Phyſico-Mechanical EXPERIMENTS On Various Subjects.

CONTAINING

An Account of ſeveral Surprizing *Phenomena*

TOUCHING

Light and *Electricity*,

Producible on the *Attrition* of BODIES.

With many other Remarkable Appearances, not before obſerv'd.

Together with

The Explanations of all the MACHINES, (the Figures of which are Curiouſly Engrav'd on Copper) and other APPARATUS us'd in making the EXPERIMENTS.

By F. HAUKSBEE, F. R. S.

LONDON,

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TO THE

Right Honourable

JOHN

Lord *Sommers*,

Lord-Preſident of Her Majeſties moſt Honourable PRIVY-COUNCIL.

May it pleaſe Your Lordſhip,

IT *would be an Inexcuſable Vanity in Me, to preſume to attempt* Your Lordſhip's *Character; which is ſo univerſally known, that it needs not; and ſo great, that it cannot, receive any Advan-*

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tage from the Descriptions even of the Ablest and most Learned Pens.

Your Lordship *will pardon my Boldness in making this Observation only, That under the greatest Weight of Public Business, and in a Multiplicity of Affairs of the highest Importance, wherein you have so eminently and so steadily promoted the true* Interest of your Country; *your Lordship has nevertheless, by the imployment of a few vacant Hours, exceeded in* Universal Learning, *those whose Lives have been wholly spent in such Studies; and have been pleas'd to become such an Encourager and Promoter of all sorts of Knowledge, that no part of Learning has been considerable in our Age, without being Ambitious of the Patronage of your Lordship's Name.*

I am very sensible, how much the Imperfection of my Performance, and my want of a Learned Education, makes me stand in need of that Favour and Protection, which the Learned'st and most Accomplish'd Writers have been proud of aspiring to.

Particularly, the Honour your Lordship has done the Royal Society, *in being some time their* PRESIDENT; *and the great Skill your Lordship is known to have attain'd to in the* Works of Nature, *no less Eminently than in the Knowledge of* Men, *and of the Nature and Ends of* Government; *have imbolden'd me to lay this small Attempt at your Lordship's Feet: Which having been already read and approv'd before the* Society, *may, with the Addition of your Lordship's Favour and Encouragement, hope to overcome* *all the Diſadvantages of coming from the Hands of ſo undeſerving a Perſon, tho', in true Honour and Eſteem for your Lordſhip's Great Merit, not inferiour to any.*

I am,

My LORD,

With all Humility and Reſpect,

Your LORDSHIP's moſt Obedient

and Faithful Servant,

Fra. Hauksbee.

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PREFACE.

THE Learned World is now almoſt generally convinc'd, that inſtead of amuſing themſelves with *Vain Hypotheſes*, which ſeem to differ little from *Romances*, there's no other way of *Improving* NATURAL PHILOSOPHY, but by *Demonſtrations* and *Concluſions* founded upon *Experiments* judiciouſly and accurately made.

By this courſe; after many Ages had paſs'd, with little or no Progreſs in the *True Knowledge* of the *Nature of Things*, greater Advances have been made within the compaſs of a ſmall number of Years, than was eaſily to be imagin'd, that the moſt Sagacious Men, with their greateſt Induſtry, could ever have been capable of attaining to.

The Honourable and most Excellent Mr. Boyle, by great Variety of Experiments, in almost *every part* of *Philosophy*, gave much Light into the Causes and Operations of Nature; and particularly by the Invention of that most Useful Instrument the *Air-Pump*.

The Principal Subject of the following Papers is, an Account of *Great* and *Further Improvements* of this Noble Machin, the *Air-Pump*, and of many New Experiments made thereby.

By the same Method, the most Learned and Incomparable Sir Isaac Newton has invented and establish'd the Theory of *Light* and *Colours*; and by Demonstrations founded on Experiments and Observations, has at once begun and finish'd that great Discovery, and advanc'd that part of Optics, concerning the Nature of *Light* and *Colours*, of which there was little (if any thing) before known, to a *Perfect* and *Complete Science*.

The *New Experiments* contain'd in the following Treatiſe, concerning the Production and Emiſſion of certain Kinds of *Light* from different Bodies, (hitherto unobſerv'd) may, 'tis preſum'd, give no ſmall Illuſtration to that Matter; and become the Occaſion of many not-unacceptable Diſcoveries, concerning ſeveral particular Circumſtances not included in the general Theory.

The general Laws of *Attraction* and *Repulſe*, common to all Matter, have by the ſame Excellent Perſon been diſcover'd and applied to Wonderful Purpoſes, in eſtabliſhing the true Syſtem of Nature, and explaining the Great Motions in the World. But the *Nature* and *Laws* of *Electrical Attractions* have not yet been much conſider'd by Any: And in the following Obſervations, 'tis hoped, the Reader may meet with many things, which may be of great Uſe in diſcovering ſome of the Wonderful and hitherto Unheeded Effects of this ſtrange Property of Bodies, in ſeveral of the Operations of *Nature*; and poſſibly in the Production and Determination even of *Involuntary Motions* in the

Parts of Animals; of which very little has yet been wrote intelligibly.

If the few Hints and Suggeſtions in this Diſcourſe, ſhall excite the Curioſity of *Ingenious Inquirers*, to make further Search into theſe Matters, my Intention in publiſhing them, which was entirely for the *Improvement* of *Natural Knowledg*, will be compleatly anſwer'd.

F. H.

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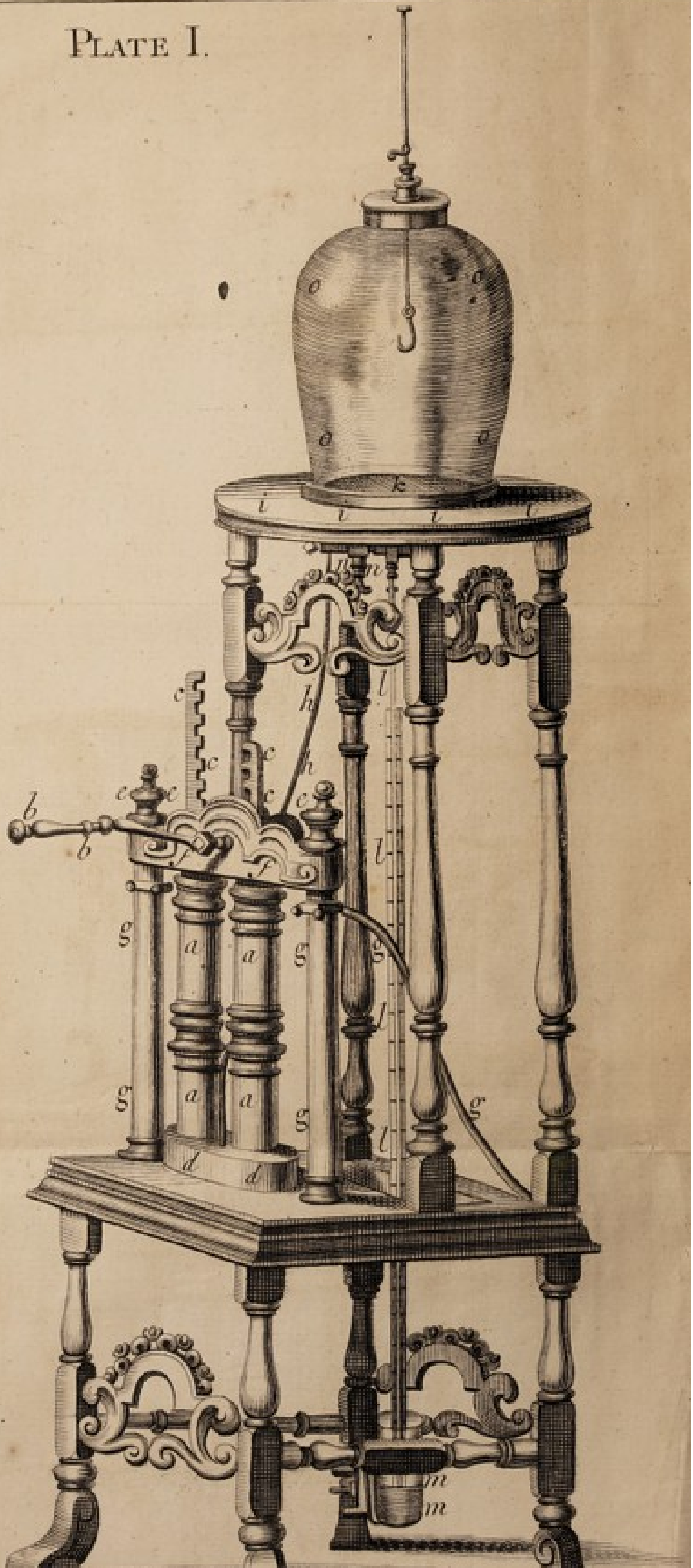

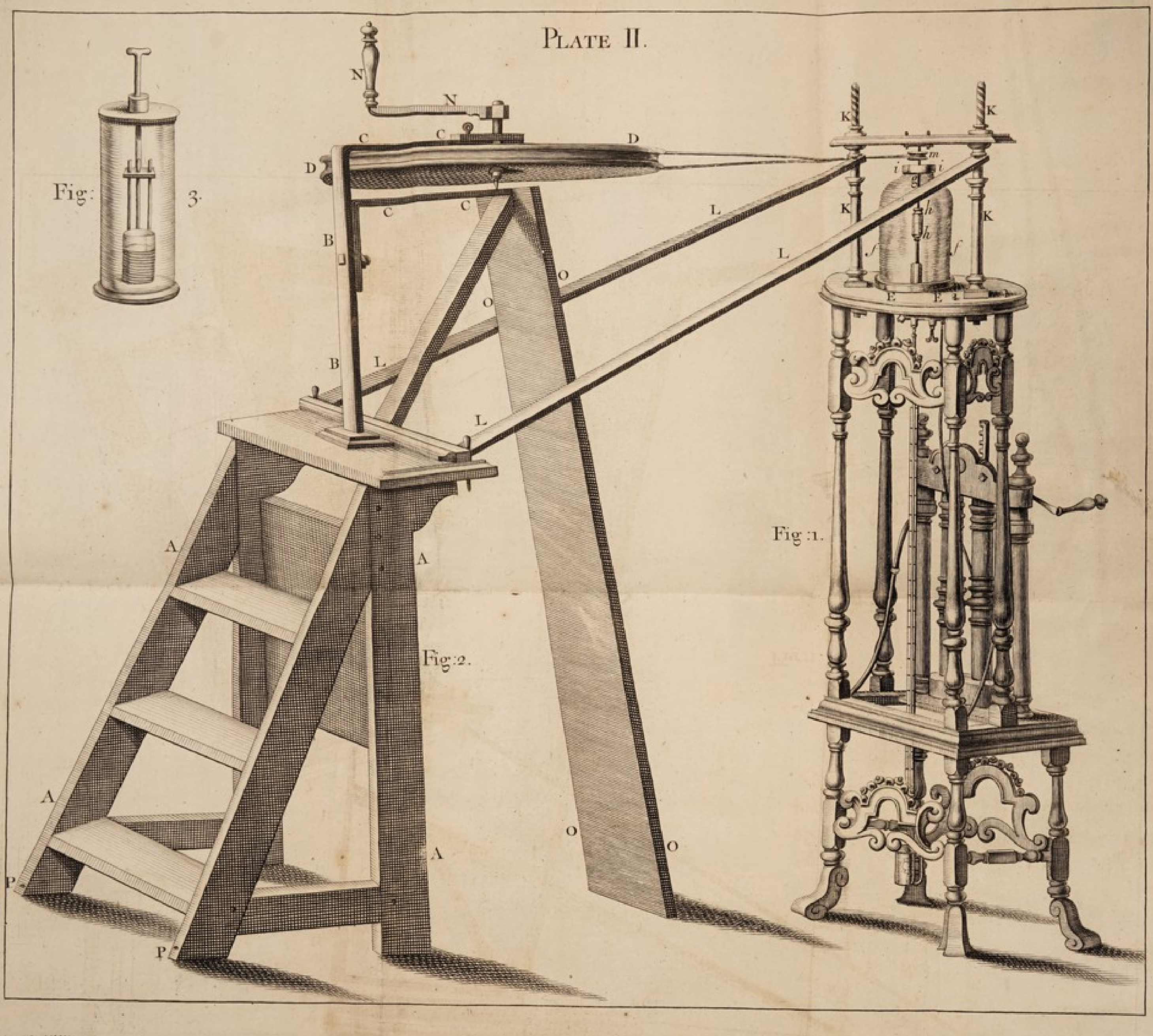
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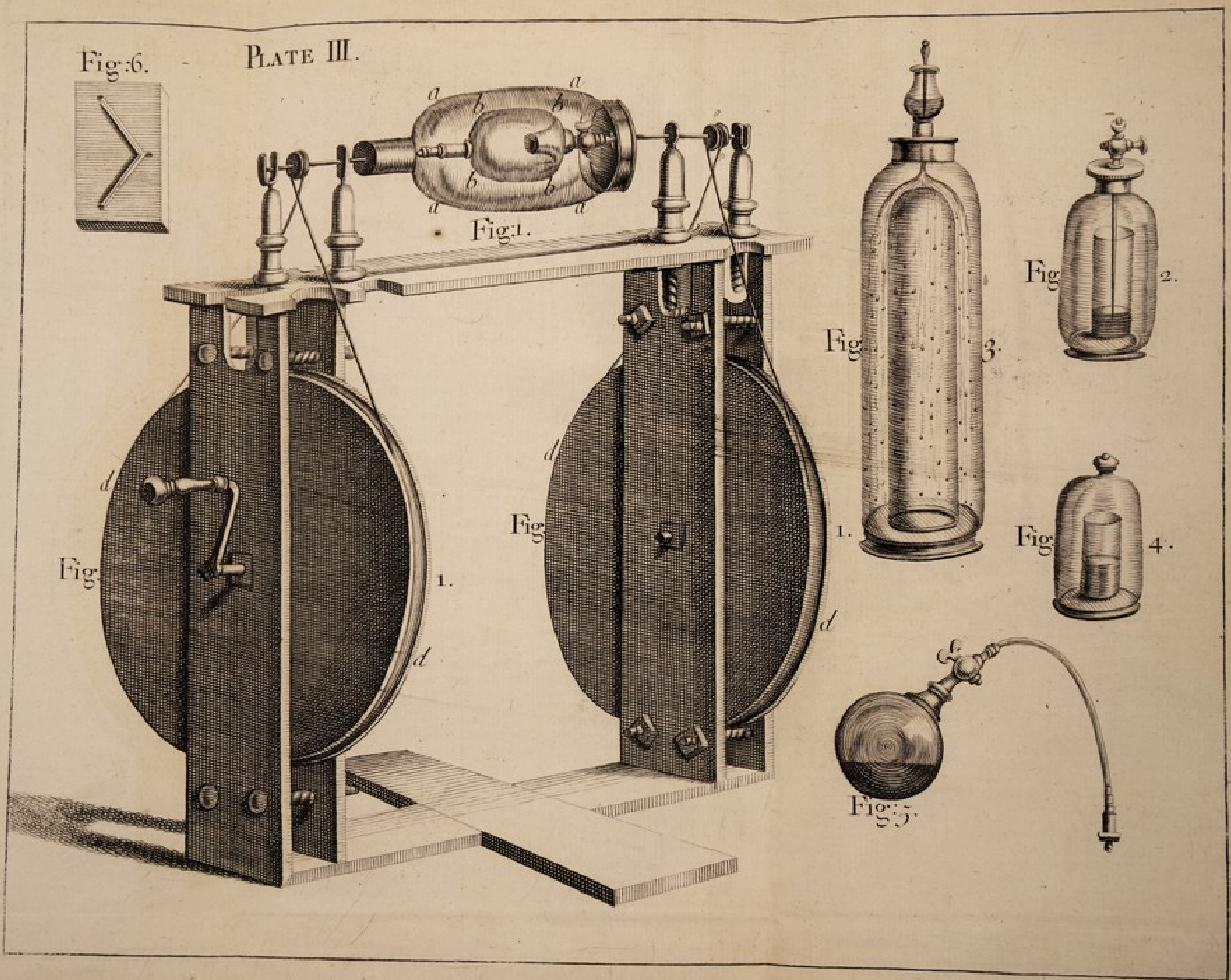
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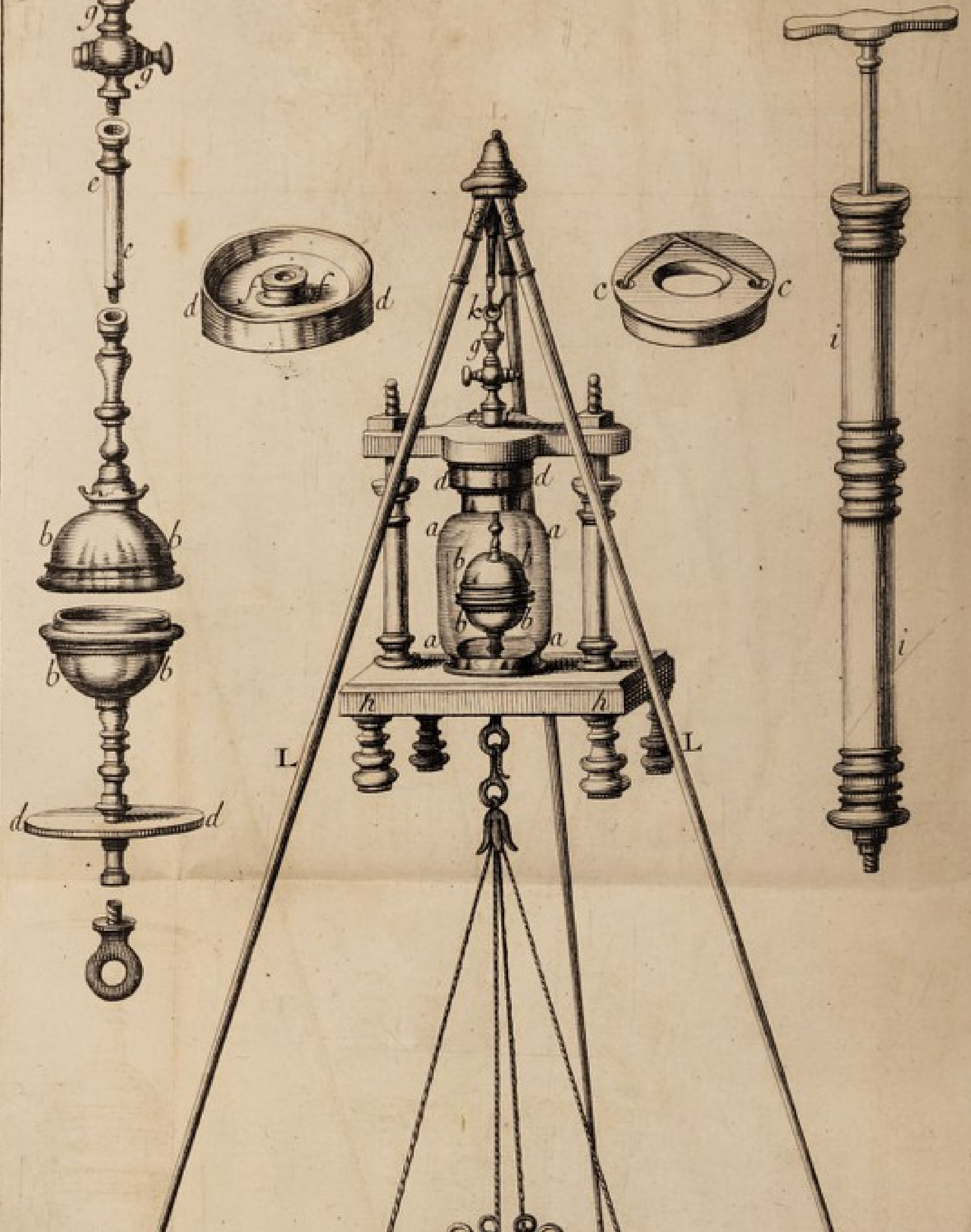

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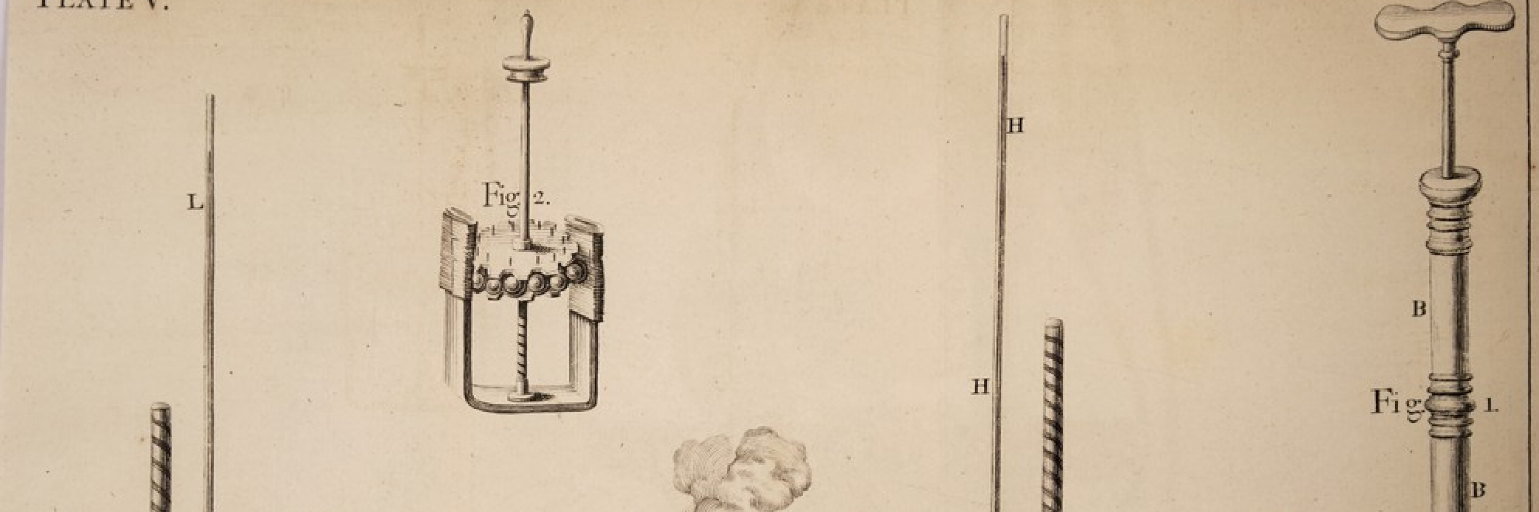

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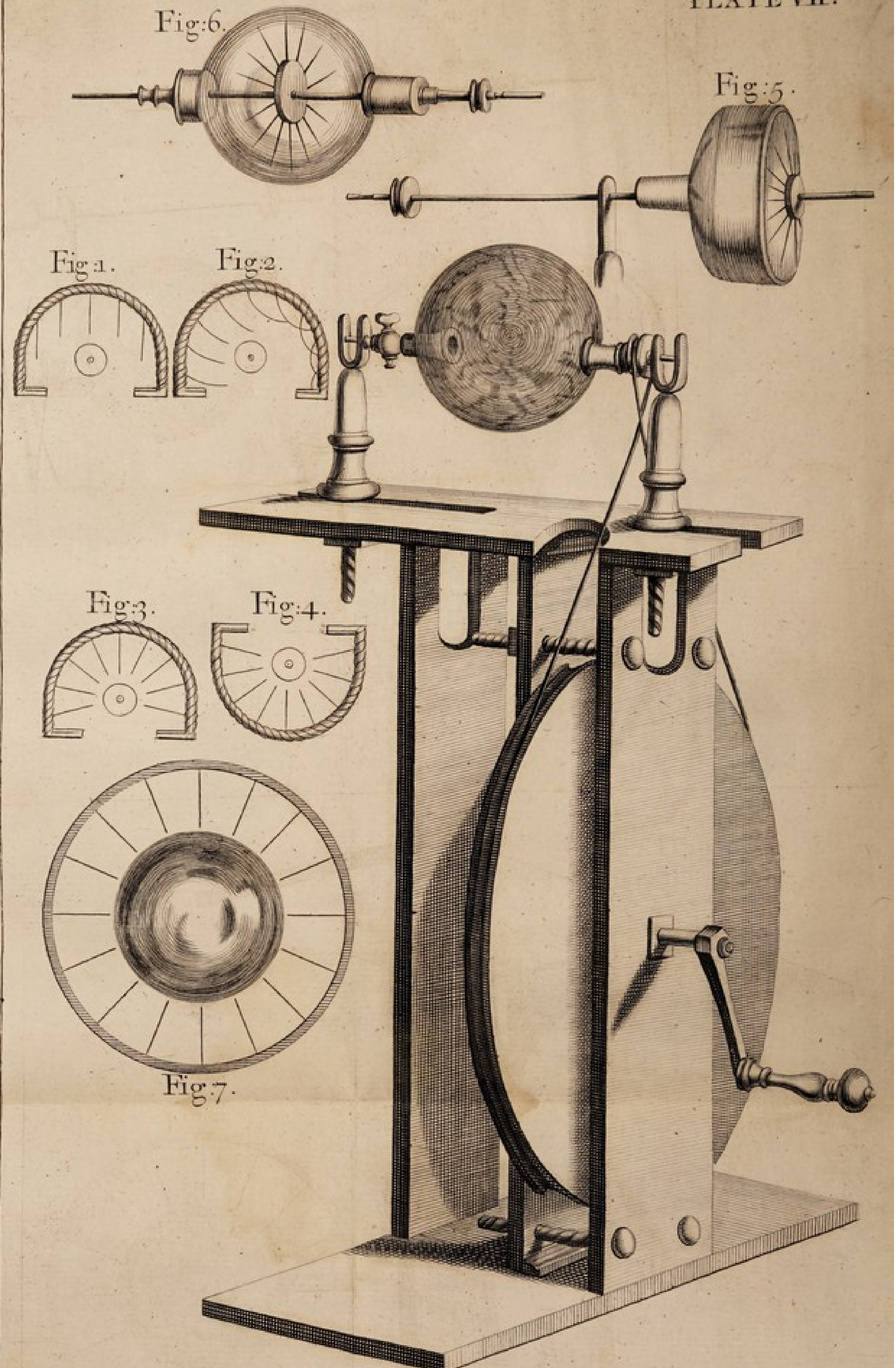

Physico-Mechanical Experiments, &c.

A General Description of the AIR-PUMP *made use of in the following* EXPERIMENTS.

THE AIR-PUMP (delineated Plate I.) consists of two Brass Barrels or Cylinders, as represented by *a a a a*, twelve inches in height, and two their diameters within. The Suckers, or *Emboli*, are rais'd and depress'd by turning the Winch *b b* backward and forward. The Winch is fasten'd to a Spindle that passes thro' a Lanthorn, whose Pins perform the Office of Cogs; for in its motion they lay hold on the Teeth of the Racks *c c c c*, and so reciprocally as one is depress'd the other is elevated: By which means the Valves, which are made of limber Bladder, and fix'd on the upper part of each *Embolus*, as well as at the bottom of the fore-mention'd Cylinders, perform their Offices mutually of exhausting and discharging the ſame Air taken from the Recipient on the Plate of the Pump. And when the Recipient comes to be pretty well exhauſted of its contain'd Air, the preſſure of the outward Air on the deſcending Sucker is nearly ſo great, that the Power requir'd to raiſe the other is very little more than what ſurmounts the friction of the moving Parts; which renders this Pump preferable to all other; for, in the Working of them, the nearer they approach a *Vacuum*, the greater is their Labour: But this that I am now deſcribing (under the ſame circumſtances) is quite contrary.

The bottom of the Barrels are plac'd in a Braſs Diſh, repreſented by *d d*, whoſe ſides are about two inches high, and is on purpoſe to put Water in, to keep the Leather Collars (on which the Braſs Cylinders ſtand) moiſt, whereby the Air is prevented from inſinuating into the Cylinders in thoſe parts. The Cylinders are ſcrew'd down on the ſame by the Nuts *e e e e*, which force the Frontiſpiece *f f* down on them, thro' which the two Pillars *g g g g* paſs. The Pillars have an Iron belonging to each of 'em, and paſs from 'em in the form of a Swan-neck, decipher'd by *g g*, which Irons are faſten'd to the hinder part of the Frame, for their better ſecurity from ſhaking. From between the two Braſs Barrels ariſes a Braſs hollow Wire, *h h h h*, which hath a communication with each of 'em, by means of a perforated piece of Braſs which lies along horizontally from one to the other. The upper end of this hollow Wire is faſten'd to another piece of perforated Braſs, which ſcrews on underneath the Plate *i i i i*, which is 10 inches over, and has a Braſs Rimm ſoder'd on it, to prevent the ſhedding of Water; for which

which there is occaſion in ſeveral Experiments. Between the middle and the ſide of this Plate ariſes a ſmall Pipe, *k*, about an inch and half in height, thro' which into the fore-mention'd hollow Wire paſſes all the Air into the Barrels, as it is taken from the exhauſting Receiver. Upon the Plate of the Pump is always laid a wet Leather, on which the Recipients are plac'd: This wet Leather prevents the Air's getting into the Glaſſes, whoſe edges are truly ground, and is of uſe for that purpoſe beyond any Cement whatſoever, and not only ſecures it from the Air's ingreſs that way, but by the uſe of it we can make ſeveral Experiments in the ſame time they formerly could make one, without any daubing or difficulty. Another Excellency in this Pump is, the Contrivance of the Gage, denoted by *l l l l*, which Gage is a Glaſs Tube about 34 inches long, and is ſo plac'd that it cannot eaſily receive damage, and is altogether out of the way of any thing that is experimented on the Pump. Its lower Orifice is plung'd in a Glaſs of Mercury, deſcrib'd by *m m*, on the ſurface of which is laid a piece of Cork with a hole in the middle for the Glaſs Tube to paſs thro': On this Cork is plac'd a Board made of Box Wood, about an inch in breadth, and groov'd in the middle to receive the fore-mention'd Glaſs Tube, which is looſely loop'd on to the ſame by two Braſs Loops, that it may have the liberty of riſing and falling as the Mercury aſcends or deſcends in the Gage. To the upper part of this Tube is cemented a Braſs Head, which Braſs Head fits into the fore-mention'd perforated Braſs Piece that is ſcrew'd on under the Plate, and has a communication as well with the Recipient on the ſame, as with the hollow Braſs Wire *h h h h*

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paſſing between the two Barrels. The Box Board is graduated into Inches and Quarters, from the ſurface of the Quickſilver to 28 inches high: from thence 'tis divided into Tenths of inches. By this Gage the Degrees of Rarefaction in any Experiment are at all times moſt nicely to be obſerv'd. The Air-cock, *n*, which lets in the Air, is likewiſe a Screw on the ſame fore-mention'd perforated Braſs, in which the upper parts of the Gage and hollow Wire are inſerted: *o o o o* repreſents a Receiver ſtanding on the Plate of the Pump, on whoſe upper part *p p*, thro' a Box of Collars of Leather, paſſes a Slip-wire, whoſe Office is to take up, let fall, or ſuſpend any thing at any determinate height, in the Receiver, without the Air's inſinuation.

