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Observatorio de San Fernando

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Observatorio de Marina
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Núm. **4495**

1871

Ex. Sono. In  *M. S.*

New Experiments
PHYSICO-MECHANICAL,
Touching the AIR.

The Second Edition.

Whereunto is added
A DEFENCE of the Authors EXPLICATION
OF THE
EXPERIMENTS,
Against the OBJECTIONS
OF

FRANCISCUS LINUS,
And,
THOMAS HOBBS.

OBSERVATORIO DE MARINA
DE
SAN FERNANDO.

New Experiments
in
Mechanics
Touching the
Use of the

The Second Edition.

Whic is added

A Preface of the Authors Explanatory

OF THE
EXPERIMENTS
AND OBSERVATIONS
OF

FRANCIS BACON
AND
THOMAS HOBBES

NEW
EXPERIMENTS
PHYSICO-MECHANICAL,

Touching
The SPRING of the AIR, and its EFFECTS,
(Made, for the most part, in a New

Pneumatical 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;



OXFORD: Printed by H. Hall, Printer to the University,
for Tho: Robinson. 1662.

THE UNIVERSITY OF CHICAGO

PHYSICS DEPARTMENT

THEORY OF QUANTUM MECHANICS

LECTURE NOTES

BY

PROFESSOR

OF PHYSICS

AND

OF THE UNIVERSITY OF CHICAGO

PHYSICS DEPARTMENT

THEORY OF QUANTUM MECHANICS

LECTURE NOTES

BY



To the READER.



Although the following Treatise being far more prolix than becomes a Letter, and then I at first intended it; I am very unwilling to encrease 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 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 than 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



To 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 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 addressed 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 tryals 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 do the generality of my Readers no unacceptable piece 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 conspicuous 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, & 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 from them for whom I shall be much more forward to learn, then to pretend to teach them. Of my being wont to speak rather doubtfully, or hesitantly, then resolvedly, 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

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who somewhere notes, That to seem to know all things certainly, and to speak positively of them, is a trick of bold and young Fellows: whereas those that are indeed intelligent and considerate, are wont to employ more wary and diffident Expressions, or (as he speaks)

ἄπειρα ἢ ἐν τῷ ἰσῶς ἢ τῷ ταῖχα.

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 indeed, having, for Reasons elsewhere 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 hath 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 imbelished 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

Ex-

To the Reader.

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 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 dictated 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 oftentimes by haste, reduc'd against my custom, to imploy) and may have escaped the Diligence of that Learned Friend, that doth me the favor to oversee the Press; especially, there being the distance of two days Journey betwixt it and me.

I need not, perhaps, represent to the equitable 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

To the Reader.

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, or the absence of the Corrector, gave me the leisure to exspatiate 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 hath detracted nothing from the Faithfulness of the Historical part of our Book, hath (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 farther to adde, by way of Excuse, That as it hath been my design in publishing these Experiments to gratifie 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, and 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

To the Reader.

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 remember Seneca speaks of Nature: Initiatos (saith 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.

Friendly



Friendly Reader,

I Know all Persons that have a publick Spirit for the Advancement of Learning, will think much that this piece came not out in a Language of more general Use, then this you see it now attir'd in; especially since the Excellent Noble Person, who is the Author, is known to be well able himself (being almost universally a Linguist) to have given it either the Old Latin, or the Newer French Dress.

But if it be an Honor to a Language to be preferr'd, and this Honor breeds sometimes an Emulation, as anciently it did between the *Greeks* and *Romans*, it cannot be thought unhandsome for an English Nobleman to have preferr'd his own: And it may be a sufficient Reason for the Gentry of Foreign Parts to learn our Speech, or keep Interpreters, that they are sure to have for their requital, from many of our English Writers (as here from this Piece) much curiously ingenious, and profitable Learning.

But as to this Particular (give me leave to use Words from a Story) *Since the Mountain cannot come to Mahomet, Mahomet will go to the Mountain*: I mean thus; Because many witty Men, Persons of Honor and Estate especially, may be suppos'd to be able to make a better account, by employing their Studies and Time on Matter then Words, and so are justly impeded from learning Languages; And because (as I may judge) the Noble Author is willing to oblige all Men, He hath already provided, that this Piece

[a 2] shall

shall shortly be done into Latine, that so it may come home to divers worthy Persons in its Stream, who cannot travel to finde it out in its first Origine.

Having therefore leave so to do, I cannot forbear to give the World the Advertisement of this Latine Edition, lest some skilful Artist should take needless pains about a work, which will, ere long (by Gods furtherance) be done to his Hands; For such unprofitable expences of Study have too frequently happened, and too much to the disadvantage of Learning, for want of a sufficient Correspondence and Intercourse between such as are exercised in the Mines of Wisdom:

This is all the trouble I shall at present give you: Nor shall I need minde you, if you have a true gust for the Book you read, to have an honor and thankful regard to the Person that hath favor'd us with the Communication of these his Tryals, and is manifestly so great a Patron and Friend to Experimental Learning, and all true Wisdom; for should you fail in this, you might deservedly be depriv'd of some other Observations on the same subject, which the Author, I hear, hath made since the finishing of this Treatise.

I desire to be excused that I do not make Excuses for the slowness of the Publication, hoping that the long expectation you have had of it, will enhance, and not diminish your delight in the enjoyment of a piece like to be, amongst the Students in accurate Philosophy, of so general acceptance. Farewel.

R: Sb:



A SUMMARY of the chief Matters treated of in this
Epistolical Discourse.

- T**He Proæmium, wherein is set down the occasion of this Discourse. 1. The motives that induc'd the Author thereunto. 2. The hints he received. 3. The things wherein this Engine excels any that have yet been made use of. 4. The description of the Engine and its parts, 5 &c. The way of preparing and using it, 8 &c. The division of the Experiments tryable ther by into two sorts, and the difficulty of excluding the Air: 10, &c.
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TO



T O T H E
L O R D of D U N G A R V A N,
My Honored and Dear N E P H E W.

My Dear Lord,

Receiving in your last from *Paris*, a desire that I would adde some more Experiments to those I formerly sent You over: I could not be so much your Servant as I am, without looking upon that Desire as a Command; and consequently, without thinking my Self obliged to consider by what sort of Experiments it might the most acceptably be obey'd. And at the same time, perceiving by Letters from some other Ingenious Persons at *Paris*, that several of the *Virtuosi* there, were very intent upon the examination of the Interest of the Air, in hindring the descent of the Quick-silver, in the famous Experiment touching a *Vacuum*: I thought I could not comply with your Desires in a more fit and seasonable manner, then by prosecuting and endeavoring to promote that noble Experiment of *Torricellius*: and by presenting Your Lordship an account of my attempts to illustrate a subject, about which (it's being so much discours'd of where you are, together with your inbred Curiosity, and love of Experimental Learning) made me suppose you sufficiently inquisitive.

And though I pretend not to acquaint you, on this occasion, with any store of new Discoveries, yet possibly I shall be so hap-

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py, as to assist you to *know* some things which you did formerly but *suppose*; and shall present you, if not with new Theories, at least with new *Proofs* of such as are not yet become unquestionable. And if what I shall deliver, hath the good fortune to encourage and assist you to prosecute the Hints it will afford, I shall account my self, in paying of a duty to you, to have done a piece of Service to the Commonwealth of Learning. Since it may highly conduce to the advancement of that Experimental Philosophy, the effectual pursuit of which, requires as well a Purse as a Brain, to endear it to *hopeful* Persons of your Quality: who may accomplish many things, which others can but *wish*, or at most, but *design*, by being able to imploy the Presents of Fortune in the search of the Mysteries of Nature.

And I am not faintly induc'd to make choice of this Subject, rather than any of the expected Chymical ones, to entertain Your Lordship upon, by these two Considerations: The one, That the Air being so necessary to humane Life, that not onely the generality of Men, but most other Creatures that breath, cannot live many *minutes* without it; any considerable discovery of its Nature, seems likely to prove of moment to Mankind. And the other is, That the ambient Air, being that whereto both our own Bodies, and most of the others we deal with here below, are almost perpetually contiguous; not only its alterations have a notable and manifest share in those obvious Effects, that Men have already been invited to ascribe thereunto (such as are the various Distempers incident to humane Bodies especially if crazy in the Spring, the Autumn, and also on most of the great and sudden changes of Weather) but likewise, the further discovery of the Nature of the Air, will probably discover to us, that it concurs more or less to the exhibiting of many *Phænomena*, in which it hath hitherto scarce been suspected to have any Interest. So that a true account of any Experiment that is New concerning a thing, wherewith we have such constant and necessary intercourse, may not onely prove of some
 advantage

advantage to humane Life, but gratifie Philosophers, by promoting their Speculations on a Subject which hath so much opportunity to sollicite their Curiosity.

And I should immediately proceed to the mention of my Experiments, but that I like too well that worthy saying of the Naturalist *Pliny*, *Benignum est & plenum ingenui pudoris, fateri per quos profeceris*, not to conform to it, by acquainting your Lordship, in the first place, with the Hint I had of the Engine I am to entertain you with. You may be pleas'd to remember, that a while before our separation in *England*, I told you of a Book that I had heard of, but not perus'd, publish'd by the industrious Jesuit *Schottius*, wherein 'twas said, He related how that ingenious Gentleman, *Otto Gericke*, Consul of *Magdeburg*, had lately practic'd in *Germany* a way of emptying Glais Vessels, by sucking out the Air at the mouth of the Vessel, plung'd under Water: And you may also perhaps remember, that I express'd my self much delighted with this Experiment, since thereby the great force of the external Air (either rushing in at the open'd Orifice of the empty'd Vessel, or violently forcing up the Water into it) was rendred more obvious and conspicuous then in any Experiment that I had formerly seen. And though it may appear by some of those Writings I sometimes shew'd your Lordship, that I had been solicitous to try things upon the same ground; yet in regard this Gentleman was before-hand with me in producing such considerable Effects, by means of the extraction of Air, I think my self oblig'd to acknowledge the assistance and encouragement, the Report of his Performances hath afforded me.

But, as few inventions happen to be at first so compleat, as not to be either blemished with some deficiencies needful to be remedy'd, or otherwise capable of improvement: so when the Engine we have been speaking of, comes to be more attentively consider'd, there will appear two very considerable things to be desir'd in it. For first, the *Wind Pump* (as some body not

improperly calls it) is so contriv'd, that to evacuate the Vessel, there is requir'd the continual labor of two strong Men for divers hours. And next (which is an imperfection of much greater moment) the Receiver, or Glafs to be empty'd, consisting of one entire and uninterrupted Globe and Neck of Glafs; the whole Engine is so made, that things cannot be convey'd into it, whereon to try Experiments: So that there seems but little (if any thing) more to be expected from it, then those very few *Phanomena* that have been already observ'd by the Author, and recorded by *Schottus*. Wherefore to remedy these Inconveniences, I put both Mr. *G.* and *R. Hook* (who hath also the Honor to be known to your Lordship, and was with me when I had these things under consideration) to contrive some Air Pump, that might not, like the other, need to be kept under Water (which on divers occasions is convenient) and might be more easily manag'd: And after an unsuccessful tryal or two of ways propos'd by others, the last nam'd Person fitted me with a Pump, anon to be describ'd. And thus the first Imperfection of the *German* Engine, was in good measure, though not perfectly remedy'd: And to supply the second defect, it was considered that it would not perhaps prove impossible to leave in the Glafs to be empty'd, a hole large enough to put in a Mans Arm cloath'd; and consequently other Bodies, not bigger then it, or longer then the inside of the Vessel. And this Design seem'd the more hopeful, because I remembred, that having several years before often made the Experiment *De Vacuo* with my own hands; I had, to examine some Conjectures that occur'd to me about it, caused Glasses to be made with a hole at that end, which uses to be seal'd up, and had nevertheless been able, as occasion requir'd, to make use of such Tubes, as if no such holes had been left in them, by devising stopples for them, made of the common Plaister call'd *Diachylon*: which I rightly enough ghes'd, would, by reason of the exquisite commixtion of its small parts, and closeness of its texture, deny all access to the external

nal Air. Wherefore, supposing that by the help of such Plaisters, carefully laid upon the commissures of the stopple and hole to be made in the Receiver, the external Air might be hindered from insinuating it self between them, into the Vessel, we caus'd several such Glasses, as you will finde describ'd a little lower, to be blown at the Glass-house; and though we could not get the Work-men to blow any of them so large, or of so convenient a shape as we would fain have had; yet finding one to be tolerably fit, and less unfit then any of the rest, we were content to make use of it in that Engine: Of which, I suppose, you by this time expect the Description, in order to the recital of the *Phænomena* exhibited by it.

To give your Lordship then, in the first place, some account of the Engine it self: It consists of two principal parts; a glass Vessel, and a Pump to draw the Air out of it.

The former of these (which we, with the Glass-men, shall often call a Receiver, for its affinity to the large Vessels of that name, used by Chymists) consists of a Glass with a wide hole at the top, of a cover to that hole, and of a Stop-cock fastned to the end of the neck, at the bottom.

The shape of the Glass, you will finde express'd in the first Figure of the annexed Scheme. And for the size of it, it contain'd about 30 Wine Quarts, each of them containing near two Pound (of 16 Ounces to the Pound) of Water: We should have been better pleas'd with a more capacious Vessel, but the Glass-men professed themselves unable to blow a larger, of such a thickness and shape as was requisite to our purpose.

At the very top of the Vessel, (A) you may observe a round hole, whose Diameter (B C) is of about four Inches; and whereof, the Orifice is incircled with a lip of Glass, almost an Inch high: For the making of which lip, it was requisite (to mention that upon the by, in case your Lordship should have such another Engine made for you) to have a hollow and tapering Pipe of Glass drawn out, whereof the Orifice above mentioned

tioned was the Basis, and then to have the Cone cut off with an hot Iron, within about an Inch of the Points (B C.)

The use of the lip, is to sustain the cover delineated in the second Figure; where (D E) points out a brass Ring, so cast, as that it doth cover the lip (B C) of the first Figure, and is cemented on, upon it, with a strong and close Cement. To the inward tapering Orifice of this Ring (which is about three Inches over) are exquisitely ground the sides of the Brass stopple (F G;) so that the concave superficies of the one, and the convex of the other, may touch one another in so many places, as may leave as little access, as possible, to the external Air: And in the midst of this cover is left a hole (H I) of about half an Inch over, invironed also with a Ring or Socket of the same Mettal, and fitted likewise with a Brass stopple (K) made in the form of the Key of a Stop-cock, and exactly ground into the hole (H I) it is to fill; so as that, though it be turn'd round in the cavity it possesses, it will not let in the air, & yet may be put in or taken out at pleasure, for uses to be hereafter mentioned. In order to some of which, it is perforated with a little hole, (8) traversing the whole thickness of it at the lower end; through which, and a little Brass Ring (L) fastned to one side (no matter which) of the bottom of the stopple (F G) a string (8, 9 10) might pass, to be employ'd to move some things in the capacity of the empty'd Vessel, without any where unstopping it.

The last thing belonging to our Receiver, is the Stop-cock, designed in the first Figure by (N,) for the better fastening of which to the neck, and exacter exclusion of the Air, there was solder'd on to the shank of the Cock (X) a Plate of Tin, (M T U W) long enough to cover the neck of the Receiver. But because the cementing of this was a matter of some difficulty, it will not be amiss to mention here the manner of it; which was, That the cavity of the Tin Plate was fill'd with a melted Cement, made of Pitch, Rosin, and Wood-ashes, well incorporated; and to hinder this liquid Mixture from getting into the Orifice

orifice (Z) of the Shank, (X) that hole was stoppt with a Cork, to which was fastned a string, whereby it might be pull'd out at the upper Orifice of the Receiver; and then, the Glass neck of the Receiver being well warm'd, was thrust into this Cement, and over the Shank, whereby it was effected, that all the space betwixt the Tin Plate and the Receiver, and betwixt the internal superficies of the Receiver, and the Shank of the Cock, was fill'd with the Cement; and so we have dispatch'd the first and upper part of the Engine.

The undermost remaining part consists of a Frame, and of a sucking Pump, or as we formerly call'd it, an Air Pump, supported by it: The Frame is of Wood, small, but very strong, consisting of three legs, (1 1 1) so plac'd, that one side of it may stand perpendicular, that the free motion of the hand may not be hindered. In the midst of which frame, is transversly nail'd a board, (2 2 2) which may not improperly be call'd a Midriff, upon which rests, and to which is strongly fastned, the main part of the Pump it self, which is the onely thing remaining to be described.

The Pump consists of four parts, a hollow Cylinder, a Sucker, a handle to move that Sucker, and a Valve.

The Cylinder was (by a pattern) cast of Brass; it is in length about 14 Inches, thick enough to be very strong, notwithstanding the Cylindrical cavity left within it; this cavity is about three Inches Diameter, and makes as exact a Cylinder as the Artificer was able to bore. This hollow Cylindre is fitted with a Sucker, (4455) consisting of two parts; the one (44) somewhat less in Diameter then the cavity of the Cylinder; upon which is nail'd a good thick piece of tann'd Shoe-leather, which will go so close to the Cylinder, that it will need to be very forcibly knock'd and ram'd in, if at any time it be taken out; which is therefore done, that it may the more exactly hinder the Air from insinuating it self betwixt it and the sides of the Cylinder whereon it is to move.

To the midst of this former part of the Sucker is strongly fastned the other; namely, a thick and narrow plate of Iron (55) somewhat longer then the Cylinder, one of whose edges is smooth, but at the other edge it is indented (as I may so speak) with a row of Teeth, delineated in the Scheme, into whose intervals are to be fitted, the Teeth of a small Iron Nut, ($\alpha \beta$) (as Trades-men call it) which is fastned by two staples (22) to the under side of the formerly mention'd transverse board (222) on which the Cylinder rests, and is turn'd to and fro by the third piece of this Pump, namely, the handle or *Manubrium*, (7) of which the Figure gives a sufficient description.

The fourth and last part of this Cylinder, is the Valve, (R) consisting of a hole bored through at the top of the Cylinder, a little tapering towards the cavity; into which hole is ground a tapering Peg of Brass, to be thrust in, and taken out at pleasure.

The Engine being thus describ'd, it will be requisite to adde, that something is wont to be done before it be set on work, for the more easie moving of the Sucker, and for the better exclusion of the outward Air: which, when the Vessel begins to be exhausted, is much more difficult to be kept out, then one would easily imagine.

There must then be first powr'd in at the top of the Receiver a little sallad Oyl, partly to fill up any small intervals that may happen to be betwixt the contiguous surfaces of the internal parts of the Stop-cock: And partly, that it may be the more easie to turn the Key (S) backwards and forwards. Pretty store of Oyl must also be pour'd into the Cylinder, both that the Sucker may slip up and down in it the more smoothly and freely, and that the Air might be the better hindred from getting in between them: And for the like reasons, a little Oyl is to be used also about the Valve. Upon which occasion, it would not be omitted (for it is strange) that oftentimes, when neither the pouring in of Water, nor even of Oyl alone, prov'd capable to make the Sucker move easily enough in the Cylinder; a mixture

mixture of both those Liquors would readily (sometimes even to admiration) perform the desired effect. And lastly, the Brass cover of the Receiver, being put into the Brass ring formerly describ'd, that no Air may get between them, it will be very requisite to plaister over very carefully the upper edges of both, with the Plaister formerly mentioned, or some other as close, which is to be spread upon the edges with an hot Iron; that, being melted, it may run into and fill up all the crannies, or other little cavities, at which the Air might otherwise get entrance.

All things being thus fitted, and the lower shank (O) of the Stop-cock being put into the upper Orifice of the Cylinder (&) into which it was exactly ground; the Experimenter is first, by turning the handle, to force the Sucker to the top of the Cylinder, that there may be no Air left in the upper part of it: Then shutting the Valve with the Plug, and turning the other way, he is to draw down the Sucker to the bottom of the Cylinder; by which motion of the Sucker, the Air that was formerly in the Cylinder being thrust out, and none being permitted to succeed in its room, 'tis manifest that the cavity of the Cylinder must be empty in reference to the Air. So that if thereupon the Key of the Stop-cock be so turn'd, as that through the perforation of it, a free passage be opened betwixt the Cylinder and the Receiver, part of the Air formerly contain'd in the Receiver, will nimbly descend into the Cylinder. And this Air being, by the turning back of the Key, hinder'd from the returning into the Receiver, may, by the opening of the Valve, and forcing up of the Sucker to the top of the Cylinder again, be driven out into the open Air. And thus by the repetition of the motion of the Sucker upward and downward, and by opportunely turning the Key, and stopping the Valve, as occasion requires, more or less Air may be suck'd out of the Receiver, according to the exigency of the Experiment, and the intention of him that makes it.

Your Lordship will, perhaps, think that I have been unnecessarily prolix in this first part of my Discourse: But if you had seen how many unexpected difficulties we found to keep out the external Air, even for a little while, when some considerable part of the internal had been suck'd out; You would peradventure allow, that I might have set down more circumstances than I have, without setting down any, whose knowledge, he that shall try the Experiment, may not have need of. Which is so true, that, before we proceed any further, I cannot think it unseasonable to advertise Your Lordship, that there are two chief sorts of Experiments, which we design'd in our Engine to make tryal of: The one, such as may be quickly dispatch'd, and therefore may be try'd in our Engine, though it leak a little, because the Air may be faster drawn out, by nimbly plying the Pump, then it can get in at undiscern'd leaks; I say at undiscern'd leaks, because such as are big enough to be discover'd, can scarce be uneasy to be stopt. The other sort of Experiments consists of those that require, not onely that the internal Air be drawn out of the Receiver, but that it be likewise for a long time kept out of it. Such are the preservation of Animal and other Bodies therein, the germination and growth of Vegetables, and other tryals of several sorts, which it is apparent cannot be well made, unless the external Air can, for a competent while, be excluded: Since, even at a very small leak, there may enough get in, to make the *Vacuum* soon loose that name; by which I here declare once for all, that I understand not a space wherein there is no Body at all, but such as is either altogether, or almost totally devoid of Air.

Now this distinction of Experiments I thought fit to premise to the ensuing Narratives, because, upon tryal, we found it so exceeding (and scarce imaginably) difficult a matter, to keep out the Air from getting at all in at any imperceptible hole or flaw whatsoever, (in a Vessel immediately surrounded with the compressed Atmosphere,) that in spite of all our care and diligence,

gence, we never were able totally to exhaust the Receiver, or keep it when it was almost empty, any considerable time, from leaking more or less: although (as we have lately intimated) by unwearied quickness in plying the Pump, the internal Air can be much faster drawn out than the external can get in, till the Receiver come to be almost quite empty. And that's enough to enable Men to discover hitherto unobserved *Phænomena* of Nature.

The Experiments therefore of the first sort, will, I fear, prove the onely ones wherewith my Avocations will allow me to entertain Your Lordship in this Letter. For till your further Commands shall engage me to undertake, by Gods permission, such an Employment, and more leasure shall better fit me for it, I know not whether I shall be in a condition to try what may be done, to enable me to give you some account of the other sort of Experiments also.

EXPERIMENT I.

TO proceed now to the *Phænomena*, exhibited to us by the Engine above described; I hold it not unfit to begin with what doth 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 rusheth 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 there will still remain less and less Air in the 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 way know how far you have emptyed 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 consiberably brisk noise, especially whil't there is any plenty of Air in the Receiver.

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 *ελαστις* or Spring of the Air, that which I meane 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 lyeth, to free themselves from that pressure, by
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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 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 rouled up; but will also, like a Spring, be still endeavouring to stretch it self out again. For though both these Haires, and the Aereal Corpuscles to which we liken them, do easily yield to externall pressures; yet each of them (by virtue of its structure) is endow'd with a Power or Principle of selfe-Dilatation; by virtue 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, whilst the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposeth its Expansion. And upon the removall of the external pressure, by opening the hand more or less, the compressed Wooll doth, as it were, spontaneously expand or display it self towards the recovery of its former more loose and free condition, till the Fleece hath either regain'd its former Dimensions, or at least, approach'd them as neare as the compressing hand (perchance not quite open'd) will permit. This Power of selfe-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 intire 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;
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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 kind of Figures which are rais'd by heat (especially that of the Sun) into that fluid and subtle Ethereall Body that surrounds the Earth; and by the restlesse agitation of that Celestial Matter wherein those Particles swim, are so whirl'd round, that each Corpuscle endeavours to beat off all others from coming within the little Sphere 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 Aëreal 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 these two differing ways, my Lord, may the Springs 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
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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 endeavour 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, 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 hath 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's 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 in 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 Wind Guns and other pneumatical Engines wherein the Air hath 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 hath 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 preceded its spontaneous Dilatation or Expansion
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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 what ever 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 the streams of the Terraqueous Globe, of which our Air in great part consists, should not rise much higher, then the Refractions of the Sun, and other Stars give men ground to think, that the Atmosphere, (even in the judgement of those Recent Astronomers, who seem willing to enlarge its bounds as much as they dare,) doth 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 turne either way with the $\frac{32}{10}$ 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 we are informed, that in the *German* Experiment, commended at the beginning of this Letter, the Ingenious Tryers of it found, That their Glass Vessel, 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 $\frac{2}{10}$ that is, an ounce and very neare a third

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 uneasie 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 *Ricciolo* makes it probable, that the Atmosphere may, at least in divers places, be at least fifty 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 compress'd, 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 it may be well doubted whether the observation, by reason of the great difficulty, hath been exactly made,) there is a manifest disparity beawixt the Air and Water: For I have not found, upon an

Experiment purposely made, (and in an other 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 selfe 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 posselt. 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 Dommé*; whereby it was found, That the *Mercury* 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, hath, 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 *Æquilibrium* 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 yeild 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 readily dilated it selfe downward, from the Receiver into the Pump, when 'tis plain, that

that it is not the incumbent Atmosphere, but onely the subjacent Air in the brass Cylinder that hath been remov'd: If this, I say, be objected, we may reply, That, when a man squeezeth a Fleece of Wool in his hand, he may feel that the Wooll 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, doth constantly endeavour to thrust away all those contiguous Bodies, (whether Aërial or more gross,) that keep it bent, and hinder the expansion of its parts, which will dilate themselves or fly abroad towards that part, (whether upwards or downwards,) where they finde their attempted Dilatation of themselves less resisted by the neighboring 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 themselves about the Terrestrial Globe; whereby it comesto 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 yeilding 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 yeild to other Bodies, or easy 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 hath an opposite determination, as strongly thrust the same Body towards the left; whereby neither of them

In a Discourse touching fluidity and firmness.

proves able to move it out of its place, the pressure on all hands being reduced as it were to an *Equilibrium*: 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 fly 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 selfe 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 causeth the easy 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 divers ingenious persons hath at first sight seem'd very wonderful.

EXPERIMENT II.

THe thing that is wont to be admired, 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 difficult thing to do so, if the Vessel be well exhausted; and even when but a moderate quantity of Air hath been drawn out, he will, when he hath 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 hath been done for merri-ment) onely a Bladder be tyed to it; it is pleasant to see how men
will

will marvel 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 *Phænomenon* seems plainly enough to be onely this, That the Air in the Receiver, being very much dilated it's Spring must be very much weaken'd, & consequently it can but faintly press up the lower end of the stopple, whereas the Spring of the external Air being no way debilitated, he that a little lifts up the stopple must with his hand support a pressure equal to the disproportion betwixt the force of the internal expanded Air, and that of the Atmosphere incumbent upon the upper part of the same key or stopple: And so men being unused to finde any resistance, in lifting things up, from the free Air above them, they are forward to conclude that that which depresseth their hands must needs be some weight, though they know not where plac'd, drawing beneath it.

And, that we have not mis-assign'd the cause of this *Phænomenon*, seems evident enough by this; That as Air is suffered by little and little to get into the Receiver, the weight that a man fancieth his hand supports, is manifestly felt to decrease more and more, the internal Air by this recruit approaching more to an *Æquilibrium* with the external, till at length the Receiver growing again full of Air, the stopple may be lifted up without any difficulty at all.

By several other of the Experiments afforded us by our Engine, the same notion of the great and equal pressure of the free Air upon the Bodies it environs, might be here manifested, but that we think it not so fit to anticipate such Experiments: And therefore shall rather employ a few lines to clear up the difficulty touching this matter, which we have observ'd to have troubled some even of the Philosophical and Mathematical Spectators of our Engine, who have wonder'd that we should talk of the Air exquisitely shut up in our Receiver, as if it were all one with the pressure of the Atmosphere; whereas the thick and close body of the Glass, wholly impervious to the Air, doth manifestly keep the incumbent Pillar of the Atmosphere from pressing in the least upon the Air within the Glass, which it can no where come to touch. To elucidate a

little this matter, let us consider, That if a man should take a fleece of Wooll, and having first by compressing it in his hand reduc'd it into a narrower compais, should nimbly convey and shut it close up into a Box just fit for it, though the force of his hand would then no longer bend those numerous springy Bodyes that compose the Fleece, yet they would continue as strongly bent as before, because, the Box they are inclos'd in, would as much resist their re-expanding of themselves, as did the hand that put them in. For thus we may conceive, that the Air being shut up, when its parts are bent by the whole weight of the incumbent Atmosphere, though that weight can no longer lean upon it, by reason it is kept off by the Glass, yet the Corpuscles of the Air within that Glass continue as forcibly bent, as they were before their inclusion, because the sides of the Glass hinder them from displaying or stretching out themselves. And if it be objected that this is unlikely, because even Glass bubbles, such as are wont to be blown at the flame of a Lamp, exceeding thin, and Hermetically seal'd, will not break; whereas it cannot be imagin'd that so thin a Prison of Glass could resist the Elastical force of all the included Air, if that Air were so compress'd as we suppose: It may be easily reply'd, That the pressure of the inward Air against the Glass, is countervail'd by the equal pressure of the outward against the same Glass. And we see in bubbles, that by reason of this, an exceeding thin film of Water is often able, for a good while, to hinder the eruption of a pretty quantity of Air. And this may be also more conspicuous in those great Spherical bubbles, that boyes sometimes blow with Water, to which Sope hath given a Tenacity. But that, if the pressure of the ambient Air were remov'd, the internal Air may be able to break thicker Glasses, then those lately mention'd, will appear by some of the following Experiments; to which, we shall therefore now hasten, having, I fear, been but too prolix in this Exursion, though we thought it not amiss to annex to our first Experiments some general Considerations touching the Spring of the Air, because (this Doctrine being yet a stranger to the Schools) not onely we finde not the thing

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it self to be much taken notice of; but of those few that have heard of it, the greater part have been forward to reject it, upon a mistaken Perswasion, that those *Phænomena* are the effects of Natures abhorrency of a *Vacuum*, which seem to be more fitly ascribable to the weight and spring of the Air.

EXPERIMENT III.

WE will now proceed to observe, that though, by the help of the handle, the Sucker be easily drawn down to the bottom of the Cylinder; yet, without the help of that Leaver, there would be required to the same effect, a force or weight great enough to surmount the pressure of the whole Atmosphere: since otherwise the air would not be driven out of its place, when none is permitted to succeed into the place deserted by the Sucker. This seems evident, from the known *Torricellian* Experiment, in which, if the inverted Tube of *Mercury* be but 25 Digits high, or somewhat more, the Quick-silver will not fall, but remain suspended in the Tube, because it cannot press the subjacent *Mercury* with so great a force, as doth the incumbent Cylinder of the Air, reaching thence to the top of the Atmosphere: Whereas, if the Cylinder of *Mercury* were three or four digits longer, it would over-power that of the external Air, and run out into the Vessel'd *Mercury*, till the two Cylinders came to an *Æquilibrium*, and no further. Hence we need not wonder, that though the Sucker move easily enough up and down in the Cylinder by the help of the *Manubrium*; yet if the *Manubrium* be taken off, it will require a considerable strength to move it either way. Nor will it seem strange, that if, when the Valve and Stop-cock are well shut, you draw down the Sucker, and then let go the *Manubrium*; the Sucker will, as it were of it self, re-ascend to the top of the Cylinder, since the spring of the external Air findeth nothing to resist it's pressing up the Sucker. And for the same reason, when the Receiver is almost evacuated, though, having drawn down the Sucker,

Sucker,

Sucker, you open the way from the Receiver to the Cylinder, and then intercept that way again by returning the Key; the Sucker will, upon the letting go the *Manubrium*, be forcibly carried up almost to the top of the Cylinder: Because the Air within the Cylinder, being equally dilated and weakned with that of the Glass, is unable to withstand the pressure of the external Air, till it be driven into so little space, that there is an *Equilibrium* betwixt its force and that of the Air without. And congruously hereunto we finde, that in this case, the Sucker is drawn down with little less difficulty, then if the Cylinder, being devoid of Air, the Stop-cock were exactly shut: We might take notice of some other things, that depend upon the Fabrick of our Engine it self; but to shun prolixity, we will, in this place, content our selves to mention one of them, which seems to be of greater moment then the rest, and it is this; that when the Sucker hath been impell'd to the top of the Cylinder, and the Valve is so carefully stopp'd, that there is no Air left in the Cylinder above the Sucker: If then the Sucker be drawn to the lower part of the Cylinder, he that manageth the Pump findeth not any sensibly greater difficulty to depress the Sucker, when it is nearer the bottom of the Cylinder, then when it is much farther off. Which circumstance we therefore think fit to take notice of, because an eminent Modern Naturalist hath taught, that, when the Air is sucked out of a Body, the violence wherewith it is wont to rush into it again, as soon as it is allow'd to re-enter, proceeds mainly from this; That the pressure of the Ambient Air is strengthened upon the accession of the Air suck'd out; which, to make it self room, forceth the neighboring Air to a violent-subingression of its parts: which, if it were true, he that draweth down the Sucker, would finde the resistance of the external Air increas'd as he draweth it lower, more of the displaced Air being thrust into it to compress it. But, by what hath been discours'd upon the first Experiment, it seems more probable, that without any such strengthening of the pressure of the outward Air, the taking quite away or the debilitating of the resistance from within, may suffice to produce the effects under consideration. But this will perhaps be illustrated

strated by some or other of our future Experiments, and therefore shall be no longer insisted on here.

EXPERIMENT IV.

HAVING thus taken notice of some of the constant *Phænomena* of our Engine it self, let us now proceed to the Experiments tryable in it.

We took then a Lambs Bladder large, well dry'd, and very limber, and leaving in it about half as much Air as it could containe, we caus'd the neck of it to be strongly ty'd, so that none of the included Air, though by pressure, could get out. This Bladder being convey'd into the Receiver, and the Cover luted on, the Pump was set on work, and after two or three exsuctions of the ambient Air (whereby the Spring of that which remain'd in the Glass was weaken'd) the Imprison'd Air began to swell in the Bladder, and, as more and more of the Air in the Receiver was, from time to time, drawn out; so did that in the Bladder more and more expand it self, and display the folds of the formerly flaccid Bladder: So that before we had exhausted the Receiver near so much as we could, the Bladder appear'd as full and stretched, as if it had been blown up with a Quill.

And that it may appear that this plumpness of the Bladder proceeded from the surmounting of the debilitated Spring of the ambient Air remaining in the Vessel, by the stronger Spring of the Air remaining in the Bladder; we return'd the Key of the Stopcock, and by degrees allow'd the external Air to return into the Receiver: Whereupon it happen'd, as was expected, that as the Air came in from without, the distended Air in the Bladder, was proportionably compress'd into a narrower room, and the sides of the Bladder grew flaccid, till the Receiver having readmitted its wonted quantity of Air, the Bladder appear'd as full of wrinkles and cavities as before.

This Experiment is much of the same nature with that which was, some years agoe, said to be made by that eminent Geometrician Monsieur *Roberval*, with a Carps Bladder empty'd and convey'd into a Tube, wherein the Experiment *De Vacuo* was afterwards try'd, which ingenious Experiment of his, justly deserveth the thanks of those that have been, or shall be solicitous to discover the nature of the Air.

But to return to our Experiment, we may take notice of this Circumstance in it, That after the Receiver hath been in some measure empty'd, the Bladder doth, at each exsuction, swell much more conspicuously then it did at any of the first Exsuctions; insomuch that towards the end of the pumping, not onely a great fold or cavity in the surface of the Bladder may be made even, by the stretching of the inward self-expanding Air: But we have sometimes seen, upon the turning of the Key to let the ambient Air pass out of the Receiver into the Cylinder, we have seen (I say) the Air in the Bladder suddenly expand it self so much and so briskly, that it manifestly lifted up some light Bodies that lean'd upon it, and seem'd to lift up the Bladder it self.

Now because it hath, by very Learned Men, been doubted, whether the swelling of the Bladder may not have proceeded (not from the Dilatation of the included Air,) but from the Texture of the Fibres, which, being wont to keep the Bladder extended when the Animal (to whom it belong'd) was alive, may be suppos'd in our Experiment to have return'd, like so many Springs to their wonted extent, upon the removal of the Ambient Air that compress'd and bent them: Because this, I say, hath been doubted, we thought fit to make this further tryall.

We let down into the Receiver with the fore-mentioned Bladder two other much smaller, and of the same kinde of Animal; the one of these was not ty'd up at the neck that there might be liberty left to the Air that was not squeez'd out (which might amount to about a fifth part of what the Bladder held before) to pass out into the Receiver: The other had the sides of it stretch'd out and press'd together, almost into the form of a Cup, that they might intercept

tercept the less Air betwixt them, and then was strongly ty'd up at the neck: This done, and the Air being in some measure suck'd out of the Pneumatical Glass (if I may so call it) the Bladder, mention'd at the beginning of our Experiment, appear'd extended every way to its full Dimensions; whereas neither of the two others did remarkably swell, and that, whose neck was not ty'd, seem'd very little, if at all less wrinkl'd then when it was put in.

We made likewise a strong Ligature about the middle of a long Bladder partly empty'd, and upon the drawing the Air out of the Receiver, could observe no such swelling betwixt the Ligature and the Neck of the Bladder, which had been purposely left open, as betwixt the same Ligature and the bottom of the Bladder, whence the included Air could no way get out.

But a farther and sufficient manifestation whence the intumescence of the Bladder proceeds, may be deduc'd from the following Experiment.

EXPERIMENT V.

TO try then at once both what it was that expanded the Bladder, and what a powerful Spring there is ev'n in the Air we are wont to think uncompress'd: we caus'd a Bladder dry, well ty'd and blown moderately full, to be hung in the Receiver by one end of a string, whose other end was fastned to the inside of the Cover: and upon drawing out the ambient Air, that press'd on the Bladder; the internal Air not finding the wonted resistance, first swell'd and distended the Bladder, and then broke it, with so wide and crooked a rent, as if it had been forcibly torn assunder with hands. After which a second Bladder being convey'd in, the Experiment was repeated with like success: And I suppose it will not be imagin'd that in this case the Bladder was broken by its own Fibres, rather then by the Imprison'd Air.

And of this Experiment these two *Phanomena* may be taken

notice of: The one, that the Bladder at its breaking gave a great report, almost like a Cracker: And the other, That the Air contain'd in the Bladder, had the power to break it with the mention'd Impetuosity, long before the ambient Air was all, or near all, drawn out of the Receiver.

But, to verifie what we say in another Discourse, where we show, That even true Experiments may, by reason of the easie mistake of some unheeded Circumstance, be unsuccessfully try'd; we will Advertise, on this occasion, that we did oftentimes in vain try the breaking of Bladders, after the manner above mention'd: Of which the cause appear'd to be this, That the Bladders we could not break, having been brought us ready blown from those that sold them, were grown dry before they came to our hands: whence it came to pass, that, if we afterwards ty'd them very hard, they were apt to fret and so become unserviceable; and if we ty'd them but moderately hard, their stiffness kept them from being clos'd so exactly, but that when the included Air had in the exhausted Receiver distended them as much as easily it could, it would in part get out between the little wrinkles of the Sphincter of the Neck: Whence also it usually happen'd, that, upon the letting in the Air from without, the Bladders appear'd more flaccid and empty then before they were put in; whereas when the Bladders were brought us moist from the Butchers, we could, without injuring them, tye their necks so close, that none of the Air once blown in, could get out of them, but by violently breaking them.

It will not be amiss on this occasion to point at something which may deserve a more deliberate Speculation then we can now afford it; namely that the Elastical Power of the same Quantity of Air may be as well Encreas'd by the Agitation of the Aërial Particles (whether onely moving them more swiftly and scattering them, or also extending or stretching them out, I determine not) within an every way inclosing and yet yeilding Body; as Display'd by the withdrawing of the Air that press'd it without. For we found that a Bladder, but moderately fill'd with Air and strongly ty'd, being a while held near the Fire, not onely grew exceeding
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turgid and hard, but afterwards, being brought nearer to the Fire, suddenly broke with so loud and vehement a noise, as stony'd those that were by, and made us, for a while after, almost deaf.

EXPERIMENT VI.

HAVING thus seen that the Air hath an Elastical Power, we were next desirous to know in some measure how far a parcel of Air might by this its own Spring be dilated. And though we were not provided of Instruments fit to measure the dilatation of the Air any thing accurately, yet because an imperfect measure of it was more desirable than none at all, we devis'd the following Method as very easily practicable.

We took a limber Lambs Bladder which was thorowly wetted in fair Water, that the sides of it being squeez'd together, there might be no Air left in its folds: (as indeed we could not afterwards upon tryal discern any) The neck of this Bladder was strongly tyed about that of a small Glass, (capable of holding five full drachmes of Water) the Bladder being first so compress'd, that all the included Air was onely in the Glass, without being press'd there; then the Pump being set on work after a few exsuctions, the Air in the little Viol began to dilate it self and produce a small Tumor in the Neck of the Bladder; and as the ambient Air was more and more drawn away, so the included Air penetrated farther and farther into the Bladder, and by degrees lifted up the sides and display'd its folds, till at length it seem'd to have blown it up to its full extent: Whereupon the external Air, being permitted to flow back into the Receiver, repuls'd the Air that had fill'd the Bladder into its former narrow receptacle, and brought the Bladder to be again flaccid and wrinkled as before: Then taking out the Bladder, but without severing it from the Glass, we did, by a hole made at the top of the Bladder, fill the Vessel, they both made up, with

Water, whose weight was five Ounces five Drachmes and a half: Five Drachmes whereof were above-mention'd to be the contents of the Bottle. So that in this Experiment, when the Air had most extended the Bladder, it possess'd in all above nine times as much room as it did when it was put into the Receiver. And it would probably have much enlarg'd its bounds, but that the Bladder by its weight and the sticking together of its sides did somewhat resist its expansion: And which was more considerable, the Bladder appear'd tumid enough, whilst yet a pretty deal of Air was left in the Receiver, whose exsuction would, according to our former Observation, probably have given way to a farther expansion of the Air, especially supposing the dilatation not to be restrain'd by the Bladder.

Since we wrote the other day the former Experiment, we have met with some Glasses not very unfit for our purpose; by means of which we are now able, with a little more trouble, to measure the expansion of the Air a great deal more accurately than we could by the help of the above-mention'd Bladder, which was much too narrow to allow the Air its utmost distention.

We took then first a Cylindrical Pipe of Glass, whose bore was about a quarter of an Inch in Diameter: This Pipe was so bent and doubled, that, notwithstanding its being about two foot in length, it might have been shut up into a small Receiver, not a Foot high: But by misfortune it crack'd in the cooling, whereby we were reduced to make use of one part which was straight and intire, but exceeded not six or seven Inches. This little Tube was open at one end; and at the other, where it was Hermetically seal'd, had a small Glass bubble to receive the Air whose dilatation was to be measur'd.

Along the side of this Tube was pasted a straight narrow piece of Parchment, divided into twenty six equal parts, marked with black Lines and Figures, that by them might be measur'd both the included Air and its dilatation. Afterwards we fill'd the Tube with Water, almost to the top; and stopping the open end with the

the Finger, and inverting the Tube, the Air was permitted to ascend to the above-mention'd Glass bubble. And by reason this ascent was very slow, it gave us the opportunity to mark how much more or less then one of the twenty six divisions this Air took up. By this means, after a tryal or two, we were inabled to convey to the top of the Glass a bubble of Air equal enough, as to sight, to one of those Divisions: Then the open end of the Tube being put into a small Viol, whose bottom was cover'd with Water, about half an Inch high; we included both Glasses into a small and slender Receiver, and caused the Pump to be set on work. The event was, That at the first exsuction of the Air there appear'd not any expansion of the bubble, comparable to what appear'd at the second, and that upon a very few exsuctions the bubble reaching as low as the surface of the subjacent Water, gave us cause to think, That if our Pipe had not been broken, it would have expanded it self much farther: Wherefore we took out the little Tube, and found that besides the twenty six divisions formerly mention'd, the Glass bubble and some part of the Pipe to which the divided Parchment did not reach, amounted to six divisions more. Whereby it appears that the Air hath taken up one and thirty times as much room as before, and yet seem'd capable of a much greater expansion, if the Glass would have permitted it. Wherefore after the former manner, we let in another bubble, that by our guess was but half as big as the former, and found, that upon the exsuction of the Air from the Receiver, this little bubble did not onely fill up the whole Tube, but (in part) break through the subjacent Water in the Viol, and thereby manifest it self to have possessed sixty and odde times its former room.

