cylinder. *Secondly*, that there is as little probability in what our Author teaches in thoſe words, *Conatur ergo Aër*, &c. I might here repeat what we formerly mention'd of the breaking of our Receivers inwards, not outwards; and I might adde, that I ſee no reaſon why the *Conatus* of the Included Air, if its *Conatus* were granted, ſhould be fruſtraneous, when the Sucker is depreſt. But I will rather demand, Why, if the Air within have ſo ſtrong an endeavour outwards, as to

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ſtretch the thick ſides of the Braſſe-Cylinder, as Mr. *Hobbs* (with what probability, let any man judge) would have it; I demand, I ſay, why this Air does not throw out the Wooden Peg or Valve, which we have often to our trouble ſeen thrown out with great force and noiſe, when the depreſt Sucker being thruſt up again whiles there was Air in the Cylinder, we forgot to leave the Valve open; though in this caſe the Air that drove out the Peg was far enough from ſtretching the Cylinder. And I further demand, how it comes to paſſe, that, if having ſtopt the hole of the Cylinder with your finger inſtead of the Peg, you ſwiftly depreſſe the Sucker, you ſhall be ſo far from feeling a Preſſure outwards againſt the Pulp of the finger from any thing contain'd in the cavity of the Cylinder, that your finger will be ſtrongly, and perhaps not without ſome pain, preſt in by the ambient Air; in ſo much that 'twas this *Phænomenon*, and one ſomewhat like it in the *Torricellian* Experiment, that ſeem to have ingag'd my other Adverſary, the Learned *Linus*, to maintain a conceipt quite contrary to Mr. *Hobbs*'s, and imagine in the deſerted cavity of the Cylinder, not a diſtending, but violently contracting ſubſtance. *Thirdly*. That as to the laſt part of the paſſage under conſideration, beginning at *ſi ergo*, &c. which ſeems to me ſomewhat intricate, I do not ſo clearly underſtand why the Air that is impell'd in ſo ſwiftly betwixt the Cylinder and the Sucker, ſhould not reſiſt the ſwift Egreſſe Mr. *Hobbs* aſcribes to the included Air by the ſame paſſage: nor why this impell'd Air, that has ſo ſtrong an endeavour outward, ſhould never depreſſe the Sucker (againſt whoſe upper part it muſt bear as well as againſt the Cylinder) as well as the ſame Air diffuſing its Motion through the vaſt ambient *Medium*, can enable the external Air to thruſt up the Sucker again; eſpecially, ſince during ſuch a depreſſion of the Sucker (as we have mention'd not to happen) made by the Rebound of the Air, forcibly impell'd in from the cloſe bottom of the Cylinder, the Air from without may all the while, with congruity enough to Mr. *Hobbs*'s principles, get in between the

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ſaid Sucker and the Cylinder. But not to inſiſt upon theſe niceties: I ſay, that the lifting up of the Sucker either is not neceſſary to prevent a *Vacuum*, or that in ſome caſes it will be hard to ſhew how a *Vacuum* can by Mr. *Hobbs* be avoided. For when the depreſt Sucker is ready to be thruſt up again, if you hang a ſomewhat greater weight at it then an hundred and odde pounds, it ſhall not be lifted up at all. And I ſhall make this further improvement of the Experiment under conſideration, that whereas this progreſs and regreſſion of the impulſe of the external Air cannot reaſonably be ſuppos'd to be very laſting, you may by a competent weight detain the Sucker depreſt, till the ambient Air is as quiet as it uſes to be; and yet if then you take off the overplus of weight, and perhaps a little more, in caſe the Pump have not been very ſtanch, the Sucker and the great weight appended will notwithſtanding be carried up: which 'tis no way likely it could be by the impulſe of the outward Air, which had time to decay and be confounded. And as for the inward Air, beſide that we have proved, that it has no ſuch *conatus* outwards as Mr. *Hobbs* pretends, why ſhould not that, were it granted, throw out the Sucker rather then cauſe it to be impell'd inward; it being no way likely, that in caſe ſome Air ſhould get out of the cavity of the Cylinder, it could ſo move the outward Air, as that the reflex of that impulſe ſhould make that free outward Air bear more ſtrongly againſt the outſide of the Sucker then the inſide of the ſame Sucker is preſt againſt by the included Air, whoſe *impetus* is incomparably leſs diffuſed? But not to be thought a more nice and diligent Opponent then the matter requires, I ſhall drive this Diſcourſe no further: but rather deſire it may be obſerv'd in general, that whatever be reſolv'd to be in the cavity of the Cylinder when the Sucker is depreſt; yet ſince 'tis manifeſt, that it is at leaſt in great part devoid of common Air, and ſince the Sucker with the appended weight may, if the Inſtrument leak not, be impell'd up, when in all probability thoſe forced Undulations of the Air, that may be ſuppoſed to have been made by the Sucker, have ceaſt; the *Carteſians*,

Mr. *Hobbs*, and thoſe others that will not have recourſe to the un-intelligible attraction of ſome rarefied ſubſtance within, muſt aſcribe ſo ſtrange a *Phænomenon* to the preſſure of the Air without. But I ſhall no further preſs this Fourth Objection, partly becauſe 'tis added to the other three onely *ex abundanti*, and partly becauſe this Chapter is grown ſo long already.

I know indeed that after the Expoſition laſt recited out of Mr. *Hobbs*, he makes the Academian Dialogiſt confeſs, that the reſt of the *Phænomena* of our Engine may alſo not uneaſily be reduc'd to his principles. But perhaps they that take notice of the variety of thoſe *Phænomena* we have ſet down in our Treatiſe, will ſcarce be of his mind; and thoſe that have conſidered what has been diſcours'd in this Chapter againſt his four principal Explications, and what I am about to ſubjoyn in the following part of this Treatiſe, concerning divers other Solutions that he gives, will perhaps be inclined to think that others may be like theſe, without being therefore neceſſarily true.

CHAP. V.

In which divers ſcattered Explications and other paſſages in Mr. Hobbs's *Dialogue are examined.*

I Proceed then to the Fifth Chapter, in which and the next I glean up and examine divers ſcatter'd paſſages, whererein he offers at ſomewhat by way of Argument againſt ſome things we had delivered in our Epiſtolical Treatiſe: I ſay, what he offers by way of *Argument*; for as to thoſe paſſages that do but either praiſe himſelf or diſparage his Adverſaries, I have almoſt as little leiſure as inclination to take notice of them, and do not much apprehend that the *Virtuoſi* (eſpecially ſuch as know us both) will think what I write the leſs rational for being civil; or will let me ſuffer in their Opinions for neglecting to trouble them in a Philo-

Philoſophical Cóntroverſie, with matters that do but very little belong to it.

To skip then what Mr. *Hobbs* is pleaſed to ſay in the firſt Page of his Dialogue, concerning ſome diſputable diſcoveries about Senſation, which he challenges to himſelf; and to paſs by divers other things in the ſecond or third following pages, which relate to him, or to the Society he writes againſt, rather then to the nature of the Air; we ſhould begin with the Opinion he thinks fit in the fifth page to impute to us, as if we diſtinguiſh'd what is fluid from what is not ſo, onely (for ſo his Ratiocination imports) by the bigneſs of the parts of which a Body conſiſts: But deſigning in an *Appendix* to be ſubjoyned to this Diſcourſe to examine what I find in this Dialogue diſperſed touching Fluidity, I ſhall now onely ſay, that he does very much miſtake and miſ-repreſent my Doctrine of Fluidity; wherein I expreſly teach, That the principall cauſe or condition of it is not the ſize, but the motion of the ſmall parts that compoſe the fluid body.

To take up then the particulars we are to examine, in the order (as far as conveniently may be) wherein I find them lye in the Authors Dialogue, and paſſing by at preſent thoſe things which either we have conſidered already, or are not to conſider in this place: The firſt particular that offers it ſelf to be taken notice of, is this paſſage at the bottom of the twelfth page; Pag. 12.

B. In vas apertum infudimus aquam, in aqua fiſtulam ſtatuimus erectam, longam, exiliſsimam; obſervavimus autem aquam è vaſe ſubjecto in erectam fiſtulam aſcendiſſe.

A. Nec mirum: nam ſuperficiem aquæ, particulæ aëri interſperſæ aquæq; contiguæ motu ſuo verberabant, ita ut aqua non potuit in fiſtulam non aſcendere, & ſenſibiliter quidem in fiſtulam valde angustam.

To this I ſay, that 'tis manifeſt by what I write in my Epiſtle, that I did not then take upon me, nor do I undertake in this place, to aſſign the true reaſon of the propos'd *Phænomenon*. An Attempt of this kind has been ſince addreſs'd to me, which being ingenious, if not alſo true, may be conſulted. In the mean

time I cannot but declare that I am no way ſatisfied with Mr. *Hobbs*'s his Expoſition: For, to ſay nothing of the motion he aſcribes to the particles diſperſed through the Air, he leaves the difficulty unſolved, ſince there being common Air as well within the Cavity of the ſlender Pipes as without it, he neither ſhews, nor ſo much as offers at, a reaſon why the preſſure of the Air within ſhould not reſiſt the preſſure of the ſame kind of Air without; as we ſee it does in greater Pipes. And poſſibly he would have paſt by this particular, if he had not over-look'd the Advertiſement I gave towards the cloſe of the 35. Experiment, That it would concern thoſe who ſhould undertake to ſhew the cauſes of this *Phænomenon* to bethink themſelves alſo of a reaſon why, if the Experiment be tryed with Quickſilver inſtead of Water, the Surface of the Liquor will, inſtead of being higher, be lower within the Pipe then without it: Whereas if Mr. *Hobbs*'s Explication be ſufficient, why ſhould not the contrary happen in Quickſilver as well as in Water?

The next paſſage I have to conſider is in the 13. page thus ſet down; *Siquis poſt impulſionem revulſionemq; Suctoris aliquoties repetitam, Epiſtomium ſuperni orificii Recipientis conetur extrahere, inveniet illud valde gravitare, tanquam ſi multarum librarum pondus ab eo penderet. Unde contingit hoc?*

Page 13.

A. Ab aëris qui eſt in Recipiente fortiſſimo conatu circulari facto à violento ingreſſu aëris inter ſuperficiem Suctoris convexam & Cylindri concavam, generato per iteratam illam impulſionem revulſionemq; Suctoris, quam vos perperam vocatis exuctionem aëris. Nam propter naturæ plenitudinem, Epiſtomium extrahi non poteſt, quin aër qui eſt in Recipiente (Epiſtomio contiguus) una extrahendus ſit. Qui quidem aër, ſi quieſceret, facillime Epiſtomium ſequeretur: ſed dum velociſſimè circuit, ſatis difficulter ſequitur; id eſt, videtur eſſe valde gravis.

B. Veriſimile eſt: Nam ut aër novus in Recipiens paulatim admittitur, etiam apparentem illam gravitatem paulatim perdit.

But, I do not much fear that this Explication will keep the Experiment from continuing to be thought, by ingenious men, a notable

notalbe Confirmation of our *Hypotheſis*. For, to paſs by ſomething that, though I am no way ſatisfied with, cannot well be examined in few words; I anſwer, Firſt, that if there be ſuch a vehement circular endeavour as he imagines of the Air in the Receiver, by which motion he elſewhere teaches (as we have ſeen above) that the Air ruſhes out with violence enough to make the Atmoſphere lift up in our Cylinder above an hundred pound weight; I ſee not why it ſhould not rather throw out the ſtopple under conſideration, then hinder its extraction. And I ſee not why, when the external Air is re-admitted at the ſtopcock into the exhauſted Receiver, and thereupon there does ſenſibly follow for a little while a whirling about of the included Air, the ſtopple, that juſt before ſeemed ſo much to reſiſt the being drawn out, ſhould ceaſe to make any ſuch reſiſtance. Nor do I ſee how the plenitude of Nature ſhould, as is here intimated, hinder the extraction of the ſtopple: For, according to the Pleniſts, the World and the Receiver muſt be at all times equally full. And if the contiguous Air muſt for Mr. *Hobbs*'s reaſon neceſſarily be extracted with the ſtopple in one caſe, I ſee not why the like ſhould not happen in another. But ſince Mr. *Hobbs* is pleaſed to call us Experimentarian Philoſophers, let us ſhew that ſuch Explications as theſe of his need not make us aſham'd of the name. I ſay then, that it appears by our Experiments that there is no ſuch *Fortiſſimus conatus circularis* in the exhauſted Receiver as he pretends; but that there is indeed an endeavour of the ambient Atmoſphere to preſs inwards the parts of the Glaſs and Cover that are contiguous to it. For, as I have alſo noted already, a light Bladder ſuſpended in the cavity of the Receiver betrayed no ſuch motion as Mr. *Hobbs* here ſuppoſes. To which I ſhall now adde, that neither were a pair of Scales ſuſpended within the ſame Cavity; nor was a long Magnetical Needle, that reſted upon the point of another Needle, at all whirled about by this imaginary motion of the Air. Beſides, if you leiſurely looſen the Braſs ſtopple, ſo that it may be very near, but not contiguous, to the ſides of the Socket, you ſhall manifeſtly perceive

perceive a ſtrong current of Air to flow into the Receiver at that paſſage: And more then once, when inſtead of that piece of Braſs we ſtopt the hole in the Cover with our Cement, one might obſerve ſometimes whileſt we were pumping, ſometimes after we had done pumping, that the outward Air by degrees depreſs'd the *ſuperficies* of the Cement expos'd to it, and made it concave, and now and then would break through it, thruſting it inward with great violence and noiſe.

In the ſame page our Author rectifies, after his way, another of our Explications in theſe words; *Vidimus item aquam demiſſam in Recipiens poſt Suctoris aliquot reciprocationes ita bullire, ac ſi ſuppoſito igne ferveſceret.*

Pag. 13.

A. Id quoq; accidit propter velocitatem aëris, ut dictum eſt, in Recipiente circumeuntis; niſi forte aquam illam dum bullit calidam quoq; eſſe deprehendatis. Nam ſi certi eſſemus illam caleſcere, alia cauſa Phænomeni excogitanda eſſet.

B. Imo certi ſumus quod non caleſcit ſenſibiliter.

A. Quid ergo tali aquæ motui conferre poſſe putas majorem vel minorem Atmoſphæræ gravitatem?

B. Neq; illum motum attribuunt, puto, Atmoſphæræ.

But, I confeſs, I ſee not how the circular motion of the Air within the Receiver could in a Vial with a long neck produce ſuch effects as in my Epiſtle are recited: eſpecially I ſee not how ſuch a wind paſſing along the ſurface of the Water could raiſe ſo many and ſo ſtrangely-big bubbles, which ſeemed many of them to riſe from the lower parts of the Water, and ſwell'd notably as they aſcended; and how ſuch a wind ſhould carry up the moſt part of the Water through the long neck of the Vial, and as it were ſpout it into the Receiver.

As for what he ſayes about the gravity of the Atmoſphere, 'tis plain enough that my conjecture aſcribes the *Phænomenon* to the taking off, not the gravity of the external Air, but of the preſſure of the Air within the Receiver: though I ſee not why the removal of the weight of the Atmoſphere, if it could be out of the Engine effected, ſhould not have a like operation.