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SECT. I.

An Account of ſeveral Experiments on the Mercurial Phoſphorus.

EXPERIMENT I.

I Took a Glaſs Receiver, open and ground at both ends, and capable of containing about 30 ounces of Water: The upper Orifice of this Receiver was clos'd with a Braſs Plate, (by the help of a wet Leather laid on the edge of it,) in the middle of which was ſcrew'd a Stopcock, that had a ſmall Glaſs Tube inſerted into the lower Orifice of it; the Inſertion was perform'd by means of a Cement: And the little Tube thus inſerted, reach'd nearly from thence to the bottom of a Glaſs, which was alſo included within the 'foreſaid Receiver, and which had as much Quickſilver in it as would cover the bottom of the Tube about a quarter of an inch. This *Apparatus* was then applied to the Pump, and the Stopcock turn'd, to hinder the Air's paſſage that way, till the Receiver was ſufficiently exhauſted: Which done, the Stopcock was turn'd again, to give the Air free liberty to enter in; and then the Air making its way thro' the Tube before mention'd, ruſh'd with a very great violence thro' the body of the *Mercury*, blowing it up forcibly againſt the ſides of the Veſſel that contain'd it. And in this confuſion and hurry of its Parts *it gave, all round, the appearance of Fire*; it look'd like one great flaming Maſſe, compos'd and made up of innumera-

See Fig. 1. Plate III.

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ble little glowing Balls, which being forc'd up and dash'd against the sides of the Glass by the impetuous Torrent of Air, fell down again by their own weight into the rest of the *Mercury*. And thus the *bright* Phenomenon continued, till the Receiver was half fill'd again with Air.

The Result of this Experiment therefore, shews us, that *Light is producible from* Mercury, *by passing common Air thro' the Body of it, after the Receiver is well exhausted : i. e.* that *Light is producible by the application of a very subtile and penetrating Mover, to a Fluid of great density, whose Parts are most minutely divided, and of a smooth and polish'd Superficies, and plac'd where it has little disturbance, but from that Body which gives the Motion to its Parts.* For such a *dense* and *polite Body*, is Mercury; such a *subtile Mover*, is the Air; and such an *apt Repository*, is an Exhausted Receiver.

EXPERIMENT II.

HAving provided a *Receiver* of about 21 inches in height, I screw'd to the upper Orifice of it a *Glass* resembling those now commonly us'd for Cupping, having an open passage thro' its Neck, in which was cemented a piece of a small Tube, drawn tapering to one end by the Flame of a Candle: This, together with the Cup, made an entire *Funnel*, the small Aperture of which was stop'd with a round little Plug of Wood, to prevent the Mercury's entring the Receiver before its due time. Within this tall Receiver was included a *Glass* of the height

height of about 17 inches, which had a round Crown like a *Shade* (as they generally call thoſe Fences which are put over Images to keep 'em from the Duſt.) This whole *Apparatus* thus ſet together, was plac'd on the *Pump*, and about a pound and a half of Mercury put into the Funnel; and then working the Pump, by that time the Air had been drawing out for the ſpace of two minutes, there was enough exhauſted for exhibiting the Phenomenon intended. Having then looſen'd the Plug therefore, which ſtop'd the Funnel, the Mercury was driven by the preſſure of the Air with great violence into the Receiver, and ſtriking forcibly on the Crown of the included Glaſs, was thereby broken into very ſmall Particles, and gave the ſurprizing appearance of a ſhowre of Fire, deſcending all round the ſides of the Glaſſes. The Light it gave in its deſcent, was ſuch, that the form of the Receiver, and the Glaſs included therein, were both very diſtinguiſhable, and continued ſo to be, till all the Mercury had paſs'd thro' the Funnel. All that ſpace of time (I ſay) the Repreſentation laſted; neither could any thing more lively expreſs ſuch a fiery Showre, than this deſcent of the Mercury in *Vacuo*. *See Fig. 3 Plate III.*

What farther occurr'd to Obſervation in this Phenomenon, I think proper to take notice of in ſome Particulars by themſelves. I obſerv'd then,

That the deſcent of the Mercury reſembled rather the fall of Snow, than that of Rain, by reaſon of the ſlowneſs of its motion.

That none of it appear'd luminous, but what was contiguous to the ſides of the Glaſſes in its deſcent.

That

That the Globules of Mercury deſcended ſome ſwifter than others, according to their different magnitudes.

That the Mercurial Globules did not barely ſlide down along the ſides of the Glaſſes, but were alſo turn'd about circularly; or, in other words, that beſides their *motion of Perpendicular deſcent*, they had alſo a *Circular one about their own Axes.*

That the parts of the Mercury contiguous to the Glaſs, were by theſe Circumgyrations continually tearing and ſeparating from their Contact with the Glaſs; and by that means were wrought up into ſuch a form or ſhape, as was proper for the production of Light from ſuch a body in ſuch a *medium.*

That the ſmaller Globules, which adhered to the Glaſs, and whoſe weight were not ſufficient to cauſe their deſcent, remain'd opake; for (in this, as well as all other Mercurial Experiments) no Light is to be obtain'd without Motion.

That the very ſame Motion as this was in *Vacuo*, given to the ſame Mercurial Globules in *Common Air*, will not produce the ſame Effect: Which I try'd, by condenſing Air ſtrongly on the Surface of Mercury, and ſo forcing that Mercury thro' Leather.

The Reſult of all which Obſervations put together, is, That *a peculiar Figure and Motion of Parts, as well as a proper Medium for thoſe Motions to be perform'd in, are requiſite to the Production of the Mercurial* Phoſphorus.

Having ſince repeated this Experiment, and that with a *greater quantity* of Mercury; I have obſerv'd ſome particular Appearances in it, ſo remarkable

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and ſurprizing, that I thought I ought not to paſs 'em by in ſilence.

I made uſe of a quantity of Mercury, about *as great again* as what I us'd before, *viz.* near upon three pounds: And now the deſcending Mercury did not only appear like a Showre of Fire, (which it did at the firſt Trial) but alſo the Light darted thick from the Crown of the included Glaſs, *like Flaſhes of Lightning*, of a very pale colour, and eaſily diſtinguiſhable from the reſt of the Light produc'd. Theſe Flaſhes I have obſerv'd to be darted, ſometimes *Horizontally*, ſometimes *inclining upwards*, at other times *downwards*. And beſides this difference with reſpect to the *manner* of the *Reverberation* of the Flaſhes, there was another thing obſervable with reſpect to the *Quarter* from whence they were ſo reverberated; for they would be thrown not only from the *included* Glaſs, but ſometimes alſo from the *including* Receiver: And I have ſometimes ſeen them rebound into Figures ſo very odd and ſurprizing, that I have no Idea of any thing that can ſerve for a juſt Compariſon with 'em. But this is certain as to theſe ſtrange Flaſhes, that they have ſometimes ſeemingly *proceeded directly from the Stream of Quickſilver*, (as it deſcended from the Funnel) *before ever it reach'd the included Glaſs*; and that their general Courſe is, to *fly to the ſide of the outward Receiver*, where the Light breaks, and ſpreads it ſelf into theſe odd forms. This is what they moſt frequently do: But if they take their original *from the ſide of the Receiver*, (as I have ſometimes obſerv'd) then their Courſe is different.

Laſtly, It deſerves notice alſo, that during all the time of the Mercury's deſcent thro' the Funnel, (which was at leaſt two minutes,) *the Crown of the* *included Glaſs appear'd to be ſenſibly more enlighten'd than all the reſt of the parts of it:* And this Light was *uniform*, and without any alteration, as long as the Quickſilver kept running.

Experiment III.

IN the 'foregoing Experiments, we have ſeen the Production of the Mercurial *Phoſphorus*, in the fine and much-rarefied Medium of a *Vacuum*; by which 'tis plain, that ſuch a Medium as that, is accommodate to the Nature of this Phenomenon, and will ſerve for the exhibition of it: But we can by no means infer from thence, that no other Medium will do; or, that Light, which may be produc'd in the rarefied Medium of a *Vacuum*, may not alſo be produc'd in a Medium leſs thin and rare than that. To bring this matter therefore to a determination, (*viz.* whether ſo thin a Medium as a *Vacuum*, or the neareſt approach to it, be abſolutely neceſſary to the production of ſuch a Light, as is diſcoverable in the Barometer by putting the Mercury in motion,) I proceeded after the following manner:

The *Mercurial Gage* (an Inſtrument now univerſally known) I concluded would be the moſt proper Inſtrument for this Diſcovery. Having therefore plac'd a ſmall *Receiver* upon the *Plate of the Pump*, the Air was exhauſted from it, till the Mercury in the Gage was elevated to 29½ inches; then ſuffering ſome Air to enter the Receiver by the *Cock*, the Mercury in the Gage deſcended, and made ſeveral

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Vibrations before it reduc'd it ſelf to a ſtate of reſt; and the Mercury having no other motion impreſs'd upon it in all theſe Vibrations, but barely that which the Air caus'd by its entrance, the Mercury all this time, tho' it did *appear luminous*, yet appear'd ſo only in the *Deſcents*, and not in the *Aſcents:* But when the Mercury came to be broken and divided by a violent agitation and ſhaking, then the broken parts appear'd *luminous* in *ſome part* of their Superficies, the *other part* always being *opake*; and that after this manner: The *undermoſt* Superficies of the Mercurial Globules in their *Aſcent* became *concave*, and there they were *luminous*; but the *uppermoſt* Superficies of the ſame Globules, in their *Deſcent*, became *concave*, and there likewiſe *luminous:* But the *uppermoſt* Superficies in the *Aſcent*, which were *convex*; and the *lowermoſt* Superficies in the *Deſcent*, which were alſo *convex*, in both caſes gave no Light at all, but continued always *opake*. And thus the Appearance continued upon every admiſſion of Air, till near half the quantity that was exhauſted was return'd again: But after that quantity of Air was admitted, then no manner of Light would enſue, tho' the Mercury had the ſame motion given it as before.

From this Experiment therefore we may draw the following Concluſion, *viz.* That *tho' the Mercurial* Phoſphorus *in the* Torricellian *Experiment is not produceable in ſo denſe a Medium as common Air, yet it by no means requires ſo thin and ſo-much-rarefied a Medium as that which makes a near approach to a Vacuity.* And this Truth receives a further Confirmation by the following

Experiment IV.

I Provided ſome Quickſilver, very fine, and free from the leaſt appearance of ſoil on its ſurface. The Glaſs alſo which contain'd it, was made very clean and dry. I included this Glaſs with its Mercury in a *Receiver* on the Plate of the Pump, and exhauſted the Air, till I found the Mercury in the *Gage* ſtanding at 28 inches, and not above, (the Mercury in the Barometer at the ſame time being at 29½ inches.) At this elevation of the Mercury in the *Gage*, the Pump was ſhaken, and by that means the Quickſilver in the included Glaſs was put into motion. *The Effect* of this Concuſſion was *the appearance of a Light*, bright enough to render the Receiver and the included Glaſs plainly viſible and diſtinguiſhable; and not only ſo, but a Man's Hands and Fingers on the outſide might eaſily be diſcern'd likewiſe. This was the Principal Matter of Fact enquir'd after by the Experiment; but I obſerv'd, farther,

See Fig. 4. Plate III.

That tho' the Light might be produc'd by a ſmall agitation of the Mercury, yet that Light would be encreas'd by a greater and ſtronger agitation.

That when a pretty brisk motion was given to the Mercury, it would give the repreſentation of Waves of Light, breaking on the ſides of the Glaſs, and ſcattering ſome Species of the ſame appearance towards the upper part of it.

That upon the repetition of the Experiment, the *Phoſphorus* ſeem'd each time more vivid than the other; till at laſt, by often ſhaking the Quickſilver,

its

its Surface became ſomething ſoil'd, and ſo the Light leſs than it had been before.

That (in this, as well as all other Experiments on the Mercurial *Phoſphorus*,) the Light exhibited is of a very pale colour.

That the firſt appearance of the Light, is when about half the Air contain'd in the Receiver is exhauſted; and, That it ſtill encreaſes with the encreaſing Rarefaction of the Medium.

From this Account it appears, that the former Concluſion is again confirm'd, *viz. That there needs not the neareſt approach to a Vacuum, to produce the Mercurial Phoſphorus.*

And theſe two laſt Experiments put together, make up the fulleſt Proof of it that can be deſir'd.

For in one of 'em the Air was *let in upon the Mercury* plac'd in the exhauſted Receiver, and a Light was produc'd that way: In the other, the Air was *not totally remov'd from the Mercury*, and a Light was produc'd that way alſo. Now there can be no third way; and therefore 'tis univerſally true, that *the moſt rarefied Medium is not neceſſary to the production of this Phenomenon.*

EXPERIMENT V.

THe Experiments hitherto related, have diſco- ver'd what Mercurial Lights may be pro- duc'd, either in *Vacuo*, or in *Mediums* making ſome approach thereto. But the following one will ſhew what Light is produceable in a Medium very diffe- rent from either of the former.

In order to this, I took a *Glaſs Globe*, (whoſe Con- tent was about 30 ounces of Water;) and having put into it near half a pound of Pure Quickſilver, I clos'd the Mouth of it with a *Braſs Cap*, which had a *Cock* inſerted in the middle of it, by which means the Mercury had a free communication with the external Air: This done, the Globe was ſhook, and the Particles of Light preſently appear'd in great plenty; they were of the bigneſs of ſmall Pins heads, very bright and vivid, ſparkling like little diminu- tive Stars in the *Lactea*, and exhibiting all together ſuch a whitiſh appearance as thoſe Stars do. Their number would encreaſe, according to the rapidity of the motion given the Globe; ſo that by proportioning the agitation, one might produce a greater or leſs number of theſe ſmall Luminaries.

Having carried the Experiment thus far; I took the ſame Glaſs Globe, with the ſame Mercury in- cluded in it, and applied it to the *Pump*, (by the help of a hollow *Braſs Pipe*, which ſcrew'd both to the *Cock* and *Pump*;) and then the Air being ex- hauſted, and the Cock turn'd to prevent its ingreſs, the Globe was taken off, and moderately ſhaken. And now the *Phenomenon* was quite alter'd; for the Mercury appear'd luminous all round: It did not

See Fig. 5. Plate III.

now

now discover (as before) a Congeries of little, bright, twinkling Sparks, but a continued Circle of Light, which lasted all the time of the agitation. And if that motion were check'd with another of greater violence, it would then appear luminous almost all over the Globe. This being try'd, the Air was admitted again into the Cavity of the Globe, and then the Mode of Light return'd to its former appearance: The continued Circle of Light was lost, (neither could it be recover'd again by any shaking whatsoever,) and the little Stars return'd sparkling as before.

From what has here been related, we may inferr,

First, That *Light is produceable by the agitation of Mercury in the open Air.*

Secondly, That *this Light produceable in the open Air, is very different from that produc'd in Vacuo, or a much-rarefied Medium.*

Thirdly, That *the difference between these Lights, consists particularly in this; that the luminous Particles are* **distinct** *and* **separate** *in the one, and* **united** *or* **blended** *into one continued body of Light in the other.*

Fourthly, That *the Presence and Action of the Air is the cause of the separation of the parts of the Mercury into so many distinct luminous Globules, which in Vacuo form'd all one continued Circle.*

And from hence it appears, that the Accounts given in this Experiment are not at all contradictory to any of the former, about the Mercurial *Phosphorus*; for *there* I speak of one kind of Light, and *here* of another. That Light which is produc'd in

Vacuo,

Vacuo, or a very-much-rarefied Medium, is not the ſame with this produc'd in the *open Air:* And therefore when I ſay, that the *Mercurial Phoſphorus* is not produceable, but in a Medium ſo or ſo qualified, it ought to be obſerv'd, that I ſpeak there concerning a Light of the ſame kind and quality with that diſcover'd upon the agitation of the Mercury in the Barometer; for *that*, is vaſtly different from *this* which appears upon the ſhaking of the Mercury in the open Air.

SECT.

SECT. II.

An Account of ſeveral Experiments made concerning the Attrition of Bodies, in various Mediums.

The Deſcription of the Machine *for giving a ſwift Motion to Bodies in* Vacuo, *without admitting the External Air; repreſented by Plate* II.

A Deſcription of the *Air-Pump* (*Fig.* I.) being before given, I ſhall forbear taking any further notice of it, ſaving what immediately relates to the following *Experiments.*

A A A A IS a Ladder, ſuch as is generally us'd in Houſes.

B B Is a Bar of Iron, which paſſes through the middle of the upper Step, and is faſten'd to the Backboard of the Ladder by two Nuts and Screws thro' both the Board and Iron.

C C the Jaws of the Iron Frame which holds the great Wheel D D, of 23 inches diameter within its Groove.

E E the Braſs Plate of the Air-Pump, on which the Recipient f f is plac'd.

g g The Spindle, to which Bodies of different magnitudes may be faſten'd, by a hole paſſing thro' the middle of them, ſufficient to receive the Spindle; and by means of the two Nuts h h, a larger or a ſmaller Body may be ſcrew'd faſt on.

i i Is a Braſs Plate turn'd true to the ground Edge of the Recipient on which it is plac'd, having a Braſs Box in the middle of it, which is full of Collars of Leather well oyl'd, thro' which the Spindle paſſes; the hole of the Braſs being likewiſe juſt fit to receive it.

k k k k Two Pillars, with Nuts to ſcrew down a piece of Board, which has an Iron faſten'd to it to receive the upper point of the Spindle; the lower one falling into a Braſs Socket, ſcrew'd to the middle of the Plate of the Air-Pump.

L L L L The Supporters, reaching from the upper Board of the Ladder to the Pillars, to prevent the Recipient's being drawn from its Place by the motion and tugg of the Wheel-band.

m m The ſmall Wheel, which the Band from the great one ſurrounds, and is one inch and half diameter.

N N The Winch which gives motion to the whole; the ſmall Wheel m m making about fifteen Revolutions to one of the large Wheel D D; ſo that a Body faſten'd to the Spindle g g, of the ſmall Wheel m m, will be turn'd fifteen times round to once of the great Wheel: And according as that ſhall exceed in diameter the ſmall Wheel, ſo will the Velocity of the Motion of the extreme Parts be proportionably encreas'd.

o o o o A ſtrong Board reaching from the lower Jaw of the Machine to the Ground, for the ſupport, or giving a ſteddy motion to the great Wheel.

p p Are two Screws, which faſten the Ladder to the Floor.

EXPERIMENT I.

Concerning the Attrition of Amber *on* Woollen *in* Vacuo.

I Took ſome *Amber Beads*, of the bigneſs of ſmall Nutmegs; and having paſs'd a Thread through 'em, apply'd 'em by that means to a circular piece of Wood, which was *turn'd* with a *Groove* on the edge of it, on purpoſe to keep the Beads from being diſplac'd by the ſmart Friction they were to endure. Likewiſe, for their better ſecurity and faſtneſs, there were ſo many *Pins*, or pieces of ſmall Wire, driven thro' the Wood; and between every Bead there was a String ty'd over from *Pin* to *Pin*; the Beads, at the ſame time, ſtanding out by the ſpace of their Semidiameters beyond the body of the Wood into which they were fix'd.

In this manner was the whole put upon the *Spindle*, and made faſt there by the two *Nuts*, (as was expreſs'd before in the *Deſcription* of the Machine:) Then the *Braſs Plate*, on which the *Woollen* was wrapt, being ſcrew'd to its place, (by means of the *Socket*, which receives the lower Point of the *Spindle*;) would ſpring back, and graſp the Amber with a moderate force. Theſe things thus prepar'd, the *Receiver* was plac'd over them all together, with its upper *Plate* and *Box* for the *Spindle* to paſs thro': The *Pump* then being ſet to work, the Mercury in the Gage was in a very little time elevated to about $29\frac{1}{2}$ inches; which ſhew'd the Receiver to be well *See Fig. 2 Plate V.*

 ex-

exhaufted. And now the great *Wheel* of the Machine being turn'd, the Amber had a very fmart Attrition on the Woollen. At firft, nothing remarkable appear'd; but, in the fpace of a *Second* or two of time, there was *a Light which became fenfible enough:* For where-ever the Attrition of the Amber was made; while the Motion went on, there, and at all times, did the Light continue without intermiffion, and might be difcern'd at three or four foot diftance. Indeed it would not continue, if the Amber did any way defert the Woollen, notwithftanding the exceeding Velocity of the motion: But where the Attrition was uninterrupted, the appearance of the Light was fo too, and propagated it felf to that diftance I have mention'd. And how fwift the Motion was, which was given in order to the production of this *Phenomenon*, may be eftimated from the Diameters of the feveral Revolving Parts; for the Diameter of the great Wheel was 23 inches; that of the fmall one mov'd by it, was $1\frac{1}{2}$; that of the Wood and Amber on the fame Spindle with the fmall Wheel, was $4\frac{1}{2}$. Now, fuppofing the great Wheel to make two Revolutions in a *Second*, I think it follows, that the Velocity of the Extreme Parts of the Amber muft be at the rate of a Mile in three minutes. And this violent Motion (as it may well be expected) was attended with Heat alfo, and fuch as difcover'd it felf by plain and fenfible marks; for the Amber (befides what the Touch difcover'd) appear'd manifeftly to be burnt and crack'd; and the Woollen was not only difcolour'd, but perfectly fcorch'd, by the intenfe Heat.

I will conclude the Account of this Experiment with this one Remark, relating to the Light produc'd by this Attrition: *viz.* That tho' the fame

Mo-

Motion and Friction was given the Amber in the *Open-Air* as in the *Vacuo*, yet in the former case the Light was *very small and faint*, in comparison of what it was in the latter.

By this Experiment therefore we learn,

1. *That Light is produceable in* Vacuo, *by the Friction of a solid Body (as Amber) against a Body of a soft and yielding nature, (such as Woollen.)*

2. *That this is not a meer lambent Fire, but such as is accompanied with a great Heat.*

3. *That this Light depends so immediately on the Attrition, as to disappear where that ceases.*

4. *That it requires a very thin and rare Medium, in order to its Appearance: And the thinner the Medium, the greater the Appearance.*

Experiment II.

Concerning the Attrition of Flint *and* Steel *in* Vacuo.

HAving provided a *Steel* Ring about 4 inches diameter, and $\frac{1}{8}$ of an inch thick, I fix'd it between two pieces of Wood (of a less diameter) on the *Spindle* with the *Nuts*, as in the 'foregoing Experiment; the Edge of the Ring standing out beyond the Extremity of the Wood which held it, about half an inch. The *Brass Plate* (which I made use of for fastening the *Woollen* for the Attrition of the *Amber)* serv'd here also to fix a Piece of Flint, an Edge or Corner of which was placed towards the

Steel:

Steel: And this Brafs Plate, by vertue of its Spring, would hold the Flint pretty ftrongly to the Steel, notwithftanding fome parts of it might be worn or chipp'd off by the rapidity of the motion: And in this manner 'twas cover'd with a *Receiver*, a *Brafs Plate* and *Box* (as the former was).

Before any Air at all was exhaufted, the great Wheel was turn'd, which gave a motion to the fmall one, and confequently to the Steel; and by its collifion with the Flint, there were Sparks of Fire produc'd in *great plenty:* But after fome Air had been withdrawn, and the motion given, as before; the Sparks which then appear'd, were neither fo *numerous* as before, nor fo *bright* and *lively*. And as more and more Air was ftill drawn out of the Receiver, fo this Change in the Sparks produc'd, became more and more manifeft. At *every ftop* made, to repeat the Experiment in an *higher degree of Rarefaction*, I found the Sparks ftill to diminifh, both in their *Luftre* and their *Quantity*; till at laft, when the Receiver came to be *well* exhaufted of Air, there *was not one Spark to be feen*, tho' a much-greater motion was given than before, and confequently a more ftrong and valid collifion of the Flint and Steel. All the Appearance in this cafe was, only a *faint, continued, little Streak of Light*, vifible on the edge of the Flint that was rubb'd by the Steel.

This being try'd, there was then fome Air let into the Receiver, upon which (the motion being given as before) *fome* Sparks were difcover'd, but of a *dull gloomy hue:* Upon the letting in a little more Air, by I know not what accident, the whole quantity of Air forc'd it felf in, and then the Wheel being fet to work again, *the Sparks appear'd as numerous and as vivid as at firft*.

The

The Conclusion therefore from this Experiment is, *That the Air's Presence is absolutely necessary to that vigorous expansive Motion of the Parts of Bodies, wherein the Nature of Culinary Fire consists.*

Qu. Whether the Light visible on the edge of the Flint, when the Receiver was well exhausted, was not of the same (*lambent*) kind with other Lights produc'd by the Friction of certain Bodies, of which mention is made in some of our Experiments?

Experiment III.

Concerning the Attrition of Glass, *and various other Bodies, in* Vacuo.

1. *Concerning the Attrition of* Glass *and* Woollen.

I Took a *Glass Globe* of about 4 inches diameter, having a Passage thro' the middle of it to receive the *Spindle*, which was fasten'd to it with *Corks* and *Screws*. The *Woollen* against which the Friction was to be made, was the coarsest sort of that which is commonly us'd for *Gartering*, which I chose purposely on the account of its harshness, as being likely to improve the Phenomenon to a higher degree than the *Cloth-List* I had us'd before. This was wrapt about the Arms of the *Brass Spring*, and being screw'd down to its place, gently embrac'd brac'd the Globe: Then a large *Receiver* was put over all, and the *Pump* being ſet to work, in a little time the Air was exhauſted out of the Receiver. The great Wheel being then turn'd, gave fifteen Revolutions to the included Globe, at each of its own: Which ſwift Motion giving a ſmart Attrition on the Woollen, quickly produc'd a beautiful Phenomenon, *viz.* a fine *purple* Light, and *vivid* to that degree, that all the included *Apparatus* was eaſily and diſtinctly diſcernable by the help of it. And thus it continued while the Friction laſted.

Upon the *letting in* a little Air, the *Light* and the *Colour* were both chang'd; the Light impair'd in its *Brightneſs*, and the Colour in its *Tincture*. And as the Air, at ſeveral times, was ſuffer'd to return into the Receiver again, ſo did the Light ſtill become more *pale* and *faint*; tho' even when the Receiver was quite fill'd with Air, ſome *feeble Light would ſtill diſcover it ſelf*, upon the ſame Attrition given as at firſt.

I found, that this Purple Light was *viſible no where*, but *only on the Arms of the Braſs Spring*, where the Glaſs in its motion rubb'd upon the Woollen; and, that the Dimenſions of it were about half an inch in breadth, and one inch in height: And farther, That it did all the while remain ſteddy in its poſition, without any the leaſt Undulation, tho' the motion of the Glaſs was ſo conſiderably ſwift.

Some

Some Varieties occurring in the Experiment laſt mention'd, at various Trials.

When this Experiment came to be repeated two or three times, with the *ſame* Glaſs; no *Purple Light would appear*, but a *Pale* one only ſucceeded in its room; neither could I recover the Purple with that *ſame Glaſs*, by any methods whatſoever that I could uſe.

When I took a *freſh* Glaſs; after I had made uſe of it (this way) two or three times, the *Purple was loſt again*, and could not be retriev'd. And if a new one were us'd, the Effect would ſtill be the ſame.

Sometimes, if the Glaſs were taken out after a violent attrition, *it would be ſo hot*, as *ſenſibly to offend the Hand* that held it: And the Woollen would appear not only *diſcolour'd*, but perfectly *burnt* through.

Sometimes the Light would not be confin'd to thoſe ſtrict Bounds already mention'd, but *would be ſpread quite round the Globe*, and *make an entire continued Circle*, all the time of the motion; notwithſtanding that it touch'd the Woollen in no more parts, than it did in the former Experiment.

Sometimes a perfect diſtinct *Halo* would appear, ſpreading it ſelf quite round the fixed Light. This I attribute to ſome *Particles* of *Water*, inſinuating themſelves, by the Spindle, thro' the Box on the upper Braſs Plate (where Water is always kept, to prevent the entrance of Air in that place:) For this Water deſcending along the *Spindle*, till it reach'd ſome part which was of a greater extent, would there (as I conceive) by the violence of the motion given, be thrown all about the *Receiver* in ſmall drops; ſome of which being very likely to fall on the *Woollen*, would there be *heated* to a conſiderable degree by the attrition of the Glaſs; and being conſequently *evaporated*, would appear there in the form of a *Halo*, ſurrounding the Light. And what confirms this Solution, is, That having ſince form'd a Contrivance to prevent the ſcattering of the Water, no ſuch appearance of any *Halo* has been obſerv'd. But to proceed,

In this Experiment I have ſhewn the Effects of the Attrition of *Glaſs* on *Ordinary Woollen*; I would now add an Experiment concerning the Attrition of *Glaſs* on *Woollen ſome way prepar'd* or qualified beforehand.

I took ſome of the 'foremention'd *Liſt of Cloath*, which had been drench'd in *Spirit of Wine*; and faſten'd it to one Arm of the *Braſs Spring:* And ſome of the ſame *Liſt*, which had been ſteep'd in Water impregnated with *Salt-petre*, I ty'd to the other Arm of the ſaid Spring: But both pieces were well dry'd before I made uſe of 'em.

Then, upon the Attrition, I obſerv'd the Light to break from the agitated Glaſs in a very odd form, reſembling that of *Lightning*. This is manifeſtly different from the laſt Phenomenon: For there indeed we had a *delicate Purple-colour'd Light*; but here, a brisk *fulgurating* Light, ſcattering it ſelf about in Flaſhes, and darting with a force from the ſurface of the revolving *Glaſs*.

2. *Con-*

2. *Concerning the Attrition of* Glaſs *on* Oyſter-ſhells.

Inſtead of the 'formention'd *Woollen*, I made uſe of two flat *Oyſter-ſhells*, well dry'd: Each Arm of the Braſs had one fix'd to it. Upon the uſual motion given, a Light appear'd, reſembling a *fierce-flaming Spark*, juſt upon that very ſpot where the Glaſs and the Shells touch'd one another. This Light did not dilate or extend it ſelf, but kept within the bounds where it firſt appear'd; and it was but a ſmall compaſs that it appear'd in.

3. *Concerning the Attrition of* Oyſter-ſhells *on* Woollen.

The Succeſs of this Trial was, that it produc'd a Light, but an *obſcure* and *dim* one, and, at beſt, like a faint *Halo.*

4. *Concerning the Attrition of* Woollen *on* Woollen.

I took ſome of the *Liſt* formerly mention'd, and bound it about the edge of a *Wooden Wheel*, which I had caus'd to be *turn'd* for that purpoſe. This Wheel (with its *Round* of Woollen) I faſten'd on the *Spindle*; and ſome of the coarſe *Gartering* was alſo put about the Braſs *Spring*. The Reſult was, that upon the motion given (as uſual,) a *ſmall glimmering* Light appear'd, but ſuch as gave no proſpect of *being any way improv'd* by the continuance of the motion: The Woollens were not in the *least discolour'd*, tho' the Friction was sometimes more than moderate; neither was there any sign at all of such an Effect, as hath been shewn to be produc'd by the Attrition of Woollen on other bodies: The Light (which had been produc'd) *totally disappear'd* upon the re-admission of less than a quarter part of the Receiver's natural content of Air, tho' the Attrition made then was as great as it had been at any time before.