These two experiments are mention'd to make way for the more easy belief of that which is now to follow. Finding then that our Tube was too short to serve our turn, we took a slender Quill of Glass which happen'd to be at hand, though it were not so fit for our purpose as we could have wished, in regard it was three or four times as big at one end as the other. This Pipe which was
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thirty Inches long, being Hermetically seal'd at the slender end, was almost filled with water; and after the above-related manner a bubble was convey'd to the top of it, and the open extreame was put into a Viol that had a little fair Water at the bottom: Then the Cover, by means of a small hole purposely made in it for the Glass Pipe to stand out at, was cemented on to the Receiver, and the Pump being set on work, after some exsuctions, not onely the Air manifestly appear'd extended below the surface of the subjacent Water; but one of the By-standers affirms, that he saw some bubbles come out at the bottom of the Pipe and break through the Water. This done, we left off Pumping, and observ'd how at the unperceiv'd leaks of the Receiver the Air got in so fast, that it very quickly impell'd up the Water to the top of the Tube, excepting a little space whereinto that bubble was repuls'd, which had so lately possess'd the whole Tube; this Air at the slender end appear'd to be a Cylinder of $\frac{3}{4}$ parts of an Inch in length, but when the Pipe was taken out and turn'd upside down, it appear'd at the other end inferior in bulk to a Pea.

These things being thus done we took (to make the Experiment the more exactly) a small pair of Scales, such as Gold Smiths use to weigh Cold Coyn in; and weighing the Tube and Water in it, we found them to amount to one Ounce thirty Grains and an half: Then we pour'd in as much Water as serv'd to fill up the Tube, wherein before we had left as much space unfill'd up as was possess'd by the bubble, and weighing again the Pipe and Water, we found the weight increas'd onely by one Grain. Lastly, pouring out the Water, and carefully freeing the Pipe from it (which yet we could not perfectly doe) we weighed the Glass alone, and found it to want two Drachmes and thirty two Grains of its former weight: So that the bubble of Air taking up the room but of one Grain in weight of Water, it appear'd that the Air by its own *ελατιε* was so rarified, as to take up one hundred fifty two times as much room as it did before: Though it where then compress'd by nothing but the ordinary pressure of the contiguous Air. I know not whether it be requisite to take notice, that this Experiment

was

was made indeed in a moist Night, but in a Room, in whose Chimney there was burning a good Fire, which did perhaps somewhat rarefie the Air of which the bubble consisted.

It hath seem'd almost incredible which is related by the Industrious *Mersennus*, That the Air by the violence of heat, though as great as our Vessels can support without fusion, can be so dilated as to take up seventy times as much room as before: Wherefore because we were willing to have a confirmation of so strange a *Phænomenon*; we once more convey'd into the Tube a bubble of the bigness of the former, and prosecuting the Experiment as before with the same Water, we observed that the Air did manifestly stretch it self so far, as to appear several times a good way below the surface of the Water in the Viol, and that too with a surface very convex toward the bottom of the Pipe. Nay, the Pump being ply'd a little longer, the Air did manifestly reach to that place where the bottom of the Tube leaned upon the bottom of the Viol, and seemed to knock upon it and rebound from it: Which Circumstances we adde, partly that the *Phænomenon* we have been relating may not be imputed to the bare subsiding of the Water that filled the Tube, upon the taking off the pressure of the ambient Air. And partly also that it may appear that if our Experiments have not been so accurately made as with fitter Instruments might perhaps be possible; yet the expansion of the Air is likely to be rather greater then lesser then we have made it: Since the Air was able to press away the Water at the bottom of the Pipe, though that were about two Inches below the surface of the Water that was then in the Viol, and would have been at least as high in the Pipe, if the Water had onely subsided and not been depressed: So that it seems not unlikely that if the Experiment could be so made, as that the expansion of the Air might not be resisted by the Neighboring Bodies, it would yet enlarge its bounds, and perhaps stretch it self to two hundred times its former bulk, if not more. However, what we have now tryed will, I hope, suffice to hinder divers of the *Phænomena* of our Engine from being distrusted: Since in that part of the Atmosphere we live in, that which we call

the free Air (and presume to be so uncompress'd) is crouded into so very small a part of that space, which if it were not hindred it would possess. We would gladly have tryed also whether the Air at its greatest expansion could be farther rarified by heat; but do what we could, our Receiver leaked too fast to let us give ourselves any satisfaction in that particular.

EXPERIMENT VII.

TO discover likewise by the means of that pressure of the Air, both the strength of Glass, and how much interest the Figure of a Body may have in its greater or lesser Resistance to the pressure of other Bodies, we made these farther tryals.

We caus'd to be blown with a Lamp, a round Glass bubble, capable of containing, by guess, about five Ounces of Water, with a slender neck about the bigness of a Swans Quill, and it was purposely blown very thin, as Viols made with Lamps are wont to be, that the thinness of the matter might keep the roundness of the Figure from making the Vessel too strong. Then having moderately emptyed the Receiver, and taken it out of the Pump, we speedily applyed to the Orifice of the bottom of it, the Neck of the newly mention'd Glass, carefully stopping the Crannies with melted Plaister, that no Air might get in at them: And after turning the Key of the Stop-cock, we made a free passage for the Air to pass out of the bubble into the Receiver: Which it did with great celerity, leaving the bubble as empty as the Receiver it self; as appear'd to us by some Circumstances not now to be insisted on. Notwithstanding all which, the Vessel, continuing as intire as before, gave us cause to wonder that the bare Roundness of the Figure should enable a Glass, almost as thin as Paper, to resist so great a pressure as that of the whole incumbent Atmosphere. And having reiterated the Experiment, we found again that the pressure of the ambient Body, thrusting all the parts inwards, made them,

by

by reason of their arched Figure, so support one another; that the
Glas remain'd as whole as at first.

Now that the Figure of the Glas is of great moment in this
matter, may be evinced by this other experiment.

EXPERIMENT VIII.

WE took a Glas Helmet or Alembick (delineated by the
seventh Figure) such as Chymists use in Distillations, and
containing by conjecture between two and three Pints: The *Ro-*
strum or Nose of it mark'd with (c) was Hermetically closed; and at
the top of it was a hole, into which was fitted and cemented one of
the Shanks of a middle-siz'd Stop-cock; so that the Glas being
turn'd upside-down, the wide Orifice (which in common Glas-
Helmets is the onely one) was upwards; and to that wide Orifice
was fitted a cast Cover of Lead, which was carefully cemented on
to the Glas: Then the other Shank of the Stop-cock being with
cement likewise fasten'd into the upper part of the Pump, the ex-
suction of the Air was endeavour'd. But it was not long before,
the remaining Air being made much too weak to ballance the pres-
sure of the ambient Air, the Glas was (not without a great noise)
crack'd almost half round, along that part of it where it began to
bend inwards: As if in the Figure, the crack had been made accor-
ding to the Line (a b), and upon an endeavour to pump out more
of the Air, the crack once begun, appear'd to run on farther;
though the Glas where it was broken seem'd to be (by conjecture)
above ten, some thought above twenty times, as thick as the bubble
mentioned in the foregoing Experiment.

This will perhaps make it seem strange, that having taken ano-
ther Glas bubble blown at the same time, and like for ought we
discerned for size, thickness and Figure, to that thin one formerly
mentioned; and having sealed it up Hermetically, and suspended it
in the Receiver, the exsuction of the ambient Air did not enable

the imprisoned Air to break, or in the least to crack the bubble; though the Experiment were laboriously tryed, and that several times with bubbles of other sizes: But that perhaps the heat of the Candle or Lamp wherewith such Glasses are Hermetically sealed, (not to mention the warmth of his hands that seal'd it) might so rarefy the contained Air, as much to weaken its Spring, may seem probable by the following Experiments.

EXPERIMENT IX.

WE took a Glass Viol able to hold three or four Ounces of Water, and of the thickness usual in Glasses of that size; into the Neck of this was put a moderately slender Pipe of Glass, which was carefully fastened with a mixture of equal parts of Pitch and Rosin to the Neck of the Viol, and which reached almost to the bottom of it, as the sixth Figure declareth.

This Viol being, upon a particular design, filled with Water, till that came up in it, a pretty deal higher then the lower end of the Pipe, was put into one of our small Receivers, (containing between a Pint and a Quart) in such manner as that the Glass Pipe, passing through a hole made purposely for it in the Leaden-Cover of the Receiver, was for the most part of it without the Vessel, which being exactly closed, the Pump was set on work: But at the very first exsuction, and before the Sucker was drawn to the bottom of the Cylinder, there flew out of the Viol a piece of Glass half as broad as the Palm of a Mans Hand, and it was thrown out with such violence, that hitting against the Neighboring side of the Receiver, it not onely dashed it self to pieces, but cracked the very Receiver in many places, with a great noise that much surpris'd all that were in the Room. But it seemed, that in so little a Receiver, the Air about the Viol being suddenly drawn out, the Air Imprisoned in the Vessel, having on it the whole pressure of the Atmosphere (to which by the Pipe open at both ends, It and the Water were

were exposed) and not having on the other side the wonted pressure of the Ambient Air to ballance that other pressure, the resistance of Glass was finally surmounted, and the Viol once beginning to break where it was weakest, the external Air might rush in with violence enough to throw the cracked parcel so forcibly against the Neighboring side of the Receiver, as to break that too.

And this may be presumed sufficient to verify what we delivered in that part of our Appendix to the first Experiment where we mentioned the almost equal pressure of the Air on either side of a thin Glass Vessel, as the cause of its not being broken by the forcible spring of the contained Air. But yet that it be not suspected that chance had an interest in so odde an Experiment as we have been Relating, we will adde that for farther satisfaction we reiterated it in a round Glass containing by guesse about six ounces of Water: This Viol we put into such a small Receiver as was lately mentioned, in such manner as that the bottome of it rested upon the lower part of the Pneumatical Glass, and the Neck came out through the Leaden-Cover of the same at a hole made purposely for it. But being made circumspect by the foregoing mischance, we had put the Viol into a Bladder, before we put it into the Receiver to hinder this last named Glass from being endangered by the breaking of the other. Then the Pneumatical Vessel being closed so that no way was left for the outward Air to get into it, but by breaking through the Viol, into whose cavity it had free access by the mouth of it, (which was purposely left open,) the Sucker being nimbly drawn down, the external Air immediately pressed forcibly as well upon the Leaden-Cover as the Viol; and the Cover happening to be in one place a little narrower then the edge of the Pneumatical Glass, was depressed, and thrust into it so violently by the incumbent Air, that getting a little within the tapering Lip of the Glass, it did (like a kinde of Wedge,) thrust out that side where it was depressed, so as, (though the Receiver was new,) to split it. This accident being thus mentioned upon

the by to confirm what we formerly said touching the fitness or unfitness of Glasses of some Figures to resist the pressure of the Atmosphere; We will proceed to relate the remaining part of the Experiment, namely, That having fitted on a wider Cover to the same Receiver, and closed both that and the crack with Cement, we prosecuted the Experiment in the manner above related, with this success: That upon the quick depressing of the Sucker, the external Air burst the Body of the Viol into above an hundred pieces, many of them exceeding small, and that with such violence, that we found a wide rent, besides many holes, made in the Bladder it self.

And to evince that these *Phænomena* were the effects of a limited and even moderate force, and not of such an abhorrency of a *Vacuum* as that to avoid it, many have been pleased to think that Nature must, upon occasion, exercise an almost boundless power; we afterwards purposely try'd this Experiment with several Glasses somewhat thicker then those Viols, and found the event to verify our conjecture, that it would not succeed: For the Glasses were taken out as intire as they were put in.

And here, My Lord, I hold it not unfit, upon occasion of the mention that hath been made of our having employ'd small Receivers, and one of them, notwithstanding its being crack'd, to annex these two Advertisements.

First then, besides the great Pneumatical Glass so often mentioned, and the proportionate Stop-cock, we thought fit to provide our selves with some small Receivers blown of Crystalline Glass, of several Shapes, and furnished with smaller Stop-cocks purposely made; and this we did upon hopes, that when we had surmounted the difficulties to be met with in Cementing the Glasses to the Stop-cocks, and the Pneumatical Vessels to the Pump, so exquisitely as is requisite for our purpose, we should from the smalness of our Receivers receive a four-fold Advantage. The first, that by reason of the slenderness of the Vessels, and their being made of much purer and clearer metall, (as the Glass-men speak,) then the great Receiver, we might have a more perfect view of every

every thing happening within them. The next, that such small Vessels might be emptied with less labour and in much lesse time. The third, that this nimble exsuction of the ambient Air would make many changes in the Bodies shut up in these glasses more sudden and conspicuous then otherwise they would prove. And the last, that we should be able to draw and keep out the Air much more perfectly from such small Vessels then from our large Receiver. But though we were not much dis-appointed in the expectation of the three first advantages, yet we were in our hopes of the fourth. For besides the great difficulty we found in fitting together the Glasses, the Stop-cocks, and the Covers; besides this I say, we found our selves seldom able to draw, and keep out the Air so far as to make the remaining Air in these Receivers weaker then the remaining Air in our great Receiver. For though sometimes the Leaks of some of these little Receivers may be much (either fewer or smaller) then those of the larger Vessel; yet a little Air getting into one of these, wherein it had but little room to expand and display it self, might press as much upon all parts of the internal surface of the Vessel, and upon the included Bodies, as a greater quantity of the Air in a Vessel in whose capacity it might finde more room to expand it self.

The other thing that we were to advertise, is, That 'tis not every small crack that can make such a Receiver as is of a roundish Figure altogether useles to our Experiment, in regard that upon the exsuction of the internal Air, the ambient Air on all sides pressing the Glas inwards or towards the middle, doth consequently thrust the Lips of the crack closer, and so rather close then increase it.

This I mention partly because Receivers fit for our turn are more easily cracked then procured, and therefore ought not to be unnecessarily thrown away as unserviceable: And partly because I think it becomes one that professeth himself a faithfull Relator of Experiments, not to conceal from Your Lordship, that after a few of the foregoing Experiments were made, there happened in the great Receiver a crack of about a Span long, beginning at
the

the upper Orifice, and occasion'd, as it seem'd, by the excessive heat of too large an Iron that was employ'd to melt the Cement about that Orifice. But having laid upon this crack a broad Plaister, (which in one of our Essays written some years since to your ingenious and hopeful Cousin *Jones*, we extoll for the mending of crack'd Receivers, and other Chymical Glasses;) and having afterwards thickly over-laid this Plaister with Diachylon, we neither could then, nor can yet perceive that the Vessel leaks sensibly at that crack.

The Plaister was made of good quick Lime finely poudred, and nimbly ground with a Pestle in a Mortar, with a quantity (I know not how much precisely, not having those Essays in this place) of scrapings of Cheese and a little fair Water, no more then is just necessary to bring the mixture to a somewhat soft Paste, which when the Ingredients are exquisitely incorporated, will have a strong and stincking smell: Then it must be immediately spread upon a Linnen Cloath of three or four fingers breadth, and presently apply'd, lest it begin to harden. But if your Lordship had seen how we mended with it Receivers even for the most subtle Chymicall Spirits, You would scarce wonder at the service it hath done in our Pneumaticall Glass.

EXPERIMENT X.

WE took a Tallow-Candle of such a size that eight of them make about a pound, and having in a very commodious Candlestick let it down into the Receiver, and so suspended it, that the Flame burnt almost in the middle of the Vessel, we did in some two minutes exactly close it up: and upon Pumping very nimbly, we found, that, within little more then half a minute after, the Flame went out, though the Snuff had been purposely left of that length we judged the most convenient for the lasting of the Flame.

But

But the second time having put in the same Candle into the Receiver, (after it had by the blasts of a pair of Bellows been freed from Fumes) the Flame lasted about two minutes from the time the Pumper began to draw out the Air; upon the first exsuction whereof, the Flame seem'd to contract it self in all its dimensions. And these things were farther observable, that after the two or three first exsuctions of the Air, the Flame (except at the very top) appear'd exceeding blew, and that the Flame still receded more and more from the Tallow, till at length it appear'd to possess onely the very top of the Week, and there it went out.

The same Candle being lighted again was shut into the Receiver, to try how it would last there without drawing forth the Air, and we found that it lasted much longer than formerly; and before it went out, receded from the Tallow towards the top of the Week, but not neer so much as in the former Experiment.

And having an intention to observe particularly what the motion of the smoke would be in these Experiments: We took notice that when the Air was not drawn out, there did, upon the extinction of the Flame, a considerable part of the Week remain kindled, which (probably by reason of the Circulation of the Air) in the Vessell, occasion'd by the heat) emitted a Steam, which ascended swiftly and directly upwards in a slender and uninterrupted Cylinder of smoke, till it came to the top, whence it manifestly recoyl'd by the sides to the lower part of the Vessel. Whereas when the Flame went out upon the exsuction of the Air one time (when the Flame retir'd very leasurely to the top) we perceived it not to be follow'd by any smoke at all. And at an other time the upper part of the Week remaining kindled after the extinction of the Flame, the slender steam of Fumes that did arise ascended but a very little way, and then after some uncertain motions this and that way, did, for the most part, soon fall downwards.

Being desirous also to try whether there would be any difference as well in our Receiver, as there is wont to be elsewhere betwixt Candles made of Wax and those made of Tallow, as to their duration,

ration; we took slender Tapers of white Wax, commonly called Virgins Wax) that being found to burn with much lesse smoke then common yellow Wax. Six of these of like bigness, and each of them of about the thickness of a Swans Quill, we press'd together into one Candle: And having lighted all the Weeks, we let in the above-mention'd Wax-Candle into the Receiver, and made what hast we could to close it up with Cement. But, though in the mean while we left open the Valve of the Cylinder, the hole of the Stop-cock, and that in the Cover of the Receiver, that some Air might get in to cherish the Flame, and the smoke might have a vent; Yet for so great a Flame the Air sufficed not so much as till the Cover could be perfectly luted on: So that before we were quite ready to imploy the Pump, the Candle was extinguished. Wherefore we took but one of the above mention'd Tapers, and having lighted it, clos'd it up in the Receiver, to try how long a small Flame with a proportionable smoke would continue in such a quantity of Air: but we found upon two severall tryals, that from the beginning of pumping, the Flame went out in about a minute of an hour. It appear'd indeed to us that the swinging of the Wier to and fro (in the Engine shaken by pumping) hasten'd the vanishing of the Flame, which seem'd by that motion to be cast sometimes on one side of the Week and sometimes on the other. But though once we purposely refrain'd pumping after a very few exsuctions of the Air, that the Flame might not be agitated, yet it lasted not much longer then the newly mention'd time. And lastly, closing up the same Taper, lighted again, to discover how long it would last without drawing out of the Air, we found that it burn'd for a while vividly enough, but afterwards began to be lessen'd more and more in all its dimensions. And we observ'd that the Flame did not, as before, retire it self by little and little towards the top, but towards the bottom of the Week (from which yet it did a little withdraw upwards just before it went out) so that the upper part of the Week appear'd for a pretty while manifestly above the top of the Flame, which having lasted about five minutes, was succeeded by a directly ascending stream of Smoak.

EXPERIMENT XI.

There was taken a Wier, which being bent almost in the form of a Screw, constituted such an Instrument, to contain Coals and leave them every way accessible to the Air, as the tenth Figure declareth; the breadth of this Vessel was no less than that it might with ease be convey'd into the Receiver: And having fill'd it to the height of about five Inches with thoroughly kindled Wood-coals, we let it down into the Glass; and speedily closing it, we caus'd the Pumper to ply his work, and observ'd that upon the very first exsuction of the Air (though perhaps not because of that onely) the Fire in the Coals began to grow very dim, and though the agitation of the Vessel did make them swing up and down (which in the free Air would have retarded the extinction of the Fire) yet when we could no longer discern any rednesse at all in any of them, casting our eyes upon a minute Watch we kept by us on this occasion, we found that from the beginning of the Pumping (which might be about two minuts after the Coals had been put in glowing) to the totall dis-appearing of the Fire, there had passed but three minutes.

Whereupon, to try the Experiment a little farther, we presently took out the Coals, in which it seems there had remained some little parcels of Fire, rather cover'd then totally quench'd: For in the open Air the Coals began to be re-kindled in several places, wherefore having by swinging them about in the Wier, thoroughly lighted them the second time, we let them down again into the Receiver, and clos'd it speedily as before; and then waiting till the Fire seem'd totally extinct without meddling with the Pump, we found that from the time the Vessel was clos'd, till that no Fire at all could be perceiv'd, there had passed about four minutes: Whereby it seem'd to appear that the drawing away of the Ambient Air made the Fire to go out sooner then otherwise it would

would have done; though that part of the Air that we drew out left the more room for the stifling steams of the Coals to be received into.

Lastly, Having taken out the Wier and put other Coals into it, we did, in the same Room where the Engine stood, let it hang quietly by a string in the open Air, to try how long the Fire would last without agitation, when no Air was kept from it. And we found that the Fire began to go out first at the top and outsides of the Coals; but inwards and near the bottom the Fire continu'd visible for above half an hour, a great part of the Coals, especially those next the bottom, being burnt to ashes before the Fire went out.

We caus'd likewise a piece of Iron to be forg'd, of the bignesse of a middle siz'd Char-coal, and having made it red hot throughout, we caus'd it in the lately mention'd Wier, to be speedily convey'd and shut up into the Receiver, being desirous to try what would become of a glowing Body, by reason of its texture more vehemently hot then a burning Coal of the same bigness, and yet unlike to send forth such copious and stifling Fumes: But we could not observe any manifest change upon the exsuction of the Air. The Iron began indeed to lose its Fiery redness at the top, but that seem'd to be because it was at the upper end somewhat more slender then at the lower: The redness, though it were in the day time, continued visible about four minutes; and then, before it did quite dis-appear, we turn'd the Key of the Stop-cock, but could not discern any change of the Iron upon the rushing in of the Air. Yet some little remainders of Wax that stuck to the Wier, and were turn'd into Fumes by the heat of the neighbouring Iron, seem'd to afford a more plentiful, or at least a much more expanded smoke when the Air was suck'd out, then afterwards; though allowance was made for the decreasing heat of the Iron. And lastly, notwithstanding a considerable exsuction of the Ambient Air, though not by far so great a one as might have been made by the Engine; and notwithstanding the inconsiderable dissipation of the parts of the Iron; the surrounding

sides

sides of the Receiver were sensibly, and almost offensively heated by it; inasmuch that a pretty while after the Iron was taken out, the sides of the Glasse manifestly retain'd a warmth: which would not be unfit to be consider'd by a Person at more leasure then I am now.

EXPERIMENT XII.

BEING willing to try after this something that would not cherish much Fire at once, and would keep Fire much longer then a Coal: We took a piece of Match, such as Souldiers use, of the thickness of a Mans little Finger, or somewhat thicker; and this being well lighted at one end, was by a string suspended with that end downwards in the cavity of the Receiver which was immediatly clos'd: And yet by that time it could well be so, the copious Fumes of the Match had neer fill'd & darken'd the Receiver. Wherefore, lest the Vessel should be endanger'd, the Pump was nimbly ply'd, & a great deal of Air and Smoke mixt together was drawn out, whereby the Receiver growing more clear, we could discern the Fire in the Match to burn more and more languidly: and notwithstanding that by the diligence us'd in Pumping, it seem'd to have room enough allow'd it to throw out Fumes; yet, after no long time, it ceas'd from being discernable either by its Light or its Smoke. And, though by that we were invited to suppose it quite extinguished, yet we continu'd pumping a while, in prosecution of another Experiment we were trying at the same time: And this we did the more willingly, because of a suspicion, the Experiment about the Coals, might easily suggest, and which the event declar'd not to have been altogether groundlesse. For upon the admission of the external Air, the Fire, that seem'd to have gone out a pretty while before, did presently revive; and being as it were refresh'd by the new Air, and blown by the Wind made by that Air in rushing in, it began again to shine and

dissipate the neighboring Fuel into Smoke as formerly.

EXPERIMENT XIII.

A While after we let down into the Receiver together with a lighted piece of Match, a great Bladder well tyed at the Neck, but very lank, as not containing actually much (if any thing) above a Pint of Air, but being capable of containing ten or twelve times as much.

Our scope in this Experiment was partly to try whether or no the smoke of the Match, replenishing the Receiver, would be able to hinder the Dilatation of the inward Air, upon the exsuction of the Ambient: And partly to discover whether the extinction of the Fire in the Match did proceed from want of Air, or barely from the pressure of its own Fumes, which for want of room to expand themselves in, might be suppos'd to Recoyl upon the Fire, and so to stifle it.

The event of our tryall was, That at the beginning of our Pumping the Match appear'd well lighted, though it had almost fill'd the Receiver with its plentiful Fumes: But by degrees it burnt more and more dimly, notwithstanding, that by the nimble drawing out the Air and Smoke, the Vessel were made lesse opacous, and lesse full of compressing matter; as appear'd by this, That the longer we pump'd, the lesse Air and Smoke came out of the Cylinder at the opening of the Valve, and consequently the lesse came into it before; yet the Fire in the Match went but slowly out. And when afterwards, to satisfie our selves of its expiration, we had darken'd the Room, and in vain endeavoured to discover any spark of Fire, (as we could not for some time before by the help of Candles discern the least rising of Smoke,) we yet continued pumping six or seven times; and after all that, letting in the Air, the seemingly dead Fire quickly revived, and manifested its recovery by Light and store of Smoke, with the latter of which
it

it quickly began to replenish the Receiver. Then we fell to pumping afresh, and continued that labour so long, till the re-kindled Match went out again: and thinking it then fit not to cease from Pumping so soon as before, we found that in lesse then half a quarter of an hour the Fire was got out for good and all, and past the possibility of being recover'd by the re-admitted Air.

Some Circumstances, besides those already mention'd, occur'd in the making of the Experiment, of which these are the principal.

First, when the Receiver was full of Smoke, if the Cylinder were emptied, immediately upon the turning of the Stop-cock, the Receiver would appear manifestly darken'd to his eye that look'd upon the light through it: and this darkness was much less when the Receiver was much less fill'd with Fumes: It was also instantaneous, and seem'd to proceed from a sudden change of place and situation in the exhalations, upon the vent suddenly afforded them and the Air they were mixt with, out of the Receiver into the Cylinder.

The next thing we observ'd was, a kinde of *Halo* that appear'd a good while about the Fire, and seem'd to be produced by the surrounding Exhalations.

And lastly, it is remarkable, That even when the Fumes seemed most to replenish the Receiver, they did not sensibly hinder the Air included in the Bladder from dilating it self after the same manner (for ought we could discern) as it would have otherwise done: So that before the Fire or the Match was quite extinct, the Bladder appear'd swell'd at least to six or seven times its former capacity.

Since the writing of these last Lines, we took a small Receiver, capable of containing (by guess) about a pound and a half of Water; and in the midst of it we suspended a lighted Match, but though within one minute of an hour (or thereabouts) from the putting in of the Match, we had cemented on the Cover, yet we could not make such haste, but that before we began to pump, the Smoke had so fill'd that small Receiver, as for ought we discern'd, to
choke

choke the Fire. And having again and again reiterated the Experiment, it seem'd still as at first, that we could not close up the Vessell, and Pump out all the Fumes time enough to rescue the Fire from Extinction, whereupon we made use of this Expedient: As soon as we had pump'd once or twice, we suddenly turn'd the Key, and thereby gave access to the excluded Air, which rushing violently in, as if it had been forced thorow a pair of Bellows, did both drive away the ashes, fill the Glass with fresh Air, and by blowing the almost extinguish'd Fire, re-kindl'd it, as appear'd by the Matches beginning again to smoke, which before it had ceas'd to do; we having by this means obtain'd a lighted Match in the Receiver, without being reduc'd to spend time to close it up, commanded the Air to be immediatly pump'd out, and found that upon the exsuction of it, the Match quickly left smoking, as it seem'd, by reason of the absence of the Air; and yet if some urgent occasions had not hinder'd us, we would for greater security have try'd, whether or no the Match rekindled as formerly, would smoke much longer, in case of no exsuction of the ambient Air.

EXPERIMENT XIV.

TO try diverse things at once, and particularly whether Fire, though we found it would not long last, might not yet be produced in our evacuated Receiver: We took a Pistol of about a Foot in length, and having firmly tyed it to a stick almost as long as the Cavity of the Receiver, we very carefully prim'd it with well dry'd Gunpowder, and then cocking it, we ty'd to the Trigger, one end of a string, whose other end was fasten'd to the Key formerly mention'd to belong to the Cover of our Receiver. This done, we convey'd the Pistol, together with the annexed Staff, into the Vessell which being clos'd up, and empty'd after the usual manner, we began to turn the Key in the Cover; and thereby shortning the string that reach'd from it to the Pistol, we pull'd
aside

aside the Tricker, and observ'd, that according to our expectation the force of the Spring of the Lock was not sensibly abated by the absence of the Air: (from whose *impetus* yet some Modern Naturalists would derive the cause of the motion of Restitution in solid Bodies) For the Cock falling with its wonted violence upon the Steel, struck out of it as many, and as conspicuous parts of Fire, as, for ought we could perceive, it would have done in the open Air. Repeating this experiment diverse times, we also observed whether or no there would appear any considerable diversity in the Motion of the shining Sparks in a place where the remaining Air was so much rarefied, but could not perceive but that they moved some of them upwards, as well as some of them downwards, and some of them side-waies, as they are wont to do, when upon such collisions they fly out in the open Air.

• We likewise caus'd a piece of Steel to be made of the form and bignesse of the Flint, in whose place we put it, and then the Pistol being cock'd and conveyed into the Receiver, the Tricker was pull'd after the Air was drawn out: And though the place were purposely somewhat darken'd, yet there appear'd not upon the striking of the two Steels against each other the least spark of Fire: Nor did we expect any (having before in vain attempted to strike Fire this way in the open Air) though we thought fit to make the Experiment, to undeceive those who fancy in rarefied Air, I know not what strange disposition, to take Fire upon a much flighter occasion than this Experiment afforded. We have indeed found, that by the dextrous Collision of two harden'd pieces of Steel, store of sparks may be struck out: But that was done with such vehement percussion of the edges of the two Steels, as could not well be compass'd in our Receiver.

But the chief thing we design'd to do with our Pistol, was, To observe whether Gun-powder would take Fire in our empty'd and closely stop'd Glass? Whether the expansion of the Flame would be considerably varied by the absence of so much of the ambient Air as was drawn out of the Receiver? & whether the Flame would diffuse it selfe upward, as it is wont, notwithstanding

its not having about it the usual proportion of Air to force it up. And though most of our Attempts to fire the Gun-powder in the Pan of the Pistol succeeded not, because we were fain to let it hang almost perpendicular in the Receiver, whereby the Powder was shaken down before the sparks could reach it: yet once the Experiment succeeded, and the kindled Powder seem'd to make a more expanded Flame then it would have done in the open Air, but mounted upwards according to its wont, whether by reason of that little portion of Air, which in spite of our pumping remained in the Receiver, or for any other cause, we have not now the leasure to consider. But we must not forget, that upon the extinction of the Flame the Receiver appear'd darken'd with smoke, which seem'd to move freely up & down, and upon the letting in the Air at the Stop-cock began to circulate much faster then before. We would have made more observations concerning this Flame, but that of two or three attempts we afterwards made to repeat the kindling of the Powder, not any one succeeded; & we have not the leasure to dwell long upon one kinde of Tryals.

EXPERIMENT XV.

TO these Experiments concerning Fire we added another, which though it succeeded not, may perhaps without impertinency be recorded: partly, because that (as we have in another Treatise amply declar'd) it is usefull to recite what Experiments miscarry as well as succeed. And partly also, because it is very possible that what we endeavoured in vain, may be performed by Your Lordship, or some other *Virtuoso* that shal have stancher Vessels then we had, & more Sunny dayes then the present Winter allows us.

We convey'd then into one of our small Receivers a piece of matter combustible, dry & black (experience declaring things of that colour to be most easily kindled) & carefully closing the Vessel, we brought

brought it to a Window at which the Sun, not very far from the Meridian, shone in very freely: then drawing out the Air with speed we united the Sun-beams with a burning Glass upon the combustible matter, which began immediatly to send forth a Smoke that quickly darkned the Receiver, but notwithstanding all our care & diligence the external Air got in so fast, that after diverse tryals we were fain to leave off the Experiment in that Glass, and induc'd to make tryal of it in our great Receiver.

Having then after some difficulty lodg'd the combustible matter in the cavity of this Vessel in such manner, as that it was almost contiguous to that side thereof that was next the Sun, we did endeavour with a pretty large burning Glass to kindle it, but found, as we fear'd, That, by reason of the thicknesse of the Glass, (which was also of a lesse pure & lesse Diaphanous matter then the other) the Sun-beams thrown in by the burning Glass, were in their passage so dislocated & scattered (not now to mention those many that being reflected, could not pierce into the cavity of the Receiver) that we could not possibly unite enough of them to kindle the matter, nor so much as to make it sensibly smoke. Yet we hope that the seeing whether Bodies (other then Gun powder) may be kindled, & what would happen to them when set on fire, in a place in great measure devoid of Air, may prove so Luciferous an Experiment, that when the Season is more favourable we shall, God permitting, make farther tryal of it, and acquaint your Lordship with the Event, if it prove prosperous. In the mean time we shall pass on to other Experiments, as soon as we have advertis'd Your Lordship that we have forborn to make such Reflections upon the severall Experiments we have set down concerning Fire, as the matter would have easily afforded, and Your Lordship may perhaps have expected. But I made the less scruple to forbear the annexing of Speculations to these Recitals, because *Carneades* & *E-leutherius* have in some Dialogues concerning Heat and Flame, (which were last year seen by some Friends, and may be, when you please, commanded by You,) mention'd divers of my Thoughts and Experiments concerning Fire.

 EXPERIMENT XVI.

WE designed to try whether or no divers Magnetical Experiments would exhibit any unusuall *Phenomena*, being made in our Evacuated Receiver instead of the open Air: But for want of leasure and conveniency to prosecute such Tryals, we were induced to reserve the rest for an other time, and to content our selves with making that which follows. We convey'd into the Receiver a little Pedestal of Wood, in the midst of which was perpendicularly erected a slender Iron, upon whose sharp point an excited Needle of Steel purposely made, and of about five Inches long, was so placed, that hanging in an *Aequilibrium* it could move freely towards either hand. Then the Air being after the usuall manner pumped out, we apply'd a Load-stone moderately vigorous to the out-side of the Glass, and found that it Attracted or Repell'd the ends of the Needle, according to the Laws Magneticall, without any remarkable difference from what the same Load-stone would have done, had none of the Air been drawn away from about the Needle, which, when the Loadstone was removed, after some tremulous Vibrations to and fro, rested in a position, wherein it look'd North and South.

EXPERIMENT XVII.

PROCEED we now to the mention of that Experiment, whereof the satisfactory tryal was the principall Fruit I promis'd my self from our Engine. It being then sufficiently known, that, in the Experiment *De Vacuo*, the Quicksilver in the Tube is wont to remain elevated, above the surface of that whereon it leans, about 27 digits: I considered, that, if the true & only reason why

why the Quick-silver falls no lower, be, that at that Altitude, the Mercurial Cylinder in the Tube is in an *Equilibriū* with the Cylinder of Air, suppos'd to reach from the adjacent Mercury to the top of the Atmosphere: then if this Experiment could be try'd out of the Atmosphere, the Quick-silver in the Tube would fall down to a levell with that in the Vessell, since then there would be no pressure upon the Subjacent, to resist the weight of the Incumbent Mercury. Whence I inferr'd (as easily I might) that if the Experiment could be try'd in our Engine, the Quick-silver would subside below 27 Digitis, in proportion to the exsuction of Air, that should be made out of the Receiver. For, as when the Air is shut into the Receiver, it doth (according to what hath above been taught) continue there as strongly compress'd, as it did whil'st all the incumbent Cylinder of the Atmosphere lean'd immediatly upon it; because the Glass, wherein it is pen'd up, hinders it to deliver it self, by an expansion of its parts, from the pressure wherewith it was shut up. So if we could perfectly draw the Air out of the Receiver, it would conduce as well to our purpose, as if we were allow'd to try the Experiment beyond the Atmosphere.

Wherefore (after having surmounted some little difficulties which occur'd at the beginning) the Experiment was made after this manner. We took a slender and very curiously blown Cylinder of Glass, of neer three foot in length, and whose bore had in Diameter a quarter of an Inch, wanting a hairs breadth: This Pipe being Hermetically seal'd at one end, was, at the other, fill'd with Quick-silver, care being taken in the filling, that as few bubbles as was possible should be left in the Mercury: Then the Tube being stopt with the Finger and inverted, was open'd, according to the manner of the Experiment, into a somewhat long and slender Cylindricall Box (instead of which we now are wont to use a Glass of the same form) half fill'd with Quick-silver: And so, the liquid metall being suffered to subside, and a piece of Paper being pasted on levell with its upper surface, the Box and Tube and all were by strings carefully let down into the Receiver: and then, by means of

the hole formerly mentioned to be left in the Cover, the said Cover was split along as much of the Tube as reached above the top of the Receiver; And the Intervall, left betwixt the sides of the Hole and those of the Tube, was very exquisitely filled up with melted (but not over hot) Diachylon, and the round chink, betwixt the Cover and the Receiver, was likewise very carefully closed up: Upon which closure there appeared not any change in the height of the Mercurial Cylinder; no more, then if the interposed Glass Receiver did not hinder the immediate pressure of the ambient Atmosphere upon the inclosed Air; which hereby appears to bear upon the Mercury, rather by virtue of its spring, then of its weight: since its weight cannot be supposed to amount to above two or three Ounces, which is inconsiderable in comparison of such a Cylinder of Mercury as it would keep from subsiding.

All things being thus in a readiness, the Sucker was drawn down; and, immediately upon the egress of a Cylinder of Air out of the Receiver, the Quick-silver in the Tube did, according to expectation, subside: And notice being carefully taken (by a mark fastened to the outside) of the place where it stopt, we caused him that managed the Pump to pump again, and marked how low the Quick-silver fell at the second exsuction; but continuing this work, we were quickly hindred from accurately marking the Stages made by the Mercury in its descent, because it soon sunk below the top of the Receiver, so that we could henceforward mark it no other ways then by the eye. And thus, continuing the labor of pumping for about a quarter of an hour, we found our selves unable to bring the Quick-silver in the Tube totally to subside; because, when the Receiver was considerably emptyed of its Air, and consequently that little that remained grown unable to resist the Irruption of the External, that Air would (in spite of whatever we could doe) press in at some little Avenue or other; and though much could not thereat get in, yet a little was sufficient to counterballance the pressure of so small a Cylinder of Quick-silver, as then remained in the Tube.

Now (to satisfy our selves farther, that the falling of the Quick-silver

silver in the Tube to a determinate height, proceedeth from the *Equilibrium*, wherein it is at that height with the external Air, the one gravitating, the other pressing with equal force upon the subjacent Mercury) we returned the Key and let in some new Air; upon which the Mercury immediately began to ascend (or rather to be impelled upwards) in the Tube, and continued ascending, till having returned the Key, it immediately rested at the height which it had then attained: And so, by turning and returning the Key, we did several times at pleasure impell it upwards, and check its ascent. And lastly, having given a free egress at the Stop-cock to as much of the external Air as would come in, the Quick-silver was impelled up almost to its first height: I say almost, because it stopt near a quarter of an Inch beneath the Paper mark formerly mentioned; which we ascribed to this, That there was (as is usual in this Experiment) some little Particles of Air engaged among those of the Quick-silver; which Particles, upon the descent of the Quick-silver, did manifestly rise up in Bubbles towards the top of the Tube, and by their pressure, as well as by lessening the Cylinder by as much room as they formerly took up in it, hindered the Quick-silver from regaining its first height.

This Experiment was a few days after repeated in the presence of those excellent and deservedly Famous Mathematick Professors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren*, who were pleased to Honor it with their Presence: And whom I name, both as justly counting it an Honor to be known to them, and as being glad of such Judicious and illustrious Witnesses of our Experiment; and 'twas by their guess, that the top of the Quick-silver in the Tube was defined to be brought within an Inch of the surface of that in the Vessel.

And here, for the Illustration of the foregoing Experiment, it will not be amiss to mention some other particulars relating to it.

First then, When we endeavored to make the Experiment with the Tube closed at one end with *Diachylon* instead of an Hermetical Seal; we perceived, that upon the drawing of some of the Air out
of

of the Receiver, the Mercury did indeed begin to fall, but continued afterwards to subside, though we did not continue pumping. When it appeared, that though the *Diachylon* that stoppt the end of the Tube, were so thick and strong, that the external Air could not press it in (as experience taught us that it would have done, if there had been but little of it) yet the subtler parts of it were able (though slowly) to insinuate themselves through the very body of the Plaister, which it seems was not of so close a Texture, as that which we mentioned our selves to have successfully made use of in the Experiment *De Vacuo* some years ago. So that now we begin to suspect, that perhaps one Reason, why we cannot perfectly pump out the Air, may be, that when the Vessel is almost empty, some of the subtler parts of the External Air may, by the pressure of the Atmosphere, be strained through the very body of the *Diachylon* into the Receiver. But this is onely conjecture.

Another Circumstance of our Experiment was this, That, if (when the Quick-silver in the Tube was fallen low) too much ingress were, at the hole of the Stop-cock, suddenly permitted to the external Air; it would rush in with that violence, and bear so forcibly upon the surface of the subjacent Quick-silver, that it would impell it up into the Tube rudely enough to endanger the breaking of the Glass.

We formerly mentioned, that the Quick-silver did not in its descent fall as much at a time after the two or three first exsuctions of the Air, as at the beginning. For, having marked its several Stages upon the Tube, we found, that at the first suck it descended an Inch and $\frac{3}{8}$, and at the second an Inch and $\frac{3}{8}$; and when the Vessel was almost emptied, it would scarce at one exsuction be drawn down above the breadth of a Barly-corn. And indeed we found it very difficult to measure in what proportion these decrements of the Mercurial Cylinder did proceed: Partly because (as we have already intimated) the Quick-silver was soon drawn below the top of the Receiver; and partly because, upon its descent at each exsuction, it would immediatly reascend a little upwards; either by
reason

reason of the leaking of the Vessell at some imperceptible hole or other, or by reason of the motion of Restitution in the Air, which, being somewhat compressed by the fall as well as weight of the Quick-silver, would repell it a little upwards, and make it vibrate a little up and down, before they could reduce each other to such an *Equilibrium* as both might rest in. But though we could not hitherto make observations accurate enough concerning the measures of the Quick-silver's descent, to reduce them into any *Hypothesis*, yet would we not discourage any from attempting it; since, if it could be reduc'd to a certainty, 'tis probable that the discovery would not be unusefull.

And, to illustrate this matter a little more, we will adde, That we made a shift to try the Experiment in one of our above mention'd small Receivers, not containing a Quart; but (agreeably to what we formerly observed) we found it as difficult to bring this to be quite empty as to evacuate the greater; the least external Air that could get in (& we could not possibly keep it all perfectly out) sufficing in so small a Vessell to display a considerable pressure upon the surface of the Mercury, & thereby hinder that in the Tube from falling to a level with it. But this is remarkable, that having two or three times try'd the Experiment in a small Vessell, upon the very first Cylinder of Air that was drawn out of the Receiver, the Mercury fell in the Tube 18 Inches and a halfe, and at an other tryal 19 Inches and a half.