And

And (to anſwer Mr. *Hobbs*'s Queſtion as it ſhould have been put) that which I think the greater or leſſer preſſure of the Air confers to this *Phænomenon* is this, That whereas common Experience ſhews us, that water by being heated is expanded, and has bubbles generated in it; and whereas our former Experiments, eſpecially the 28. have made it appear, that there is wont to be in Water and other liquors Aerial Particles, which tend to expand themſelves, and do actually do ſo, in numerous bubbles, when the preſſure of the incumbent Air is conſiderably leſſened: In the preſent *Phænomenon* that preſſure being by the exhauſtion of the Receiver taken off, the Aerial particles and agitated Vapours that abound in the hot water are allowed to expand themſelves, as before they could not, and to make ſuch numerous and great bubbles, that thereby a good part of the water is carried out of the Vial. So that I ſomewhat wonder what makes Mr. *Hobbs* ſpeak as if there were no ſenſible heat of the water under conſideration, ſince 'tis expreſly ſaid that it was put in hot; and if it were put in cold, could by no pumping be brought to the leaſt ſhew of Effervveſcence. And as for his Explication of the *Phænomenon*, the Experimentarian Philoſophers need not the Objection lately made againſt it: For I have already evinced by Experiments, that there is in our exhauſted Receivers no ſuch peculiar motion of the Air as he aſcribes the *Phænomenon* to; nay, when there is manifeſtly a whirling about of the Air in the Glaſs upon the admiſſion of the external Air, the production of numerous bubbles in the water preſently ceaſeth. And therefore I ſee not why Mr. *Hobbs* might not have let alone my Conjecture, (for I propos'd it, and look upon it, as no more) unleſs he could either have diſproved it better, or ſubſtituted a more probable one then he has done in its place.

As for what he adds in theſe words, *Ab hoc experimento manifeſtum eſt, quod Recipiens per exuctionem hanc quam vocatis Aëris, non ſit vacuum. Nam moveri aqua non potuit niſi à movente aliquo moto & contiguo. Itaq; Phænomenum hoc demonſtrationem ſuppoſitionis meæ continere videtur non infirmam.* Page 3.

I

I am not obliged to anſwer it, but leave that to thoſe that are profeſt Vacuiſts; againſt whom I muſt doubt whether his Ratiocination will conclude, though the conſequence be not manifeſt to me. For himſelf allows his Terreſtrial Atoms an innate circular motion, which conſequently needs not depend upon ſome body contiguous and moved; and the Vacuiſts will ſay, that the particles of the water being ſtrongly agitated when it was put into the Receiver, (whether by fiery Corpuſcles ſwarming in it, or otherwiſe) and the reſiſtance of the incumbent Air being taken off, the *Phænomenon* would be produced juſt as it is, though we ſhould ſuppoſe no other body to ſucceed in the room of the exhauſted Air. And beſides, though ſome ſubtile particles of active matter ſhould get in to agitate the Aqueous and Aerial Corpuſcles, yet (they may ſay) there is no neceſſity that ſuch minute particles ſhould be numerous enough to *fill up exactly* all the little ſpaces deſerted by the Air. And even upon this ſuppoſition, as it would not follow that ſuch relinquiſh'd ſpaces were all of them quite empty; ſo would not the *Phænomenon* at all prove, much leſs manifeſtly prove, that they were *quite full.* And ſince an actual heat, that is, a briſk and various Agitation of its ſmall parts, is requiſite to the boyling of the water in this Experiment; perhaps others will not think it more abſurd, that the removal of the preſſure of the Air ſhould occaſion this expanſive motion in the water, then that which Mr. *Hobbs* muſt allow, that in Air compreſt by Quickſilver, or ſome other weight, the removal of that preſſure is ſufficient to make that Air expand it ſelf by the flying abroad of its parts.

And whereas Mr. *Hobbs* urges this other Argument againſt the Vacuiſts, *Præterea dic mihi, bullientem aquam potuiſtin' conſpicere?*

Page 13, 14.

B. Quidni?

A. Nonne viſionem fieri concedunt veſtri per actionem continuam ab objecto ad oculum? Nonne etiam putant actionem eſſe motum, & omnem motum eſſe corporis? Quomodo ergo potuit ab objecto, nempe aqua, ad oculos tuos motus per vacuum (id eſt, per non corpus) derivari?

B. Non

B. Non affirmant nostri ita vacuum esse Recipiens, ut nullus omnino aër relictus sit.

A. Nil refert an totum Recipiens vacuum sit, an magna ejus pars; nam utrumvis supponatur, derivatio motus ab objecto ad oculum intercipietur.

B. Ita videtur, nec habeo quod respondeam. The Vacuists will perhaps answer him as I answered *Franciscus Linus* to an almost like Objection. And those of them (which make far the greatest number) who plead but for an interspersed *Vacuum*, will perhaps tell him, that they take Vision to be made, not by such a Propagation of Impulse as he does, but by a Trajection of *Effluvia*, that issuing out of the Sun, and traversing the Diaphanous bodies interpos'd, rebound from the object to the eye. And according to this Doctrine they may ask Mr. *Hobbs*, why a motion may not be made through a *Vacuum* or *non corpus*: nay, how it can naturally be stopt *in vacuo* where there is nothing to resist it. But Controversies of this nature it lyes not upon me to prosecute.

In the 14. page Mr. *Hobbs* having recited that Experiment of ours, of killing Animals included in our Receiver by the exhaustion of the Air in two or three minutes of an hour, subjoynes these words, *Credin' tu animalia ista tam cito interempta esse eo quod carerent aëre? Quomodo ergo sub aquam vivunt Urinatores, quorum aliqui (assueti à pueritiâ) caruere aëre per horam integram? Inclusa in Recipiente animalia occidit motus ille idem vehementissimus, quo distenduntur rumpunturq; inclusæ vesicæ.* Page 14. But, though he sayes no more in this place concerning this matter, yet it seems he either much liked his own Conjectures, or greatly disliked mine, since in his Epistle Dedicatory to the learned *Sorberius* he singles out this sole *Phænomenon* to explicate; *Ego contra* (sayes he there) *neq; aërem exugi posse, neq; inclusum animal (etsi exuctus esset) tam cito moriturum esse existimo. Actio quidem quam mors illa sequitur videri potest vel suctio quædam (& propterea exuctione conclusi aëris interfici animal, respiratione sublatâ) vel etiam compulsio aëris ab omni parte versus centrum sphæræ*

cui animal includitur; & sic videri potest mori à tenacitate compressi aëris, quasi aqua suffocatum; nimirum haustum in intima pulmonum aërem solito tenaciorem, inter arteriam & venam pulmonis, cursum sanguinis intercipiendo sistere. But, as I proposed my Conjecture doubtingly, and profess my self to be in a further enquiry about the use of the Air and of Respiration; so I must still think, that we want some further or clearer discovery about that matter, notwithstanding what has been delivered concerning it by Mr. *Hobbs*. For his Argument against my Conjecture is in the passage that proposes it answer'd by himself: for he plainly intimates that *Divers* who can live without Air (which yet I might question, if he means without any Air at all) for a whole hour, are accustomed to it from their Childhood. Wherefore, unless the Animals that dyed in my Engine had been for a long time framed by degrees to live without Air, it will not follow that the want of it could not dispatch them in a short time, as ordinary men may be drowned in a few minutes. And having purposely let down some Mice and small Birds into a deep Glass fill'd with water, and kept them from emerging by a Weight tyed to their legs or tayls, though some lived longer then others, yet I observed them to be kill'd fast enough to keep my Conjecture from being incredible: especially the last we made tryal of, though a large and lusty Mouse, appear'd to be quite dead within somewhat less then one minute, measured by the Vibrations of a *Pendulum*. And we particularly took notice, that before drowning divers bubbles, which seemed to be the respired Air, came out of their mouths, and ascended through the water.

And as for the Explication that Mr. *Hobbs* would recommend instead of mine, not to urge that I could wish he had been pleas'd to shew us how the tenacious Air he imagines to be inspired comes to produce those strange Convulsions and other Symptomes mentioned in my Epistle; not to urge this, I say, we have already disproved the supposition his Opinion is built on, namely, that there is in the exhausted Receiver such a *motus vehementissimus* as he pretends: besides that he shews not how this motion

motion comes to kill the included Animals which I was wont to keep, not near the Centre of the Receiver, where he seems to think this motion most operative, but near the bottom of it, that the included Animal might have something firm under his feet. Nor does it at all clear the difficulty, that he would have this motion the same whereby included Bladders are distended and broken. For, besides that 'tis very hard to conceive how the tenacity of the Air, or its beating from all parts upon the convex Surface of an almost quite empty Bladder, (for in such also the Experiment he refers to will succeed) should make it burst outwards; besides this (I say) we have already proved that the distension and breaking of Bladders in our Receiver proceeds not from any such motion of the neighbouring Air as is here presumed, but from a quite differing, if not from a contrary cause.

In the same page our Author makes a digression from the Engine, and discourses of another Experiment which I have long since often made: but though his Explication be lyable enough to just Exceptions, (as I can make good if it be required) yet because the Experiment is none of those I delivered, I shall leave it to be examined by others: and for the same reason I forbear to meddle with that he has in the next page concerning the Wind-gun, as to what he has in the same 16. page in these words, *Placet mihi tua magis Hypothesis quam illa de vi aëris elastica: nam video quod à veritate illius veritas de-* Page 16. *pendet vel vacui vel pleni; sed à veritate hujus nihil sequitur in neutram partem quæstionis. Aëris, inquit, structura similis est compressæ lanæ. Bene est. Lana fit ex filis. Recte. Sed cujus figuræ? Si Parallelopipedi, nulla potest esse compressio partium: si non Parallelopipedi, erunt inter fila illa spatia quædam relicta; quæ si vacua sunt, supponunt Vacuum, ad probandum quod Vacuum est possibile; si plena, plenum dicunt quod vacuum putant.* To this passage, I say, I cannot but represent, that the Question is not, whether from the *Hypothesis* that ascribes a Spring to the Air, depends the proof of a *Vacuum* or a *Plenum*, but whether the *Hypothesis* it self be true or no. For, sure there are many things certain in

Natural Philoſophy, from whoſe truth that of a *Plenum* or a *Vacuum* cannot be deduc'd. And to what he addes concerning the ſtructure of the Aerial Particles, the Vacuiſts may tell him, that they make no ſuch Argument as he is pleaſed to make for them; and do not commonly imploy the Figure of the Aerial Particles to prove a *Vacuum*, but other Arguments, ſuch as Mr. *Hobbs* has not yet well anſwered: and having by them, as they judge, prov'd interſperſed Vacuities, they might without inconvenience ſuppoſe in an Aerial Corpuſcle little empty Pores, upon whoſe account it may be capable of compreſſion, in caſe they ſhould think fit (which I know not that any of them does) to aſſign it the Figure of a *Parallelopipedon*. But this Controverſie the Vacuiſts may, if they pleaſe, proſecute. In the ſame page Mr. *Hobbs* begins, and in the next he continues, a long diſcourſe concerning the going out of fire in our Receiver upon the exhauſtion of the Air: the paſſage is too prolix, and does too little concern the Spring of the Air to be here totally tranſcribed, or examined Period by Period. In ſumme, he endeavours to do two things: the one is to reduce what happens to kindled Coals placed in our Engine to what happens in certain Mines, wherein when ſome thick damps aſcend, both Charcoals and Candles are ſoon extinguiſhed thereby: the other is to ſhew, that by the Reciprocation of the Sucker, the Air impell'd firſt into the Cylinder, and then into the Receiver, is put into ſuch a motion as gives it a certain middle conſiſtence, as he ſpeaks, betwixt the conſiſtence of pure Air and that of Water. But I ſhall not need to examine this ſecond part of his diſcourſe, becauſe I deny the firſt; and being able to diſprove the thing it ſelf, namely, the thickneſs of the Air in the exhauſted Receiver, I need not ſpend time about what he teaches *de modo*.

To examine then onely the firſt of the above-mentioned particulars, I ſhall begin with obſerving that his ſtory of the damp to be met with in Mines is more largely ſet down by Mr. *Hobbs* in that Chapter of his Elements of Philoſophy, where he treats of Gravity; in which place he ſeems to mention it (to uſe his

own

own Expreſſion) as a ſtory of doubtful credit, which 'tis not like he would have done if he had then ſeen it. Which I mention, not that I deny the ſtory for the main, nor that I would bring Mr. *Hobbs* into a ſuſpicion of relating things untrue as matters of fact, his enemies themſelves having not accuſed him of ſuch a meanneſs; but becauſe, if he have not ſince obſerved the thing himſelf, there may eaſily be a miſtake in ſome of the circumſtances: as for inſtance, the number of Minutes wherein the thick Air choaks the fire; and 'tis upon that circumſtance that the validity of what he deduces from the obſervation chiefly depends. But, however the matter fare with theſe ſubterraneal Damps, we have already proved by ſeveral of the Experiments of our Engine, that in the exhauſted Receiver there is no ſuch motion of the Air as is here ſuppoſed. And it may be ſufficiently proved, that whatever remains in the Receiver is not ſuch a ſubſtance as Mr. *Hobbs* would have it: for that, he here tells us, is of a conſiſtence betwixt Air and Water; and in the above-cited place of his Elements he ſayes, that 'tis not much lighter then Water. But by the *Magdeburgick* Experiment (we have already had occaſion to mention two or three times) 'tis evident that the Receiver by being exhauſted of common Air is ſo far from growing heavier, much leſs ſo much heavier as it muſt if it were filled with a ſubſtance not much lighter then Water, that it loſt above an Ounce of its former weight. And to this agrees what we ſee happen in *Æolipiles*, that grow lighter when the Air is expell'd. Beſides, if the Receiver be in our preſent caſe filled with a ſubſtance whoſe conſiſtence is ſo much nearer that of Water then is our common Air, as Mr. *Hobbs* would have it; how chance a *Pendulum* ſhould not move very ſenſibly ſlower in it, when in Water the *Diadromes* are ſo exceedingly much more ſlow? And the breaking of an Hermetically-ſeal'd bubble in our Receiver outwards, when the Air was much exhauſted, and not before, together with divers other Experiments that might be eaſily applyed to this purpoſe, in our Epiſtle, do ſufficiently evince, that 'tis not a thicker and far heavier Air, but a more yielding

and

and lighter, that remains after Pumping in the Cavity of our Receiver. And thus much as to Mr. *Hobbs*'s discourse upon our Experiment. But as for the thing it self, it appears that when I related it I thought it might admit a further enquiry: And indeed there may be so many wayes of extinguishing Fire (as we see that the flame of a Candle may be blown out by the wind, or quenched in water, or put out by the compression of a pair of Snuffers, or suffocated for want of Air to receive its fuliginous steams, or (if that be a different way) stifled by the thick Exhalations of deep Mines or of new Wine) that as 'tis not in all cases so easie to assign the true cause of the extinction of fire, so 'tis unsafe to conclude with Mr. *Hobbs*, that because a Candle or a live Coal may a great way under ground be extinguisht by a thick Damp, therefore the effect must proceed from the like cause in our Receiver, where there is no sign of any Damp or unusual thickness of the Air, but of the contrary.