'Tis further to be observ'd, that I could never find, that the *different Colours* of *Woollen* contributed any thing to the *different Colours of Light*, exhibited in any of these Experiments.

The several Particulars of Fact which we learn from these Experiments, are reducible to the following Heads:

That a Purple Light was produc'd by the Attrition of Glass on Woollen (in *Vacuo.*)

That the Purple Light decay'd, both as to its Tincture and Vigour, upon the admission of the Air; and did more and more do so, as more Air was let in.

That this Purple Light is lost, after two or three successive Trials with the same Glass.

That the bounds to which the Purple Light confines it self, are of different and various extents; reaching sometimes all round the Globe, *and at other times being* only about the place of the attrition; *but still steddy, and without Undulation.*

That a brisk Fulgurating Light was produc'd by the attrition of Glass, on Woollen impregnated with saline and spirituous parts.

That a Red Fiery Light was produc'd upon the attrition of Glass on Oyster-shells, which Light was confin'd

within

within a narrow compaſs, and did not ſpread it ſelf farther about.

That a faint dim Light was produc'd, upon the attrition of Woollen on Oyſter-ſhells.

That a ſmall glimmering Light was produc'd, upon the attrition of Woollen on Woollen; which was not encreas'd by the continuance of the Motion, and which diſappear'd upon a ſmall admiſſion of Air.

That in the attrition of Glaſs on Woollen, not only a Light, but alſo a great Heat was produc'd; diſcoverable by the Glaſs, and its Effects on the Woollen too.

And from theſe Obſervations we may make the following Remarks.

Firſt, *That different ſorts of Bodies afford us remarkably-different Lights*, different in *Colour*, and different in *Force* and *Vigour*. This appears from the particular matters of fact now ſet down.

Secondly, *That the Effects of an Attrition may be various, according to the different preparation and management of the Bodies which are to endure it.* Thus the Woollen, tinctur'd with Salts and Spirits, gave ſuch a Friction, as produc'd quite a new ſort of Light.

Thirdly, *That Bodies which have yielded a particular Light, may be brought by Friction to yield no more of that Light.*

This is plain, from the *ſtrange and ſurprizing loſs of the Purple Colour*, after two or three Trials made with the ſame Glaſs; for that *Purple* could not be recover'd by any Art or Means whatſoever.

A *Pale* Light indeed was produceable from the ſame Glaſs afterwards, but the *Purple* was irretreivably loſt: Therefore the Purple-colour'd luminous Matter *muſt either be quite ſpent* (that is, *evapo-*

rated

rated and carried off) by the Attrition, or else the *Configuration and Texture of the Glass underwent such a change by that same Action, that it was no longer capable of admitting the Purple Light.*

If the Cause were from the *Consumption of the Matter*, then we may reason thus; That *that certain determinate quantity of colour'd-luminous Matter, which is lodg'd in some Bodies, may, by a Course of repeated Attritions, be quite exhausted and carried away.*

Further; because a Pale Light is produceable after the other is gone, we may likewise upon the same Supposition argue thus: *That some sorts of colour'd-luminous Matter are more easily separable from Bodies than others*; or, *That they require different degrees of Force and Friction, in order to their evaporation and discharge.*

And this is highly reasonable, since the Parts of differently-colour'd Lights are undoubtedly of *different magnitudes and bulks*; and consequently, That Force which may be sufficient to put some of 'em into *vibratory expansive motions*, may not be sufficient to produce the like Effects upon others.

On the other hand, if the loss of the Purple was owing to the *alter'd Tone or Texture of the Glass*, then the Conclusion will be, *That Bodies may be so chang'd by proper degrees of Friction, as to retain some sort of colour'd-luminous Matter, which they did once emit (as here the* Purple;) *and to emit another sort, which they did at first retain*; (as in our Case the *Pale* Light.)

Now, in favour of the *latter* of these two (and, as I take it, the only two possible) Causes, it may be alledg'd, That the next Experiment will prove to us a *Diminution* of *Light*, or a *Decay of its Force and Colour, consequent upon an Attrition.* But then, as we shall see, those Bodies are both *hard* and *inflexible*,

flexible, ſuch as, by rubbing, would wear and alter one another's Texture : And beſides, the Phenomenon there, is only a *diminution or decay of the Strength and Luſtre of a Colour* ; a meer *gradual alteration* of the *ſame* Colour, not the production of a *new* and *different* one. On the contrary, in this Phenomenon now before us, one Colour is *loſt*, and a very different one appears in its room : And the Attrition here us'd, was that of a *ſoft* and *yielding* Body against a *hard* and *inflexible* one ; ſo that the *wearing* of the Parts, and the deſtruction of their *Spring* or *Tone* conſequent thereupon, is not here ſo eaſily conceivable. However, upon the whole, I ſhall leave it as a

Query. Whether the *loſs* of the Purple Colour be owing to the *Conſumption* or the *Retention* of the matter ?

EXPERIMENT IV.

Concerning the Attrition of Glaſs *on* Glaſs.

Firſt, In *Vacuo.*

I Took a *Glaſs Globe* about three inches in diameter, which I fix'd to the *Spindle*; and to the two Arms of the *Braſs Spring* were ty'd two *ſlips* of *thin Board*, which had pieces of a *Glaſs Tube* faſten'd to 'em, by the means of ſome ſmall *Neal'd Wires* which were put thro' their cavities. Theſe Wires likewiſe paſs'd thro' ſome holes in the Board, made for that purpoſe; and ſo kept the pieces of Tube firm and tight in their places. All this was cover'd with a large *Receiver* (as uſual); and the *Pump* being wrought, the Air was exhauſted.

*See Fig.*6. *Plate* III.

The great Wheel then being turn'd, a ſwift motion was given to the included Globe; and by the friction of this on the 'foremention'd Tubes, a *conſiderable Light was produc'd.* The whole included *Apparatus* was perfectly diſcernible by it; and the Illumination had been much greater, had not the Day-light prevented. (For it was then but a little paſt Five P. M. in the Month of *February*, the Horizon being clear, and the Room open wherein the Experiment was made.) The *Colour* of the Light produc'd, reſembled that of *melted Glaſs:* And, which is very remarkable, this glowing Colour did not appear only juſt upon the *Parts where the Friction was made*, but alſo at the *Extremities* of the Tubes, where the Globe did not touch 'em.

Secondly,

Secondly, In *leſs-rarefied* and *Common* Air.

THE Air being ſuffer'd to enter the Receiver by degrees, and at ſeveral times, the Motion was given at each of thoſe Ingreſſes of the Air: And the Phenomenon here obſerv'd was, that *no ſenſible decay of Light or Colour was to be diſcover'd*, at any of thoſe times: Except only that at laſt, when the Tubes by much rubbing were worn, and conſequently their Spring leſſen'd and impair'd, then indeed the Light would be diminiſh'd, in proportion to the abatement of their force on the moving body. And this I have often obſerv'd, when the motion has been made for ſome time in *Vacuo*, or in *Common* Air. So that I queſtion not, but if the Experiment had been begun where it ended, (*in this Trial*,) the leſſer Light would then have been exhibited in *Vacuo*. (That is, had the Experiment been made, even in *Vacuo*, with Tubes thus rubb'd and worn, and conſequently their Spring weaken'd, a leſs Light would have been produc'd, than if the Experiment had been made in *Pleno*, with Tubes not thus weaken'd and impair'd by the attrition.)

NB. I have ſince tried this ſame Experiment in *Vacuo*, and that about *Noon*, in a clear day. And I found, that the Light was even then as ſenſible, as that of a piece of *Red-hot Glaſs*, of the ſame bigneſs, would have been in the open Air. Yet this appearance, as diſcernible as 'tis, *continues not any longer than the motion is continued.*

By this Experiment we learn,

First, *That a considerable Light is produceable by the Attrition of Glass on Glass*, (that is, of some sorts of hard Bodies, on others of the same kind) *in Vacuo.*

Secondly, *That this Light continues unalter'd upon the admission of Air.*

Coroll. *We may see the difference therefore between this Light here produc'd, and those mention'd in other Experiments:* For this endur'd without change the shock of the returning Air; whereas most or all of those formerly recited, underwent several gradual alterations, according to the degrees of the Air's admission.

Thirdly, *That when the Tubes were worn by the Friction, there was an abatement of the Light: and the more wearing, the more abatement.*

Coroll. *We may see from hence, how much the due Spring and Tone of the Parts contributes to the emission of Light, in those Bodies which will emit it.* So that we may reasonably believe in general, that *very great alterations may be wrought in Bodies as to their Luminous qualities and properties, either lessening and destroying them on one hand, or perhaps encreasing and improving them on the other by the actions of other contiguous Bodies upon them, according as those actions tend, either to the weakning or confirming the Spring and Tone of their Parts.*

I have only two Observations to make farther, with regard to this Experiment.

First, That the *Glaze* or *Polish* on the surface of a Glass, *adds nothing to the Light*, as far as I can find by any Observation.

Secondly, That tho' this Colour be like that of *red-hot* or melted Glass; yet the Glass is not really in such a state as to be red-hot, because this *Colour never outlives the motion*, but is gone presently upon the ceasing of it; (as has always been observ'd, in the darkest

Night:)

Night:) whereas if 'twere really red-hot with the motion, the appearance must *necessarily be preserv'd for some small time*, at least.

Experiment V.

Concerning the Attrition of Glass *on* Glass, *under* Water.

THIS is no more than a Repetition of the last Experiment in another Medium; for the whole Apparatus (there mention'd) was now entirely immers'd in *Water*. Upon the first Friction of the Globe on the Tubes, a *pretty brisk Light was produc'd*, which inlighten'd the whole Body of the Water. The *parts of the Tubes, where the Friction was made*, were distinguishably *Red*; but the appearance soon began to *lessen*, and in no long time *quite died away*. For the Water grew *thick* and *turbid*, by the *Grit* or *Powder* of the Glasses, which was worn off by the *Attrition*. It approach'd continually nearer and nearer to the Colour of *Whey*, so that the Light could be but just discover'd, *as glimmering* through the body of it; and that not *continually* neither, but with *interruption*, and like *faint Flashes* sent out at a distance. Tho' at some other trials, I have observ'd the Light to be more considerable than it was at this time.

The *Dust* rubb'd off by the Attrition of the Glasses, I view'd thro' a good *Microscope*; the Particles of it appear'd to be of a long and slender figure, but I cou'd not discover the least sign of their having been any way in *Fusion* or *melted*.

 We

We see therefore, that Light is produceable by the Attrition of *Glass* on *Glass*; not only in *Vacuo*, and *Common Air*, but also in *Water* too. And hence 'tis evident further, that the Glasses are not heated red-hot by the Attrition; whatever such resemblance the Colour carries along with it.

An Experiment concerning the Production of a considerable Light, upon a slight Attrition of a Glass Globe exhausted of its Air.

I Took a Glass Globe of about 9 inches diameter, and exhausted the Air out of it; then (having turn'd a *Cock*, which prevented the return of the Air) I took it from the *Pump*. The Globe being thus secur'd, I fix'd it to a Machine, which gave it a swift Motion with its Axis perpendicular to the Horizon: and then applying my naked Hand (expanded) to the surface of it, the result was, That in a very little time a considerable Light was produc'd. And as I mov'd my Hand from one place to another (that the moist Effluvia, which very readily condense on the Glass, might, as near as I could, be thrown off from every part of it,) by this means the Light improv'd; and so continued to increase, till words in Capital Letters became legible by it: (as has been observed by Spectators.) Nay, I have found the Light produc'd to be so great, that a large Print might without much difficulty be read by it: and at the same time, the Room, which was large and wide, became sensibly enlightned, and the Wall was visible at the remotest distance, which was at least 10 Foot. The Light was of a curious Purple Colour,

Colour, and was produc'd by a very ſlender touch of the Hand; the Globe at the ſame time being ſcarce ſenſibly warm: neither could I ever find, that a more violent Attrition did contribute any thing to the encreaſe of the Light.

Now after this Attrition of the exhauſted Globe had been continued for ſome time, the Cock was turn'd, which gave liberty to the Air to enter into the Globe through the Joynts of the Screws; the motion of the great Wheel, and the application of the Hand, continuing all the while as before. Then I obſerv'd, that as the Cavity of the Globe became more and more repleniſh'd with Air, ſo the mode of the Light continued to alter, till the ſame quantity of Air was re-admitted, as had been exhauſted. And when ſuch a quantity was once enter'd, there was then as great a difference between that Light and the Light produc'd when the Globe was empty of Air, as between the Lights produc'd from Mercury, when the Experiment was made in *Vacuo* and the open Air. For if a Man touch'd the Globe with his Fingers, there were ſpecks of Light (tho' without any great Luſtre) ſeen to adhere to them. Nay, while my Hand continued upon the Glaſs, (the Glaſs being in motion,) if any Perſon approach'd his Fingers towards any part of it in the ſame Horizontal Plane with my Hand, a Light would be ſeen to ſtick to 'em, at the diſtance of an inch or thereabouts, without their touching the Glaſs at all; as was confirm'd by ſeveral then preſent. And 'twas obſerv'd alſo, that my Neckcloth, at the ſame time, at an inch or two diſtance from the Globe, appear'd of a fiery Colour, without any Communication of Light from the Globe.

This

This was the Event of this Experiment, at the *first time* of making it; in which case the Air was let into the exhausted Globe *all at once.*

But at the *second time* of making this Experiment the Air was not all let in at once (as before,) but *gradually*, and *at several times*; by which means the Modes of Light produc'd in the several different Mediums was the better observable: Tho' it must be own'd, that here was no great alteration of the Light, either as to its Vigour or Colour, till so considerable a quantity of Air was let in, as amounted to more than one quarter part of the Globe's natural Content. But many times, before half its Content (as near as I could guess) was let in, the Light began to branch it self into pleasant Figures, from that side of the Globe touch'd by the Hand; so that the whole body of the Globe was fill'd with these fine Appearances. And as more Air was admitted, so the Stems of these Branches of Light became more and more slender and minute, striking then also against the opposite side of the Glass, and rebounding from thence in a manner very surprizing, and delightful to behold; till at length, more Air still being let in, the Light and the Figures both diminish'd, and continued so to do, till the Appearance became the same as was related at the conclusion of the first Trial.

I would only note here, That what difference soever there was, as to the latter parts of these two Trials, the former parts of both were alike; except only, that in the second Trial, upon the application of a piece of white Sheeps-leather, a good Light was produc'd while 'twas held to the Globe with the Wooll-side next it; but when 'twas turn'd

with

with the other ſide to the Globe, tho' it was continued thus expos'd for ſome time, yet no Light appear'd: But then turn it again, and the ſame Light would appear as at firſt. And thus it happen'd upon ſeveral times repeating the ſame.

Having taken notice (in the Experiment now recited) *that the higheſt degree of Rarefaction of the Air in the Globe, is by no means neceſſary to the production of this Light, ſince it would be very little leſſen'd, either as to Vigour or Colour, till (perhaps) more than one fourth part of the Air was let in:* I would add further, That I have often obſerv'd the ſame thing, as to the Light produc'd in the *Mercurial* Experiments; tho' the Colour indeed was not the ſame, for in thoſe Experiments it was always pale. And ſince in thoſe Experiments with Mercury, there is ſuch a ſeeming congruity of Appearances, in all circumſtances, with thoſe made on the Attrition of Glaſs without it; one might conjecture with ſome probability, that the Light produc'd proceeds from ſome Quality in the Glaſs, (upon ſuch a Friction or Motion given it,) and not from the Mercury upon any other account, than only as it is a proper Body, which, by beating or rubbing on the Glaſs, produces the Light. And that which ſeems to back ſuch a Conjecture, is, that having rubb'd the upper or empty part of a Mercurial Barometer between my Fingers, a Light follow'd upon it without any motion of the Quickſilver. But notwithſtanding all this, the matter is doubtful; and there may (for all that we know) be a Luminous Quality in Mercury, as well as in Glaſs or other Bodies; which the following Experiment (purpoſely made) ſeems to countenance.

I took

I took a ſmall quantity of Quickſilver, and put it into a Galley-pot, wherein Varniſh had often been us'd, by which means the Pot was pretty well lined with it. The Weather at that time was *moiſt*, which influenc'd the Varniſh ſo far, as to ſoften it a little. Now when this Galley-pot, with its contain'd Mercury, came to be in *Vacuo*; as ſoon as the Pump was ſhook, a Light appear'd: and this without any concurrence of Glaſs, or the aſſiſtance of a more proper and favourable Seaſon. I purpoſely mention the moiſture of the Weather, becauſe a humid Air would ſometimes render the Experiment unſucceſsful, even in Glaſs, or at leaſt take off very much from the Appearance of it.

Farther; I am inform'd by ſeveral Perſons of Credit, That *Mercurius dulcis*, if broken in the dark, will yield notable Flaſhes of Light. But ſince the Mercury in that Preparation is pointed with Salts, (and each Globule of it conſequently wrapt up in the ſame,) a Man can't be ſure that the Salts do not contribute to the Phenomenon: For I have often obſerv'd, that Loaf-ſugar, when ſtruck or broken in the dark, affords a Light; and I can't tell but Salts, as cloſely united in their parts as Sugar, may, upon a violent ſeparation of 'em, do ſo likewiſe: But this I intend to enquire into by ſome farther Trials, *viz.* *Firſt*, I deſign to try whether *Mercurius dulcis* will afford any Light, when broken in *Vacuo*; ſince, if it proceeds from the Mercury, and there be ſuch a quality in that Body, 'tis highly reaſonable to expect it then, ſo rare a Medium being the moſt proper to diſcover it in. *Secondly*, What the Salts will do without Mercury, both in the open Air and in *Vacuo*; for there are ſome Bodies, which appear luminous in the open Air, and yet totally

loſe

loſe that quality in *Vacuo*. As for inſtance; I took a piece of Wood, (which I ſuppoſe had lain underground a conſiderable time,) very moiſt, but not rotten. In the dark it appear'd very vividly of the colour of Fire: But having inclos'd it in a Receiver on the Pump, I found that as the Air was withdrawn, the Fire-like appearance proportionally decay'd; till at laſt in *Vacuo*, it became perfectly void of Light; and then, as the Air was let in again, ſo it recover'd its firſt brightneſs. This I repeated ſeveral times with the like ſucceſs.

But to return now to the Experiment. The matters of Fact to be obſerv'd from thence, are reducible to the *following* Heads.

The *Production of a Light* by the *Friction* of the Hand on the ſurface of *the Exhauſted* Globe.

The *great improvement and encreaſe of this Light*, by the *motion* of *the Hand* from one place to another.

The *ſlightneſs of the Friction* requiſite to produce this Light; and its not encreaſing by a more *violent* one.

The *Alteration of the Light*, upon the *Re-admiſſion* of the Air.

The *Continuance of that Alteration*, as more and more Air *was admitted*.

The *Light's communicating it ſelf to Bodies plac'd near it*, when the *whole quantity of Air* drawn out was re-admitted.

This, when the Air was let in *all at once*.

But when the Air was let in *gradually*; then *no great change of the Light*, as to *colour* or *vigour*, till more then $\frac{1}{4}$ of the Globes natural content of Air was admitted.

The wonderful *Figures*, *Branchings*, and *Reverberations* of the Light, as more Air was let in, till it came to the bounds of a certain quantity of Air admitted, where the appearance was at the height.

The *gradual diminution both of the Light and Figures*, (after it was come to that limit,) upon the admiſſion of more and more Air.

An Experiment concerning the Electricity *of Glaſs, diſcovering it ſelf in an extraordinary manner, upon a ſmart Attrition of it.*

HAving procured a Tube, or hollow Cylinder, of fine *Flint* Glaſs, about one inch diameter, and thirty in length; I rubb'd it pretty vigorouſly with *Paper* in my Hand, till it had *acquir'd ſome degree of heat*. I then held it towards ſome pieces of *Leaf-Braſs*; which were no ſooner within the Sphere of Activity of the *Effluvia* emitted by the Tube, but they began to be put into brisk motions, and yielded the following ſurprizing appearances.

They would *leap towards the Tube, at a very conſiderable diſtance from it*; nay, I have found, that ſometimes the diſtance of 12 or more inches has not prevented their doing ſo.

Sometimes they would *adhere and faſten* to the Tube, ſettling themſelves on its ſurface, and there remain quiet: and ſometimes they *would be thrown off from it, with a very great force*, even to the diſtance of 6 or 7 inches. And not only when they *adher'd to the ſurface* of the Tube, would they thus ſuddenly and precipitantly be driven from it; but alſo *in their motion of aſcent towards it*, even when they were advanc'd vanc'd ſo far as to touch the Tube, this repellent force would take place, and hurry them *downwards* with a great velocity.

And (which ſtill adds to the Wonderfulneſs of the Phenomenon) they would often *repeat this alternate riſing and falling*; the *Attractive* and *Repulſive* forces (whatever they are,) exerting themſelves as it were by turns; the one drawing up, and the other beating down theſe light bodies; and that for ſeveral times one after the other.

Neither is this all the variety which the Phenomenon afforded; for ſometimes they would *move but ſlowly* towards the Tube, ſometimes they would *remain a ſmall time ſuſpended, between the Tube and the Table* on which they were firſt laid; and at other times (which is no leſs ſtrange than the former) they would *ſeem to ſlide along in the Direction of the ſides of the Tube*, and that *without touching it.*

But beſides theſe, there are yet ſome other Obſervations relating to this Experiment, which I think not amiſs here to mention.

Firſt, The hotter the Tube was made by rubbing, to the greater diſtance did the attractive force extend it ſelf. But that it will anſwer in proportion to any degree of heat excited, is what I will not venture to determine. I mean; whether, encreaſing ſtill the degree of heat in the Tube, the Sphere of the attractive Power will ſtill be proportionably enlarged; or whether there may not be ſome certain degree of heat, which may carry the attraction to its utmoſt limits of diſtance, ſo that all degrees above that, ſhall produce leſs powerful effects this way: Whether this be ſo or no, (I ſay,) is a point which will require farther Trials, and more Conſideration, in order to a compleat deciſion of it.

Secondly, Tho' all the various appearances now mention'd, as to the motions of the attracted little Bodies, do not happen at every Trial, yet many of them are obſervable at all times; neither are there any hinted, but what I have at ſome time or other punctually obſerved. And the reaſon of this difference, ſeems very probably to ariſe from the different Temper and Conſtitution of the Air. For when the Air is clogg'd, either with *humid and aqueous*, or other *more groſs and ſolid parts*, rais'd up from the vaſt Fund of Terreſtrial matter here below, there's no doubt but the reſiſtence theſe fine Effluvia then meet with in their way, muſt be much greater than when the Air is free, and no ſuch Impediments lie to oppoſe them in their paſſage. For the Effluvia, how ſubtile ſoever they can be imagin'd to be, are yet *Body* and *Matter*, and muſt therefore be liable to the common Laws of Bodies, which is to be reſiſted in ſome proportion to the ſtrength and denſity of the *Medium*. Neither is it improbable, but that in a moiſt Conſtitution of the Air, the watery parts may run together, and condenſe on the ſurface of the Tube; and ſo choak up and obſtruct the paſſage of the Matter, which otherwiſe would be emitted very vigorouſly from thence. And indeed, I have always obſerv'd, that moiſture is a great Enemy to all Experiments of this kind: The reaſon of which, I think, is pretty obvious from what was juſt now hinted about the *reſiſtence*.

How-

However, there is an Experiment, which looks, as it were, like an ocular Demonſtration of this; in which, the reſiſtence of the Effluvia by certain little Bodies interpos'd, was render'd manifeſt even to the Senſe. For having plac'd a piece of fine *Muſlin* between the Tube and the foremention'd pieces of Leaf-Braſs; all the excitation I could give the Effluvia by ſtrong rubbing, would not raiſe them ſo far, as to make them give the leaſt ſenſible motion to any one of thoſe little Bodies, even tho' the Tube was held very near them: yet at the ſame time, if the Muſlin were taken out of the way, the Effluvia would impreſs thoſe Bodies ſo vigorouſly, as to give them a motion at 3 or 4 times that diſtance. And this Phenomenon, I think, will eaſily be allow'd to be the meer Effect of the Obſtruction of the paſſage of the Effluvia by the fine Threads of the Linnen; which threads either wholly ſtop'd and detain'd, or elſe broke and obtunded the force of thoſe active parts emitted from the Tube by Friction; ſo that they could not ſhake or agitate the light Bodies, that lay expos'd to them.

I could add moreover, That not only the *moiſt*, but the *cold* Temper of the Air, may be partly a cauſe of the Differences diſcernible in the Effects of this Experiment: For when it was firſt made, it was Summer-time, as well as dry weather; and the ſucceſs ſeem'd to be more conſiderable then, than it has been ſince the declenſion of that Seaſon of the Year. And indeed, conſidering that all thoſe Effects of the Effluvia upon other Bodies, muſt depend either upon the *quantity of the ſubtile matter emitted*, or *the force and velocity of its impulſe*, or elſe upon *both theſe conjunctly*; 'tis not ſtrange that warm Summer-weather ſhould cauſe a conſiderable difference: becauſe the

vigorous

vigorous action of the Solar Rays does then more effectually ſhake the parts of all Bodies, opens and unlocks their Pores, and ſo makes way for a freer and more plentiful emiſſion of their Effluvia; and becauſe alſo, at the ſame time, the more fine and rarefied ſtate of the ambient Medium (the natural reſult of a predominant heat) will allow them to expand themſelves with more advantage. However, in colder weather, the Effects are very ſenſible, and great enough (notwithſtanding the diſadvantage ariſing from thence) to procure any curious Perſon's Attention to the Phenomenon.

Thirdly, When the Tube became hotteſt, by the ſtrongeſt Attrition; the Force of the Effluvia was rendred manifeſt to another *Senſe* too, namely, that of *feeling.* They did not then only produce all the forementioned Effects in a more remarkable manner, but were alſo plainly to be *felt* upon the Face, or any other tender part, if the rubb'd Tube were held near it. And they ſeem'd *to make very nearly ſuch ſort of ſtroaks upon the Skin, as a number of fine limber Hairs puſhing againſt it* might be ſuppos'd to do. This vigorous Action of the Effluvia put me upon an attempt, to find in what manner ſuch a motion was propagated, and in what Figure or ſort of Track it went along. For which end, I held the rubb'd Tube near the *Flame* of a Candle, *Smoke*, *Steam*, *Duſt*, and the *ſurfaces of Liquids*; but without any manner of ſucceſs.

The reaſon of which, I attribute to the impediments the Effluvia met with from theſe Bodies the Tube was plac'd near. For the ſmall parts of Duſts and Powders, the ſteams of Liquids, the oleaginous Fumes of Flame, and the like ſort of parts in Smoke it ſelf, immediately adher'd to the ſurface of the Tube,

Tube, and ſo kept in the Effluvia: which therefore requir'd the aſſiſtance of a freſh Attrition to open their paſſage and give them vent again.

And thus much for the Firſt part of this Experiment; in which *the Tube was rubb'd while it continued full of Air*.

Secondly, We are now *to conſider the Effects of the Attrition of the ſame Tube, when the Air was exhauſted and drawn out of it.* And here I obſerv'd, that tho' it were rubb'd with equal or greater force than was imploy'd about it when full of Air, *yet the attractive power was very little diſcernible.* The Effluvia ſcarce diſcover'd themſelves, by any motion or diſturbance given to the Leaf-Braſs, even tho' it was plac'd within a quarter of the diſtance at which it had been attracted before. Indeed, when the Tube was very warm, and held very near, there would be ſome little motion given to thoſe ſmall Bodies; but 'twas too ſmall by far to be compar'd with what was impreſs'd when the Tube was full of Air. Beſides, I doubt not but there was ſome portion of Air left in the Tube, and ſo the Attraction might continue in proportion to the quantity of Air remaining: or perhaps, the heat produc'd by a ſmart Attrition of the Glaſs, may (in this Experiment, as well as ſome others) be in the ſtead, and ſupply the place, of ſuch a quantity of that Element.

But to return to the Experiment. When the Air was let into the Tube again, the attractive power was immediately reſtor'd. Before any new Attrition could be given the Tube, or that it was remov'd from the diſtance and poſition it was held at when exhauſted, even then did ſeveral of the forementioned Bodies (which before ſeem'd to be wholly at reſt) begin ſuddenly to move; and ſome of

them

them were attracted up to the Tube it ſelf. And then, laſtly, upon a freſh Attrition, the Tube recover'd it's *Electrical* quality as vigorouſly as at firſt.

Hitherto we have conſider'd the Phenomena of the Tube both when full of Air, and when exhauſted; but in each caſe *ſtill in the Light*. We are now to conſider what occurr'd upon the *like Attrition given it in the Dark*. And firſt, the Tube being *full of Air*; 'tis obſervable, that when the Glaſs became warm, *a Light would continually follow the motion of the Hand* backward and forward: And at the ſame time, if another Hand was held near the Tube, a Light would evidently break forth from it, and That *accompanied with a Noiſe*, reſembling that of the cracking of a green Leaf in the Fire; tho' not ſo loud: Yet when the Experiment has been made in a very ſtill and ſilent place, I have heard ſeveral cracks, at ſeven or eight foot diſtance, or more. Farther; if any other Object (beſides the Hand) was brought near the Tube, tho' it did not touch it, yet *a Light would fix upon it*, and give much the ſame appearance as upon the Hand; as I have tried with *Gold*, *Silver*, *Braſs*, *Ivory*, *Wood*, &c.

But now, when the Tube was *exhauſted of its Air*, there was a conſiderable difference, as to this Light and its effects: For upon the firſt Attrition of the Tube, *a much greater Light indeed did enſue*; but then the *quality of giving Light to a Body held near it*, *ſeem'd to be quite loſt*: And (which is another Difference no leſs remarkable,) the Light produc'd upon the Attrition of the exhauſted Tube, appear'd to be wholly within it; whereas that which was diſcover'd when the Tube was full of Air, ſeem'd to be altogether on its out-ſide.

And

And thus much for the Experiment, as made upon *a Tube or hollow Cylinder of Glaſs.*

Having now, in the next place, procur'd *a ſolid Cylinder of Glaſs*; I made the ſame Trials with that as the former; and *found no great difference in the Effects*; only the emiſſion of the Effluvia *ſeem'd to continue a little longer*, *but not to attract at a greater diſtance than the other* (as far as I can yet diſcover). With this new ſolid Tube, I made the following Experiment.

I took a little quantity of *Lamp-Black*, and having dried it on a Paper before the Fire, I expos'd it to the Tube (which had been rubb'd till it was warm): And it was no ſmall Entertainment, to ſee how briskly the little blackParticles were agitated by the force of the Effluvia from the Glaſs. They appear'd to riſe and fall,to move upwards and downwards with great velocity. And tho' their Specifick Gravity was ſo ſmall, that when they fell by their own weight, they could not be heard; yet they were return'd upon the Paper with ſuch a force from the Tube, that their ſtroaks upon the ſame made a very ſenſible noiſe. So great was the repellent force of the Effluvia from the ſolid Tube. So that now laying the accounts of this Experiment together, we have the following Heads of matter of Fact to conſider.

