

NEW 1846  
EXPERIMENTS

*Physico-Mechanicall,*

Touching

The SPRING of the AIR,

and its EFFECTS,

(Made, for the most part, in a New

PNEUMMATICAL ENGINE)

Written by way of LETTER

To the Right Honorable Charles

Lord Vicount of Dungarvan,

Eldest Son to the EARL of CORKE.

By the Honorable Robert Boyle Esq;



August OXFORD:

Printed by H. Hall, Printer to the University,  
for Tho: Robinson. 1660.



## To the Reader

**A**lthough the following Treatise being far more prolix then becomes a Letter, and then I at first intended it; I am very unwilling to increase the already excessive bulk of the Book by a Preface, yet there are some particulars that I think myself oblig'd to take notice of to the Reader, as things, that will either concern him to know, or me to have known.

In the first place then: If it be demanded why I publish to the World a Letter, which by its Stile and diverse Passages, appears to have been written as well For, as To a particular Person; I have chiefly these two things to answer: The one, That the Experiments therein related, having been many of them try'd in the presence of Ingenious Men; and by that means having made

## To the Reader

Some noise among the Virtuosi (insomuch that some of them have been sent into Foreign Countries, where they have had the luck not to be despis'd) I could not without quite tiring more then one Amanuensis, give out half as many Copies of them as were so earnestly desired, that I could not civilly refuse them. The other, That intelligent Persons in matters of this kinde perswaded me, that the publication of what I had observ'd touching the Nature of the Air, would not be useless to the World; and that in an Age so taken with Novelties as is ours, these new Experiments would be grateful to the Lovers of free and real Learning: So that I might at once comply with my grand Design of promoting Experimental and Useful Philosophy, and obtain the great satisfaction of giving some to ingenious Men; the hope of which, is, I confess, a temptation that I cannot easily resist.

Of my being somewhat prolix in many of my Experiments, I have these Reasons to render, That some of them being altogether new, seem'd to need the being circumstantially related, to keep the Reader from distrusting them: That divers Circumstances I did here and there set down for fear of forgetting them, when I may hereafter  
have

## To the Reader

have occasion to make use of them in my other Writings: That in divers cases I thought it necessary to deliver things circumstantially, that the Person I address'd them to, might without mistake, and with as little trouble as is possible, be able to repeat such unusual Experiments: and that after I consented to let my Observations be made publick, the most ordinary Reason of my prolixity was, That foreseeing that such a trouble as I met with in making those trials carefully, and the great expence of time that they necessarily require, (not to mention the charges of making the Engine, and employing a man to manage it) will probably keep most men from trying again these Experiments; I thought I might doe the generality of my Readers no unacceptable peice of service, by so punctually relating what I carefully observ'd, that they may look upon these Narratives as standing Records in our new Pneumaticks, and need not reiterate themselves an Experiment to have as distinct an Idea of it, as may suffice them to ground their Reflections and Speculations upon.

And because sometimes 'tis the Discourse made upon the Experiment that makes it appear prolix, I have commonly left a con-

## To the Reader.

spicuous interval betwixt such Discourses, and the Experiments whereunto they belong, or are annexed; that they who desire onely the Historical part of the account we give of our Engine, may read the Narratives, without being put to the trouble of reading the Reflections too: Which I here take notice of, for the sake of those that are well vers'd in the New Philosophy, and in the Mathematicks; that such may skip what was design'd, but for such Persons as may be less acquainted even then I, with matters of this nature (scarce so much as mention'd by any Writer in our Language) and not for them from whom I shall be much more forward to learn, then to pretend to teach them. Of my being wont to speak rather doubtfully, or hesitatingly, then resolutely, concerning matters wherein I apprehend some difficulty, I have in another Treatise (which may, through Gods Assistance, come abroad ere long) given a particular, and I hope a satisfactory account: Wherefore I shall now defend my Practice but by the Observation of Aristotle, who somewhere notes, That to seem to know all things certainly, and to speak positively of them, is a trick of bold and yong Fellows: Whereas those that are indeed