But let us follow Mr. *Hobbs* to the next passage, wherein he seems fond enough of playing the Censor. For, speaking of our 11. Experiment, wherein the Coals that seemed almost dead in our exhausted Receiver, being taken out into the Air began to shine again, having made his Academian Dialogist say, *Fuere eo-*

Page 18. *rum aliqui qui remansisse dixerunt in carbonibus illis (quanquam extincti videbantur) particulas quasdam igneas, quæ admisso aëre ventilatæ cæteram molem denuo accenderent:* The other (by whom Mr. *Hobbs* is meant) answers him, *Næ illi quæ dicerent non videntur cogitasse, sed sortitos esse.* This is very severe. But let us see what weighty reason he has to be so: *Credin' tu in*

Page 83 *carbone ignito partem aliquam non carbonem, sed ignem esse; aut in candente ferro partem inesse quod ferrum non sit, sed ignis?* But some will think that by these words he does rather propose then prove his Opinion: wherefore let us hear his Ratiocination, which he annexes in the following words, *Ab uni-*

Page 18. *ca scintilla magna urbis incendium nasci potest. Atqui si ignis corpus ab ignito diversum sit, non plures potuere esse partes igneæ in toto incendio quam in una illa scintillula. Videmus*

corpora

corpora diverſorum generum à luce Solis tam per refractionem quam per reflexionem factam in ſpeculis comburentibus accendi poſſe: neq; tamen quenquam eſſe credo qui putet particulas igneas à Sole ejectas tranſire poſſe per ſubſtantiam globuli cryſtallini. In aëre intermedio ignis nullus eſt. But I doubt his Adverſaries will ſay, that he is ſo far from having in this paſſage well confuted their Opinions, that he ſeems not to have well underſtood them: For they will tell him, that they teach not that the fire is materially different from every part of the fewel; but that the igneous Corpuſcles, whileſt they are divided, blended and oppreſt with the others, have not the power to ſhine or burn, till being by ſome ſpark or other body actually burning, or by ſome other equivalent cauſe extricated, they flock together in ſwarms, and then are able to burn and ſhine, that is, to appear fire: Which fire is yet but a part of the fewel; as appears by the Phlegme, Aſhes, and perhaps other incombuſtible parts of the Coal or other fewel. So that the Atomiſts and divers others will not allow what Mr. *Hobbs* infers about an *incendium*. And whereas he tells us he believes that no body thinks, *Particulas igneas à Sole ejectas tranſire poſſe per ſubſtantiam globuli cryſtallini*; he ſeems to me to have very little heeded the Epicurean *Hypotheſis*. For, not onely the learned *Gaſſendus*, but I know not how many other Atomiſts (beſides other Naturaliſts) Ancient and Modern, expreſly teach the Sun-beams to conſiſt of fiery Corpuſcles, trajected through the Air, and capable of paſſing through Glaſs; whereby theſe Authors give an account of thoſe *ſpecula uſtoria* that burn by reflexion. Theſe things I repreſent, not that I intend here to adopt the Atomiſts Opinion of the nature of Fire, of which I am not obliged to declare my thoughts *here*, and have done it *elſewhere*; but to ſhew that Mr. *Hobbs*'s Arguments are not a ſufficient ground for ſo heavy a Cenſure. And if a Coal be kindled at one end, though Mr. *Hobbs* would have the kindled end a Coal, not Fire; yet if he pleaſe to hold it in his naked hand, he may find that differs enough from the other end to deſerve another name. And I, that but related a *Phænomenon*, did

not

not perhaps expreſs my ſelf much leſs warily, if not more ſo, then Mr. *Hobbs* himſelf. For whereas my words are theſe,

Page 79. *We preſently took out the Coals, in which it ſeems there had remained ſome little parcels of fire, rather covered then totally quench'd: for in the open air the Coals began to be re-kindled in ſeveral places:* Mr. *Hobbs* even in his Elements of Philoſophy ſpeaks thus upon a ſomewhat-like occaſion, *If a*

Chap. 30. Sect. 14. *Grate filled with Coals throughly kindled, and burning never ſo brightly, be let down, as ſoon as ever it is below* C. *the fire will begin to grow pale, and ſhortly after (loſing its light) be extinguiſht, no otherwiſe then if it were quencht in water: but if the Grate be drawn up again preſently, while the Coals are ſtill very hot, the fire will by little and little be kindled again, and ſhine as before.*

As for the reaſon Mr. *Hobbs* aſſigns of our Experiment in the

Page 18. lately mentioned paſſage of his Dialogue, being grounded upon ſuch a thickneſs of the Air in the Receiver as we have already diſproved, it needs not to be examined. And laſtly, as to what he ſubjoynes in theſe words, *Quan-*

Page 18. *do autem eſt quod de homine vere pronunciare poſſumus quod eſt mortuus, ſive (quod idem eſt) animam expiravit. Cognitum enim eſt homines nonnullos pro mortuis habitos, poſtridie elatos revixiſſe.*

A. De puncto temporis quo anima à corpore ſeparatur difficile eſt ſtatuere. Perge igitur ad experimenta alia.

I confeſs I ſee not why that needleſs Queſtion might not have been well ſpared, if he deſigned to give it no better Anſwer.

CHAP.

CHAP. VI.

Wherein other paſſages of Mr. Hobbs's *Dialogue that con-cern the Author are examin'd.*

WHat our Author has in the 19. page concerning a Bladder has been already examined, wherefore I proceed to the next paſſage in the ſame page, which is this;

B. Si acus magnete excitus libere pendeat intra Recipiens, ſequetur tamen ille motum ferri quod circumducitur extra Recipiens. Item objecta intus poſita ab iis qui extra ſunt videbuntur, & ſoni intus facti audientur, omnia hæc æque poſt atq; ante exuctionem aëris, niſi quod ſoni ſunt aliquanto poſt quam ante debiliores.

A. Manifeſtiſſima hæc ſunt ſigna Recipientis ſemper pleni, nec poſſe inde exugi aërem. Quod autem ſoni inde ſentiantur debiliores, ſignum eſt conſiſtentiæ aëris. Conſiſtentia autem aëris à motu ejus eſt per lineas diametraliter oppoſitas. But I meet with few of the Vacuiſts, who, even in the *Torricellian* Experiment, think the place relinquiſhed by the Quickſilver to be perfectly void, moſt of them allowing, that though it be not quite full of body, yet it may contain ſome of the Earth's magnetical ſteams, or of thoſe igneous Corpuſcles that flow from the Sun, or both of them. Now againſt theſe who would from our Experiments deduce but onely an interſperſed *Vacuum*, I ſee not that the *Phænomena* mentioned by Mr. *Hobbs* do conclude half ſo manifeſtly as he pretends: For, as to the motion of the Needle within the Receiver, 'tis known that they are wont to aſcribe Magnetical Attraction to certain *Effluvia*, that iſſuing out of the Loadſtone are ſubtile enough to paſs through the Pores of the cloſeſt bodies, without excepting Glaſs; ſo that although the Receiver were quite empty'd before, the Needle might be wrought upon by Magnetical Corpuſcles, that need not be ſuppoſed to fill the 10. part of the Receiver. I know indeed that Mr. *Hobbs* has ano-

ther *Hypotheſis* of the *Phænomena* of the Loadſtone, but I know that divers learned Writers have abſolutely rejected it, and not one ſuch that I have heard of has approved it. And as for the other two *Phænomena* here mentioned by Mr. *Hobbs*, the Atomiſts may anſwer, That the firſt (touching Objects ſeen in the Receiver) has been ſhewn already not to overthrow their Doctrine: and that the other (concerning the Debilitation of Sounds) makes againſt him, not for him; ſince we have already diſproved that conſiſtence of the Air whereto he aſcribes it. And the ſame Arguments that overthrow that Opinion may make it ſeem ſomewhat ſtrange, that he ſhould ſubjoyn our Experiment of two like *Pendula*, whoſe Vibrations we found not manifeſtly to differ within and without our exhauſted Receiver. For the former ſhould move far ſlower then the other, according to Mr. *Hobbs*'s Conceit, that the Receiver, when we ſay 'tis exhauſted, is filled with a ſubſtance of a middle conſiſtence betwixt pure Air and Water, and not much lighter then Water. But whether the Receiver be in ſuch caſes adequately full or no, the Vacuiſts may further conſider. For its being granted to be full would not overthrow either of my *Hypotheſes*, namely, the Weight and Spring of the Air.

Compare that in the 30 Chap. 14 Sect. of his Phyſicks with that of his Dialogue, page 15, 16.

In the ſame 19. and ſome following pages Mr. *Hobbs* has a long Diſcourſe againſt my Conjecture at the reaſon I propoſe in my 31. Experiment, why (as I there expreſs it) if the exquiſitely-poliſht Surfaces of two flat pieces of Marble be ſo congruous to each other, that upon their mutual application there reſults an immediate contact, they will ſtick ſo faſt together, that he that lifts up the uppermoſt ſhall, if the undermoſt be not exceeding heavy, lift up that too, and ſuſtain it aloft in the free Air. The Conjecture it ſelf is in the ſame page thus ſet down, *That*

Page 6. *the lower ſuperficies of that* (undermoſt) *ſtone being freely expoſed to the Air, is preſt upon by it; whereas the uppermoſt ſurface being contiguous to the ſuperiour ſtone, is thereby defended from*

from the preſſure of the Air, which conſequently preſſing the lower ſtone againſt the upper hinders it from falling, as we have elſewhere more fully declared. Which laſt words I therefore omit not, becauſe they ſhew that I handle this matter in this place but incidentally, and may make uſe of what I have deliver'd where I treat of it more expreſly; as I have ſince done in Print in the Hiſtory of Fluidity and Firmneſs, which Mr. *Hobbs* appears to have ſeen by thoſe cenſures of ſome paſſages of it that I ſhall hereafter examine.

His whole Diſcourſe concerning my Conjecture, and his Scheme, would be too prolix to be entirely inſerted. But the thing his Diſcourſe drives at is to ſhew, that neither the Spring nor Weight of the Air have any thing to do with this *Phænomenon:* and therefore when he had made his Academian relate, that two coherent Marbles ſuſpended in our Receiver did not fall aſunder upon the exhauſtion of it, he ſubjoynes that it was, *Quia nihil iſtic erat quod ageret Atmoſphæræ pondus*; and annexes, *Experimento hoc excogitari contra opinionem eorum qui Vacuum aſſerunt aliud argumentum fortius aut evidentius non potuit.* Page 10. *Nam ſi duorum cohærentium alterutrum ſecundum eam viam in qua jacent ipſæ contiguæ ſuperficies propulſum eſſet, facile ſepararentur, aëre 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.* But how this ſhould be ſo cogent and manifeſt an Argument againſt the Vacuiſts I confeſs I do not well diſcern. For that which it proves (if it prove any thing) ſeems to be, That in caſe the cohering Marbles could be ſo ſevered as to loſe at once their whole contact, the world might be concluded not to be full: But I ſee not how it thence follows, that therefore there can be no *Vacuum*. For my part I would demand, whether the ſo ſtrong coheſion of the Marbles be neceſſary or not to the plenitude of the world. If it be, how chance a ſufficient weight hung to the lower

Marble can immediatly draw them asunder? and if it be not, why does not Mr. *Hobbs* assign some other cause of their so strong adhesion, if it depend neither upon the Spring nor Weight of the Air? As for the non-separation of the two Marbles in our Receiver, I have said in the same 31. Experiment, that the cause may probably be the pressure of the Air remaining in the Receiver not sufficiently exhausted. And this Conjecture I have more fully defended in what I have written about it against *Franciscus Linus*, where I shew, that 'tis no way unlikely the remaining Air should be able to sustain a weight of four or five Ounces hanging at the lower Marble, since the free Air was able to support between 400. and 500. Ounces hanging at the same.

But Mr. *Hobbs* tells us, that the cause I assign of the cohesion of our Marbles is lyable to huge inconveniences; of the greatness of which we may judge by the first of them, *Confitentur* (sayes he) *tum ipsi tum alii omnes, ponderationem omnem conatum esse per lineas rectas undiquaq; ad centrum terræ; & proinde non per Cylindrum vel Columnam fieri, sed per Pyramidem, cujus vertex est centrum terræ, basis pars superficiei Atmosphæræ.* As if it were much material whether a body whose Basis is scarce two Inches Diameter, and whose length amounts to some thousands of Miles, be considered as a Cylinder or a Pyramid. Certainly *Stevinus* and other learned Writers of the *Hydrostaticks* would scarce have made this an Objection, since they scruple not to make it a *postulatum*, that all not very distant Perpendiculars be looked upon as parallel, though they allow such Perpendiculars would meet in the Centre of the Earth. What he addes partly in these words, *Conatus ergo punctorum omnium ponderantium propagabitur ad superficiem Marmoris superioris antequam possit propagari ulterius (puta) ad terram*, and partly in the following lines, to prove that the whole endeavour of the Pyramid that leans upon the upper Marble is terminated there, and that there is no endeavour of the Atmosphere against the under-superficies of the lowermost Marble; This Ratiocination seems grounded, partly upon a Conceit of his

Page 20.

Page 20.

about

about the nature of Gravity, according to which I see not why any Body plac'd between the sides of that Pyramid, or rather Cone, whereof the upper-superficies of the higher Marble is the Basis, should descend upon the account of gravity; and partly from a mistake of my Opinion: for I do no where, that I know of, speak as if I thought this sustentation of the lower Marble were performed by little Globuls or other minute bodies protruding one another directly towards the Centre of the Earth, and rebounding from a perfectly smooth superficies; nor need I say, that the lower stone is sustained by the pressure of the self-same pillar of the Atmosphere that is incumbent on the upper, since other parts of the Atmosphere, some on the one hand and some on the other, pressing obliquely upon the uneven surface of the Earth, may have their pressure upward terminated against the lower surface of the undermost Marble. And in the mentioned History of Fluidity and Firmness, speaking (*pag.* 187.) of the adhesion of flat Glasses, (and the reason is the same in our flat Marbles) I plainly deduce it from the pressure of the fluid Air, which, like a liquor, diffusing it self upon the surface of the Terrestrial Globe, because its descent is there resisted, does, like water and other liquors, press almost equally every way, and strongly endeavour to thrust away any body against which it can bear; so that whereever the pressure is taken off from one part of a body and not from the opposite, that body will be prest toward that part, whether it be downwards, or sidewards, or upwards, where that formerly-equal resistance is removed. And this Explication I do in that Discourse back with Considerations and Experiments, which Mr. *Hobbs* is not pleased to take any notice of; wherefore I hold it not amiss to adde here two or three other Experiments to one of those mentioned there.

First then, an *Æolipile* being by heat freed from Air as much as you can, if the little hole at the extremity of the neck, by which the Air gets in and out, be presently and carefully stopt with Wax, and afterwards suffer'd to cool, there will not be in the Cavity of the *Æolipile* a resistance any thing near equal to

the

the preſſure of the outward Air. And therefore if you perforate the Wax, that Air will violently be impell'd in at the unſtopt Orifice, whether the neck be held parallel or perpendicular to the Horizon, or in any other ſituation in reſpect of the Centre of the Earth. And the like will happen if the *Æolipile* be unſtopt under water.