The *Various ſurprizing motions of the Leaf-Braſs*, expos'd to the hollow Tube, upon the Attrition of it.

The *Encreaſe of that Effect*, upon the encreaſe of the Heat in the rubb'd Tube.

The *Difference obſervable in that Effect*, according to the different Temper and Conſtitution of the Air.

The *Destruction or Ceasing of that Effect*, upon the exhaustion of the Air out of the Cavity of the Tube.

The *Return of that Effect, in a good degree*, upon the Return of the Air, even without the help of any new Attrition: and the compleat recovery of it to all degrees, when that Attrition was again given.

The *Emission of a Light* from the Tube full of Air, when rubb'd to a degree of warmth in the dark: and this Light accompanied with a cracking noise.

That Light's fixing and settling it self upon Bodies laid in its way.

The *Loss of this Quality* (of fixing upon other Bodies,) tho' with the advantage of a bigger Light; when the Attrition was given to a Tube exhausted of its Air.

The *different Seat and Place* of the Light, when the Tube was full of Air, and when exhausted: being in the *former* case *without*, and in the *latter* wholly *within* the Tube.

Lastly, The same Effects (except what depended on the Exhaustion) produc'd by a *solid*, as by a *hollow* Tube.

Having now deduc'd some things from the Experiment, which may (I think) pass without much dispute for Truths and Matters of Fact; I would propose some things by way of *Query*, in order to a farther clearing of this Subject.

Query 1. How is it that Attrition does excite and bring forth the Effluvia of Electrical Bodies? They are, in the present case, propagated from the Body of the Glass with a considerable force: What is the immediate cause of that *Impetus*? And by what means do they come to be so affected?

Query 2.

Query 2. Why are the Effects of the Effluvia so much greater when the hollow Tube is *full of Air*, than when it is *exhausted*?

Query 3. Why does the Light produc'd upon the Attrition of the *exhausted* Tube, appear wholly *within* it; and That produc'd upon the Attrition of the Tube *full of Air*, appear altogether on its *out-side*?

Query 4. Can the preserving, or the taking away the Equilibrium between the external Air and the Air included in the Cavity of the Glass Tube, be of any moment towards the production of the Effects mention'd in the two last Queries?

A Continuation of the Experiments on the Attrition of Glass.

I Procur'd a Glass of a Figure as nearly *Cylindrical* as might be, whose diameter and length were each about 7 inches. The Axis of this Glass lying parallel to the *Horizon*, and the contain'd Air being exhausted, it had a Motion given it by a Machine of a new Contrivance. And the Effects *Plate* VII of this, with respect to the Light produc'd upon the Attrition of it, were much the same as those in the Experiments formerly mention'd. But when the Air was let in again, and the Motion and Attrition given, as at first; I was surpriz'd with the appearance of a *brisk and vigorous Light continued between the point of my Finger and the Glass*. It was not only plainly visible on the Finger; but besides, seem'd as it were to strike with some force upon it, being easily to *be felt by a kind of gentle pressure,* tho' the moving Body was not touch'd with it by near half an inch. This Light seem'd to issue from the Glass with *a considerable noise* (not much unlike that of wheezing, tho' something smarter;) and 'twas easie enough to distinguish it from the noise made by the working of the Engine, which notwithstanding was not a small one. And the Phenomenon was the same, as to both the Parts of it, (I mean both the *Light* and the *Noise*,) when the Experiment was made in the Day-light, as when in the Dark. For in a very light Room, an Hour or two after Noon, the Glass being put into Motion, and the Attrition made, and the Finger approach'd near it (as before;) a pure Purple Light immediately extended it self from the Finger to the Cylinder, and was accompanied with the like noise as before-mention'd. And this Trial I have repeated several times, at different hours; but still with the same Success.

I render'd these luminous Effluvia more remarkably conspicuous, and at the same time more pleasing to the Eye of a Spectator, by another Experiment made after this manner: I took a bit of fine *Muslin*, and sew'd it to a couple of Wires bent circular-wise, that so it might surround the upper surface of the Glass; which it did at near 4 inches distance: The Muslin I made as ragged as I could, by breaking the Threads of it every where. The Glass being then put in motion, and the Friction made (as usually;) the Light threw it self abroad vigorously, and settled in small lucid sparks upon the ends of the torn Threads; looking there like so many little Stars, seen by a good Teleſcope in the *Milky way.* And from all theſe little Balls of Light together, there reſulted ſuch a ſort of a whiteneſs, as may be obſerved in the Heavens by the faint and weak Lights of all thoſe ſmall Stars mingled together.

In all theſe caſes, I could never find *that the addition of any external Heat, would do any thing to encreaſe the appearance of the Light produc'd.* I tried by placing a red-hot Iron juſt under the moving Glaſs; but this would do nothing at all, without the Attrition of the Glaſs; and if the Glaſs were rubb'd, the Effect was no greater, than if the hot Iron had not been there. And the reſult (as to this matter of *heat,*) was the ſame, both when the Glaſs was exhauſted of its Air, and when it was full.

And thus much, as to the Light *produc'd by the Attrition of the Cylindrical Glaſs.*

Secondly, as to the *Electricity* of it; I did not find this Effect more conſiderable here as to quantity, than what was related in the former Experiments of the Tubes. But notwithſtanding, I have diſcover'd ſome Properties of this Electrical matter, which may ſeem wonderful to thoſe that nicely conſider them; *ſince they afford us a ſort of repreſentation of the great Phenomena of the Univerſe.* For, having obſerv'd that light Bodies, plac'd near any part of the rubb'd Cylinder, ſeem'd to be equally attracted; I contrived a Semi-circle of Wire, which I could faſten at a conſtant diſtance, making it encompaſs the upper Semi-cylindrical Surface of the Glaſs, at 4 or 5 inches diſtance. This Wire had ſeveral pieces of Woollen Thread faſten'd to it, ſo as to hang down from it at pretty nearly equal diſtances. The length of them was ſuch, that being extended in a direction towards the center of that imaginary

Circle

Circle on the ſurface of the Glaſs, in the Plane of which the Wire was plac'd; they would then reach within leſs than an inch of the Circumference of that Circle: but if left to their own liberty, they hung in that parallel poſition to each other, which is re-
Plate VII preſented in *Fig.* 1. The Cylinder was plac'd with its Axis parallel to the Horizon; and in this poſture, it was turn'd ſwiftly about; and then by the rapid motion and agitation of the ſurrounding Air, the Threads were forc'd into ſuch poſitions, as are expreſs'd in *Fig.* 2. *viz.* they were lifted up and bent upwards from the Axis of the Cylinder.

All this while, here was only the ſwift motion of the Cylinder round its Axis, without any Attrition. But now, when I came to apply my Hand to the lower part of this Glaſs (ſo ſwiftly whirl'd about,) and conſequently to add *Attrition* to the former motion; *the Threads preſently began to change their di-*
Fig. 3. *rection, and all harmoniouſly pointed to the center of the Circle, in whoſe Plane the Wire was plac'd.* Neither were they at all diſorder'd or flung out of that poſition, by the Wind occaſion'd by that violent motion; but (as if there had been no ſuch hurry of the Air about them) they ſtill perſiſted in their central direction. And to render it moſt ſenſibly convincing, how abſolutely this Effect depended upon the Attrition; I found *I could by ſhifting the place of the Attrition hither or thither, draw the Threads towards this or that end of the Cylinder; but yet they all ſtill went uniformly converging towards ſome center in the Axis of it; ſo that they form'd themſelves into a ſort of Conical Surface.*

Farther;

Farther; if the Wire with its looſe Threads was revers'd, ſo as to encompaſs the lower part of the Cylinder, (as before it did the upper part;) yet the Effect ſtill anſwer'd with the ſame exactneſs. *For the Threads were all erected into ſo many ſtrait lines, ſtill directing themſelves to a center in the Axis of the Glaſs.* Fig. 4.

Hitherto the Axis of the Cylinder was plac'd *Horizontally*; in the next place, I ſet it in a *vertical* poſition, ſo that it ſtood perpendicular to the Plane of the Horizon; in which caſe I made uſe of a Wire-hoop, which was neceſſarily to be plac'd parallel to the Horizon, that it might encompaſs the Cylinder in the ſame manner as the Semicircular-Wire did before: only one ſmall part of this circular Wire was left open, to make way for the touch of the Hand, which was to give the Attrition. And the Wire being thus plac'd, it was evident that the Threads (without ſome external force to ſupport 'em) muſt all flagg and hang perpendicularly downwards. Yet, as ſoon as the Motion and Attrition were given, the Threads preſently began to be extended; and, as if they were become ſtiff and hard, form'd themſelves into an Horizontal Plane; their looſe ends pointing to a center in the Axis of the Glaſs (as before.)

And thus in all ſorts of Poſitions whatſoever, both of the Wire, and of the Glaſs too; were the Threads acted by a ſort of centripetal force, to the Laws of which they were always conformable.

The matters of Fact obſervable in this Experiment, may be compris'd under theſe Heads.

The *Continuation* of a brisk Light between the Hand and the Glaſs.

The

The *Senſible Force* and *Preſſure* of that Luminous matter; which was very eaſie to be felt.

The *Noiſe* accompanying the eruption of the Luminous matter, and the Loudneſs of it, which render'd it diſtinguiſhable from that of the Engine.

The *Conſtancy* of the Light and Noiſe at all Seaſons.

The *Indifferent regard* of this Light to the preſence or abſence of Heat.

This as to the *Light*.

As to the *Electricity*.

An *Equable diffuſion of Electrical Matter*, in Planes parallel to the Baſis of the Cylinder; and the *Direction* of the Threads to the center of thoſe Circular Planes.

The *ſteddy and unvaried* Direction of the Threads, notwithſtanding the Wind occaſion'd by the violence of the Motion.

The *Eaſie Excitation* of the Electrical Matter in any of the parallel Planes of the Cylinder, occaſion'd by the ſhifting of the Hand into a new place: Alſo the *Variation* of the *Figure*, and *Direction* of the Threads, conſequent thereupon.

The *Conſtant Direction* of the Threads to ſome center in the Axis, in *all the Poſitions* of the Wire, and of the Cylindrical Glaſs; the Effect being ſtill the ſame, whether the Wire was *above* or *under*, or the Glaſs plac'd with its Axis either *parallel* or *perpendicular* to the Horizon.

Some

Some farther Experiments relating to the Electricity *of* Glaſs.

IT has been ſhewn before, that the ends of the Threads which were diſpos'd in the *Semicircular Wire*, were, upon the Attrition, directed to a *Center* in the *Axis of the Cylindrick Glaſs*. There are ſome other Obſervations and Diſcoveries which I have ſince made, relating to the ſame thing; which are no leſs ſurprizing; and which I ſhall therefore here give in their order.

Experiment I.

WHEN the Attrition of the Glaſs has been continued a little while, and the *Woollen Threads* laid hold of by the *Effluvia*; then, tho' the Glaſs had no motion at all, and no Friction continued, yet would all the Threads continue in their *ſtrait directed poſture*, and that for the ſpace of 4 or 5 minutes: nay, it would ſometimes be longer before they could diſengage themſelves from the force and action of the Effluvia.

And while the Effluvia acted with ſo much vigour, as to ſuſtain the Threads in their *Central Direction*; if a Finger (or any other body) were put near the Extremities or *Pointing ends* of the Threads, they would *avoid and flie* from it, just as a *Magnetick Needle* does, when the *disagreeing* Pole of the Load-stone approaches to it.

But if the Finger were held at about an inch distance from the end of any such Thread, the Thread would (usually, tho' not always,) be *attracted towards it*; plainly removing it self out of its place, to come nearer thereto.

If any Body be *interpos'd between the Glass and the said directed Thread*; then the Thread, depriv'd of the directing force of the Effluvia, immediately returns to its *first* and *natural position*, *viz.* such as its own gravity gives it. But if that interpos'd Body *be taken away again*, then (provided the Thread has not remov'd it self too far out of the reach of the Effluvia) it will *return again to its central tendency*, and remain in it, till its gravity becomes too great for the decreasing force of the Effluvia to support it any longer in such a Direction.

NB. I have since tried the same thing with a Glass *Globe*, and found that when the Attrition was
Fig. 7. Plate VII made, *it would attract the surrounding Threads in all manner of Positions, and direct them to its Center.* What we may observe from hence therefore, is,

First, The *vigorous* and *lasting Action of the Effluvia*, excited by this Attrition; seeing the *Tension* of the Threads was still continued, tho' the *Friction and Motion* of the Glass were both ceased.

Secondly, A plain Instance of a *Repulsive* and *Attractive* force. For the Threads *avoided* the Finger, (as if acted by some *centrifugal* force,) when it approach'd very near their Extremities: and at another di-

diſtance, ſomething like a *centripetal* force would cauſe them to incline and *move themſelves towards it.* So that in theſe ſmaller Orbs of matter, we have ſome little reſemblances of the Grand Phenomena of the Univerſe.

Thirdly, The *certain Dependence* of this Phenomenon (*viz. the extenſion of the Threads*) upon *the action of ſome Matter,* whoſe *Direction is in ſtrait lines towards the Glaſs.* For upon the interpoſition of any Body between the Threads and the Glaſs, they loſe their regular extenſion, and hang as their own weight cauſes them. And what can ſuch Interpoſition of a Body poſſibly do in this caſe, but only interrupt the courſe of the Matter which is the cauſe of the Extenſion, and conſequently make way for the gravity (which was overcome by a ſuperiour force before) to exert it ſelf again?

EXPERIMENT II.

I Took an *Hemiſpherical Glaſs* (ſuch as is repreſented at *Fig.* 5.) of about 6 inches diameter: Into this I convey'd a *Stick,* in manner of an *Axis,* which had the Woollen-Threads (formerly made uſe of) tied about it. The Glaſs was ſcrew'd by the *Neck* to one end of a *Spindle*; and being fix'd on the *Machine,* the Great *Wheel* was turn'd, and the Friction made on the outer ſurface of the Glaſs, as uſual. And now the Threads preſented a Phe- *Plate* VII *Plate* VII nomenon not a little pleaſant and ſurprizing to behold; but yet ſuch as I expected and hop'd for, in the contrivance of this particular *Apparatus*. For here was juſt the *Reverſe* of what happen'd when the Semicircular Wire was plac'd on the out-ſide: That is, the Threads *here* iſſued like Rays, *from* a Center *outwards*; as *there* they *converg'd to* a Center *within*. The nearer they were to the *concave* ſurface of the Glaſs, the farther their Extremities diverged from one another, in *this* caſe; as in *that* caſe, the nearer they were to the *convex* ſurface, the nearer were their Extremities to one another. In either caſe, the *Divergency* or *Convergency* was *from* or *to* ſome Center in the Axis; and all the difference was, that in the one caſe the tendency was *from without*, *inwards*; and in the other, 'twas *from within*, *outwards*. The appearance of the Threads in this laſt Experiment, was as is repreſented at *Fig*. 6.

Plate VII

And what was very conſiderable farther, was, that while the Threads *lay in this poſition*, like ſo many Rays of a Circle extended; if the Finger were held near the *out-ſide* of the Glaſs, a Motion would be communicated to the point of that *Thread which was neareſt within*: ſo that *by the motion of the Finger* the *Thread would be driven any way* before it. And it would ſeem to fly and avoid the Finger, held on any ſide; tho' the convex ſurface of the Glaſs were not touch'd by it, by ½ inch and more (as I have ſometimes ſeen.) Likewiſe, if the Threads were remov'd to the *out-ſide*, and the Finger mov'd about within, the Threads would play about with the like motion.

And

And indeed, *generally* ſpeaking, *the Threads ſeem to avoid the approach of the Finger :* tho' I have ſometimes obſerv'd them *to jump ſuddenly towards it*, at more than an inch diſtance.

From the Experiment now recited, we may obſerve,

The remarkable *Uniformity* and *Agreement conſpicuous in this Phenomenon, with That where the Threads were diſpos'd on the out-ſide of the Glaſs.* For in both, the Threads were directed, according to the courſe of the matter; which in one caſe acted *one* way, and in the other the *contrary.* And by comparing both, 'tis very plain, that *there is no more than one and the ſame Cauſe of both.* For the ſame Cauſe which in one circumſtance (*viz.* when the Threads were plac'd on the *out-ſide)* would make them *converge* towards the *Convex* ſurface of the Glaſs, would alſo in the contrary circumſtance (*viz.* when they were plac'd on the *in-ſide)* make them *diverge* towards the *Concave* ſurface.

Again, the Agreement anſwers, not only with reſpect to the *Direction* of the Threads, but alſo to the *Motion* of them. For in the former Experiment, the Threads would ſometimes *avoid*, and ſometimes *incline towards* the Finger: and in the Experiment now mention'd, there were the like Indications both of a *Centrifugal* and *Centripetal* Force.

Experiment III.

I Took a Glaſs Globe of 9 inches diameter, which being *exhauſted* of its Air, I fix'd to a Machine, to give Motion to it, perpendicular to the Horizon. And to another Engine I fix'd another Glaſs Globe, plac'd at a diſtance ſomething leſs than an inch from the former Globe; and *having none of its Air drawn out.* The Machines being ſet to work, I apply'd my naked Hand to the Globe which *was full of Air*; the Effluvia of which (excited by the Attrition) quickly reach'd the *exhauſted* Globe, and *produc'd a Light* on that part of it which was neareſt the other. Now here was not the Friction of any other Body upon this Glaſs, to raiſe the Effluvia, and produce a Light; there was nothing *but the bare Action of the Effluvia from the other Globe*, which Effluvia ſupplied the place of a more ſolid Body, and made ſuch an Attrition as was neceſſary to the production of the Phenomenon. The Light was pretty vigorous, and ſpread it ſelf on the Globe as far as the Effluvia were capable of ſtriking on it. Its Colour was not near ſo much inclined to *Purple*, as it was when the Light was produc'd by the attrition of the Hand: But it would continue upon the Globe for half a minute or more, after the motion of the rubb'd Glaſs had ceas'd. On the other Hand, if the rubb'd Glaſs were kept in motion, the other being at reſt; the Light would die preſently;

ſently; but immediately recover'd **again, upon the** firſt motion given to it.

As an Appendix to this Experiment, I ſhall relate another, much to the ſame purpoſe.

I took a *long Glaſs*, whoſe Air was exhauſted, and which had lain by in that ſtate above ſix Months. After I had rubb'd this Glaſs a little with my Hand, to clear it of all Moiſture on the ſurface; I held it over the *unexhauſted Globe*, which was then in motion; and at the ſame time alſo I gave it (*viz.* the unexhauſted Globe) an Attrition with my Hand; upon which there were immediately large and ſurprizing *Flaſhes of Light* produc'd in the long Glaſs, tho' it neither touch'd the moving Globe, nor was provok'd it ſelf by any immediate *ſenſible* Attrition.

The Matters of Fact afforded by this whole Experiment, are reducible to the following Heads.

The Production of a continued Light on one *Glaſs exhauſted of its Air,* by another, *at a diſtance from it, being rubb'd while it was full of Air:* both being in motion, but one only rubb'd.

The Continuance of the Light on the exhauſted Glaſs, for ſome time after the Motion of the other Glaſs is at an end.

The ſudden Ceaſing of the Light, when the exhauſted Glaſs is at reſt, tho' the full Glaſs (on which alone the attrition is made) be kept in motion.

The Production of an interrupted Flaſhing Light upon an exhauſted Quieſcent Glaſs, held over another unexhauſted one in motion; the unexhauſted one being rubb'd at the ſame time.

And from hence we may obſerve,

First, *The Force and Vigorous Action of the Effluvia, by which they perform the Office of a ſolid Body.* In other Experiments, Lights were produc'd by the attrition of *one ſolid Body againſt another*: but here a Light is produc'd by *the friction of a very ſubtile Fluid upon a Solid.* So that this may come in, amongſt many others, as an inſtance of the powerful Effects of *ſmall* Bodies, when put into brisk and vigorous motions.

Secondly, *The great Intereſt which the motion of the exhauſted Glaſs has in the continuance and preſervation of the Light*: For if That be ſtop'd, the Light dies away preſently, tho' the unexhauſted Glaſs be in motion. And indeed 'tis evident, that the Action of the Effluvia upon the ſaid exhauſted Glaſs, is more extenſive, (and ſo capable of producing more conſiderable Effects,) when that Glaſs is in motion, and the parts of it conſequently ſucceſſively expos'd, by a quick Revolution, to the ſtroaks of thoſe Effluvia; than when it is at reſt, and the Effluvia beat only on one and the ſame particular part of the Surface.

And this is effectually confirm'd by the Obſervation of the *kind* of *Light produced on the exhauſted quieſcent long Glaſs*: For that was not a *continued*, but a *Flaſhing* Light; that is, ſuch as diſappear'd as ſoon as it was produc'd on the Surface of that quieſcent Glaſs.

Thirdly, *The difference in Degree and Intenſity of the Colour, which the different Circumſtances of the Friction are capable of producing.* For the Light produc'd by the *Attrition of the Effluvia* did not come near, as to the degree of *Purple*, to that Light which was produc'd when the *Attrition was made by the Hand.*

An

An Account of an Experiment, confirming the Production of Light by the Effluvia of one Glaſs falling on another.

HAving obſerv'd that the Effluvia of Glaſs, when they fell on an exhauſted Glaſs in motion, would exhibite ſuch an appearance, as if it were rubb'd by a viſible ſolid Body; I thought this further Confirmation of it would not be unacceptable. I took a large Receiver, of ſuch a form as is expreſs'd by a a a a; (Fig. 1.) within the Body *Plate* III of which I fix'd another, of ſuch a form, and in ſuch poſition, as is repreſented by b b b b. Their Axes were parallel to the Horizon, and fix'd one within another at c c. The outward Surface of the inward Glaſs was at leaſt an inch diſtant from the inward Surface of the outward one: and they were both turn'd by two large Wheels d d d d, whoſe *Bands* related to the ſmall Wheels e e e e, fix'd on the Axes of the Glaſſes. Before the Glaſſes were thus adapted to each other, the innermoſt was exhauſted of its Air; and then being ſet as the Figure deſcribes, I order'd that Wheel only to be moved which gave motion to the great Glaſs. The Thought which guided this Proceſs, was this; that when the Effluvia of the great Glaſs (by the application of my Hand upon it) ſhould reach the other; this other, notwithſtanding it was at reſt, would nevertheleſs be influenc'd by the Effluvia, and give a

 Light.

Light. The Effect anſwer'd my Expectation; for the Light appear'd, and ſpread it ſelf in numerous branches all over. This done, I caus'd the other Wheel (*viz.* that which gave motion to the includ-ed Glaſs,) to be turn'd; and then the Light became much more conſiderable, and, I think, the greateſt that has yet been produc'd in any Experiment made on this Subject. And I doubt not, but 'twould have been much more conſiderable, had the inward Glaſs fitted ſo, as nearly to touch the inward ſur-face of the outer Glaſs; the Effluvia of which (as it appears to me) would then have been capable of acting with more vigour on the inclos'd exhauſted moving Receiver.

Having in the next place caus'd both the great Wheels to turn the Glaſſes one and the ſame way, with as equal a degree of Velocity as they could; I did not find but the Light was then as ſtrong, as when their motions were juſt the reverſe. So that (as far as I can perceive) neither the contrariety nor agreement of the Motions does contribute any thing to this Phenomenon; but Motion it ſelf (without any particular Rules or Limitations) is ab-ſolutely neceſſary: As this, and the whole Courſe of Experiments on this Head, abundantly ſhews.

I obſerv'd farther, that tho' the Effluvia ſeem'd to be equally diſtributed on the outward ſurface of the inward moving Glaſs, yet the Light appear'd moſt vigorouſly on that ſide of it next the Attrition. And when either of the Glaſſes was at reſt, the other continuing in motion, (I ſay *either*; for upon Trial I found very little difference either way;) the ap-pearance of the Light would remain a conſidera-ble time within the exhauſted Glaſs, till the Efflu-via of the other were no longer capable of acting with

with a force upon it, requisit to produce the Effect. 'Twas not a little surprizing also to observe, that after both Glasses had been in motion for some time, and the Hand apply'd during that time to the surface of the outer one, that then, the Motions both ceasing, and no Light appearing at all, if I did but approach my Hand again near the surface of the outer Glass, there would be Flashes of Light (like Lightning) produc'd in the inward Glass; just as if the Effluvia from the outer Glass, had been push'd with more force upon it by means of the approaching Hand.

The matters of Fact afforded by this Experiment, may be compris'd under the following Heads.

The Production of a Light by the Effluvia of the outer circulating Glass, falling on the inward quiescent one.

The Extraordinary Augmentation of the Light, upon the circular motion of the inner Glass together with the outer one.

The Constant and Unvaried state of the Effect (as to the Vigour aud Strength of the Light,) whether the two Glasses were moved the same or the contrary way.

The most vigorous appearance of the Light upon the side next the Attrition, when both the Glasses were mov'd; notwithstanding the Effluvia seem'd equally distributed on the surface of the inward Glass.

The Continuance of the Light in the exhausted inner Glass till the Force of the Effluvia was spent; when either Glass was in motion, and the other at rest.

The Flashes of Light produc'd upon the inner Glass, by the approach of the Hand to the outer one, no Light at all appearing before: both Glasses being then quiscent, tho' both had been in motion, and the outward one rubb'd all the time of the Motion.

Query 1. Why is a Medium ſo very-much-rarefied, (as that in the exhauſted Receiver) requiſite to produce a Light?

Query 2. Does not this Light, produc'd by the Attrition of the Effluvia, ſhew, that Minute and Fluid Bodies, when put in violent motion, are capable of performing the ſame Effects as others of a more groſs and ſolid Nature?

SECT.

SECT. III.

An Experiment ſhewing the Difficulty of ſeparating two Hemiſpheres, *upon the injecting an* Atmoſphere *of Air on their outward Surfaces, without exhauſting the included Air.*

THE beſt Proof that can be given of the Truth of any Hypotheſis, is, that the Experiments made for that end, do all of them, and every way agree: That trying Nature on one ſide, and on the other, yet every way ſhe ſtill confeſſes the ſame thing. Thus with reſpect to the nature of *Sounds*; 'tis demonſtrable that the Air is the proper Vehicle or Medium for the propagation of them; becauſe Sounds do not only *leſſen and grow weaker*, according to the degrees of the Air's *Rarefaction*; but alſo become more *intenſe and ſtrong*, according to the degrees of its *Condenſation*. And I offer the following Experiment, to ſhew, that we have the very ſame degree of certainty of the Preſſure and Gravitation of the Air; in that the very ſame Effect is produced, when we make uſe of a *condens'd* Atmoſphere to work againſt *common* Air, as when we make uſe of *common* Air to work againſt a very-much-rarefied Medium or *Vacuity*. This Experiment, I hope, will be no leſs than deciſive of the Point ſo long in agitation, and ſet the Truth free even from

any

any poſſibility of being attack'd by the Objections of the Favourers of *Suction* and the *Funicular* Hypotheſis. 'Tis true, the Doctrine of the Air's Preſſure has been fairly and clearly demonſtrated by a great number of Experiments, already made for that purpoſe. But ſtill theſe People have found ſome Shifts and Subterfuges, by which they have made a ſhew of evading the Concluſive Force of the Experiments. They have ſtill had room left to ſay ſomething or other, which, how little ſoever it has really been to the purpoſe, has yet ſerv'd to keep the Controverſie alive, and make the Unskilful or Unthinking believe they had ſome Probability on their ſide. Now, to ſhew how unreaſonable thoſe Evaſions hitherto made uſe of, have been; as alſo at once to ſet the matter in a ſatisfactory Light; I proceeded in the manner following.

I took a ſtrong Glaſs *Receiver*, open, and arm'd
See Plate IV.
with *Braſs Hoops* at top and bottom; in which I plac'd two Braſs *Hemiſpheres*, joyn'd together on a wet Leather at b b b b (the diameter being 3½ inches), as alſo a Mercurial *Gage* repreſented by c c c c. To the Braſs *Hoops* were applied two Braſs Plates d d d d, with wet Leathers between them. To the upper Hemiſphere was ſcrew'd a large *Braſs Wire* e e, that paſs'd through a *Box* of Leathers f f, which was ſcrew'd on the upper Plate; and this Wire could eaſily be mov'd up and down without ſuffering any Air to paſs in along with it. This moveable Wire had a *Cock* g g g g ſcrew'd at the upper part of it, thro' which the Air was to be injected. In this manner were the upper and lower Plates firmly ſcrew'd to the Receiver by the Frame and Pillars h h h h h h.

Theſe

These things thus provided; an Atmosphere of Air was thrown into the Receiver; the quantity of which injection was easily discover'd by the forementio'd Gage cccc; the Air therein possessing but half the space it did before. When this was done, the *Syringe* ii was taken off, and an *Iron* with an *Eye*, represented by kk, was screw'd on in its place; by which means the whole Apparatus was suspended on a *Triangle* lllll. (Note, that the moveable Wire and upper Hemisphere related to this *Iron*; all the rest being part of the weight made use of to separate them.) After this, the Scale which hung at the Bottom had so much weight put into it, that all together made full 140 pound; and nothing less then this weight of 140 pound, would part the Hemispheres; so powerful was the Force and Pressure of the Atmosphere injected on their outward Surfaces, to hinder that Separation and keep them together. Now how those that espouse the *Funicular* Hypothesis, or that of *Suction*, will solve this from their Principles, I can't imagine! For how is it possible that any thing of that kind should take place in the matter before us? How and which way does any *Suction* drive these two Hemispheres together with such a force? or where's any room for a *Funiculus*, that may be imagin'd to be the cause of their Union and Compression? I can't see but the Matter of Fact is plainly beyond all Exceptions; and that What I have propos'd, is no less than an *Experimentum crucis*. Therefore,

Corollary. From hence *the Doctrine of the Pressure of the Air is certain.*

For

For there can't poſſibly be any thing aſſign'd as the Cauſe of this Compreſſion of the Hemiſpheres, but the Preſſure of the external condens'd Air on their Surfaces. For, whatever other Cauſe any one ſhall think fit to aſſign; it may eaſily be ſhewn to be impoſſible, from the Circumſtances of this Experiment. From whence the certainty of the Preſſure of the Air being eſtabliſh'd, I may fairly conclude, that the ſame Principle was the Cauſe of the Compreſſion of the Hemiſpheres in the common *Magdeburg* Experiment alſo. For Nature would not do it by *Preſſure* here, and by *Suction* there: This would be ſuch an unequal acting, that the Wiſdom and Simplicity of Nature is by no means to be blemiſh'd with ſuch an Imputation.

And therefore I think I may venture to affirm, That all the Objections that have been made againſt this Doctrine, have (at beſt) been the Reſult of nothing elſe, but fallacious and miſtaken Reaſonings.

However, to prevent all Scruples in Them that may be apt to retain any Doubt of the Air's Preſſure, I ſhall add one or two convincing Circumſtances more.

Firſt, Having caus'd the ſame two Hemiſpheres to be exhauſted of their Air, and none but the common open Air being about their outward Surfaces; I found that the ſame weight was requir'd to ſeparate them then, as was requir'd for their ſeparation when they were full of common Air and had an Atmoſphere of Air condens'd on their outward Superficies.