But, on this occasion, I hold it not unfit to give your Lordship notice that I hop'd from the descent of the Quick-silver in the Tube upon the first suck, to derive this advantage: that I should thence be inabled to give a near guess at the proportion of force betwixt the pressure of the Air (according to its various states, as to Density and Rarefaction) and the gravity of Quick-silver, then hitherto hath been done. For in our Experiment there are diverse things given, that may be made use of towards such a discovery. For first we may know the capacity of the Receiver wherein the Experiment is made, since, by filling it with water, we may easily compute how many Quarts, or Measures of any other denomination,

nation, it containeth of Air; which Air, when shut up in the Vessell may be suppos'd to have a pressure equal to that of the Atmosphere; since it is able to keep the Quick-silver in the Tube from falling any lower then it did in the free and open Air. Next here is given us the capacity of the brass Cylinder empty'd by the drawing down of the Sucker (its bore and height being mention'd in the description of our Pump) whereby we may come to know how much of the Air contain'd in the Receiver is drawn out at the first suck. And we may also easily define, either in weight or cubick measures, the Cylinder of Quick-silver that answers to the Cylinder of Air lately mention'd (that Mercuriall Cylinder being in our Engine computable by deducting from the entire altitude of that Cylinder of Quick-silver, the altitude at which it rests upon the first exsuction.) But though, if this Experiment were very watchfully try'd in Vessels of severall sizes, and the various descents of the Quick-silver compar'd among themselves, 'tis not improbable, that some such things, as we hop'd for, may thereby be discovered. Yet because not onely the solid contents of as much of the Glass-tube as remains within the concave surface of the Receiver, and (which is more difficult) the varying contents of the Vessell containing the Mercury, and of as much of the Mercury it self as is not in the Tube, must be deducted out of the capacity of the Receiver, but there must also an allowance be made for this, that the Cylinder that is empty'd by the drawing down of the Sucker, and comes to be fill'd upon the letting of the Air out of the Receiver into it, is not so replenish'd with Air as the Receiver it self at first was: because there passeth no more Air out of the Receiver into the Cylinder, then is requisite to reduce the Air in the cavity of the Cylinder, and in that of the Receiver to the same measure of dilatation. Because of these (I say) and some other difficulties that require more skill in Mathematicks then I pretend to, and much more leasure then my present occasions would allow me, I was willing to refer the nicer consideration of this matter to some of our Learned and accurate Mathematicians, thinking it enough for me to have given the Hint already suggested.

For farther confirmation of what hath been delivered, we likewise tryed the Experiment in a Tube of less then two foot long: and, when there was so much Air drawn out of the Vessel, that the remaining Air was not able to counterballance the Mercurial Cylinder, the Quick-silver in the Tube subsided so visibly, that (the Experiment being try'd in the little Vessell lately mention'd) at the first suck it fell above a span, and was afterwards drawn lower and lower for a little while; and the external Air being let in upon it, impell'd it up again almost to the top of the Tube: So little matters it how heavy or light the Cylinder of Quick-silver to subside is, provided its gravity overpower the pressure of as much external Air as bears upon the surface of that Mercury into which it is to fall.

Lastly we also observ'd, That if (when the Mercury in the Tube had been drawn down, and by an Ingress permitted to the external Air, impell'd up again to its former height) there were some more Air thrust up by the help of the Pump into the Receiver, the Quick-silver in the Tube would ascend much above the wonted height of 27 digits, and immediatly upon the letting out of that Air would fall again to the height it rested at before.

Your Lordship will here perhaps expect, that as those who have treated of the *Torræcellian* Experiment, have for the most part maintained the Affirmative, or the Negative of that famous Question, Whether or no that Noble Experiment infer a *Vacuum*? so I should on this occasion interpose my Opinion touching that Controversie, or at least declare whether or no, in our Engine, the exsuction of the Air do prove the place deserted by the Air suck'd out, to be truly empty, that is, devoid of all Corporeal Substance. But besides, that I have neither the leasure, nor the ability, to enter into a solemn Debate of so nice a Question, Your Lordship may, if you think it worth the trouble, in the Dialogues not long since refer'd to, find the Difficulties on both sides represented; which then made me yeild but a very wavering assent to either of the parties contending about the Question: Nor dare I yet take upon me to determine so difficult a Controversy.

For on the one side it appears, that notwithstanding the extraction of the Air, our Receiver may not be destitute of all Bodies, since any thing placed in it, may be seen there; which would not be, if it were not pervious to those beams of Light which rebounding from the seen Object to our eyes, affect us with the sense of it: And that either these Beams are Corporeal Emanations from some lucid body, or else at least, the light they convey doth result from the brisk Motion of some subtle Matter, I could, if I mistake not, sufficiently manifest out of the Dialogues above mention'd, if I thought your Lordship could seriously imagine that Light could be convey'd without, at least, having (if I may so speak) a Body for its Vehicle.

By the sixteenth Experiment, it also appears that the closeness of our Receiver hinders it not from admitting the Effluvia of the Load-stone; which makes it very probable that it also freely admits the Magnetical steams of the Earth; concerning which, we have in another Treatise endeavour'd to manifest that numbers of them do alwaies permeate our Air.

But on the other side it may be said, That as for the subtle Matter which makes the Objects enclosed in our evacuated Receiver, visible, and the Magnetical Effluvia of the Earth that may be presum'd to pass thorow it, though we should grant our Vessel not to be quite devoyd of them, yet we cannot so reasonably affirm it to be replenish'd with them, as we may suppose, that if they were gather'd together into one place without Intervalls between them, they would fill but a small part of the whole Receiver. As in the thirteenth Experiment, a piece of Match was inconsiderable for its bulk, whilest its parts lay close together, that afterwards (when the Fire had scatter'd them into smoke) seem'd to replenish all the Vessel. For (as elsewhere our Experiments have demonstrated) both Light and the Effluvia of the Load-stone, may be readily admitted into a Glasse, Hermetically seal'd, though before their Admission, as full of Air as hollow Bodies here below are wont to be; so that upon the extraction of the Air, the large space deserted by it, may remain empty, notwithstanding the pretence of those
subtle

subtle Corpuscles, by which Lucid and Magnetical Bodies produce their effects.

And as for the Allegations above mention'd, they seem'd to prove but that the Receiver devoid of Air, *May* be replenish'd with some *Ethereal* Matter, as some Modern Naturalists write of but not that it really *is* so. And indeed to me it yet seems, that as to those spaces which the *Vacuists* would have to be empty, because they are manifestly devoid of Air; and all grosser Bodies the *Plenists* (if I may so call them) do not prove that such spaces are replenish'd with such a subtle Matter as they speak of, by any sensible effects, or operations of it (of which diverse new Tryals purposely made, have not yet shewn me any) but onely conclude that there must be such a Body, because there cannot be a Void. And the reason why there cannot be a Void, being by them taken, not from any Experiments, or *Phænomena* of Nature, that clearly and particularly prove their *Hypothesis*, but from their notion of a Body, whose Nature, according to them, consisting onely in extension (which indeed seems the property most essential to, because inseparable from a Body) to say a space devoid of Body, is to speak in the School mens Phrase, a Contradiction *in Adjecto*. This reason I say, being thus desum'd, seems to make the Controversie about a *Vacuum*, rather a Metaphysical, then a Physiological Question; which therefore we shall here no longer debate, finding it very difficult either to satisfy Naturalists with this Cartesian Notion of a Body, or to manifest wherein it is erroneous, and substitute a better in its stead.

But though we are unwilling to examine any farther the Inferences wont to be made from the *Torricellian* Experiment, yet we think it not impertinent to present Your Lordship with a couple of Advertisements concerning it.

First, then if in trying the Experiment here or elsewhere, you make use of the English measures that Mathematicians & Tradesmen are here wont to imploy, You will, unlesse you be forewarn'd of it, be apt to suspect that those that have written of the Experiment have been mistaken. For whereas men are wont generally

rally to talk of the Quick-silvers remaining suspended at the height of between six or seven and twenty Inches; we commonly observed, when divers years since we first were sollicitous about this Experiment, that the Quick-silver in the Tube rested at about 29 Inches and an halfe above the surface of the Restagnant Quick-silver in the Vessell, which did at first both amaze and perplex us, because though we held it not improbable that the difference of the grosser English Air, and that of *Italy* and *France*, might keep the Quick-silver from falling quite as low in this colder, as in those warmer Climates; yet we could not believe that that difference in the Air should alone be able to make so great an one in the heights of the Mercurial Cylinders; and accordingly upon enquiry we found, that though the various density of the Air be not to be over-looked in this Experiment, yet the main Reason why we found the Cylinder of Mercury to consist of so many Inches, was this, That our English Inches are somewhat inferior in length to the digits made use of in Foreign Parts, by the Writers of the Experiment.

The next thing I desire Your Lordship to take notice of, is, That the height of the Mercurial Cylinder is not wont to be found altogether so great as really it might prove, by reason of the negligence or incogitancy of most that make the Experiment. For oftentimes upon the opening of the inverted Tube into the Vessell'd Mercury, you may observe a bubble of Air to ascend from the bottom of the Tube through the subsiding Quick-silver to the top; and almost always you may, if you look narrowly, take notice of a multitude of small bubbles all along the inside of the Tube betwixt the Quick-silver and the Glasse: (not now to mention the Particles of Air that lye concealed in the very Body of the Mercury) Many of which, upon the Quick-silvers forsaking the upper part of the Tube, do break into that deserted space where they finde little or no resistance to their expanding of themselves. Whether this be the reason that upon the Application of warm Bodies to the emptyed part of the Tube, the subjacent Mercury would be depressed somewhat lower, we shall not determine; though

though it seem very probable, especially since we found, that, upon the application of Linnen cloaths dipped in Water, to the same part of the Tube, the Quick-silver would somewhat ascend; as if the cold had condensed the Imprisoned Air, (that pressed upon it,) into a lesser room. But that the deserted space is not wont to be totally devoid of Air, we were induced to think by several Circumstances. For when an eminent Mathematician, and excellent Experimenter, had taken great pains and spent much time in accurately filling up a Tube of Mercury, we found that yet there remained store of inconspicuous bubbles, by inverting the Tube, letting the Quick-silver fall to its wonted height, and by applying (by degrees) a red hot Iron to the out-side of the Tube, over against the upper part of the Mercurial Cylinder, (for hereby the little unheeded bubbles, being mightily expanded, ascended in such numbers, and so fast to the deserted space, that the upper part of the Quick-silver seemed, to our wonder, to boyl.) We farther observed, That in the tryals of the *Torricellian* Experiment we have seen made by others, and (one excepted) all our own, we never found that upon the inclining of the Tube the Quick-silver would fully reach to the very top of the sealed end: Which argued, that there was some Air retreated thither that kept the Mercury out of the unreplenished space.

If Your Lordship should now demand what are the best expedients to hinder the intrusion of the Air in this Experiment; we must answer, That of those which are easily intelligible without ocular demonstration, we can at present suggest, upon our own tryals, no better then these. First, at the open end of the Tube the Glass must not onely be made as even at the edges as you can, but it is very convenient (especially if the Tube be large) that the bottom, be every way bent inwards, that so the Orifice, not much exceeding a quarter of an Inch in Diameter, may be the more easily and exactly stopped by the Experimenter's finger; between which and the Quick-silver, that there may be no Air intercepted (as very often it happens that there is) it is requisite that the Tube be filled as full as possibly it can be, that the finger which is to stop it,
 pressing

pressing upon the accumulated and protuberant Mercury; may rather throw down some, then not finde enough exactly to keep out the Air. It is also an useful and compendious way not to fill the Tube at first quite of Mercury, but to leave near the top about a quarter of an Inch empty; for if you then stop the open end with your finger, and invert the Tube that quarter of an Inch of Air will ascend in a great bubble to the top, and in its passage thither, will gather up all the little bubbles, and unite them with it selfe into one great one; so that if by reinverting the Tube, you let that bubble return to the open end of it, you will have a much closer Mercurial Cylinder then before, and need but to adde a very little Quick-silver more to fill up the Tube exactly. And lastly, as for those lesser and inconspicuous parcels of Air which cannot this way be gleaned up, You may endeavor before you invert the Tube, to free the Quick-silver from them by shaking the Tube, and gently knocking on the out-side of it, after every little parcel of Quick-silver which you pour in; and afterwards, by forcing the small latitant bubbles of Air to disclose themselves and break, by imploying a hot Iron in such manner as we lately mentioned. I remember that by carefully filling the Tube, though yet it were not quite free from Air, we have made the Mercurial Cylinder reach to 30 Inches and above an eight, and this in a very short Tube: which we therefore mention, because we have found, by Experience, that in short Tubes a little Air is more prejudicial to the Experiment then in long ones, where the Air having more room to expand it selfe, doth less potently press upon the subjacent Mercury.

And since we are fallen upon the consideration of the Altitude of the Mercurial Cylinder, I must not conceal from Your Lordship an Experiment relating thereunto, which perhaps will set both You and many of your Friends the *Virtuosi* on thinking; and, by disclosing some things about the Air or Atmosphere, that have scarce hitherto been taken notice of, may afford you some hints conducive to a further discovery of the subject of this Epistle.

EXPERIMENT XVIII.

WE took a Glass Tube, which though it were not much above three Foot long, we made choice of, because it was of a more then ordinarily even thicknesse. This we fill'd with Mercury, though not with as much care as we could yet with somewhat more then is wont to be used in making the *Torricellian* Experiment. Then, having according to the manner inverted the Tube, & open'd the mouth of it beneath the surface of some other Quick-silver, that in the Tube fell down to the wonted heighth, leaving, as is usual, some little Particles of Air in the space it deserted, as we ghest by observing, that upon the Application of hot Bodiesto the upper part of the Tube, the Quick-silver would be a little depress'd. Lastly, having put both the Tube and the Vessel it lean'd on into a convenient Wooden Frame, to keep them from mischances: we plac'd that Frame in a Window within my Bed-chamber, that I might both keep the Mercury from being stirr'd, and have opportunity to watch from time to time the *Phænomena* it was to exhibit. For the better discovery of which, when the Quick-silver both in the Tube and adjacent Vessel was perfectly at rest, we took notice, by a mark made on the outside of the Glass, how high the included Liquor then reach'd.

During severall Weeks that the Tube was kept in that Window (which was very rarely open'd) I had the opportunity to observe, that the Quick-silver did sometimes faintly imitate the Liquor of a Weather-glasse, subsiding a little in warm, and rising a little in cold Weather, which we ascribed to the greater or lesser pressure of that little Air that remain'd at the top of the Tube, expanded or condens'd by the heat or cold that affected the ambient Air. But that which I was chiefly carefull to observe, was this, That oftentimes the Quick-silver did rise & fall in the Tube, & that very notably, without conforming it self to what is usual in Weather-glasses,

whose Air is at the top, nay quite contrary thereunto: for some times I observ'd it in very cold weather (such as this Winter hath already afforded us good store of) to fall down much lower than at other times, when by reason of the absence of both Frost, Snow and sharp Winds, the Air was comparatively much warmer. And I farther observ'd, That sometimes the Quick-silver would for some daies together rest almost at the same height; and at other times again it would in the compasse of the same day considerably vary its altitude, though there appear'd no change either in the Air abroad, or in the temper of the Air within the Room (wherein was constantly kept a good Fire) nor in any thing else, to which either I, or some eminently Learned Men, whom I then acquainted with the Experiment, could reasonably impute such a change. Especially considering that the space wherein the Mercury wandered up and down, within about five Weeks, amounted to full two Inches, of which we found by our severall marks whereabout we had taken notice of its severall removes, that it had descended about $\frac{2}{16}$ of an Inch from the place where it first settled, and the other Inch and $\frac{2}{16}$ it had ascended. And it seems probable that the height of the Mercurial Cylinder would have varied yet more, if the Experiment had been made in the open Air, and in a long Tube, where the Particles of the Imprison'd Air, by having more room to display themselves in, might not have had so strong a Spring to work upon the Quick-silver with. But for want both of time and of a competent quantity of Mercury (which was not to be procur'd where we then happen'd to be) we were unable to make any farther tryals: which therefore chiefly troubled us, because we would gladly have try'd an ingenious Experiment which was suggested unto us by that excellent Mathematician Mr. *Wren*, who being invited to name any thing he would have us try touching the pressure of the Air, desired us to observe whether or no the Quick-silver in a long Tube would not a little vary its height according to the Tides, especially about the New and Full Moons about which times Mariners observe those great Flowings and Ebbs of the Sea, that they call the Spring-Tides. For he sagaci-

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ously and plausibly conjectur'd that such observations accurately made, would discover the truth or erroneoufnesse of the *Cartesian Hypothesis* concerning the Ebbing and Flowing of the Sea: which *Des Cartes* ascribeth to the greater pressure made upon the Air by the Moon, and the Intercurrent Ethereal Substance at certain times (of the Day, and of the Lunary Moneth) then at others. But in regard we found the Quick-silver in the Tube to move up and down so uncertainly, by reason, as it seems, of accidental mutation in the Air; I somewhat doubt whether we shall find the Altitude of the Quick-silver to vary as regularly as the Experiment is ingeniously propos'd. The success we shall (God permitting us to make tryal of it) acquaint your Lordship with; and in the meantime take notice, that when we had occasion to take the Tube out of the Frame (after it had staid there part of *November* and part of *December*) a good Fire being then in the room, because it was a Snowy day, we found the Quick-silver in the Tube to be above the upper surface of the subjacent Mercury 29 Inches three quarters.

If Your Lorpship should now aske me what are the true causes of this varying altitude of the Mercurial Cylinder, I should not undertake to answer so difficult a question, and should venter to say no more, then that among divers possible causes to which it may be ascribed, it would not be, perhaps, absurd to reckon these that follow.

First then we may consider, that the Air in the upper part of the Tube is much more rarefied, & therefore more weak then the External Air, as may appear by this among other things, That upon the inclining of the Tube, the Quick-silver will readily ascend almost to the very top of it, and so take up eight or nine tenth parts, and perhaps more of that space which it deserted before: which would not happen, if that whole space had been full of unrarefied Air, since that (as tryal may easily satisfie you) would not have suffer'd it self to be thrust into so narrow a room by so weak a pressure. So that although in our Tube when the included Air was heated, the Quick-silver was somewhat depress'd; yet there is this difference

betwixt such a Tube and common Weather-Glasses, that in these the included and the ambient Air are in an *Æquilibrium* as to pressure, and the weight of the Water that keeps them separate is scarce considerable. Whereas in such a Tube as we are speaking of, the Air within, is very much more dilated then that without. And 'tis not so much the spring or resistance of the included Air, as the weight of the Mercurial Cylinder it self, that hinders the Quick-silver from ascending higher: For if we should suppose that deserted part of the Tube perfectly devoid of Air, yet would the Quick-silver rise but a little higher in it, and be far from filling it; in regard the outward Air would not be able to impell up such a weight much higher: whereas it may, by our former Experiments appear, that if all the Air in the upper part of a Weather-Glass were away, the Water would be impell'd up to the very top of it, though the Pipe were above thirty Foot long.

We may next consider, that this rarefied Air at the upper part of our Tube being exactly shut up betwixt the Glass and the Quick-silver, it was scarce subject to any discernable alterations, save those it receiv'd from heat and cold.

And we may farther consider, that yet the external Air or Atmosphere is subject to many alterations, besides them that proceed from either of those Qualities.

For the Experiment that occasion'd this Discourse, seems to make it probable enough, that there may be strange Ebbings and Flowings, as it were, in the Atmosphere, or at least, that it may admit great and sudden Mutations, either as to its Altitude or its Density, from causes, as well unknown to us, as the effects are unheeded by us. And that You may not think that there is nothing in Nature but our Experiment that agrees with this our conjecture, we might put Your Lordship in mind of the Pains and Aches that are often complain'd of by those that have had great Wounds or Bruises, and that do presage great Mutations in the Air oftentimes, whilst to strong and healthy Persons no sign of any such thing appears. And that is also very memorable to this purpose, which I remember I have somewhere read in a Book of the Ingenious

ous *Kircherus*, who giving a pertinent admonition concerning the various refractions that may happen in the Air, relateth, That during his stay in *Malta*, he often saw Mount *Atna*, though the next day, notwithstanding its being extremely clear, he could not see it; adding, that *Vintemilius*, a very Learned Person, did oftentimes, from a Hill he names, behold the whole Island he calls *Luprica* protuberant above the Sea, though at other times, notwithstanding a clear Sky, he could not see it. And though perhaps this may be in part ascribed to the various light & position of the Sun, or to the various disposition of the Spectators eye, or peradventure to some other cause; yet the most probable cause seems to be the differing Density of the Air, occasion'd by Exhalations capable to increase the refraction, and consequently bring Beams to the Eye, which otherwise would not fall on it. We have likewise, in another Treatise, mention'd our having often observ'd with Telescopes a plenty of Steams in the Air, which without such a help would not be taken notice of, & which, as they were not at all times to be seen even through a Telescope, so they did sometimes, especially after a shower of Rain, hastily disappear: and when we have visited those places that abound with Mines, we have several times been told by the Diggers, that even when the Sky seem'd clear, there would, not seldome, sudden'y arise, and sometimes long continue, a certain Steam (which they usually call a damp) so gross and thick, that it would oftentimes put out their very Candles, if they did not seasonably prevent it. And I think it will easily be granted, that the ascension of such Steams into this or that part of the Air, and their mixing with it, are very like to thicken it; as on the other side either heat or the sudden condensation of the Air in another part of the Atmosphere (to mention now no other causes) are capable of rarefying it.

Nor will it very much import the main scope of our Discourse, whether it be suppos'd that the copious Steams the earth sends into the air, thicken that part of the Atmosphere that receives them, and make it more heavy: Or that sometimes the Fumes may ascend with such celerity, that though the Air be thicken'd yet they rather diminish then encrease its gravitation, in regard that

the quickness of their ascent, not onely keeps them from gravitating themselves, but may hinder the pressing downwards of many Aërial Corpuscles that they meet with in their way upwards. This I say, is of no great importance to our present Discourse, since either way the Terrestrial Steam may here and there considerably alter the gravity or pressure of the Atmosphere.

Your Lordship may also be pleased to remember, That by our seventeenth Experiment it appear'd, that, as when the Air in the Receiver was expanded more then ordinarily, the Quick-silver in the Tube did proportionably subside; so when the Air in the same Receiver was a little more then ordinarily compress'd, it did impell up the Quick-silver in the Tube, above the wonted height of betwixt six and seven and twenty digits.

And if to these things we annex, that for ought we can find by tryals purposely made, the degree of rarity or density of the Air, shut up into our Receiver, doth not sensibly alter its temperature as to cold or heat. It will not, I hope, appear absurd to conceive, That since the Air, included in the Tube, could but very faintly hinder the ascent of the Quick-silver, or press it downwards; Since too, that included Air could scarce immediately receive any sensible alteration, save either by heat or cold; And since also, that according to the bare density or rarity of the Air incumbent on the subjacent Quick-silver in the Vessel, that in the Tube was impell'd more or less high; such changes happening in the neighboring part of the outward Air, either by the ascension of gross or copious exhalations, or by any other cause (of which there may be divers) as were capable to make considerable alterations in the consistence of the Air, as to rarity and density, *may* be able proportionably to alter the height of the Quick-silver. I rather say, that such alterations *may* be, then that they *are* the causes of our *Phænomènon*: because I think it sufficient, if I have propos'd conjectures not altogether irrational about a new Mystery of Nature, touching which, the chief thing I pretend to, is to give occasion to the Curious to enquire farther into it, then I have been yet able to do,

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EXPERIMENT XIX.

THE same Reason that mov'd us to conclude, that by the drawing of the Air out of the Receiver, the Mercury would descend in a Tube shorter then six and twenty digits, induc'd us also to expect, that by the same means Water might be brought to subside in Glafs Tubes of a moderate length: Though by the noble Experiment, said to have been accurately made in *France*, by *Monsieur Paschal*, we are informed that a Tube of no lesse then about two and thirty Foot, was found requisite to make the Experiment *De Vacuo* succeed with Water instead of Quick-silver: so tall a Cylinder of that lighter Liquor, being, it seems, requisite to equal the weight of a Mercurial Cylinder of six or seven and twenty digits, and surmount the pressure of the Atmosphere.

We took then a Tube of Glafs, Hermetically seal'd at one end, of about four foot in length, and not very slender: This, at the open end, we fill'd with common Water, and then stop't that end till we had inverted the Tube, and open'd it beneath the surface of a quantity of the like Water, contain'd in a somewhat deep and slender Vessel. This Vessel, with the Tube in it, was let down into the Receiver, and the Receiver being clos'd up after the accustomed manner, the Pump was set on work.

As much of the event as concerns our present purpose, was this, That till a considerable part of the Air was drawn out of the Receiver, the Tube continu'd top-full of Water as when it was put in, it being requisite that a great part of the Air, formerly contain'd in the Receiver, should be drawn out, to bring the remaining Air to an *Equilibrium* with so short and light a Cylinder of Water. But when once the Water began to fall in the Tube, then each exsuction of Air made it descend a little lower, though nothing near so much as the Quick-silver at the beginning did in the Experiment formerly mention'd. Nor did there appear so much inequality

equality in the spaces transmitted by the Water in its descent, as there it did in those observ'd in the fall of the Quick-silver, of which the cause will scarce seem abstruse to him that shall duly reflect upon what hath been already deliver'd. And whereas we drew down the Quick-silver in the Tube so far as to bring it within an Inch of the surface of the other Quick-silver into which it was to fall; the lowest we were able to draw down the Water was, by our conjecture, to about a Foot or more above the surface of that in the Vessel; of which I know not whether it will be needfull to assign so obvious a cause as that, though the little Air remaining in the Receiver could not hinder a Cylinder of above an Inch high of Quick-silver from subsiding; yet it might be very well able, by its pressure, to countervail the weight of a Cylinder of a Foot long or more, of a Liquor so much less ponderous then Quick-silver, as Water is. And in fine, to conclude our Experiment, when the Water was drawn down thus low, we found, that by letting in the outward Air, it might be immediately impell'd up again to the higher parts of the Tube.

We will adde no more concerning this Experiment, save that having try'd it in one of our small Receivers, we observ'd, That upon the first exsuction of the Air the Water did usually subside divers Inches, and at the second (exsuction) fall down much lower, subsiding sometimes near two Foot; as also that upon the letting in of the Air from without, the Water was impell'd up with very great celerity.

EXPERIMENT XX.

THat the Air hath a notable Elastical power (whencesoever that proceeds) we have, I suppose, abundantly evinc'd, and it begins to be acknowledg'd by the eminentest Modern Naturalists. But whether or no there be in Water so much as a languid one, seems hitherto to have been scarce consider'd, nor hath been yet,
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ought I know, determin'd either way by any Writer, which invited us to make the following Experiment.

There was taken a great Glass-bubble, with a long neck; (such as Chymists are wont to call a Philosophical Egge) which being fill'd with common Water till the Liquor reach'd about a span above the bubble, and a piece of Paper being there pasted on, was put unstop'd into the Receiver, and then the Air was suck'd out after the wonted manner. The event was this, That a considerable part of the Air, pent up in the Receiver, was drawn out, before we discern'd any expansion of the Water; but, continuing the labour of pumping, the Water manifestly began to ascend in the stem of the Glass, and diverse bubbles loosening themselves from the lower parts of the Vessel, made their way through the Body of the Water, to the top of it, and there brake into the Receiver: And after the Water once appear'd to swell, then at each time the Stop-cock was turn'd to let out the Air from the Receiver into the Pump, the Water in the Neck of the Glass did suddenly rise about the breadth of a Barly-corn in the Neck of the Glass, and so attain'd, by degrees, to a considerable height above the mark formerly mention'd. And at length (to make the expansion of the Water more evident) the outward Air was suddenly let in, and the Water immediately subsided and deserted all the space it had newly gain'd in the Glass.

And, on this occasion, it will not perhaps be amiss to acquaint Your Lordship here (though we have already mention'd it in another Paper, to another purpose) with another Expedient that we made use of two or three years ago, to try whether or no Water had a Spring in it. About that time then, That Great & Learned Promoter of Experimental Philosophy Dr. *Wilkins*, doing me the Honor to come himself, and bring some of his inquisitive Friends to my Lodging, we there had in readiness a round and hollow Vessel of Pewter, great enough to contain two pounds of Water, and exactly close every where, but at one little hole where it was to be fill'd; then partly by sucking out the Air, and partly by injecting Water with a Syringe, it was (not without

some difficulty) fill'd up to the top; and that hole being plac'd directly upwards, there was a little more Water leisurely forc'd in by the Syringe. Upon which, though the Vessel were permitted to rest, and the hole kept in its former posture, yet the compress'd Water leisurely swell'd above the Orifice of the hole, and divers drops ran over along the sides of the Vessel. After this we caus'd a skilfull Pewterer (who had made the Globe) to close it up in our presence with Soder so exquisitely, that none suspected there was any thing left in it besides Water. And lastly, the Vessel thus soder'd up, was warily and often struck in diverse places with a Wooden Mallet, and thereby was manifestly compress'd, whereby the inclosed Water was croud'd into less room than it had before: And thereupon we took a Needle, and with it and the Mallet perforated the Vessel, and drew out the Needle again; the Water (but in a very slender Stream) was suddenly thrown after it into the Air, to the height of two or three Feet. As for the other *Phænomena* of this Experiment, since they belong not to our present purpose, and are partly mention'd in another of our Papers, we shall, instead of recording them here, give this Advertisement: That as evidently as this Experiment, and that made in our Receiver, seem to prove a power in the Water to expand and restore it self after compression; yet for a reason to be met with ere long, I judg'd it not safe, to infer that Conclusion from these Premises, till I had made some of the following tryals, to the mention of which I will therefore hasten.

E X P E R I M E N T XXI.

TO discover whether the Expansion of the Water really proceeded from an Elastical power in the parts of the Water it self, we thought it requisite to try two things: The one, Whether or no the Atmosphere gravitates upon Bodies under Water; and the other, Whether in case it do gravitate, the Intumescence of
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the Water may not be ascribed to some substance subtler then it self, residing in it. In order to the satisfying my self about the first of these, I intended to let down into the Receiver a Vessel of Water, wherein should be immerf'd a very small oyl'd Bladder, almost devoid of Air, but strongly ty'd up at the Neck with a string, and detain'd a little under Water by such a weight fasten'd to that string, as should just be able to keep the Bladder from swimming, and no more. For I suppos'd, that if when all things were thus order'd, the Receiver were empty'd, in case there were any such pressure of the Atmosphere upon Water, as I was inclin'd to believe, the Air within the Bladder, being upon the exsuction of the Air within the Receiver, freed from that pressure, and being press'd onely by the small weight of the incumbent Water, would considerably expand it self; but whilst we were preparing Bladders for this Experiment, there occur'd an easie way for the making at once both the Discoveries I desir'd.

We took then a Glafs Viol, containing by ghes a pound and some ounces of Water, this we fill'd top full, and then we put into the Neck of it a Glafs Pipe a pretty deal bigger then a Goose Quill, open at both ends, and of diverse Inches in length: One end of this Pipe was so put into the Neck of the Viol, as to reach a little below it, and then was carefully cemented thereto that no Air might get into the Viol, nor any Water get out of it, otherwise then through the Pipe; and then the Pipe being warily fill'd, about half way up to the top, with more Water, and a mark being pasted over against the upper surface of the Liquor; the Viol thus fitted with the Pipe, was, by strings let down into the Receiver, and according to the wonted manner exquisitely clos'd up in it.

This done, we began to Pump out the Air, and when a pretty quantity of it had been drawn away, the Water in the Pipe began to rise higher in the Pipe, at the sides of which some little bubbles discover'd themselves. After a little while longer, the Water still swelling, there appear'd at the bottom of the Pipe a bubble about the bignesse of a small Pea, which ascending through the

Pipe to the top of the Water, staid there a while and then broke; but the Pump being nimbly ply'd, the expansion of the Water so encreas'd, that quickly, getting up to the top of the Pipe, some drops of it began to run down along the out-side of it, which oblig'd us to forbear pumping a while, and give the Water leave to subside within less then two Inches of the bottom of the Pipe. After this the Pump being again set on work, the bubbles began to ascend from the bottom of the Pipe, being not all of a size, but yet so big, that estimating one with another, they appear'd to be of the size of the smaller sort of Peas; and of these we reckon'd about sixty which came up one after another, besides store of smaller ones, of which we made no reckoning: And at length, growing weary of reckoning and pumping too (because we found, that in spite of all our pains and industry, some undiscern'd Leak or other in the Receiver hinder'd us from being able to empty it altogether) we thought fit to desist for that time, after tryal made of what operation the external Air, being let in upon the expanded Water, would have; and accordingly turning the Key to let in the Air, we saw, as we expected, that the Water in the Pipe in a moment fell down almost to the bottom of it.

Now of this Experiment there are two or three Circumstances yet to be mention'd, which are no less, then those already recited, pertinent to our present purpose.

In the first place then, when the greater part of the Air had been pump'd out of the Receiver, the rising bubbles ascended so very slowly in the Pipe, that their Progress was scarce discernable; which seem'd to proceed from this, That their bigness was such, That they could not sufficiently extend themselves in the cavity of the Glafs, without pressing on both hands against the sides of it, whereby they became of more difficult extrusion to the Water. And though it may seem strange that these bubbles should be of any considerable bulk, since 'tis like they consisted of lesser parcels of the Air lurking in the Water, then those that were vigorous enough to make their way through, long before them

them: yet they were commonly much larger then before, some of them being equal in quantity to four or five Peas: whether this their increase of bulk proceeded from the greater decrement of the pressure of the Air, or from the Union of two or three of those numerous bubbles which were then generated below the bottom of the Pipe, where we could not see what was done among them.

Another thing we noted in our bubbles was, That whereas in ordinary ones the Air, together with the thin film of Water that invests and detains it, is wont to swell above the surface of the Water it swims on, and commonly to constitute Hemispherical Bodies with it, the little parcels of Air that came up after the Receiver was pretty well empty'd, did not make protuberant bubbles, but such whose upper surface was either level with, or beneath that of the Water, so that the upper surface being usually somewhat convex, the lesse protuberant parts of it had a pretty quantity of Water remaining above them.

We also farther observ'd, that whereas in the bubbles that first appear'd in the Pipe, the ascending Air did, as in other common bubbles, make its way upwards, by dividing the Water through which it pass'd, in those bubbles that appear'd at the latter end of our Experiment, when the pressure of the little external Air, remaining in the Receiver, was grown inconsiderable, the ascending parcels of Air having now little more then the weight of the Incumbent Water to surmount, were able both so to expand themselves as to fill up that part of the Pipe which they pervaded, & by pressing every way against the sides of it, to lift upwards with them what Water they found above them, without letting any considerable quantity glide down along the sides of the Glass: So that sometimes we could see a bubble thrust on before it a whole Cylinder of Water of perhaps an Inch high, and carry it up to the top of the Pipe; though as we formerly noted, upon the letting in the external Air, these tumid bubbles suddenly relaps'd to their former inconspicuousness.

All these things laid together seem'd sufficiently to confirm that, which the consideration of the thing it self would easily

enough perswade, namely, That the Air, and such like Bodies being under VWater, may be press'd upon as well by the Atmosphere, as by the weight of the incumbent VWater it self.

Hence likewise we may verifie what we observ'd at the close of the foregoing Experiment, namely, That from the sole swelling of VWater there recorded, it cannot be so safely concluded, that VWater, when freed from compression, is endow'd with an Elastical power of expanding it self: since thereby it appears that the Intumescence produc'd by that Experiment, may (at least in great part) be ascribed to the numerous little bubbles which are wont to be produc'd in VWater, from which the pressure of the Atmosphere is in great measure taken off. So apt are we to be mis-led, even by Experiments themselves, into Mistakes, when either we consider not that most Effects may proceed from various Causes, or mind onely those Circumstances of our Experiment, which seem to comply with our preconceiv'd *Hypothesis* or Conjectures.

And hence it seems also probable, that in the Pores or invisible little recesses of VWater it self there lie commonly interspers'd many parcels of either Air, or at least something Analagous thereunto, although so very small, that they have not been hitherto so much as suspected to lurk there. But if it be demanded how it appears that there is interspers'd through the Body of VWater any substance thinner then it self, and why that which produc'd the bubbles above mention'd should not be resolutely said to be nothing else then a more active and spirituous part of the VWater, we shall, in order to the Elucidation of this matter, subjoyn to what was formerly deliver'd the following Experiment.

EXPERIMENT XXII.

VVE recited in our nineteenth Experiment, how by drawing most of the Air out of the Receiver, we made the VWater subside

subside by degrees in a Glass not four Foot long: We shall now adde, that in the like Experiment made in such a Tube, or a greater, it may be observ'd, That when the Water begins to fall, there will appear store of bubbles fasten'd all along to the sides of the Glass; of which bubbles, by the agitation of the Vessel consequent upon pumping, there will arise good numbers to the top of the Water, and there break; and as the Cylinder of Water is brought to be lower and lower, so the bubbles will appear more numerous in that part of the Tube which the Water yet fills; and the nearer the surface of the Water, in its descent, approacheth to these bubbles, the greater they will grow, because having the less weight and pressure upon them, the Expansion of that Air which makes them, can be the less resisted by the pressure of the incumbent Water and Air; as seems probable from hence, that upon the letting in a little externall Air, those bubbles immediately shrink.

It may indeed, as we lately intimated, be conjectur'd, that these bubbles proceed not so much from any Air pre-existent in the Water, and lurking in the Pores of it, as from the more subtle parts of the Water it self; which by the expansion allow'd them upon the diminish'd pressure of the ambient Bodies may generate such bubbles. And indeed, I am not yet so well satisfied that bubbles may not (at least sometimes) have such an Origination: but that which makes me suspect that those in our tryals contain'd real Air, formerly latent in the Pores of the Water, is this, That upon the inletting of the External Air, the Water was not again impell'd to the very top of the Tube whence it began to fall, but was stopt in its ascent near an Inch beneath the top. And since, if the upper part of the Tube had been devoid of any other then such Ethereal matter as was subtle enough freely to penetrate the pores of the Glass, the external Air would have been able to impell the Water to the top of a Tube seven or eight times as long as ours was; The *Phænomenon* under consideration seem'd manifestly to argue, that the many bubbles that broke at the top of the Water, did contain a real Air, which being collected into one place, and hinder'd by the top of the Glass from receding, was able to with-

stand

stand the pressure of the outward Air. As we see that if never so little Air remain in the Tube upon the making the Experiment *De Vacuo* with Quick-silver, no inclining of the Tube, (though a long one,) will enable a Man to impell the Mercury up to the very top, by reason (as we formerly noted) of the resistance of the included Air, which will not be compress'd beyond a certain degree.

But in order to a farther Discovery what our bubbles were, we will, on this occasion, inform Your Lordship, that we try'd the XIXth Experiment in one of our small Receivers, and found, that upon the drawing down of the Water, so many bubbles disclos'd themselves, and broke into the upper part of the Tube, that having afterwards let in the external Air, the Water was not thereby impell'd to the top of the Tube (three Foot in length) within a little more then half an Inch. And whether or no it were Air that possess'd that space at the top of the Tube which was not fill'd with Water, we took this course to examine. We drew the second time the Air out of the Receiver, and found, that by reason of the body that possess'd the top of the Tube, we were able not onely to make the Water in the Tube fall to a level with the surface of the Water in the Vessel; But also (by plying the Pump a little longer) a great way beneath it: which, since it could not well be ascrib'd to the bare subsiding of the Water, by reason of its own weight, argued that the Water was depress'd by the Air: which was confirm'd by the Figure of the surface of the Water in the Tube, which was much more concave then that of Water in Tubes of that bigness useth to be. And this farther tryal (to adde that upon the by) we made at the same time, That when the Water in the Pipe was drawn down almost as low as the Water without it, we observ'd, that (though we desisted from pumping) by the bare application of a hand moderately warm to the deserted part of the Tube, the remaining Water would be speedily and notably depress'd. And having for a while held a kindled Coal to the outside of the Tube, (the Pump being still unemploy'd, because the Vessel chanced to hold extraordinarily well) the Air was by the
heat

heat so far expanded, that it quickly drove the Water to the bottom of the Tube, which was diverse Inches beneath the surface of the ambient Water. Whereby it appears (by the same way by which we formerly measur'd the dilatation of the Air) that the Air, even when it is expanded to between 90 and 100 times its extent, will yet readily admit of a much farther rarefaction by heat.

I consider'd also that in case the Bubbles we have been speaking of, were produc'd by the parcels of Air latent in the Water, that Air being now got together to the top of the Tube, though the Air were again drawn out of the Receiver, the taking off its pressure would not disclose bubbles as before; and accordingly, the Air being again pump'd out, the Water in the Tube descended as formerly: but for a great while we scarce saw one bubble appear, onely when the Receiver had been very much exhausted, and the Water was fallen very low, there appear'd near the bottom of the Tube, certain little bubbles, which seem'd to consist of such parcels of Air as had not, by reason of their smalness, got up to the top of the Water, with the more bulkie and vigorous ones. And that which is not inconsiderable, is, That having, by letting in the Air, forc'd up the Water into the Tube, we could not perceive that it ascended near the top, though we permitted the Engine to remain unemploy'd for two or three Nights together, and watch'd whether the Water would swell up and fill the Tube. And on this occasion I remember, that having try'd such an Experiment as this with Quick-silver in stead of Water, in a Tube of about a Foot and a half long, wherein it might seem more hopeful to escape bubbles; yet upon the drawing down the Quick-silver as low as we could, & letting in the external Air upon it, we found that some lurking particles of Air were got up to the top of the Tube, & hinder'd the Quick-silver from being forc'd up again so high. And though the Quick-silver were by this means brought to appear a very close and lovely Metalline Cylinder, not interrupted by interspers'd bubbles as before; yet having caus'd the Air to be again drawn out of the Receiver, I could perceive several little bubbles to disclose themselves, fasten'd

to the inside of the Tube, near the bottom of it; and having purposely watch'd one or two of the chiefest, I had the pleasure to observe, that though they grew bigger and bigger as the surface of the Mercurial Cylinder fell nearer and nearer to them, so as that at length they swell'd into a conspicuous bulk; yet upon the wary letting in the Air upon them, they did not break, but presently shrunk up into a littleness that render'd them inconspicuous.

Whence it seems very probable, if not certain, that even in the closest and most ponderous Liquors, and therefore much more in Water, there may lurk undiscernable parcels of Air, capable, upon the removal of the pressure of the ambient Air (though but in part) and that of the Liquor wherein it lurks, to produce conspicuous bubbles. And consequently, if it seem inconvenient to admit an Elastical power in the Water, it may be said that the swelling of the compress'd Water in the Pewter Vessel lately mention'd, and the springing up of the Water at the hole made by the Needle, were not the effects of any internal *Elater* of the Water, but of the spring of the many little particles of Air dispers'd through that Water, and acting upon it in their sudden recovering themselves to a greater extent, than that to which a violent compression had reduc'd to them.

But though, from all these particulars, it seems manifest that the bubbles we have been all this while treating of, were produc'd by such a substance as may be properly enough call'd Air; yet till we shall have had the opportunity of making some farther tryals concerning the nature of the Air, we shall not resolutely determine whether or no Air be a Primogenial Body (if I may so speak) that cannot now be generated or turn'd either into Water or any other Body. Yet in the mean while (because it is an important Question, and if rightly determin'd, may much conduce to the knowledge of the nature of the Air) we think it not unfit to make a brief mention of some of the particulars which at present occur to our thoughts in favor of either part of the Question.

First then, diverse Naturalists esteem the Air (as well as other Elements) to be ingenerable and incorruptible. And reasons plausible enough may be drawn to countenance this Opinion from the consideration of that permanency that ought to belong to the corporeal Principles of other Bodies.

Next, Experience may be pleaded to the same purpose, for I have read of some who in vain attempted to turn Air into Water, or Water into Air.

The diligent *Schottus* tells us, that amongst other rarities to be met with in that great Repository of them, the *Museum Kercherianum*, there is a round Glass with a tapering Neck near half full (as one may guess by the Scheme he annexeth) of ordinary Spring-water, which having been Hermetically shut up there by *Clavius* the famous Geometrician, the included water is to this day preserv'd, not only clear and pure, as if it were but newly put in: But (as it seems) without (in the least) turning into Air, notwithstanding its having been kept there these fifty years: For he tells us, That the Water hath continued there all this while without any diminution.

Nor doth it appear in those Glasses, which for Chymical Experiments we usually close with *Hermes* his Seal (as they call it) that the included Air doth, during its long Imprisonment, notwithstanding the alteration it receiveth from various degrees of heat, discernably alter its nature. Whereas we plainly perceive in our Digestions and Distillations, that though Water may be rarefied into invisible Vapors, yet it is not really chang'd into Air, but onely divided by heat, and scatter'd into very minute parts, which meeting together in the Alembick or in the Receiver, do presently return into such Water as they constituted before. And we also see, that even Spirit of Wine, and other subtle and fugitive Spirits, though they easily fly into the Air, and mingle with it, do yet in the Glasses of Chymists easily lay aside the disguise of Air, and resume the divested form of Liquors. And so volatile Salts, as of Urine, Harts-horn, &c. though they will readily disperse themselves through the Air, and play up and down in the capacity of an

Alembick or a Receiver: yet will they, after a while, fasten themselves to the insides of such Glasses in the form of Salts.