Next, I relate in the mentioned Hiſtory, that having drawn ſome of the Air out of a large Glaſs with a narrow mouth, and thereby deſtroyed the equality of force betwixt the weight of the outward Air, and the now weakned ſpring of the inward; I found that by immediately applying a flat body to the Orifice of the Veſſel, that body was readily lifted up and ſuſtain'd in the Air as long as I pleas'd, though the weight of it exceeded 20. Ounces.

Thirdly, I lately met with the relation of an Experiment, which does abundantly make out the power of the ambient Atmoſphere to preſs bodies againſt each other, when it cannot get between their internal ſurfaces. For the ingenious Author of the *Magdeburgick* Experiment writes to the induſtrious *Schottus*, that having cauſed two Copper Plates to be made almoſt in the form of Scales a little above half an Ell in Diameter, and exactly congruous if laid upon one another; *Has* (ſayes our Jeſuite) *ſi mutuo ſibi imponit, & aërem extrahit, adeo ab externi aëris gravitate compreſſæ atq; unitæ tenentur, ut ſex viri robuſti eas divellere non poſſint. Quod ſi tandem adhibito omni conatu divellantur, crepitum edunt ſclopeti aut Muſquetæ exploſioni non minorem; quamprimum vero per claviculum ſeu Epiſtomium apertum vel minimus aditus laxatur aëri, ſponte ſeparantur.*

Schot. Mech. Hyd Pneumat. pag. 461.

And, Fourthly, if a Glaſs-Vial (ſuch as will be anon more fully deſcrib'd) have a Pipe open at both ends ſo fitted into it, that no Air can get in or out betwixt the neck and it; and if the Vial be ſo far filled with water, that the lower end of the Pipe be well immerſed therein; if then you ſuck at the upper end of the Pipe, the water will aſcend to a good height: which argues its being forc'd

forc'd upwards by the oblique preſſure of the Air in the Vial; for 'tis onely in the Pipe and not in the Vial that there is any Air in the ſame Perpendicular with the Water that is impell'd up.

But let us follow Mr. *Hobbs* a little further. Having ask'd this Queſtion, *Sed vis illa elaſtica quam in aëre eſſe dicunt, nihilne ad marmor ſuſtinendum conferre poteſt?* he anſwers, *Nihil omnino; non enim conatus in aëre eſt ullus ad centrum terræ magis quam ad aliud quodvis punctum Univerſi. Quoniam enim gravia omnia tendunt à circumferentia Atmoſphæræ ad centrum terræ, & inde rurſus ad circumferentiam Atmoſphæræ per eaſdem lineas reflexas, conatus ſurſum conatui deorſum æqualis erit, & proinde mutuo ſe perimentes neutrâ conabuntur viâ.* But that the Spring of the Air may perform ſomewhat in the caſe propoſed, I hope the newly-mention'd Experiments have evinc'd. And the reaſon he annexes to his Negative, as alſo the difficult Example he ſubjoyns, of a man lying in the bottom of the Sea, ſeem rather oppos'd to the Weight then the Spring of the Air. But we have already both by Experiments and by his own Conceſſions ſufficiently prov'd that the Air is not devoid of gravity; and that it likewiſe gravitates upon the terraqueous Globe, which in this page he ſeems to deny, we have proved by divers reaſons, and particularly in our 25. Experiment by the vaſt expanſion of Air under Water, when the preſſure of the incumbent Air was taken off from the Water. Pag. 20, 21.

As for the Scheme he annexes, I confeſs I do not well ſee what he drives at in it; atleaſt if it be intended for a Confutation of the Conjecture I have been defending: nor am I the onely perſon that complain of his writing often enough obſcurely. And as far as we can judge by the Concluſion couch'd in theſe words, *Non poteſt ergo pars, &c.* (which in the Explication of the Scheme he calls, *Pars Atmoſphæræ intra totam poſita ubicunq;*) *(propter magnitudinem) quantumvis gravis ſit deſcendere, neq; ergo premere ſive gravitare*; his Notions about Gravity are ſomewhat ſtrange, and probably either do not concern us, or will be found repugnant to thoſe Experiments on which our Conjecture is grounded.

And

Page 22. And as for what he adds, *Si possibile esse negarem, ut diligentiâ & arte humanâ duæ superficies corporum duorum inter se per omnia puncta ita accurate congruæ fiant, ut ne minimo quidem corpusculo creabili transitus permittatur, non video quomodo illi aut suam Hypothesim tueri, aut negationem nostram improbitatis arguere jure possent:* I confess I do not see how this Argumentation proves any thing against the Interest, either of the Weight, or so much as of the Spring of the Air in the cohesion of our Marbles. For, provided that the Corpuscles of the Air get not in between the two stones, the pressure of the Air may well suffice to keep them together. And, lastly, as for that modest passage (that immediately precedes the words newly recited)

Page 21, 22. wherein he sayes, *Utraq; illa phantasia, tum gravitatis Atmosphæræ, tum vis Elasticæ sive Antitupiæ aëris, somnium erat. Siquidem autem illis concederetur esse aliquam in filiculis aëris Antitupiam, quæretq; aliquis, unde illa curvata quidem sed quiescentia moverentur rursus ad rectitudinem, deberent illi, si Physici haberi volunt, causam ejus aliquam possibilem assignare:* We have already given an account why we forbore to assign a cause of the motion of Restitution; but methinks Mr. *Hobbs* might have, for the speaking so, chosen a fitter place then this, where he gives me so fair an opportunity to tell him again, that he should, if he would be thought a Naturalist, have assigned some cause of the *Phænomenon* about which he had all this while been disputing: which since even he himself has declin'd to do, perhaps the *Phænomenon* will be thought somewhat difficult, and my attempt at discovering the cause of it will be at least excused.

But after having so long dwelt upon the consideration of our Marbles, it is high time to proceed to what remains.

In the 24 page Mr. *Hobbs* has that passage I formerly recited touching the Glass-Fountains, out of which water is thrown up by compress'd Air. But though I as well as others have made use of such Fountains, invented by *Vincenzo Vincenti* of *Urbin*, and was unsatisfied with the account he gives of their *Phænomena*; yet in regard some learned men, and particularly Dr. *Ward*, have

have already examined his Explication, and I am not obliged to do ſo; I ſhall onely take notice of what our Author objects to prove that this *Phænomenon* cannot be ſolv'd by the Spring of the Air, in theſe words:

B. Cur non poteſt aqua, quæ cum injiceretur particulas aëris comprimebat, ab iiſdem particulis ſe explicantibus rurſus rejici? Page 24.

A. Quia locum explicatæ majorem non requirunt quam compreſſæ. Quemadmodum in vaſe aqua pleno, in qua eſſet multitudo anguillarum, anguillas ſive in ſe volutas ſive explicatas idem ſemper capit locum. Propellere ergo aquam per vim Elaſticam, quæ alia non eſt quam motus corporum ſe explicantium, non poſſunt.

B. Comparatio illa aëris cum aqua anguillis plena, noſtris, credo, non diſplicebit.

But the Elateriſts will anſwer, that neither can his earthly Atoms, to whom he aſcribes the Rejection of the water forc'd in, truly fill up more parts of ſpace at one time then at another; and therefore the Objection might have been ſpared: but indeed it reaches us not. For we, as Mr. *Hobbs* knows well, are not wont to compare the Air to Eeles, but to Wooll: and though each hair that makes up a compreſs'd lock of wooll, do not really fill more ſpace with wooll when extended then when crumpl'd; yet when there is a *congeries* of theſe hairs compreſs'd together, the whole fleece or *congeries* does by its ſpring endeavour to thruſt away thoſe contiguous bodies by which it is penn'd up, as I have more fully explain'd in my Epiſtle; ſo that theſe Aerial Corpuſcles, being pent up by the water forc'd into the Glaſs, ſtill endeavour to expand themſelves by throwing it out.

What our Author addes in the ſame page, as if they were miſtaken that think the Experiments of the Pleniſts tended (eſpecially till of late) to prove that the generality of them did not alwayes mean by a *Vacuum* a ſpace perfectly devoid of all corporeal ſubſtance, but any ſpace here below that is not fill'd with a viſible body, or at leaſt with Air, (for theſe are my words to which I ſuppoſe Mr. *Hobbs* alludes) To this, I ſay, it is ſcarce worth

while to make answer, the Controversie being of such small moment, (though I think I could easily enough do it) especially since he rather excuses those that may have negligently expresst themselves, then disproves what I said. And since I spoke chiefly and by name of the *Peripatetick* Schools, he may well allow that their Expressions concerning this matter were not alwayes so accurate, whilest in this very passage he concludes with

Page 25. these words, *Vides quam ineptè sit ad explicationem effectuum talium advocare verba Metaphorica, ut fugâ vacui, horrorem naturæ, &c. quibus olim ad existimationem suam tuendam usæ sunt Scholæ.* Nor is what he adds concerning the *Vacuum* to be attributed to *Democritus* and *Epicurus* either clear enough, or of concernment enough to our Dispute, to be insisted on by us; especially since I see not to what purpose he brings it in.

But there are in this passage two particulars, which, though they make little or nothing against what I said of the Plenists, may deserve to be taken notice of.

The second (for I think it expedient to dispatch that first) is

Page 25. couch'd in these words, *In Hydriis perforatis ideo hæret aqua, quia quæ per tantillum foramen exiturit, adeo exigua est, ut non possit ita in longitudinem se diffundere, ut descendendo aditum aëri faciat per foraminum circumferentias; neq; aër ab exeunte aqua pulsus locum alium (in mundo pleno) habere potest præterquam quem aqua deseret.*

But this Experiment I have already examined as 'tis propos'd in his Elements of Philosophy; and therefore I shall now onely say to the light variation I find of it here, that the reason here assign'd why the water in Gardeners pots clos'd at the top does not descend, is not rightly assign'd, since (to omit other Objections) by Monsieur *Paschall*'s Experiment it appears, that though in Pipes of no great length the water will not run out, yet if the Pipe be long enough, though the Orifice be no wider, the water will descend without giving passage to the Air at the circumference of it.

But the other particular here mentioned by Mr. *Hobbs*, who thus

thus propoſes it, *Qui per fiſtulam ore aquam ſugit, aerem medium prius ſugit, quo diſtentum aerem externum removet; qui remotus locum (in pleno) habere niſi proximum removendo non poteſt, & ſic continua pulſione aqua tandem pellitur in fiſtulam, ſucceditq; aeri qui exugitur*; deſerves a more particular conſideration. For this account of the aſcenſion of Liquors by ſuction is not onely here given by Mr. *Hobbs*, but for the main by the learned *Gaſſendus* himſelf, and other Atomiſts, and is generally acquieſc'd in by the modern Philoſophers; perhaps the rather, becauſe it ſeems not to eſtabliſh or overthrow a *Vacuum*. But though I ſhall not deny but that many *Phænomena* of Nature may be probably explicated by this Propagation and return of Motion; yet there are ſome *Phænomena* here below, which I ſee not how the *Carteſians*, or the Atomiſts, or Mr. *Hobbs* can explicate without admitting the Spring of the Air, and which perhaps by the Spring of the Air may be explicated without the recurring to ſuch a propagation and return of impulſe. Divers inſtances to this purpoſe I elſewhere conſider, but at preſent I ſhall propoſe onely one Experiment purpoſely devis'd to ſhew that both Vacuiſts and Pleniſts ſhould admit an Elaſtical power in the Air. I took then a Glaſs-veſſel conſiſting of two parts; the one was a Vial capable of containing about a pound of water, and the other a Pipe open at both ends, the lower of which reach'd within two Inches of the bottom of the Vial: this Pipe was by the Glaſs-man faſtned into the neck of the Vial, not by any Cement, leſt it ſhould be pretended that the Air might undiſcernedly get in or out, but with melted Glaſs of a good thickneſs: into this Veſſel by the open Pipe I at length (for it is ſomewhat difficult) pour'd water enough to ſwim a prety way above the lower extreme of the pipe, and then often inclin'd the Veſſel, to give a free intercourſe betwixt the Air within the Vial and that without it, that if the internal Air were compreſt by the affuſion of the water it might free it ſelf, as it readily did by aſcending in bubbles along the inclined Pipe, till the outward and inward Air were reduc'd to an equality of preſſure. Now if all Suction were

Page 25.

produc'd by the preſſure of the Air, thruſt away by the dilated Cheſt of him that ſucks, and ſo thruſting the water or other liquor into the Pipe at which he ſucks, it ſeems evident, in our caſe, that the water would not aſcend by ſuction: ſince by the contrivance of the Veſſel, the Air thruſt away by him that ſucks cannot at all come to bear or preſs upon the water. And yet, whether the Pipe were inclin'd or erected, the water did according to my expectation eaſily enough aſcend, upon ſuction, to the top of the Pipe, and ran over into my mouth. I ſay, *eaſily enough*, becauſe that though the Spring of the Air pent up in the Vial were able, upon the decreaſe of the preſſure of the outward Air, occaſion'd by my ſucking, to impell the water ſtrongly enough into the Pipe; yet, when a pretty quantity of water had been ſo impell'd up, the included Air gaining thereby more room to expand it ſelf, its Spring was thereby ſo far weakned, that the water aſcended far leſs eaſily then in ordinary ſuctions. The other circumſtances worth noting in this Experiment belong not to this place; and what has been delivered may, I hope, ſuffice for the purpoſe 'tis alledg'd for. Onely one particular I ſhall here adde, by way of confirmation of what I ſaid touching the weaken'd Spring of the Air, and it is this; That partly to ſhew ſome, who yet embrace the Opinion of the Schools, that the aſcenſion of the water in the Pipe did not proceed from any ſuch tendency in the water it ſelf to aſcend for prevention of a *Vacuum*, and partly for other reaſons that concern not this place, I did carefully take out the water by degrees, till the lower end of the Pipe was but very little under the ſurface of the water, though in the cavity of the Pipe the water, as it uſually will be in Pipes that are not wide, was a pretty deal higher: then ſuffering the Veſſel to reſt, and ſucking at the upper end of the Pipe, the water (as I foreſaw it would be) was impell'd up, yet without reaching near the top, till the ſurface of it was fallen a little below the bottom of the Pipe. But then, though I continu'd ſucking, no more water aſcended into the Pipe, but the Air paſſing through it towards my mouth, did in

its

its paſſage toſs up the water that was already in the Pipe, and turn it into bubbles, (of a ſtrange bigneſs when the cavity of the Pipe would permit it) which broke (not without noiſe) one after another: and thus the aſcending Air for a pretty while kept the water in the Pipe from falling back to that in the Vial. But when I remov'd my mouth, the Spring of the Air remaining in the Cavity of the Vial, being debilitated by the receſs of the Air I had (as men are wont to ſpeak) ſuck'd out, it was not able to reſiſt the preſſure of the outward Air, and accordingly the water in the Pipe was not onely depreſs'd into the Vial, but the outward Air forc'd its way in many bubbles, and not without ſome noiſe, through the water contiguous to the bottom of the Pipe, till the preſſure of the included Air and that of the Atmoſphere were reduc'd to an equality.