## To the Reader.

deed intelligent and considerate, are wont to employ more wary and diffident Expressions, or (as he speaks) *οὐκ ἀποκρίσειν ἀπὸ τοῦ ἰσως, ἢ τὸ τὰ ἄλλα.*

There are divers Reflections, and other Passages in the following Epistle, and even some Experiments (occasionally mention'd) which may seem either impertinent or superfluous, but are not so: Being purposely written, either to evince some truth oppos'd, or disprove some erroneous conceit maintain'd, by some eminent New Philosopher, or by some other Ingenious Men, who, I presum'd, would easily forgive me the having on such occasions purposely omitted their Names; though an inquisitive Person will probably discover divers of them, by the mention of the Opinions disprov'd in the Experiments I am excusing.

Ever since I discern'd the usefulness of speculative Geometry to Natural Philosophy, the unhappy Distempers of my Eyes, have so far kept me from being much conversant in it, that I fear I shall need the pardon of my Mathematical Readers, for some Passages, which if I had been deeply skill'd in Geometry, I should have treated more accurately.

And

## To the Reader

And indeed, having, for Reasons else where deduc'd, purposely kept my self a stranger to most of the new Hypotheses in Philosophy, I am sensible enough that the Engine I treat of has prevail'd with me to write of some subjects which are sufficiently remote from those I have been most conversant in. And having been reduc'd to write the greatest part of the ensuing Letter at a distance, not onely from my Library, but from my own Manuscripts, I cannot but fear that my Discourses do not onely want many choice things wherewith the Learned Writings of others might have enriched or imbellished them: But that partly for this Reason, and partly for that touch'd upon a little before, It is possible I may have mention'd some Notions already publish'd by others, without taking notice of the Authors, not out of any design to defraud deserving Men, but for want of knowing such particulars to have been already publish'd by them: Especially the Experiments of our Engine being themselves sufficient to hint such Notions as we build upon them.

The order of the Experiments every Reader may alter, as suits best with his own Design in perusing them: For not onely all those

## To the Reader

those betwixt whom there is an Affinity in Nature (by belonging to one subject) are not always plac'd one by another, but they are not still set down so much as in the order wherein they were made; but most commonly in that casual one wherein my occasions induc'd me to dispatch them to the Press. And, which is worse, I did usually send quite away the former Experiments, before the later were written, or perhaps so much as made: Whereby I lost the advantage of correcting and supplying the Imperfections of what I had formerly written, by the light of my subsequent Tryals and Discoveries.

Besides all this, the distemper in my eyes forbidding me not onely to write my self so much as one Experiment, but even to read over my self what I dictat'd to others. I cannot but fear, that besides the Authors mistakes, this Edition may be blemish'd by many, that may be properly imputed to a very unskilful Writer (whom I was often times by haste reduc'd against my custom to employ) and may have escaped the Diligence of that Learned Friend, that does me the favor to over-see the Press; especially there being the distance of two days Journey betwixt it and me.

I need not perhaps represent to the equitable

## To the Reader

table Reader, how much the strange Confusions of this unhappy Nation, in the midst of which I have made and written these Experiments, are apt to disturb that calmness of Minde, and undistractedness of Thoughts, that are wont to be requisite to Happy Speculations. But I presume, that by all these things put together, he will readily perceive, That I have been so far from following the Poets prudent Counsel touching the slow Publication of Books design'd to purchase credit by,

— Nonumque prematur in Annum

that I suffer this Treatise to come abroad into the World with a multitude of Disadvantages.