Secondly, Having exhauſted the included Air, I injected an Atmoſphere (the ſame quantity as in the former Experiment) on the external Surfaces of the

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Hemiſpheres; and then I found that 280 pound (which was double the weight before requir'd) was not ſufficient to ſeparate them: I was unwilling to add more weight (tho' I knew a ſmall addition muſt have done it) for fear of breaking ſome of the weaker parts of the Machine, which might have been in danger by the fall of ſuch a weight; the Experiment being full and concluſive without it.

Now, what can be a plainer Demonſtration (even to Senſe,) of the Preſſure of the Air, than this? Here was no greater quantity of Air injected on the out-ſide of the Hemiſpheres, than when the Common Air was left within them; and yet they were preſs'd together by a force above twice as great as that in the former caſe: (for the force that compreſſes, is always proportionable to the weight requiſite to make the ſeparation.) Therefore that ſame *Air*, contiguous to their outward Surfaces, *preſs'd* againſt thoſe outward Surfaces; and that with a force above twice as great as it did in the former caſe. This Property of the Air therefore, is certain beyond all diſpute.

L SECT.

SECT. IV.

An Experiment concerning the Proportion of the weight of Air, to the weight of an equal bulk of Water, without knowing the abſolute Quantity of either.

I Took a Bottle which held more than 3 Gallons, (but how much more, we have no occaſion at preſent to take notice of,) and of a form ſomething oval: which Figure I made choice of, for the advantage of its more eaſie Libration in Water. Into this Bottle I put as much Lead as would ſerve to ſink it below the Surface of the Water. And the reaſon why I choſe rather to have the weight of Lead inclos'd within the Bottle, than fix'd any where on the out-ſide, was, to prevent the Inconveniences which in the latter caſe muſt needs have aroſe from *Bubbles of Air*: For theſe *Bubbles* would have inevitably adher'd to, and lurk'd in great plenty about the body of the Weight, had it been plac'd on the out-ſide: Which muſt have cauſed ſome Errors in the Computations of an Experiment that requir'd ſo much exactneſs and nicety.

Theſe things thus provided; the Bottle (containing Common Air ſo clos'd up,) was by a Wire ſuſpended in the Water, at one end of a very good *Balance*; and *was counterpoiz'd in the Water by a weight of* *of* 358½ *grains* in the oppofite Scale. Then being taken out of the Water and fcrew'd to the *Pump*, in 5 minutes time it was pretty well exhaufted; the Mercury in the *Gage* ftanding at near 29½ inches. After which (having turn'd a Cock that fcrew'd both to the Bottle and the Pump, and fo prevented the Air's return into it again,) it was taken off from the Pump, and fufpended as before, at one end of the Balance in the Water. And now the weight of it was but 175½ grains; which therefore fubtracted from 358½ grains (*the weight of the Bottle with the inclos'd Air, before it had been applied to the Air Pump,*) gave for the difference 183 grains; which difference muft confequently be the weight of the quantity of Air, drawn from the Bottle by the Pump.

Having thus determin'd the weight of the exhaufted Air, a Cock was open'd under Water, upon which the Water was at firft impell'd with a confiderable violence into the Bottle, (tho' this force abated gradually afterwards;) and continued to rufh in, till fuch a quantity was enter'd, as was equal to the bulk of the Air withdrawn. And then the Bottle being examin'd by the Balance again, was found to weigh 162132 grains: From which fubtracting 175½ grains, (*the weight of the Bottle with the fmall remainder of included Air, after it was taken from the Air-Pump,*) there remains 161956½ grains, for *the weight of a maffe of Water equal in bulk to the quantity of Air exhaufted. So that the proportion of the weights of two equal bulks of Air and Water*, is as 183 to 161956½; which is as 1 to 885$\frac{1}{122}$; or, in round Numbers, as 1 to 885.

And there are two things particularly obſervable in this Experiment.

Firſt, That in making it after this manner, *one need not be very ſollicitous about a nice and accurate Exhauſtion of the Receiver.* The Succeſs of the Experiment does not at all depend upon it; for to what degree ſoever the Exhauſtion be made, it muſt ſtill anſwer in proportion to the quantity taken out. Neither can any more Water poſſibly enter into the Receiver, than what will juſt ſupply the place and fill up the room, deſerted by the exhauſted Air.

But, *Secondly*, *The Seaſon of the Year is to be conſider'd in making of this Experiment.* I made it in the warm Month of *May*; the Mercury in the Barometer ſtanding at the ſame time at $29\frac{7}{10}$ inches.

From whence 'tis reaſonable to conclude that a ſenſible Difference would ariſe, were it to be tried in the Months of *December* or *January*, when the State and Conſtitution of the Air, is uſually different from what 'tis in the forementíon'd Month.

SECT.

SECT. V.

An Experiment ſhewing that the Aſcent of Liquids in ſmall Tubes open at both ends, is the ſame in Vacuo *as in the open Air.*

I Took three *ſmall Tubes* of different diameters, and fix'd them in a piece of Cork, in an exact *perpendicular* poſition: Alſo their lower Orifices were ſet as nicely in one and the ſame *Horizontal Plane* as I could. This Cork I faſten'd to a *Wire*, which paſs'd thro' ſome *Collars* of *Leather*, included in a *Box* on the upper Plate of the Receiver; by which means I could at pleaſure elevate or depreſs the ſmall Tubes, without any danger of the Air's getting in. Then ſome Water, which was tinged with a deep Colour, being ſet on the lower Plate; the ſmall Tubes (which had never been wetted) were drawn to the upper part of the Receiver by the help of the foremention'd Wire. And the Air being exhauſted, the ſaid Tubes were made to deſcend (by the ſame Wire which drew them up,) till their lower Orifices were *immers'd juſt below the ſurface* of the tinged Liquid. This was no ſooner done, but the Liquor *mounted up in each of them to a conſiderable height above its ſurface in the Baſon*; but *higher in the ſmaller Tubes than in the larger ones.* And, what was farther worth notice, the Liquid *See Fig.3 Plate* II.

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ſo elevated was alſo retain'd in theſe ſmall Tubes, tho' their lower Orifices were lifted out of the Water.

Upon the re-admiſſion of the Air, the Fluid ſtood at the very ſame elevation, in each of the Tubes, as it did before. What height ſoever it mounted to in *Vacuo*; it preſerv'd the ſame, without the leaſt ſenſible alteration, when the Air was permitted to have free acceſs to it again.

So that the Matter of Fact obſervable in this Experiment, is contain'd under theſe two Heads.

Firſt, That *the Fluid roſe in the ſmall Tubes in the Exhauſted Receiver.*

Secondly, That *the admiſſion of the Air made no change in the Height.*

From both which put together, it follows directly (as I take it) that *the Air is not the Cauſe of the Riſe of Liquids in ſmall Tubes.* For if it be, how then does the Liquid come to riſe in the exhauſted Receiver?

If it be ſaid, that the *Vacuum* is not a perfect one, and there is ſome portion of Air left in the Receiver; I enquire then, if that ſmall portion of very-much-weaken'd Air left in the Receiver was ſufficient to raiſe the Fluid to ſuch a height, would not a new force of Air let in, have made an alteration, and carried it yet to a greater height? If the Liquid riſes by means of that Air left in the Receiver, 'tis certainly by vertue of its Preſſure on the Surface of the Stagnant Fluid, into which the Orifice of the ſmall Tube is put: And therefore when that Preſſure is ſtrengthned by the force of a new quantity of Air admitted in, this more power-

ful

ful Caufe fhould produce a greater Effect; and the Fluid fhould rife higher: Which yet it does not, but keeps at the fame unvaried height. From whence, I think, I may without fcruple conclude, that the Air has nothing at all to do in this matter: For 'tis plain Fact, that the abfence of it does not hinder, nor its prefence help the Effect: And what neither helps, nor hinders, no Philofophy in the World will allow to be a Caufe.

Befides, if to the Matter of Fact afforded by this Experiment we add a Confideration or two more, it will render the Argument yet more fenfibly convincing. For Liquids will rife in fmall Tubes, *in the open Air* (as we fee every day:) Again, they will keep the fame height they have rifen to in *Pleno*, after the Air be drawn out and they be left in *Vacuo*. Now joyn thefe two Confiderations with the former, and I think it renders the Evidence as compleat as can be defired. For if Fluids will rife (in fmall Tubes) *in the open Air*, and alfo in the empty Receiver; And if they will keep their height they rofe to in *Pleno*, tho' you make a *Vacuum*; and keep their height they rofe to in *Vacuo*, tho' you make a *Plenum*; then 'tis manifeft, that this Phenomenon is abfolutely indifferent, with refpect either to the *Prefence* and *Action*, or to the *Abfence* and *Non-action* of the Air; and therefore that the Air it felf cannot be the Caufe of it.

I would farther add here an Obfervation or two that I have made, *concerning the Properties of thefe fmall Tubes*.

First,

First, If a ſmall Tube be bent into the form of a Syphon, then *obſerve how high the Liquid would of it ſelf riſe in the ſhorter Leg of ſuch a Syphon, if it were immers'd in Water*; for *the Orifice of that ſhorter Leg of ſuch a Tube, muſt always be at leaſt as far below the ſurface of the ſtagnant Fluid as that Height amounts to, before it will run out at the longer Leg.* Which is a pretty remarkable difference between theſe ſmall and the vulgar larger Syphons. For in Thoſe we are not limited to any certain and particular depth, at which the Orifice of the ſhorter Leg muſt be plac'd before the Water will run out at the longer: But in Syphons made of very minute Tubes, ſuch as Liquids will ſpontaneouſly elevate themſelves in, there is requir'd a certain depth at leaſt, for the immerſion of the Orifice of the ſhorter Leg; ſince all Depths leſs than the Height of the ſpontaneous Aſcent, will cauſe no effuſion of the Liquid out of the Orifice of the longer Leg.

And from hence 'tis an obvious *Corollary*, *That in ſmall Syphons, whoſe Orifices are of different diameters, thoſe need to be plunged to the leaſt depth* (for cauſing the Water to run out at the other Leg,) *whoſe Orifices are the largeſt.* For in Tubes of the largeſt Orifices, the Fluid aſcends of it ſelf to the leaſt height. Wherefore ſince in order to the running of the Liquor, the depth of the Immerſion muſt be (at leaſt) equal to the height of the ſpontaneous Aſcent; it apparently follows, that Syphons of a larger Orifice will run at a leſs depth of the ſhorter Leg's immerſion below the Surface of the ſtagnant Liquid, than thoſe of a narrower Orifice will do.

Secondly, By Trials made with Tubes of various ſizes and proportions, I found this to be a conſtant and perpetual Rule; *viz. That ſo much of the Liquor would always remain ſuſpended in them, when lifted up out of the ſtagnant Fluid; as would be elevated above the Surface of it, while they were immers'd in it.* From whence it follows, that ſome Cauſe (whatſoever it be) which concurrs to the elevating of the Fluid into the Tube, while it is immers'd; does contribute as powerfully to keep it at the ſame height, after the Tube is taken out of the ſtagnant Liquid.

An Account of an Experiment, concerning the quantity of Air produced from a certain quantity of Gun-powder fired in Common Air.

I Took a fine Glaſs Tube, about 36 inches long, the diameter of whoſe *Bore* was near ¼ of an inch. The upper Orifice had a *Ferrel*, ſoder'd to a *Screw* cemented on it, to which was ſcrew'd a *Cock*: The lower Orifice was quite naked and open, it being no way needful to have any guard ſet on that part. Near the upper part of this Tube, in the in-ſide, was fix'd a piece of *Cork*, notch'd on its edges, to give the greater ſcope and liberty to the Exploſion. The Cork had a ſmall *Cavity* in the middle of it, the better to hold the Gun-powder, which was let down upon it thro' a ſmall Glaſs *Funnel*, before the Cock was ſcrew'd on. And in this manner, was the lower Orifice of the Tube plunged under the ſurface of the Water contain'd in a Veſſel. The Cock being then ſcrew'd on, and open; 'twas an eaſie matter, by ſucking at it with ones Mouth, to remove the Preſſure of the *inward* Air: by which means the Preſſure of the *outward* Air would raiſe the Water in it to any determinate height. And the Tube being accurately graduated by a *File* on its out-ſide, one might meaſure the quantity of the Aſcent with all the eaſe and exactneſs imaginable. When the Water had got up to the intended mark, the Cock was turn'd, which kept it ſuſpended there. And a *Burning Glaſs* being applied, the Rays, were drawn to a *Focus* upon the Gun-powder; which fired it very quickly, and forc'd the Water down with a great violence; but it roſe again ſuddenly afterwards; however, it reſted ſo far below the mark it ſtood at before the Exploſion, as was equal to the quantity of *ſeeming Air* produc'd thereby. The quantity of Gun-powder uſed in this Experiment, was exactly one grain. And I found that the quantity of ſpace the Water had deſerted juſt after the Exploſion, was ſuch as would contain nearly a cubical inch of Gun-powder, the weight of which was equal to 222 grains. So that 222 grains of the ſame Powder, ſeem (as ſoon as fired) to produce ſomething, which poſſeſſes the ſpace of ſo many cubical inches of Air. *Now whether the ſpace deſerted by the Water is poſſeſs'd by a Body of the ſame weight and denſity, or which has the ſame qualities with Common Air*, I dare not determine; ſince an Experiment I lately made (*to try how much the heat produc'd by the Exploſion of the Gun-powder, might contribute to the largeneſs of the*

ſpace

ſpace deſerted by the Water) ſeems to conclude otherwiſe. That matter was thus:

The whole ſpace deſerted by the Water, was divided (length-wayes) into 20 equal parts. Now an hour after the firing of the Gun-powder, the Water had aſcended about $\frac{2}{20}$ of the whole ſpace, which was 2 inches accurately ſpeaking, or ſuppoſe it to be $2\frac{1}{2}$ inches. At the diſtance of two Hours after the firing, it had got up to near $\frac{4}{20}$ of the ſame. And then I judg'd it might have been of an equal Temper with the external Air, (and conſequently not have given way to the Liquid to have riſen any higher.) But continuing the Experiment ſtill farther, I found (to my great ſurprize) that two hours after the laſt Obſervation, the Water had mounted to about $\frac{5}{20}$ of the ſpace. Next Morning (which was about 18 hours diſtance) it had reach'd near $\frac{10}{20}$, or $\frac{1}{2}$ the firſt deſerted ſpace. And continuing thus to riſe, I found that at the end of 12 days, the Water had aſcended to ſomething more than $\frac{17}{20}$. And at 18 days, it had reach'd 19 of the 20 parts at firſt deſerted by it. And *at this ſtation it reſted, continuing there for* 8 *days, without alteration.*

I would *obſerve* one or two things here, before I make any *Deductions* from this Experiment.

Firſt, That *I all-along conſider'd the Temperature of the Air, and found that it contributed nothing atall to this odd Phenomenon.*

Secondly, That tho' the Account here given *may ſeem to thwart ſome Accounts formerly given about the firing of Gun-powder in* Vacuo, yet, conſidering the *vast difference of the Mediums* in which the Experiments were made, they may be reconcil'd to one another. For when the Gun-powder was fired in ſo thin a Medium as *a near Approach to a Vacuum*, 'tis plain that the Air remaining in the *Receiver* could ſuffer no more by the Exploſion, than in proportion to its quantity: which quantity being ſo very inconſiderable, the Effects could but anſwer accordingly. Beſides, were thoſe Experiments to be repeated again, ſome Occurrences, which at the firſt Trials might paſs unheeded, would perhaps be taken notice of, which might render all more eaſie and agreeable than now it ſeems to be.

Corol. 1. *'Tis plain, that the matter produc'd by the Exploſion, (whatever it were,) was of a ſpringy contractile Nature, and but very little in quantity, in proportion to the ſpace which it at firſt forc'd the Water out of.* For it reduced it ſelf at laſt into the 20th part of the ſpace deſerted by the Water; that is, into the 20th part of a ſpace equal in content to a cubical inch, or 222 grains of Gun-powder. So that it was in bulk equal to no more than about 11 grains, which is nearly the 23th part of the aforeſaid number.

Corol. 2. *The Contraction or Reſtitution of this ſpringy matter was not equable and uniform, nor indeed (as far as I could find,) according to any regular Law; but very diſproportional with reſpect to the Times.* For the degrees of the Contraction would be as *the Spaces (reciprocally)* into which the Matter was reduc'd by that Contraction; and the Spaces into which the Matter was reduc'd, were exactly diſcover'd by the aſcent of the Water. Now at one hour (after the firing of the Powder) the Water had aſcended 2 of the Diviſions;

visions; at 2 Hours, 4; at 4 Hours, 5; at 18 Hours, 10; at 270 Hours, 17; at 432 Hours, 19; where it stood without alteration for the space of 8 Days: So that the encrease of the Waters Ascent, and consequently the Restitution of this contractile Matter, was very far from being equable and regular. At first it answer'd in proportion to the Times; but afterwards varied enormously from that Law; as is apparent. For in the first 4 Hours, it rose 5 Divisions; and at the end of 18 Hours, it had risen but 10: So that in the last 14 Hours of the 18, it had gain'd no more space than it had the first 4 Hours; which was 5 Divisions. And so in the rest that follow, the difference was still greater.

NB. Whether the Matter, which was the Cause of this Phenomenon, were real Common Air or no, is what I will not venture to determine: 'Tis sufficient for my purpose to have propos'd the Matter of Fact, and to have prov'd that this Matter had such and such Properties. To me it seems highly probable that it should be an *Heterogeneous Compound of Common Air, of some Aerial parts residing in the Gunpowder, and of the Nitrous and Sulphureous Matter* which are Ingredients in the same. For all these must needs (I should think) be violently hurried and mix'd together, upon the Explosion; and consequently, must all joyntly compose one Heterogeneous Medium, which afterwards display'd it self by such Effects as I have now mentioned.

The *Elastick* or *Self-restoring Property* of this Matter, seems cheifly to be owing to the *Air* contain'd therein. And that the *Springs of Air may be so disordered by a violent impulse, as to require Time to recover their Natural state again*, will be very clearly made out by the following Experiment.

An Experiment about disturbing the Spring of the Air.

I Took my *Condensing Engine*, and put about ½ a pint of Water into the bottom part of its *Brass Receiver.* Then the upper part being strongly screw'd on, I threw into it with the *Syringe* about 3 or 4 Atmospheres of Air, as near as I could guess: and in this state I suffer'd it to remain something more than an Hour. Then letting out as much of the Air (by taking off the *Syringe*) as would readily go away, I presently screw'd on in its room a *Box of Leather Collars*, thro' which there pass'd a small *Glass Tube*, open at both ends, whose lower Orifice was plunged under the surface of the included Water. After this, in a very little time, I found the Water had ascended near a Foot in the Tube; and it continued rising, till it had reach'd near 16 inches.

Upon a *Repetition of this Experiment*, I let the Air remain in that state of Compression for about 18 Hours. And then (proceeding in all respects as before) I found the included Water ascend gradually in the Tube; and observ'd That motion for the space of 6 Hours: At which time the little Tube was accidentally broken, and so farther Observations for that time prevented. But however, from hence we may infer,

Corol.

Corol. 1. That *the Springs of Air may be ſo diſturb'd by violent Impulſes, or ſtrong Compreſſions, as to require conſiderable time to recover their Natural Tone and Temper again.*

Corol. 2. *And the Times which the Springs of Air will require for their compleat reſtitution, will be greater or leſs, according as the Forces by which they are thus impell'd or compreſs'd, are greater or leſs; or according as the Times during which they continue in that violent ſtate, are greater or leſs.* That it ſhould be ſo, is perfectly conſonant to the Nature and Properties of the Air; and that it is ſo, the Circumſtances of the Experiment evince. For when the Air had lain compreſs'd for about 18 Hours, the Aſcent of the Water was more ſlow and deliberate; it creeping up gradually for the ſpace of 6 Hours together. But when it had ſuffer'd the Compreſſion but for the ſpace of an Hour; the Water advanc'd upwards in the Tube ſo faſt, that in a very ſmall time it had mounted a whole Foot. And there's no reaſon at all to doubt, but that longer Time, and more valid Compreſſions, would produce ſtill greater and more conſiderable Effects, as to the times of the Springs recovering themſelves.

And hence therefore; *were this Proportion once ſettled and eſtabliſh'd by a ſufficient number of Experiments; from the Air's foregoing Compreſſion, one might limit and foretell the Motions of the included Liquid; and,* vice verſâ, *from the Motion of the Liquid, one might infer the Air's foregoing Compreſſion.*

Corol. 3. *Wherever therefore (in any Bodies whatſoever) the included Springs of Air ſuffer any ſuch Com-*

preſſions

pressions as these are, or any answerable to these; and there be any contiguous Fluid Matter for these Springs, as they restore themselves, to press upon; these same Effects must follow: That is, the Fluids must be put in motion, and advance according as the others press them; and if they were in any sort of motion before, that motion must be alter'd, and either accelerated or retarded, according as the Course and Direction of the Fluid be either with or against that of the Aerial Springs, while they are in this action of Self-restitution. And perhaps several Phenomena, as well in Plants and Animal Bodies, as in other Systems of Matter in our Globe, may owe their true Rise to some such Cause as this. 'Tis certainly a *possible* Cause, from the Laws of *Staticks*; and perhaps inquisitive Persons may find out some Effects of it.

An Account of an Experiment, shewing the Cause of the Descent of the Mercury *in the Barometer, in a Storm.*

'TWAS observable in the late violent Storm of Wind, that the Mercury in the Barometer did not only subside very considerably, but also that, upon extraordinary Gusts, there were sensible and manifest Vibrations of it in the Tube.

Now, to account both for the *Depressions* and *Vibrations* of the Mercury in these and such-like Cases, I contriv'd the following

EXPE-

Experiment.

Having provided a *Receiver*, A, which held about 16 Quarts, I compreſs'd in it about three or four times its natural content of Air, by the help of the *Syringe* B: Which Inſtrument is for that purpoſe ſcrew'd on at c c. This done, and the *Stopcock* D, fitted to this Receiver, being turn'd; the Syringe was taken off, and a Braſs *Pipe* about half an inch diameter, repreſented by E E, ſcrew'd on in its room. This Pipe is inſerted into a well-fitted *Braſs Socket*, which is fix'd in a ſquare *piece of Wood* F F, and that directly againſt a *Tube*, G G, which enters the ſame piece of Wood, and is plac'd parallel to the Horizon. Now, out of the ſame piece of Wood, there riſes a naked *Barometer*, H H, the *Ciſtern* of which lies open to the paſſage leading from the *foreſaid Pipe* E, to the *Horizontal Tube* G. Beſides this, out of that ſame *piece* F, there proceeds another Horizontal *Pipe* I I, which runs to another ſquare *piece of Wood* K K, plac'd at the diſtance of 3 foot from the former. And this ſecond *piece* K, has likewiſe *a Barometer* ariſing out of it, L L; the *Ciſtern* of which is alſo open to the Horizontal Tube I, and by that means maintains a Communication with the open Ciſtern of the other Barometer. All the parts of the Machine being thus diſpos'd, the Stop-cock was turn'd; and the condens'd Air ruſh'd out of the Receiver with a great force thro' the Pipe E, which diſcharg'd it into the Horizontal Tube G. The reſult of which was, that *this rapid current of Air ſo leſſen'd the Preſſure of the Atmoſphere upon the ſtagnant Mercury in the Ciſterns of the reſpective Barometers, that the Mercury was made thereby to deſcend at leaſt 2 inches.*

See Fig. 1 Plate V.

Nay, that Barometer L L, which was 3 foot diſtant from the Aerial Stream, *was equally affected as the nearer one* H H; the Mercury ſubſiding nearly to a juſt Parallelifm in both. And 'tis farther obſervable, that *as the Force of the current of Air diminiſh'd, ſo the Weight of the Atmoſphere recover'd its Strength again, and forc'd the Mercury in the Barometers to a gradual aſcent.* Hence,

Corol. 1. *We have a clear and natural account of the Deſcent and Vibrations of the Mercury, in violent Storms and Hurricanes.* For the mighty Force of thoſe guſts of Wind, will weaken the Preſſure of the incumbent Atmoſpherick Columns; from whence a Deſcent of the Mercury muſt neceſſarily follow. And the interrupted uneven Action of thoſe *Blaſts*, or the quick and ſudden Returns of them, are capable of producing and continuing the Vibratory Motions (*i. e.* the quick Aſcents and Deſcents) of the ſame.

Corol. 2. *Not only the different* Forces, *but alſo the different* Directions *of Winds, are capable of producing a Difference in the ſubſiding of the Mercury.* That Winds of different *Strengths* ſhould produce proportional Effects, in breaking the Preſſure of the Atmoſpheric Columns; is as reaſonable, as that a greater Power ſhould ſuſtain a *greater* Pondus, or take off more of the the Preſſure of the *ſame* Pondus, than a leſs can do. And 'tis no leſs evident, that the different *Directions* of the Aerial Currents, muſt be attended with different Effects too; thoſe whoſe Courſe is from the *lower* towards the *higher* Regions of the Atmoſphere, having both *ſhorter* and *finer* Columns of Air to encounter the Force of, than thoſe whoſe Courſe is from the *higher* to the *lower*, where the Columns have both more *length* and *denſity* too. I ſpeak this

with

with reſpect to the real difference of the Effect in it ſelf, and not as to the outward ſenſibleneſs of it to us; for changes may happen, when we can't or don't obſerve them. But all other Circumſtances being alike, this Circumſtance of different *Direction* muſt, I doubt not, produce a real Variety. And if all other Circumſtances are not alike, then the Proportions of the Effects will be had from the Compoſition of the Proportions (either *direct*, *reciprocal*, or *both together*) of the Cauſes which make thoſe different Circumſtances.

Corol. 3. *Strong Winds may affect the Animal Oeconomy, upon this very account, of their altering the Preſſure of the Atmoſphere.*

Let us imagine a number of Pipes or Canals, of an *elaſtick flexible* nature, replete with ſome Fluid; the Preſſure of the incumbent Atmoſphere is in this caſe to be conſider'd as a *Pondus*, acting againſt the Force of theſe Elaſtick Canals, with that of their contain'd Fluid. And according to *Mechanick* Laws, theſe diſtractile Tubes will be ſo far compreſs'd by that incumbent Weight, till a juſt Equlibrium is produc'd between the two Antagoniſt Forces; and then they will preſerve themſelves on both ſides in that ſtate, till ſome farther alteration ſhall happen, to leſſen the *Momentum* either of one or the other. If therefore the Preſſure of the incumbent Columns of Air be in any meaſure broken or taken off; the Canals will reſtore themſelves ſo far forth by their Elaſticity, till the *Momentum* of their Renitency becomes equal to that of the diminiſh'd Preſſure. From whence 'tis manifeſt, (ſuppoſing the contain'd Fluid to be in Motion) that the Rate of the Progreſs of that Fluid, muſt needs undergo an alteration, in proportion to that of the Change made in

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the external Preſſure. For the ſtronger Preſſure will ſtraiten the Canals, and conſequently encreaſe the Velocity of the Fluid: as, on the other hand, the more feeble Preſſure will give way to the Canals endeavouring to enlarge themſelves; and by that means will contribute to the more ſlow and deliberate motion of the Fluid. The application of all which to the Bodies of Animals, is very obvious and eaſie: For they are nothing more than ſo many complications of branching Canals, and tender flexible Membranes, eaſily yielding to an external Preſſure or Pulſion, and capable of reſtoring themſelves by their innate Spring. The great weight of the Atmoſphere is always preſſing down upon theſe Machines; and 'tis the ſpring and renitency of their parts, which is the counter-balance to it, and preſerves them from receiving injuries by it. The Veſſels conſequently, which ſerve for the Diſtribution of the Animal Fluids, being differently ſtraiten'd and compreſs'd by the various weights of the incumbent Atmoſphere; the Liquids are affected with new and different degrees of velocity. And therefore when any extraordinary Changes happen in the Weight and Preſſure of the Atmoſphere, there muſt be (*cæteris paribus*) as conſiderable Changes in the motions of the Fluids. But violent guſts of Wind, Hurricanes, and the like, will neceſſarily produce very great differences in the weight of the incumbent Atmoſphere: And therefore, I ſay, very conſiderable Alterations may be made in the Motions of the Liquids in Animal Bodies, by ſuch Cauſes as theſe.

From whence it follows, that whatever Changes are poſſible to be produc'd in Animal Bodies, by the meer alteration of the velocity of the Liquids; are (in ſome meaſure at leaſt) producible by very

ſtrong

ſtrong and violent Winds: And theſe changes in the Animal Oeconomy, (*viz.* that depend upon the alter'd Velocities of the Fluids) are not a few.

Corol. 4. The weight of the Atmoſphere, being diminiſh'd in one place, it is alſo as much diminiſh'd at the ſame time in another place, which holds a communication with the former.

This is plainly viſible in the Experiment it ſelf. For the Force of the Air in the Ciſtern in the Incloſure F, being broken by the violent eruption out of the Pipe E; that alſo of the Ciſtern in the incloſure K, which communicated with the former, was ſo far diminſh'd too, that the Mercury (whoſe height depended upon it) ſubſided in that Barometer juſt (or near) as much as in the other. And the like Effects muſt be produc'd otherwhere, when the Circumſtances anſwer to theſe here.

An Account of ſome Experiments made on the Phoſphorus *in* Vacuo.

Experiment I.

HAving provided a dark Room, I drew ſome Lines with the *Phoſphorus* on a piece of *Blue* Paper: This immediately became luminous in the *open* Air, and apear'd with a Wave-like Undulating Motion. But being plac'd in a *Receiver*, after ſome few Exſuctions, the undulation ceas'd; but the Light ſeem'd conſiderably augmented. The Receiver being farther exhauſted, it grew ſtill brighter, and continued with that encreaſe of Luſtre, till an admiſſion of Air, which did ſenſibly diminiſh it. This decay of the Light was alſo gradual, anſwering to the gradual admiſſion of the Air: Tho' upon the Repetition of the Experiment, ſome Perſons preſent believ'd the Light not altogether ſo brisk and vivid as at firſt.

Experiment II.

I Took two or three ſmall pieces of *Phoſphorus*; which being put into a Glaſs Diſh, I mix'd with it a ſmall quantity of *Oil of Vitriol*, *Oil of Tartar per Deliquium*, and *Oil of Cloves*. This mixture fir'd the Phoſphorus in the open Air: but it was extinguiſh'd again by the addition of a little common Water. This Preparation being included in a Receiver; very little Light appear'd. But the Air being exhauſted, it became very apparent, with a brisk and vigorous emiſſion of Steams. The Ingredients of this Compoſition in the Diſh, ſeem'd at the ſame time to reſemble a boyling Flame, and exhibited a copious Light; ſo that ſeveral Objects that were near, became very diſtinguiſhable. And this lucid appearance continued till the Air was admitted: but upon that, all became opake and dark. Neither would ſhaking the Engine (by which means the mixture it ſelf underwent an agitation) produce any ſenſible recovery of the Light.

EXPE-

Experiment III.