Besides, since Air is confessedly endow'd with an Elastical power that probably proceeds from its Texture, it appears not, what it is, that in such light alterations of VVater, as are by many presum'd capable of turning it into Air, can be reasonably suppos'd so to contrive the Particles of VVater, as to give them, (and that permanently,) the structure requisite to a Spring. I adde the word Permanently, because the newly mention'd observations seem to argue the Corpuscles of Air to be irreducible into VVater, whereas the Aqueous Particles may perhaps for a while be so vehemently agitated, as to press almost like Springs upon other Bodies; yet upon the ceasing of the agitation, they quickly, by relapsing into VVater, disclose themselves to have been nothing else whilst they counterfeited the Air.

Lastly, The Experiment formerly made in our Engine with a piece of Match, seems to evince, that even those light and subtle Fumes (for the most part not aqueous neither) into which the Fire it self shatters dry Bodies, have no such Spring in them as the Air, since they were unable to hinder or repress the expansion of the Air included in the Bladder they surrounded.

Natural &
Moral Hist.
of the In-
dies. Lib. 3.
C. 9.

Geogr. Ge-
neral. Lib.
3. C. 19.

I remember indeed that the Learned *Josephus Acosta*, in his History of the *West Indies*, tells us, That he saw in those parts some Grates of Iron so rusted and consum'd by the Air, that the Metal being press'd between the Fingers, dissolv'd (to use his words) to powder, as if it had been Hay or parched Straw. And I remember too, that the accurate *Varenius* tells us, That in the Islands commonly called *Azores*, the Air (and Wind) is so sharp, that in a short time it frets not onely Iron Plates, but the very Tiles upon the Roofs of Houses, and reduceth them to dust. And I have elsewhere mention'd some recent Observations of this kind. But it may be said, That the above mention'd Authors ascribe the recited effects chiefly to the Winds, and that, however the corrosion of the Iron and the Tiles may proceed not from the Air it self, or any of its genuine parts, but from some saline Gorpuscles dispersed

dispers'd through the Air, and driven by the Winds against the Bodies it is presum'd to fret. And that such volatile Salts may copiously ascend into the Air, and yet retain their Nature, as doth the more fixt Salt in the Sea Water, the sublimations of *Sal-Armoniack* may sufficiently evince. Not to mention, that I have shown some Friends a secret kinde of saline Substance incomparably subtler then *Sal-Armoniack*, which did not onely easily enough ascend it self, but carried up with it (and that in a very great proportion) the solid and ponderous Body even of uncalcin'd Gold in the form of subtle exhalations, which did afterwards fasten themselves to the upper parts of the Vessels, & yet manifest themselves to continue Gold. We remember also, that to try whether Water could be turn'd into Air, we once took an *Æolipile*, into which we had before convey'd some Water, and placing it upon kindled Coals when the heat forc'd out a vehement streame of aqueous Vapors; we ty'd about the Neck of it, that of a Bladder, which we had before empty'd of Air; and finding the *Æolipile* after a while to blow up the Bladder, we carefully ty'd it again that the included substance might not get away. Then slipping it off from the *Æolipile* we convey'd it into our Receiver, to try whether or no that which in part distended the Bladder would appear by its Spring to be true Air: whereby we found that upon the exsuction of the ambient Air, the included substance expanded it self and the Bladder, to a very much greater bulk then it was of before. And for farther satisfaction, having again taken out the Bladder, we suffer'd it to remain ty'd up till next morning, to try whether time, and the coldness of the night, would make the contain'd substance relapse into Water: But the next Morning we found it little less tumid then before. I remember, I say, that I once made this Experiment; but I might say in answer to it, that the chief reason of my mentioning it, is, To let Your Lordship see, how requisite it is to be circumspect and considerate, when we are to make and to build upon nice Experiments. For though I may seem to have used sufficient caution, yet afterward considering with my self that the *Æolipile* I had imploy'd was a very large one, and

that it required much more care then one that hath not try'd it would imagine, to drive out all the Air from a large *Aolipile*, I easily suspected that the distension of the Bladder in our pneumatical Vessel, might proceed not from the Watery steams that came out at the narrow mouth of the *Aolipile*, and had very much wetted the Bladder, but from the rarefied Air which in that sort of Vessels is wont for a good while together to come out with the rarefied Water: and accordingly having reiterated the Experiment, I found it very difficult (by reason of the shrinking of the Bladders (upon their being heated) and of other impediments) to make it so accurately as to deduce from it, that Water may be rarefied into true Air.

Against the other four above mention'd Considerations, we cannot spend time to frame Objections, but must forthwith proceed to the mention of those things that seem to argue that Air (at least such as produc'd our bubbles) may be generated of Water and other Bodies.

First then we have found by Experience that a vapid Air, or Water rarefied into vapor, may at least for a while emulate the elastical power of that which is generally acknowledg'd to be true Air. For if you take a good *Aolipile*, with a moderately strong and slender Neck, and filling it with Water, lay it upon quick Coals, you may after a while observe so great a pressure by some of the parts contain'd in the *Aolipile* upon others, that the Water will sometimes be thrown up into the Air above three or four Foot high; and if you then take the *Aolipile* almost red hot from off the Fire, you may perceive that the Water will for a longer time then one would easily imagine continue to be spouted out in a violent Stream. And if there remains but little Water in the *Aolipile* when tis taken very hot from the Fire, immersing the Neck of it into cold Water, you will find, that after it begins to suck in some Water, there will be made from time to time store of large bubbles in that Water wherein the Neck was plunged. Which bubbles seem manifestly to proceed from hence, that for a while the heat in the *Aolipile* continues strong enough to rarefie
part

part of the Water that is suck'd in, and expell it in the form of Vapors through the Water incumbent on the Pipe. If also when the *Aolipile* is almost full of water, and therefore can contain but little Air; you hold a Coal or Brand in that stream of Vapors that issues out of the narrow mouth of it, you will find this vapid or rorid Air, (if I may so call it) to blow the Fire very strongly, and with a roaring noise. And that it be not said that tis by the external Air which the aqueous steams drive before them, and not by the Steams themselves, that the Blast is made and the Flame excited; it hath been observ'd, that by approaching the Coal or Brand almost to the mouth of the *Aolipile*, the wind appear'd more vehement, then if the Body to be kindled were held some Inches off.

But in regard the elastical power of the Stream, issuing out of an *Aolipile*, seems manifestly due to the heat that expands and agitates the aqueous Particles whereof that Stream consists, and that such rapid winds seem to be but water scatter'd into little parts and set a moving; since we find, that holding a Knife, or any solid, smooth, and close Body against the stream that issues out of the *Aolipile*, the vapors condensing upon it, will presently cover it with water: It will be very pertinent to subjoyn a notable Experiment that I remember I have met with in the description given us by the Industrious *Kircher*, of several Musical Engines. And (though it may seem somewhat prolix) we will recite what he delivers in his own words, which are these.

Cum eodem tempore quo hac scripsi, summi Pont: Innocentii X^{mi} mandato organi hydraulici in horto Quirinali constituendi cura mihi commendata esset, Aoliam cameram insigni sanè successu construi iussimus, eâ quæ sequitur ratione.

Erat longitudo sive altitudo camera AH 5 Pedum, Latitudine 3 ferè ex lateribus constructa; in medio duo tenebat Diaphragmata CD & EF in modum cribri pluribus foraminibus pertusa. Paulo infra canalis G aquam advehens inferebatur in H eidem epistomium parabat exitum. Aqua itaque per canalem G maximo impetu ruens vehementissimum ventum mox intus excitabat; qui ventus nimia humiditate

*Kirch: Artz
Mag: Con:
& Diffon:
lib. 9.
P. 309.*

See the fifteenth Figure.

humiditate imbutus, ut purior exiret sicciorq̄, Diaphragmata illa in cribri modum pertusa, ordinata sunt. Intra hæc enim aquæ vehemens agitatio rupta fractaq̄, Aerem puriorem per A canalem subtilioremq̄, emittebat: Verum cum postea inventum sit Aerem plus equo humidum interioribus Organi meatibus maximum detrimentum inferre: Hinc, ut Aer aquosus siccissimam consistentiam acquireret, ordinavimus canalem plumbeum Q R in helicem contortum vasi S aliquantulum capaci in modum Urnæ efformato, insertum. Intra Urnam enim plumbeam & canalem tortuosum illis Aer humidus, ita ab omni aquositate defecabatur, ut ex furno in Organum derivatus dici poterit. Urna S canalis tortuosi Q R ultimum orificium Z inseritur anemotheca organi. Et hunc modum organis hydraulicis omnium aptissimum reperi.

Debet autem camera illa situari in loco quantum fieri potest sicciori, ita ut longo canali aqua intra eam derivetur, ne locus humiditate sua Organis officiat.

Thus far the Ingenious Kircherus, whom I the rather cite, because although I have been informed of diverse Ventiducts (as they call them) by very knowing Travellers that have observ'd them: yet this relation of our Author being very punctual, and deliver'd upon his own particular Experience, hath I confesse, made me wish I had had the good fortune when I was at Rome, to take notice of these Organs; or that I had now the opportunity of examining of such an Experiment. For if upon a strict enquiry I should find that the breath that blows the Organs doth not really upon the ceasing of its unusual agitation by little and little relapse into Water, I should strongly suspect that 'tis possible for Water to be easily turn'd into Air. I remember indeed, that we have formerly taught, that there lurks an interspersed Air in the pores of ordinary Water, which may possibly be struck out by the breaking of the Water in its fall into the Æolian Chamber, (as he calls it.) But in regard the Scheme seems to represent that Chamber as closely shut, and thereby forbids us to suppose that any Air is carried into it, but what is latitant in the Water, it will scarce seem probable (to him who remembers how small a proportion

of

of Air, that appear'd to be when its rarefication ceased, which was conceal'd in the Water we freed from bubbles in our Receiver) that so little Air as is commonly dispers'd through Water, should be able, in so little Water as was requisite for so small a room, to make so vehement a Wind as our Author here tels us of. I have sometime therefore suspected, that in this case the Wind may be produc'd by small particles of the water it self, forcibly expell'd out of the Chamber into the Organs. And to the Objection, to which I foresaw this ghes to be liable, namely, That, no heat intervening, there appear'd nothing that should raise the Water into exhalations, and give them an impulse. I thought it might be said, that motion alone, if vehement enough, may, without sensible heat, suffice to break Water into very minute parts, and make them ascend upwards, if they can no where else more easily continue their agitation. For I remember, that Travelling betwixt *Lyons* and *Geneva*, I saw, not very far out of the Way, a place where the River of *Rhone*, coming suddenly to be streighten'd betwixt two Rocks, so near each other, that a Man may (if my Memory fail me not) stand astride upon both at once: that rapid Stream dashing with great impetuosity against its Rocky Boundaries, doth break part of its Water into such minute Corpuscles, and put them into such a motion, that Passengers observe at a good distance off, as it were a Mist arising from that place, and ascending a good way up into the Air. Such, I say, was my suspicion touching the Wind we have been considering, but it seems something odde that aqueous Vapors should, like a dry Wind, pass through so long and tortuous a Pipe of Lead, as that describ'd by our Author, since we see in the Heads of Stills, and the Necks of *Aolipiles*, how quickly such vapors are even by a very little cold recondens'd into Water. But to this also something may be speciously reply'd; wherefore contenting my self to have mention'd our Author's Experiment as a plausible, though not demonstrative proof, that Water may be transmuted into Air. We will pass on to mention in the third place another Experiment, which we try'd in order to the same enquiry.

We took a clear Glass bubble (capable of containing by ghes about three Ounces of Water) with a Neck somewhat long and wide, of a Cylindrical form; this we fill'd with Oyl of Vitriol and fair water, of each almost a like quantity, and casting in half a dozen small Iron Nails, we stopt the mouth of the Glass (which was top full of Liquor) with a flat piece of *Diapalma* provided for the purpose, that, accommodating it self to the surface of the water, the Air might be exquisitely excluded: and speedily inverting the Viol, we put the Neck of it into a small wide mouth'd Glass that stood ready with more of the same Liquor in it, to receive it. As soon as the Neck had reach'd the bottom of the Liquor it was dipp'd into, there appear'd at the upper part (which was before the bottom) of the Viol a bubble, of about the bigness of a Pea, which seem'd rather to consist of small and recent bubbles, produc'd by the action of the dissolving Liquor upon the Iron, then any parcel of the external Air that might be suspected to have got in upon the inversion of the Glass, especially since we gave time to those little Particles of Air which were carried down with the Nails into the Liquor to fly up again. But whence this first bubble was produced, is not so material to our Experiment, in regard it was so small: For soon after we perceiv'd the bubbles produc'd by the action of the *Menstruum* upon the Metal, ascending copiously to the bubble already named, and breaking into it, did soon exceedingly increase it, and by degrees depress the water lower and lower, till at length the substance contain'd in these bubbles possessed the whole cavity of the Glass Viol, and almost of its Neck too, reaching much lower in the Neck then the surface of the ambient Liquor, wherewith the open-mouth'd Glass was by this means almost replenished. And because it might be suspected that the depression of the Liquor might proceed from the agitation whereinto the exhaling and imprison'd steams were put, by that heat which is wont to result from that action of corrosive salts upon Metals, we suffered both the Viol and the open-mouth'd Glass to remain as they were, in a Window, for three or four dayes and nights together; but looking upon them several times during

during that while, as well as at the expiration of it, the whole cavity of the Glass bubble, and most of its Neck, seem'd to be possess'd by Air, since by its spring it was able for so long to hinder the expell'd and ambient Liquor from regaining its former place. And it was remarkable, that just before we took the Glasse bubble out of the other Glass, upon the application of a warme hand to the convex part of the bubble; the Imprison'd substance readily dilated it self like Air, and broke through the Liquor in diverse bubbles, succeeding one another.

Having also another time try'd the like Experiment with a small Viol, and with Nails dissolv'd in *Aqua fortis*, we found nothing incongruous to what we have now deliver'd. And this Circumstance we observ'd, that the newly generated steams did not onely possess almost all the whole cavity of the Glass, but diverse times without the assistance of the heat of my hand, broke away in large bubbles through the ambient Liquor into the open Air: So that these Experiments with corrosive Liquors, seem'd manifestly enough to prove, though not that Air may be generated out of the Water, yet that in general Air may be generated anew.

Lastly, to the foregoing Arguments from Experience we might easily subjoyn the Authority of *Aristotle*, and of (his followers) the Schools who are known to have taught, that Air and Water being Symbolizing Elements (in the quality of moisture) are easily transmutable into one another. But we shall rather to the foregoing Argument adde this, drawn from Reason, That if, as *Leucippus*, *Democritus*, *Epicurus* and others, follow'd by diverse modern Naturalists, have taught, that the difference of Bodies proceeds but from the various Magnitudes, Figures, Motions, and Textures of the small parts they consist of, (all the qualities that make them differ, being deducible from thence) there appears no reason why the minute parts of Water, and other Bodies, may not be so agitated or connected as to deserve the name of Air. For if we allow the *Cartesian Hypothesis*, according to which, as we noted at the beginning of this Letter, the Air may consist of any terrene or aqueous Corpuscles, provided they be kept swimming

in the interfluent Celestial Matter; it is obvious that Air may be as often generated, as Terrestrial Particles, minute enough to be carried up and down, by the Celestial Matter, ascend into the Atmosphere. And if we will have the Air to be a *congeries* of little slender Springs, it seems not impossible, though it be difficult, that the small parts of diverse Bodies may by a lucky concurrence of causes be so connected, as to constitute such little Springs, since (as we note in another Treatise) Water in the Plants it nourisheth is usually contriv'd into Springy Bodies, and even the bare alter'd position and connexion of the parts of a Body may suffice to give it a Spring that it had not before, as may be seen in a thin and flexible Plate of Silver; unto which, by some strokes of a Hammer, you may give a Spring, and by only heating it red hot, you may make it again flexible as before.

These, My Lord, are some of the Considerations at present occurring to my thoughts, by which it may be made probable, that Air may be generated anew. And though it be not impossible to propose Objections against these, as well as against what hath been represented in favour of the contrary Doctrine; yet having already almost tyred my self, and I fear more then almost tyred Your Lordship with so troublesome an Enquiry after the Nature of Bubbles, I shall willingly leave Your Lordship to judge of the Arguments alledged on either side, and I should scarce have ventur'd to entertain You so long concerning such empty things as the Bubbles, which have occasioned all this Discourse, but that I am willing to invite You to take notice with me of the obscurity of things, or the dimness of our created Intellects (which yet of late too many so far presume upon, as either to Deny or Censure the Almighty and Omniscient Creator himself) and to learn hence this Lesson, That there are very many Things in Nature that we disdainfully over-look as obvious or despicable, each of which would exercise our Understandings, if not pose them too, if we would but attentively enough consider it, and not superficially contemplate, but attempt satisfactorily to explicate the nature of it.

EXPERIMENT XXIII.

Since the writing of the twenty first and twenty second Experiments (and notwithstanding all that hath been on their occasion deliver'd concerning bubbles) we made some farther tryals in prosecution of the same inquiry whereto they were designed.

We chose then, amongst those Glasses which Chymists are wont to call Philosophicall Eggs, one that containing about nine Ounces of Water, had a Neck of half an Inch in Diameter at the top, & as we ghest, almost an Inch at the bottom; which breadth we pitched upon for a reason that will by and by appear; then filling it up with common Water to the height of about a Foot and an half, so that the upper part remained empty, we shut it into the Receiver, and watch'd what would follow upon pumping, which proved that a great part of the Air being drawn out, the Bubbles began to discover themselves at the bottom and sides of the Glass; and increasing, as the Air was more and more drawn away, they did from time to time ascend copiously enough to the top of the Water, and there quickly break: but by reason that the wideness of the Glass allow'd them free passage through the Water, they did not appear as in the former Experiments to make it swell: The Water scarce ever rising at all above the mark affixt to its upper surface, when it was put in, and upon the return permitted to the outward Air, and consequently the shrinking in of the remaining bubbles, the Water seem'd to have lost of his first extent, by the avolation of the formerly interspers'd Air.

Being willing likewise to try whether distilled Water were, by having been divided into minute parts, and then re-united, more or less dispos'd to expand it self then Water not distill'd: We took out of our Laboratory some carefully distill'd Rain-water, and put about two Ounces of it into a round Glass bubble, with a very small Neck (not exceeding the sixth part of an Inch in Diameter)

which we filled half way to the top, and then convey'd it into the Receiver; the issue was, that though we drew out more Air then ordinary, yet there appeared not the least intumescence of the Water, nor any ascending bubbles.

But suspecting that either the small quantity of the water or the Figure of the Vessel might have an interest in this odde *Phenomenon*, we took the lately mentioned Philosophical Egge, and another not much differing from it; the former we fill'd up with distill'd Rain-water to the old mark, and into the latter we put a long Cylinder or Rod of solid Glass to streighten the cavity of the Neck by almost filling it up; and then pouring some distilled Water into that also, till it reach'd within some Fingers breadth of the top, the Eggs were let down into the Receiver. In this Experiment the Air was so far drawn forth, before there appeared any bubble in either of the Glasses, that the disparity betwixt this and common water was manifest enough. But at length, when the Air was almost quite pump'd out, the bubbles began to disclose themselves, and to increase as the pressure of the Air in the Receiver decreas'd. But whereas in the first mentioned Philosophical Egge the bubbles were very small, and never able to swell the Water, that we took notice of, at all above the mark: In the other, whose Neck, as we lately said, was straightned, and their passage obstructed, great numbers of them, and bigger, fastned themselves to the lower end of the Glasse rammer (if we may so call it) and gather'd in such numbers between that and the sides of the Neck, that the Water swell'd about a Fingers breadth above the mark, though upon the admitting of the external Air it relaps'd to the former mark, or rather fell somewhat below it. And although thereupon in the first named Vessel all the bubbles presently disappeared, yet in the other we observed, that diverse remained fastned to the lower part of the Glass rammer, and continued there somewhat to our wonder, for above an hour after, but contracted in their Dimensions.

Moreover, having suffered the Glasses to remain above twenty

ty four houres in the Receiver, we afterwards repeated the Experiment, to try what change the exuction of the external Air would produce in the Water, after the internal and latent Air had (as is above recited) in great measure got away in bubbles, and whether or no the Water would by standing re-admit any new particles of Air in the room of those that had forsaken it. But though we exhausted the Receiver very diligently, yet we scarce saw a bubble in either of the Glasses; notwithstanding which, we perceiv'd the Water to rise about the breadth of a Barly-corn, or more, in the Neck of that Glass wherein the solid Cylinder had been put; the Liquor in the other Glass not sensibly swelling.

And lastly, upon the letting in of the Air, the Water in the straightned Neck soon subsided to the mark above which it had swollen, which whether it ought to be ascrib'd to the same small expansion of the parts of the Water it self, or to the rarification of some yet latent Air broken into such small particles, as to escape our observation, seems not easily determinable, without such farther tryals, as would perhaps prove tedious to be recited as well as to be made; though I was content to set down those already mentioned, that it might appear how requisite it is in nice Experiments to consider variety of Circumstances.

EXPERIMENT XXIV.

After having thus discovered what operation the exsuction of the ambient Air had upon Water, we thought good to try also what changes would happen in other Liquors upon the like taking off the pressure of the external Air. We took then a Glass Egge, somewhat bigger then a Turkey Egge, which had a long Neck or Stem of about a $\frac{1}{3}$ part of an Inch in Diameter; and filling it up with Sallet Oyl until it reach'd above half way to the top of the Neck, we inclos'd it in the Receiver together with com-
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mon Water in a resembling Vessel; that we might the better compare together the operation of the exsuction of the Air upon those two Liquors. The Pump being set on work, there began to appear bubbles in the Oyl much sooner then in the Water, and afterwards they also ascended much more copiously in the former Liquor then in the latter: Nay, and when by having quite tired the Pumper, and almost our own patience, we gave over, the bubbles rose almost (if not altogether) in as great numbers as ever, insomuch that none of the various Liquors we tryed either before or since, seemed to abound more with Aerial Particles then did this Oyl. In which it was farther remarkable, that between the time it was set into the Receiver, and that, at which we could get ready to pump, it subsided notably (by ghes about half an Inch) below the mark it reached before it was put in.

After this express'd Oyl, we made tryal of a distill'd one, and for that purpose made choice of the common Oyl or Spirit (for in the Shops where it is sold, the same Liquor is promiscuously called by either name) of Turpentine, because 'twas only of that Chymical Oyl, we had a sufficient quantity: which, being put into a small Glass bubble with a slender Neck, so as to fill it about two Inches from the top, did, upon the evacuating of the Receiver, present us with great store of bubbles, most of which rising from the bottom, expanded themselves exceedingly in their ascent, and made the liquor in the Neck to swell so much by degrees, that at length it diverse times ran over at the top: by which means, we were hindred from being able to discern upon the letting in of the Air, how much the subsidence of the Oyl below the first mark was due to the recess of the bubbles.

Having likewise a mind to try whether, as strong a solution of Salt of Tartar in fair Water as could be made (we having then no Oyl of Tartar *per deliquium* at hand) though it be accounted, Quick-silver excepted, the heaviest of Liquors, would afford us any bubbles; we put in a Glass Egge full of it at the same time, with other Liquors, and found that they did long yield store of bubbles before any discovered themselves in the Liquor of Tartar

tar; and having pursued the Experiment, it appear'd, That of all the Liquors we made tryal of, this afforded the fewest and smallest Bubbles.

Spirit of Vineger being tryed after the same manner, exhibited a moderate number of bubbles, but scarce any thing else worth the mentioning.

Nor could we in red Wine, try'd in a Glass Egge, take notice of any thing very observable. For though upon the exsuction of the Air the bubbles ascended in this Liquor, as it were in Sholes, and shifted places among themselves in their ascent; yet the Intumescence of the whole bulk of the Liquor was scarce at all sensible, the bubbles most commonly breaking very soon after their arrival at the top, where during their stay, they composed a kind of shallow froth, which alone appeared higher in the Neck of the Glass, then was the Wine when it was first let down. Neither yet did Milk, conveyed into our Pneumatical Vessel, present us with any thing memorable, save that (as it seem'd by reason of some unctuoussness of the Liquor) the bubbles not easily breaking at the top, and thrusting up one another, made the intumescence appear much greater then that of common Water.

We likewise conveyed Hens Eggs into the Receiver, but after the exsuction of the Air, took them out whole again. That which invited us to put them in, was, That (as perhaps we mention in other papers) we had among other Experiments of cold, made Eggs burst, by freezing them within doors with Snow and Salt: The Ice, into which the aqueous parts of the Egge were turned by the cold, so distending (probably by reason of the numerous bubbles wont to be observable in Ice) the outward parts of the Egge, that it usually crack'd the Shell, though the inner Membrane that involv'd the several Liquors of the Egge, because it would stretch and yeild, remain'd unbroken. And hereupon we imagin'd that in our Engine it might appear, whether or no there were any considerable Spring, either in any of the Liquors, or in any other more spirituuous substance included in the Egge.

We took also some Spirit of Urine, carelessly enough defleg-

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med, and put it into the same Glass (first carefully scour'd and cleansed) wherein we had put the Oyl-olive above mentioned: We took also another Glass, differing from a Glass Egge, only in that its bottom was flat, and fill'd it up to about $\frac{2}{3}$ of the Neck (which was wider then that of the Egge) with rectified Spirit of Wine.

We took also another Glass Egge, and having fill'd it with common Water till it reached to the middle of the Neck, we pour'd to it of the same Spirit of Wine, till it reached about an Inch higher.

These three Glasses having marks set on them, over against the edges of the contain'd Liquors were put into the Receiver, and that beginning to be evacuated, the bubbles in all the three Liquors began to appear. The mixture of the Spirit of Wine and Water disclos'd a great store of bubbles, especially towards the top; but scarce afforded us any thing worth the remembring. The Spirit of Urine appear'd to swell near an Inch and an half above the mark; and besides that, sent forth store of bubbles, which made a kind of froth at the upper part of it. And above that spume, there appear'd eight or ten great bubbles one above another, in a very decent order, each of them constituting, as it were, a Cylinder of about half an Inch high, and as broad as the internal cavity of the Neck: So that all the upper part of the Neck (for these bubbles reach'd to the top) seem'd to be divided into almost equal parts, by certain Diaphragmes, consisting of the coats of the bubbles, whose edges appeared like so many Rings suspended one above another.

In the Spirit of Wine there did arise a great multitude of bubbles, even till wearinesse did make us give over the Experiment. And in these bubbles two or three things were remarkable; as first, That they ascended with a very notable celerity: Next, That being arrived at the top, they made no stay there: and yet, notwithstanding the great thinness and spirituousness of the Liquor, did, before they broke, lift up the upper surface of it, and for a moment or two form thereof a thin film or skin which appeared protuberant above the rest of the superficies like a small Hemisphere. Thirdly,

Thirdly, That they ascended straight up, whereas those produced at the lower part of the Vessel, containing the mixture of the Water and Spirit of Wine, ascended with a wavering or wrigling motion, whereby they described an indented Line. Lastly, it was observab'e in the Spirit of Wine (and we took notice of the like in the Oyle of Turpentine lately mentioned) that not only the bubbles seem'd to rise from certain determinate places at the bottom of the Glass, but that in their ascension they kept an almost equal distance from each other, and follow'd one another in a certain order, whereby they seem'd part of small Bracelets, consisting of equally little incontiguous Beads: the lower end of each Bracelet, being as it were, fastned to a certain point at the bottom of the Glass.

The Air being sparingly let into the Receiver, the great bubbles formerly mentioned as incumbent upon one another, in that Glass that contained the Spirit of Urine, were by orderly degrees lessened, till at length they wholly subsided. Notwithstanding the recess of so many bubbles as broke on the top of the Spirit of Urine, during all the time of the Experiment; yet it scarcely appear'd at all to be sunk below the mark: Nor did the mixture of Spirit of Wine and Water considerably subside. But that is nothing to what we observ'd in the Spirit of Wine, for not only it conspicuously expanded it self in the Neck of the Vessel that contain'd it; notwithstanding the largeness of it, and that the bubbles were wont to break at the top of it almost as soon as they arriv'd there: But upon the readmission of the external Air, the Spirit of Wine retain'd its newly acquired expansion. And though we let it alone for near an hour together, in expectation that it might subside; yet when we took it out, we found it still swell'd between a quarter and half an Inch above the Mark; and although it was not easily imaginable how this *Phenomenon* could proceed from any mistake in trying the Experiment, yet the strangeness of it invited me to repeat it with fresh Spirit of Wine. Which, swelling in the Neck as formerly, I left all night in the Receiver, allowing free access to the external Air at the Stop-cock, and the next

day found it still expanded as before, save that it seem'd a little lower: which decrement perhaps proceeded from the avolation of some of the fugitive parts of so volatile a Liquor. And for better satisfaction having taken out the Glass, and consider'd it in the open Air, and at a Window, I could not find that there was any remaining bubbles that could occasion the persevering and admir'd expansion.

E X P E R I M E N T X X V .

BEing desirous to discover what difference there might be as to gravity and levity, between Air expanded under Water, and it self before such expansion; we took two very small Viols, such as Chymical Essences (as they call them) are wont to be kept in, and of the size and shape expressed by the 8^h Figure: into one of these we put so much of a certain ponderous Mercurial mixture (happning to be then at hand) that the mouth being stop't with a little soft Wax, the Glass would just sink in Water & no more; this we let fall to the bottom of a wide-mouth'd Chistal Jar, fill'd with about half a pint of common Water, & into the same Vessel we sunk the other Essence Glass unstop'd, with as much Water in it as was more then sufficient to make it subside. Both these sunk with their mouthes downwards, the former being about three quarters full of Air, the latter containing in it a bubble of Air that was guess'd to be of the bigness of half a Pea: This done, the wide-mouth'd Glass was let down into the Receiver, and the way of employing the Engine was carefully made use of.

The success was, That having drawn out a pretty quantity of Air, the bubbles began to disclose themselves in the Water, as in the former Experiments; and though for a good while after the bubbles ascended in swarms from the lower parts of the Water, and hastily broke at the top; yet we prosecuted the Experiment so long without seeing any effect wrought upon the Essence bottles that

that we began to despair of seeing of them rise. But continuing to ply the Pump, that little Glass, whose mouth was open'd, came to the top of the Water, being, as it were, boy'd up thither by a great number of bubbles that had fastned themselves to the sides of it; swimming thus with the mouth downward, we could easily perceive that the internal Air above mention'd had much dilated it self, and thereby seem'd to have contributed to the emerging of the Glass, which remain'd floating, notwithstanding the breaking and vanishing of most of the contiguous bubbles: being hereby encouraged to persist in pumping, we observed with some pleasure, that at each time we turn'd the Key, the Air in the little Glass did manifestly expand it self and thrust out the Water, generally retaining a very protuberant surface where it was contiguous to the remaining Water. And when after divers exsuctions of the Air in the Receiver, that in the Viol so dilated it self as to expel almost all the Water, it turn'd up its mouth towards the surface of the Water in the Jar, and there deliver'd a large bubble, and then relapsed into its former floating posture. And this Experiment taught us, among other things, that it was a work of more time and labour then we imagin'd, to exhaust our Engine as much as it may be exhausted: for although before the emerging of the small Viol, we did (as hath been touch'd already) think we had very considerably emptied the Receiver, because there seem'd to come out but very little or almost no sensible Air at each exsuction into, and out of the Cylinder; yet afterwards, at each drawing down the Sucker, the Air included in the Viol did manifestly dilate it self, so long, that it did no less then nine times turn its mouth upwards, and discharge a bubble by conjecture about the bigness of a Pea, after the manner newly recited. But as for that Violl which had the weight in it, it rose not at all. So that being not able by quick pumping to gain another bubble from the Air in the swimming Glass, which proceeded from some small leak in the Vessel, though it held in this Experiment more stanch then was usual, we thought fit to let in leasurely the Air from without, upon whose

whose admission that within the Violl shrinking into a very narrow compass, the Glafs did, as we expected, fall down to the bottom of the Jar.

But being desirous before we proceed to any new Experiment, to try once more whether the little Glafs, that had the weight in it, might not also be rais'd: After we had suffer'd the Engine to remain clos'd as it was, for five or six hours, the Pump was again ply'd with so much obstinacy, that not onely about the upper part of the Jar there appear'd a good number of bubbles (but very much smaller then those we saw the first time) but afterwards, there came from the bottom of the Jar, bubbles about the bigness of a smal Peas: which the Pump being still kept going, follow'd one another, to the number of forty, coming from the stopp'd Violl; whose mouth, it seems, had not been shut so strongly and closely, but that the included Air, dilating it selfe by its owne spring, made it self some little passage betwixt the Wax and the Glafs, and got away in these bubbles; after which, the unstopp'd Glafs began to float again, the Air shut up in it being manifestly so dilated as to expel a good part of the Water, but not so much as to break quite thorow. And at length, when our expectation of it was almost tired out, the heavier of the two Viols began to come aloft, and immediately to subside again; which appear'd to be occasion'd by the Air within it, whose bulk and spring being weaken'd by the recess of the forty bubbles before-mention'd, it was no longer able, as formerly, to break forcibly through the incumbent Water; but forming a bubble at the mouth of the Glafs, boyed it up towards the top; and there getting away, left it to sink again till the pressure of the Air in the Receiver being farther taken off, the Air in the Violl was permitted to expand it self farther, and to create another bubble, by which it was again for a while carried up. And it was remarkable, that though after having emptyed the Receiver as far as well we could, we ceased from pumping; yet the Vessel continuing more stanch then it was wont, this ascent and fall of the Violl was repeated to the ninth time;

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the included Air, by reason of the smalness of the vent at which it must pass out, being not able to get away otherwise then by little and little; and consequently, in divers such parcels as were able to constitute bubbles, each of them big enough to raise the Viol and keep it aloft until the avolation of that bubble. Whereby it may appear, that the grand rule in *Hydrostaticks*, *That a Body will swim in the Water, in case it be lighter then as much of the Water as equals it in bulk*, will hold likewise when the pressure of the Atmosphere is in very great measure, if not when it is totally taken off from the Liquor and the Body: though it were worth inquiring what it is that so plentifully concurs to fill the bubbles made in our Experiment by the so much expanded Air. For to say with the old Peripatetick Schooles, That the Air in Rarefaction, may acquire a new extent, without the admission of any new substance, would be an account of the *Phenomenon* very much out of date, and which, I suppose, our Modern Naturalists would neither give, nor acquiesce in.

I know not whether it may be requisite to adde, that in this Experiment, as in the former, the outward Air, being let in, did soon precipitate the floating Viol. But I think it will not be amiss to note, that (congruously to what hath been above recorded of the vast expansion of the Air) the Water which in the heavier Viol succeeded in the room of those forty odde, if not fifty great bubbles of Air, which at several times got out of it, amounted but to a very inconsiderable bigness.

EXPERIMENT XXVI.

IT having been observ'd by those that have consider'd what belongs to *Pendulums* (a Speculation that may, in my poor judgment, be highly useful to the Naturalists) that their Vibrations are more slowly made, and that their motion lasts less in a thicker, then in a thinner Medium: We thought it not amiss

to try if a *Pendulum* would swing faster, or continue swinging longer in our Receiver, in case of exsuction of the Air, then otherwise. Wherefore we took a couple of round and polish'd *Pendulums* of Iron or Steel, of equal bigness, as near as we could get the Artificer to make them, and weighing each of them twenty Dragmes, wanting as many Graines. One of these we suspended in the cavity of the Receiver by a very slender silken string, of about seven Inches and a half in length from the cover of the Receiver to which it is fasten'd. Then (by inclining the Engine) we made the *Pendulum* swing too and fro in it, and describ'd as long Arches as in the capacity of so brittle a Vessel we thought safe and convenient. And one of the Assistants telling the recursions of the other *Pendulum* hanging in the free Air, by a string of about the same length, we shortned and lengthned this other *Pendulum*, till it appear'd to keep the same pace in its Vibrations, with that shut up in the Receiver. Then having carefully drawn away the Air, we did again set the *Pendulum* in the Receiver a vibrating; and giving the other *Pendulum* such a motion as made it describe an Arch, according to ones ghes, equal to that of the included *Pendulum*; we reckon'd, one of us, the Recursions of that *Pendulum* which was swinging within the Receiver; and another of us, that which was moving in (that which one would think a much more resisting *medium*) the Air. But once, one of us reckon'd near two an twenty Recursions of the included *Pendulum*, whilst the other reckon'd but twenty of the *Pendulum* that vibrated without. And another time also, the former of these *Pendula* was reckon'd to have made one and twenty Recursions, wherein the other made but twenty: Yet this Experiment seem'd to teach us little, save that the difference betwixt the motion of such a *Pendulum* in the common Air, and in one exceedingly rarefied, is scarce sensible in Vessels no bigger then our Receiver; especially, since though during this Experiment it held very well, yet we could not suppose it to be altogether devoid of Air. We observed also, that when the Receiver was full of Air, the included *Pendulum* continued its Recursions about fifteen minutes (or a quarter

the Receiver. And this Experiment being reiterated in another place, succeeded after the like manner. Which seems to prove, that whether or no the Air be the onely, it is at least, the principal medium of Sounds. And by the way it is very well worth noting, that in a Vessel so well clos'd as our Receiver, so weak a pulse as that of the ballance of a Watch should propagate a motion to the Ear in a Physically straight Line, notwithstanding the interposition of so close a Body as Glas, especially Glas of such thickness as that of our Receiver: since by this it seems that the Air imprison'd in the Glas, must, by the motion of the ballance, be made to beat against the concave part of the Receiver, strongly enough to make its convex part beate upon the contiguous Air, and so propagate the motion to the Listners Ears. I know this cannot but seem strange to those, who, with an eminent Modern Philosopher, will not allow that a Sound, made in the cavity of a Room, or other place so clos'd, that there is no intercourse betwixt the external and internal Air, can be heard by those without, unless the sounding Body do immediately strike against some part of the inclosing Body. But not having now time to handle Controversies, we shall onely annex, That after the foregoing Experiment, we took a Bell of about two Inches in Diameter at the bottom, which was supported in the mid'st of the cavity of the Receiver by a bent stick, which by reason of its Spring press'd with its two ends against the opposite parts of the inside of the Vessel: in which, when it was clos'd up, we observed that the Bell seemed to sound more dead then it did when just before it sounded in the open Air. And yet, when afterwards we had (as formerly) emptyed the Receiver, we could not discern any considerable change (for some said they observ'd a small one) in the loudness of the sound. Whereby it seemed that though the Air be the principal medium of sound, yet either a more subtile matter may be also a medium of it, or else an ambient Body that contains but very few particles of Air, in comparison of those it is easily capable of, is sufficient for that purpose. And this, among other things, invited us to

consider, whether in the above-mentioned Experiment made with the Bell and the Load-stone, there might not in the deserted part of the Tube remain Air enough to produce a sound: since the Tubes for the Experiment *De Vacuo* (not to mention the usual thinness of the Glafs) being seldom made greater then is requisite, a little Air might beare a not inconsiderable proportion to the deserted space: And that also, in the Experiment *De Vacuo*, as it is wont to be made, there is generally some little Air that gets in from without; or at least store of bubbles that arise from the Body of the Quick-silver, or other Liquor it self, Observations heedfully made have frequently informed us: And it may also appear, by what hath been formerly delivered concerning the *Torricellian* Experiment.

On the occasion of this Experiment concerning sounds, we may adde in this place, That when we tryed the Experiment formerly mentioned, of firing Gun powder with a Pistol in our evacuated Receiver, the noise made by the striking of the Flint against the Steel, was exceeding languid in comparison of what it would have been in the open Air. And on divers other occasions it appeared that the sounds created within our exhausted Glafs, if they were not lost before they reach'd the Ear, seem'd at least to arrive there very much weaken'd. We intended to try whether or no the Wire-string of an Instrument shut up into our Receiver, would, when the ambient Air was suck'd out, at all tremble, if in another Instrument held close to it, but without the Receiver, a string tun'd (as Musicians speak, how properly I now examine not) to an Unison with it, were briskly toucht, and set a Vibrating. This, I say, we purpos'd to try to see how the motion made in the Air without, would be propagated through the cavity of our evacuated Receiver. But when the Instrument wherewith the tryal was to be made came to be employ'd, it prov'd too big to go into the Pneumaticall Vessel: and we have not now the conveniency to have a fitter made.

We thought likewise to convey into the Receiver a long and
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ter of an hour) before it left off swinging; and that after the extraction of the Air, the Vibration of the same *Pendulum* (being fresh put into motion) appear'd not (by a minutes Watch) to last sensibly longer. So that the event of this Experiment being other then we expected, scarce afforded us any other satisfaction, then that of our not having omitted to try it. And whether, in case the tryal be made with a *Pendulum* much less disproportionate to the Air then Steel is, the event will much better answer expectation, experience may be consulted.

EXPERIMENT XXVII.

THat the Air is the medium whereby sounds are convey'd to the Ear, hath been for many Ages, and is yet the common Doctrine of the Schools. But this Received Opinion hath been of late oppos'd by some Philosophers upon the account of an Experiment made by the Industrious *Kircher*, and other Learned Men: who have (as they assure us) observ'd, That if a Bell, with a Steel Clapper, be so fasten'd to the inside of a Tube, that upon the making the Experiment *De Vacuo* with that Tube, the Bell remain'd suspended in the deserted space at the upper end of the Tube: And if also a vigorous Load-stone be apply'd on the outside of the Tube to the Bell, it will attract the Clapper, which upon the Removal of the Load-stone falling back, will strike against the opposite side of the Bell, and thereby produce a very audible sound; Whence divers have concluded, That 'tis not the Air, but some more subtle Body that is the medium of sounds. But because we conceiv'd that, to invalidate such a consequence from this ingenious Experiment (though the most luciferous, that could well be made without some such Engine as ours) some things might be speciously enough alleadg'd; we thought fit to make a tryal or two, in order to the Discovery of what the Air doth in conveying of sounds, reserving divers other Experiments tryable in our Engine concerning sounds, till we can obtain more

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leasure to prosecute them. Conceiving it then the best way to make our tryal with such a noise as might not be loud enough to make it difficult to discern slighter variations in it; but rather might be, both lasting, (that we might take notice by what degrees it decreas'd;) and so small, that it could not grow much weaker without becoming imperceptible. We took a Watch, whose Case we open'd, that the contain'd Air might have free egress into that of the Receiver. And this Watch was suspended in the cavity of the Vessel onely by a Pack-thread, as the unlikelyest thing to convey a sound to the top of the Receiver: And then closing up the Vessel with melted Plaister, we listen'd near the sides of it, and plainly enough heard the noise made by the ballance. Those also of us, that watch'd for that Circumstance, observ'd, that the noise seem'd to come directly in a straight Line from the Watch unto the Ear. And it was observable to this purpose, that we found a manifest disparity of noise, by holding our Ears near the sides of the Receiver, and near the Cover of it: which difference seem'd to proceed from that of the Texture of the Glass, from the structure of the Cover (and the Cement) through which the sound was propagated from the Watch to the Ear. But let us prosecute our Experiment. The Pump after this being imploy'd, it seem'd that from time to time the sound grew fainter and fainter; so that when the Receiver was empty'd as much as it us'd to be for the foregoing Experiments, neither we, nor some strangers that chanc'd to be then in the room, could, by applying our Ears to the very sides, hear any noise from within; though we could easily perceive that by the moving of the hand which mark'd the second minutes, and by that of the ballance, that the Watch neither stood stil, nor remarkably varied from its wonted motion. And to satisfie our selves farther that it was indeed the absence of the Air about the Watch that hinder'd us from hearing it, we let in the external Air at the Stop-cock, and then, though we turn'd the Key and stopt the Valve, yet we could plainly hear the noise made by the ballance, though we held our Ears sometimes at two Foot distance from the outside of the

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we argued, from this, That whereas when the Viol was put in the time before, the Water remained all the while perfectly free from bubbles; at this time the bottom of the Glass appear'd all covered with them, and they, upon the egress of the excluded Air into the Receiver, did presently flag and shrink up.