But in the ſame 25. page our Author tells us, that the Society he writes againſt would have the cauſe of Filtration, and that of the paſſage of water through Siphons, to be the ſame. To which he annexes this peremptory paſſage, *Id vero impoſsibile eſt. Nam in Siphone niſi ambo crura aquâ impleantur, aqua è pelvi non aſcendet. Aſcenſionis cauſa in pannum eſt motus ille terrearum atomorum quæ aquæ contiguæ ſunt, motus (inquam) circularis ſimplex, aëri in quo moventur communicatus; quæ atomi aquam ferientes in materiam laneam incutiunt, incuſſæ autem magis magiſq; madefaciunt, donec madida tota ſit. Cum vero madida tota ſit, &c.* Thus far he; but the paſſage in my Epiſtle, upon which he ſeems to have grounded his Oppoſition, is but this (wherewith I begin my 35. Experiment) *Some learned Mathematicians* (I meant the induſtrious *Schottus* and ſome *Carteſians*) *have of late ingeniouſly endeavoured to reduce Filtres to Siphons; but ſtill the true cauſe of the aſcenſion of water and other liquors both in Siphons and in Filtration needing (for ought we have yet found) a clearer diſcovery and explication, we were deſirous to try, &c.* So that neither did I aſcribe this reduction (of Filtres to Siphons) to a Society which was not then in being, nor perhaps ſo much as deſign'd; nor did I adopt it my ſelf; but expreſs'd a deſire to have it further examin'd.

But

But as for the cauſe of Filtration it ſelf, I may take a fitter opportunity to diſcourſe of it; in the mean time I doubt whether the reaſon here aſſign'd by Mr. *Hobbs* will not ſeem as well precarious, as the *motus circularis ſimplex* of earthly Atoms, whereon it is grounded. Nor does his Explication render a reaſon why Quickſilver will not aſcend the 14. part as high in the Filtre, though in part immers'd into it, as water; nay, will not reach ſo high where 'tis contiguous to the Filtre, as where 'tis not: nor why it ſhould begin to aſcend, ſince, for ought he ſhews to the contrary, the preſſure of the Air, even in the ſenſe he takes the Air, ought to be the ſame on that part of the Surface of the Liquor which is contiguous to the Filtre, and on any other part of the ſame Surface. To which I ſhall onely adde, that as reſolutely as Mr. *Hobbs* ſayes 'tis impoſſible for the water to aſcend out of the Veſſel into a Siphon, unleſs both the legs be fill'd with that Liquor, he would probably have ſpoken more warily, and diſtinguiſh'd betwixt Siphons, if he had been pleaſed to take notice of what I relate in the fore-mentioned 35. Experiment, of a ſmall Glaſs-Siphon I devis'd, whereof when the ſhorter leg was but dipp'd in water, the liquor did preſently as it were of it ſelf run down the longer leg. Which Experiment, beſides other conſiderations, may induce us to ſuſpect that the nature of Siphons and of Filtration may not yet be ſo throughly underſtood, as not to deſerve a further enquiry.

But to draw at length towards a Concluſion of our troubleſome *Examen*; it remains only that I take ſome notice of the general Corollary that Mr. *Hobbs* is pleaſed to deduce from his whole Diſcourſe, of the Experiments exhibited in our Engine.

Page 23. *A. Fateris ergo* (ſayes he) *nihil hactenus à Collegis tuis promotam eſſe ſcientiam cauſarum naturalium, niſi quod unus eorum machinam invenerit qua motus excitari aeris poſſit talis, ut partes ſphæræ ſimul undiquaq; tendant ad centrum, & ut Hypotheſes* Hobbianæ, *ante quidem ſatis probabiles, hinc reddantur probabiliores.*

B. Nec fateri pudet; nam, Eſt aliquid prodire tenus, ſi non datur ultra.

A. Quid

A. Quid tenus? Quorsum autem tantus apparatus & sumptus machinarum factu difficilium, ut eatenus tantum prodiretis quantum ante prodierat Hobbius? *cur non inde potius incepistis ubi ille desiit? cur principiis ab eo positis non estis usi? Cumq;* Aristoteles *recte dixisset, Ignorato motu ignorari naturam, quomodo tantum in vos suscipere onus ausi estis, &c.*

As to what he sayes to the disparagement of the Assembly, and in his own praise, the laws I prescribed my self at the beginning of this Discourse forbid me to reply to what is more likely, amongst civil and judicious Readers, to prejudice his own reputation then theirs he is so displeased with. And as for that which he assigns to be the use of our Engine, I shall very willingly leave it to the ingenious to judge what use may be made of it. But as for this mentioned by Mr. *Hobbs*, if he means here, as he elsewhere teaches *(pag.* 13. & 19.) that the Motion he speaks of is produc'd by that of the Air impell'd in betwixt the sides of the Sucker and the Cylinder; perhaps it will be thought I have sufficiently proved, that 'tis not *any* of its uses, so far is it from being its *chief* or *onely* use. But I confess I somewhat wonder Mr. *Hobbs* should quarrel with me, (for 'tis I that in my Epistle employ the following Verse) for saying,

Est aliquid prodire tenus, si non datur ultra.

And this not, as some would perhaps suspect, because I do not imitate him, in speaking of my self, as he does of himself, but because he thinks the expression too arrogant. For since he here confesses that his *Hypotheses* are by this Engine rendered more probable, some will perchance think that to be enough to entitle my Experiments to some degree of usefulness, unless Mr. *Hobbs*'s Doctrine of the Air had found more embracers then before these seasonable, though despised, Experiments it was observed to have. But, since either of us may be partial in his own case, I am very well content to leave others to judge both whether my Expression have been guilty of Arrogance, and how much he has done more then *prodire tenus* in all the past long Discourse against

me,

me, when they have considered what new Experiment or matter of fact Mr. *Hobbs* has therein added to enrich the History of Nature, what new Truths he has discovered, or what Errors (except one of his own in the last page) he has well confuted. These things, I say, I am very well content to leave to be judged of by all dis-interessed persons, without being either much discouraged by the differing strain wherein Mr. *Hobbs* thinks fit to speak of his own performances & mine, or invited to imitate him in that way of writing, my Endeavours (such as they are) having hitherto been favored with such a Reception among the *Virtuosi*, that possibly I may have almost as little need as reason to commend them.

But 'tis somwhat a troublesom work to argue long with a man that's angry with an Expression, which perhaps none but he would have found fault with for want of modesty; & therefore as I have left un-recited several provoking and very undeserved Expressions he emploies in the same page, and even that passage, where to prove that our Naturalists and Mathematicians professed they would not receive Truth coming from him, he alledges onely a saying (whether true or no I examine not) of Dr. *Owen*, who, besides that his profession was Divinity, not Philosophy or Mathematicks, neither is nor ever was of our Society: as, I say, I have (for the reason newly intimated) declin'd taking notice of matters of this nature; so I will not now stay to enquire why he urges us, whom he would have men take for Vacuists, with the Authority of *Aristotle*, whom on other occasions he is wont to use with as little respect as if he were a Member of our Society. Nor shall I now examine, why here and elsewhere he sends us to his own Writings for the Doctrine of Motion, as if, to omit ancienter Authors, such great Personages as *Galilæo*, *Mersennus*, *Verulam*, *Des-Cartes*, *Gassendus*, *Balianus*, *Joannes Marcus Marci*, *Honoratus Fabris*, (not to mention other Moderns, nor those of our own Assembly, as the eminently learned Sir *Kenelme Digby*, and the others, whom their Modesty forbids me here to name) had not most of them learnedly, and some of them copiously, written of Local Motion before Mr. *Hobbs*'s Books, where he

he treats of it, came abroad into the World. This, I ſay, I ſhal l not inſiſt on, becauſe I would haſten to a Concluſion. Onely one thing I muſt adde; that whereas he accuſes us of deviſing elaborate and ſumptuous Engines, I do not fear to find ſo many Readers of his mind, that I need make them Excuſes for what perhaps will obtain their Thanks. And whatever Mr. *Hobbs* may think, for my part I freely confeſs, that I love Truth ſo well, that I do not think, not onely my Pains and Charges, but even what I rate much higher, my Time it ſelf, too much for the *diſcovery* of Truth; or (that Mr. *Hobbs* may not think me partial) even for the *eſtabliſhment* of ſuch Truths as, though diſcover'd by ſome, are as yet far more generally oppoſed then embraced.

There remain yet ſome other pages of Mr. *Hobbs*'s Dialogue, wherein he ſpeaks of Fire, and Cold, and Ice, and Light, and Colours, and Fluidity, and Hardneſs, and Ethicks, and Politicks, and the Duplication of the Cube, and the Quadrature of the Circle, and ſeveral other Subjects. But theſe I forbear to meddle with, not that I approve them all, or the greateſt part; but *partly*, becauſe I am too much tired already to be fond of engaging in Controverſies that I am not tyed to meddle with, (except what concerns Fluidity and Firmneſs, which I ſhall, God willing, examine by it ſelf;) *partly*, becauſe divers paſſages relate to Perſons, not Things; *partly*, becauſe I do not much fear that Mr. *Hobbs* will find every Reader ſo eaſie and *complaiſant* as he makes his *Academian*, who in many paſſages of the Dialogue ſpeaks not unſuitably to what he does in the laſt page of it, where he excepts but one particular (and that is neither the *Duplication of the Cube*, nor the *Squaring of the Circle*) when he tells Mr. *Hobbs*, *Diſcedo jam multo (ut mihi videor) quam ante certior, & quæ dixiſti omnia teneo & probo:* and *partly*, becauſe Mr. *Hobbs* has ſome things, as about Fire, and *certain* Colours, which I am not forward to reject, though the Conſiderations that incline me to ſome ſuch Opinions be perhaps very differing from the Grounds on which he propoſes them. And indeed as well my Nature as my Cuſtom forbids me to ſcruple to learn, if I

can,

can, of Persons much less famous then Mr. *Hobbs*. 'Tis far from my humour to write against all that every man sayes, that (how causlesly soever) writes against me; and I am almost as much indisposed to *reject* as to *embrace*, without distinction, *whatever* it be that this or that man teaches.

CHAP. VII.

(*Being an* APPENDIX *to the past Discourse*)
Wherein is examin'd what Mr. Hobbs *teaches concerning Fluidity and Firmness.*

ALthough Mr. *Hobbs* do not name me for the Maintainer of the Opinions which I have proposed in the History of Fluidity and Firmness, and which he censures: yet since that History, after having been mentioned in the Book I have hitherto been defending, was published a good while before Mr. *Hobbs*'s Dialogue; and since some of the chief particulars he censures or takes notice of, are delivered there, and some of them perchance no where else; I think it may concern me to examine what he sayes either against my Opinions, or in favour of his own, touching Fluidity and Firmness. And if it be said that he meant not his Objections peculiarly against me, but rather against some other person, who may have liked such Opinions: I shall answer, That I am content to leave to others the defence of their particular Opinions, and to have as much of the following Discourse as is concerned in this Allegation, looked upon as written onely upon this supposition, that my writings are those he designs to oppose. But there being other things in what he discourses about Fluidity and Firmness, which for the reasons freshly intimated I think fit here to consider, I chuse to gather up together the passages touching these subjects which I find scattered in his Dialogue, that I may have the better opportunity to clear up the matter it self under debate.

But,

But, before I go further, I must at the very entrance of this Discourse take notice, that in the fifth page, where Mr. *Hobbs* begins to dispute against our Notion of Fluidity, he very much mistakes my Opinion, as may appear by these words, which he puts into the mouth of his Academian, *Sed pleriq; nostrum naturam fluidam à non fluida distinguimus magnitudine partium ex quibus corpus aliquod constat, & quasi compingitur: Itaq; non modo aërem, aquam & liquorem omnem, sed etiam cinerem & pulverem, tanquam fluida contemplamur. Et fluida ex non fluidis composita esse posse non negamus; nam divisibilitatem illam infinitam non concoquimus.* To which he answers, *Divisio quidem infinita concipi non potest, divisibilitas autem facile. Ego contra, distinctionem non capio inter fluida & non fluida quam sumitis à magnitudine partium; nam si caperem, ruina illa sive rudera illa quæ jacent in Ecclesia Paulina mihi dicenda essent fluida: sin propter nimiam lapidum magnitudinem fluida illa esse negaveritis, desini mihi magnitudinem illam quam habens pars ruentis muri, propter eam sit dicenda fluida. Tu vero qui divisibilitatem infinitam non capis, dic mihi quæ tibi apparet causa, quare Deo omnipotenti difficilius esse putem creare corpus fluidum, & cujus partes actu diffluant, omni data atomo minus, quam creare Oceanum. Itaq; desperare me facis omnem conventus vestri fructum, dicendo quod putant aërem, aquam, & cætera fluida constare ex non fluidis, tanquam si murum, cujus ruentes lapides aliquousq; discurrunt, dicerent esse fluidum. Si sic loquendum est, nihil non est fluidum; nam etiam marmor comminui potest in partes omni atomo Epicureana minores.* Page 5.

Thus he. But in my History, though I make the smalness of the parts whereof a body consists one of the requisites to its being fluid; yet at the end of the 13. Section I call the various agitation of those Particles the principal qualification of all, and in the beginning of the 14. Section I call it the chief condition of a fluid body. And therefore he much mistakes, if he thinks that we alwayes consider Ashes and Dust as fluids absolutely speaking. But as he * somewhere tells us, that by Fire he understands the combustible matter it self, not

* *Hobbs de Corp. ch. 27.*

ſimply and alwayes, but then onely when, &c. So neither do I look upon the Duſt of Alabaſter, (to my words about which, I ſuppoſe, he has a reſpect in the paſſage under conſideration) as a fluid body ſimply and alwayes; for I clearly teach the contrary: but onely when and whileſt its parts are not alone reduced to a competent ſmalneſs, but are alſo actually put into ſuch a various agitation as makes the body they compoſe (even according to Mr. *Hobbs*'s own Definition, which is, That ***Fluid bodies are thoſe whoſe parts may by very weak endeavour be ſeparated from one another***) emulate a fluid body by the very eaſie Ceſſion of its component Corpuſcles, and by its boyling like a liquor. By this the Reader may diſcern how little that makes againſt me which Mr. *Hobbs* talks, and ſeems to do it ſeriouſly, of the Ruines of *Pauls*; as if according to my Opinion, *Ruina illa ſive rudera quæ jacent in Eccleſia Paulina dicenda eſſent fluida*. For 'tis moſt evident that I require in the parts of a fluid body both minuteneſs and ſuch a motion along each other as makes them eaſily yield to the touch: which qualifications how well they belong to the ruines of *Pauls*, is, I think, not very difficult to determine; though in the ſame paſſage Mr. *Hobbs* do again make uſe of the like example, to which he ſubjoyns, *Si ſic loquendum eſt, nihil non eſt fluidum*; which how little it follows from my Doctrine about Fluidity there needs not a quick-ſighted Reader to diſcern. As for the Reaſon he annexes in theſe words, *Nam etiam marmor comminui poteſt in partes omni atomo Epicureana minores*; I would gladly know by what art Mr. *Hobbs* can divide Marble into leſſer Particles then ſuch as are naturally indiviſible, (for ſuch *Epicurus* makes his Atoms to be:) nor do I ſee how, in caſe this could be done, it proves, that there is not any thing that is not fluid. For I ſay that the blocks of Marble before Comminution are not fluid, either according to him or according to me; nay, the greateſt Comminution imaginable would not, according to my Doctrine, make a lump of Marble fluid, unleſs the heap compoſed of the parts, how minute ſoever, were actually and variouſly ſet a moving amongſt themſelves.