HAving put a ſmall quantity of the foremention'd Compoſition into a Bottle with a narrow Neck, I included in it in a Receiver; and it yielded then but very little Light. But upon the exhauſtion of the Air, it began to be luminous; and the Light improv'd in proportion to the encreaſing Rarefaction of the Air, iſſuing out of the Bottle in a Pyramidical form.

At laſt (tho' the Receiver was well exhauſted) the Steams, then emitted, did fairly aſcend in that very rare and thin Medium, and reach'd the upper parts of the Receiver, (which was not a tall one,) but deſcended down again by the ſides of it. Upon the Re-admiſſion of the Air, the Light perfectly vaniſh'd; and it would have been in vain (as I have often try'd) to have expected the recovery of it in the open Air.

Theſe three Experiments do all exactly agree, in confirming this Concluſion, *viz. That the Phoſphorus-Light is improv'd, by the Rarefaction of the Air.* Common Air is therefore ſome way or other an Impediment to the Action of thoſe Steams on which the *Light-giving* Quality depends. It remains therefore to be enquir'd, By vertue of what Property of the Air it is, that the Action of the Luminous Steams is thus impeded?

And I think it highly reaſonable to conclude, that *the Preſſure of the Atmoſphere is that Impediment upon the Luminous Matter in this caſe.* For the Air, as a *Pondus* incumbent on the Body which contains the luminous Matter, urging the Surface on all ſides, and rendring it conſequently more cloſe and compact; the lucid Steams, whoſe efforts and endeavours to expand themſelves cannot balance this over-ruling Preſſure, are by that means kept in, and cannot be diſcharg'd.

Yet I do not ſee that we can argue here from the Denſity and Gravity of the Air, conſider'd as a *Medium*. For if the luminous Matter were ſpecifically *heavier* then common Air, it would much more be heavier than rarefied Air in an Approach to a *Vacuum*; and conſequently the Steams could not riſe, nor the Light appear, (much leſs improve,) in the exhauſted Receiver; as we ſee it does.

Again. If any thing depended on the Luminous Matter's being ſpecifically *lighter* than Common Air, yet in the ſeveral degrees of Rarefaction approaching toward a Vacuity, there would be Mediums produc'd, approaching ſtill nearer and nearer to the Specifick Gravity of the luminous Matter. And conſequently, as the Receiver is more and more exhauſted, ſo the Fumes ſhould be diſcharg'd in leſs plenty, and aſcend with leſs velocity. Whereas on the contrary, they riſe more copiouſly; and (the Light being more bright and vivid too) 'tis plain that they expand themſelves, not with leſs, but greater force. And therefore I think the Gravity of the Air, as a *Medium*, has very little (if any thing at all) to do in this affair. But the Preſſure or Gravity of the Air, as a *Pondus*, I believe, will account for it; and, as far as I can ſee, is the only Property that will do ſo.

I would remark here farther, particularly with regard to the third Experiment, that the *Phoſphorus* Steams were apparently ſpecifically heavier than the

the Medium produc'd in the Receiver by the laſt exſuction. For they did *deſcend* in that Medium. And from hence I may ſecurely infer, that they did not aſcend in that Medium by *Hydroſtatical Laws*, but by the meer Impetus of their own Vibratory Expanſive Motion, or the Force with which they were emitted from the Body which contain'd them, upon the removal of the Preſſure which was before an Impediment to their diſcharge. For, that Impetus being ſpent, or overpower'd by their Gravity, they neceſſarily deſcended again by vertue of that *Law*, which obtains in all Portions of Matter of all ſorts whatever.

An Account of ſome Experiments made about the Propagation of Sounds *in* Condens'd *and in* Rarefied *Air.*

Experiment I.

Concerning the Propagation of Sound *in* Condens'd Air.

A Bell being included in a *Braſs Receiver*, was plac'd at one end of a Room about 50 yards in length: At the other end of which, ſome Perſons ſtood to obſerve the Sound. Before any Air at all was injected, the Bell (by shaking the Receiver) *might be heard at that distance, tho' not without diligent attending to it.* When one Atmosphere was injected (*if I may take the liberty to use that expression,*) the Bell being shaken as before, the Sound was observ'd to be very *sensibly augmented.* When two Atmospheres were injected, there was manifestly *a much more considerable improvement of the Sound.* But upon the intrusion of the 3d, 4th, and 5th Atmospheres, the Sound *seem'd not to be augmented in proportion to what it was at the first and second.* However, it was observ'd, that at this 5th and last injection, the Sound was very near as loud and sensible at the 50 yards distance, as it was when the Bell was struck in the open Air, without being inclosed in the Receiver at all.

Now the Reasons of the Sound's not proportionably encreasing in so much greater Condensations, I believe, may be these.

First, *The Deficiencies of the injected quantities of Air.* For the *Valve*, which should have hinder'd the return of the injected Air, might not perform its Office so exactly, or hold so tight as it should have done; and by that means some portions of Air might escape, and consequently the quantities injected not be so great as was suppos'd: from whence it would be no great wonder, that there should be a failure in the proportion of the Encrease and Propagation of the Sound.

Secondly, Tho' 25 Compreſſions of the *Syphon* are equal to the Natural Content of the *Receiver*; yet when the Air becomes pretty ſtrongly condens'd (as 'tis by the intruſion of 4 or 5 Atmoſpheres,) the remaining Air at every ſtroke, which will lie between the bottom of the *Embolus* and the *Valve*, tho' it be but little, yet is of the ſame denſity, at that time, as the Air in the *Receiver*; which therefore, upon drawing up the *Embolus*, will extend it ſelf to ſuch a ſpace of the *Cylinder*, as it can fill up by expanding it ſelf into the ſtate of common Air; and is ſo much as this comes to, of what ſhould be injected at every ſtroke: 25 of which ſtrokes, as I ſaid before, are equal to the natural Content of the *Receiver*. And hence the Deficiencies of the real quantities, which ſhould be injected by a certain number of ſtrokes, may be very conſiderable; and to compute 'em, would be a buſineſs of as much difficulty.

EXPERIMENT II.

THE ſame Trial was made abroad in the open Fields, and with the ſame ſucceſs as the former. Upon ſhaking the Bell before any Air was injected, the Sound was but juſt audible at 30 yards diſtance. When one Atmoſphere was injected, it was heard as diſtinctly at 60 yards diſtance, as before at 30. Upon a ſecond injection, the Bell might be heard at 90 yards diſtance. But after that, tho' near 100 ſtrokes of the *Forcer* were repeated, yet it could hardly be heard 20 yards farther; which I attribute in great meaſure to the Reaſons before-mention'd.

The time when this Experiment was made, was early, about five in the Morning, in the Month of *June*. The weather very *miſty*, and little or no Wind ſtirring. And the ſilence requiſite for the nice making ſuch an Experiment, was by degrees interrupted by the Sounds of the five a-Clock Bells, and other noiſes from the City: all which in ſome meaſure contributed to the unſucceſsfulneſs of the latter part of the Experiment. But this I hope ſome time or other to proſecute farther; not diſpairing in the mean time, of contriving ſuch a *Gage*, as will ſhew the certain Quantities injected, without any danger or hazard in the Attempt.

Experiment III.

Concerning the Propagation of Sound in Rarefied Air.

HAving included a *Bell* in a *Receiver*, which was ſhaken to make it ſtrike, it was very obſervable that the interpoſition of the Glaſs betwixt the *Ear* and the *Bell*, was a great Impediment to the Propagation of the Sound, tho' it might be heard at a good diſtance from it. But the Air being gradually exhauſted, and ſeveral ſtops made, to ſhake the Bell at the ſeveral different degrees of Rarefaction; I found that the Sound was remarkably diminiſh'd at each of thoſe ſtops. At laſt, when the Receiver was very well exhauſted, the Sound was ſo little, that the beſt Ears could but juſt diſtinguiſh it: it being like a ſmall ſhrill Note, heard at a mighty diſtance. As the Air was gradually admitted into the Receiver again, ſo the Sound gradually encreas'd; this *augmentation* in the more *denſe* Medium, anſwering by proportional degrees to the *Diminution* in the more *Rarefied* one. And when the Receiver was again replete with Air, the Sound ſeem'd ſomething more clear and diſtinguiſhable, than it did when the Bell was firſt included, before any of the Air had been drawn out.

The Obſervation therefore to be deduc'd from theſe Experiments, is this, *viz. That Sounds are augmented in Condens'd, and diminiſh'd in Rarefied Air*: or, that That Undulating Motion in which Sound conſiſts,

is

is propagated with more facility and advantage in *Condens'd*, than in *Common*; and in *Common*, than in *Rarefied Air*.

And from hence we may infer,

Corol. 1. That *the Diſtances at which the equally-ſtrong Percuſſions of the ſame ſonorous Body ſhall be equally audible to the ſame Ear, in Condens'd, Common, and Rarefied Air, (or, which is the ſame thing, in Airs of different degrees of Denſity,) muſt be taken in ſome proportion to the Denſities of thoſe ſeveral Mediums, thro' which the Sound is thus propagated. And that therefore, were that proportion eſtabliſh'd by ſufficient Experiments; from the Denſities given, the Diſtances might be inferr'd; or from the Diſtances given, we might conclude the Denſities requiſite to make a Sound of a given Degree, to be equally audible at thoſe given Diſtances.* And therefore, were we to ſpeak of *the utmoſt Limits of Diſtance*, at which any given Sound is audible at all; 'tis plain that theſe Limits muſt be determin'd by the ſame Law of Proportion concerning the Denſities of the Mediums. Becauſe the *utmoſt Limits, at which any given Sound is audible at all*, in any given Mediums; are likewiſe *the Diſtances, at which that ſame Sound is equally audible in thoſe Mediums.* For when a Sound is but juſt audible in any Mediums, 'tis then *equally* audible in thoſe Mediums.

Corol. 2. *The Diſtances at which the different or unequally-ſtrong Percuſſions of the ſame ſonorus Body ſhall be equally audible to the ſame Ear, in Mediums of different Denſities, muſt be taken, in ſome proportion, compounded of the ſtrengths of the Percuſſions, and the Denſities of the Mediums.* And univerſally, to have

Sounds

Sounds (*cæteris paribus*) audible or diſtinguiſhable in any given ratio; will require ſome compoſition of the Proportions of *Diſtances*, *Denſities*, and *Forces of Percuſſion*.

Corol. 3. *Speaking ſtrictly, Sounds are not at all times equally audible to us here upon the Surface of the Earth.* I mean, the like Percuſſions of the ſame ſounding Bodies, are not at all times to be heard with the *ſame facility*, at the *ſame diſtances*. The Reaſon of which is ſufficiently manifeſt, ſince the *ſtate of the Atmoſphere here about us, undergoes ſuch frequent Viciſſitudes*, (and thoſe ſometimes very conſiderable ones too,) as to *Rarefaction* and *Condenſation*.

Corol. 4. *Sounds diminiſh or become leſs audible, as we aſcend upwards from the Surface of the Earth: And therefore in the upper Regions of the Atmoſphere, and eſpecially in thoſe where the Planets revolve, ſonorous Bodies muſt be at a diſtance almoſt infinitely near, (that is, in contact with the Organ it ſelf;) or the Force with which they are ſtruck, almoſt infinitely great; for Sounds to be equally audible, with what they are here upon the Surface of the Earth.* The Reaſon of which is plain, from the prodigious Rarefaction of the Medium in thoſe Regions. A Globe of ſuch Air as we have here at the Surface; if plac'd at the height of a Semi-diameter of the Earth, would expand it ſelf at ſuch a rate, as to fill all the Planetary Orbs as far as that of *Saturn*; nay, and a much greater ſpace than that. And That Medium, in which the Planets perform their Revolutions, is ſo fine and rare, as that its Reſiſtance is wholly imperceptible, though they have gone their Rounds in it for ſo many Ages. To what a degree of Rarefaction then does the

Medium

Medium ariſe in thoſe ſublime Regions? And what perception ſhould we have there of ſuch Sounds, as are here propagated to our Organs with a great deal of eaſe and force? For, the ſtrokes of the ſounding Body being equally forcible, the diſtance of the Organ muſt be leſs, in a rarer Medium, in ſome proportion to that rarity; that the Sound may be equally audible, as in a Denſer: And the Diſtances being the ſame, the Strength of the Percuſſions muſt be proportion'd to the Rarety of the Medium, in order to produce the ſame Effect: And conſequently, when the Rarefaction of the Medium is (as it is in thoſe Regions,) ſo vaſtly tranſcendent to what 'tis here on the Surface of the Earth; an Organ ſo made as ours is, muſt either approach almoſt infinitely nearer; or the Sonorius Body muſt be ſtruck with a force almoſt infinitely greater; that a Sound may impreſs the Organ there, equally with what it does here. The crackling of Thorns in a Fire, would ſhake our Ear with a vaſtly-more conſiderable force here, than the largeſt Cannon, or the moſt dreadful Claps of Thunder, would do there; were either of them diſcharg'd at a much leſs diſtance from the Hearer than what we are now ſpeaking of.

The *Muſick of the Spheres* therefore is an Entertainment, which we ought to deſpair of ever hearing: and That *Conſort*, be it as Celeſtial as it will, yet wants a fit Medium (if *that were all that is wanting*,) to convey it to us. The old Philoſophers were much in the right, in ſaying theſe fine Sounds were never to be heard; and as much in the wrong, in laying down that for the Cauſe, that the Noiſe was too ſtrong and overwhelming to the Organ, for us to have any perception of it.

Corol. 5.

Corol. 5. *The Diminution of Sounds in Aſcents or Elevations above the Surface of the Earth, will be in ſome proportion to the Deſcents of the Mercury in the Barometer at thoſe Elevations.*

For were the Diminution of Sounds exactly in a *ſimple* or *direct* proportion to the Rarefaction or Expanſion of the Medium, at any heights in the Atmoſphere; that Diminution would be exactly in a ſimple *reciprocal* proportion to the heights of the Mercury in the Barometer at thoſe Elevations: becauſe the Expanſions of the Air are found to be reciprocally as the heights of the Mercury. And therefore if the Diminution of Sounds be in ſome *complicate direct* proportion of the Expanſions of the Medium, it will be alſo in ſome *complicate reciprocal* proportion of the heights of the Mercury in the Barometer. And conſequently the Barometer might be made uſe of, to diſcover and determine the Diminution of Sounds in any Region of the Atmoſphere; provided it were well determin'd by Experiments beforehand, in what proportion Sounds diminiſh according to the Rarefaction.

An Account of an Experiment, concerning the Re-silition *or* Rebounding *of Bodies, in* Various Mediums.

I Provided a tall *Glaſs Receiver*, in the upper part of which I had a Contrivance for the lodgment of four *Marbles*, (ſuch as are generally ſold at the Shops,) and from whence I could let them drop down on a *Plane* at pleaſure. The diſtance from the *Plane* to the place where the Marbles were lodg'd, (and conſequently the ſpace of their deſcent,) was about 13½ inches. And as to the bulk of them, two of theſe Marbles weigh'd 59 grains; and the other two, 63 grains. The Plane on which they were to fall, was a round flat piece of ſolid Glaſs, about 1 inch thick, and 3½ inches over; the upper *Surface* of which was very well *ground* and *poliſh'd*. It was fix'd in a *Tin Frame*, contriv'd on purpoſe to keep its lower Surface from being contiguous to the *Plate* or *Leather* on which the Receiver was plac'd: the reaſon of which Contrivance was, to prevent an Inconvenience which would otherwiſe ariſe; as ſhall be ſhewn by and by.

All things being thus provided, the Marbles *were dropt in Common Air*; that is, in the Air included in the Receiver. After this, the Air was exhauſted, and *they were dropt in Vacuo*. And then an Atmoſphere of Air was injected, beſides the natural Content of the Receiver, and *they were let fall in that*

Con-

Condens'd Air. I ſay, *one Atmoſphere*; for I did not dare to venture more, leſt the breaking of the Receiver (which would be a hazardous thing) ſhould have been the Conſequence of it.

Now I found, that the Reſilition of the Marbles dropt in *Vacuo*, was ſomething more than that of thoſe in *Common Air*: And thoſe let fall in *Common Air*, had ſome advantage in their Rebound, above thoſe let fall in *Air Condens'd*. The Rebound *in Vacuo* was about 10½ inches, (which was more than ¼ of their Deſcent.) In *Condens'd Air*, it was about 10 inches. Accordingly, in *Common Air*, we muſt count the Reſilition to be a *Mean* between the other two: For 'tis extreamly difficult to determine to a Nicety in a Motion ſo ſudden, and of ſo ſhort a duration. But this however is certain, that there was a ſenſible difference between the Rebound of thoſe dropt *in Vacuo*, and thoſe in *Condens'd Air*. As for the *difference of the Weight of theſe Bodies*, I could not find that That made any diſcernible alteration in their Reflexion.

I would give one Caution here, which may ſerve to prevent thoſe, whoſe Curioſity may lead them to make theſe Experiments, from falling into an Errour, which I my ſelf very narrowly eſcap'd.

The Glaſs (as I ſaid before) *was fix'd in a Tin Frame, on purpoſe to keep the lower ſurface of it from being contiguous to the Plate or Leather, on which the Receiver was plac'd.* For when I firſt try'd theſe Experiments, I us'd a *Stone* Plane, laid careleſly *upon* the Leather which cover'd the Plate on which the Receiver ſtood: And accordingly, the *Air being exhauſted*, the Marbles would not rebound ſo high by an inch, as when the Experiment came to be made on the ſame Plane in *Common Air*. The reaſon of which was plainly this: That the Air being exhauſted, the Leather conſequently ſwell'd, and by that ſwelling rais'd the Plane which lay on it; and ſo, cauſing it to lie more ſoft and hollow than when 'twas only in Common Air, by this means the Reſilition became leſs in *Vacuo*, than in *Common Air*: and the Event of the Experiment prov'd quite contrary, both to what it ought to have been, to what was expected, and to what after came to paſs. For having fix'd the *Apparatus*, as before mention'd, all things ſucceeded then, both according to *expectation*, and to *Philoſophical Theory*.

Corol. 1. *In any exact Computations therefore of the Reſilitions of Bodies, Account muſt be taken of the State of the Ambient Medium*: For the Rebounds of the ſame Body will not be the ſame, in all the various conditions of that, as to Rarity and Denſity.

Corol. 2. *Here's a manifeſt Proof of the Air's Reſiſtence.*

I know there are many other Proofs beſides; but, I ſay, this Experiment alſo furniſhes one. For the Difference in the Rebounds is no otherwiſe poſſible to be accounted for, ſince the Experiment may be rely'd on as made to a ſufficient degree of Nicety.

Some farther Experiments concerning the Electricity *and* Light, *produc'd from various Bodies by* Attrition.

Experiment I.

Being a farther Improvement of one made before, to the same purpose.

IT has been shown in one of the foregoing Experiments, how Bodies included in a Glass, might be affected with a very sensible Motion, by the bare approach of one's Finger near the outside. I have here something to add to the Account of that *Surprizing Phenomenon*, which will render it more wonderful still: And the *Appearance* in this Trial was so much the more conspicuous, by how much the *Apparatus* made use of was better contriv'd and adapted than in the former.

I observ'd then,

That when the Motion and Attrition of the Glass had been continued about two or three minutes, and then ceas'd; the Threads seem'd to hang in great disorder, and without any degree of erection at all, for some small time. They continued in this posture (as near as I could count) for about three *Plate* VII. *Fig.* 6.

three or four *ſeconds*, and *then they were extended every way towards the Circumference of the Glaſs*; and that with ſuch a ſtrength, that the Motion of the Glaſs alone would not very much affect 'em. But the ſtrangeſt thing of all, was to ſee, that *a Motion might be impreſs'd upon them by the approach of one's Finger, Hand, or any other Body, at more than three inches diſtance from the outward ſurface of the Glaſs, tho' the Threads themſelves did not touch the inward one.*

I obſerv'd further,

That every time the Motion of the Wheel and the Attrition of the Glaſs were repeated, the Threads might be mov'd, by the approach of ones Finger on the outſide, at a ſtill greater diſtance.

Nay, I have found ſince, *that by blowing with ones Mouth only towards the Glaſs, at three or four foot diſtance, the Threads would have a very conſiderable motion given 'em.*

And when I have ſuddenly ſpread my Hands upon the upper and lower parts of the Globe, *there has been a violent Agitation of the Threads within, which has alſo laſted for ſome time.*

From theſe Obſervations we may gather,

Firſt, That the Cauſe of the Erection of the Threads (whatever it be,) tho' *certainly* excited by the Motion and Attrition of the Glaſs, *yet does not neceſſarily work its Effect* immediately, *upon that Motion and Attrition.*

For we ſee the Threads were quite looſe and motionleſs for three or four ſeconds of time; and then they were extended, like ſo many *Radii*, towards the Circumference of the Glaſs.

'Tis

'Tis worth enquiry here, Whether the ſpace of time between the *Ceſſation of the Motion*, and the *beginning of the Erection of the Threads*, will be the ſame in *all Seaſons*, and in *all Conditions of the Ambient Air*. As alſo, *Whether the longer or ſhorter continuance of the Motion and Attrition of the Glaſs, before they ceaſe; does contribute any thing to the lengthning or ſhortning this Time of the unactive ſtate of the Threads, before they begin to be erected.*

Secondly, 'Tis manifeſt, *there's a Communication between the Medium without, and That within the Glaſs.*

This follows from the Motions and Tremblings of the Threads, upon the approach of other Bodies poſited on the outſide.

Thirdly, Not only a *Communication*, but a *Continuity*, of the Matter which occaſions the Motion of the Threads. The Progreſs of it ſeems to be in a ſtreight and direct track; in which the Matter is puſh'd by the *ſhorteſt Courſe*, from the *Approach'd Body* to the Threads that are ſhaken by it. And if the Threads are mov'd by influence of any Matter emitted from the Glaſs, it appears to be impoſſible to explain how they ſhould be ſo, and *at ſuch diſtances*, without a Continuity. So that the Caſe ſeems to be thus; That the Effluvia paſs along, as it were in ſo many *Phyſical Lines*, or *Rays*; and all the Parts that compoſe them, adhere and joyn to one-another, in ſuch manner, that when any of 'em are puſh'd, all in the ſame Line are affected by that Impulſe given to others.

And for this purpoſe the *following Obſervations* deſerve to be conſider'd.

Observat. 1. Having laid a piece of Leaf-Braſs between two pieces of Wood, about an inch thick, and an inch aſunder; I apply'd a well-rubb'd Tube as near as the Wood would permit; but the Braſs receiv'd no manner of motion. But as ſoon as the Wood was remov'd, it was attracted vigorouſly, without any freſh Attrition of the Tube.

Obſerv. 2. When the Tube was well rubb'd; if a piece of Paper were immediately apply'd, ſo as to touch the upper part of it; the Leaf-Braſs, ſcatter'd up and down upon the Table, would not be attracted at all, tho' the Tube were held very near: But upon removing the Paper, thoſe Bodies were put into ſenſibly brisk motions.

Obſerv. 3. When a piece of Leaf-Braſs is hunted about a Room; it keeps its diſtance, according as the Effluvia are more or leſs vigorouſly emitted: Nor will it by any means ſink into the Sphere of the Effluvia, unleſs it meet a Body in its way; and then it will be *attracted* and *repell'd* ſeveral times, with a great ſwiftneſs.

Obſerv. 4. It may alſo be very properly urg'd upon this account too; that in the Experiment *for* Plate III Fig. 1. *producing Light by the Effluvia of the outward Glaſs, falling on the inward exhauſted Glaſs in motion*; after the Motions were *ceas'd*, a Light might be produc'd on the inward Glaſs, by approaching ones Hand near the ſurface of the outer one. Which ſeems convincingly to ſhew That Property of the Effluvia, I have been here ſpeaking of.

Obſerv.

Observ. 5. *When the Tube was fill'd with ſome other matter than Air, the Attractive Power of the Effluvia was conſiderably abated.*

Thus when I had ſtopt up one end of it with a Cork, and fill'd its Cavity with dry *Writing Sand*; tho' the ſame Attrition was made as before, yet the Leaf-Braſs had no Motion given it, till the Tube was brought within an inch or thereabouts of it. *But if the Sand were ſuddenly ſhot out of the Tube, then it would attract the ſame Bodies at double or treble the foremention'd diſtance, without any new Attrition at all.*

This laſt Experiment compar'd with one before recited, makes way for a very conſiderable Obſervation: which is this.

I have formerly ſhown, *That when the Air contain'd in the Tube was exhauſted, the Attractive Power was quite loſt*, or very near ſo.

And here it appears, *That when the Tube was fill'd with a Heterogeneous Body, the Attractive Power was exceedingly weakned.*

Now in both Caſes there was *an Excluſion of Air*; and in both Caſes the ſame kind of Effect follow'd, *viz.* the *Loſs of the Attractive Power.*

Only, where the Tube was *exhauſted*, the Air being *more perfectly excluded*, the Attraction was alſo more remarkably loſt, than when it *was fill'd with Sand*, by which the Air was excluded too, but *not ſo perfectly* as by the Exhauſtion.

Now I take the Reſult of theſe two Experiments in conjunction, *to be a ſignal Demonſtration of the Influence and Intereſt of the Air, in theſe Phenomena.* And if upon the filling the Tube with other ſorts of matter (than what I made this Trial with,) *the Effect ſtill* *appears to be the ſame*; it muſt then paſs for a Truth not to be diſputed. Tho' on the other hand, *if*, when the Tube was fill'd with other Matter, *the Effect ſhould not anſwer in the ſame manner or degree*, but the Attractive Power ſhould be pretty ſtrong and vigorous: yet *this will be no concluſive Argument* againſt the Intereſt of the Air in thoſe particular Experiments, which I have here mention'd. For 'tis poſſible that other Matter may in one caſe yield that aſſiſtance towards the production of the Effect, which ſome Action or Operation of the Air may afford in another different Caſe.

And I think it may ſtand for a very uſeful Enquiry. *How far other ſorts of Matter, with which the Tube may be fill'd, will any ways influence the Attractive Force of the Effluvia, ſo as to make any ſenſible alteration in it?*

Experiment II.

Concerning the Electricity *of* Sealing-Wax.

I Fitted a Wooden Cylinder (of about 4 inches diameter, and 3 in length) to an Axis, and plung'd it into melted Sealing-Wax, in which I kept it moving round till it had gotten a Coat of above ½ an inch thick, on its Surface. The Wax was of the beſt ſort I could procure; and the quantity melted was 12 *lib.* Having thus prepar'd the Cylinder, I plac'd it on the *Machine*, and gave it the uſual *Motion* and *Attrition*; which having been continued ſome ſmall time, I held the *Hoop of Threads* (made uſe of in the *Experiment of the Glaſs Globe and Cylinder*) directly over it. The Effect was the ſame, as in thoſe Experiments. *For the Threads were directed by the Attraction, towards the center of that Circle, in the Plane of which the Hoop* was placed. And while they remain'd thus directed, they would in like manner *fly the approach of ones Finger*. And Leaf-Braſs would be *ſtrongly attracted* and *return'd*, or *carried about a Room*, by the Effluvia of the *Wax*, as I have elſewhere ſhewn it would be by thoſe of *Glaſs*. And the Effluvia of Wax likewiſe will be *ſenſibly felt* upon the back of ones Hand, if the Wax (after Attrition) be moved to and fro, near it; juſt as thoſe of Glaſs may be. So that the *Electrical Qualities* of theſe two Bodies are the ſame, as to all the moſt *General Properties*. They differ only in *degrees*; the Effluvia of *Glaſs* producing more Powerful Effects, than thoſe of *Wax*. Plate VII Fig. 3.

Concerning the Light *producible from* Sealing-Wax.

At *Night*, I gave the forementioned Cylinder the ſame Motion I had given it in the *Day* (when I tried the Electricity of it,) and I applied ſome clean new *Flannel* to it; but there was very little or no Light at all produc'd by the Friction of *theſe* two Bodies.

But when, inſtead of the Flannel, I applied *my naked Hand*, a *conſiderable Light appear'd:* the Properties and Circumſtances of which (as far as I have obſerv'd them) I ſhall comprehend in the following Particulars.

This Light was viſible only in that part, where the Attrition was made. The Light produc'd by the Attrition of the Glaſs Globe, was viſible by its odd Flaſhes, all over the Surface of the Globe. It ſpread far beyond the part where the Attrition was made.

This Light depended moſt immediately upon the Motion; and would continue no longer, than that continued. Some Lights produc'd by the Attrition of Glaſs, have laſted for a while, even tho' the Motion has actually ceas'd.

None of this Luminous Matter would be communicated to ones Finger, when held near it. Whereas in the Lights produc'd from Glaſs, it was otherwiſe.

This Light produc'd by the Friction of the Hand, on the Wax, in the open Air; was hardly ſo conſiderable, as that produc'd by an Attrition made with Flannel, in Vacuo.

For That Light in *Vacuo*, was very diſcernable on each *Arm of the Braſs Spring*, that embrac'd the Flannel. And could the Attrition have been made with ones Hand in that *very-rare Medium*, there's no doubt but the Light would have been ſtill *much greater.* So that in this caſe there ſeems to be an Agreement between the Lights produc'd from *Wax* and *Glaſs*; viz. *That both appear to more Advantage in* Vacuo,

Vacuo, *than in open Air*; that is, in a very-*weak* and *much-rarefied* Medium, than in one of more *Force* and *Denſity*. And there's no Reaſon to believe, but all other Lights will agree in this Property too: Nor is it at all ſtrange that it ſhould be ſo, conſidering the *fine and tender Nature* of the Effluvia, on which theſe Lights, produc'd by Attrition, do depend.

Having thus ſhewn the Properties of this Light, produc'd by the Attrition of *Sealing-Wax*; I would ſubjoyn an Obſervation or two, which, I think, may deſerve notice.

Firſt, The Light produc'd by the Attrition of *Sealing-Wax* and *Woollen*, agrees exactly in one remarkable Property, with *that* which is produc'd by the Attrition of *Amber* and *Woollen*.

For *they* (*both of them*) *diſappear when the Attrition ceaſes*.

Secondly, This Light agrees, in another Property, with *ſome* Light produc'd by the Attrition of *Glaſs* and *Woollen*.

For the Light of *Sealing-Wax is confin'd to that part only where the Attrition is made*. And ſo it is ſometimes in the Friction of *Glaſs* on *Woollen*: For tho' the Light does many times *ſpread all over*, yet at other times *it is limited only to that part which is rubb'd*: as was obſerv'd in one of the foregoing Experiments.

Thirdly, *This* Light, and *That* produc'd from *Glaſs*, agree in another conſiderable Property, tho' in very different Circumſtances of the Bodies themſelves.

For,

For, *if a Glaſs Tube be* exhauſted *of its Air, the Light produc'd will not adhere to Bodies plac'd near it.* Nor will any Parts of the Light, produc'd from *Sealing-Wax*, in the *open Air*, faſten upon Bodies which are fairly expos'd to it, tho' brought very near.

So that the *ſame Property* which, in *one* caſe, diſcovers it ſelf in the *open Air*; in the *other*, neceſſarily requires a *Vacuity*, in order to the production of it.