From these tryals it seem'd deducible enough, that whil'st the Viol continued to be well stop't, the included Water did, from the Air, shut up with it, sustaine a pressure equal to that of the Atmosphere; since till the Air could get out of the Glass, there appeared no bubbles in the Water, notwithstanding the want of pressure in the ambient Body.

But to be sure to reach the chief end of our Experiment, we made use of this other expedient: We caused a convenient quantity of Water to be put, and Hermetically shut up into a Glass Egge, to whose long Neck (which was purposely made of an unequal thickness) was fastened to one end of a string, whose other end was tyed to the Cover of our Receiver, after the manner elsewhere mentioned already: Then the Egge being conveyed into the Pneumatical Vessel, and that being evacuated, we did, by turning the brass Stopple formerly describ'd amongst the parts of our Engine, so shorten the string as to breake the Glass; whereby liberty being given to the Air imprisoned in the Egge, to pass into the cavity of the Receiver, the sudden recess of the Air made the bubbles in a trice appear so numerous, and ascend so swiftly in the Water, that their motion look'd like that of a violent shower of Rain; save that the bubbles did not, like the drops of Rain, tend downwards, but upwards. Which made me resemble this *Phænomenon* to what I have seen happen in the dissolution of Seed-Pearl in some acid *Menstruum*, in which, if a good quantity of the little Pearls be cast whole, they will at first, if the *Menstruum* be sharp enough, be carried in swarms from the bottom to the top of the Liquor. We will adde, that without sealing up the Glass, this Experiment may be tryed in one of our smallest Receivers. For there the extraction of the ambient Air may be performed so nimbly, that

that immediately the bubbles lurking in the Water are allowed to display themselves, and ascend in throngs; insomuch, as having in such a Receiver try'd the Experiment with Wine (as a more spirituous Liquor) instead of Water, the Red-Wine appeared all cover'd, with a copious, but vanishing white Froth, almost as if a Vessel full of bottl'd drink had been unwarily opened.

EXPERIMENT XXIX.

IT may not a little conduce to the clearer explication of divers Points in the Doctrine of Meteors, and perhaps of some other Physiologicall difficulties, to discover what the Air doth to the motion of those Steams or Exhalations that ascend into it, namely, Whether they mount upwards by vertue of any such *positive levity* (as some Peripateticks speak) acquir'd together with their Aërial nature, as inables them to pierce through part of the Atmosphere, and over-come its resistance. Or else, whether these steams being once raised above the Earth by their agitation, have their ascent and sustentation aloft, rather promoted then hindred by the Air: as the inferior parts of that, being thicker and heavier then the superior, the steams can more easily continue for a while their agitation upwards then downwards; And afterwards are by the same fluidity and thickness of the Air, carried to and fro in it, and kept from relapsing to the Earth: as in the Sea-Water the saline parts are kept from subsiding by those aqueous ones wherewith they are associated.

We hop'd to illustrate this matter, by observing the motion of the smoke, proceeding from kindled or flaming Bodies in our exhausted Receiver. But as we formerly noted, upon the extraction of the Air, the smoking of those Bodies presently ceas'd. We had thoughts also of conveying into our Pneumatical Glass a hot Iron, with some Body easie to be dissipated into smoke set upon it, but consider'd, that neither was that way free from inconveniencies; especially this, that the hot Body would make the Imprisoned Air circulate within the Receiver, and

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slender pair of Bellows, made after the fashion of those usually employ'd to blow Organs, and furnished with a small Musical instead of an ordinary Pipe. For we hop'd, that by meanes of a string fastned to the upper part of the Bellows, and to the moveable stopple that makes a part of the Cover of our Receiver, we should, by frequently turning round that stopple, and the annexed string, after the manner already often recited, be able to lift up and distend the Bellows; and by the help of a competent weight fastned to the same upper part of the Bellows, we should likewise be able, at pleasure, to compress them: and by consequence, try whether that subtler matter then Air (which, according to those that deny a *Vacuum*, must be suppos'd to fill the exhausted Receiver) would be able to produce a sound in the Musical Pipe; or in a Pipe like that of ordinary Bellows, to beget a Wind capable to turne or set on moving some very light matter, either shap'd like the Sails of a Wind-Mill, or of some other convenient form, and expos'd to its Orifice. This Experiment, I say, we thought to make, but have not yet actually made it for want of an Artificer to make us such a pair of Bellows as it requires.

We had thoughts also of trying whether or no, as Sounds made by the Bodies in our Receiver become much more languid then ordinary, by reason of the want of Air: so they would grow stronger, in case there were an unusual quantity of Air crouded and shut up in the same Vessel. Which may be done (though not without some difficulty) by the help of the Pump, provided the Cover and Stopples be so firmly fasten'd (by binding and Cement, or otherwise) to the Glais and to each other, that there be no danger of the condensed Airs blowing of either of them away, or its breaking through the junctures. These thoughts, My Lord, as I was saying, we entertain'd; but for want of leasure, as of as good Receivers as ours, to substitute in its place, in case we should break it before we learn'd the skill of condensing the Air in it, we durst not put them in practice: Yet, on this occasion, give me leave to advertise Your Lordship once for all,
That

That though for the reasons newly intimated, we have, Only in the seventeenth Experiment, taken notice, that by the help of our Engine the Air may be condens'd as well as rarified; yet there are divers other of our Experiments, whose *Phænomena* it were worth while to try to vary, by means of the compression of the Air.

EXPERIMENT XXVIII.

WE taught, among divers other things, when we discoursed of our first Experiment, That the Air shut up in our Receiver, presseth as strongly upon the Bodies shut up with it, as if they were expos'd to the pressure of the whole Atmosphere. That this was not inconsiderately propounded, we hope Your Lordship hath gather'd from divers of the things already recited: But yet perhaps it will not be amiss to subjoyn, by way of farther confirmation of the same truth, the following Experiment: which would have accompanied the 20th, but the Paper wherein the one was written chanced not to be at hand, when the other was sent away.

We convey'd into the Receiver a new Glass Viol, capable of holding about 6 or 7 ounces of Water, into which we had before put 2 or 3 Spoon-fulls of that Liquor, and stopt it close with a fit Cork. The Pneumatical Vessel being emptyed, there appear'd not any change in the inclos'd Water, the Air imprisoned with it, not having the force to blow out the stopple. Which event, though it were no other then we expected, was differing from what we desir'd. For we would gladly have seen what change would have appeared in the Water upon the Bottles being suddenly unstopp'd, in a place where the ambient Body was so differing from our common Air. Wherefore we did again put in the Viol, but less strongly clos'd then formerly, though as strongly stopt as seem'd requisite on ordinary occasions: But when the Air was pumped out of the Receiver, that within the Viol did quickly, as we expected, find or make it selfe little passages to get out at: as

we

consequently make it questionable whether the ascent of the steams would not be due to the new and acquired motion of the Air.

Wherefore I bethought my self of another way to satisfy in some measure my curiosity, to wit, by means of a certain Liquor, which I call'd to minde that some years agoe I had (for a design that belongs not to our present purpose) prepar'd; which, I suppose, I shew'd Your Lordship, and which had the luck to be taken notice of by divers very Ingenious and Famous Men. For this Liquor, though most of its Ingredients be Metals, and all of them ponderous enough, is yet of that nature, that whil'st the Viol wherein it is kept is stop't (how slight a Cover soever) both the Liquor and the Glasse are transparent; and so is that upper half of the Glasse to which the Liquor reacheth not: But as soon as ever the stopple is taken out, and full access is given to the external Air, both the inward part of the Cork, and the Liquor it self, do presently send upwards, and scatter abroad a fume as thick and white, as if there were a quantity of Alabaster-dust thrown up into the Air: And this smoking of the Liquor lasts till my unwillingness to waste it invites me to stop it again, and then the ascension of the fumes suddenly ceaseth, till the Viol be again unstop'd.

This fuming Liquor then I thought would much conduce to the discovery I desir'd to make, since it sav'd me the need of conveying any hot Body with it into the Receiver, and would not darken it with fumes before the time. Wherefore having ty'd to the Viol a great weight of Lead, to keep it from being lifted up by the drawing out of the Cork; and having ty'd to the stopple one end of a string, of which the other end was made fast to the Cover of the Pneumatical Glasse, the Liquor was carefully clos'd up after the wonted manner; then the Air being diligently pump'd out, the Viol was unstop'd in the empty'd Receiver. And though immediately, upon the drawing out of the Cork, there appear'd to be as it were thrown up some white fumes, which seem'd to

proceed from the Air before imprison'd in the Viol, and diffusing it self suddenly into the capacity of the Receiver: Yet we afterward observ'd, as we expected, That the fumes did not mount and disperse themselves as they use to do in the open Air; but that, when by reason of the agitation of the Corpuscles of the Liquor, which could not continue their motion in so narrow a space as the Viol afforded them, and were therefore reduc'd to thrust one another out of it; when, I say, by these assistances the fumes were ascended to the lip of the Viol, they mounted no higher, but ran down along the out-side of the Viol to the bottom of it; and thence along, a long and inclining piece of Lead, on which the Viol rested, like a little Stream (not very much bigger then a Swans Quill) whose nature they seem'd to emulate so well, that it quitted not the Viol till it was come to the bottom of it, and then forsook it in such a manner as a stream of Water of the same bigness would have done. And this stream lasted a pretty while, and would probably have lasted longer, but that being loath to waste my Liquor, I let in at the Stop-cock a pretty deal of the external Air; notwithstanding which, finding after a while that the stream did run afresh, though, as it seem'd not altogether so copious as before; I let as much more Air, as would, come in: and found (somewhat to my wonder) that though the stream formerly mention'd dis-appear'd, yet there appear'd not any white fumes to arise, either from the Cork, or out of the Viol it self, no not when the Cover was remov'd from the Receiver; though not onely after a while there ascended white fumes from the Receiver: but having forthwith taken out the Viol into the open Air, it emitted white exhalations as before; and having presently after unstopp'd it in an open Window, we found both it and the Cork immediatly to send forth a yet much more plentiful smoak: Though it be now divers years since this Numerical Liquor was prepared, after the manner mention'd either by *Carneades* or *Eleutherius* (for I do not well remember which) in those Dialogues concerning Heat and Flame that have above been mention'd.

More

• More Circumstances concerning these Fumes we might have observ'd, had we not been deterr'd by an Indisposition in point of health, from having much to do with steams of so dangerous a nature, as by that of the Ingredients of this Liquor these seem likely to be of.

The Reflections that may be made upon this Experiment, we have not now the leasure to prosecute, and therefore shall content our selves to recommend the several Circumstances of it to Your Lordships serious consideration; and to take notice (*en passant*) that steams in an ambient Body, or a medium thinner then themselves, may both tend downwards, and otherwise emulate the nature of a Liquor; which I therefore point at, that it may appear the less strange, if we sometimes speak of the Atmosphere as of a kinde of Liquor, in comparison of that more thin and subtle Celestial Matter that surrounds it.

And though it might perchance suffice to have on this occasion intimated thus much; yet, lest this way of speaking of the Atmosphere should be thought too bold and extravagant, I am content to borrow an Experiment of the Discourse formerly mention'd (touching fluidity and firmness) and subjoyn it here with alterations suitable to the contrivance of our Engine; and this the rather, because I hope it may conduce to the discovery of the nature of the Atmosphere: for which reason it might have been annex't to what hath been noted either upon the first, or eighteenth Experiment, but that when they were written and sent away, it came not into my minde. The Experiment then as we try'd in our Engine, was as follows.

E X P E R I M E N T X X X .

VVE took one of the small Receivers, often mention'd already, and into it we convey'd a piece of well lighted Match; and letting it remain there till it had fill'd the Receiver with smoak, we took it out & hastily clos'd again the Receiver, that the smoak might not get away. Then staying awhile to let these

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fumes

fumes leisurely subside, we found, as we expected, that after some time they settled themselves in the lower half of the Receiver, in a darkish Body, leaving the upper half of the Receiver transparent, and as to sight, full of nought but clear Air. Now to manifest that this smoak thus settled emulated a Liquor, we inclin'd the Engine that contain'd it, sometimes to one side, and sometimes to the other; and observ'd the smoak to keep its surface almost Horizontal, notwithstanding the stooping of the Vessel that held it, as Water, or another Liquor would in the like case have done. And if by a quicker rocking of the Engine the smoak were more swiftly shaken, it would, like Water, either vibrate too and fro from one side to the other of the Glais, or else have its surface manifestly curl'd with Waves, but preserve its self in an entire and distinct Body from the incumbent Air; and being permitted to rest a while, would soon recover its former smooth and level *superficies*. If also the Key were turn'd and the Valve unstopp'd, so that there was a free, though but a narrow passage open'd betwixt the external Air and the cavity of the Receiver, then would some of this smoak fall down, as it were, in a stream into the subjacent Cylinder, and a proportionate quantity of the outward Air, would manifestly ascend through it into the incumbent Air, much after the same manner as if you invert a Viol with a long Neck, and well fill'd with Red-Wine, into a Glais full of fair Water, you shall see the Water and Wine by degrees mingle with one another; the one falling down as it were in little colour'd streams, and the other ascending in its room in the like curled streams, sometimes preceded by round parcels of Water, which, by reason of their transparency, look almost like bubbles. The other circumstances of this Experiment, belonging not all of them to our present purpose, we shall content our selves with taking notice of one which seems the most important, and may illustrate and confirm some things formerly delivered. And it was, That if, when the *superficies* of our smoak lay smooth and Horizontal, a hot Iron were held near the outside of the Receiver, the Neighboring part of the included

fumes

fumes (for the rest did not very much alter their former *superficies*) being rarified by the heat, would readily ascend in a large Pillar of smoak to the very top of the Receiver, yet without seeming to loose a distinct *superficies*, or to be confounded with Air; below which, upon the recess of the adventitious heat that by agitating it impell'd it upward, it would again subside.

All which being aeded to the late Experiment of the smoking Liquor, and to what may be from that which hath been elsewhere said, gather'd to the same purpose, will, I hope, keep it at least from appearing absur'd, If, since we see that there is so great an inequality in the density and weight of Liquors, that Water is neer fourteen times thinner or lighter then Quick-silver of the same bulk; and well dephlegm'd Spirit of Wine, yet much lighter then Water, we venter to speak sometimes of the Atmosphere, as if it were a peculiar kinde of thin and halitious Liquor (if I may so call it) much lighter then Spirit of Wine.

To these things I know not whether it will be requisite to adde, That as we lately took notice of conspicuous Waves that appear'd upon the *superficies* of our agitated smoak: So some such thing may not absurdly be conjectur'd to happen on the *superficies* of the Atmosphere, by those strange ruggednesses that appear (especially in the Spring and Fall, when Exhalations and Vapors are wont to ascend most plentifully) upon the limb or edge of the Rising and Setting Sun. I speak thus diffidently upon this occasion, because I know, that by the Fluctuation or Boyling of the Suns own *superficies*, divers eminent Mathematicians have plausibly enough (but how truly, I leave Your Lordship to judge) endeavor'd to give an account of it. But if we will joyn with those that have ascrib'd of late this *Phanomenon*, to the refraction of the Sun-beams suffer in our vapid air, we may, as hath been intimated, promote their Doctrine, by deducing from it, That probably the surface of the Atmosphere is oftentimes (if not always) exceedingly curll'd or wav'd. And certainly it is somewhat wonderful, as well as very pleasant to behold, how, to him that looks upon the setting

ting Sun through a long and excellent Telescope, there will not onely appear strange inequalities in the edge of it (inſomuch that I have often ſeen it more indented then a Saw) but thoſe inequalities will vaniſh in one place, and preſently appear in another, and ſeem perfectly to move like Waves ſucceeding and deſtroying one another, ſave that their motion oftentimes ſeems to be quick-eſt; as if in that vaſt Sea they were carryed on by a Current, or at leaſt by a Tide. And this (as we elſewhere note) appears to the Eye, not onely when it looks directly through the Telescope upon the Sun; but alſo when a large and well defin'd Image of the Sun is by the ſame Telescope brought into a Room and caſt upon a ſheet of white Paper. But to inſiſt on this were to digreſs: and therefore I will proceed to Experiments of another kinde.

E X P E R I M E N T X X X I .

IT hath been admir'd by very ingenious Men, that if the exquisitely poliſh'd ſurfaces of two flat pieces of Marble, be ſo congruous to each other, that from their mutual application there will reſult 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. A probable cauſe of this ſo cloſe adheſion, we have elſewhere endeavored to deduce from the unequal preſſure of the Air upon the undermoſt ſtone; For the lower *ſuperficies* of that ſtone being freely expoſ'd to the Air, is preſſ'd upon by it, whereas the uppermoſt ſurface, being contiguous to the ſuperior ſtone, is thereby defended 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 declar'd. Upon theſe grounds we conjectur'd, that in caſe we could procure two Marbles exactly ground to one another, and in caſe we could alſo ſufficiently evacuate our Receiver; the lower ſtone would, for want of the wonted and ſuſtaining preſſure of the Air, fall from the upper.

But

But the farther tryal of this Experiment we must, unless Your Lordship think it worth Your making at *Paris*, put off till a fitter opportunity. For where we now are, we cannot procure Marbles so exactly ground, that they will sustain one another in the Air above a minute or two, which is a much shorter time then the emptying of our Receiver requires. We did indeed try to make our Marbles stick close together, by moistening their polish'd surfaces with rectifi'd Spirit of Wine, in regard that Liquor, by its sudden avolation from Marble, if powr'd thereon, without leaving it moist or less smooth, seem'd unable to sustain them together after the manner of a glutinous Body, and yet seem'd sufficient to exclude and keep out the Air. But this we try'd to little purpose, for having convey'd into the Receiver two black square Marbles (the one of two Inches and a third in length or breadth, and somewhat more then half an Inch in thickness: The other of the same extent, but not much above half so thick) fasten'd together by the intervention of pure Spirit of Wine; and having suspended the thicker by a string from the cover, we found not that the exsuction of the ambient Air would separate them, though a weight amounting to four Ounces were fasten'd to the lowermost Marble to facilitate its falling off.

I would gladly have the Experiment try'd with Marble, so well polish'd, as to need no Liquor whatsoever to make them cohere, and in a Vessel out of which the Air may be more perfectly drawn then it was out of ours. But in the mean time, though we will not determine whether the spirit of Wine did contribute to the strong cohesion of these stones, otherwise then by keeping ev'n the subtlest parts of the Air from getting in between them: yet it seem'd that the not falling down of the lowermost Marble, might, without improbability, be ascrib'd to the pressure of the Air remaining in the Receiver; which, as we formerly noted, having been able to keep a Cylinder of Water, of above a Foot in height, from falling to the bottom of the Tube, may well enough be suppos'd capable of keeping so broad a flat Marble from descending. And
 though

though this may seem a strange proof of the strength of the Spring of the Air, ev'n when rarefied, yet it will scarce seem incredible to him that hath observ'd how exceeding strong a cohesion may be made betwixt broad Bodys, onely by their immediate touching one another. A notable instance of which, I have met with in this short Narrative of the Learned *Zucchius*. *Fuveni* (saith he) *lacertorum suorum robur jactanti proposita semel est lamina area, per ansam in medio extantem apprehensam elevanda è tabula marmorea, cui optime congruebat: qui primo tanquam rem ludicram puero committendam contempsit: tum instantibus amicis manum utramque admo- vens, cum luctatus diu harentem non removisset, excusavit impotentiam, objecta peregrini & potentissimi glutinis interpositione, quo fortissime copulante nequiret divelli; donec vidit ab alio per tabulam facilimè laminam deduci, & ad extrema productam, & actam in transversum inde deportari.* But that we may learn from our own Engine, that two Bodies, though they touch each other but in a small part of their surfaces, may be made to cohere very strongly, onely by this, That the Air presses much more forcibly upon the inferior superficies of the lowermost Body, then upon the upper surface of the same: We will hereunto annex the following Experiment, though out of the order wherein they were made.

EXPERIMENT XXXII.

I Remember I have, in a Discourse touching Fluidity and Firmness, made mention of my having, by the exsuction of the Air out of a Glais Vessel, made that Vessel take up, or suck up (to speak in the common Language) a Body weighing divers Ounces; but our Engine affording us the opportunity of making considerabler Experiments of that kinde, We thought fit to make a further tryal of the force of the Atmosphere's pressure upwards, after the following manner.

The Receiver having been exquisitely clos'd, as we have often taught already, and the Air being in a good measure drawn out of it, it was remov'd from off the Pump: and to the lower
Branch

P. Nic.
Zucchius
apud Schot:
part 1.
Mec: Hy-
draulo.
pneum.

Branch of the Stop-cock, there was speedily apply'd a tapering Valve of brass, such as is described in the 9^h fig: made fit to go with its narrower end into the cavity of the branch, and to fill the Orifice of that cavity with its broader part. And that the Air might not get in at the little intervals, left here and there between the convex surface of the stopple and the internall edge of the branch, those intervals were stop't with a little Diachylon. And to the doore, or, (if you please) that part of the Valve which was to move to and fro, and in this Experiment hung perpendicular to the Horizon, there was, at a button of brass belonging to the Valve, fasten'd a broad scale wherein weights were to be put. This done the key of the Stop-cock was turn'd, and the externall Air beating like a forcible streame upon the Valve to get in there, it did suddenly both shut the Valve and keep it shut so strongly, that we had time to cast in diverse weights one after another into the Scale; til at length the weight overpowering the pressure of the Atmosphere, drew downe the Valve by the strings that tyed the Scale to it, and gave liberty to the outward Air to rush into the Receiver. Though another time, when the Valve had but little weight hanging at it, being, by I know not what accident, drawn down beneath its former place, it was by the impetuous current of the outward Air suddenly impell'd up into it again, and kept there. But in the former Experiment it is remarkable, That though the Receiver were not well exhausted, and though it leak'd whil'st the rest of the Experiment was in prosecution, and though the Valve whereon the Cylinder of the Atmosphere could press, were not above an Inch and a half in Diameter, yet the weight kept up by suction, or rather supported by the Air, (namely the Valve, the Scale and what was cast out of it,) being sent to be weigh'd, amounted to about ten of our common Pounds, consisting of sixteen Ounces a piece: So that we doubted not but that, had the Experiment been made with favorable Circumstances, the Air endeavoring to press in at the Orifice of the Stop-cock, would have kept a very much greater weight from falling out of it; I say the Air, because we

found, by tryal purposely made, that neither the imperfect contact of the Valve and the Stop-cock, nor the Diachylon that was employ'd to fill up the little Crannies left betwixt them, were considerable in this Experiment. By which it may, among other things, appear, that I did not without cause in the above-named Discourse touching Fluidity and Firmness, ascribe a great force, even to such Pillars of Air as may be supposed to begin at the top of the Atmosphere, and recoyling from the ground, to terminate on the Bodies on which they press: since in the present Experiment such a weight was supported by so slender a Cylinder of Air, rebounding from the Earth to the Valve whereon it did beare.

EXPERIMENT XXXIII.

BUT in regard we have not yet been able to empty so great a Vessel as our Receiver, so well as we can the Cylinder it selfe; our Pump alone may afford us a nobler instance of the force of the Air we live in, insomuch, that by help of this part of our Engine, we may give a pretty near guess at the strength of the Atmosphere, computed as a weight. And the way may be this; First, the Sucker being brought to move easily up and down the Cylinder, is to be impelled to the top of it: Then the Receiver must be taken off from the Pump, that the upper Orifice of the Cylinder remaining open, the Air may freely succeed the Sucker, and therefore readily yield to its motion downwards. This done, there must be fastn'd to one of the Iron Teeth of the Sucker, such a weight as may just suffice to draw it to the bottom of the Cylinder. And having thus examin'd what weight is necessary to draw down the Sucker, when the Atmosphere makes no other then the ordinary resistance of the Air against its descent; the Sucker must be again forc'd to the top of the Cylinder, whose upper Orifice must now be exactly closed; and then (the first weight remaining) we easily may, by hanging a Scale to the above mention'd Iron (that makes part of the Sucker) cast in known weights so long, till in spite of the reluctancy of the Atmosphere the Sucker be drawn

drawn down. For to these weights in the Scale, that of the Scale it self being added, the sum will give us the weight of a Column of Air, equal in Diameter to the Sucker, or to the cavity of the Cylinder, and in length to the height of the Atmosphere.

According to this method we did, since the writing of the last Experiment, attempt to measure the pressure of the Atmosphere, but found it more difficult then we expected, to performe it with any accurateness; for though by the help of the *Manubrium* the Sucker moved up and down with so much ease, that one would have thought that both its convex surface, and the concave one of the Cylinder were exquisitely smooth, and as it were slippery; yet when the Sucker came to be moved onely with a dead weight or pressure (that was not (like the force of him that pumped) intended as occasion required) we found that the little rufnesses or other inequalities, and perhaps too, the unequal pressure of the Leather against the cavity of the Cylinder, were able, now and then, to put a stop to the descent or ascent of the Sucker, though a very little external help would easily surmount that impediment; and then the Sucker would, for a while, continue its formerly interrupted motion, though that assistance were withdrawn. But this discouragement did not deterre us from prosecuting our Experiment, and endeavoring, by a careful trial, to make it as instructive as we could. We found then that a Leaden Weight, of 28 pounds (each consisting of sixteen Ounces) being fastned to one of the teeth of the Sucker, drew it downe closely enough, when the upper Orifice of the Cylinder was left open: though by the help of Oyl and Water, and by the frequent moving the Sucker up and down with the *Manubrium*, its motion in the Cylinder had been before purposely facilitated. This done, the upper Orifice of the Cylinder was very carefully and closely stopped, the Valve being likewise shut with its wonted Stopple well oyl'd, after the Sucker had been again impell'd up to the top of the Cylinder. Then to the precedent twenty eight pound, we added a hundred and twelve pounds more; which forcing down

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the

the Sucker, though but leifurely, we took off the twenty eight pound weight; and being unable to procure juft fuch weights as we would have had, we hung on, inftead of it, one of fourteen pound: but found that, with the reft, unable to carry down the Sucker. And to fatisfie our felves, and the Spectators, that it was the refiftance of the ambient Air that hinder'd the defcent of fo great a weight, after that we had try'd that upon unftopping the Valve, and thereby opening an access to the external Air, the Sucker would be immediately drawn down. After this, I fay, we made this farther Experiment, That having by a Man's ftrength forcibly depreff'd the Sucker to the bottom of the Cylinder, and then faftned weights, to the above named Iron that makes part of that Sucker, the preffure of the external Air finding little or nothing in the cavity of the evacuated Cylinder to refift it, did prefently begin to impell the Sucker, with the weights that clogg'd it, towards the upper part of the Cylinder; till fome fuch accidental Impediment, as we formerly mention'd, check'd its courfe. And when that rub, (which eafily might be,) was taken out of the way, it would continue its afcent to the top, to the no fmall wonder of thofe By-ftanders, that could not comprehend how fuch a weight could afcend, as it were, of it felf; that is, without any invifible force, or fo much as Suction to lift it up. And indeed it is very confiderable, that though poffibly there might remain fome particles of Air in the Cylinder, after the drawing down of the Sucker; yet the preffure of a Cylinder of the Atmosphere, fomewhat lefs then three Inches in Diameter (for, as it was faid in the defcription of our Engine, the cavity of the Cylinder was no broader) was able, uncompress'd, not only to fuffain, but even to drive up a weight of an hundred and odde pounds: for befides the weight of the whole Sucker it felf, which amounts to fome pounds, the weights annexed to it made up an hundred and three pounds, befides an Iron Bar, that by conjecture weigh'd two pounds more; and yet all thefe together fall fomewhat fhort of the weight which we lately mention'd, the refiftance of the Air, to have held fufpended in the cavity of the Cylinder.

And

And though (as hath been already acknowledg'd) we cannot peradventure, obtain by the recited means so exact an account as were to be wish'd, of what we would discover: Yet, if it serve us to ground Conjectures more approaching to the Truth, then we have hitherto met with, I hope it will be consider'd (which a famous Poet judiciously sayes)

Est quoddam prodire tenus, si non datur ultra.

Peradventure it will not be impertinent to annex to the other Circumstances that have been already set down concerning this Experiment, That it was made in Winter, in Weather neither Frosty nor Rainy, about the change of the Moon, and at a place whose latitude is near about 51^{d} and a half: For perhaps the force or pressure of the Air may vary, according to the Seasons of the Year, the temperature of the Weather, the elevation of the Pole, or the phases of the Moon; all, or even any of them seeming capable to alter either the height or consistence of the incumbent Atmosphere: And therefore it would not be amiss if this Experiment were carefully tryed at several times and places, with variety of Circumstances. It might also be tryed with Cylinders of several Diameters, exquisitely fitted with Suckers, that we might know what proportion several Pillars of the Atmosphere bear, to the Weights they are able to sustain or lift up; and consequently, whether the increase or decrement of the resistance of the ambient Air, can be reduced to any regular proportion to the Diameters of the Suckers: These, and divers other such things which may be try'd with this Cylinder, might most of them be more exactly try'd by the Torricellian Experiments, if we could get Tubes so accurately blown and drawn, that the Cavity were perfectly Cylindrical.

To dwell upon all the several Reflections, that a speculative Wit might make upon this and the foregoing Experiment, (I mean the thirty third and thirty second) would require almost a Volume; whereas our occasions will scarce allow us time to touch upon three or four of the chief Inferences that seem

seem deducible from them, and therefore we shall content ourselves to point at those few.

And first, as many other *Phænomena* of our Engine, so especially, the two lately mention'd Experiments, seem very much to call in question the received Opinion of the Nature or Cause of Suction. For 'tis true indeed, that when men suck, they commonly use some manifest endeavour by a peculiar motion of their Mouthes, Chests, and some other conspiring parts, to convey to them the body to be suckt in. And hence perhaps they have taken occasion, to think that in all Suction there must be some Endeavour or motion in the sucking to attract the suck'd Body. But in our last Experiment it appears not at all how the upper part of the empty'd Cylinder that remains moveless all the while, or any part of it, doth at all endeavour to draw to it the depressed Sucker and the annexed weights. And yet those that behold the ascension of the Sucker, without seriously considering the cause of it, doe readily conclude it to be raised by something that powerfully Sucks or attracts it, though they see not what that may be or where it lurks. So that it seems not absolutely necessary to Suction, that there be in the Body, which is said to suck, an endeavour or motion in order thereunto, but rather that Suction may be at least for the most part reduced to Pulsion, and its effects ascrib'd to such a pressure of the neighboring Air upon those Bodies (whether Aërial, or of other natures) that are contiguous to the Body that is sayd to attract them, as is stronger, then that Substance, which possesseth the cavity of that sucking Body, is able to resist. To object here, that it was some particles of Air remaining in the empty'd Cylinder that attracted this weight to obviate a *Vacuum*, will scarce be satisfactory; unless it can be cleerly made out by what little hooks, or other grappling Instruments, the internal Air could take hold of the Sucker; how so little of it obtained the force to lift up so great a weight; and why also, upon the letting in of a little more Air into one of our evacuated Vessels, the attraction is, instead of being strengthened, much weakened; though,

though, if there were danger of a *Vacuum* before, it would remain, notwithstanding this ingress of a little Air. For that still there remained in the capacity of the exhausted Cylinder store of little rooms, or spaces empty or devoid of Air, may appear by the great violence wherewith the Air rusheth in, if any way be open'd to it. And that 'tis not so much the decrement of the *Vacuum* within the cavity of the vessel that debilitates the attraction, as the Spring of the included Air (whose presence makes the decrement) that doth it by resisting the pressure of the external Air, seems probable, partly from the Disability of vacuities, whether greater or lesser, to resist the pressure of the Air; and partly by some of the *Phænomena* of our Experiments, and particularly by this Circumstance of the three and Thirtieth, that the Sucker was, by the pressure of the Ambient Air, impell'd upwards with its weight hanging at it, not onely when it was in the bottom of the Cylinder, and consequently left a great *Vacuum* in the cavity of it; but when the Sucker had been already impell'd almost to the top of the Cylinder, and consequently, when the *Vacuum* that remain'd was become very little in comparison of that which preceded the beginning of the Sucker's ascension.

In the next place, these Experiments may teach us, what to judge of the vulgar Axiom received for so many Ages as an undoubted Truth in the Peripatetick Schools; That Nature abhorres and flyeth a *Vacuum*, and that to such a degree, that no humane power (to go no higher) is able to make one in the Universe; wherein Heaven and Earth would change places, and all its other Bodyes rather act contrary to their owne Nature, then suffer it. For, if by a *Vacuum* we will understand a place perfectly devoid of all corporeal Substance, it may indeed then, as we formerly noted, be plausibly enough maintained that there is no such thing in the world; but that the generality of the Plenists, (especially till of late yeares some of them grew more wary) did not take a *Vacuum* in so strict a Sense, may appear by the Experiments formerly, and ev'n to this Day imploy'd by the Deniers of a *Vacuum*, to prove it impossible that there

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can be any made. For when they alleadge (for Instance) that when a man sucks Water through a long Pipe, that heavy Liquor, contrary to its Nature, ascends into the Sucker's mouth, only, to fill up that room made by the Dilatation of his Brest and Lungs, which otherwise will in part be empty. And when they tell us, that the reason why if a long Pipe exactly clos'd at one end be filled top-full of Water, and then inverted, no Liquor will fall out of the open Orifice; Or, to use a more familiar Example, when they teach, that the cause, why in a Gardiner's watering Pot shaped conically, or like a Sugar-Loaf, fill'd with Water, no Liquor falls down through the numerous holes at the bottome, whilst the Gardiner keeps his Thumb upon the Orifice of the little hole at the top, and no longer; must be that if in the case proposed the Water should descend, the Air being unable to succeed it, there would be left at the upper and deserted part of the Vessel a *Vacuum*, that would be avoided if the hole at the top were open'd. When (I say) they alleadge such Experiments, the Tendency of them seems plainly to import, that they mean, by a *Vacuum*, any space here below that is not filled with a visible body, or at least with Air though it be not quite devoy'd of all Body whatsoever. For why should Nature, out of her detestation of a *Vacuum*, make Bodies act contrary to their own Tendency, that a place may be fill'd with Air, if its being so were not necessary to the avoiding of a *Vacuum*.

Taking then a *Vacuum* in this vulgar and obvious sence, the common opinion about it seems lyable to several Exceptions, whereof some of the cheif are suggested to us by our Engine.

It will not easily then be intelligibly made out, how hatred or averfation, which is a passion of the Soule, can either for a *Vacuum*, or any other object, be supposed to be in Water, or such like inanimate Body, which cannot be presumed to know when a *Vacuum* would ensue; if they did not bestirre themselves to prevent it: nor to be so generous as to act contrary to what is most conducive to their own particular preservation for the pub-

publique good of the Universe. As much then of intelligible and probable Truth, as is contain'd in this Metaphoricall Expression, seems to amount but to this; That by the Wise Author of Nature (who is justly sayd to have made all things in number, weight, and measure,) the Universe, and the parts of it, are so contriv'd, that it is as hard to make a *Vacuum* in it, as if they studiously conspir'd to prevent it. And how far this it self may be granted, deserves to be farther consider'd.

For in the next place, our Experiments seem to teach, that the supposed Aversion of Nature to a *Vacuum* is but accidental, or in consequence, partly of the Weight and Fluidity, or, at least, Fluxility of the Bodies here below; and partly, and perhaps principally, of the Spring of the Air, whose restless endeavour to expand it self every way, makes it either rush in it self, or compel the interposed Bodies into all spaces, where it finds no greater resistance then it can surmount. And that in those motions which are made *ob fugam Vacui* (as the common phrase is) Bodies Act without such generosity and Consideration, as is wont to be ascrib'd to them, is apparent enough in our 32^d Experiment, where the torrent of Air, that seem'd to strive to get into the Empty'd Receiver, did plainly prevent its own Designe by so impelling the Valve, as to make it shut the onely Orifice the Air was to get out at. And if afterwards either Nature, or the internal Air, had a designe the external Air should be attracted, they seem'd to prosecute very unwisely by continuing to suck the Valve so strongly; when they found that by that Suction the Valve it selfe could not be drawn in: Whereas by forbearing to suck, the Valve would by it's own weight have fall'n down, and suffer'd the excluded Air to returne freely, and to fill again the exhausted Vessel.

And this minds me to take notice of another deficiency, pointed at by our Experiments in the common Doctrine of those Plenists we reason with; for many of those unusual motions in Bodies, that are sayd to be made to escape a *Vacuum*, seem rather made to fill it. For why, to instance in our newly mention'd

Experiment, as soon as the Valve was depressed by the weight we hung at it, should the Air so impetuously and copiously rush into the cavity of the Receiver; if there were before no vacant room there to receive it? and if there were, then all the while the Valve kept out the Air, those little spaces in the Receiver, which the corpuscles of that Air afterwards fill'd, may be concluded to have remain'd empty. So that the seeming violence, imploy'd by Nature on the occasion of the evacuating of the Vessel, seems to have come too late to hinder the making of Vacuities in the Receiver, and only to have, as soon as we permitted, fill'd up with Air those that were already made.

And as for the Care of the Publique Good of the Universe ascrib'd to dead and stupid Bodies, we shall onely demand, why in our 19th Experiment, upon the Exsuction of the ambient Air, the Water deserted the upper half of the Glass-Tube; and did not ascend to fill it up, till the external Air was let in upon it: whereas by its easy and sudden regaining that upper part of the Tube, it appeared both that there was there much space devoid of Air, and that the Water might with small or no resistance have ascended into it, if it could have done so without the impulsion of the readmitted Air; which, it seems, was necessary to remind the Water of its formerly neglected Duty to the Universe.

Nay, for ought appeareth, even when the excluded Air, as soon as 'twas permitted, rush'd violently into our exhausted Receiver, that flowing in of the Air proceeded rather from the determinate Force of the Spring of the neighbouring Air, than from any endeavour to fill up, much less to prevent vacuity's. For though when as much Air as will, is gotten into our Receiver our present Opponents take it for granted that it is full of Air; yet if it be remembered that when we made our 17th Experiment we croud'd in more Air to our Receiver than it usually holds; and if we also consider (which is much more) that the Air of the same consistence with that in our Receiver may in wind-guns, as is known, and as we have tryed, be compressed at least into halfe its wonted room (I say at least, because some affirme, that

that the Air may be thrust into an 8th, or a yet smaller part of its ordinary extent) it seems necessary to admit either a notion of condensation and rarefaction that is not intelligible, or that in the capacity of our Receiver when presumed to be full of Air, there yet remain'd as much of space as was taken up by all the Aërial corpuscles, unpossessed by the Air. Which seems plainly, to infer that the Air that rush'd into our empty'd vessel did not do it precisely to fill up the Vacuities of it, since it left so many unfill'd, but rather was thrust in by the pressure of the contiguous Air: which as it could not, but be always ready to expand it self, where it found least resistance, so was it unable to fill the Receiver any more, then untill the Air within was reduc'd to the same measure of Compactness with that without.

We may also from our two already often mention'd Experiments farther deduce, that, (since Natures hatred of a *Vacuum* is but Metaphorical and Accidental, being but a consequence or result of the pressure of the Air and of the Gravity, and partly also of the Fluxility of some other Bodies) The power shee makes use of to hinder a *Vacuum*, is not (as we have elf-where also noted) any such boundless thing as men have been pleas'd to imagine. And the reasons why in the former Experiments, mentioned in favour of the Plenists, Bodies seem to forget their owne Natures to shun a *Vacuum*, seems to be but this; That in the alleadged cases the weight of that Water that was either kept from falling or impell'd up, was not great enough to surmount the pressure of the contiguous Air; which, if it had been, the Water would have subsided, though no Air could have succeeded. For not to repeate that Experiment of Monsieur *Paschal* (formerly mention'd to have been tryed in a Glafs exceeding 32 Foot) wherein the inverted Pipe being long enough to contain a competent weight of Water, that Liquor freely ran out at the lower Orifice: Not to mention this (I say) we saw in our nineteenth Experiment, that when the pressure of the ambient Air was sufficiently weaken'd, the Water would fall out apace at the Orifice even of a short Pipe, though the Air

could not succeed into the room deserted by it. And it were not amiss if tryal were made on the tops of very high Mountains, to discover with what ease a *Vacuum* could be made near the confines of the Atmosphere, where the Air is probably but light in comparison of what it is here below. But our present (three and thirtieth) Experiment seems to manifest, not onely that the power, exercis'd by Nature, to shun or replenish a *Vacuum*, is limited, but that it may be determin'd even to Pounds and Ounces : Insomuch that we might say, such a weight Nature will sustain or will lift up to resist a *Vacuum* in our Engine ; but if an Ounce more be added to that weight, it will surmount Her so much magnifi'd detestation of Vacuities. And thus, My Lord, our Experiments may not onely answer those of the Plenists, but enable us to retort their Arguments against themselves : since, if that be true which they alleadge, that, when Water falls not down according to its nature, in a Body wherein no Air can succeed to fill up the place it must leave, the suspension of the Liquor is made *Ne detur Vacuum*, (as they speak) it will follow, that if the Water can be brought to subside in such a case, that deserted space may be deem'd empty, according to their own Doctrine ; especially, since Nature (as they would perswade us) bestirs her self so mightily to keep it from being deserted.

I hope I shall not need to remind Your Lordship, that I have all this while been speaking of a *Vacuum*, not in the strict and Philosophicall sense, but in that more obvious and familiar one that hath been formerly declar'd.

And therefore I shall now proceed to observe in the last place, that our 33^d Experiment affords us a notable proof of the unheeded strength of that pressure which is sustain'd by the Corpuscles of what we call the free Air, and presume to be uncompressed. For, as fluid and yielding a Body as it is, our Experiment teacheth us, That ev'n in our Climate, and without any other compression then what is (at least here below) Natural, or (to speak more properly) ordinary to it, it bears so strongly upon the Bodies whereunto it is contiguous, that a Cylinder of
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this free Air, not exceeding three Inches in Diameter is able to raise and carry up a weight, amounting to between sixteen and seventeen hundred Ounces. I said even in our Climate, because that is temperate enough; and as far as my observations assist me to conjecture, the Air in many other more Northern Countries may be much thicker, and able to support a greater weight: which is not to be doubted of, if there be no mistake in what is Recorded concerning the *Hollanders*, that were forc'd by the Ice to Winter in *Nova Zembla*, namely, That they found there so condens'd an Air, that they could not make their Clock goe, ev'n by a very great addition to the weights that were wont to move it.

I suppose Your Lordship will readily take notice, that I might very easily have discoursed much more fully and accurately than I have done, against the common opinion touching Suction, and touching natures hatred of a *Vacuum*. But I was willing to keep my selfe to those considerations touching these matters, that might be verified by our Engine it selfe, especially, since, as I said at first, it would take up too much time to insist particularly upon all the Reflections that may be made even upon our two last Experiments. And therefore passing to the next, I shall leave it to your Lordship to consider how far these tryals of ours will either confirm or disfavour the new Doctrine of several eminent Naturalists, who teach, That in all motion there is necessarily a Circle of Bodies, as they speak, moving together; and whether the Circles in such motion be an *Accidental* or *Consequential* thing or no.

EXPERIMENT XXXIII.

TIs a known thing to those that are conversant in the Hydrostaticks, That two Bodies which in the Air are of equal weight, but of unequal bulk, as Gold, for instance and Iron, being afterwards weighed in Water, will lose their *Aequilibrium* upon the change of the ambient Body: so that the Gold will sink lower than the Iron; which, by reason of its greater bulk, hath more Water to lift or displace, that it may sink. By Analogy

Aere frigido existente tardius moventur Automata quã aere calido, adeo quidem ut Automaton quod Belgæ in Nova Zembla agentes in ædibus suis collocaverunt, omnino à motu cessaverit et si multo majus pondus ei addidissent quam antea ferre solebat. Varnius Geog. General. lib. III. Propo: 7. pag. 648.

to this Experiment, it seemed probable, that if two weights did in our Engine ballance each other, when the Glass was full of Air; upon the exsuction of a great part of that Air, so notable a change in the consistence of the ambient Body, would make them lose their *Aequilibrium*.

But being desirous at the same time to make a tryal, for a certain Design that needs not here be mention'd, we took for one of our weights a dry Bladder, strongly tyed at the Neck, and about half fill'd with Air (that being a weight both slight, and that would expand it selfe in the evacuated Glass) and fastning that to one part of our formerly mentioned exact ballance (which turns with the 32^d part of a Grain) we put a Metalline counterpoise into the opposite Scale; and so the two weights being brought to an *Aequilibrium*, the ballance was convey'd into the Receiver, and suspended from the Cover of it.