Mr. *Hobbs de Corpore, cap. 26. part. 4.*

ſelves. But he would perhaps have ſpoken more warily, if he had conſidered the difference there is betwixt ſaying that all things are fluid, and ſaying, as I do, that there are many bodies that are now ſolid, which by Comminution, Motion, and other requiſite alterations, may be made parts of a fluid body: As hard Ice may be turned into fluid Water, and Quickſilver precipitated *per ſe* into a red Powder, may be reduced to running Mercury. As for what he ſayes of an infinite diviſibility of body, it is ſcarce in this place worth while to examine it.

For I have ſhewn in the Hiſtory of Firmneſs, that this Diviſibility (which I had then conſidered) does no way overthrow my Doctrine of Fluidity; nor does Mr. *Hobbs* here anſwer what I there diſcourſe. Beſides that indeed I do not ſo well underſtand what he here means and drives at, when he ſayes that *Diviſio quidem infinita concipi non poteſt, Diviſibilitas autem facile.* For ſince in this very paſſage and within a very few lines he has recourſe in this matter to God's Omnipotence, I ſee not why an infinite diviſion cannot be as well conceived as an infinite diviſibility, ſince ſure an Omnipotent Agent is able to do what is poſſible to be done: and why elſe ſhould a body be called infinitely diviſible? Beſides, when Mr. *Hobbs* has recourſe to what God *can* do, (whoſe Omnipotence we have both great reaſon to acknowledge) it imports not to the Controverſie about Fluidity to determine what the Almighty Creator *can* do, but what he actually *has* done. And, laſtly, whereas my Adverſary requires to have the magnitude defined which a part of a falling Wall ought to have to deſerve the name of fluid; firſt, he ſhould have clearly proved that Fluidity belongs to any one ſingle part of matter how minute ſoever, and not rather to an aggregate of Particles. And next, I ſay thoſe Corpuſcles that compoſe a fluid body may be of ſeveral ſizes, as thoſe of Water, Oyle, and Quickſilver, provided they be little enough to be put into the agitation requiſite to give the aggregate they make up the qualities that are wont to denominate bodies fluid: and 'tis no more requiſite for me to define preciſely the magnitudes of the parts

of

of a fluid body, then for Mr. *Hobbs* in his Definition above-recited to define (which he will not easily do) what precise degree of endeavour must be signified by that *very weak* endeavour, by which if the parts of a body can be separated from one another, he thinks fit to call them fluid. But though I thought it not amiss to make these Animadversions upon Mr. *Hobbs*'s Ratiocination, yet as to the Opinion it self, for whose sake he speaks so severely and so despairingly of our Society, if it be considered as I propos'd it, he shews me as yet no cause at all to renounce it. For that which I taught is this, That if a solid body be reduc'd into parts minute enough, those solid Corpuscles, whilest they are put into a convenient motion, *may* become parts of a fluid body. And against this Mr. *Hobbs*'s Indignation seems stronger then his Argument: For that which he objects being, as we have lately seen, that at this rate all bodies must be fluid; 'tis evident by what I have already argued, that he infers this Absurdity not from my Opinion, but his own mistake of it: nor did I content my self with the proofless proposal of my Conjecture, but I delivered in several parts of the often-mention'd History particular Experiments to evince what I taught: As that a consistent *coagulum* of pure spirits of Urine and Wine may by bare digestion be turned into a permanent liquor; and that the fluid body of Quickfilver may without any sensible addition be turned into a permanent dry Powder, and may again in a trice by bare heat be turned into a lastingly fluid body. Whereto I added other Experiments, which together with these Mr. *Hobbs* would possibly have thought fit to answer, if he had found it easie for him to do so.

After this passage extant in Mr. *Hobbs*'s fifth page that I have all this while been examining, I remember nothing in his Dialogue that requires to be insisted on about Fluidity and Firmness, till we come to the 29. page, where having asked what cause the Academians assign of Hardness, 'tis answered that some

Page 29. of them assign three: to which Mr. *Hobbs* so far agrees as to say, *Quin corpuscula (qualia sunt atomi quas sup-*

ponit

ponit Lucretius *atq; etiam* Hobbius) *jam ante dura facile posſint ab aliqua dictarum cauſarum compingi, ita ut totum ex illis factum durum fiat, dubitandum non eſt.* But then he would have us aſſign the cauſe of that he calls *durum primum.* But after ſome diſcourſe, wherein he is pleaſed to approve an Objection of mine againſt ſome learned men that aſcribe all Coheſion of bodies to a certain Glue, he anſwers himſelf the Objection he frames againſt my Doctrine about Hardneſs, and thereby allowed me to proceed to what he further preſſes in theſe words, *Si dura ex primis duris fieri dicant, quare non & fluida fieri putant ex primis fluidis? An creari fluida maxima potuere, ut æther, minima non potuere? Qui corpuſculum durum aut fluidum primus fecit, potuit, ſi libuiſſet, illud feciſſe tum majus, tum minus quocunq; corpore dato. Quod ſi fluidum fiat ex non fluidis, ut vos dicitis, & durum ex duris tantum, nonne ſequitur ex fluidis primis neq fluidum fieri neq; durum?* Page 30. But againſt this paſſage I have divers things to repreſent. For, firſt, (not now to mention that it may be queſtioned with what propriety one part of matter more then another may be called *primum durum*) he ſhould have told us what he means by his *prima fluida*, and how he proves that there are any ſuch; which ſince he has not done, 'twill be at leaſt as hard for a conſidering man to acquieſce in his Queſtion as to anſwer it. For my part, I know no fluid body upon whoſe account, as of an Ingredient, all others are fluid. And, I think, 'twill be hard for Mr. *Hobbs* to ſhew that Water, Quickſilver, and purely-rectifi'd Chymical Oyles (to name now no other liquors) do conſiſt of ſuch *fluida prima* as he teaches, whereto they owe all their Fluidity. And 'tis plain by ſeveral Experiments delivered in our Hiſtory, and even by the obvious changes of Water and Ice into one another, that 'tis the motion, reſt, and the texture of the Corpuſcles which compoſe a Body, that make it firm or fluid. As for what Mr. *Hobbs* demands, whether the ſmalleſt Fluids imaginable could not as well have been created as the *Æther*, it proves nothing againſt me, the Queſtion not being, what might have been made, but what is ſo. And he ſhould have anſwered

the

the Arguments I * alledge to make it improbable that a fluid body is, as he would perſwade us in his book † *De Corpore*, alwayes diviſible into bodies equally fluid, as Quantity into Quantities. 'Tis true, he there tells us that, though many others do not, *He underſtands by Fluidity that which is made ſuch by Nature equally in every part of the fluid body, in ſuch manner as water ſeems fluid, and to divide it ſelf into parts perpetually fluid.* But whether others will take this for a clear Notion of Fluidity I think may well be doubted; and he ſhould not barely ſay, but prove, (which, I think, he will find hard to do) that the Corpuſcles of water divide themſelves ſo as he teaches, ſince we ſee, that not onely they cannot penetrate Glaſs, but are unable to be driven in at the Pores of more open bodies, which other liquors eaſily pierce into. And, laſtly, as to Mr. *Hobbs*'s Queſtion, *Quod ſi fluidum fiat, &c.* 'tis eaſie to foreſee what according to my Doctrine I may anſwer: For, not to mention that the Argumentation is invalid, unleſs by *Fluidum* he mean *Omne fluidum*, I reply, That till he have explained what he means by his *Fluida prima*, and proved that there are ſuch, the Queſtion needs no Anſwer. Beſides that whatever he upon miſtakes ſtrives to infer, my Doctrine is ſo far from affirming that there are many parts of matter of which neither fluid nor hard bodies can be made, that I teach, as we have lately ſeen, that there are multitudes of parts that may, by being reduced to a ſufficient ſmalneſs and put into a convenient motion, or by being brought to a mutual contact and reſt, be made to conſtitute either a fluid body or a firm one: as may be exemplified in the formerly-mentioned inſtances of two ſubtile liquors that immediately compoſed a conſiſtent body; and of Quickſilver, which without additaments was made ſometimes a Powder, and ſometimes a Liquor. What Mr. *Hobbs* addes in the next page about the difficulty of explaining the Diaphaneity of Glaſs or Cryſtal, in caſe they conſiſted of Corpuſcles hard and implicated, or having their Pores in any way whatſoever diſ-

joyned,

* Hiſtory of Firmneſs, pag. 227. and elſewhere.

† Chap. 16. Sect. 4.

Page 31.

joyned, I muſt not now inſiſt on; ſince beſide that ſuch a diſquiſition would require almoſt a Volume, the true and general cauſe of Tranſparency in bodies is in my poor Opinion one of the abſtruſeſt things in Natural Philoſophy; and Mr. *Hobbs*'s Explica ion of it (though none of his worſt Conceits) has for ought I can find fallen ſhort of ſatisfying the Curious, as well as thoſe of other men have done. But to me, that have not taken upon me to write Elements of Philoſophy, it is enough that I have by competent Experiments and other Proofs confirm'd the truth of my Doctrine about the cauſe of Firmneſs; though I attempt not to explicate the other qualities of the ſame bodies, whoſe Explication my undertaking does not exact. Wherefore I hope I may now haſten to conclude this *Appendix*, with ſpending a few words on the Notion of Fluidity and Firmneſs Mr. *Hobbs* would ſubſtitute inſtead of mine. For, having now (perhaps but too prolixly) examined what he has been pleaſed to object againſt my Doctrine, I ſhall not need ſpend time to vindicate the Experiments and Conſiderations whereon I built it, Mr. *Hobbs*, for reaſons beſt known to himſelf, not having thought fit to take notice of them.

Mr. *Hobbs*'s Theory of Fluidity and Firmneſs is thus delivered by himſelf;

B. Quænam duri & fluidi ſunt principia? Page 30.

A. Quid aliud niſi fluidi quidem, Quies, duri autem, Motus quidam ad illum effectum producendum idoneus? Per Quietem intelligo duarum partium inter ſe quietem cum ſe mutuo tangunt quidem, ſed non premunt: Nam & fluida moveri tota poſſunt retenta fluiditate, & dura quieſcere, ut tamen partes eorum moveantur.

I doubt not but this will to moſt Readers ſeem a Paradox. And as for his Ratiocination contain'd in the two laſt lines, I ſhall readily allow him that *Fluida moveri tota poſſunt retenta fluiditate*, ſince that I think agrees at leaſt as well with my *Hypotheſis* as his: but whereas he adds that hard bodies may reſt, and yet their parts be moved, that may in one caſe be conceived, and in another not. For indeed the implicated parts of a firm body may be

made to tremble, or a little vibrate as it were to and fro, as those of a sounding Bell do, or as in a Hedge the branches and twigs may be shaken by the wind, whilest the trees and bushes themselves continue rooted in the ground. But that in a body the constituent Corpuscles should all or most of them be moved quite out of their places in respect of one another, as was lately shewn to happen in fluids, and yet the body continue hard, is more requisite then easie to be proved.

But Mr. *Hobbs* contents himself to alledge in favour of his strange Notion touching Fluidity and Firmness three particulars, which, I confess, afford me not the least satisfaction.

The first is drawn from what he formerly taught touching the swift motion of the Air in our Cylinder; which Example (as he calls it) having repeated, he adds, *Atq; hinc manifestum*

Page 30. *est vehementem esse in aëre ita moto & clauso compressionem, quantam scilicet efficere potest vis illa qua incussus erat; atq; etiam à tanta compressione aliquem gradum consistentiæ fieri, quam consistentia aquæ minorem. Quod si esset in iisdem particulis aëris omnibus, præter motum illum quo altera alteram premit, motus ille circularis simplex, isq; satis vehemens, impossibile fere esset unam earum à suo circello dimoveri, quin reliquis particulis resistentibus, totus simul premeretur, id est, totum durum esset: durum enim est tum illud, cujus nulla cedit pars nisi cedente toto. Vides ergo posse fieri duritiem in fluidissimo aëre per motum hunc circularem simplicem particularum, quibus duo motus contrarii ante dederant vertiginē.*

But, I confess, I do not see how the *Motus circularis simplex* he talks of should give such a hardness to the fluid Air: nor is it manifest to me how the Air that perfectly fills the Cylinder can be by motion compress'd, especially so far as to obtain thereby a degree of consistence fit to be mentioned, as he speaks of it, when (without adding the word *much*, or any other equivalent term) he sayes that yet 'tis *less then the consistence of water*. For the Cylinder being, according to him, perfectly full of Air, I see not how the Pumping can make the Cavity (to use his own expression elsewhere) *fuller then full*; nor consequently can com-

press

preſs the Air to a conſiſtence any thing near that of water, without penetration of Dimenſions. But theſe things were mentioned onely *ex abundanti*, for the violent motion it ſelf of the Air in the Cylinder (which motion the Argument ſuppoſes) has been already, in the *Examen* of one of the former paſſages of his Dialogue, ſufficiently manifeſted to be contrary to Experience.

The ſecond thing Mr. *Hobbs* alledges is his Conceit of the Generation of Fleſh within the Muſcles of the humane body. But, beſides that he takes for granted two or three things which many learned Anatomiſts and Phyſicians, even among the Moderns, will ſcarce allow him, and which he does not prove: beſides this, I ſay, (which I may elſewhere have occaſion to conſider further) the account he gives of the Generation of Fleſh from theſe ſuppoſitions, is far from being evidently enough deduc'd to vie for clearneſs with many of thoſe Experiments which I have alledged in favour of the Opinion he oppoſes. And whereas he adds, *Atq; talis quidem eſſe poteſt cauſa efficiens Duri primi; Duri autem ſecundi, id eſt, Duri à cohæſione durorum primorum, cauſa poteſt eſſe motus ille idem circularis ſimplex conjunctus cum contactu eorundem ſuperficiali, vel etiam intricatione:* Page 31. Not again to ſay any thing to his *durum primum*, I confeſs I do not ſee how the *motus circularis ſimplex* ſhould need to be ſuperadded to the Contact or Intrication of the cohering firm Corpuſcles, to procure a Coheſion, to which 'tis needleſs, and which in divers caſes may be rather hindered then furthered by it.

The third thing that Mr. *Hobbs* alledges is not ſo much a confirmation of his own Doctrine, as an Objection, as he ſeems to think, againſt that of his Adverſaries. For, *Si ſupponamus* (ſayes he preſently after his laſt-recited words) *cum illis, duritiei cauſam eſſe magnitudinem aut craſſitiem partium, quam rationem reddere poterimus, cur durior vel firmior ſit aqua congelata, quam eſt eadem aqua ante congelationem?* Page 31. But it may eaſily be replyed, That we make not the bigneſs or groſneſs

neſs of the parts of a body the onely or chief cauſe of its hardneſs, but their reſt by one another, which the parts of frozen water have; whereas thoſe of unfrozen water have it not, but are in a ſtate of Fluidity, and conſequently not of Firmneſs.