So that the Effluvia of two different Bodies, (which otherwiſe do not agree in the ſame Property) may come to agree in the ſame Property, by the bare alteration of an external Circumſtance, or by ſome change in the ſtate of the adjacent Bodies. As here, by the meer Subſtraction of Air from the Cavity of the Tube, the Luminous Effluvia of *Glaſs* (which otherwiſe had the Property of *adhering* to Bodies plac'd near,) become now of that Nature, as *not to adhere*; wherein they agree exactly with thoſe of *Sealing-Wax*, provok'd by Attrition in the open Air.

Query. Suppoſing *Lac* and *Vermilion*, to be the Ingredients in the Compoſition of *Sealing-Wax*; whether the *Attractive Quality* be owing cheifly to the *Former* or the *Latter?* Which will be anſwer'd, by trying the Attractive Power of *equal Bulks or Maſſes* of the ſame Sealing-Wax, made up with *different Proportions* of theſe two Ingredients. *Ex. gr.* Suppoſe I take any two quantities of *Lac* and *Vermilion*, and with them make a *Spherical* or *Cylindrical* Body of *Sealing-Wax*: And then for a ſecond Compoſition, take either a greater or a leſſer quantity of *Lac* than before; and mix ſo much *Vermilion* with it, as will make a Spherical or Cylindrical Body, of the ſame dimenſions exactly as the former.

former: If the *Lac* be *more*, and I find the Attraction of the second mixture *stronger* than that of the first; 'tis plain that the Attraction is principally owing to the *Lac*: But if the *Lac* be less, and the Attraction be still stronger; 'tis plain that the Advantage of Attraction lies on the side of the *Vermilion*: Or *vice versâ*. And so with respect to any other *Factitious* Bodies, we may discover what Ingredients (and in what proportion) do principally conduce to this Effect.

And the same Enquiry may be usefully made, with respect to the *Luminous*, as well as the *Attractive*, Quality.

And I should think it no inconsiderable step towards the advancement of our Knowledge of the True Nature of Bodies, to be satisfied upon what Principles or Ingredients in their composition, their *Light* and *Electricity* do mainly depend.

For this Point well settled, with respect to *Factitious* Bodies; we might be enabled to judge more truly of the Causes of the like Effects in *Natural* Compositions.

Experiment III.

Concerning the Electricity, *&c. of* Sulphur *and* Rosin.

HAving provided two *Wooden Cylinders*, of the same dimensions as that mention'd in the former Experiment; I coated their outsides, the one with *Sulphur*, and the other with *Colophony* or *Rosin* mix'd with *Brickdust*; which was added on purpose to bind the Rosin, and make it more hard.

Having given the *first* of these the usual Motion and Attrition, I brought it near the *Hoop fitted with Threads*; and found that *the Threads were attracted, and directed to its Centre*; but not near so strongly, as when the *Sealing-wax* was us'd.

And this, upon several Trials, was still much the same.

Then I try'd the latter, (*viz.* the Cylinder coated with *Rosin*,) and found that *the Threads were drawn to the Centre with more force and vigour*, even than when the Experiment was made with *Sealing-wax*: But this is to be added, that the Rosin having been melted, was not quite cold at the time when the Trial was made.

This is the main of the Experiment; to which I must subjoyn these following Observations.

First,

First, That when the Trial was repeated with theſe Bodies, the next day; *the Electricity of both was ſo inconſiderable, as ſcarce to deſerve notice.*

Secondly, The *Roſin*, while *warm*, would attract *Leaf-Braſs*, at the diſtance of an inch or two, *without any Attrition at all.*

Thirdly, In both theſe Trials, the Threads would fly the approach of ones Finger; but if *Sealing-wax* or *Amber* were held near them, *tho' they were neither of them rubb'd, yet the Threads would have a ſtrong tendency towards them.* A Phenomenon I never obſerv'd any thing of before; and which gives a very ſurprizing inſtance of the *Attractive* and *Repulſive* Forces. That the Threads ſhould be attracted by an Electrical Body, *while warm*, tho' the Attrition were ceas'd; (as for example, by the Roſin, while it yet retain'd a degree of Heat;) this is no great wonder: but that they ſhould be attracted by ſuch a Body, in a ſtate wherein that Body was perfectly free from any degree of Heat, and without any preceding Attrition to excite and rouze the *Effluvia*; this I think has ſomething very odd and peculiar in it. Nor do I think That centrifugal Motion of the Threads, upon the approach of a Finger, leſs ſurprizing. The Threads were altogether *in the ſame ſtate*, when each of theſe Bodies, that *produc'd theſe very different Effects*, were plac'd near them. And yet they were *repell'd* from ſome of them, and attracted by others. But now, before that the *Amber* and *Sealing-Wax* on the one hand, or the Finger on the other, were brought near them; the Threads had been mov'd and *acted* *upon*, by thoſe *Electrical Bodies mention'd in the Experiment*. From whence theſe two Queries.

Firſt, Does that *previous motion and excitation of the Threads*, any way *cauſe* or *contribute* to this ſo very different an Effect, of their *flying from one Body, and ſtrongly tending to another?*

Or, *Secondly*, Is the Reaſon of this Phenomenon to be entirely deduc'd *from the Natures of the Bodies themſelves, to which the Threads were expos'd?* So that by Vertue of *ſome Law* or other, unknown to us, the Threads ſhould *tend towards This Body*, and *fly the approach of That.*

Which of the two is the true Cauſe, I muſt leave at preſent to farther enquiry: And I think alſo it will not be very difficult.

Thus much for *the Electricity* of theſe Bodies. As to their *Luminous Quality*, I have but little to ſay.

Upon an Attrition of the *Roſin* in the *Dark*, I *could find no Light at all.*

And but *very little* from the *Sulphur:* And that not by a Friction made with my open Hand neither; but by holding the ends of my Nails very hard upon it, while it was in motion. And therefore, *it either contains but a very ſmall portion of luminous Matter in it*; or elſe, *That Matter is ſtrongly retain'd within the Body of it*; ſo that the ordinary degrees of Attrition are not ſufficient to bring it forth.

I am apt to believe, that the *Latter* is more the true Reaſon, why ſo ſmall a quantity of Light is producible from Sulphur; than the *Former*. For *Action* and *Re-action are equal in all Bodies.* Now as Light acts more upon *Sulphur* (and *Sulphureous* Bodies)

than

than it does upon any others; ſo reciprocally Sulphur acts more upon *Light*. And therefore, *it being more ſtrongly held* in the Body of the Sulphur, by vertue of that *Law*; the *Emiſſion of it is much more difficult.*

For *ſuch a Degree of Attrition*, *the* Momentum *whereof exceeds the* Momentum *of the Attraction of the contain'd Luminous Matter by the Body which contains it*; *is neceſſary to educe Light*, *or Luminous Matter*, *out of that Body.*

And therefore Bodies which do with equal facilities emit their Light, ſhould ſeem to have equal attractive forces on that Matter.

And *Univerſally*, The Attractions ſhould be proportional to the Forces of Attrition, all other Circumſtances being alike.

I tried whether *Sulphur* would emit any Light, by an Attrition in *Vacuo:* but with all my endeavours *I could find none.*

Now there was a very vigorous Light produc'd by an Attrition of *Sealing-Wax* in *Vacuo*; more conſiderable than that produc'd from the ſame Body in the open Air. Whereas the Effects of *Sulphur* were juſt the contrary: There was a ſmall Light, produc'd with much labour, in the *open Air*; and none at all in *Vacuo.*

Query 1. Does the *Abſence of the Circumjacent Medium* any ways contribute *to the more ſtrong Retention* of the Luminous Matter in the Body of the *Sulphur?* If ſo; it *has an Influence to produce the contrary Effect* in the caſe of the *Sealing-Wax*, where a briſk Light appear'd when the Air was withdrawn.

Or, 2. Are the Effluvia of the Sulphur indeed emitted in *Vacuo*, as well as in the open Air; but not ſenſibly *Luminous* in that State of the Ambient Medium, that is, not *Viſible?*

Experiment IV.

Concerning ſome very Uncommon Effects *of the* Effluvia *of Sealing-Wax.*

IN that Experiment, where the Threads are included in a Glaſs Globe, and, upon the Attrition of it, *point every way from the Center to the Circumference*, it was obſerv'd that, in that State, a Motion might be given to the Threads, by the approach of ones Hand near the outſide. And this odd appearance, we know, is to be attributed to the Effluvia of the Glaſs, excited by the Attrition. For by ſome things formerly mention'd, it appears they are endowed with a Quality, which renders them capable of producing ſuch Effects.

But I find, that *the Effluvia of other Bodies, held without the Globe, will alſo perform the ſame Thing.* Tho' the Threads *are included there*, and *the Globe has no Motion nor Attrition at all given it*; yet if *another Electrical* Body be plac'd near, they will *move after a very ſtrange and ſurprizing manner.*

For they did ſo when I held rubb'd *Sealing-Wax* at the diſtance of 3 or 4 inches from the Globe. Alſo *Amber*, or a *Glaſs Tube*, would produce the ſame Effect.

Leaf-Braſs, cover'd cloſe with a flatbottom'd Glaſs, upon a Table; would have a brisk motion given it, by holding the rubb'd Sealing-Wax over it. And *one ſingle Attrition* of the Wax, would *be ſufficient to keep thoſe included little Bodies* ſtirring for a conſiderable time. Nay, they have *continued their Motion* after the Wax has been *taken away*.

This ſhews the Penetration, Subtilty, and very great Activity of the Effluvia (at leaſt of theſe) *Electrical Bodies.*

But 'tis to be noted here,

Firſt, That *this Experiment will not always ſucceed.* Sometimes not at all; much leſs in that degree, I have here related. And the reaſon of this I take to be from a more Humid Temper of the Air, in which ſtate ſome little moiſture was probably condens'd upon the Surface of the Glaſs; and enough to be ſure, might eaſily be, to obſtruct the Paſſage of Bodies ſo fine and ſubtile as theſe Effluvia.

Secondly, This *Inconvenience may be remedied, when it does happen.* For if the Glaſs be plac'd a while in the *Sun-ſhine*, or a little warm'd by the *Fire*, or well rubb'd with a warm dry *Linnen Cloath*; then the Leaf-Braſs, if the rubb'd Wax be held over it, will be put into as brisk motions as before.

Thirdly,

Thirdly, This *warming or rubbing of the Glaſs, ſeems not only to clear it of the moiſture, that might be condens'd on its Surface; but alſo, by actuating the parts of the Glaſs themſelves, and perhaps raiſing ſome little quantity of Effluvia from it, encreaſes the force of thoſe of the Sealing-Wax, and renders their action on the included little Bodies more conſiderable.*

This I conclude from hence, *viz.* That when I had us'd any of the foremention'd methods, to clear the Glaſs from any thing of Soil or Moiſture it might have contracted; I found I could *give a Motion to the Leaf-Braſs, only by rubbing my Finger on the outſide of the Glaſs, without any aſſiſtance from the Wax.* But yet when the rubb'd Wax was held over it, *the motion of the included Bodies would be much more brisk.*

However, when the Air is *warm* and *dry*, I never found any occaſion to do any thing to help forward the Action of the Effluvia; their paſſage being then ſufficiently clear; and the Bodies within, ſhewing by their various agitations how much they lie expos'd to their Power.

An Account of the Succefs of an Attempt to keep feveral Atmofpheres of Air condens'd in the fpace of one, for a confiderable Time.

I Took a very *ftrong thick Flint-Bottle*, which I had procur'd to be made on purpofe for this Experiment; into which I injected with my *Syringe* between 4 and 5 Atmofpheres of Air; as an included *Gage*, of about $4\frac{1}{2}$ inches in length, plainly enough fhew'd. For, the Mercury rifing up fo far, as to fill about $\frac{5}{6}$ of the whole Gage, confequently compreſs'd the Air in the upper part of it, into nearly $\frac{1}{6}$ part of the fpace it poffefs'd before. This Air continued in that ftate of violent condenfation from *March* the 30th, till about the 7th of *Auguft* following. At which time happening to look on it (as I ufually did once in 4 or 5 days) I found that the *impri-fon'd Element had made its efcape.* Nor was I at any lofs for the Caufe hereof, when I confider'd the *intemperate heat of the weather* for fome time before. For one day efpecially, I obferv'd that the *Spirit in the Thermometer* had afcended 120 degrees above the *Freezing Point.* This hot ftate of the Ambient Medium, was fufficient to produce the foremention'd Effect; and to render the *Cement*, by which the Brafs-Cap of the Bottle was faften'd, (*even tho' it was pre-ferv'd*, for the greater fecurity, *under Water*,) to make it, I fay, fo foft and yielding, as not to be able to refift the Efforts of the mighty Spring of the inclos'd Air. By this means, all thofe parts of it, whofe Springs preferv'd their *Tone*, readily exerted themſelves, and got away out of the Bottle, leaving others behind them which were not able to unbend and conſequently to gain themſelves liberty. For I found that the *Mercury* in the *Gage*, continued ſtill about $\frac{3}{4}$ of an inch above the ſurface of that wherein the open end of the Gage was immers'd; by which it appear'd, that the Air in the upper part of the Gage, ſtill remain'd compreſs'd into a ſpace, which was about $\frac{1}{6}$ part leſs than what the ſame bulk took up before the injection. But what deſerves moſt particular conſideration is, that *the Mercury ſtill kept the ſame height, after its Surface in the Bottle was expos'd to the open Air.* So that thoſe remaining parts of the Air inclos'd within the Gage, though they *had all the Scope and Freedom poſſible, to expand themſelves, yet did not do it*; and therefore were ſome way or other *render'd uncapable* of ſo doing. Had they been as able as the others, which went off before; they had likewiſe gone off too. So that their long detention in that violent ſtate, muſt needs have made them unable to unwind themſelves, ſo far as was neceſſary to their own diſcharge. And had not the 'foremention'd Accident happened, but they had continued in the condition they were in at the firſt injection; *there's no doubt but the diſorder they ſuffer'd* would have been ſtill greater, and their *incapacity of Reſtitution* conſiderably more.

Thus much for the Experiment it ſelf.

And tho' the ſucceſs of it was not ſo perfect as might be wiſh'd for, yet 'tis ſufficient to inform us,

That *Air by long and violent Compreſſions, may* (to all appearance) *be depriv'd of much of its Elaſtick Power.*

That the Self-reſtoring Quality of thoſe *fine Springs* (which in many inſtances produce ſuch wonderful

Effects,) is so far impair'd by their being *held bent* for a long time together, that afterwards they do not sensibly exert themselves, tho' plac'd in the most *favourable and likely Circumstances* for so doing.

Thus we see, that that portion of Air which was left in the Gage, was not able to depress the Mercury, tho' the Surface of the Mercury in the Bottle was now expos'd to the open Air. The *injur'd Springs* could not recover themselves to their former *tone and temper*, but continued in a sluggish rest upon the *Mercury*, even after way was made for their *free and easie Expansion.*

Now if this was the Effect of the Condensation of *less then* 5 *Atmospheres* of Air; *the greatest part of which, made its escape*; and the *longest time* that any part of it was held in this state of violence, *was little more than* 4 *Months*; what would it be if 9 *or* 10 *Atmospheres* were crouded into the room of one, and continu'd in that condition for *as many years, as the other did Months?* Would not this Air lose its Elasticity much more than the former? *Would its tender Springs be able to unwind themselves much, after so long and powerful a restraint?*

Would not the Mercury *keep its Height and Station in the* Gage, *notwithstanding that the Vessel containing Air so compress'd as this, should afterwards come to be expos'd to the open Element?*

What kind of Liquid would so many Atmospheres of condens'd unelastick *Air, compose?*

Would not Terrestrial Animals be suffocated in it, as they would be in Water or other Fluids?

Would not Fire *in like manner be quickly extinguiſh'd, and perhaps with ſome noiſe and hiſſing, if put into ſuch a Fluid as this?*

Might not light Bodies (ſuch as thin Glaſs bubbles) *float upon ſuch a Medium; ſuppoſing them not above* 5 *or* 6 *times ſpecifically heavier than common Air?*

Is it impoſſible that Air by this means ſhould become a Viſible, Palpable *Fluid; and be Subject to ſome of the ſame managements that other Fluids are?*

What would be the conſequence of the action of an intenſe Heat upon Air thus compreſs'd and depriv'd of all its Spring? Would it *rarefie*, and at laſt *recover its Elaſticity again, by the Changes poſſible to be wrought* by the long continued action of Fire? Or, would its parts be only violently hurried about, as thoſe of other Liquids are by the like cauſe, which afterwards ſettle and compoſe themſelves again?

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An Experiment concerning the Production of Light in an exhausted Glass (lin'd within-side with Sealing-Wax,) *upon an Attrition made without.*

HAving procur'd a Glass *Globe*, of about 6 inches diameter; I put into it a convenient quantity of broken *Sealing-Wax*, and held it over a *moderate* Fire till the Wax was melted. Then turning the Globe about, that the Wax might slip from one place to another; it had quickly got a pretty thick Lineing, on more than half its inside: But 'tis to be observ'd, that it was not in all places equally thick, it being impossible to manage the melted Wax in such a manner as to make it so.

Having done thus, I plac'd the Globe in *a fit* posture, and *left it till it was perfectly cold*; and then having fix'd the *Brass-work* to it, I *exhausted it of its Air*. It was (immediately upon this) applied to the *Machine, represented in* Plate VII; *where the manner of giving motion to it, is so obvious, as needs no description*; and then making an *Attrition* with my *Hand*, I observ'd the following *surprizing Phenomenon*, which *the Evening* (the proper time for such Observations) permitted me to do with great advantage.

My Hand was no sooner applied to *that part of the Globe which was lined with the Sealing-Wax*, but *I saw the shape and figure of all the parts of my Hand* (which touch'd the convex Surface of the Glass) *distinctly and perfectly upon the concave Superficies of the Wax within.* When the Glass alone,

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without any ſuch Lining on the inſide, is made uſe of; 'tis obvious to any one (who has ſeen or does but conſider the Experiment) how plainly a Hand muſt be ſeen, which is plac'd on the convex Surface of a Globe all over enlightned with a ſtrong-flaſhing Light. And perhaps it may ſeem ſtrange, if I ſhould ſay, that *the Appearance was now as plain and viſible as then, notwithſtanding the interpoſition of the thick body of Wax.* 'Twas as if there had been only pure Glaſs, and no Wax in the way; or as if the Glaſs had been away, and the Wax were tranſparent. This *Lining*, where it was ſpread the *thinneſt*, would but juſt allow the ſight of a Candle through it *in the dark.* But in ſome places, it was at leaſt $\frac{1}{8}$ of an inch thick. And yet even in thoſe parts, *the Light and Figure were as diſtinguiſhable* as any where elſe. Nay, tho' ſome parts of the Sealing-Wax did not adhere ſo cloſe to the Glaſs as others, *yet the Light appear'd on theſe, juſt as on the reſt.* This Light produc'd, was *not diſcernible at all thro' the body of the Wax*, but was to be ſeen by looking thro' the other parts, where the Glaſs was free and tranſparent. The *Colour*, and other Properties of it, reſembled thoſe of *the Lights produc'd from pure Glaſs*; except in this one Inſtance, *That upon the admiſſion of a ſmall quantity of Air into the Globe, the Light wholly diſappear'd in that part cover'd with the Sealing-Wax*, and not in the other.

When all the Air was let in, the Hoop of Threads being held over the Glaſs, *the Threads were attracted at greater diſtances, by that part which was coated with the Wax*, than by the other. But *even then, when all the Air was exhauſted*, the Wax would attract Bodies plac'd near the outſide of the Glaſs: For in this caſe I found *the Threads had their Central directi-*

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on, tho' *not ſo vigorouſly*, as when all the Air was let in.

But this is further remarkable too, with reſpect to that ſtate of the Air's abſence; *viz.* that *the Threads would not be attracted, if held over that part of the Glaſs which had no lining of Wax on the inſide:* whereas if they were brought within the Sphere of the *Effluvia of the Wax*, they would direct themſelves towards it.

So that in the Courſe of this Experiment, there are theſe following things to be taken notice of.

Firſt, *A diſtinct and lively repreſentation of the Shape and Form of an Object, upon a ſolid opake Body, to which that Object was not immediately applied; and this by the Light produc'd upon the Attrition of another Body (ſolid,* tho' not *opake,) to which that Object was immediately applied.*

A Man would have been thought the Author of a very ſtrange *Paradox*, that ſhould have aſſerted this, and at the ſame time conceal'd the Experiment; which ſhews how and which way 'twas done.

Or, ſhould it have been propos'd by way of *Problem*, thus: *To repreſent the Figure of an Object (plac'd behind an opake Body) upon the contrary ſide of that opake Body*; and *this without the help of Optick Glaſſes, or any foreign adventitious Lights:* perhaps the Solution might have been thought impoſſible; or, it may be, the very Terms of the Problem it ſelf, abſurd and contradictory.

For, the Body on which the Figure is to be ſeen, muſt be an *opake* one, (by the Hypotheſis;) and the Object it ſelf plac'd on the contrary ſide to that which it is ſeen on : So that either the Light muſt *be tranſmitted* thro' this Body, and then *'tis not opake*, which is contrary to the Hypotheſis; or elſe the Light muſt *not be tranſmitted*, and then *no Figure could be ſeen*; for all diſtributions of Light by *Optical Artifices*, are excluded by the firſt Suppoſition.

But we ſee, this is not only poſſible, but alſo plain Matter of Fact.

From whence, I think, it may be uſeful to obſerve, *That many odd Effects and Appearances, which we may argue very plauſibly to our ſelves, againſt the Poſſibility of; and ſeem to find downright Abſurdities and Contradictions in; may yet be brought about by the genuine Forces of Nature, acting in convenient circumſtances, upon proper and ſuitable Bodies.*

And from hence; That we do not, upon ſuch occaſions, proceed to conclude too peremptorily, what may or may not be done; and think, that every Difficulty or *Apparent* Impoſſibility to us, is a *Real* one to Nature it ſelf.

Secondly, *The uniform Clarity and Perſpicuity of the Figure repreſented, thro' all the parts of the opake Body,* (*viz.* the Wax,) *on which it was ſeen; being as viſible in the thickeſt and groſſeſt, as in the fineſt and thinneſt parts of it; and on thoſe which lay out farther from the Glaſs, as on thoſe which adher'd more cloſely to it.*

Thirdly, *A total Diſappearance of the Light in all that part cover'd with the Wax, upon the admiſſion of a ſmall quantity of Air; and its Continuance in the other parts of the Glaſs* at the ſame time.

Both

Both these last-recited Heads do also furnish something that looks pretty strange.

Here's a Figure transmitted thro' the most dense and compact parts of an opake Body, with the same facility and advantage to the Eye, that it is thro' those which should seem the most easily pervious to the radiant Matter which is to form the Representation.

Again: Here's a *notable Distinction observable in the Lights produc'd*. They were such, that one and the same Cause *destroy'd* the one, and *left the other untouch'd*. The Air swept away all which arose from the *parts lin'd with the Wax*; whilst the other Regions of the Glass preserv'd their Light without any diminution.

Fourthly, *The more strong and vigorous Attraction, from that part of the Glass lin'd with the Wax, than from the other:* which was manifest by the extent of the Attractive Power, from thence to greater distances than what the other would reach to.

Fifthly, *The Attraction and Central Direction of the Threads to the Wax, even while the Globe was exhausted of its Air.*

This answers to a like Phenomenon of the *Load-stone*; whose *Effluvia* will work their Effect, even when the *Stone* it self is plac'd *in vacuo*. So here the Threads were push'd towards the Wax, when at the same time it was included in a Glass, whose Air was drawn out. But then here's this difference, that *the Threads were less vigorously drawn in this state, than when all the Air was let in*; whereas all *Magne-*

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tick Attractions are (*at the least*) equally ſtrong in *vacuo*, as in the *open Air.*

Sixthly, *The Limitation of the Sphere of Attraction to that particular part of the Globe, which had the Wax on the inſide,* (during that ſtate of the Air's abſence.)

Theſe things thus obſerv'd; we may now reaſon a little upon 'em, in the *modeſt way of Enquiries.*

1. *May not one Body* attract (and as it were imbibe) *the Effluvia of another contiguous Body; eſpecially when Motion and Warmth have made an eaſie paſſage for ſuch Effluvia into the Interſtices of that Body, whoſe attractive Power tends to fetch them thither?*

2. *Might not* (therefore) *the Sealing-wax, by vertue of that Law, incorporate with it ſelf the Luminous Effluvia emitted from the contiguous Glaſs?* Glaſs gives a free paſſage to the Effluvia of Sealing-wax: May not Sealing-wax (on the other ſide) as freely admit the Effluvia of Glaſs?

3. Suppoſing the Body of the Sealing-wax thus charg'd and repleniſh'd with the Luminous Effluvia of the Glaſs; *Would it not in that ſtate appear Luminous it ſelf?* Do not all Bodies that ſhine, do ſo by vertue of Lucid Matter lodg'd in 'em; and, in ſome degree, more or leſs forcibly darted from 'em? Why ſhould not *Wax, every where replete with ſhining Corpuſcles, appear ſhining*; as well as *Wood* charg'd with *fiery* parts, gives us the Senſation of a *burning Coal*; or *Smoke throughly heated,* that of *a lively Flame?*

4. *What*

4. *What is it to be* pellucid, *but to* tranſmit *Light* receiv'd? *And does not the Wax thus tranſmit the Luminous Matter, attracted and imbib'd from the Glaſs?*

5. *Has not the Wax* (therefore) *in this ſtate, a ſort of Tranſparency?* I ſay *in this ſtate*: For *the Property is limited to the preſent Circumſtances* of theſe Bodies concern'd in the Experiment?

During the *Attrition*, there is an *Eruption* of Luminous Effluvia from the Body of the Glaſs.

Does not the Attraction take place, *as ſoon as the Matter to be attracted is furniſh'd* by the contiguous Glaſs? Is not the Wax *ſaturated with Light* as ſoon as the Attraction commences? And when the Wax is ſaturated with Light, does it not *then appear luminous?* (that is, does it not communicate ſome parts of the *Light receiv'd*, to the circumjacent Medium?)

6. Since therefore the Sealing-Wax in this ſtate, is not to be conſider'd purely as an *opake Body*, which oppoſes the Tranſmiſſion of Light, (*as it really is in all other Circumſtances*,) but as a Body every where *pervious to the Lucid Matter* emitted from the neighbouring Glaſs; *May we not from hence conceive, how the Figure of an Object plac'd on the one ſide, may be repreſented on the contrary ſide thereof*, (namely, that ſide which is turn'd towards the Eye of the Spectator?) Why ſhould I not as well ſee my Hand plac'd on the Glaſs, whilſt the Wax is *thus open to the Luminous Effluvia*; as ſee it when I place it behind any ordinary tranſparent Body whatſoever?

In a word; 'Tis plain matter of Fact, that *the Figure of the Hand is ſeen on the contrary ſide of the Wax.* And 'tis demonſtrable from the very Circumſtances of the Experiment, that *that Figure is not form'd there by any of the common ways of picturing Objects by Reflected or Refracted Light.*

The Figure therefore is *tranſmitted thro' the Body of the Sealing-Wax.* But no *Species* or Picture can be tranſmitted thro' an Opake Body while it continues to be Opake; that is, while it continues *impervious to the Rays of Light.* Therefore the Wax muſt, *at that time*, be in the *contrary ſtate*; that is, *pervious* to the Luminous Matter. This Luminous Matter is originally emitted from the Glaſs in *the Act of Attrition*; but how it ſhould paſs from thence into the body of the Wax, without *an attractive force bringing it thither*, I cannot (at preſent) tell how to conceive.

7. *Qu.* Whether the *Figure* was not as *diſtinctly form'd* on the thickeſt, as on *the thinneſt parts of the Wax, upon account of the* Quantity of Luminous Effluvia *every where attracted in proportion to the quantity of attracting Matter*? And whether, it was not upon account of the very ſmall (*comparative*) difference between the *diſtances of the fartheſt and neareſt parts of the Wax, with reſpect to the Glaſs*; *that the Luminous Matter was pretty equally drawn to both*, and *ſo the appearance became* (to Senſe) *equally diſtinct on both*? Or that the *vibratory motion* of the Effluvia at their Eruption from the Glaſs, might bring them as well within the Attractive Sphere of the remoteſt, as the neareſt parts of the Wax?

8. *Is not the more ſtrong and vigorous Attraction from that part of the Glaſs lin'd with the Wax, caus'd by the* united attractive Forces *of the* Glaſs *and* Wax?

An Account of ſeveral Experiments about the Aſcent of Liquids, between the nearly-contiguous Surfaces of Bodies.

WHenever we give *Natural Cauſes* an Opportunity of exerting themſelves in the *ſame* or *ſimilar* Circumſtances, we have reaſon to expect the *ſame* or *ſimilar* Effects. If any Phenomenon be the Reſult of *ſuch a Principle or Power* in Nature, upon *ſuch or ſuch an Application or Diſpoſition* of External Matter; then, when the like Diſpoſition is made again, there's little doubt of the appearance of the ſame Phenomenon. Some Effects indeed there are, plainly reſtrain'd and conſign'd to ſome *particular* Qualities of Matter; as the Phenomena of *Light* and *Electricity*, (before diſcours'd of;) which won't ſucceed in all Bodies alike. Others depending upon a far more *general and comprehenſive Cauſe*, require no more, in order to their appearance, than *fit Circumſtances*, or a convenient Diſpoſition of Bodies, with reſpect to one-another; and ſo, things being brought within the *Sphere* of that Cauſe on which ſuch Effects depend, they are immediately produc'd of courſe, by ſome *univerſal eſtabliſh'd Law of Nature.*

Of this *later* sort (if I do not greatly mistake,) *are those Phenomena which we have now under Consideration.*

The Experiments made upon the Ascent of Liquors in small Tubes, gave me an occasion to think, what Varieties might occurr upon the making the Experiment after a manner different from what before had been us'd: And what Success I have had in these Trials, I have here given a large and particular Account of, under the following Heads. In all which the *Philosophical Reader* will discover an *exact Uniformity of Appearances and Effects*, consequent upon the *similar Circumstances* and Conditions of External Bodies.

Here were no *small Tubes* made use of in any of these Experiments: But when Bodies were plac'd together in such a manner, that something equivalent to *small Tubes would necessarily result* from their *very Position*, with respect to one-another; then the same thing always came to pass, that would have done had *common small Tubes* themselves been made use of.

EXPERIMENT I.

Of the Ascent of Liquor between two Glass Planes, in the open Air.