But if it be demanded, why then I did not make it fitter for the Press before I sent it thither? my Answer must be, That not at first imagining that this sort of Experiments would prove any thing near so troublesome, either to make, or to Record, as I afterwards found them, I did, to engage the Printer to dispatch, promise him to send him the whole Epistle in a very short time: So that although now and then the occasional vacations of the Press, by reason of Festivals,

## To the Reader.

vals, or the absence of the Corrector, gave me the leisure to exspaciate upon some subject; yet being oftentimes call'd upon to dispatch the Papers to the Press, my promise, and many unexpected Avocations, obliged me to a haste, which, though it have detracted nothing from the Faithfulness of the Historical part of our Book, has (I fear) been disadvantageous enough to all the rest. And I made the less scruple to let the following Papers pass out of my hands, with all their Imperfections; because, as the publick Affairs, and my own, were then circumstanc'd, I knew not when (if at all) I should be again in a condition to prosecute Experiments of this kinde; especially, since (to omit my being almost weary of being, as it were, confin'd to one sort of Experiments) I am pre-ingag'd (if it please God to vouchsafe me Life and Health) to employ my first leisure in the publication of some other Physiological Papers, which I thought 'twould make me much the fitter to take in hand, if I first dispatch'd all that I had at this time to write touching our Engine.

I have this further to adde, by way of Excuse, That as it has been my design in publishing these Experiments to gratifie Inge-

## To the Reader.

Ingenious men; so, if I have not been much flattered, I may hope that the various hints to be met with in the following Letter, will (at least) somewhat awaken mens thoughts, & excite them to new speculations (such as perhaps even inquisitive men would scarce else light upon) and I need not despair, that even the examination of such new Suspicions and Enquiries will hence also, at least Occasionally, be facilitated: I said Occasionally, because it being, as 'tis proverbially said, Facile Inventis addere. It seems not irrational to expect, that our Engine it self, and divers of our Experiments, will be much promoted by the Industry of Inventive and Mathematical Wits, whose contrivances may easily either correct or supply, and consequently surpass many of those we have made use of. And, particularly, if Men by skill and patience can arrive both to evacuate such Receivers as ours, till there be no more Air left in them, then there seems to have remain'd in the Glasses made use of about the Magdeburgick Experiment (hereafter to be mentioned) and to keep out the Air for a competent while; the Usefulness and Discoveries of our Engine, will not be a little advanc'd. And perhaps that may belong to it, which I re-

mem-

## To the Reader.

member Seneca speaks of Nature, Initiatos (says he) nos credimus, in Vestibulo ejus hæremus: For being now in a place where we are not quite destitute of moderately skilful Artificers, we have, since the Conclusion of the following Letter, made some Additions to our Engine, by whose help we finde (upon some new tryals) that we may be able, without much of new trouble, to keep the ambient Air out of the exhausted Receiver for a whole day; and perhaps we should be able to keep it out much longer, if before we shall have dispatch'd some urgent Affairs, and publish'd some Papers for which a kinde of Promise is thought to make us Debtors to the Press, we could be at leisure to prosecute such Experiments, as may possibly afford a Supplement to the following Treatise, from which I shall now no longer detain the Reader.

I know

Experi-  
ment 1.

**T**O proceed now to the *Phænomena*, exhibited to us by the Engine above described; I hold it not unfit to begin with what does constantly and regularly offer it self to our observation, as depending upon the Fabrick of the Engine it self, and not upon the nature of this or that particular Experiment which 'tis employed to try.

First, Then upon the drawing down of the Sucker, (the Valve being shut) the Cylindrical space, deserted by the Sucker, is left devoid of Air; and therefore, upon the turning of the Key, the Air contained in the Receiver rushes into the emptied Cylinder, till the Air in both those Vessels be brought to about an equal measure of dilatation. And therefore, upon shutting the Receiver by returning the Key, if you open the Valve, and force up the Sucker again, you will finde, that after this first exsuction you will drive out almost a whole Cylinder full of Air: But at the following exsuctions, you will draw less and less of Air out of the Receiver into the Cylinder, because that there will still remain less and less Air in the Receiver

Receiver it self; and consequently, the Particles of the remaining Air, having more room to extend themselves in, will less press out one another. This you will easily perceive, by finding, that you still force less and less Air out of the Cylinder; so that when the Receiver is almost exhausted, you may force up the Sucker almost to the top of the Cylinder, before you will need to unstop the Valve to let out any Air: And if at such time, the Valve being shut, you let go the handle of the Pump, you will finde the Sucker forcibly carryed up to the top of the Cylinder, by the protrusion of the external Air; which, being much less rarified then that within the Cylinder, must have a more forcible pressure upon the Sucker, then the internal is able to resist: And by this means you may know how far you have emptied the Receiver. And to this we may adde, on this occasion, that constantly upon the turning of the Key to let out the Air from the Receiver, into the emptied Cylinder, there is immediately produced a considerably brisk noise, especially whil'st there is any plenty of Air in the Receiver.