Which may be illuſtrated by what I* elſewhere relate of pure Oyle of Aniſeeds, and a ſubſtance I diſtilled out of *Benzoin*, both which bodies were ſometimes fluid and ſometimes conſiſtent, as the greater or leſſer warmth of the Air kept their parts in a due motion or ſuffered them to reſt.

* In the Hiſtory of Fluidity and Firmneſs.

But in exchange of theſe few and unconcluding Arguments, which are all that Mr. *Hobbs* alledges to countenance his Paradox, how many Experiments and Reaſons might we tranſcribe out of our Hiſtory of Fluidity and Firmneſs in favour of the contrary Truth?

And as Critical as Mr. *Hobbs* appears in laying down the requiſites of a good *Hypotheſis*, I muſt make bold to the two conditions he mentions (pag. 11.) *Ut ſit conceptibilis (id eſt, non abſurda)* (which whether it be enough I now diſpute not) *& Ut ab ea conceſſa inferri poſſit Phænomeni neceſſitas*, to adde a third, namely, That it be not inconſiſtent with any other Truth or *Phænomenon* of Nature. Which third condition whether divers of Mr. *Hobbs*'s *Hypotheſes* (which himſelf in this place calls *mirandæ*) do not want, we have in part already conſidered in the Treatiſe to which this is an *Appendix*; and (as I newly intimated) I might further ſhew as to his Notion of Fluidity and Firmneſs, if I would here repeat all the Experiments mentioned in my Hiſtory of them, though they be not all that I have made ready to the ſame purpoſe againſt another opportunity: but partly wearineſs, and partly a natural unwillingneſs to repeat, induces me rather to refer my Reader thither. Which when I do, I do not forget that Mr. *Hobbs* appears offended at me and others for troubling our ſelves to make un-obvious Experiments. But that I may not repeat what in divers Treatiſes I repreſent concerning the Uſefulneſs of ſuch Experiments, I ſhall now only oppoſe

pose to the Authority of Mr. *Hobbs* in this Dialogue, wherein he has been pleased to chuse those he calls the *Experimentarian Philosophers* for his Adversaries, the Authority and Reason of the same Mr. *Hobbs* in another Dialogue (published but the year before) where one of his two Discoursers having said, *Qui corpora corporibus admovendo, nova & mirabilia ostendunt Naturæ opera, mirum in modum incendunt animos hominum amore Philosophiæ, & ad causas investigandas non parum instigant, eoq; nomine laude digni sunt* : the other confirms it by adding, *Ita est ; nam historiam Naturalem (sine qua scientia Naturalis frustra quæritur) locupletant.* And howsoever Mr. *Hobbs* needed not have recourse to such Experiments as he would be thought to disapprove, (I mean Elaborate ones) to discern that his Notions do not over-well agree with the *Phænomena.* For, if there be not a various motion in the small parts of Water and such liquors, whence is it that a lump of common Salt being thrown into a pot of water is there dissolved into minute bodies, whereof many are carried to the very top of the water, and are so exquisitely diffused and mingled with the liquor, that each least drop of it contains numbers of Saline Corpuscles? And if motion be the cause rather of hardness then fluidity, how comes it to pass that in frosty weather Ice is by heat (which Mr. *Hobbs* will not deny to be motion, or an effect of it) turned from a hard to a fluid body? And that Metals, as Gold and Silver, &c. whilest they are either cold, or exposed but to a moderat heat, are firm and consistent bodies ; and by a violent heat, which does manifestly give their parts a various and vehement agitation, (as appears by their sudden dissipating of Spittle, Greace, and far more stable bodies, cast upon them, into smoke) are put into a fluidity, which upon their removal from the fire they quickly exchange for firmness?

Mr. *Hobbs in Examinat. & Emendatione, &c. Dialog. 6.*

But since the want of more to say would not in haste put a Period to this Discourse, I am content to let my Haste break it off ; especially since after I have thus examined what Mr. *Hobbs* teaches concerning Fluidity and Firmness, either here, or in that Section of his

his Elements where he pretends to define them, I think I need not fear that a Doctrine which I have perhaps with some care endeavoured to establish, for the main, upon Experiments, should be overthrown by Opinions whose grounds are but such as we have already seen; and in pleading for which the Author is pleased not onely to leave almost all my Arguments untouch'd, but not so much as to offer at explicating by his Principles any of those numerous and important *Phænomena* of Fluidity and Firmness delivered in the Treatise he opposes.

And now leaving Mr. *Hobbs* to apply my self to the Reader, I have to the things hitherto discourst but this one thing to adde concerning them, That as little cause as Mr. *Hobbs* has hitherto given me to distrust what I have written of Fluidity and Firmness; yet I am not now more confident of my Conjectures then I was, when toward the end of the Preface to the History of these two Qualities I spoke diffidently enough of the Theorical part of that Treatise. And I freely confess, that the great difficulty of things, and the little abilities I find my self furnisht with to surmount it, do often in general beget in me a *great* distrust even of things whereof my Adversaries Objections give me not *any*.

FINIS.

The Citations Engliſh'd.

Pag. 11, 12. *Quanquam vis, &c.* Although the force of that motion in the evacuated Receiver be diminiſhed, being oppreſt by the conſiſtence of the Air moved within, yet is it not extinguiſhed: and therefore that oppreſſion being taken off will have ſtrength enough to excite an appearance of light, though ſomewhat weaker then ordinary.

p. 13. *Sine, &c.* Without which *Hypotheſis* let never ſo much labour, art, coſt, be beſtowed for the finding out of the inviſible cauſes of natural things, all will be in vain.

p. 15. *Cum ejus, &c.* Seeing almoſt all its parts are flexil, like little ſoft feathers or fine threds.

Ibid. *Sed quiſquis, &c.* But it matters not who was the Author of that Suppoſition. For the very *Hypotheſis* it ſelf, wherein is ſuppoſed a motion of ſubtil matter, which is ſwift without any cauſe aſſigned, and hath moreover divers innumerable circulations of Corpuſcles generated from the ſingle motion of that matter, is not the conceipt of a man of wit or ſenſe.

Ibid. *Nempe hoc, &c.* This is the thing that the great *Des-Cartes* ſomewhere admired, that he, whether his Poſitions are true or falſe, doth never in argumentation make any right inference from his Suppoſitions.

p. 17. *Quod ſane, &c.* Which is indeed a moſt evident argument of the weight of the Air.

Ibid. *Quod quidem lanx, &c.* That the Scale in which the Bladder is, is more depreſt then the other, they may be certain, their eyes bearing them witneſs: but that this comes from the natural gravity of the Air they cannot be aſſured; eſpecially if they are ignorant what is the efficient cauſe of Gravity.

p. 18. *Quod veſica, &c.* That the Bladder, whether it be blown up with a pair of Bellows, or with the breath of ones mouth, is heavier then when it is not blown up, I will not deny, becauſe of the greater quantity of Atoms from the Bellows, or of fuliginous Corpuſcles that are blown in from the breath. But notwithſtanding they gather nothing of ſufficient certainty from this Experiment of a blown Bladder. They ought to have put into the Scales two Veſſels of equal weight, whereof one ſhould be ſhut and the other open: For by this means Air not blown in, but onely incloſed, had been weighed. When therefore you ſhall ſee Air ſo weighed, we will afterwards conſider what may be ſaid concerning the *Phænomenon* you bring.

Ibid. *Quod Atmosphæræ, &c.* That many Particles both of Earth and Water mingled with the Æthereal body are in the Atmosphere, I am easily perſuaded: but that in the middle of the *Æther* they ſhould move upwards, downwards, every way, and that one leaning on the back of another they ſhould not gravitate, is a thing utterly unconceivable.

p.20. *Aër quo, &c.* The Air, with which in the beginning the Spherical Glaſs was full, being moved by thoſe Earthy Corpuſcles in a ſimple circular motion, and being compreſt by the force of the Injection, that of it which is pure (penetrating the injected Water) gets out into the open Air, and gives place to the Water. It follows therefore that thoſe earthy Corpuſcles have leſs place left, in which they can exerciſe their natural motion: therefore beating one upon another they force the water to go out; it thus going out, the external Air (becauſe the Univerſe is ſuppoſed to be full) penetrates it, and ſucceſſively takes up the place of the Air that goes out, until the Corpuſcles, the ſame quantity of Air being reſtored, regain a liberty natural to their motion.

p.21. *Quoniam per, &c.* Becauſe by the drawing back of the Sucker the pure Air was thruſt in, but the earthy parts were not thruſt in; there was a greater proportion of earthy Particles, which without the Cylinder were near the Sucker, unto the pure Air, in which they exerciſed their motion as well after this revulſion as before. Wherefore theſe Particles ſo moved having leſs place to exerciſe their natural motion in, ſome of them fell foul and beat upon the reſt. So that of neceſſity the Particles that were near the ſurface of the Sucker muſt drive it upwards.

Ibid. *Vidiſti, &c.* You ſee now that this Spring of the Air, which they ſuppoſe, is either an impoſſible thing; or they muſt (for its defence) have recourſe to the *Hypotheſis* of Mr. *Hobbes*.

p.23. *Quia cuticula omnis, &c.* Becauſe every skin is made up of ſmall threds or filaments, which by reaſon of their figures cannot accurately touch in all points. The Bladder therefore, being a skin, muſt be pervious not onely to Air but to Water alſo, as to ſweat: Therefore of the Air beat in by force there is the ſame compreſſion within the Bladder that there is without. The endeavour of which, the way of its motions being every way croſs, tends every way to the concave *ſuperficies* of the Bladder. Wherefore it is of neceſſity that it muſt ſwell every way, and the vehemency of the endeavour increaſing, be torn at laſt.

p.26. *Intellexti, &c.* Have you underſtood my *Hypotheſes?* 1. That there are with the Air interſperſt many earthy Particles, endued with a ſimple circular motion, congenite to its nature. 2. That there is a greater quantity of theſe Particles in the Air that is near the Earth, then in that which is more remote from it.

Ibid.

Ibid. *Neq; est, &c.* Nor is there any one that hitherto has brought any reason why it may not be so.

p.28. *Nihil, &c.* Nothing is moved but by a contiguous Body that is in motion.

p.29. *Dum Sultor, &c.* While the Sucker is drawn back, by how much a greater place is left (*within*,) by so much a lesser place is left to the external Air, which being thrust backwards by the motion of the Sucker towards the outmost parts, doth move in like manner the Air that is next it self, and that Air the next, and so forwards; so that it is of necessity at last that the Air must be compell'd into the space deserted by the Sucker, and to enter betwеen the convex and surface of the Sucker, and the concave of the Cylinder. For it being supposed that the parts of the Air are infinitely subtil, it is impossible but they should insinuate themselves that way by which the Sucker is drawn down. For first, the contact of those surfaces cannot be perfect in all points, because the surfaces themselves cannot be made infinitely smooth. Then, that force which is applied to draw back the Sucker, doth distend in some measure the cavity of the Cylinder. Lastly, if in the confines of (*that is, betwixt*) the two surfaces any one single hard Atom should enter, pure Air will enter at the same way, although with a weak endeavour. I might also have accounted that Air which for the same cause insinuates it self through the Valve of the Cylinder. You see therefore the consequence from the retraction of the Sucker, to the being of an *Empty place* is taken away. It will follow also that the Air which is driven up into the place deserted by the Sucker, because it is driven up thither by a great force, is moved with a very swift and circular motion betwixt the top and the bottom in the Cylinder, because there is nothing there that can weaken its motion: and you know that there is nothing that can give motion to its own self, or diminish it.

p.38. *Hærent hic, &c.* Here our men are at a stand: How will you expedite this difficulty?

A. I have don't already: For the Air being beaten back by the retraction of the Sucker, and finding no place in the world (which we suppose to be full) where it might dispose it self, besides that which by driving out other bodies from their places it may make for it self, is by perpetual pulsion at length forced in the Cylinder with so great swiftness, betwеen the concave surface of the Cylinder and the convex surface of the Sucker, as may answer that store of power which you found necessary to the drawing back of the Sucker. Now the Air, with what swiftness it enters, retains the same within, and then distends every way the sides of the Brass Cylinder, which is *(of it self)* Elastical. Therefore the Air in the

Cylinder being vehemently moved, endeavours or thrusts against all parts of the concave surface of the Cylinder; but in vain, untill the Sucker is drawn back: But as soon as the Sucker, having slipt the hand, ceases to make its impulse upon the Air, that Air which was before driven in, by reason of its endeavour against every point of the internal *superficies* of the Cylinder and of the *Elastical* force of the Air, will insinuate it self between the same surfaces with the same swiftness as that by which it was impell'd, that is, with that velocity which answers the strength of the Impulsion. If therefore so great a power of Weight be hung upon the Sucker as may answer the power of the hands by which it was impuls'd; the swiftness with which the same Air goes out of the Cylinder, finding no place in the world (which is full) where to dispose it self, will again impell the Sucker to the top of the Cylinder, for the same reason that the Sucker a little before made an impulse upon the Air.

P. 43. *In vas, &c.* *B.* We poured water into an open Vessel, we placed in the water a long, streight, slender Tube; and we observed that the water did ascend from the Vessel underneath into the erected Tube.

A. No wonder: For the small Particles that are interspers'd in the Air near the Water, did by their motion beat upon the surface of the Water, so that the Water must of necessity ascend into the Pipe, and that sensibly into a Pipe that was so exceeding slender.

P. 44. *Siquis, &c.* If any one after the frequently-repeated impulse and retraction of the Sucker, endeavour to draw out the Stopple of the upper Orifice of the Receiver, he shall find it gravitates very much, as if a weight of many pounds hung upon it. Whence comes this?

A. From the strong circular endeavour of the Air within the Receiver, made by the violent ingress of the Air between the convex surface of the Sucker and the concave of the Cylinder, procured by the repeated impulse and revulsion of the Sucker, which you improperly call the Exsuction of the Air. For by reason of the fulness of Nature the Stopple cannot be drawn out, but the Air that is in the Receiver (contiguous to the Stopple) must be drawn out too: which Air, if it were settled and at rest, the Stopple would easily be drawn out; but whiles that does most swiftly circulate, it comes out very hardly, that is, it seems to be very heavy.

B. Very likely: For as soon as fresh Air is by degrees let into the Receiver, it likewise by degrees loses this seeming gravity.

P. 46. *Vidimus, &c.* We saw also water, being let down into the Receiver, after some returns of the motion of the Sucker, to bubble so as if it had boiled over a fire.

A. This

A. This likewiſe happens, as we ſpake, by reaſon of the ſwiftneſs of the circulating Air: unleſs perhaps you find the water hot too whiles it bubbles. For if we were ſure it was hot, we muſt find out ſome other cauſe of the *Phænomenon*.