But before we proceed farther, we must note, That presently after the laying on of the Cover, the Bladder appear'd to preponderate, whereupon the Scales being taken out, and reduc'd very near to an *Aequilibrium*, yet so, that a little advantage remained on that side to which the Metalline weight belonged; they were again let down into the Receiver, which was presently made fast with Plaister, and a hot Iron: Soon after which, before the Pump was employ'd, the Bladder seem'd again a little to preponderate. Afterwards the Air in the Glass being begun to be drawn out, the Bladder began (according to the formerly mention'd Observations) to expand it selfe, and manifestly to outweigh the opposite weight, drawing down the Scale to which it was fastned very much beneath the other, especially when the Air had swell'd it to its full extent.

This done, we very leisurely let in the external Air; and observ'd, that upon the flagging of the Bladder, the Scale whereto it was fastned, not onely by degrees return'd to an *Aequilibrium* with the other, but at length was a little outweighed by it.

But because we suspected there might have interven'd some unheeded Circumstance in this last part of the Experiment

we would not presently take out the Scales, nor meddle with the Cover, but leaving things as they were, we perceiv'd, that after a little while the Bladder began again to preponderate, and by degrees to sink lower and lower for divers hours, wherefore, leaving the Vessel closed up all night, we repair'd to it next Morning, and found the Bladder fallen yet lower. As if the very substance of it, had imbibed some of the moisture where-with the Air (the Season being very rainy) did then abound: As Lutestrings, which are made likewise of the Membranous parts of Guts, strongly wreath'd, are known to swell so much, oftentimes as to break in rainy and wet weather. Which conjecture is the more to be regarded, because congruously unto it one of the company having a little warm'd the Bladder, found it then lighter then the opposite weight. But this must be look'd upon as a bare conjecture, till we can gain time to make farther tryals about it. In the mean while we shall adde, that without removing the Scales or the Cover of the Receiver, we again caused the Air to be drawn out (the weather continuing very moist) but found not any manifest alteration in the ballance; whether because the *Aequilibrium* was too far lost to let a small change appear, we determine not.

But to make the Experiment with a Body less apt to be altered by the temperature of the Air, then was the Bladder; we brought the Scales again to an *Aequilibrium* with two weights, whereof the one was of Lead, the other of Cork. And having evacuated the Receiver, we observed, that both upon the extraction, and after the returne of the Air, the Cork did manifestly preponderate, and much more, a while after the Air had been let in again, then whilst it was kept out. Wherefore, in the room of the Cork, we substituted a piece of Char-coal, as less likely to imbibe any moisture from the Air, but the event proved much the same with that newly related: So that this Experiment seems more liable to Casualties then any, excepting one we have made in our Engine. And as it is difficult to prevent them, so it seems not very easie to discover the causes of them, whereof we shall

shall therefore at present forbear mentioning our Conjectures.

EXPERIMENT XXXV.

SOME Learned Mathematicians have of late ingeniously endeavoured to reduce Filters 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 whether or no the pressure of the Air might reasonably be supposed to have either the principal, or at least a considerable Interest in the raising of those Liquors. But because we found that we could not yet so evacuate our Receiver, but that the remaining Air, though but little in comparison of the exhausted, would be able to impell the Water to a greater height than is usual in ordinary Filtrations: we resolved instead of a List of Cotton, or the like Filtre, to make use of a *Siphon* of Glass, delineated in the third Figure, consisting of three pieces, two straight, and the third crooked to joyn them together; whose Junctures were diligently clos'd that no Air might finde entrance at them. One of the Legs of this *Siphon* was (as it should be) somewhat longer than the other, and was pervious at the bottom of it onely, by a hole almost as slender as a hair, that the Water might but very leisurely drop out of it, lest it should all run out before the Experiment were compleated. The other and shorter Leg of the *Siphon* was quite open at the end, and the same wideness with the rest of the Pipe, whose bore was about $\frac{1}{4}$ of an Inch. The whole *Siphon* made up of these several pieces put together, was design'd to be about a Foot and a halfe long; that the remaining Air, when the Vessel was exhausted after the wonted manner, might not be able to impell the Water to the top of the *Siphon*; which being inverted, was fill'd with Water, and of which the Shorter leg being let down two or three Inches deep into a Glass Vessel full of Water, and the upper parts of it being fasten'd to the inside of the Cover of the Receiver, we proceeded to close first, and then to empty the Vessel.

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The effect of the tryal was this, That till a pretty quantity of Air had been drawn out, the Water dropp'd freely out at the lower end of the lower leg of the *Siphon*, as if the Experiment had been performed in the free Air. But afterwards, the Bubbles (as had been apprehended) began to disclose themselves in the Water, and ascending to the top of the *Siphon*, imbodyed themselves there into one, which was augmented by little & little, by the rising of other bubbles that from time to time broke into it, but much more by its own dilatation, which increas'd proportionably to the extraction that was made of the Air out of the Receiver. So that at length the Water in the shorter Leg of the *Siphon* was reduc'd, partly by the extraction of the ambient Air, and partly by the expansion of the great Bubble at the upper part of the *Siphon*, to be but about a Foot high, if so much; whereby it came to pass, that the course of the Water in the *Siphon* was interrupted, and that which remain'd in the longer Leg of it, continu'd suspended there without dropping any longer. But upon the turning of the Stop-cock, the outward Air (being let into the Receiver) got into the *Siphon* by the little hole at which the Water formerly dropt out; and traversing all the incumbent Cylinder of Water, in the form of Bubbles, joyn'd it self with that Air that before possess'd the top of the *Siphon*.

To prevent the inconveniencies arising from these Bubbles, two Glass Pipes, like the former, were so placed, as to terminate together in the midst of the Belly of a Glass Viol, into whose Neck they were carefully fasten'd with Cement; and then both the Viol and the Pipes being (which was not done without difficulty) totally fill'd with Water, the *Siphon* describ'd in the fifth Figure, was plac'd with its shorter Leg in the Glass of Water, as formerly; and the Experiment being prosecuted after the same manner, much more Air then formerly was drawn out, before the bubbles, disclosing themselves in the water, were able to disturb the Experiment; because that in the capacity of the Viol there was room enough for them to stretch themselves, without depressing the Water below the ends of the Pipes; and, during this time,

the Water continued to drop out of the propending Leg of the *Siphon*. But at length the Receiver being very much empty'd, the passage of the Water through the *Siphon* ceas'd, the upper ends of the Pipes beginning to appear a little above the remaining Water in the Viol, whose dilated Air appear'd likewise to press down the Water in the Pipes, and fill the upper part of them.

And hereby the continuity of the Water, and so the Experiment it self being interrupted, we were invited to let in the Air again, which, according to its various proportions of pressure to that of the Air in the Viol and the Pipes, did for a good while exhibit a pleasing variety of *Phanomena*, which we have not now the leisure to recite. And though upon the whole matter there seem'd little or no cause to doubt, but that, if the Bubbles had not disturb'd the Experiment, it would manifestly enough have appear'd that the course of Water through *Siphons* depends upon the pressure of the Air: yet we resolv'd, at our next leisure and conveniency, to try the Experiment again, with a quantity of Water before freed from Bubbles by the help of the same Engine.

This occasion I have had to take notice of *Siphons*, puts me in minde of an odde kinde of *Siphon* that I caus'd to be made a pretty while ago; and which hath been since, by an Ingenious Man of Your acquaintance, communicated to divers others. The occasion was this: An eminent Mathematician told me one day, that some inquisitive French Men (whose Names I know not) had observ'd, That in case one end of a slender and perforated Pipe of Glass be dip'd in Water, the Liquor will ascend to some height in the Pipe, though held perpendicular to the plain of the Water. And, to satisfie me that he mis-related not the Experiment, he soon after brought two or three small Pipes of Glass, which gave me the opportunity of trying it: though I had the less reason to distrust it, because I remember I had often, in the long and slender Pipes of some Weather Glasses, which I had caus'd to be made after a somewhat peculiar fashion, taken notice of the like ascension of the Liquor, though (presuming it might
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be casual) I had made but little reflection upon it. But after this tryal, beginning to suppose, that though the Water in these Pipes that were brought me, rise not above a quarter of an Inch, (if near so high) yet, if the Pipes were made slender enough, the Water might rise to a very much greater height; I caus'd several of them to be, by a dexterous Hand, drawn out at the flame of a Lamp, in one of which that was almost incredibly slender, we found that the Water ascended (as it were of it self) five Inches by measure, to the no small wonder of some famous Mathematicians, who were Spectators of some of these Experiments. And this height the Water reach'd to, though the Pipe were held in as erected a posture as we could: For if it were inclin'd, the Water would fill a greater part of it, though not rise higher in it. And we also found, that when the inside of the Pipe was wetted beforehand, the Water would rise much better then otherways: But we caus'd not all our slender Pipes to be made straight, but some of them crooked, like *Siphons*: And having immerf'd the shorter Leg of one of these into a Glass that held some fair Water, we found, as we expected, that the Water arising to the top of the *Siphon*, though that were high enough, did of it self run down the longer Leg, and continue running like an ordinary *Siphon*. The cause of this ascension of the Water, appear'd to all that were present so difficult, that I must not stay to enumerate the various Conjectures that were made at it, much less to examine them; especially having nothing but bare Conjectures to substitute in the room of those I do not approve. We try'd indeed, by conveying a very slender Pipe and a small Vessel of Water into our Engine, whether or no the exsuction of the ambient Air would assist us to finde the cause of the ascension we have been speaking of: But though we imploy'd red Wine instead of Water, yet we could scarce certainly perceive thorow so much Glais, as was interpos'd betwixt our eyes and the Liquor, what happen'd in a Pipe so slender that the redness of the Wine was scarce sensible in it. But as far as we could discern, there happen'd no great alteration to the Liquor: which seem'd the less strange, because the spring

of that Air that might depress the Water in the Pipe, was equally debilitated with that which remain'd to press upon the surface of the water in the little Glass. Wherefore, in favor of his Ingenious Conjecture who ascrib'd the *Phænomenon* under consideration, to the greater pressure made upon the water by the Air without the Pipe, then by that within it, (where so much of the water (consisting perhaps of Corpuscles more pliant to the internal surfaces of the Air) was contiguous to the Glass) it was shown, that in case the little Glass Vessel that held the water, of which a part ascended into the slender Pipe, were so clos'd, that a Man might with his mouth suck the Air out of it, the water would immediately subside in the small Pipe. And this would indeed infer, that it ascended before onely by the pressure of the incumbent Air; But that it may (how justly I know not) be objected, That peradventure this would not happen, in case the upper end of the Pipe were in a *Vacuum*: And that 'tis very probable the water may subside, not because the pressure of the internal Air is taken off by Exsuction, but by reason of the spring of the external Air, which impels the Water it findes in its way to the cavity deserted by the other Air, and would as well impel the same water upwards, as make it subside, if it were not for the accidental posture of the Glasses. However, having not now leisure to examine any farther this Matter, I shall onely minde Your Lordship, that if You will prosecute this Speculation, it will be pertinent to find out likewise, Why the surface of water (as is manifest in Pipes) useth to be concave, being depress'd in the middle, and higher on every side? and why in Quick-silver on the contrary, not onely the surface is wont to be very convex, or swelling, in the middle; but if you dip the end of a slender Pipe in it, the surface of the Liquor (as 'tis call'd) will be lower within the Pipe, then without. Which *Phænomena*, whether, and how far, they may be deduc'd from the Figure of the Mercurial Corpuscles, and the Shape of the springy Particles of the Air, I willingly leave to be consider'd.

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EXPERIMENT XXXVI.

Several ways we have met with propos'd, partly by the excellent *Galileo*, and partly by other ingenious Writers, to manifest that the Air is not devoid of weight; some of these require the previous absence of the Air to be weighed; and others, the violent condensation of it. But if we could lift a pair of Scales above the Atmosphere, or place them in a *Vacuum*, we might there weigh a parcel of Air it self, as here we do other Bodies in the Air, because it would there be heavier than that which surrounds it, as are grosser Bodies we commonly weigh, than the medium or ambient Air. Wherefore, though we have above declin'd to affirm, that our Receiver, when empty'd, deserves the name of a true *Vacuum*, and though we cannot yet perfectly free it from Air it selfe, yet we thought fit to try how far the Air would manifest its gravity in so thin a medium, as we could make in our Receiver, by evacuating it. We caus'd then to be blown at the Flame of a Lamp, a Glass-bubble of about the bigness of a small Hen-egge, and of an Oval form, save that at one end there was drawn out an exceeding slender Pipe, that the Bubble might be sealed up, with as little rarefaction as might be, of the Air included in the great or Oval Cavity of it. This Glass being sealed, was fastened to one of the Scales of the exact pair of Ballances formerly mention'd; & being counterpois'd with a weight of Lead, was convey'd into the Receiver, and clos'd up in it. The Beam appearing to continue Horizontal, the Pump was set on work, and there scarce past above two or three Exinctions of the Air, before the Ballance lost its *Aequilibrium*, and began to incline to that side on which the Bubble was; which, as the Air was farther and farther drawn out, did manifestly more and more preponderate, till he that pumped began to grow weary of his Employment: after which the Air being leasurely let in again, the Scales by degrees returned to their former *Aequilibrium*. After that we took them out, and casting into that Scale to which the lead belonged three quarters of a grain, we convey'd the ballance into the Receiver,

Receiver, which being closed up, and exhausted as before, we observ'd, that as the Air was drawn out more and more, so the Glass bubble came nearer and nearer to an *Equilibrium* with the other weight, till at length the Beam was drawn to hang Horizontal; which (as we had found by another tryal) we could not bring it to do, when a quarter of a Grain more was added to the Scale, to which the Lead belong'd: though it seem'd questionless, that if we could have perfectly empty'd the Receiver of the contain'd Air, that included in the bubble would have weighed above a Grain, notwithstanding its having been probably somewhat rarefied by the flame, by the help of vvhich, the bubble was seal'd up. Let us adde, That on the regrefs of the excluded Air, the Lead, and the weight cast into the same scale, did again very much preponderate.

We likewise convey'd into the Receiver, the same bubble, open'd at the end of the slender Pipe above-mention'd, but having draw'n out the Air, after the accustomed manner, we found not as before, the bubble to out-weigh the opposite Lead: so that by the help of our Engine vve can vveigh the Air, as vve weigh other Bodies, in its natural or ordinary consistence, without at all condensing it: Nay, which is remarkable, having convey'd a Lambs bladder about half full of Air into the Receiver, we observed, that though upon the draw'ing out of the ambient Air, the imprisoned Air so expanded it self, as to distend the Bladder so, as to seem ready to break it; yet this rarefied Aire did manifestly depresse the Scale vvhich it vvas annexed.

Another thing we must not forget to mention, that happened to us, whil'st vve vvere making tryals concerning the vveight of the Air; namely, That having once caus'd the Pump to be somewhat obstinately ply'd, to discover the better vvhich may be expected from the thinness of the medium in this Experiment; the Imprison'd Air broke its brittle Prison, and throwing the greatest part of it against the side of the Receiver, dash'd it against that thick Glass into a multitude of pieces. Which accident I mention,

tion, partly that it may confirm vvhhat vve deliver'd in our Reflexions, upon the first Experiment, vvhether vve considered vvhhat vvhould probably be done by the spring of the Air Imprison'd in such Glasses, in case the ballancing pressure of the ambient Air vvere vwithdrawn; and partly, that vve may thence discern of how close a Texture Glass is, since so very thin a film of Glass (if I may so call it) prov'd so impervious to the Air, that it could not get away through the Pores, but vvas forc'd to break the Glass in pieces to free it self; and this, notwithstanding the time and advantage it had to try to get out at the Pores. And this I mention, that neither our Experiments, nor those of divers Learned Men, might receive any prejudice from an Experiment which I happen'd to make divers years ago, and which, having been so much taken notice of by curious Men, may be dravvn to countenance their erroneous Opinion, vvhich vvhould fain perswade us, That Glass is penetrable by Air properly so called. Our Experiment vvas briefly this: We vvere distilling a certain Substance, that much abounded vwith subtle Spirits and volatile Salt, in a strong Earthen vessel of an unusual shape, to vvhich vvas luted a large Receiver, made of the courser sort of Glass (vvhich the Trades-men are vvont to call green Glass) but in our absence, the Fire, though it vvere to be very strong, vvas, by the negligence or mistake of those we appointed to attend it, so excessively increas'd, that when vve came back to the Fornace, vve found the spirituuous and saline Corpuscles pour'd out (if I may so call it) so hot, and so copiously into the Receiver, that they made it all opacous, and more likely to shie in pieces, then fit to be touch'd. Yet, being curious to observe the effects of a Distillation, prosecuted vwith so intense and unusual a degree of heat, vve ventur'd to come near, and observ'd, among other things, that on the out-side of the Receiver, at a great distance from the juncture, there was settled a round whitish Spot or two, vvhich at first vve thought might be some stain upon the Glass; but after, finding it to be in divers Qualities like the Oyl and Salt of the Concrete vve vvere Distilling, vve began to suspect that the most subtle and fugitive parts of
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the impetuously ascending Steams, had penetrated the substance (as they speake) of the Glasse, and by the cold of the ambient Air were condensed on the surface of it. And though we were very backward to credit this suspition, and therefore call'd in an Ingenious Person or two, both to assist us in the Observation, and have Witnesse of its event, we continued a while longer to watch the escape of such unctuous Fumes; and upon the whole matter unanimously concluded that (all things consider'd) the subtle parts of the distill'd matter being violently agitated, by the excessive heat had pass'd through the Pores of the Glasse, widen'd by the same heat. But this having never happen'd but once in any of the Distillations we have either made or seen, though these be not a few, it is much more reasonable to suppose, that the perviousnesse of our Receiver to a Body much more subtle then Air, proceeded partly from the looser Texture of that particular parcel of Glasse the Receiver was made of (for Experience hath taught us, that all Glasse is not of the same compactnesse and solidity) and partly from the enormous heat, which, together with the vehement agitation of the penetrant Spirits, open'd the Pores of the Glasse; then to imagine that such a substance as Air, should be able to permeate the Body of Glasse contrary to the testimony of a thousand Chymicall and Mechanicall Experiments, and of many of those made in our Engine, especially that newly recited: Nay, by our fifth Experiment it appeares that a thin Bladder will not at its Pores give passage even to rarefied Air. And on this occasion we will annex an Experiment, which hath made some of those we have acquainted with it, doubt, whether the Corpuscles of the Air be not lesse subtle then those of Water.

But without examining here the reasonableness of that doubt, we will proceed to recite the Experiment it selfe, which seems to teach, That though Air, when sufficiently compressed, may perchance get entrance into narrower holes and crannies then Water; yet unless the Air be forc'd in at such very little holes,

it will not get in at them, though they may be big enough to let Water pass through them.

The Experiment then was this: I took a fair Glass *Siphon*, the lower end of whose longest Leg was drawn by degrees to such a slenderness, that the Orifice, at which the Water was to fall out, would hardly admit a very small Pin: This *Siphon* being inverted, the matter was so order'd, that a little Bubble of Air was intercepted in the slenderest part of the *Siphon*, betwixt the little hole newly mention'd, and the incumbent Water, upon which, it came to pass, that the Air being not to be forc'd through so narrow a passage, by so light a Cylinder of Water, (though amounting to the length of divers Inches,) as lean'd upon it, hindered the farther efflux of the Water, as long as I pleased to let it stay in that narrow place: whereas, when by blowing a little at the wider end of the *Siphon*, that little parcel of Air was forced out with some Water, the remaining Water, that before continu'd suspended, began freely to drop down again as formerly. And if you take a Glass Pipe, whether it be in the form of a *Siphon*, or no, that being for the most part of the thickness of a Mans Finger, is yet towards one end so slender, as to terminate in a hole almost as small as a Horse-hair; and if you fill this Pipe with Water, you will finde that Liquor to drop down freely enough thorow the slender Extream: But if you then invert the Pipe, you will finde that the Air will not easily get in at the same hole through which the Water passed. For in the sharp end of the Pipe, some Inches of Water will remain suspended, which, 'tis probable, would not happen, if the Air could get in to succeed it, since if the hole were a little wider, the Water would immediately subside. And though it be true, that if the Pipe be of the length of many Inches, a great part of the Water will run down at the wider Orifice: yet that seems to happen for some other reason, then because the Air succeeds it at the upper and narrow Orifice, since all the slender part of the Pip, and perhaps some Inches more, will continue full of Water.

And on this occasion I remember, that whereas it appears by

our fifth Experiment, That the Aërial Corpuscles (except perhaps some that are extraordinarily fine) will not pass thorow the Pores of a Lambs Bladder, yet Particles of Water will, as we have long since observ'd, and as may be easily try'd, by very closely tying a little *Alcalizate* Salt (we us'd the Calx of Tartar, made with Nitre) in a fine Bladder, and dipping the lower end of the Bladder in Water; for if you hold it there for a competent while, you will finde that there will strain thorow the Pores of the Bladder, Water enough to dissolve the Salt into a Liquor.

But I see I am slip'd into a Digression, wherefore I will not examine, whether, the Experiment I have related, proceeded from hence, That the springy Texture of the Corpuscles of the Air, makes them less apt to yield and accommodate themselves easily to the narrow Pores of Bodies, then the more flexible Particles of Water; or whether it may more probably be ascrib'd to some other Cause. Nor will I stay to consider how far we may hence be assisted to guess at the cause of the ascension of Water in the slender Pipes, and *Siphons* formerly mention'd, but will return to our Bubble; and take notice, That we thought fit also to endeavor to measure the capacity of the Bubble we had made use of, by filling it with Water, that we might the better know how much Water answered in weight to $\frac{3}{4}$ of a Grain of Air, but notwithstanding all the diligence that was used to preserve so brittle a Vessel, it broke before we could perfect that we were about, and we were not then provided of another Bubble fit for our turn.

The haste I was in, My Lord, when I sent away the last Sheet, made me forget to take notice to you of a Problem that occur'd to my thoughts, upon the occasion of the slow breaking of the Glass Bubble in our evacuated Receiver. For it may seem strange, since by our sixth Experiment it appears, that the Air, when permitted, will by its own internall Spring expand it selfe twice as much as *Mersennus* was able to expand it, by the heat even of a candent *Æolipile*: Yet the *Elater* of the Air was scarce able to break a very thin Glass Bubble, and utterly unable to
break

break one somewhat thicker, within whose cavity it was imprison'd; whereas Air pent up and agitated by heat is able to perform so much more considerable effects, that (not to mention those of Rarefaction that are more obvious) the Learned Jesuit *Cabæus* (he that writ of the Load-stone) relates, That he saw a Marble Pillar (so vast, that three men together with display'd arms could not embrace it, and that 1000 Yoke of Oxen drawing it several ways with all their strength, could not have torn it assunder) quite broken off in the midst, by reason of some Wood, which happening to be burnt just by the Pillar, the heat proceeding from the neighboring Fir, so rarefied some Air or Spirituous Matter which was shut up in the cavities of the Marble, that it broke through the solid Body of the Stone to obtain room to expand it self.

P: Nicol:
Cab: lib: 4.
Meteor: A.
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I remember I have taken notice that probably the reason why the included Air did not break the hermetically seal'd Bubbles that remain'd intire in our empty'd Receiver, was, That the Air, being somewhat rarefied by the Flame employ'd to close the Glass, its Spring, upon the recess of the heat, grew weaker then before. But though we reject not that guess, yet it will not in the present case serve the turne, because that much smaller Glass bubbles exactly clos'd; will, by the included Air (though agitated by the heat of a very moderate Fire) be made to fly in pieces. Whether we may be assisted to solve this Problem, by considering that the heat doth from within vehemently agitate the Corpuscles of the Air, and adde its assistance to the Spring they had before, I shall not now examine: since I here but propose a Problem, and that chiefly that by this memorable Story of *Cabæus*, notice may be taken of the prodigious power of Rarefaction, which hereby appears capable of performing stranger things then any of our Experiments have hitherto ascrib'd to it.

We should hence, My Lord, immediatly proceed to the next Experiment, but that we think it fit, on this occasion, to acquaint You with what some former tryals (though not made in our Engine) have taught us, concerning what we would have

discover'd by the newly mention'd Bubble that broke. And this the rather, because (a great part of this Letter supposing the gravity of the Air) it will not be impertinent to determine more particularly then hitherto we have done, what gravity we ascribe to it.

We tooke then an *Aeolipile* made of Copper, weighing six ounces, five drachms, and eight and forty graines: this being made as hot as we durst make it, (for feare of melting the mettle, or at least the Sodar) was removed from the fire and immediately stopped with hard Wax that no Air at all might get in at the little hole, wont to be left in *Aeolipiles* for the fumes to issue out at: Then the *Aeolipile* being suffer'd leasurely to coole, was again weighed together with the Wax that stopt it, and was found to weigh (by reason of the additionall weight of the Wax) six ounces, six drachms, and 39 graines. Lastly, the Wax being perforated without taking any of it out of the Scale, the external Air was suffered to rush in (which it did with some noyse) and then the *Aeolipile* and Wax, being againe weighed amounted to six ounces, six drachms, and 50. graines. So that the *Aeolipile* freed as farre as our fire could free it, from it's Air, weighed less then it self when replenished with Air, full eleven graines. That is, the Air containable within the cavity of the *Aeolipile* amounted to eleven graines and somewhat more; I say somewhat more, because of the particles of Air, that were not driven by the fire out of the *Aeolipile*. And by the way (if there be no mistake in the observations of the diligent *Mersennus*) it may seem strange that it should so much differ from 2. or 3. of ours; in none of which we could rarefie the Air in our *Aeolipile* (though made red hot almost all over, and so immediately plung'd into cold Water) to halfe that degree which he mentions, namely to 70 times it's naturall extent, unless it were that the *Aeolipile* he imploy'd was able to sustaine a more vehement heat then ours (which yet we kept in so great an one, that once the Soder melting, it fell asunder into the two Hemispheres it consists of.)

The fore-mentioned way of weighing the Air by the help of an *Aeolipile*, seems somewhat more exact then that which *Mersennus*

sennus used, In that in ours the *Æolipile* was not weighed, till it was cold; whereas in his, being weighed red hot, it is subject to loose of it's substance in the cooling, for (as we have elsewhere noted on another occasion) Copper heated red hot, is wont in the cooling to throw off little thin scales in such plenty, that having purposely watcht a Copper *Æolipile* during its refrigeration, we have seen the place round about it almost covered with those little scales it had every way scatter'd: which, however they amount not to much, ought not to be over-looked, when 'tis so light a Body as Air, that is to be weighed. We will not examine whether, the *Æolipile* in cooling may not receive some little increment of weight, either from the vapid or saline Steams that wander up and downe in the Air: But we will rather mention, that (for the greater exactnesse) we imployed to weigh our *Æolipile*, both when fill'd onely with Air and when replenish'd with Water, a paire of Scales that would turne (as they speak) with the fourth part of a grain.

As to the proportion of weight betwixt Air and Water, some learned men have attempted it by wayes so unaccurate that they seeme to have much mistaken it. For (not to mention the improbable accounts of *Kepler* and others.) The learned and diligent *Ricciolus*, having purposely endeavoured to investigate this proportion by meanes of a thin bladder, estimates the weight of the Air to that of the Water to be as one to ten thousand, or thereabouts. And indeed I remember that having formerly, on a certain occasion, weighed a large bladder full of Air, and found it when the Air was all squeezed out, to have contained fourteen graines of Air. I found the same bladder afterwards fill'd with Water to containe very neer 14 pound of that liquor: according to which account, the proportion of Air to Water was almost as a graine to a pound, that is, as one, to above 7600. To this we may adde, that one the other side, *Galileo* himselfe using another, (but an unaccurate way too,) defined the Air to be in weight to Water, but as one to 4 hundred. But the way formerly proposed of weighing the Air by an *Æolipile*, seemes by great oddes more exact

exact; and (as farre as we could ghesse) seemed to agree well enough with the Experiment made in our Receiver. Wherefore it will be best to trust our *Aolipile* in the enquiry we are about. And according to our observations the water it contained amounting to one and twenty ounces and an halfe, and as much Air as was requisite to fill it weighing eleven graines, the proportion in gravity of Air to Water of the same bulk will be as one to 938. And though we could not fill the *Aolipile* with water, so exactly as we would, yet in regard we could not neither as perfectly as we would, drive the Air out of it by heat, we think the proportion may well enough hold: but those that are delighted with round numbers (as the phrase is) will not be much mistaken if they reckon Water to be neere a thousand times heavier then Air. And (for farther proof that we have made the proportion betwixt these two Bodyes rather greater then lesser then indeed it is; and also to confirme our former observation of the weight of the Air) we will adde, That, having another time put some Water into the *Aolipile* before we set it on the fire, that the copious vapours of the rarefied liquor might the better drive out the Air, we found, upon tryall carefully made, that when the *Aolipile* was refrigerated, and the included vapours were by the cold turned againe into Water (which could not have happen'd to the Air, that the preceeding Steames expelled) the Air, when it was let in, increas'd the weight of the *Aolipile* as much as before, namely, Eleven Grains; though there were already in it twelve Drachmes and a halfe, besides a couple of Grains of Water, which remained of that we had formerly put into it to drive out the Air.

Mersennus indeed tells us, that by his account Air is in weight to Water, as 1 to 1356. And adds, that we may, without any danger, believe that the gravity of Water to that of Air of a like bulke, is not less then of 1300 to 1. And consequently that the quantity of Air to a quantity of Water equiponderant thereto, is as 1300 to 1. But why we should relinquish our own carefully repeated tryals, I see not. Yet I am unwilling to re-

reject those of so accurate and useful a Writer: And therefore shall propose a way of reconciling our differing Observations, by presenting, that the discrepance between them may probably arise from the differing consistence of the Air at *London* and at *Paris*: For our Air being more cold and moist, then that which Your Lordship now breaths, may be suppos'd also to be a fourth or fifth part more heavy. I leave it to be consider'd, whether it be of any moment that our Observations were made in the midst of Winter, whereas his were perhaps made in some warmer time of the Year. But I think it were not amiss, that, by the method formerly propos'd, the gravity of the Air were observ'd both in several Countries, and in the same Country, in the several Seasons of the Year and differing Temperatures of the Weather. And I would give something of value to know the weight of such an *Aolipile* as ours full of Air, in the midst of Winter in *Nova Zembla*, if that be true which we formerly took notice of, namely, That the *Hollanders*, who Wintered there, found that Air so thick that their Clock would not go.

If Your Lordship should now ask me, if I could not by the help of these, and our other Observations, decide the Controversies of our Modern Mathematicians about the height of the Air or Atmosphere, by determining how high it doth indeed reach: I should answer, That though it seems easie enough to shew that divers Famous and Applauded Writers have been mistaken in assigning the height of the Atmosphere: Yet it seems very difficult precisely to define of what height it is. And because we have hitherto but lightly touch'd upon a matter of such importance, we presume it will not be thought impertinent, upon this occasion, to annex something towards the Elucidation of it.

What we have already try'd and newly set down, allows us to take it for granted, that (at least about *London*) the proportion of gravity betwixt Water and Air, of equal bulk, is as of a thousand to one.

The next thing therefore that we are to enquire after, in order to our present design, is the difference in weight betwixt Water
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and Quick-silver: And though this hath been defin'd already by the Illustrious *Verulam*, and some other inquisitive Persons, that have compared the weight of several Bodies, and cast their Observations into Tables, yet we shall not scruple to annex our own tryals about it: Partly, because we finde Authors considerably to dis-agree; partly, because we used exacter Scales, and a somewhat more wary method then others seem to have done: And partly also, because having profecuted our inquiry by two or three several ways; the small difference between the events may assure us that we were not much mistaken.

We took then a Glass Pipe, of the form of an inverted *Siphon* whose shape is delineated in the sixteenth Figure: And pouring into it a quantity of Quick-silver, we held it so, that the superficies of the Liquor, both in the longer and shorter leg, lay in a Horizontal Line, denoted in the Scheme by the prick'd Line E F; then pouring Water into the longer Leg of the *Siphon*, till that was almost fill'd, we observ'd the surface of the Quick-silver in that Leg to be, by the weight of the Water, depressed, as from E to B; and in the shorter Leg, to be as much impell'd upwards as from F to C: Whereupon having formerly stuck marks, as well at the point B, as at the opposite point D, we measur'd both the distance D C to have the height of the Cylinder of Quick-silver, which was raised above the Point D (level with the surface of the Quick-silver in the other Leg) by the weight of the Water, and the distance B A which gave us the height of the Cylinder of Water. So that the distance D C amounting to $2\frac{13}{54}$ Inches, and the height of the Water amounting $30\frac{45}{54}$ Inches; and the whole numbers on both sides, which the annexed Fractions being reduc'd to improper Fractions of the same denomination, the proportion, appear'd to be (the denominators being left out as equal on both sides) as 121 to 1665; or by reduction, as one to $13\frac{22}{121}$.

Besides this unusual way of determining the gravity of some things, we measur'd the proportion betwixt Quick-silver and Water, by the help of so exact a ballance, as looseth its *Aequilibrium* by the hundredth part of a Grain: But because there is

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wont to be committed an oversight in weighing Quick-silver and Water, especially if the Orifice of the Vessel wherein they are put be any thing wide, in regard that men heed not that the surface of Water in Vessels will be concave, but that of Quick-silver notably convex or protuberant: To avoid this usual oversight (I say) we made use of a Glass bubble, blown very thin at the Flame of a Lamp, that it might not be too heavy for the Ballance, and terminating in a very slender neck, wherein the concavity or convexity of a Liquor could not be considerable: This Glass weighing $23\frac{1}{2}$ Grains, we fill'd almost with Quick-silver, and fastning a mark over against the middle of the protuberant Superficies as near as our Eyes could judge, we found that the Quick-silver alone weighed $299\frac{7}{32}$ Grains; Then the Quick-silver being pour'd out, and the same Glass being fill'd as full of common Water, we found the Liquor to weigh $21\frac{7}{8}$ Grains. Whereby it appear'd the weight of Water to Quick-silver, is as one to $13\frac{19}{28}$: Though our Illustrious *Verulam* (questionless not for want of Judgement or Care, but of exact Instruments) makes the proportion betwixt those two Liquors to be greater then of 1 to 17. And to adde, that upon the by, since Quick-silver and well rectified Spirit of Wine, are (how justly I say not) accounted, the one the heaviest, and the other the lightest of Liquors; we thought to fill the same Glass, and with the same Scales to observe the difference betwixt them, which we found to be as of 1 to $16\frac{64}{1084}$; whereby it appear'd, That the difference betwixt Spirit of Wine, that may be made to burn all away, (such as was ours) and common Water, is as betwixt 1 and $1\frac{44}{171}$.

We might here take occasion to admire, that though Water (as appear'd by the Experiment formerly mention'd of the Pewter Vessel) seems not capable of any considerable condensation, and seems not to have interspersed in it any store of Air; yet Quick-silver, of no greater bulk then Water, should weigh near fourteen times as much. But having onely pointed at this as a thing worthy of consideration, we will proceed in our inquiry after the height of the Atmosphere: And to avoid the trouble of Fra-

tions, we will assume, that Quick-silver is fourteen times as heavy as Water, since it wants so little of being so.

Wherefore, having now given us the proportion of Air to Water, and Water to Quick-silver, it will be very easie to finde the proportion betwixt Air and Quick-silver, in case we will suppose the Atmosphere to be uniformly of such a consistence as the Air we weighed here below. For since our Engine hath sufficiently manifested that 'tis the *Æquilibrium* with the external Air, that in the *Torricellian* Experiment keeps the Quick-silver from subsiding; And since, by our accurate Experiment formerly mention'd, it appears that a Cylinder of Mercury, able to ballance a Cylinder of the whole Atmosphere, amounted to near about thirty Inches; and since, consequently we may assume the proportion of Quick-silver to Air to be as fourteen thousand to one; it will follow, that a Cylinder of Air, capable to maintain an *Æquilibrium*, with a Mercurial Cylinder of two Foot and an half in height, must amount to 35000 Feet of our English Measure; and consequently (reckoning five Foot to a Geometrical Pace, and one thousand such Paces to a Mile) to seven full Miles.

But this (as we lately intimated) proceeds upon the supposition, that the Air is every where of the same consistence that we found it near the surface of the Earth; but that cannot with any safety be concluded, not onely for the reason I finde to have been taken notice of by the Ancients, and thus exprest in *Seneca*: *Omnis Aër* (says he) *quo propior est terris hoc crassior; quem admodum in aqua & in omni humore fax ima est, ita in Aëre spississima quæq; desidant*, but much more, because the springy Texture of the Aërial Corpucles; makes them capable of a very great compression, which the weight of the incumbent part of the Atmosphere is very sufficient to give those that be undermost and near the surface of the Earth. And if we recall to minde those former Experiments, whereby we have manifested, That Air, much rarefied without heat, may easily admit a farther rarefaction from heat, and that the Air, even without being expanded by heat, is capable of being rarefied to above one hundred and fifty

Senec: Nat:
quest: lib. 4.
cap. 10.

times

times the extent it usually possesseth here below ; How can it be demonstrated that the Atmosphere may not, for ought we know, or at least for ought can be determin'd by our Statical and Mechanical Experiments, rise to the height of Five and twenty *German Leagues*, if not of some hundred of common Miles ?

And this conjecture it self may appear very injurious to the height whereunto Exhalations may ascend, if we will allow that there was no mistake in that strange Observation made at *Toulouse* in a clear Night in *August*, by the diligent Mathematician *Emanuel Magnan*, and thus Recorded by *Ricciolus*, for I have not at hand the Authors own Book: *Vidit* (sayes he) *abhora undecima post meridiem usque ad mediam noctem Lunâ infra horizontem positâ, nubeculam quandam lucidam prope Meridianum fere usque ad Zenith diffusam quæ consideratis omnibus non poterat nisi à sole illuminari ; ideoque altior esse debuit tota umbra terræ. Addit* (continues *Ricciolus*) *simile quid evenisse Michaeli Angelo Riccio apud Sabinos versanti nempe viro in Matthesi eruditissimo.*

Ricciol:
Alma:
Nov: Tom:
2. lib. 10.
sect. 6. prop.
50. Ex
magnan:
lib. 1. Per-
spective
horarie
prop: 38.

Various Observations made at the feet, tops, and interjacent parts of high Mountaines, might perchance somewhat assist us to make an estimate in what proportion, if in any certain one, the higher Air is thicker then the lower, and ghes at the difform consistence, as to laxity and compactness of the Air at several distances from us. And if the difficulties about the refractions of the Celestial Lights, were satisfactorily determin'd, that might also much conduce to the placing due limits to the Atmosphere (whose Dimensions those Observations about Refractions seem hitherto much to contract.) But for the present we dare not pronounce any thing peremptorily concerning the height of it, but leave it to farther inquiry: contenting our selves to have manifested the mistake of divers eminent Modern Writers, who will not allow the Atmosphere to exceed above two or three Miles in height (as the Famous *Kepler* will not the *Aër refractivus*) and to have rendered a reason why in the mention we made in the Notes upon the first Experiment touching the height of the Atmosphere, we scrupled not to speak of it, as if it might be many Miles high.

E X P E R I M E N T X X X V I I .

WE will now proceed to recite a *Phenomenon*, which, though made amongst the first, we thought fit not to mention till after many others, that we might have the opportunity to observe as many Circumstances of it as we could, and so present Your Lordship at once, most of what we at several times have taken notice of concerning so odde a *Phenomenon*.

Our Engine had not been long finish'd, when, at the first leisure we could steal from our occasions to make tryal of it, we caused the Air to be pump'd out of the Receiver; and whilst I was busied in entertaining a Learned Friend that just then came to visite me, an Ingenious By-stander, thought he perceiv'd some new kind of Light in the Receiver, of which giving me hastily notice, my Friend and I presently observ'd, that when the Sucker was drawn down, immediately upon the turning of the Key, there appear'd a kind of Light in the Receiver, almost like a faint flash of Lightning in the Day-time, and almost as suddenly did it appear and vanish. Having, not without some amazement, observ'd divers of these Apparitions of Light, we took notice that the Day was clear, the hour about ten in the Morning, that the onely Window in the Room faced the North; and also, that by interposing a Cloak, or any opacous Body between the Receiver and the Window, though the rest of the Room were sufficiently enlightned, yet the flashes did not appear as before, unless the opacous Body were removed. But not being able on all these Circumstances to ground any firm Conjecture at the cause of this surprising *Phenomenon*, as soon as Night was come, we made the Room very dark; and plying the Pump, as in the Morning, we could not, though we often try'd, find, upon the turning of the Key, so much as the least glimmering of Light; whence we inferred, that the flash appearing in the Receiver, did not proceed from any new Light generated there, but from some reflections of the light of the Sun, or other Luminous Bodies plac'd without it; though whence that Reflection should proceed, it pos'd us to conjecture.

Where-

Wherefore the next Morning, hoping to inform our selves better, we went about to repeat the Experiment, but though we could as well as formerly exhaust the Receiver, though the place wherein we made the tryal was the very same; and though other Circumstances were resembling, yet we could not discover the least appearance of Light all that Day, nor on divers others on which tryal was again fruitlessly made; nor can we to this very time be sure a Day before-hand that these Flashes will be to be seen in our great Receiver. Nay, having once found the Engine in a good humour (if I may so speak) to shew this trick, and sent notice of it to our Learned Friend Doctor *Wallis*, who express'd a great desire to see this *Phænomenon*, though he were not then above a Bow-shoot off, and made hast to satisfy his Curiosity; yet by that time he was come, the thing he came for was no longer to be seen; so that having vainly endeavored to exhibit again the *Phænomenon* in his presence, I began to apprehend what he might think of me, when unexpectedly the Engine presented us a flash, and after that a second, and as many more, as suffic'd to satisfy him that we might very well confidently relate, that we have our selves seen this *Phænomenon*, though not confidently promise to shew it others.

And this unsuccessfulness whereto our Experiment is lyable, being such, that by all our watchfulness and tryals, we could never reduce it to any certain Rules or Observations; since in all constitutions of the Weather, times of the Day, &c. It will sometimes answer, and sometimes dis-appoint our Expectations; We are much discourag'd from venturing to frame an *Hypothesis* to give an account of it: which if the Experiment did constantly succeed, might the more hopefully be attempted; by the help of the following *Phænomena* laid together: some of them produc'd upon tryals purposely made to examine the validity of the conjectures, other tryals had suggested.

First then we observ'd, that the Apparition of Light may be made as well by Candle-light, as by Day-light; and in whatever position the Candle be held, in reference to the Receiver, as on this or that hand of it, above it, beneath it, or any other way, provided the

the Beams of Light be not hinder'd from falling upon the Vessel.

Next, we noted that the flash appears immediately upon the turning of the Key, to let the Air out of the Receiver into the emptied Cylinder, in so much that I remember not that when at any time in our great Receiver, the Stop-cock was open'd before the Cylinder was exhausted (whereby it came to pass that the Air did rather descend, then rush into the Cylinder) the often mentioned flash appear'd to our eyes.

Yet, we farther observ'd, that when instead of the great Receiver we made use of a small Glass, not containing above a pound and a half of Water, the *Phænomenon* might be exhibited though the Stop-cock were open, provided the Sucker were drawn nimbly down.

We noted too, that when we began to empty the Receiver, the appearances of Light were much more conspicuous then towards the latter end, when little Air at a time could pass out of the Receiver.

We observed also, that when the Sucker had not been long before well Oyl'd, and instead of the great Receiver, the smaller Vessel above-mention'd was emptied; We observ'd, I say, that then, upon the opening of the Stop-cock, as the Air descended out of the Glass into the emptied Cylinder, so at the same time there ascended out of the Cylinder into the Vessel a certain Steam, which seem'd to consist of very little Bubbles, or other minute Corpuscles thrown up from the Oyl, rarefied by the attrition it suffered in the Cylinder. For at the same time that these Steams ascended into the Glass, some of the same kinde manifestly issued out like a little Pillar of Smoke at the Orifice of the Valve, when that was occasionally opened. And these Steams frequently enough presenting themselves to our view, we found, by exposing the Glass to a clear Light, that they were wont to play up and down in it, and so by their whiteishness, to emulate in some measure the apparition of Light.

For we likewise sometimes found, by watchful observation, that when the Flash was great, not onely at the very instant the Receiver lost of its transparency, by appearing full of some

some kinde of whitish substance ; but that for some short time after the sides of the Glass continued somewhat opacous , and seem'd to be darken'd, as if some whitish Steam adher'd to the inside of them.