C 3

For



For the more easie understanding of the Experiments tryable by our Engine, I thought it not superfluous, nor unseasonable in the recital of this first of them, to insinuate that notion by which it seems likely that most, if not all, of them will prove explicable. Your Lordship will easily suppose, that the Notion I speak of is, That there is a Spring, or Elastical power in the Air we live in. By which *ελασπρ* or Spring of the Air; that which I mean is this: That our Air either consists of, or at least abounds with, parts of such a nature, that in case they be bent or compress'd by the weight of the incumbent part of the Atmosphere, or by any other Body, they do endeavor, as much as in them lies, to free themselves from that pressure, by bearing against the contiguous Bodies that keep them bent; and, as soon as those Bodies are remov'd or reduced to give them way, by presently unbending and stretching out themselves, either quite, or so far forth as the contiguous Bodies that resist them will permit, and thereby expanding the whole parcel of Air, these elastical Bodies compose.

This

This Notion may perhaps be somewhat further explain'd, by conceiving the Air near the Earth to be such a heap of little Bodies, lying one upon another, as may be resembled to a Fleece of Wooll. For this (to omit other likenesses betwixt them) consists of many slender and flexible Hairs; each of which, may indeed, like a little Spring, be easily bent or rould up; but will also, like a Spring, be still endeavouring to stretch it self out again. For though both these Haires, and the Aerial Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by vertue of its structure) is endow'd with a Power or Principle of self-Dilatation; by vertue whereof, though the hairs may by a Mans hand be bent and crouded closer together, and into a narrower room then suits best with the nature of the Body: Yet whil'st the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its Expansion. And upon the removall of the external pressure, by opening the hand more or less, the compressed Wooll does, as it were, spontaneously expand or display it self towards

the recovery of its former more loose and free condition, till the Fleece have either regain'd its former Dimensions, or at least, approach'd them as near as the compressing hand (perchance not quite open'd) will permit. This Power of self-Dilatation, is somewhat more conspicuous in a dry Sponge compress'd, then in a Fleece of Wooll. But yet we rather chose to imploy the latter, on this occasion, because it is not like a Sponge, an entire Body, but a number of slender and flexible Bodies, loosely complicated, as the Air it self seems to be.

There is yet another way to explicate the Spring of the Air, namely, by supposing with that most ingenious Gentleman, Monsieur *Des Cartes*, That the Air is nothing but a Congeries or heap of small and (for the most part) of flexible Particles; of several sizes, and of all kinde of Figures which are rais'd by heat (especially that of the Sun) into that fluid and subtle Etheriall Body that surrounds the Earth; and by the restless agitation of that Celestial Matter wherein those Particles swim, are so whirl'd  
round,

round, that each Corpuscle endeavours to beat off all others from coming within the little Sphear requisite to its motion about its own Center; and (in case any, by intruding into that Sphear shall oppose its free Rotation) to expell or drive it away: So that according to this Doctrine, it imports very little, whether the particles of the Air have the structure requisite to Springs, or be of any other form (how irregular soever) since their Elastical power is not made to depend upon their shape or structure, but upon the vehement agitation, and (as it were) brandishing motion, which they receive from the fluid *Ether* that swiftly flows between them, and whirling about each of them (independently from the rest) not onely keeps those slender Aërial Bodies separated and stretcht out (at least, as far as the Neighbouring ones will permit) which otherwise, by reason of their flexibleness and weight, would flag or curl; but also makes them hit against, and knock away each other, and consequently require more room, then that which if they were compress'd, they would take up.