B. We are certain it is not ſenſibly hot.

A. In what therefore can the greater or leſſer motion of the Atmoſphere promote ſuch a motion as this?

B. I ſuppoſe they do not attribute this motion to the Atmoſphere.

p.47. *A.* It is manifeſt from this Experiment, that the Receiver is not made empty by this exſuction of Air, as you call it: For the water could not be moved but by ſome contiguous mover, that was it ſelf in motion. Therefore this *Phænomenon* ſeems to contain no weak demonſtration of my *Hypotheſis*.

p.48. Beſides, tell me, could you ſee the water bubbling in that manner?

B. What elſe?

A. Do not your Aſſociates grant that Viſion is made by a continued action from the object unto the eye? Do they not alſo think action to be motion, and all motion to be of ſome body? How therefore could the motion be derived from the object, the water, unto your eyes through a *Vacuum*, that is ſomewhat that is not a body?

B. Our friends do not affirm the Recipient to be ſo empty that no Air at all is left.

A. No matter whither the Receiver be wholly, or for the greater part, empty; for which ever you ſuppoſe, the derivation of the motion from the object to the eye will be intercepted.

B. It may be ſo; I can't tell what to anſwer.

p.49. *Credin' tu, &c.* Do you think theſe Animals were therefore ſo quickly killed becauſe they wanted Air? How then do they who make a trade of Diving live under water, of whom there be ſome who being accuſtomed from their childhood have wanted Air a whole hour? No. That moſt vehement motion by which Bladders ſhut therein are diſtended and broken, kills theſe Animals ſhut up in the Receiver.

Ibid. *Ego contra, &c.* I on the contrary think that neither the Air can be ſuck'd out, nor that the Animal would ſo ſoon dye if it were ſuck'd out. The action indeed to which this death is a conſequent may ſeem either a certain ſuction (and ſo, that the Animal is kill'd by the exſuction of the included Air, its Reſpiration being taken away) or a compulſion of the Air from all parts towards the Centre of the ſpherical Glaſs in which the Animal is incloſed, and ſo may be ſeen to dye ſtifled by the tenacity of the

com-

compreſs'd Air, as it were, with Water; the Air, more tenacious then uſual, being drawn into the inwards of the Lungs, and there between the Pulmonary Artery and Vein ſtopping the courſe of the bloud.

P. 51. *Placet, &c.* Your *Hypotheſis* pleaſes me better then that of the Spring of the Air: For from its truth depends the truth of a *Vacuum* or a *Plenum*; but from the truth of that nothing follows on either part of the Queſtion. The make of the Air (ſayes he) is like that of compreſt wooll. Well; wooll is made of hairs or threds. Right; but of what figure? if of a *Parallelopipedon*, there can be no compreſſion of parts: if not of a *Parallelopipedon*, there will be betwixt the hairs certain ſpaces left, which if they be empty they ſuppoſe ſome place empty, to prove that a *Vacuum* is poſſible; if full, they ſay that is full which they ſuppoſe to be empty.

P. 54. *Fuere, &c.* There were ſome of them that ſaid there remained in thoſe coals (though they ſeem'd extinguiſh'd) ſome fiery Particles, which being blow'd up by the Air upon its admiſſion did re-kindle the reſt of the maſs.

Ibid. *Ne, &c.* In good faith they ſeem not ſo much to have conſidered what they ſhould ſpeak, as to have taken it up at all adventures. Do you believe that in a kindled coal, there is any part which is not a coal but fire; or in a red-hot Iron there is any part that is not Iron but Fire? A great City may be ſet on fire by one ſpark: Now if the body of fire be different from the thing fired, there can be no more parts of fire in the whole Town on fire then that one ſpark. We ſee bodies of divers kinds may be ſet on fire by the light of the Sun, as well by the Refraction as the Reflexion that is made in Burning-glaſſes. And yet I do not believe that there is any man thinks that Particles of fire darted from the Sun can paſs through the ſubſtance of a cryſtal Globe. And in the Air between the Sun and the Globe there is no fire.

P. 56. When is it that we may truly ſay of a man that he is dead, or (which is the ſame) hath expired his Soul? For it has been known that ſome men who have been taken for dead, being brought out the next day revived.

A. It is hard to determine the point of time in which the ſoul is ſeparated from the body. Proceed therefore to other Experiments.

P. 57. *Si acus, &c.* If a Needle excited by a Loadſtone hang freely within the Receiver, it will nevertheleſs follow the motion of the Iron which is drawn about without the Receiver. So objects put within will be ſeen by thoſe that are without, and ſounds made within will be heard without; all theſe as well after as before the exſuction of the Air, except that the ſounds are ſomewhat more weakly heard after then before.

B. Theſe

B. Theſe are moſt manifeſt ſigns that the Receiver is alwayes full, and that the Air cannot thence be ſuck'd out. That the ſounds thence are more weak to ones hearing is a ſign of the conſiſtence of the Air; for the conſiſtence of the Air is diametrically oppoſite from its motion.

p.59. *Quia nihil, &c.* Becauſe there was nothing there that the weight of the Atmoſphere ſhould do; no more ſtrong or evident Argument could be made againſt a *Vacuum* then this Experiment. For if of two coherent Marbles either of them ſhould be thruſt forward that way that their ſurfaces lye contiguous, they would eaſily be ſever'd; the neighbouring Air ſucceſſively flowing into the deſerted place. But ſo to pull them aſunder, that at one time they ſhould loſe their whole contact, is impoſſible, the world being full. For then either motion muſt be made from one term to another in an inſtant, or two bodies at the ſame time muſt be in the ſame place: to ſay either of which is abſurd.

p.60. *Confitentur, &c.* They themſelves and all others confeſs, that all Ponderation is an endeavour every way by right lines unto the Centre of the Earth; and ſo that it is made not by the figure of a Cylinder or Column, but by a Pyramide, whoſe top is the Centre of the Earth, and whoſe Baſis is part of the ſurface of the Atmoſphere.

Ibid. *Conatus, &c.* Therefore the endeavour of all the points that ponderate will be propagated to the ſurface of the upper Marble, before it can be propagated further (ſuppoſe) to the Earth.

p.62. *Has, &c.* Theſe Scales he puts one upon another and draws out the Air, and then are they kept ſo compreſt and united by the gravity of the external Air, that ſix ſtrong men cannot pull them aſunder. But if at length by the uſe of utmoſt endeavour they are pluckt in ſunder, they make a noiſe equal to the report of a Muſquet; but as ſoon as ever by the Stop-cock open'd there is the leaſt entrance given to the Air, they are ſevered of their own accord.

p.63. *Sed vis, &c.* But can the Spring, which they ſay is in the Air, confer nothing to the holding up the Marble? —— Nothing at all: For there is no endeavour of the Air to the Centre of the Earth, more then to any other point in the Univerſe. For ſeeing that all heavy things tend from the circumference of the Atmoſphere unto the Centre of the Earth, and thence again to the circumference of the Atmoſphere by the ſame reflected lines, the endeavour upwards will be equal to the endeavour downwards, and ſo deſtroying one another they will endeavour neither way.

p.63. *Non poteſt ergo pars* BC, *&c.* Therefore the part *BC* (that is a part of the Atmoſphere placed any where within the whole) cannot (by reaſon of its greatneſs) deſcend, although it be heavy, and therefore it cannot preſs or gravitate.

p.64.

P.64. *Si possibile, &c.* If I should deny it possible, that by the art of man two surfaces of two bodies could be made so accurately fit that they should touch in all points, so that there could no creable Corpuscle pass between them; I do not see how they could defend their own *Hypothesis*, or disprove our Negative assertion.

Ibid. *Utraque, &c.* Both those Fancies, as well that of the Weight as of the Spring or *Antitupy* of the Air, are Dreams. But if it be granted that there is a kind of Recoyling in those small hairs or slender Corpuscles of which the Air consists; one may enquire whence it is that those crooked bodies, settled and at quiet in that posture, came to be moved into a streightness. They ought, if they will be esteemed Natural Philosophers, to assign some possible cause of this.

P.65. *Cur non, &c.* Why cannot the water, which when it was injected did compress the particles of Air, be again cast out by the same particles explicating themselves?

A. Because when explicated they require no greater place then when compresst: As in a vessel full of water, wherein are many Eeles, the same proportion of place receives them, whether they are folded round or at length. Therefore they cannot drive up the water by their Spring, which is nothing else but the motion of bodies explicating themselves.

B. The comparison of Air to Eeles in water I suppose will be well received by our Academians.

P.66. *Vides, &c.* You see how foolish a thing it is to bring for the explication of such effects Metaphorical words, as the *shunning of a Vacuum*, the *abhorrence of Nature, &c.* which heretofore the Schools used to defend their reputation.

Ibid. In the Gardeners Watering-pots therefore is the water suspended, because that which issues out at so small a hole is so little, that it cannot diffuse it self to such a length, that by its descent it may give passage to the Air through the circumferences of the holes. Nor can the Air driven off by the water going out find any other place besides that which the water leaves.

P.67. *Qui per, &c.* He that sucks water into his mouth by a Pipe, first sucks up the Air between, whereby he removes the distended external Air, which being removed (the world being full) it can have no place but by removing the next, and so by continual pulsion the water is at length driven into the Pipe, and doth succeed the Air which is suckt out.

P.69. *Id vero, &c.* But that is impossible: For in a Siphon, unless both legs are filled with water, the water will not ascend out of the Bason. The cause of its ascent into the cloth is that motion of the earthy Atoms which

which are near the water, I say the simple circular motion communicated to the Air in which they move, which Atoms striking the water beat it up into the woolly matter, which beating of them against the cloth makes it more and more moist, till it becomes all over wet. And when it is so, &c.

p.70. *A. Fateris, &c.* You confess then that your Collegiates have as yet in nothing advanced the knowledge of natural causes, but that one of them hath found out an Engine, in which there may be such a motion of the Air excited, that the parts of the Sphere may together every way tend unto the Centre, and that the *Hypotheses* of Mr. *Hobbes*, before probable enough, may be thence made more probable.

B. Right; I am not ashamed to confess it; for it is somewhat to arrive so far, if we can make no further progress.

p.71. *A.* Why *so far*? To what end such preparation and charge for Engines difficult to be made, to make no further discovery then Mr. *Hobbes* had made before you? Why did you not rather begin where he ended? Why did you not use the Principles he had laid? and when *Aristotle* had rightly said, That *without the knowledge of Motion there is no knowledge of Nature*, how durst you take such a task upon your selves?

Ibid. *Est, &c.* *Thus to have made an entrance, though we miss*
Of further progress, some performance is.

p.75. But most of us distinguish the nature of fluid from that which is not fluid, by the greatness of the parts of which any body consists and is made up with. Wherefore we do not onely look upon Air, Water, and all Liquors, but upon Ashes also and Dust, as fluid bodies. And we deny not that fluid things may be made of things not fluid; for we do not digest the Notion of infinite Divisibility.

A. Infinite Division cannot be conceived, but (*infinite*) Divisibility may easily. I on the contrary do not understand the distinction of Fluids and not Fluids, which you take from the greatness of the parts: could I digest this, I must say, the ruines of shattered rubbish stones that lye in *Pauls* were fluid. But if those ruines cannot be called fluid because the stones are too big, define me the bigness that the parts of a ruin'd wall must have that they may be called fluid. But you that cannot understand infinite Divisibility, tell me what you think to be the cause why I should think it more hard for Almighty God to create a fluid body less then any Atom proposed, that its parts might actually flow, then to create the Ocean. Therefore you make me despair of any fruit of your meeting, by saying, that they think Air, Water, and other *fluids* consist of *Non-fluids*; as if a wall that began to fall and be ruinous were called by them a fluid body.

If they may ſpeak ſo, every thing is fluid, for even Marble it ſelf may be broken into parts leſs then any Atom imagin'd by *Epicurus*.

P.76. *Ruina, &c.* The ruines that lye in *Pauls* Church might be called fluid.

Ibid. *Si ſic, &c.* If they may ſo ſpeak, there is nothing but is fluid, for even Marble may be beaten into parts leſs then any *Epicurean* Atom.

P.77. *Diviſio, &c.* Diviſion that is infinite cannot be conceived, Diviſibility may eaſily.

P.78,79. *Quin corpuſcula, &c.* But the Corpuſcles (ſuch as are the Atoms ſuppoſed by *Lucretius* and alſo by Mr. *Hobbes*) being hard before might be eaſily compacted by any of the mention'd cauſes, ſo that it is not to be doubted but that the whole to be made of thoſe Corpuſcles will be hard.

P.79. *Si dura, &c.* If hard bodies are made out of parts originally hard, why are not fluid bodies made of parts originally fluid? Could great *fluids*, as the *Æther*, be created; and could not ſmall ones? He that firſt made a body hard or fluid, could if he would have made it greater or leſs then any other propoſed body. Now if a fluid body be made of parts not fluid, (as you ſpeak) and hard bodies onely from hard parts; doth it not follow that nothing neither fluid nor hard is made of original fluids?

P.81. *B. Quænam, &c.* What are the Principles of Fluidity and Firmneſs?

A. Of Fluidity nothing but Reſt, of Firmneſs Motion, ſuch as is fit to produce that effect. By Reſt I underſtand the reſt of two parts one with another, when they each touch, but neither preſs one another. For entire bodies of fluids may be in motion their fluidity abiding, and hard bodies be at reſt although their parts be in motion.

P.82. *At ex hinc, &c.* And hence it is manifeſt that there is a great compreſſion in the Air ſo moved and ſhut up, namely, ſo great as that force by which it was driven in was able to make; and alſo that from ſo great compreſſion ſome degree of conſiſtence muſt be made, though leſs then that of the conſiſtence of water. Now if in all the ſame Particles of Air, beſides the motion by which one preſſes another, there was alſo the ſimple circular motion, and that vehement enough, it would be almoſt impoſſible any one of them ſhould be moved from its little circle; but that the other Particles reſiſting, the whole would be preſſed together, that is, become hard: For that is hard of which no part gives place but upon the motion of the whole. You ſee therefore that hardneſs may be made in a moſt fluid body by this ſimple circular motion of Particles, which was before imparted to them by two contrary motions.

p.83. *At qꝫ talis, &c.* And such indeed may be the cause of the *Durum primum*, or first hard body. But of the second, that is, of the cohesion of two of these first hard bodies, the cause may be the very same simple circular motion, conjoyned with their superficial contact, or perhaps their being one with another intricated.

Ibid. *Si supponamus, &c.* If we suppose with them that the cause of hardness is the greatness or thickness of the parts, what reason can we give why congeled water should be harder or firmer then the same water is before such congelation?

p.84. *Ut sit, &c.* That it be conceivable, that is, not absurd; and that from its being granted the necessity of the *Phanomenon* may be inferr'd.

p.85. *Qui, &c.* They which putting bodies to bodies shew the new and admirable works of Nature do wonderfully inflame the minds of men with the love of Philosophy, and do not a little instigate them unto the search of Causes, and on that account are worthy of commendation.

True; for they enrich Natural History, without which Natural Science is in vain sought for.

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Mr.

FINIS.

ERRATA.

PAge 19. line 26. lege *Earthy particles.* p.20. l.14. *plena erat.* p.47. in the Marg. lege *Page* 13. p.63. l.30. *Ergo pars* BC.

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AND WEIGH