He that would render a Reason of the *Phanomenon*, vvhwhereof all these are not all the Circumstances, must do tvvo things ; whereof the one is difficult, and the other little less then impossible : For he must give an account not onely whence the appearing whiteness proceeds, but wherefore that vvhiteness doth sometimes appear, and sometimes not.

For our part, vve freely confess our selves at a loss about rendering a Reason of the less difficult part of the Problem : And though Your Lordship should ev'n press us to declare vvhhat Conjecture it was, that the above recited Circumstances suggested to us, we should propose the thoughts we then had, no otherwise then as bare Conjectures.

In case then our *Phanomenon* had constantly and uniformly appear'd, we should have suspected it to have been produc'd after some such manner as follows.

First, vve observ'd that, though that which we saw in our Receiver seem'd to be some kinde of Light, yet it vvas indeed but a whiteness vvhich did (as hath already been noted) opacate (as some speak) the inside of the Glass.

Next we consider'd, that our common Air abounds with Particles, or little Bodies, capable to reflect the Beams of Light. Of this vve might easily give divers proofs, but vve shall name but tvvo : The one, that vulgar observation of the Motes that appear in multiudes swimming up and down in the Air, when the Sunbeams shooting into a Room, or any other shady Place, discover them, though othervvise the Eye cannot distinguish them from the rest of the Air : The other proof we will take from vvhhat vve (and no doubt very many others) have observ'd, touching the Illumination of the Air in the Night. And we particularly remember, that, being at some distance from *London* one Night, that the People, upon a very well-come occasion, testified their Joy by nume-

rous Bon-fires ; though, by reason of the Interposition of the Houses, we could not see the Fires themselves, yet we could plainly see the Air all enlighten'd over and near the City ; which argu'd, that the lucid Beams shot upwards from the Fires, met in the Air with Corpuscles opacous enough to reflect them to our Eyes.

A third thing that we considered, was, That white may be produc'd (without excluding other ways, or denying invisible Pores in the solideft Bodies) when the continuity of a Diaphanous Body happens to be interrupted by a great number of Surfaces, which, like so many little Looking-glasses, do confusedly represent a multitude of little and seemingly contiguous Images of the lucid Body. We shall not insist on the explanation of this, but refer You for it to what we have said in another Paper (touching Colours.) But the Instances that seem to prove it are obvious: For Water or whites of Eggs beaten to froth, do lose their transparency and appear white. And having out of one of our lesser Receivers carefully drawn out the Air, and so order'd it, that the hole by which the Water was to get in, was exceeding small, that the Liquor might be the more broken in its passage thorow it, we observ'd with pleasure, That, the Neck being held under Water, and the little hole newly mention'd being open'd, the Water that rush'd in was so broken, and acquired such a multitude of new Surfaces, that the Receiver seem'd to be full rather of Milk than Water. We have likewise found out, That by heating a lump of Crystal to a certain degree, and quenching it in fair Water, it would be discontinu'd by such a multitude of Cracks, (which created new Surfaces within it) that though it would not fall asunder, but retain its former shape, yet it would lose its transparency, and appear white.

Upon these Considerations, My Lord, & some others, it seem'd not absur'd to imagine, That upon the rushing of the Air out of the Receiver into the empty'd Cylinder, the Air in the Receiver being suddenly and vehemently expanded, the Texture of it was as suddenly alter'd, and the parts made so to shift places (& perhaps some of them to change postures) as during their new & vehement

Motion

Motion and their varied Situation, to disturb the wonted continuity, and so the Diaphaneity of the Air; which (as we have already noted) upon its ceasing to be a transparent Body, without the interposition of colour'd things, must easily degenerate into white.

Several things there were that made this Conjecture seem the less improbable. As first, That the whiteness always appear'd greater when the exsuction began to be made, whil'st there was store of Air in the Receiver, then when the Air was in great part drawn out. And next, That, having exhausted the Receiver, and apply'd to the hole in the Stop-cock a large bubble of clear Glass, in such a manner, that we could at pleasure let the Air pass out at the small Glass into the great one, and easily fill the small one with Air again, We observ'd with pleasure, That, upon the opening the passage betwixt the two Glasses, the Air in the smaller having so much room in the greater to receive it, the Diffusion of that Air was so great, that the small Viol seem'd to be full of Milk; and this Experiment we repeated several times. To which we may adde, That, having provided a small Receiver, whose upper Orifice was so narrow that I could stop it with my Thumb, I observ'd, that when, upon the exsuction of the Air, the capacity of the Glass appear'd white, if, by a sudden removal of my Thumb, I let in the outward Air, that whiteness would immediately vanish. And whereas it may be objected, That in the Instance formerly mention'd, Water turning from perspicuous to white, there intervenes the Air, which is a Body of a Heterogeneous nature, and must turn it into Bubbles to make it lose its transparency. We may borrow an Answer from an Experiment we deliver in another Treatise, where we teach, how to make two very volatile Liquors, which being gently put together are clear as Rock-water, and yet will almost in a moment, without the sub-ingression of Air to turn them into Bubbles, so alter the disposition of their insensible parts, as to become a white and consistent Body. And this happens not as in the precipitation of *Benjamin*, and some other Resinous Bodies, which being dissolv'd in Spirit of Wine, may, by the effusion of fair Water, be

turn'd into a seemingly Milky substance. For this whiteness belongs not to the whole Liquor, but to the Corpuscles of the dissolv'd Gum, which after a while subsiding leave the Liquor transparent, themselves onely remaining white: Whereas in our case, 'tis from the vary'd texture of the whole formerly transparent fluid Body, and not from this or that part, that this whiteness results: For the Body is white thorowout, and will long continue so; and yet may, in process of time, without any addition, be totally reduc'd into a transparent Body as before.

But besides the Conjecture insisted on all this while, we grounded another upon the following Observation, which was, That having convey'd some smoke into our Receiver plac'd against a Window, we observ'd, that upon the exsuction of the Air, the Corpuscles that were swimming in it, did manifestly enough make the Receiver seem more opacous at the very moment of the rushing out of the Air: For considering that the whiteness, whose cause we inquire of, did but sometimes appear, it seem'd not impossible but that at such times the Air in the Receiver might abound with Particles, capable of reflecting the Light in the manner requisite to exhibit a white colour, by their being put into a certain unusual Motion. As may be in some measure illustrated by this, That the new motion of the freshly mention'd Fumes, made the inside of the Receiver appear somewhat darker then before: And partly by the nature of our formerly mention'd smoking Liquor, whose parts, though they seem'd transparent whil'st they compos'd a Liquor, yet when the same Corpuscles, upon the unstopping of the Glass, were put into a new motion, and dispos'd after a new manner, they did opacate that part of the Air they mov'd in, and exhibited a greater whiteness then that which sometimes appears in our Pneumatical Vessel. Nor should we content ourselves with this single Instance, to manifest, That little Bodies, which being rang'd after one manner, are Diaphanous and Colourless, may, by being barely agitated, dispers'd, and consequently otherways rang'd, exhibit a colour, if we were not unwilling to rob our Collection of Experiments concerning Colours.

But,

But, My Lord, I foresee You may make some Objections against our proposed ghes, which perhaps I shall scarce be able to answer, especially, if You insist upon having me render a Reason why our *Phenomenon* appears not constantly.

I might indeed answer, that probably it would do so, if instead of our great Receiver we use such a small Viol as we have lately mention'd, wherein the Diffilition of the Air being much greater, is like to be the more conspicuous: Since I remember not that we ever made our tryal with such small Vessels, without finding the expected whiteness to appear. But it would remain to be explicated, why in our great Receiver the *Phenomenon* should sometimes be seen, and oftentimes not appear. And though that Conjecture which we last made should not be rejected, yet if we were farther press'd to assign a reason why the Air should abound with such Particles, as we there suppose, more at one time than another, we are not yet provided of any better Answer, then this general one, That the Air about us, (and much more that within the Receiver,) may be much alter'd by such causes as few are aware of: For, not to repeat those probable Arguments of this Assertion which we have occasionally mention'd here and there in the former part of this Epistle, we will here set down two or three Instances to verifie the same Proposition. First, I finde that the Learned *Josephus Acosta*, among other Judicious Observations he made in *America*, hath this concerning the Effects of some Winds: *There are* (saith he) *Winds which naturally trouble the Water of the Sea, and make it green, and black; others, clear as Crystal.* Next, we have observ'd, That though we convey'd into the Receiver our Scales, and the *Pendula* formerly mention'd, clean and bright; yet after the Receiver had been empty'd, and the Air let in again, the glosse or lustre both of the one, and of the other, appear'd tarnish'd by a beginning rust. And in the last place, we will subjoyn an Observation we made some Years ago, which hath been heard of by divers Ingenious Men, and seen by some of them: We had, with pure Spirit of Wine, drawn a Tincture out of a certain Concrete which useth to be reckon'd among Mineral Bodies;

*Joseph: A-
costa: Nat:
& Mor:
Hyst. of the
Indies, lib.
3. cap. 9.*

Bodies ; And this Tincture being very pure and transparent, we did, because we put a great value upon it, put into a Chrystal Viol which we carefully stopp'd, and lock'd up in a Press among some other things that we specially priz'd. This Liquor being a Chymical Rarity, and besides very defecate, and of a pleasing Golden colour ; we had often occasion to look upon it, and so to take notice, that one time it seem'd to be very much troubled, and not clear as it was wont to be : Whereupon we imagined, that though it would be something strange, yet it was not impossible, that some Precipitation of the Mineral Corpuscles was then happening, and that thence the Liquor was opacated. But, finding after some days that though the expected Precipitation had not been made, yet the Liquor, retaining its former vivid Colour, was grown clear again as before ; we somewhat wondered at it, and locking it up again in the same Press, we resolv'd to observe, both whether the like changes would again appear in our Tincture ; and whether in case they should appear, they would be ascribable to the alterations of the Weather. But though, during the greatest part of a Winter and a Spring, we took pleasure to observe, how the Liquor would often grow turbid, and after a while clear again : Yet we could not finde that these Mutations depended upon any that were manifest in the Air, which would be often dark and clouded, when the Tincture was clear and transparent ; as on the other side, in clear Weather the Liquor would appear sometimes troubled, and more opacous. So that being unable to give an account of these odde changes in our Tincture (which we suppose we have not yet lost, though we know not whether it hath lost its fickle Nature) either by those of the Air, or any thing else that occur'd to our thoughts ; we could not but suspect, that there may be in divers Bodies, as it were Spontaneous Mutations, that is, such changes as depend not upon manifest Causes. But, My Lord, what hath been all this while said concerning our *Phenomenon*, is offer'd to You, not as containing a satisfactory account of it, but to assist You to give Your self one.

EXPERI.

EXPERIMENT XXXVIII.

WE took a Glass Vessel, open at the top, and into it we put a mixture of Snow and common Salt, (such a mixture as we have in another Treatise largely discoursed of) and into the midst of this mixture we set a Glasse, of a Cylindrical form, closely stopp'd at the lower end with Plaister, and open at the upper, at which we fill'd it with common Water. These things being let down into the Receiver, and the Pump being set on work, the Snow began to melt somewhat faster then we expected; Whether upon the account of the Exsuction of the Air, or because there was but little of the Snow, or whether for any other Reason, it appear'd doubtful. But however, by that time the Receiver had been considerably exhausted, which was done in less then $\frac{1}{4}$ of an hour, we perceived the Water near the bottom of the Glass Cylinder to Freeze, and the Ice by a little longer stay, seem'd to encrease, and to rise somewhat higher then the surface of the surrounding Liquor, whereinto almost all the Snow and Salt were resolv'd. The Glass being taken out, it appear'd that the Ice was as thick as the inside of the Glass it fill'd, though into that I could put my Thumb. The upper surface of the Ice was very concave, which whether it were due to any unheeded accident, or to the exsuction of the Air, we leave to be determin'd by farther tryal. And lastly, the Ice held against the Light, appear'd not destitute of Bubbles, though some By-standers thought they were fewer then would have been found if the Water had been frozen in the open Air. The like Experiment we try'd also another time in one of our small Receivers, with not unlike success.

And on this occasion, My Lord, give me leave to propose a Problem, which shall be this: Whence proceeds that strange force that we may sometimes observe in frozen Water, to break the Bodies that Imprison it, though hard and solid? That there is such a force in Water expos'd to Congelation, may be gathered not onely from what may be often observ'd in Winter, of
the

the bursting of Glasses too close stopp'd, fill'd with Water or aqueous Liquors, but by Instances as much more considerable as less obvious. For I remember, that an Ingenious Stone-cutter not long since complain'd to me, That sometimes, through the negligence of Servants, the Rain being suffered to soak into Marble Stones, the supervening violent Frosts would burst the Stones, to the Possessor's no small damage. And I remember another Tradesman, in whose House I had Lodgings, was last Winter complaining, that even Implements made of Bell metal, being carelessly expos'd to the wet, have been broken and spoil'd by the Water, which, having gotten into the little Cavities and Crannies of the Metal, was there afterwards frozen and expanded into Ice. And to these Relations, we can adde one of the formerly mention'd *Cabeus's*, whereby they not onely may be confirm'd, but are surpass'd: For he tells us, That he saw a huge Vessel of exceeding hard Marble, split asunder by congeal'd Water, whose rarefaction, saith our Author, prov'd so vehement, that the hardness of the Stone yielded to it; and so a Vessel was broken, which would not have been so by 100 Yoke of Oxen drawing it several ways. I know, My Lord, that to solve this Problem, it will be said, That Congelation doth not (as is commonly, but erroneously presum'd) reduce Water into less room than it possess'd before, but rather makes it take up more. And I have eliewhere prov'd by particular Experiments, That whether or no Ice may be truly said to be Water rarefi'd (for that seems questionable) it may be said to take up more room than the Water did before Glaciation. But though we grant that freezing makes Water swell, yet, how cold (which in Weather-Glasses manifestly condenseth the Air) should expand either the Water, or the intercepted Air so forcibly, as to perform such things as we have newly related, will yet remain a Problem.

In lib. 4.
Meteor
Arist.

EXPERIMENT XXXIX.

WE took an Oval Glass, clear and (least it should break) pretty strong, with a short Neck at the obtuser end; through

through this Neck, we thrust almost to the bottom, a Pipe of Glass, which was closely cemented to the newly mention'd Neck, the upper part of which Pipe, was drawn in some places more slender then a Crows Quill, that the changes of the Air in that Glass Egge might be the more conspicuous; Then there was convey'd into the Glass five or six Spoon-fulls of Water, part of which, by blowing Air into the Egge, was rais'd into the above-mention'd slender part of the Pipe, so that the Water was interpos'd between the external Air, and that included in the Egge. This Weather-glass (delineated in the fourteenth Figure) was so plac'd, and clos'd up in the cavity of one of our small Receivers, that onely the slender part of the Pipe, to the height of four or five Inches, passing thorow a hole in the Cover remain'd expos'd to the open Air.

The Pump being set a work, upon the Exsuction of the Air, the Water in the Pipe descended about a quarter of an Inch, and this upon two or three reiterated tryals; which seem'd sufficiently to argue, that there was no heat produc'd in the Receiver upon the exsuction of the Air: For even a little heat would probably have been discover'd by that Weather-glass, since upon the bare application of my hand to the out-side of the Receiver, the warmth having after some time been communicated or propagated through both the Glasses, and the interval betwixt them, to the Imprison'd Air, did so rarefie that, as to inable it, by pressing upon the subjacent Water, to impel that in the Pipe very many times as far as it had fallen downwards upon the exsuction of the Air.

Yet shall not we conclude, that in the cavity of the Receiver the cold was greater after the Exsuction of the Air then before.

For if it be demanded what then could cause the fore-mention'd subsiding of the water? it may be answered, That probably it was the reaching of the Glass Egge, which, upon the Exsuction of the ambient Air, was unable to resist altogether as much as formerly the pressure of the included Air, and of the Atmosphere.

sphere, which, by the intervention of the Water, press'd upon its concave surface: Which seem'd probable, as well by what was above deliver'd, in the Experiment about the breaking of the Glass by the force of the Atmosphere; as by this notable Circumstance (which we divers times observ'd) That when by drawing the Air out of the Receiver, the Water in the Pipe was subsided, upon the re-admission of the external Air, to press against the convex surface of the Egge, the water was presently re-impell'd to its former height: Which would perhaps appear less strange to Your Lordship, if You had yet seen, what we have heretofore taught in another Treatise, concerning the Spring that may be discover'd in Glass, as rigid and inflexible a Body as it is generally esteem'd. And in the mean while it may serve the turn, to cause a Glass Egge to be blown exceeding thin; and then, having broken it, try how far you can by degrees bend some narrow parts of it; and how readily, upon the removal of what kept it bent, it will restore it self to its former state or posture. But to return to our Experiment: From thence it seems probable, either that there succeeds no Body in the room of the Air drawn out of our Receiver; or that it is not every Matter that is subtle enough readily to pass through the Pores of Glass, that is always agitated enough to produce Heat where ever it is plentifully found. So that if no *Vacuum* be to be admitted, this Experiment seems to invite us to allow a great disparity, either as to bulk, or as to agitation, or as to both, betwixt some parts of the Etherial Substance, and those that are wont here below to produce Heat and Fire.

We try'd also what Operation the drawing out of the Air would have upon Camphire, that being a Body, which, though not a Liquor, consists of such Volatile or Fugitive parts, that without any greater agitation then that of the open Air it self, they will copiously flie away. But we found not that even this loose Body was sensibly alter'd by the Exsuction of the ambient Air.

EXPERI-

EXPERIMENT XL.

IT may seem well worth trying, whether or no in our exhausted Glass the want of an ambient Body, of the wonted thickness of Air, would disable even light and little Animals, as Bees, and other winged Insects, to fly. But though we easily foresaw how difficult it would be to make such an Experiment; yet not to omit our endeavors: We procur'd a large Flesh fly, which we convey'd into a small Receiver. We also another time shut into a great Receiver a Humming Bee, that appear'd strong and lively, though we had rather have made the tryall with a Butter-fly, if the cold Season would have permitted us to finde any. * The Fly, after some Exsuctions of the Air, dropp'd down from the side of the Glass whereon she was walking: But, that the Experiment with the Bee might be the more instructive, we convey'd in with her a bundle of Flowers, which remain'd suspended by a string near the upper part of the Receiver: And having provok'd the Bee, we excited her to flie up and down the capacity of the Vessel, till at length, as we desir'd, she lighted upon the Flowers; whereupon we presently began to draw out the Air, and observ'd, That though for some time the Bee seem'd to take no notice of it, yet within a while after she did not flie, but fall down from the Flowers, without appearing to make any use of her Wings to help her self. But whether this fall of the Bee, and the other Insect, proceeded from the mediums being too thin for them to flie in, or barely from the weakness, and as it were swooning of the Animals themselves, you will easily gather from the following Experiment.

** Since the writing of this XLth Experiment, we procur'd a white Butter-Fly, and inclosed it in one of our smaller Receivers, where, though at first he fluttered up and down, yet presently, upon the exsuction of the Air, he fell down as in a swoon, retaining no other motion then some little trembling of the wings.*

EXPERIMENT XLI.

TO satisfy our selves in some measure, about the account upon which Respiration is so necessary to the Animals, that Nature hath furnished with Lungs, we took (being then unable

to procure any other lively Bird, small enough to be put into the Receiver) a Lark, one of whose Wings had been broken by a shot of a Man, that we had sent to provide us some Birds for our Experiment ; but notwithstanding this hurt , the Lark was very lively , and did, being put into the Receiver, divers times spring up in it to a good height. The Vessel being hastily , but carefully clos'd , the Pump was diligently ply'd , and the Bird for a while appear'd lively enough ; but upon a greater Exsuction of the Air , she began manifestly to droop and appear sick, and very soon after was taken with as violent and irregular Convulsions, as are wont to be observ'd in Poultry, when their heads are wrung off: For the Bird threw her self over and over two or three times, and dyed with her Breast upward , her Head downwards, and her Neck awry. And though upon the appearing of these Convulsions, we turn'd the Stop-cock, and let in the Air upon her , yet it came too late ; whereupon , casting our eyes upon one of those accurate Dyals that go with a *Pendulum* , and were of late ingeniously invented by the Noble and Learned *Hugenius* , we found that the whole Tragedy had been concluded within ten Minutes of an hour, part of which time had been imploy'd in cementing the Cover to the Receiver. Soon after we got a Hen-sparrow which being caught with Bird-lime was not at all hurt ; when we put her into the Receiver, almost to the top of which she would briskly raise her selfe , the Experiment being try'd with this Bird , as it was with the former , she seemed to be dead within seven minutes , one of which were imployed in cementing on the Cover: But upon the speedy turning of the Key , the fresh Air flowing in , began slowly to revive her , so that after some pantings she opened her eyes , and regain'd her feet, and in about a $\frac{1}{4}$ of an hour after , threaten'd to make an escape at the top of the Glas, which had been unstopped to let in the fresh Air upon her: But the Receiver being closed the second time , she was killed with violent Convulsions, within five Minutes from the beginning of the Pumping.

A while after we put in a Mouse, newly taken, in such a Trap

as had rather affrighted then hurt him; whil'st he was leaping up very high in the Receiver, we fasten'd the Cover to it, expecting that an Animal used to live in narrow holes with very little fresh Air, would endure the want of it better then the lately mentioned Birds: But though, for a while after the Pump was set a work, he continued leaping up as before; yet, 'twas not long ere he began to appear sick and giddy, and to stagger: after which he fell down as dead, but without such violent Convulsions as the Bird dyed with. Whereupon, hastily turning the Key, we let in some fresh Air upon him, by which he recovered, after a while, his senses and his feet, but seemed to continue weak and sick: But at length, growing able to skip as formerly, the Pump was plyed again for eight minutes, about the middle of which space, if not before, a very little Air by a mischance got in at the Stop-cock; and about two minutes after that, the Mouse divers times leap'd up lively enough, though after about two minutes more he fell down quite dead, yet with Convulsions far milder then those wherewith the two Birds expired. This alacrity so little before his death, and his not dying sooner then at the end of the eighth minute, seemed ascribable to the Air (how little soever) that slipt into the Receiver. For the first time, those Convulsions (that, if they had not been suddenly remedied, had immediately dispatch'd him) seized on him in six minutes after the Pump began to be set a work. These Experiments seemed the more strange, in regard that during a great part of those few minutes the Engine could but considerably rarefie the Air (and that too, but by degrees) and at the end of them there remained in the Receiver no inconsiderable quantity; as may appear by what we have formerly said of our not being able to draw down Water in a Tube, within much less then a Foot of the bottom: with which we likewise consider'd, that by the exsuction of the Air and interspersed Vapors, there was left in the Receiver a space some hundreds of times exceeding the bigness of the Animal, to receive the fuliginous Steams, from which, expiration discharges the Lungs; and, which in the other cases hitherto known, may be suspected,

for want of room, to stifle those Animals that are closely pen'd up in too narrow Receptacles.

I forgot to mention, that having caus'd these three Creatures to be open'd, I could, in such small Bodies, discover little of what we sought for, and what we might possibly have found in larger Animals; for though the Lungs of the Birds appear'd very red, and as it were inflam'd, yet that colour being usual enough in the Lungs of such winged Creatures, deserves not so much our notice, as it doth, That in almost all the destructive Experiments made in our Engine, the Animals appear'd to die with violent Convulsive Motions: From which, whether Physicians can gather any thing towards the Discovery of the Nature of Convulsive Distempers, I leave to them to consider.

Having proceeded thus far, though (as we have partly intimated already) there appear'd not much cause to doubt, but that the death of the fore-mention'd Animals proceeded rather from the want of Air, than that the Air was over-clogg'd by the steams of their Bodies, exquisitely pen'd up in the Glass; yet I, that love not to believe any thing upon Conjectures, when by a not over-difficult Experiment I can try whether it be true or no, thought it the safest way to obviate Objections, and remove Scruples, by shutting up another Mouse as close as I could in the Receiver, wherein it lived about three quarters of an hour; and might probably have done so much longer, had not a *Virtuoso* of quality, who in the mean while chanc'd to make me a Visit, desir'd to see whether or no the Mouse could be kill'd by the exsuction of the ambient Air, whereupon we thought fit to open, for a little while, an intercourse betwixt the Air in the Receiver, and that without it, that the Mouse might thereby (if it were needfull for him) be refresh'd, & yet we did this without uncementing the Cover at the top, that it might not be objected, that perhaps the Vessel was more closely stopp'd for the exsuction of the air then before.

The Experiment had this event, that after the Mouse had liv'd ten Minutes, (which we ascrib'd to this that the Pump, for want of having been lately Oyled, could move but slowly, and
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could not by him that manag'd it, be made to work as nimbly as it was wont) at the end of that time he dy'd with Convulsive Fits, wherein he made two or three bounds into the Air, before he fell down dead.

Nor was I content with this, but for Your Lordships farther satisfaction, and my own, I caused a Mouse, that was very hungry, to be shut in all Night, with a Bed of Paper for him to rest upon: And to be sure that the Receiver was well clos'd, I caus'd some Air to be drawn out of it, whereby, perceiving that there was no sensible leak, I presently re-admitted the Air at the Stopcock, lest the want of it should harm the little Animal; and then I caused the Engine to be kept all Night by the Fire side, to keep him from being destroyed by the immoderate cold of the Frosty Night. And this care succeeded so well, that the next Morning I found that the Mouse not onely was alive, but had devour'd a good part of the Cheese that had been put in with him. And having thus kept him alive full twelve hours, or better, we did, by sucking out part of the Air, bring him to droop, and to appear swell'd; and by letting in the Air again, we soon reduc'd him to his former liveliness.



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*A Digression containing some Doubts
touching Respiration.*

I Fear Your Lordship will now expect, that to these Experiments I should adde my Reflections on them, and attempt, by their assistance, to resolve the Difficulties that occur about Respiration; since at the beginning I acknowledg'd a farther Enquiry into the Nature of that, to have been my Design in the related Tryals. But I have yet, because of the inconvenient Season of the Year, made so few Experiments, and have been so little satisfied by those I have been able to make, that they have hitherto made Respiration appear to me rather a more, then a less Mysterious thing, then it did before. But yet, since they have furnished me with some such new Considerations, concerning the use of the Air, as confirms me in my Diffidence of the Truth of what is commonly believ'd touching that matter; That I may not appear sullen or lazy, I am content not to decline employing a few hours in setting down my Doubts, in presenting Your Lordship some Hints, and in considering whether the Tryals made in our Engine, will at least assist us to discover wherein the Deficiency lies that needs to be supplied.

And this, My Lord, being all my present Design, I suppose You will not expect that (as if You knew not, or had forgotten what Anatomists are wont to teach) I should entertain You with a needless Discourse of the Organs of Respiration, and the variety of their Structure in severall Animals; though if it were necessary, and had not been perform'd by others, I should think, with *Galen*, that by treating of the Fabricks of living Bodies, I might compose Hymns to the wise Author of Nature, who, in the excellent contrivance of the Lungs, and other parts of (those admirable Engines) Animals, manifests himself to be indeed what the Eloquent Prophet most justly speaks him, *Wonderfull in Council, and excellent in working.*

Nor shall we any farther meddle with those Controversies so much agitated among the Moderns, namely, *Whether the motion of the Lungs in Respiration be their own, or but consequent to the motion of the Thorax, Diaphragme, and (as some Learned Men would have it) the Abdomen; And, Whence it is that the Air swells the Lungs in Inspiration,* any farther then they may receive light from our Engine: But that it may appear what kinde of service it is that may be expected from it on this occasion, we must premise a few Words to shew wherein the strength of the Objection we are to answer, lies: In favor then of those that would have the Lungs rather passive then active in the business of Respiration, it may against the common opinion be alledg'd, That as the Lungs being destitute of Muscles and of Fibres, are unfit to dilate themselves; so it appears, that without the motion of the *Thorax* they would not be fill'd with Air. Since as our Learned Friend Dr. *Highmore* hath well (and congruously, to what our selves have purposely tryed) observed, if a live Dog have a great wound made in his Chest, the Lobes of the Lungs on that side of the *Mediastinum* will subside and lie still; the *Thorax* and the Lobes on the other side of the *Mediastinum*, continuing their former motion. And if suddenly at once the Muscles of the Chest be on both sides dissected, upon the Ingress of the Air, the whole Lungs, though untouch'd, will remain moveless, at least, as to any expansion or contraction of their substance.

To which we may adde the Observation of the diligent *Bartholinus*, who affirms the like of the *Diaphragme* also, namely, That it being wounded, the Lungs will fall together, and the Respiration cease, which my Experiments oppose not, provided the Wound be any thing great. And indeed the *Diaphragme* seems the principal Instrument of ordinary and gentle Respiration, although to restrained Respiration (if I may so call it) the intercostall Muscles, and perhaps some others may be allowed eminently to concur. But the chief of the Controversies formerly pointed at, is not yet decided, namely, *what it is that conveys*
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the Air into the Lungs. For when, to counterballance all that hath been alleadg'd, those that plead for the Lungs demand what it is that should bring the Air into the Lungs, if themselves do not attract it, their Antagonists disagree about the Reply. For when to this question some of the best Modern Philosophers answer, That by the dilatation of the Chest the contiguous Air is thrust away, and that pressing upon the next Air to it, and so onwards, the Propulsion it continued till the Air be driven into the Lungs, and so dilate them: When this (I say) is answered, it is Objected even by *Bartholine* himself, as a convincing Reply, that, according to this Doctrine, a Man could not fetch his Breath from a great Vessel full of Air, with a slender Neck, because, that when his Mouth covers the Orifice of the Neck, the dilatation of his *Thorax* could not propell the Air in the Vessel into his Lungs, by reason of its being separated by the inclosing Vessel from the ambient Air; and yet, say they, Experience witnesseth, that out of such a Vessel a Man may suck Air. But of this difficulty our Engine furnisheth us with an easie Solution, since many of the former Experiments have manifested, That in the case proposed, there needs not be made any (though 'tis true that in ordinary Respiration there is wont to be made some) propulsion of the Air by the swelling *Thorax* or *Abdomen* into the Lungs; since upon the bare Dilatation of the *Thorax*, the Spring of that internal Air, or halituous substance that is wont to possess as much of the Cavity of the Chest as the Lungs fill not up, being much weaken'd, the external and contiguous Air must necessarily press in at the open Winde-Pipe into the Lungs, as finding there less resistance then any where else about it.

And hence (by the way) we may derive a new assistance to judge of that famous Controversie disputed among Naturalists and Physitians, ever since *Galens* time, some maintaining that the Chest, with the contained Lungs, may be resembled to a pair of Bellows, which comes therefore to be fill'd because it was dilated: And others pleading to have the comparison made to a Bladder, which is therefore dilated because it is fill'd. For as to
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the *Thorax*, it seems evident from what hath been lately said, that it, like a pair of Bellows, happens to be partly fill'd with Air, but because it was dilatad: But as for the Lungs themselves, who want Fibres to distend them, they may fitly enough be compar'd to a Bladder; since they are dilated by being fill'd, namely, by that Air which rusheth into them upon the dilatation of the Chest, in whose increased Cavity it findes (as we freshly noted) less resistance to its Spring then elsewhere. And this brings into my minde that strange Observation of *Nicolaus Fontanus*, a Physitian at *Amsterdam*, who testifieth, That in a Boy of the same Town, four years old, there was found, instead of Lungs, a certain Membranous Bladder; which being fill'd with Wind, and furnish'd with little Veins, had its origination from the Wind-Pipe it self; which being suppos'd true, how well it will agree with most of the Opinions touching Respiration, I leave to be consider'd.

And thus may the grand Objection of *Bartholine*, and others, be answered: But I leave to Anatomists to consider what is to be said to some Observations that seem to contradict those Anatomical Experiments already mention'd: Such was particularly that which I remember I have read in *Sennertus* (from the observation of his Father-in-law *Schato*) of a Melancholy Student, who having stabb'd himself, and pierced the *Diaphragme* in the thinner or tendinous part (call'd by many the Nervous Circle) lived seven Moneths after he had wounded himself, though after his death (preceded by violent Vomiting) the Wound (perchance dilated by those strainings) appear'd so great, that the whole Stomack was found to have got in by it into the left side of the *Thorax*. And such also was the accident that happen'd to a Noble Man, whom I remember I have seen, and who is yet alive, in whose Chest there hath, for these many years, remain'd a hole so great, that the motion of his Heart may be perceiv'd by it. These (I say) and some other Observations, I shall now forbear to insist on, because I hold it not unfit, before we come to consider the use of Respiration, that we acquaint Your Lordship with an Ingenious Conjecture, that was made at the cause of the hasty death of the Animals

imals our Engine kill'd: namely, That it was not the want of Air that destroy'd them, but the Pressure of the innate Air in the cavity of the Chest; as if the Spring of this Air being no longer counterballanc'd by the ambient Air, was thereby become so strong, that it kept the *Thorax* forcibly distended, and hinder'd its wonted contraction; & so compressed the Lungs & their Vessels, as to obstruct the Circulation of the Blood. And this Conjecture, as it is specious enough, so I might have admitted it for true; but that I consider'd, that (not to mention that one, especially of the Animals kill'd in our Engine, seemed manifestly for a pretty while, and not long before he dy'd, to move his *Thorax*, as if he exercis'd Respiration) the diligent *Wallens* relates, That he divers times observed, in the Dissection of live Bodies, that the Membrane that invests the Lungs, had Pores in it as big as the larger sort of Peas; which agreeth with the Observations of Chyrurgions and Physitians, *viz.* That matter collected in the *Thorax*, hath penetrated into the Lungs, & been discharged by coughing. And I remember too, that most of the Animals we kill'd in our Engine were Birds, of whose Lungs *Harvey* somewhere informs us, That he observ'd them very manifestly to open at their Extremities into the *Abdomen*: And by such perforations we may well suppose the passage free betwixt the external Air, and that in the *Abdomen*: But this Conjecture may be farther consider'd. Besides, to show that the Animals that dyed in our Glasses, need not be supposed to have been kill'd by the want of Air, we foresee another Argument that we must deal so ingeniously with Your Lordship, as not to conceal. You very well know, that besides the generality of the Schools, there are many new Philosophers who, though they dissent from the old Peripateticks in other things, do, as they, deny the possibility of a *Vacuum*; and hold, that those spaces which are devoid of Air, and other grosser Bodies, are all of them exactly replenished with a certain Etheriall Matter, so thin and subtle, that it can freely permeate the Pores of the compactedst and closest Bodies, and ev'n of Glais it self. Now some of those Naturalists that are of this persuasion

swasion may object, That the Animals that died in our Receiver, did so, not so much for lack of Air, as by reason that the Air that was pump'd out was necessarily succeeded by an Ethereal Substance; which consisting of parts vehemently agitated, and so very small, as without resistance to pass in and out through the very Pores of Glass; it may well be supposed, that a considerable quantity of this restless and subtle Matter, meeting together in the Receiver, with the excessive heat of it, may be quickly able to destroy a little Animal, or at least, make the Air too intemperately hot to be fit for Respiration.

But though this be a Difficulty not so easily to be resolved without the assistance of our Engine, yet I suppose we have already answer'd the Objection by our 38th and 39th Experiments; which, though we made partly for other purposes, yet we premis'd them onely to clear up the difficulty proposed.

Another suspicion we should have entertain'd concerning the death of our Animals, namely, That upon the sudden removal of the wonted pressure of the ambient Air, the warm Blood of those Animals was brought to an Effervescence or Ebullition, or at least so vehemently expanded, as to disturb the Circulation of the Blood, and so disorder the whole Oeconomy of the Body. (This (I say) I should have had some suspicion of) but that Animals of a hot Constitution are not the sole ones that cannot in our exhausted Engine exercise the Function of Life. But I must not now dwell upon matters of this nature, because I think it high time to proceed to the consideration of the principall subject of our Engine, namely, The use of Respiration; or rather, The use of the Air in Respiration. For whereas of the divers uses of it mentioned by Anatomists the most, such as the Production and Modulation of the Voice by the Elision of the Air, the *Larynx* &c. the expulsion of Excrements by Coughing, the conveying in of Odours by Inspiration, and some others, rather convenient for the well being of an Animal, then absolutely necessary to his Life: Whereas (I say) the other uses are such as we have said, The great *Hippocrates* himself gives this notable Testimony to

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the use of the Air, as to Animals endow'd with Lungs: *Mortalibus* (saith he) *hic* (spiritus) *tum vitæ, tum morborum agrotis causa est. Tantâque corporibus omnibus spiritûs inest necessitas, ut siquidem aliis omnibus & cibis & potionibus, quis abstineat, duos tamen aut tres, vel plures dies possit vitam ducere: At si quis spiritus in corpus vias intercipiat, vel exiguâ diei parte, homini pereundum sit; Adeo necessarius est usus spiritûs in corpore. Ad hæc quoque, quum omnibus aliis actionibus homines quiescant, quod mutationibus innumeris vita sit exposita, ab hæc tamen solâ actione nunquam desistant animantia, quin aut spiritum adducant, aut reddant.*

But touching the account upon which the Inspiration and Expiration of Air (both which are comprehended in ἀναπνοή, Respiration) is so necessary to Life, both Naturalists and Physicians do so disagree, that it will be very difficult either to reconcile their Opinions, or determine their Controversies.

For first, Many there are who think the chief (if not sole) use of Respiration to be the cooling and tempering of that Heat in the Heart and Blood, which otherwise would be immoderate: And this Opinion, not onely seems to be most received amongst Scholastick Writers, but divers of the new Philosophers, Cartesians, and others, admitted with some variation; teaching, That the Air is necessary, by its coldness, to condense the Blood that passeth out of the right Ventricle of the Heart into the Lungs, that thereby it may contain such a consistence, as is requisite to make it fit Fuel for the vital fire or flame, in the left Ventricle of the Heart. And this Opinion seems favor'd by this, That Fishes, and other cold Creatures, whose Hearts have but one cavity, are also unprovided of Lungs, and by some other considerations. But though it need not be deny'd, that the inspir'd Air may sometimes be of use by refrigerating the Heart; yet (against the Opinion that makes this Refrigeration, the most genuine and constant use of the Air) it may be Objected, That divers cold Creatures (some of which, as particularly Frogs, live in the Water) have yet need of Respiration, which seems not likely to be needed for Refrigeration by them that are destitute of any sensible heat, and besides, live in the cold

cold Water: That even decrepid old Men, whose natural Heat is made very languid, and almost extinguish'd by reason of Age, have yet a necessity of frequent Respiration: That a temperate Air is fittest for the generality of breathing Creatures; and as an Air too hot, so also an Air too cold, may be inconvenient for them (especially, if they be troubled with an immoderate degree of the same Quality which is predominant in the Air:) That in some Diseases the Natural heat is so weaken'd, that in case the use of Respiration were to cool, it would be more hurtful then beneficial to Breath; and the suspending of the Respiration, may supply the place of those very hot Medicines that are wont to be employ'd in such Distempers: That Nature might much better have given the Heart but a moderate heat, then such an excessive one, as needs to be perpetually cool'd, to keep it from growing destructive; which the gentle, and not the burning heat of an Animals Heart, seems not intense enough so indispensably to require. These, and other Objections, might be oppos'd, and press'd against the recited Opinion: But we shall not insist on them, but onely adde to them, That it appears not by our foregoing Experiments (I mean the 38th and 39th) that in our exhausted Receiver, where yet Animals die so suddenly for want of Respiration, the ambient Body is sensibly hotter then the common Air.

Other Learned Men there are, who will have the very substance of the Air to get in by the Vessels of the Lungs, to the left Ventricle of the Heart, not onely to temper its heat, but to provide for the generation of Spirits. And these alledge for themselves the Authority of the Ancients, among whom *Hippocrates* seems manifestly to favor their Opinion; and both *Aristotle* and *Galen* do sometimes (for methinks they speak doubtfully enough) appear inclinable to it. But for ought ever I could see in Dissections, it is very difficult to make out, how the Air is convey'd into the left Ventricle of the Heart, especially the *Systole* and *Diastole* of the Heart and Lungs being very far from being Synchronical: Besides, that the Spirits seeming to be but the most subtle and unctuous Particles of the Blood, appear to be of a very differing Nature

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Nature from that of the lean and incombustible Corpuscles of Air. Other Objections against this Opinion have been proposed, and press'd by that excellent Anatomist, and my Industrious Friend, Doctor *Highmore*, to whom I shall therefore refer you.

Another Opinion there is touching Respiration, which makes the genuine use of it to be Ventilation not of the Heart, but of the Blood, in its passage through the Lungs; in which passage, it is dis-burthened of those Excrementitious Steams, proceeding, for the most part, from the superfluous Serosities of the Blood, (we may adde) and of the *Chyle* too, which (by those new Conduits of late very happily detected by the famous *Pecquet*) hath been newly mix'd with it in the Heart.) And this Opinion is that of the Industrious *Mæbius*, and is said to have been that of that excellent Philosopher *Gassendus*; and hath been in part an Opinion almost vulgar: But this *Hypothesis* may be explicated two ways: For first, The necessity of the Air in Respiration, may be suppos'd to proceed from hence; That as a Flame cannot long burn in a narrow and close place, because the Fuliginous Steams it uncessantly throws out, cannot be long receiv'd into the ambient Body; which, after a while, growing too full of them to admit any more, stifles the flame: So that the vital Fire in the Heart requires an ambient Body, of a yielding nature, to receive into it the superfluous Serosities, and other Recrements of the Blood, whose seasonable Expulsion is requisite to depurate the Mass of Blood, and make it fit both to Circulate, and to maintain the vital heat residing in the Heart. The other way of explicating the above-mentioned *Hypothesis*, is, by supposing, that the Air doth not onely, as a Receptacle, admit into its Pores the Excrementitious vapors of the blood, when they are expell'd through the Wind-pipe, but doth also convey them out of the lungs, in regard that the inspired Air, reaching to all the ends of the *Aspera Arteria*, doth there associate it self with the exhalations of the circulating Blood, and when 'tis exploded, carries them away wth it self: as we see that winds speedily dry up the surfaces of wet Bodies, not to say any thing

thing of what we formerly observed touching our Liquor, whose fumes were strangely elevated upon the Ingresse of the Air.

Now of these two ways of Explicating the use of Respiration, our Engine affords us this Objection against the first; That upon the Exsuction of the Air, the Animals die a great deal sooner than if it were left in the Vessel; though by that Exsuction the ambient space is left much more free to receive the steams that are either breathed out of the Lungs of the Animal, or discharg'd by insensible Transpiration through the Pores of his Skin.

But if the *Hypothesis* propos'd, be taken in the other sense, it seems congruous enough to that grand observation, which partly the *Phænomena* of our Engine, and partly the relations of Travellers, have suggested to us: namely, That there is a certain consistence of Air requisite to Respiration; so that if it be too thick, and already over-charged with vapors, it will be unfit to unite with, and carry off those of the Blood, as Water will dissolve, and associate to it selfe but a certain proportion of saline Corpuscles; and if it be too thin or rarefied, the number or size of the Aërial Particles is too small to be able to assume and carry off the halituous Excrements of the Blood, in such plenty as is requisite.

Now that Air too much thicken'd (and as it were clogg'd) with Steams, is unfit for Respiration, may appear by what is wont to happen in the Lead-Mines of *Devonshire*, (and, for ought I know, in those too of other Countrys, though I have seen Mines where no such thing was complain'd of) for I have been informed by more than one credible Person (and particularly by an Ingenious Man, that hath often, for curiosity, digg'd in those Mines, and been imploy'd about them) that there often riseth Damps, as retaining the *Germane* Word by which we call them) which doth so thicken the Air, that unless the Work-men speedily make signs to them that are above, they would (which also sometimes happens) be presently stifled for want of Breath, and though their Companions do make haste to draw them up, yet fre-

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frequently, by that time they come to the free Air, they are, as it were in a swoon, and are a good while before they come to themselves again. And that this swooning seems not to proceed from any Arsenical or Poysonous Exhalation contain'd in the Damp, as from its over-much condensing the Air, seems probable from hence; That the same Damps oftentimes leisurely extinguish the flames of their Candles or Lamps; and from hence also that it appears (by many Relations of Authentical Authors) that in those Cellars where great store of new Wine is set to work, men have been suffocated by the too great plenty of the steams exhaling from the Must, and too much thickning the Air: as may be gathered from the custom that is now used in some hot Countries, where those that have occasion to go into such Cellars, carry with them a quantity of well kindled Coals, which they hold near their Faces; whereby it comes to pass, that the Fire dissipating the Fumes, and rarefying the Air, reduceth the ambient Body to a consistence fit for Respiration.