By

By these two differing ways, my Lord, may the Spring of the Air be explicated. But though the former of them be that, which by reason of its seeming somewhat more easie, I shall for the most part make use of in the following Discourse: yet am I not willing to declare peremptorily for either of them, against the other. And indeed, though I have in another Treatise endeavoured to make it probable, that the returning of Elastical Bodies (if I may so call them) forcibly bent, to their former position, may be Mechanically explicated: Yet I must confess, that to determine whether the motion of Restitution in Bodies, proceed from this, That the parts of a Body of a peculiar Structure are put into motion by the bending of the spring, or from the endeavor of some subtle ambient Body, whose passage may be oppos'd or obstructed, or else it's pressure unequally resisted by reason of the new shape or magnitude, which the bending of a Spring may give the Pores of it: To determine this, I say, seems to me a matter of more difficulty, then at first sight one would easily imagine it. Wherefore I shall decline meddling with a subject, which is much more hard to be explicated,

ted, then necessary to be so, by him, whose business it is not, in this Letter, to assign the adequate cause of the Spring of the Air, but onely to manifest, That the Air has a Spring, and to relate some of its effects.

I know not whether I need annex that, though either of the above-mention'd Hypotheses, and perhaps some others, may afford us an account plausible enough of the Air-spring; yet I doubt, whether any of them gives us a sufficient account of its Nature. And of this doubt, I might here mention some Reasons, but that, peradventure, I may (God permitting) have a fitter occasion to say something of it elsewhere. And therefore I should now proceed to the next Experiment, but that I think it requisite, first, to suggest to your Lordship what comes into my thoughts, by way of Answer to a plausible Objection, which I foresee you may make against our propos'd Doctrine, touching the Spring of the Air. For it may be alleadged, that though the Air were granted to consist of Springy Particles (if I may so speak) yet thereby we could onely give an account of the Dilatation of the Air in Wine-Guns and other

other pneumatical Engines wherein the Air has been compress'd, and its Springs violently bent by an apparent externall force; upon the removall of which, 'tis no wonder that the Air should, by the motion of restitution, expand it self till it have recovered its more natural dimensions: whereas in our above-mentioned first Experiment, and in almost all others tryable in our Engine, it appears not that any compression of the Air precedits spontaneous Dilatation or Expansion of it self. To remove this difficulty, I must desire Your Lordship to take notice, that of whatever nature the Air, very remote from the Earth, may be, and whatever the Schools may confidently teach to the contrary, yet we have divers Experiments to evince, that the Atmosphere we live in is not (otherwise then comparatively to more ponderous Bodies) light, but heavy: And did not their gravity hinder them, it appears not why the steams of the Terraqueous Globe, of which our Air in great part consists, should not rise much higher then the Refraction of the Sun, and other Stars give men ground to think, that the Atmosphere, even in the judgement of those

Recent

Recent Astronomers, who seem willing to enlarge its bounds as much as they dare, does reach.

But lest you should expect my seconding this Reason by Experience; and lest you should object, That most of the Experiments that have been propos'd to prove the gravity of the Air, have been either barely propos'd, or perhaps not accurately try'd; I am content, before I pass further, to mention here, That I found a dry lambs-bladder containing near about two thirds of a pint, and compress'd by a packthred tyed about it, to loose a grain and the eighth part of a grain of its former weight, by the recess of the Air upon my having prickt it: And this with a pair of Scales, which when the full Bladder and the correspondent weight were in it, would manifestly turn either way with the 32 part of a grain. And if it be further objected, That the Air in the Bladder was violently compress'd by the Pack-thred and the sides of the Bladder, we might probably (to wave prolix answers) be furnish'd with a Reply, by setting down the differing weight of our Receiver, when empty'd and when full of uncompress'd Air, if we could here procure scales fit for so nice an experiment;

since

since we are informed, that in the *German* Experiment, commended at the beginning of this Letter, the Ingenious Tryers of it found, That their Glais Vessei, of the capacity of 32 measures, was lighter when the Air had been drawn out of it, then before, by no less then one ounce and  $\frac{2}{16}$  that is, an ounce and very near a third: But of the gravity of the Air, we may elsewhere have occasion to make further mention.