We will adde (by way of confirmation) the following Experiment: In such a small Receiver, as those wherein we kill'd divers Birds, we carefully clos'd up one, who, though for a quarter of an hour he seem'd not much prejudiced by the closeness of his Prison, afterwards began first to pant very vehemently, and keep his Bill very open, and then to appear very sick; and last of all, after some long and violent strainings, to cast up some little matter out of his Stomack: which he did several times, till growing so sick, that he stagger'd and gasp'd, as being just ready to die. We perceiv'd, that within about three quarters of an hour from the time that he was put in, he had so thickened and tainted the Air with the Steams of his Body, that it was become altogether unfit for the use of Respiration: Which he will not much wonder at, who hath taken notice in *Sanctorius* his *Statica Medicina*, how much that part of our Aliments, which goeth off by insensible Transpiration, exceeds in weight all the visible and grosser Excrements both solid and liquid.

That (on the other side) an Air too much dilated is not serviceable

viceable for the ends of Respiration, the hasty death of the Animal we kill'd in our exhausted Receiver, seems sufficiently to manifest. And it may not irrationally be doubted, whether or no, if a Man were rais'd to the very top of the Atmosphere, he would be able to live many minutes, and would not quickly dye for want of such Air as we are wont to breath here below. And that this Conjecture may not appear extravagant, I shall on this occasion subjoyn a memorable Relation that I have met with in the Learned *Josephus Acosta*, who tells us, That when he himself past the high Mountains of *Peru*, (which they call *Pariacaca*) to which, he sayes, That the *Alps* themselves seem'd to them but as ordinary Houses, in regard of high Towers, he and his Companions were surpris'd with such extream Pangs of Straining and Vomiting, (not without casting up Blood too) and with so violent a Distemper, that he concludes he should undoubtedly have dyed, but that this lasted not above three or four hours, before they came into a more convenient and natural temperature of Air: To which our Learned Author addes an Inference, which being the principal thing I design'd in mentioning, the Narrative I shall set down in his own Words: *I therefore (says he) perswade my self, That the Element of the Air is there so subtle and delicate, as it is not proportionable with the breathing of Man, which requirs a more gross and temperate Air; and I believe it is the cause that doth so much alter the Stomack, and trouble all the Disposition.* Thus far our Author, whose Words I mention, that we may guess by what happens somewhat near the Confines of the Atmosphere (though probably far from the surface of it) what would happen beyond the Atmosphere. That, which some of those that treat of the height of Mountaines, relate out of *Aristotle*, namely, That those that ascend to the top of the Mountain *Olympus*, could not keep themselves alive, without carrying with them wet Sponges, by whose assistance they could respire in that Air, otherwise too thin for Respiration: (That Relation I say) concerning this Mountain) would much confirm what hath been newly recited out of *Acosta*, if we had sufficient reason to believe it: But I confess, I am very diffi-

dent of the truth of it; partly because when I pass'd the *Alps*, I took notice of no notable change betwixt the consistence of the Air at the top and the bottom of the Mountain; partly because in a punctual relation made by an English Gentleman, of his ascension to the top of the Pike of *Tenariff* (which is by great odds higher then *Olympus*) I find no mention of any such difficulty of breathing; and partly also because the same Author tells us out of *Aristotle*, That upon the top of *Olympus* there is no motion of the Air, insomuch, that Letters traced upon the dust, have been, after many years, found legible and not discompos'd; whereas that Inquisitive *Busbequius* (who was Ambassador from the *German* to the *Turkish* Emperor) in one of his Eloquent Epistles, tells us, upon his own knowledge, That *Olympus* may be seen from *Constantinople*, blanch'd with perpetual Snow; which seems to argue, That the top of that, as well as of divers other tall Hills, is not above that Region of the Air wherein *Meteors* are formed. Though otherwise, in that memorable Narrative which *David Fralichius*, made of his ascent to the top of the prodigiously high *Hungarian Mountain Carpathus*: he tells us, That when, having pass'd through very thick Clouds, he came to the very top of the Hill, he found the Air so calm and subtle, that not a hair of his head mov'd, whereas in the lower Stages of the Mountain he felt a vehement Wind. But this might well be casual, as was his, having a clear Air where he was, though there were Clouds, not onely beneath him, but above him.

Epist. 3.

Fralichius
apud *Va-*
ren: Geo-
gra: Gener:
lib. 1. cap.
19.

But (though what hath been hitherto discours'd, incline us to look upon the Ventilation and Depuration of the Blood, as one of the principal and constant uses of Respiration; yet) methinks it may be suspected that the Air doth something more then barely help to carry off what is thrown out of the Blood in its passage through the Lungs, from the right Ventricle of the Heart to the left. For we see, in Phlegmatick Constitutions and Diseases, that the Blood will circulate tolerably well, notwithstanding its being excessively serous: And in Asthmatical Persons, we often see, that though the Lungs be

be very much stuff'd with tough Phlegm, yet the Patient may live some Moneths, if not some Years. So that it seems scarce probable, that either the want of throwing out the superfluous *Serum* of the Blood for a few Moments, or the detaining it, during so short a while, in the Lungs, should be able to kill a perfectly sound and lively Animal: I say, for a few moments, because, that having divers times try'd the Experiment of killing Birds in a small Receiver, we commonly found, that within half a minute of an hour, or thereabouts, the Bird would be surpris'd by mortal Convulsions, and within about a minute more would be stark dead, beyond the Recovery of the Air, though never so hastily let in. Which sort of Experiments seem so strange, that we were obliged to make it several times, which gain'd it the Advantage of having Persons of differing Qualities, Professions and Sexes, (as not only Ladies and Lords, but Doctors and Mathematicians) to witness it. And to satisfie Your Lordship, that it was not the narrowness of the Vessel, but the sudden exsuction of the Air that dispatched these Creatures so soon; we will adde, That we once inclos'd one of these Birds in one of these small Receivers, where, for a while, he was so little sensible of his Imprisonment, that he eat very chearfully certain seeds that we conveyed in with him, and not onely lived ten minutes, but had probably lived much longer, had not a great Person, that was Spectator of some of these Experiments, rescu'd him from the prosecution of the Trial. Another Bird being within about half a minute, cast into violent Convulsions, and reduced into a sprawling condition, upon the Exsuction of the Air, by the pittie of some Fair Lady's (related to Your Lordship) who made me hastily let in some Air at the Stop-cock, the gasping Animal was presently recovered, and in a condition to enjoy the benefit of the Lady's compassion. And another time also, being resolved not to be interrupted in our Experiment, we did, at night, shut up a Bird in one of our small Receivers, and observ'd that for a good while he so little felt the alteration of the Air, that he fell asleep with his head under his wing; & though he afterwards awak'd sick, yet he continu'd upon his legs between

tween forty minutes and three quarters of an hour ; after which, seeming ready to expire ; we took him out, and soon found him able to make use of the liberty we gave him for a compensation of his sufferings.

If to the foregoing Instances of the sudden destruction of Animals, by the removal of the ambient Air, we should now annex some, that we think fitter to reserve till anon ; perhaps Your Lordship would suspect, with me, that there is some use of the Air which we do not yet so well understand, that makes it so continually needful to the Life of Animals. *Paracelsus* indeed tells us, *That as the Stomack concocts Meat, and makes part of it useful to the Body, rejecting the other part, so the Lungs consume part of the Air, and proscribe the rest.* So that according to our Hermetick Philosopher (as his followers would have him stil'd) it seems we may suppose that there is in the Air a little vital Quintessence (if I may so call it) which serves to the refreshment and restauration of our vital Spirits, for which use the grosser and incomparably greater part of the Air being unerviceable, it need not seem strange that an Animal stands in need of almost incessantly drawing in fresh Air. But though this Opinion is not (as some of the same Author) absur'd, yet besides that, it should not be barely asserted, but explicated and prov'd; and besides that, some Objections may be fram'd against it, out of what hath been already argu'd against the Transmutation of Air into vital Spirits: Besides these things, it seems not probable, that the bare want of the Generation of the wonted quantity of vital Spirits, for less then one minute, should within that time be able to kill a lively Animal, without the help of any external violence at all.

But yet, on occasion of this Opinion of *Paracelsus*, perhaps it will not be impertinent, if before I proceed, I acquaint Your Lordship with a Conceit of that deservedly Famous Mechanician and Chymist, *Cornelius Drebell*, who among other strange things that he perform'd, is affirmed (by more then a few credible Persons) to have contrived for the late Learned King *James*, a Vessel to go under Water; of which, tryal was made in the *Thames*, with

with admired success, the Vessel carrying twelve Rowers, besides Passengers ; one of which is yet alive, and related it to an excellent Mathematician that inform'd me of it. Now that for which I mention this Story, is, That having had the curiosity and opportunity to make particular Enquiries among the Relations of *Drebell*, and especially of an Ingenious Physitian that marryed his daughter, concerning the grounds upon which he conceived it feasible to make men unaccustomed to continue so long under Water without suffocation, or (as the lately mention'd Person that went in the Vessel affirms) without inconvenience. I was answer'd that *Drebell* conceiv'd, that 'tis not the whole body of the Air, but a certain Quintessence (as Chymists speake) or spirituous part of it, that makes it fit for respiration, which being spent, the remaining grosser body, or carcase (if I may so call it) of the Air, is unable to cherish the vital flame residing in the heart : So that (for ought I could gather) besides the Mechanicall contrivance of his vessell he had a Chymicall liquor, which he accounted the chiefe Secret of his submarine Navigation. For when from time to time he perceived, that the finer and purer part of the Air was consumed, or over clogg'd by the respiration, and steames of those that went in his ship, he would, by unstopping a vessell full of this Liquor, speedily restore to the troubled Air such a proportion of Vital parts, as would make it again for a good while fit for Respiration, whether by dissipating, or precipitating the grosser Exhalations, or by some other intelligible way, I must not now stay to examine, contenting my self to add, that having had the opportunity to do some service to those of his Relations, that were most Intimate with him, and having made it my business to learne what this strange Liquor might be, they constantly affirm'd that *Drebell* would never disclose the Liquor unto any, nor so much as tell the matter whereof he made it, to above one Person, who himself assured me what it was.

This account of *Drebell's* performance, I mention, not that I any farther assent to his opinion then I have already intimated, but because the man, and the Invention being extraordinary, I suppose

Your

Your Lordship will not be displeas'd to know the utmost I could learne about it; especially not having found it mentioned by any Writer. Wherefore I have been sometimes inclin'd to favourable thoughts of their opinion, who would have the Air necessary to ventilate, and cherish the vitall flame, which they do suppose to be continually burning in the heart. For we see, that in our Engine the flame of a Lamp will last almost as little after the Exsuction of the Air, as the life of an Animal: Nay I remember, that though I devis'd a more promising way, to make a fire last in our exhausted Receiver, yet it would not succeed: We tooke a hard body made in the forme of a Clove, but twice as long, and proportionably thick, this body being made of such a Composition, that if it be kindl'd at the upper end, it will most certainly burn away to the very bottome, much better then a Match; we convey'd it divers times kindl'd at the upper end, into one of our small Receivers, but still found, that though presently upon the Exsuction of the Air, it would leave smoking, and seem quite gone out, and again begin to smoak as soon as the Air was let in upon it; yet if the Air were kept out but four or five minutes, the fire would be totally, and irrevocably extinguish'd. To which we will adde, that though we convey'd into a great Receiver, a small lamp with rectifi'd spirit of Wine, that being so pure as not to smut the Cotton weck, or so much as a piece of white Paper held over it; yet we could not by divers tryalls make the flame last a couple of minutes after the Air was begun to be drawn out. But though our Engine thus shews us a new kind of resemblance betwixt fire and life: yet the opinion we have last mentioned is not free from Difficulties. For, though in the hearts of many Animals, Blood be a warm liquor, and in some even a hot one; yet it is not easie to conceive either how the Air (in substance) can get thither, or how, in case it could, it were able to increase the heat. Since, however, the Air may encrease the heat of a coale by blowing off the ashes, & making the active Corpuscles pierce farther into the kindled body, and shatter it the more yet we see hot liquors have their heat allayed, & not augmented, by having Air blown on them. And
 whereas

whereas some Eminent Naturalists think it not inconvenient, to make the heat residing in the heart to be a true flame, provided they adde, that 'tis such a temperate, and almost insensible fire, as the flame of spirit of Wine, which will long burne upon fine white Linnen or Paper without consuming either: give me leave to wish that they had been more curious to make differing tryals with that liquor. For (as we observe in another Treatise) the reason why a Linnen cloth, dipped in common Spirit of Wine, is not burnt by the flame of it, is, because the Phlegm of the Liquor defends the Cloth: And the Flame of Spirit of Wine is so far from being too weak to burn a piece of Paper, or of Linnen, that I have us'd it in Lamps to distill Liquors out of tall Cucurbites, and found that the Spirit burned away indeed much faster then Sallet Oyl, but gave at least as great a heat: Nay, I have, for curiosity sake, melted crude Gold, and that readily enough, with the bare Flame of pure Spirit of Wine.

But not to press this any farther, we will, on this occasion, venture to subjoyn an odde Observation, which may perhaps invite to a farther Enquiry into the Opinion we have for Discourse sake oppos'd. Our English *Democritus*, Dr. *Harvy*, proposeth this difficult and noble Problem to Anatomists, *Why a fætus, even out of the Womb, if involv'd in the secundines, may live a good while without Respiration; but in case after having once began to breath, its respiration be stopp'd, it will presently die.* We are far from pretending to solve so hard a Problem, but this we try'd in relation to it; We took a Bitch that was said to be almost ready to whelp, and having caused her to be hang'd, we presently open'd her *Abdomen*, and found four Puppeys in her Womb; one of these we took out, and having freed him from the Teguments that involv'd him, and from the Liquor he swam in, we observed that he quickly opened his Mouth very wide, moved his Tongue, and exercised Respiration; then we opened both his *Abdomen* and his Chest, and cut assunder the *Diaphragme*, notwithstanding which, he seem'd often to endeavor Respiring, & mov'd in a notable manner, both the Intercostal Muscles, part of the *Diaphragme*, the Mouth.

Mouth and the Tongue: But that which we mention this Puppy for, was this, That being desirous to try whether the other yong ones that had not yet breath'd at all, would long survive this or no; we took them also out of the Womb, and having open'd them found none of them so much alive, as to have any perceptible motion in his heart, whereas the heart of that Puppy which had once enjoy'd the benefit of Respiration, continued beating so long, that we our selves observ'd the Auricle to beat, after five or six hours; and a Servant that staid up and watch'd it after we were gone to Bed, affirm'd, That he saw the Pulsation continue about 2 hours longer. I shall leave it to others to make Reflections upon this Observation, compar'd with Dr. *Harvey's* Problem.

It is much doubted, whether Fishes breath under Water, and we shall not take upon us, as yet, to determine the Question either way, because we have not yet been able to procure little Fishes alive to make Experiments upon: That such as are not Setaceous (for such manifestly breath) have not Respiration, properly so called, such as is exercised by four footed Beasts, and Birds, may be argu'd from their having no cavity in their Hearts, & from their want of Lungs, whence they are observed to be Mute; unless we say, what is not altogether absurd, That their Gills seem somewhat Analogous (as to their use) to Lungs. But that on the other side, Air is necessary to the Lives even of Fishes, and that therefore 'tis probable they have some obscure kind of Respiration, seems manifest by two or three Observations and Experiments, mentioned by divers Authors, who tell us, *That Fishes soon die in Ponds and Glasses quite fill'd with Water; if the one be so frozen over, and the other so closely stopp'd, that the Fishes cannot enjoy the benefit of the Air,* if we allow them to be true. But because these Relations are not wont to be deliver'd by Writers upon their own Knowledge; as I shall not reject them, so I dare not build upon them, till I have opportunity to examine them by experience. In the mean time, we will adde, That our Engine hath taught us two things that may illustrate the matter in hand: The one, That there is wont to lurk in water, many little parcels of interspers'd Air, where-

whereof it seems not impossible that Fishes may make some use, either by separating it, when they strain the Water thorow their Gills, or by some other way: The other, what may be collected from the following Experiment.

We took a large Eele (being able to procure no other Fish alive) and removing it out of the vessel of Water, wherein it was brought us, into our great Receiver, we caus'd the air to be pump'd out; and observ'd, That the Eele, after some motion too and fro in the Glass, seem'd somewhat dis-compos'd; and that when we had prosecuted the Exsuction of the Air somewhat obstinately, she turn'd up her belly, as dying fishes are wont to do, and from thence-forward lay altogether moveless, just as if she were stark dead; and though I did not think her so, yet the continuing in that posture, even after the Cover of the Receiver was taken off (whereby the Air was let in) I should have been of the opinion of the By-standers, if the Diffidence I am wont to exercise in trying Experiments (especially such as are not usual) had not invited me to take the Fish out of the Receiver, upon which she shew'd her self, by her vivid motions, as much alive as before.

But that is most strange which we observ'd of a great, gray, House-Snail (as they call it) which being clos'd up in one of our small Receivers, did not onely not fall down from the side of the Glass, upon the drawing out of the Air. (For that may be ascrib'd to the tenacity of the liquor wherewith Snails use to stick themselves, even to the smoothest Bodies) but was not so much as depriv'd of progressive motion by the recess of the Air: Though, except this Snail, we never put any living Creature into our Receiver, whom it did not either kill, or at least reduce to seem ready to dye. But as we shall not here examine what interest the glutinous, and uneasily dissipable nature of the Juices of Snails, may have on this event; so whether this escape of our Eele be ascrib'd to the particular and vivacious nature of this sort of Fishes; or to this, That the air is not indeed necessary to the life of Fishes; or finally to this, That though these Animals need some Air, yet they need so little, that that which could not be drawn out of the Receiver, might (at least for a while) suffice them, we will not now determine.

Nor are we at leisure to examine that Paradox of *Hippocrates*, which some Learned Physicians have of late reviv'd, namely, *That the Fœtus respire in the womb*: For on the one side it seems very difficult to conceive, how Air should traverse the Body of the Mother, and the Teguments of the Childe: And since Nature hath, in new-born Babes, contriv'd peculiar and Temporary Vessels, that the Blood may circulate thorow other Passages, then it is wont to do in the same Individuals when they come to have the free use of their Lungs, it seems unlikely, that Infants in the Womb do properly respire: But then since our Experiments have manifested, That almost all kinde of Liquors do, as well as Water, abound with interspers'd Corpuscles of Air, it seems not altogether absurd to say, That when the *Fœtus* is grown big, he may (especially the upper part of the involving *Amnios*, being destitute of Liquor, and fill'd onely with an halituous Substance) exercise some obscure Respiration; especially, since 'tis not (as many wise Men think it) a Fable, That Children have been heard to cry in the Mothers Womb: For though it happens exceeding rarely, yet sometimes it hath been observ'd. And I know a young Lady, whose Friends, when she was some Years since with Childe, complain'd to me, that she was several times much frighted with the Cries of her Infant, which, till I disabus'd Her, She and Her Friends look'd upon as Portentous. And such Observations are the more credible, because not onely Houswives, but more judicious Persons, mention it as no very unfrequent thing to hear the Chick pip or cry in the Egg, before the Shell be broken. But this I mention but as a probable, not a cogent Argument, till I can discover whether an Elision of an halituous Substance, though no true Air, may not at the top of the *Larynx* produce a Sound, since I finde that the Blade of a Knife, held in several postures in the stream of Vapors (or rarefied Water) that issues out of an *Æolipile*, will afford various and very audible Sounds.

I had thoughts of conveying into our Receiver young ones, ripped out of the Womb of their Dams, with their involving Coats intire, but could not procure them. And I have also had thoughts of trying whether it be not practicable, to make a Receiver,

ver, though not of Glass, yet vvith little Glass windowvs, so placed, that one may freely look into it, capacious enough to hold a Man, who may observe several things, both touching Respiration, and divers other matters; and who, in case of fainting, may, by giving a sign of his weakness, be immediately reliev'd, by having Air let in upon him. And it seems not impossible, but that by accustomance, some Men may bring themselves to support the want of Air a pretty while, since we see that divers will live, so much longer then other Men, under Water: that those that dive for Pearls in the *West Indies* are said to be able to stay a whole hour under Water. And *Cardan* tells us of one *Colanus* a Diver in *Sicily*, who was able to continue (if *Cardan* neither mistake nor impose upon us) three or four times as long. Not to minde Your Lordship, that You have Your self often seen in *England*, a corpulent Man, who is wont to descend to the bottom of the *Thames*, and bring out of deep holes at the bottom of the Banks, large fishes alive in his hands. And *Acosta* tells us, he saw in *Peru* the like manner of fishing, but more difficult, practised by the *Indians*.

I made mention of some Men, and of *Accustomance*: because there are but very few, who, though they use themselves to it by degrees, are fit to support, for many Minutes, the want of Air. In-
somuch that an ingenious Man of my acquaintance, who is very famous for the useful skill of drawing Goods, and even Ordnance, out of sunk Ships, being asked by me how long he was able to continue at the depth of 50 or 60 foot under Water, without the use of Respiration, confessed to me, that he cannot continue above two minutes of an hour, without resorting to the Air, which he carries down with him in a certain Engine (whereof I can show Your Lordship a description.) Another thing I also learn'd of him by enquiry, that was not despicable: For asking him, whether he found any use of chewing little sponges, dipt in oyl, in his mouth, when he vvas perfectly under Water, and at a distance from his Engine; he told me, that by the help of these sponges he could much longer support the want of his vvanted Respiration, then he vvas able to do vvithout them. The true cause of vvwhich, vvould

perhaps, if discovered, teach us some thing pertinent to the Problem touching the Respiration of Fishes.

But the necessity of Air to the most part of Animals unaccustomed to the want of it, may best be judg'd of, by the following Experiments, which we try'd in our Engine, to discover, whether Insects themselves have not, either Respiration, or some other use of the Air equivalent thereunto.

We took then an humble Bee, one of those common Flyes that are call'd Flesh-flies, and one of those hairy Worms that resemble Caterpillars, and are wont to be call'd Palmer-worms: These three we convey'd into one of our small Receivers, and observ'd, to the great wonder of the Beholders, that not onely the Bee, and the Fly fell down, and lay with their Bellies upwards; but the worm it self seem'd to be suddenly struck dead: all of them being reduc'd to lie without motion, or any other discernable sign of life, within somewhat less (if we mistake not) then one minute of an hour: And this, notwithstanding the smallness of the Animals in proportion to the capacity of the Vessels: which circumstance we the rather mention, because we found that the Vessel was not free from leaks. And to satisfy the Spectators, that 'twas the absence of the Air that caus'd this great and suddain change: we had no sooner re-admitted the Air at the stop-cock, then all the three Insects began to shew signs of life, and by little & little to recover. But when we had again drawn out the Air, their motions presently ceased, and they fell down seemingly dead as before, continuing moveless; as long as, by continuing to pump, the vessel was kept exhausted. This invited us thankfully to reflect upon the wise goodness of the Creator, who by giving the Air a spring, hath made it so very difficult, as Men finde it, to exclude a thing so necessary to Animals: and it gave us also occasion to suspect, that if Insects have no Lungs, nor any part analogous thereunto, the ambient Air affects them, and relieves them at the Pores of their skin, it not being irrational to extend to these Creatures that of *Hippocrates*; who saith, That a living Body is throughout perspirable; or to use his expression, *ἐσπνέει ὅ ἐκπνέει*, dispos'd to admit and part
with

with what is Spirituous: Which may be somewhat illustrated by what we have elsewhere noted, That the moister parts of the Air readily insinuate themselves into, and recede from the pores of the Beards of wilde Oates, and those of divers other wilde Plants; which almost continually wreath & unwreath themselves according to, even, the light variations of the temperature of the ambient air.

This Circumstance of our Experiment we particularly took notice of, that when at any time, upon the ingress of the Air, the Bee began to recover, the first sign of Life she gave, was a vehement panting, which appear'd near the Tail: Which we therefore mention, because we have observ'd the like in Bees drown'd in Water, when they first come to be reviv'd by a convenient heat: As if the Air were in the one case as proper to set the Spirits and Alimental Juice a moving, as heat is in the other; and this may, perchance, deserve a farther consideration.

We may adde, That we scarce ever saw any thing that seem'd so much as this Experiment, to manifest, That even living Creatures (Man always excepted) are a kinde of curious Engines, fram'd and contriv'd by nature (or rather the Author of it) much more skilfully then our gross Tools and imperfect Wits can reach to. For in our present Instance vve see Animals, vivid and perfectly sound, depriv'd immediately of motion, and any discernable signs of life, and reduc'd to a condition that differs from death, but in that it is not absolutely irrecoverable. This (I say) vve see perform'd without any, so much as the least external violence offered to the Engine; unless it be such as is offered to a Wind-Mill, vvhhen the Wind ceasing to blow on the Sails, all the several parts remain moveless & useles, till a nevv Breath put them into motion again.

And this vvas farther very notable in this Experiment; That vvhwhereas 'tis known that Bees and Flies vwill not onely vwalk, but flie for a great vvhile, after their heads are off; and sometimes one half of the Body vwill, for divers hours, vwalk up and dovvn, vvhhen it is sever'd from the other: Yet, upon the exsuction of the Air, not onely the progressive motion of the vvhole Body, but the very motions of the Limbs do forthvwith cease; as if the
presence

presence of the Air were more necessary to these Animals, than the presence of their own Heads.

But it seems, that in these Insects, that fluid Body (whether it be a Juice or Flame) wherein Life chiefly resides, is nothing near so easily dissipable, as in perfect Animals. For vvhwhereas vve have above-recited, That the Birds vve conveyed into our small Receiver vvere vwithin tvvo minutes brought to be past Recovery, vve vvere unable (though by trying him that pump'd) to kill our Insects by the exsuction of the Air: For though, as long as the Pump vvas kept moving, they continued immovable; yet vwhen he desisted from pumping, the Air that press'd in at the unperceiv'd Leaks (did though slowly) restore them to the free exercise of the functions of Life.

But, My Lord, I grow troublesome, & therefore shall pass onto other Experiments: Yet without despairing of your pardon for having entertain'd you so long about the use of Respiration, because it is a subject of that difficulty to be explain'd, and yet of that importance to humane Life, that I shall not regret the trouble my Experiments have cost me, if they be found in any degree serviceable to the purposes to which they were design'd. And though I despair not but that hereafter our Engine may furnish us with divers *Phænomena* useful to Illustrate the Doctrine of Respiration; yet having not, as yet, had the opportunity to make the other trials, of various kinds, that I judge requisite for my Information: I must confess to Your Lordship, that in what I have hitherto said, I pretend not so much to establish, or over-throw this or that *Hypothesis*, as to lay together divers of the Particulars that occur'd to me, in order to a future inquiry. I say, divers of the Particulars, because I could adde many others, but that I want time, and fear that I shall need Your Lordships pardon, for having been so prolix in writing; and that of Physitians (which perhaps I shall more easily obtain) for having invaded Anatomy, a Discipline which they challenge to themselves, and indeed have been the almost sole Improvers of. Without denying then that the inspir'd and exspir'd Air may be sometimes very useful, by condensing and cooling the
Blood

Blood that passeth through the Lungs; I hold that the depuration of the Blood in that passage, is not onely one of the ordinary, but one of the principal uses of Respiration. But I am apt also to suspect, that the Air doth something else in Respiration, which hath not yet been sufficiently explain'd; and therefore, till I have examin'd the matter more deliberately, I shall not scruple to answer the Questions that may be asked me, touching the genuine use of Respiration, in the excellent Words employ'd by the acute *St. Austin*, to one that ask'd him hard Questions: *Mallem quidem (says he) eorum qua à me quaesivisti, habere scientiam quam ignorantiam: sed quia id nondum potui, magis eligo cautam ignorantiam confiteri, quam falsam scientiam profiteri.*

EXPERIMENT XLII.

HAVING (partly upon the consideration of some of the foregoing Experiments, and partly upon grounds not now to be insisted on) entertain'd a suspicion, that the action of Corrosive Liquors in the dissolving of Bodies, may be considerably varied by the gravitation or pressure of the incumbent Air, and the removal of it; I thought fit to examine my Conjecture by the following Experiment.

I tooke whole pieces of red Coral, and cast them into as much Spirit of Vinager, as sufficed to swim above an Inch over them: These substances I made choice of, that the Ebullition upon the Solution might not be too great, and that the operation might last the longer.

Having then put about half-a-score Sprigs of Coral, together with the *Menstruum*, into a somewhat long neck'd Viol, whereof they seem'd scarce to fill a third part, we convey'd that Viol into one of our small *Pneumatical* Glasses, containing by ghes about a Quart of Water; and having fastned on the Cover, after the accustomed manner, we suffered the Liquor to remaine unmov'd a while, to observe whether the *Menstruum* would work upon the Coral otherwise then before. But finding there did onely arise, as formerly, a pretty number of small Bubbles, that made
there

no sensible froth upon the surface of the distill'd Vinager, there where made two or three Exsuctions of the Air; upon which, there emerg'd from the Coral such a multitude of Bubbles, as made the whole Body of the *Menstruum* appear white; and soon after a Froth, as big as all the rest of the Liquor, was seen to swim upon it: And the *Menstruum* plainly appear'd to boil in the Glass, like a seething Pot. And though, if we desisted but one minute from pumping, the decrement of the Froth and Ebullition, upon the getting in of a little Air, at some leak or other, seem'd to argue, that the removal of the pressure of the external Air was the cause, or, at least, the occasion of this effervescence: Yet to evince this the more clearly, we turn'd the Key, and let in the external Air at the Stop-cock; immediately upon whose entrance the Froth vanish'd, and so many of the Bubbles within the body of the Liquor disappear'd, that it lost its whiteness, and grew transparent again: The *Menstruum* also working as languidly upon the Coral, as it did before they were put into the Receiver: But when we had again drawn out the Air, first the whiteness re-appear'd, then the ebullition was renew'd, which, (the pumping being a while longer and nimbly pursued,) grew so great, that for 3 or 4 times one after another, when ever the Air was let out of the Receiver into the empty'd Cylinder, the frothy liquor over-flow'd the Glass, and ran down by the sides of it: And yet, upon the readmitting of the excluded Air, the boiling Liquor grew immediatly as calme and as transparent as at first: As if indeed the operation of it, upon the Coral, had been facilitated by the exsuction of the incumbent Air, which on its recess, left it more easy for the more active parts of the liquor to shew themselves such, then it was whil'st the wonted pressure of the Air continued unremoved. It may indeed be suspected, that those vast and numerous Bubbles proceeded, not from the action of the *Menstruum* upon the Coral, but from the suddain emerfion of those many little parcels of Air that (as we formerly observ'd) are wont to be dispers'd in liquors, without excluding Spirit of Vinagar; but having had this suspition before we try'd the Experiment, we convey'd our distill'd Vinager
alone

alone into the Receiver, and kept it a while there, to free it from its Bubbles (which were but very small) before ever we put the Coral into it. It may be suspected likewise, that the agitation of the Liquor, necessary following upon the shaking of the Glass, by pumping, might occasion the recited Ebullition; but upon tryal made, there appear'd not any notable change in the liquor, or its operation, though the containing Vessel were shaken, provided no Air were suck'd out of it. The former Experiment was another time try'd in another small Receiver, with Coral grossly poudred, and the success was very much alike, scarce differing in any thing, but that the Coral being reduc'd to smaller parts, upon the ebullition of the liquor, so many little lumps of Coral would be carried and Boy'd up by the emerging Bubbles, as sometimes to darken the Viol; though the same Coralline Corpuscles would be let fall again upon the letting in of the Air.

Some thing also we try'd in our great Receiver, concerning the solution of Metals in *Aqua fortis*, and other Corrosive Liquors; but partly the stink, and partly some accidents, kept us from observing any thing peculiar and remarkable about those Solutions.

One thing we must not omit, that when the Spirit of Vinager was boiling upon the Coral, we took off the Cover of the Receiver, and took out the Viol, but could not finde, that notwithstanding so very late an Ebullition, the Liquor had any heat great enough to be at all sensible to our hands.

E X P E R I M E N T XLIII.

WE will now subjoyn an Experiment, which, if the former did not lessen, the wonder of it would probably appear very strange to Your Lordship, as it did to the first Spectators of it.

The Experiment was this: We caus'd Water to be boyl'd a pretty while, that by the heat it might be freed from the latent Air, so often already taken notice of in common Water: Then almost filling with it a Glass Viol, capable of containing near four Ounces of that Liquor; we convey'd it, whilst the Water was yet hot, into one of our small Receivers (big enough to hold about a pound of Water) and having luted on the Cover, we caus'd the Air to be drawn out: Upon the two first exsuctions, there scarce appear'd any change in the Liquor, nor was there any notable alteration made by the third; but at the fourth, and afterwards, the Water appear'd to boyl in the Viol, as if it had stood over a very quick Fire; for the Bubbles were much greater then are usually found upon the Ebullition of very much more Water then was contain'd in our Viol. And this Effervescence was so great in the upper part of the Water, that the Liquor boyling over the top of the Neck, a pretty deal of it ran down into the Receiver, and sometimes continued (though more languidly) boyling there. Prosecuting this Experiment, we observ'd, that sometimes, after the first Ebullition, we were reduc'd to make divers Exsuctions of the Air, before the Liquor would be brought to boyl again. But at other times, as often as the Key was turn'd to let the Air pass from the Receiver into the Pump, the Effervescence would begin a fresh, though the Pump were ply'd for a pretty while together; which seem'd to argue, that the boyling of the Water proceeded from hence, That upon the withdrawing the pressure of the incumbent Air, either the Fiery Corpuscles, or rather the Vapors agitated by the heat in the Water (which last, what we have formerly noted touching the rarefied Water of an *Aolipile*, manifest to be capable

ble of an Elastical Power) were permitted to expand themselves mightily in the evacuated Receiver; and did, in their tumultuous Dilatation, lift up (as the Air is wont to do) the uppermost part of the Water, and turning it into Bubbles, made the Water appear boiling. This conjecture was farther confirm'd by these additional Circumstances: First, The Effervescence was confin'd to the upper part of the Water, the lower remaining quiet, unless the Liquor were but shallow. Next, although sometimes (as is already noted) the Ebullition began again, after it had ceas'd a pretty while, which seem'd to infer, That some concurrent cause (whatever that were) did a little modifie the operation of heat; yet, when the Water in the Viol could by no pumping be brought to boil any more, the self-same Water, being in the very same Viol warm'd again, and reconvey'd into the Pneumatical Glass, was quickly brought to boyl afresh, and that vehemently and long enough; not to mention, that a new parcel, taken out of the same parcel of the boiled Water with the former, and put in cold, could by no pumping be brought to the least shew of Effervescence. Besides, having try'd the Experiment in hot Sallet Oyl, being a much more tenacious Liquor, and requiring a stronger heat to make it boil, could not be brought to an Effervescence in our Receiver; whereas the Chymical Oyl of Turpentine, being thinner and more volatile, was presently made to boyl up, till it reach'd four or five times the former height in the Viol, in whose bottom it lay, and continued boyling till it was almost reduc'd to be but luke-warm. Wine also being a more thin and spirituous Liquor than Water, being convey'd in hot, instead of the Oyl, did, as I remember, at the very first exsuction begin to boyl so vehemently, that, in a short time that the Pump was kept moving, four parts of five, by our ghes, boyl'd over out of the Viol, though it had a pretty long Neck. On which occasion we will adde, that even the Water it self, near one half, would sometimes boyl over into the Receiver before it became luke-warm. And it was also remarkable, that once, when the Air had been drawn out, the Liquor did, upon a single Exsuction, boyl so long with prodigiouly

vast Bubbles, that the Effervescence lasted almost as long as was requisite for the rehearsing of a *Pater Noster*. Now the Experiment having been try'd more then once, and found to succeed as to the main, seems much to countenance the conjecture we made at the beginning of this Letter, where we told Your Lordship, That perhaps the pressure of the Air might have an interest in more *Phænomena* then Men have hitherto thought. For as we had not then made this Experiment, so now we have made it, it seems to teach, That the Air, by its stronger or weaker pressure, may very much Modifie (as the School-men speak) divers of the Operations of that vehement and tumultuous Agitation of the small parts of Bodies, wherein the nature of heat seems chiefly, if not solely, to consist. Insomuch that if a heated Body were convey'd above the Atmosphere, 'tis probable that the heat may have a differing operation, as to the power of dissipating the parts of it, from what it hath here below.

To conclude, This Experiment might have been farther prosecuted, but our want of leisure makes us content our selves to adde at present; That perhaps it would not be lost labor if this were try'd, not onely with other Liquors, but with variety of heated, and especially soft or melted Bodies: But in such cases the Receiver ought to be shap'd, as is most proper to preserve the Cement wherewith the Cover must be fastned on, from being melted by the heat of the included Matter; the inconvenience to be hereby avoided, having befallen us in the use of a Receiver too shallow, though otherwise capacious enough.

The Conclusion.

BEING come thus far, My Dear Lord, not without thoughts of proceeding farther: The unwelcome Importunity of my Occasions becomes so prevalent, that it quite hinders, for the present, my design'd Progress; and reduceth me, not onely to reserve for another opportunity that kinde of Experiments, which, at some distance from the beginning of this Letter, I call'd (as Your Lordship may remember) Experiments of the second sort; but to leave unessay'd some of the first sort, which I might try in the Engine, as it now is, were it not that my Avocations are grown so urgent, for my remove from the place where the Engine was set up, that I am put to write Your Lordship this Excuse, Weary, and in an Inne which I take in my way to my Dearest Brother *Corke*: Who being at length arriv'd in *England*, after I have for divers Years been deprived of his Company, and wish'd for it as long; what ever my other Occasions may be, my first Business must be to wait on Him and Your excellent Mother; in whose grateful Company I may hope to forget a while those publick Calamities that distress this too unhappy Nation. Since that is in-dear'd to me, both by their personal Merit; by the near Relation which Nature gives me to Him, Affinity to Her, and Friendship to both; and also by their many Favors, especially that of my owing them My *Lord of Dungarvan*. But I suffer my self to be transported too far with these delightful thoughts; To return therefore to our Engine. Though I finde this Letter is beyond my expectation swell'd, not onely into a Book, but almost into a Volume; yet the Experiments already mentioned in it, are so far from comprising all those that may be try'd by the help of our Engine, that I have not yet been able to try all those, which, presently occurring to my thoughts, upon my first seeing the working of it, I caus'd to be set down in a Catalogue within less then half an hour. But I doubt I have but too much cause to apprehend

hend that the Affairs, and other things I complain of, have made it needful for me to Apologize, as well for the things I have set down, as for those I am necessitated to omit. For as partial as Men use to be to the Children of their own Brains, as well as to those of their Loins, I must not deny that the foregoing Tryals are not altogether free from such unaccuratenesses, nor the recital of them from such Imperfections, as I my self can now discern, and could, perhaps, partly mend, if I had the leisure to repeat the Experiments, with the Circumstances that have since offer'd themselves to my Thoughts, as things that might have been worth Observation or Enquiry. But the truth is, that I was reduc'd to make these Experiments, when my Thoughts had things that more concern'd me to imploy them, and the same avocations made me set them down, for the most part, assoon as I had made them, and in the same order, and that so fast, that I had not over-frequently the opportunity to minde any more then the bare Truth of what I set down; without allowing it any of those Advantages that Method, Stile, and decent Embellishments, are wont to confer on the Composures they are imploy'd do adorn.

But, My Lord, though to invite and encourage You and Your learned Friends at *Paris*, to make a farther use of this Engine, then I have yet been able to do, I am thus free to acknowledge the Imperfections of the foregoing Letter: yet, if some Intelligent Persons mistake not, by what hath been done, such as it is, there is a way open'd, whereby Sagacious Wits will be assisted to make such farther Discoveries in some points of Natural Philosophy, as are yet scarce dream'd of. And I am the more desirous to engage You to that Imployment, because I am apt to think, that if the Making and Writing of such Experiments shall cost You as much trouble as they have me, You will be inclin'd to excuse me; and if the Discoveries give You as much pleasure as they gave me, You will (perhaps) be invited to thank me. However, I think (My Lord) I may justly pretend, that the things I have set down have been faithfully Recorded, though not elaborately Written; and, I suppose, my former Papers may have long since satisfied
 You

You, that though many devise Experiments better then Your Servant, none perhaps hath related them more carefully and more truly: And particularly of These; sometimes one, sometimes another, hath been performed in the presence of Persons, divers of them eminent for their Writings, and all for their Learning. Wherefore, having in the foregoing Narratives made it my business to enoble them with the chief Requisites of Historical Composures, Candor, and Truth, I cannot dispair that You will either Excuse their Imperfections, or, at least, Forgive them: Especially, considering that this unpolish'd Letter is as well a Production of Your Lordships Commands and my Obedience, as a Testimony of my Desire to make others beholden to My Lord of *Dungarvan*, by the same way by which I endeavour to expresse my Self

*Becons-field, this
20th of Decem-
ber, 1659.*

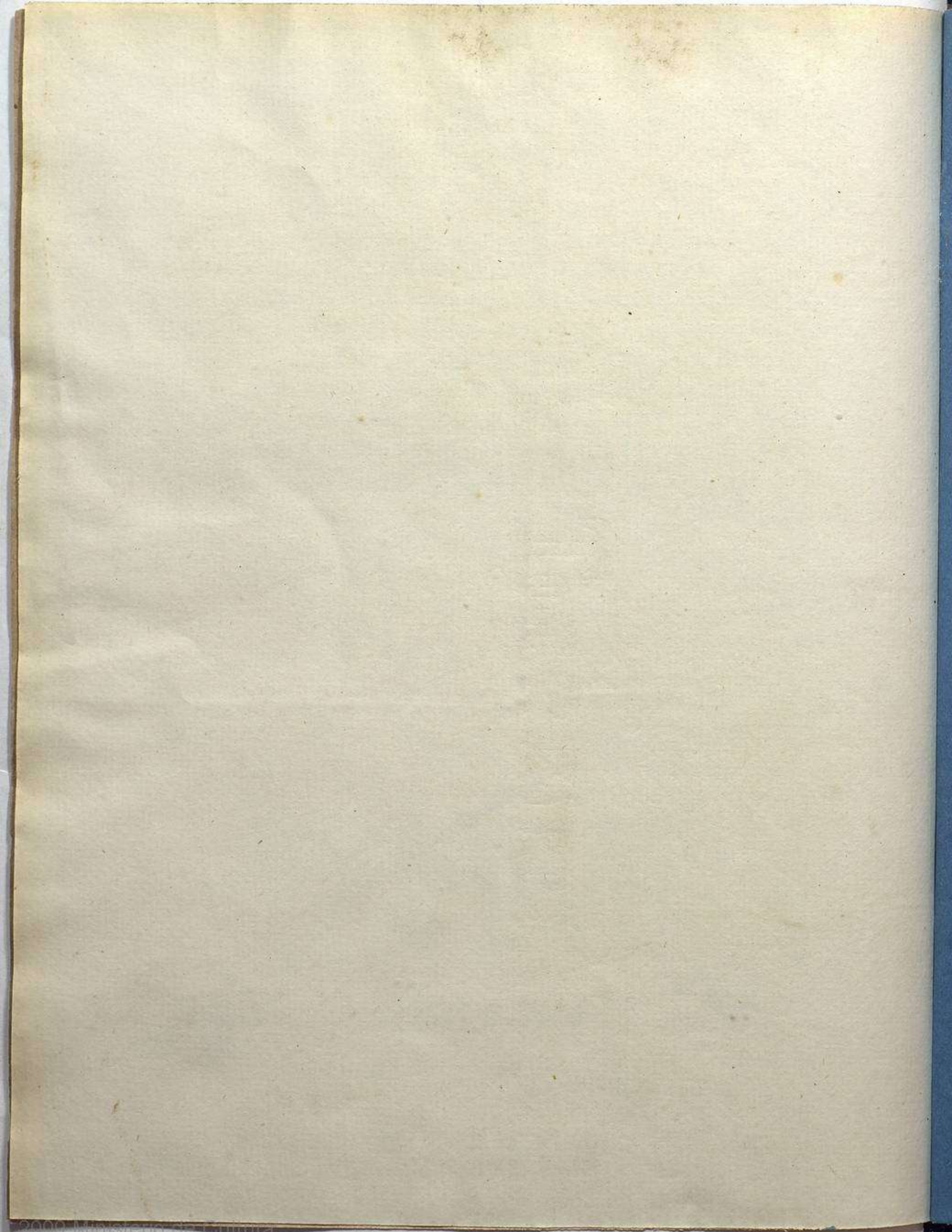
His Lordships

Most obedient Servant,

And

Most Affectionate Uncle,

ROBERT BOYLE.



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