Taking it then for granted that the Air is not devoid of weight, it will not be uneasy to conceive, that that part of the Atmosphere wherein we live, being the lower part of it, the Corpuscles that compose it, are very much compress'd by the weight of all those of the like nature that are directly over them, that is, of all the Particles of Air, that being pil'd up upon them, reach to the top of the Atmosphere. And though the height of this Atmosphere, according to the famous *Kepler*, and some others, scarce exceeds eight common miles; yet other eminent and later Astronomers, would promote the confines of the Atmosphere, to exceed six or seven times that number of miles. And the diligent and learned  
*Rivolo*

*Rivolo* makes it probable, that the Atmosphere may, at least in divers places, be at least 50 miles high. So that according to a moderate estimate of the thickness of the Atmosphere, we may well suppose, that a Column of Air, of many miles in height, leaning upon some springy Corpuscles of Air here below, may have weight enough to bend their little springs, and keep them bent: As, to resume our former comparison, if there were fleeces of Wooll pil'd up to a mountainous height upon one another, the Hairs that compose the lowermost locks which support the rest, would, by the weight of all the Wool above them, be as well strongly compressed, as if a man should squeeze them together in his hands, or imploy any such other moderate force to compress them. So that we need not wonder, that upon the taking off the incumbent Air from any parcel of the Atmosphere here below, the Corpuscles, whereof that undermost Air consists, should display themselves, and take up more room then before.

And if it be objected, That in Water, the weight of the upper and of the lower part is the same: I answer, That besides that,

that it may be well doubted whether the observation, by reason of the great difficulty have been exactly made, there is a manifest disparity betwixt the Air and Water: For I have not found, that upon an Experiment purposely made, (and in another Treatise Recorded) that Water will suffer any considerable compression; whereas we may observe in Wind-Guns (to mention now no other Engines) that the Air will suffer it self to be crouded into a comparatively very little room; in so much, that a very diligent Examiner of the *Phænomena* of Wind-Guns would have us believe, that in one of them, by condensation, he reduc'd the Air into a space at least eight times narrower then it before possesseth. And to this, if we adde a noble *Phænomenon* of the Experiment *De Vacuo*; these things put together, may for the present suffice to countenance our Doctrine. For that noble Experimenter, Monsieur *Pascal* (the Son) had the commendable Curiosity to cause the *Torricellian* Experiment to be try'd at the foot, about the middle, and at the top of that high Mountain (in *Auvergne*, if I mistake not) commonly call'd *Le Puy de Domme*; whereby it was found, That the *Mercury*  
in

in the Tube fell down lower, about three inches, at the top of the Mountain then at the bottom. And a Learned Man a while since inform'd me, That a great *Virtuoso*, friend to us both, has, with not unlike success, tryed the same Experiment in the lower and upper parts of a Mountain in the West of *England*: Of which, the reason seems manifestly enough to be this, That upon the tops of high Mountains, the Air which bears against the restagnant Quick-silver, is less press'd by the less ponderous incumbent Air; and consequently is not able totally to hinder the descent of so tall and heavy a Cylinder of Quick-silver, as at the bottom of such Mountains did but maintain an *Equilibrium* with the incumbent Atmosphere:

And if it be yet further Objected against what hath been propos'd touching the compactness and pressure of the Inferior Air; That we finde this very Air to yield readily to the motion of little Flies, and even to that of Feathers, and such other light and weak Bodies; which seems to argue, that the particles of our Air are not so compress'd as we have represented them, especially, since by our former Experiment it appears, that the Air rea-  
D dily

dily dilated it self downward, from the Receiver into the Pump, when 'tis plain, that it is not the incumbent Atmosphere, but onely the subjacent Air in the bra's Cylinder that has been remov'd: If this, I say, be objected, we may reply, That when a man squeezes a Fleece of Wool in his hand, he may feel that the Wool incessantly bears against his hand, as that which hinders the hairs it consists of, to recover their former and more natural extent. So each parcel of the Air about the Earth, does constantly endeavour to thrust away all those contiguous Bodies, whether Aërial or more gross, that keep them bent, and hinder the expansion of its parts, which will dilate themselves or flie abroad towards that part, whether upwards or downwards, where they finde their attempted Dilatation of themselves less resisted by the neihgoring Bodies. Thus the Corpuscles of that Air we have been all this while speaking of, being unable, by reason of their weight, to ascend above the Convexity of the Atmosphere, and by reason of the resistance of the surface of the Earth and Water, to fall down lower, they are forced, by their own gravity and this resistance, to expand and  
diffuse

diffuse themselves about the Terrestrial Globe; whereby it comes to pass, that they must as well press the contiguous Corpuscles of Air that on either side oppose their Dilatation, as they must press upon the surface of the Earth, and, as it were recoyling thence, endeavor to thrust away those upper particles of Air that lean upon them.

And as for the easie yielding of the Air to the Bodies that move in it, if we consider that the Corpuscles whereof it consists, though of a springy nature, are yet so very small, as to make up (which 'tis manifest they doe) a fluid Body, it will not be difficult to conceive, that in the Air, as in other Bodies that are fluid, the little Bodies it consists of are in an almost restless motion, whereby they become (as we have more fully discoursed in another Treatise) very much disposed to yield to other Bodies, or easie to be displac'd by them, and that the same Corpuscles are likewise so variously mov'd, as they are intire Corpuscles, that if some strive to push a Body plac'd among them towards the right hand (for instance) others, whose motion has an opposite determination, as strongly thrust the same  
D 2 Body

*In a Discourse touching fluidity and firmness.*

Body towards the left; whereby neither of them proves able to move it out of its place, the pressure on all hands being reduced as it were to an *Aequilibrium*: so that the Corpuscles of the Air must be as well sometimes considered under the notion of little Springs, which remaining bent, are in their entire bulk transported from place to place; as under the notion of Springs displaying themselves, whose parts flie abroad whilst as to their entire bulk they scarce change place: As the two ends of a Bow, shot off, fly from one another, whereas the Bow it self may be held fast in the Archers hand; and that it is the equal pressure of the Air on all sides upon the Bodies that are in it, which causes the easie Cession of its parts, may be argu'd from hence: That if by the help of our Engine the Air be but in great part, though not totally drawn away from one side of a Body without being drawn away from the other; he that shall think to move that Body too and fro, as easily as before, will finde himself much mistaken.

In verification of which we will, to divert your Lordship a little, mention here a *Phenomenon* of our Engine, which even  
to

to divers ingenious persons has at first sight seem'd very wonderful.

**T**He thing that is wont to be admired, *Experiment 2.* and which may pass for our second Experiment is this, That if, when the Receiver is almost empty, a By-stander be desired to lift up the brass Key (formerly described as a stopple in the brass Cover) he will finde it a very difficult thing to do so, if the Vessel be well exhausted; and even when but a moderate quantity of Air has been drawn out, he will, when he has lifted it up a little, so that it is somewhat loose from the sides of the lip or socket, which (with the help of a little oyl) it exactly filled before, he will (I say) finde it so difficult to be lifted up, that he will imagine there is some great weight fastned to the bottom of it. And if (as sometimes has been done for merriment) onely a Bladder be tyed to it, it is pleasant to see how men will marvail that so light a Body, filled at most but with Air, should so forcibly draw down their hand as if it were fill'd with some very ponderous thing: whereas the cause of this pretty *Phenomenon* seems plainly enough to



MIT OpenCourseWare  
<http://ocw.mit.edu>

STS.003 The Rise of Modern Science  
Fall 2010

For information about citing these materials or our Terms of Use, visit: <http://ocw.mit.edu/terms>.