I Procur'd a couple of *Glass Planes*, which were part of a broken Looking-glass, being about 7 inches long, and $1\frac{1}{2}$ inch in breadth. Now tho' these, when clapt together, were very close, as seeming to *touch in many parts*, yet *when they came to be immers'd* in a Liquid, it *would ascend between 'em*: as was manifest upon their separation, when they were

found

found actually wet on all their parts. But this Liquor being ſo *thin* and *colourleſs*, the Aſcent of it between the Planes was not ſo *eaſily diſcernible.* Wherefore, to make it more obvious, I put a *ſmall piece of Paper* on each corner, by which means they were ſeparated by *an Interval equal to the thickneſs of the Paper*, when they came to be applied to one another. This done, I *plung'd one end* of them under the ſurface of a *ſtrongly-tinged Liquor*; upon which it began immediately to aſcend, but *not with that ſwiftneſs as in ſmall Tubes:* However, the Motion of it was very odd, being ſometimes *higher in one part than in another*, and ſhooting out very pleaſantly into diverſe *Branches*; which it continued to do, till it had *reach'd its greateſt height.*

But the Height of its Aſcent *varied according to the diſtance of the Planes.* For if inſtead of *one piece* of Paper on each corner, *two* were laid there, the Liquor would *not mount ſo high* in the later caſe, as in the former when the Planes were ſeparated only by *a ſingle Paper.* And then, if the Planes were *any ways declin'd*, the Liquor would ſtill ſpread it ſelf farther and farther, in *proportion to the degree of Declination.*

And, upon ſeveral Trials, this all ſucceeded much after the ſame manner.

Experiment II.

The same in Vacuo.

BEing willing to try the Ascent of the Liquid between the Glass Planes, in an *exhausted Receiver* as well as in the *open Air*; I fix'd the Planes so to a *Brass Wire*, (which pass'd thro' the Cover of a *Receiver*,) that I could make 'em descend at pleasure. In this manner I convey'd them into the *Receiver*, together with a *Dish of tinged Liquor*; which having plac'd on the *Pump*, the *Gage* in a little time shew'd the Air to be pretty nicely drawn out. Then I plunged the Plates (separated by pieces of *thin Paper*, as before,) into the Liquor; *which arose between them, as in the open Air*. Nor was there any other difference than only this; *That there appear'd more Intervals or Spaces between the Branchings of the ascending Liquid*, than when 'twas try'd in the *open Air*.

However, *when the Air came to be let in again, those Spaces were fill'd up with Liquid*; which was now an *entire Body*, without interruption.

Experiment III.

The Ascent of Liquids, between Marble *and* Brass Planes, *in the* open Air.

I Procur'd a pair of *Marble Planes*, ground as true as the Workman could possibly make them: These I joyn'd together *dry*, and *without any thing betwixt 'em*; which having done, I *immers'd* the lower Edge of 'em about ¼ of an inch *below the surface of the Water*, and held 'em there for some minutes: Then taking 'em out, I found I could not easily part 'em, without *sliding* 'em one off from the other. But having separated 'em that way, I presently found how far the Water had insinuated it self betwixt 'em.

This Ascent of the Liquor *I found, upon various Trials, to be different*; but always observ'd, that *when I had newly rubb'd the Planes over with Wood-ashes, the Water would ascend the highest.*

After this, I made use of a pair of *round Brass Planes*; which having order'd as before, *the Success was very agreeable to what it was in the former Case.*

And there's little reason to doubt, but the same thing would happen, if any other sort of Bodies were us'd, whose Surfaces are very *plain* and *smooth*, and posited so, as to be *nearly contiguous* to each other.

EXPERIMENT IV.

The Aſcent of Liquors between two round Glaſs Planes, *in the* open Air.

I Laid theſe round Planes one on the other, without any thing at all to ſeparate 'em; and having plunged the round Edge juſt under the ſurface of the tinged Liquor, I obſerv'd, *That it immediately ſpread it ſelf thro' the whole ſurface of 'em, and reach'd the extream parts.*

In the *other* Caſes (before mention'd,) a *ſtreight flat* Edge was applied to the Liquid; but *here* only a *circular* one; ſo that fewer parts of the Glaſs were dipt in this Trial, than in the former where *ſquare* and *oblong Planes* were us'd. Notwithſtanding which difference, the Water mounted upwards, and that in as *little time* too, as in the former Experiments.

EXPERIMENT V.

The Aſcent of Water thro' a Tube fill'd with Aſhes, *in the* open Air.

I Took a Glaſs Tube, whoſe length was 32 inches, and the diameter of its Cavity near $\frac{1}{4}$ of an inch. To one end of this Tube I ty'd a *piece of Linnen Cloth*, and then fill'd it with *Aſhes*, which had been *ſifted* thro' a pretty *fine Searſe*. As I put in the Aſhes by by *ſmall quantities* at a time, I ramm'd them down ſtrongly with a *Rammer*, whoſe *Baſis* was very little leſs then the *Bore* of the Tube ; by which means *I crouded them together as cloſe as was poſſible.* When the Tube was full, I ty'd over that end of it (by the *Neck*) a *thin limber Bladder*, (which I freed from all its included Air) in order to receive that Air, which I expected would be *forc'd thro' the Aſhes upon the Aſcent of the Water.* This done, I plung'd that end of the Tube (to which the Linnen was ty'd) under the Surface of the Water in a Glaſs; and *found that the Water did preſently begin to riſe.* The *very firſt Aſcent* was pretty conſiderable; For in the ſpace of 16 Minutes it had got up near 1 inch and $\frac{3}{4}$. But as it *advanc'd ſtill higher*, its progreſs was *ſlower*, and that in ſuch Proportions as here follow.

At the end of 24 Hours, it had riſen but to 16 inches; the *Bladder at the top being then near half fill'd with the Air*, which had quitted the Aſhes, as the Water paſt thro' them. But here happen'd an Accident, which *prevented any farther Obſervation of the Swelling or Diſtention of the Bladder by this expel'd Air.* For the *upper part* of the Tube (to which the Bladder was ty'd) being crack'd round, ſoon after drop'd off. However, this hinder'd not *the continuation of the Experiment with reſpect to the Aſcent of the Water.* For at 24 Hours from the *laſt Obſervation*, I found it had *gain'd 6 inches more* in height. And 'twas very eaſie to trace it in all its motions, by *the change of colour the Water gave thoſe parts of the Aſhes it paſt thro'*, which render'd them very diſtinguiſhable from *thoſe which were yet dry.*

When 24 Hours more were compleated, the Water had riſe 4½ inches, and ſomething better. And at the *ſame diſtance of Time* again, it had aſcended 3 inches higher. The ſucceeding 24 Hours brought it 2 inches higher ſtill; and now it was gotten within ½ inch *of the top of the Tube*. In the ſpace of 10 Hours more, it finiſh'd that *little remaining part*, and reach'd the Extremity of the Tube compleatly.

Such was the Progreſs of the Water, and at this rate did it make its way thro' the *compact Body of the Aſhes* with which the Tube was fill'd.

Having finiſh'd this Obſervation, I then reſolv'd to know, *what quantity of Water the Aſhes had abſorb'd*; in order to which I proceeded thus. I weigh'd a Glaſs of Water very nicely, and pour'd part of it into the Glaſs; wherein the Tube had all-along ſtood, *till it reach'd the mark* at which the Water ſtood, when the Tube was firſt immers'd in it. Then (weighing the remainder) *I found the quantity of that pour'd forth* (which was therefore equal to that *abſorb'd by the Aſhes*) to weigh 1792 grains; which is pretty nearly that of the *bulk of 7 Cubical inches*. Now the Capacity of the Tube (its Diameter being ¾ of an inch, and its height 32) was about 14 cubical inches: So that the quantity of Water equal to about ½ the Content of the Tube, was drank up by the Aſhes.

I ſhall now take notice of the Particulars occurring in this Experiment, which ſeem to me to be well worth obſervation: And theſe I ſhall offer *here by themſelves*; intending to make ſome *general Remarks upon this whole Claſs of Experiments*, after I have related all the Experiments that belong thereto.

Firſt,

First, Tho' the Ashes were ramm'd so very close together, yet the *Interstices of them were capable of admitting a quantity of Water equal to half the Content of the Tube.* For the Content of the Tube was but little more than 14 cubical inches, and the bulk of Water absorb'd was as good as 7; as was but now observ'd.

Secondly, The *Progress of the Water* thro' the Ashes, was *very disproportional to the Times:* Because 'twas found, that in the equal Intervals of 24 hours, it made its way according to the *following Series*; *viz.* 16, 6, $4\frac{1}{4}$, 3, 2, (and in the last 10 hours) $\frac{1}{2}$ an inch.

Thirdly, The Force with which the Water made its Ascent, was very considerable; *being such as was sufficient to overcome the Resistance of the Air imprison'd in the Interstices of the Ashes, and to drive it away before it, towards the upper part of the Tube.*

Now 'tis plain the Resistance of the contain'd Air was not a very small one, from hence, that it was *superiour to that Force by which the Thorax is contracted, and the Air thrown out of the Lungs, in a strong Expiration.* Because when I endeavour'd to force Air by *my Breath*, thro' the Tube *not above half fill'd* with Ashes, I could not presently satisfie my self that I did do it: Whereas we see the Water easily made it self a Passage, when the Tube was not only quite full, but also the Ashes were press'd together, as hard and close as possible.

But to put it out of all doubt, that the ascending Water did actually meet with and overcome such a Resistance as what I speak of, *viz. That of Air lodg'd in the Interstices of the Body it pass'd thro'*; let it be observ'd in the next place, *Fourthly*, That *it was visible by the gradual Intumescence of the Bladder* at the top of the Tube, that the Air was really protruded out of the Ashes by the Water, as it ascended along.

I believe none will attribute *this Swelliug of the Bladder* to any other Cause than the force of some included Air, which stretch'd it, and plainly endeavour'd to get away by so doing. And that it was forc'd out of the Ashes by the Water, is as obvious as any thing can well be; since there could be no other possible Cause that should expel it at that time. And besides, in that it gather'd more and more in the Bladder, as the Water advanc'd higher and higher, by that means it plainly *pointed out the Cause* which forc'd it thither.

Fifthly, The Water rose not only in the *Ashes adjoyning to the inward surface of the Tube, but also thro' the whole body of it*, and that *equally* too, (as appear'd upon examination.)

Whatever therefore were the Cause of the Water's Ascent, *that Cause acted uniformly*, since the Water was in all parts and places *equally influenc'd* by it.

Sixthly, The Bulk of Air forc'd out of the Interstices of the Ashes, by the Water, we may conclude (and I think rightly) to be *equal to that of the Water which supplied its place*. And if so, then 'twas as much as half the Content of the Tube, or pretty nearly as much as the Bulk of Ashes therein contain'd, (as follows plainly enough from the *first Observation*.)

Seventh-

Seventhly, The *Aſcent of the Water was by far ſwifter, when there was a much-greater quantity of impriſon'd Air to oppoſe its paſſage, by reaſon of the longer Column of Aſhes* (in which that Air was contain'd) than when it had made more way, and (by *getting higher in the Tube, having ſhortned the Column of Aſhes*) had a *leſs quantity of Air* to reſiſt it in its motion upwards.

Query 1. Does not this Phenomenon of the Aſcent of the Water through the Interſtices of the Aſhes, *amount to the very ſame Caſe with that of its riſing in ſmall Tubes, or between two Glaſs Planes?* Do not the Particles of this Matter, by their little Hollows and Intervals, form a Congeries of *minute ſlender Pipes*, or *Surfaces very nearly approach'd to each other*; ſo that the Liquid riſes in each Caſe by vertue of *one and the ſame Cauſe?*

Qu. 2. Why is the Aſcent of the Water ſlower, the higher it riſes in the Tube?

'Tis evident, that at firſt there is more intercepted Air to be remov'd out of the way, than afterwards, when the Water has ſhortned the Column of Aſhes.

Is it therefore true, that the Water does at firſt actually meet with a more powerful Reſiſtance, and notwithſtanding riſes with more Velocity, than when 'tis leſs reſiſted? Or, ſhould we not rather conclude, that it does indeed meet with leſs Reſiſtance at firſt, than afterwards; and therefore, that *this intercepted Air is not in reality that Obſtacle, which at firſt ſight it appears to be?*

Qu. 3.

Qu. 3. Whether the *encreaſing Weight* of the Water, as it aſcends, may not be eſteem'd the Cauſe of the *diminution of its Velocity?* Becauſe, from *Statick* Principles, the ſame Power moving *different Weights*, ſhould produce different Rates of *Velocity*.

Experiment VI.

The Aſcent of Water thro' Aſhes in Vacuo.

HAving fill'd a Tube about 10 inches in length with Aſhes, (as before) it was plac'd in a *Receiver*, and the Air exhauſted. I ſuffer'd it to ſtand ſome time in *that ſtate*, to give liberty to the Air contain'd in the Aſhes to get away: Then plunging the lower end of the Tube under the Water, *I found* (according to my Expectation) *that the Water roſe much faſter in that very-much-rarefied Medium, than in the open Air*. Becauſe, in about 4 hours time, *it had mounted as high as it could go*, having compleatly reach'd the top of the Tube.

So that comparing the Reſult of this Trial with the former, we find that *Here* was a Height of 10 inches ſurmounted in 4 hours; whereas *There* 32 inches took up 130 hours to finiſh it.

By which Account it appears, that the *Heights* are in the proportion of $3\frac{1}{5}$ to 1, but the *Times* as $32\frac{1}{2}$ to 1. So that the Water was more than 32 times as long in going (in *Common Air*) a Space *triple* to that, which was finiſh'd in *Vacuo*.

But

But this Eſtimate of the ſwiftneſs of the Water's Aſcent *would have been more exact*, had it been obſerv'd, *at what Time* preciſely the Water reach'd the *ſame Height* in both Tubes. *Ex. gr.* As here *in Vacuo*, the Tube made uſe of was 10 inches long; ſo if it had been obſerv'd, in the *other Caſe*, at what time the Water had reach'd 10 inches in that Tube alſo, (*as it was obſerv'd at what time it roſe 16 inches, viz. at the end of* 24 *hours*) then the Proportions of theſe *different Times* in which the Water had aſcended to the *ſame Heights* in both Tubes, would have given a nearer account of the *Velocities*. For if the Motions were *equable*, the Velocities would be juſt *reciprocally*, as thoſe *Times*. But if they are not *uniform*, yet the rate of the Swiftneſſes may be more nearly gueſs'd at, *by taking the Liquid at the ſame Height in each Tube*, than at *different* Heights.

EXPERIMENT VII.

The Aſcent of Liquors in ſmall Tubes, of unequal Thickneſs, *but* equal Bores *or* Cavities.

HAving procur'd two Tubes, the Diameters of whoſe Cavities were as *nearly equal* as they could be made, but one at leaſt ten times as thick as the other; I put them into the 'forementioned *tinged Liquor*. The Reſult was, That *there was no difference to be perceiv'd between the Heights, the Liquor had aſcended to in each Tube.*

EXPERI-

Experiment VIII.

The Aſcent of various Liquors *between* two ſquare Glaſs Planes.

I Try'd this in *Spirit of Wine*, *Oyl of Turpentine*, and *Common Oyl*.

All theſe roſe between the Planes, as the *tinged Water* did. The Difference lay in this, that they all aſcended in *an entire Body*, from one ſide of the Planes to the other, without *thoſe Interruptions* and *Intervals*, which generally happen when the Water aſcends. And this, even tho' the Planes were held together *without any thing to ſeparate 'em*; and not only ſo, but alſo *forcibly preſs'd* together: in which caſe they muſt needs touch in many parts. And notwithſtanding *that*, the Courſe of the Liquor ſeem'd to be perfectly uninterrupted.

There was *a remarkable Difference between the Times ſpent* by the *Spirit of Wine*, and the *Oyls*, in their Aſcents.

The *Common Oyl* mov'd extreamly *ſluggiſhly*, in compariſon with the *Oyl of Turpentine* and *Spirit of Wine*; inſomuch that the *former* was near an *Hour* in riſing as high as the *two later* would do in leſs than *half a Minute*.

Having now given an Account of the Experiments themſelves, and ſubjoyn'd what *Remarks had a more immediate relation* to any of them in *particular*; I ſhall now make ſome *general Obſervations* upon the whole, and then conſider *how the Phenomenon it ſelf may be ſolv'd*.

Firſt,

First of all then, we find, that *this Phenomenon of the Ascent of Liquors*, (between the Surfaces of nearly-contiguous Bodies) *like that in small Tubes, does no way depend upon any action or influence of the Air.*

For in all these Trials, the Liquor rose with as much ease and freedom in an *exhausted Receiver*, as the *open Air*; but in *one* case particularly, it ascended with a vastly-greater velocity in so thin a Medium as *that we call a Vacuum*, than under all the Pressure and Vigorous Action of *common Air*. There was indeed some difference, with respect to *the branching and spreading* of the Liquor in its ascent; but this is a trivial consideration in comparison with what ought chiefly to be regarded in this matter, and that is the *Height* and *Force* of the Liquour; which *without* the Air, will be *at least* (to say no more than that) as considerable as *with it*.

Secondly; *Some Liquids rise after a manner very different from what others do.*

This is plain upon these two accounts.

1. Some Liquids, as they rise, *branch* themselves into *various little Streams or Rivulets*, and by that means leave (to all appearance) vacant spaces and intervals betwixt them; after which manner, 'twas observ'd before, that the *ting'd Water* rose between the Glass Planes. But others again mount up all in an entire body, from one side of the Planes to the other; as *common Oyl*, that of *Turpentine*, and Spirit of Wine.

2. Some ascend with *a prodigious swiftness*, in comparison of others.

Thus the *Two laſt mention'd* Liquors made at leaſt 120 *times as much haſte* to get up between the Planes, as the former did (as is plain from what was before related of them.)

And perhaps *other Liquors* may be diſcover'd, which may as much exceed *theſe* in the velocity of their Aſcent, as *they* did the *common Oyl.* And it may be, *all imaginable proportions of Velocity*, may be anſwer'd by thoſe of Liquors (of ſome ſort or other) *aſcending thus between the contiguous ſurfaces of Bodies, or in ſmall Tubes.* For the *Cauſe* of this Phenomenon (if it be what I take it to be) *is capable of producing an infinite diverſity of Effects*, according to the *difference of the Matter* it has to work upon.

Thirdly; *Liquids aſcend not only in perpendicular Directions, but in all imaginable Angles of Obliquity to the Horizon.*

For when the Experiment was made with the *round* Planes, the *tinged Liquor* immediately diffus'd it ſelf, to the extremities or edges of them, every where thro' the *whole Circumference.* Now the Liquid could riſe *perpendicularly* but in one Direction only, *viz.* that which we may conceive to paſs thro' the Center of the two contiguous circular Planes. In all the other Directions it muſt aſcend *obliquely*, diverging juſt as *an infinite number of Chords in a Circle, drawn from the end of the ſame Diameter.*

And ſuppoſing it reach'd *all parts* of the Circumference, *at the ſame time* (as it did without the leaſt difference to Senſe) we have then here as it were the Reverſe of *Galileo*'s famous Propoſition, about the *Equitemporaneous Deſcents* of heavy Bodies in the *Chords of a Circle*: For in *this* caſe, the *aſcending Li-*

quid

quid deſcribes them all in *equal times*, as in *that* caſe, the *deſcending Solid* does. And if the *one aſcends*, and the *other deſcends*, by virtue of one and the ſame Cauſe (as I cannot forbear thinking but they do); then 'tis no wonder that there ſhould be ſuch an Agreement betwixt them, and that the *ſame Cauſe* ſhould produce a *ſimilar Effect*, both in *Solids* and *Liquids*, when *ſimilar Circumſtances* are ſuppos'd on both ſides. And it all amounts to no more, than *Attraction* upwards in one caſe, and downwards in the other; and this in the *ſame ſort of Figure* too namely, a *Circle*.

Fourthly; This Phenomenon is not *confin'd to any one particular ſort of Matter*.

The Liquids roſe, not only between the *Glaſs*, but the *Marble* and the *Braſs* Planes too. And there's no doubt, but had the Experiment been made with Planes of various other ſorts of Matter, it might have ſucceeded in like manner. 'Tis poſſible, that *ſome Liquids* may not riſe between the ſurfaces of *ſome Bodies*, which others will riſe freely between: nay, I know not, whether *inſtead of riſing*, they may not be *ſunk and depreſs'd*. One and the ſame Cauſe, acting in *different* Circumſtances, is capable of producing a *great variety of Effects*.

Fifthly; A *greater quantity of Matter* contributes nothing to the *riſing of the Liquid*.

This is plain from the Experiment of the two Tubes of *equal Cavities*, but *unequal Thickneſſes*. And by a *parity of Reaſon*, the Thickneſs or Thinneſs of the *Planes* ſhould produce no alteration, with reſpect to the Liquor's aſcent between them.

Sixthly; The *Aſcent of the Liquour is favour'd* and *promoted* by ſmall Particles of Matter laid in its way.

Thus the Water *aſcended higheſt*, when the Planes were rubb'd over with *Wood-Aſhes*. Perhaps *other Matter* might be as great an *Impediment*; or *give more aſſiſtance to ſome Liquors in their Aſcent, than to others*. But theſe and many other things (*which now I can but hint*) may perhaps ſome time or other be propos'd as Subjects of further enquiry.

Having made theſe Obſervations (*General* and *Particular*) upon the ſeveral EXPERIMENTS propos'd, the next thing is the *Solution of the Phenomenon it ſelf*. And here I make no ſcruple, to reduce all the Varieties mention'd to the *ſimple Caſe of ſmall Tubes*; becauſe they all of them (as is plain by conſidering the Circumſtances) amount to no more than *That*.

For example: The two *Glaſs Planes* in theſe Experiments, being plac'd *very near* one-another, compoſe a Tube *of the Form of a Parallelipipid*, whoſe thickneſs is exceedingly *ſmall*. So that therefore, having found *a Solution for the Phenomena of ſmall Tubes*, the ſame may eaſily be accommodated to all the reſt.

To proceed then; It appears evident to me, that *the Principle we ought to have recourſe to in this Caſe, is no other than that of Attraction*.

A Principle which governs far and wide in Nature, and by which moſt of its Phenomena are explicable. I know very well there have been Attempts made, to ſolve *this Appearance* diverſe other ways. Some have argued from the *impeded* or *diminiſh'd Action of the Air*; others from the *Innixion or Reſting of* *the Parts of the Fluid, on the Pores and Aſperities of the Glaſs*; others again from the *Congruity and Incongruity of the Parts of Matter* one to another. This *laſt* Notion, without farther explication, is ſomewhat more unintelligible than the two former: And tho' perhaps they are all of them wrong, yet the *Firſt Two* ways of ſolving the Difficulty have this advantage above the other, that they are *perſpicuouſly* Falſe; whereas this latter is more *myſteriouſly* ſo, leaving the Underſtanding in ſome doubt, whether it may be True, or no; becauſe of the hard Words of *Congruity* and *Incongruity*, which being not explain'd, *may poſſibly* carry ſome better Meaning along with 'em than they ſeem to promiſe. If it ſhould be thought that *Attraction* is a Word no leſs hard and unintelligible than the former are, I can only ſay this, *That 'tis plain Fact that there is a Power in Nature, by which the Parts of Matter do tend to each other*; and that not only in the *larger Portions or Syſtems of Matter*, but alſo the *more minute* and *inſenſible Corpuſcles*. And that the Law which obtains in the Former Caſe (*viz.* amonſt the greater Bodies of the Univerſe) is *fully determin'd* and *ſettled*, namely, that the Attraction or *Centripetency decreaſes reciprocally, as the Squares of the Diſtances (*of the Attracted from the Attracting Body*) do encreaſe.* But the Law by which the *ſmaller Portions* of Matter tend to each other, is not ſo compleaty ſettled, but *left yet for further diſcovery*; only 'tis known, that it muſt be very different from the other, and *that the attractive Forces here do decreaſe in a greater proportion than that by which the Squares of the Diſtances do encreaſe:* but the nature of *that Proportion*, or how *complicate* it is, or what *Varieties* there may be in it, is not yet accounted for; nor will not eaſily, becauſe of the ſeeming *invincible Difficulties*

that

that attend the making Experiments and Obſervations requiſite to ſettle *ſo nice* a Point. Only the Fact it ſelf is paſt diſpute, and the Diſcoveries made by *that very great Man*, Sir ISAAC NEWTON, *(the Honour of our Nation and Royal Society)* have ſet *both theſe Laws of Attraction* thus far, in a very clear Light to all that will uſe their Eyes to ſee them.

Now ſince we are certain there is ſuch a Principle in Nature, and one ſo *Extenſive* and *Predominant* too, as *that of Attraction*; I think it would be a ſatisfactory Proof enough of the Intereſt of that Principle in this Phenomenon, to ſhew that it may be handſomly accounted for by it, without being forc'd upon any of thoſe *obſcure precarious Suppoſitions*, which in other Solutions a Man can't well avoid.

However, before I do this, I ſhall argue the Point another way, and by *ſhewing ſome remarkable Agreements of this Phenomenon, with others in which* Attraction *is moſt evidently concern'd*, do ſomething (I hope) to perſwade a belief of the Intereſt of the *ſame Cauſe* here alſo.

What I propoſe to conſider at preſent, is the *Magnet* or *Loadſtone*, ſome of whoſe Effects coincide with thoſe of *ſmall Tubes* to a Wonder.

1. A Loadſtone of *any Form* whatſoever, will attract *Iron*.

So we find that Bodies *ſet together after any manner*, or in *any figure* whatſoever, *ſo they do but compoſe a ſmall Tube* (or what is equivalent thereto) will give occaſion to the *Liquor to aſcend between their Surfaces*.

2. The Magnet exerts its force as well in *Vacuo*, as the *open Air*.

And

And we find that *Liquids riſe as freely in the one, as the other of theſe:* in the moſt *thin* and rarefied Medium, as well as the moſt groſs and *denſe.*

3. *Small Loadſtones* (for the generality) have a *ſtronger* attractive Power (in proportion to their bulk) than the *large* ones have: And ſo *ſmall Tubes* will make the Liquid aſcend higher than great ones will. And as the inward *Cavities* and *Surfaces are leſſen'd*, ſo the Liquid will riſe *higher* and *higher.*

4. If a Loadſtone be *divided into ſeveral parts*, or ſmall Loadſtones, *theſe little ones* (ſuppoſing the vertue of the Stone to be *equably* ſpread thro' the body of it) *will all together ſuſtain a vaſtly greater weight of Iron*, than the *one great* one alone before would do; tho' taken *collectively*, they contain very nearly the *ſame quantity of Matter* with it.

So, were a Tube of a *very ſmall Bore*, but a *great Thickneſs*, to be divided into ſeveral Tubes or *parallel Surfaces*, the Quantities of Water ſuſtain'd in all of them together, would vaſtly tranſcend *that*, which was ſuſtain'd when they were *all united together*, and compos'd but one ſingle Tube.

So that *in ſmall Tubes, as well as Loadſtones*, the Encreaſe of Superficies is That on which the Encreaſe of the Force ſeems mainly to depend. Nor *does this Aſſertion any way thwart* what was *ſaid before, viz.* That as the *Surfaces did decreaſe*, ſo the *Aſcent of the Liquid* would be more and more conſiderable: For *there* I only compar'd the Effects of Tubes of *different Diameters* one with another; and ſhew, that the leſſer Tube has the advantage of the greater: But *here* I compare the Effects of *innumerable little*

Tubes,

Tubes, all made out of one ſingle Tube, with the Effect of that ſingle Tube it ſelf; and ſhew, that the *Encreaſe of Superficies* conſequent upon ſuch a *diviſion*, gives the *Aggregate*, or Collection of Tubes, (by many degrees) the advantage of the *ſingle* one.

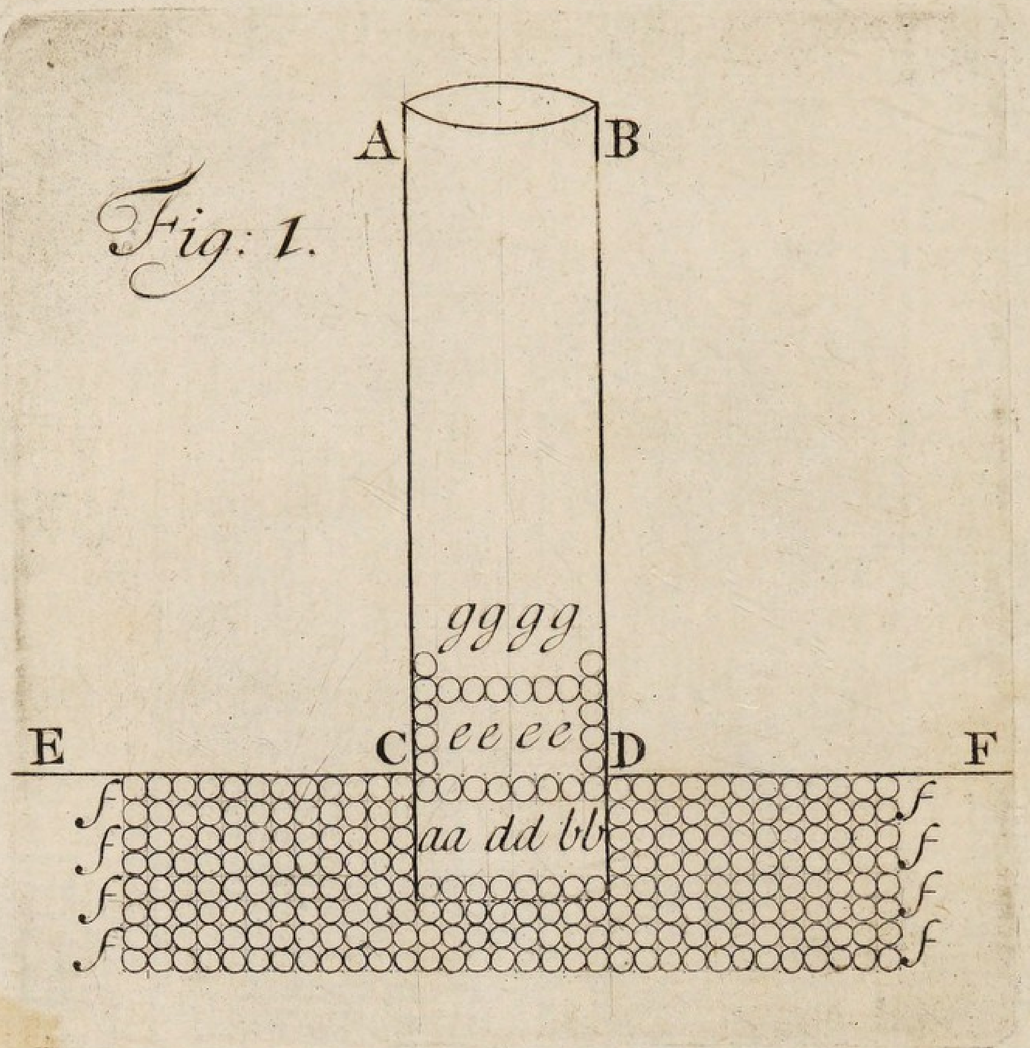
Now, upon theſe accounts, I think I have ſome Grounds to believe, that the Phenomena of the *Load-Stone*, and of *ſmall Tubes*, depend upon one and the ſame Principle in general: For here's a ſurprizing *Correſpondence of Effects*, why then may they not agree in their *Cauſe?*

But to proceed. That *Attraction* I ſpeak of (as the Cauſe of the Aſcent of Liquids in ſmall Tubes) I make to proceed (mainly, if not ſolely) from the *Innermoſt* or *Concave Surface* of ſuch a Tube; and not from the *Solidity* or *Quantity* of *Matter* which it contains. And a Proof that this is ſo in *Fact*, may be deduc'd from the *Experiment of the two Tubes*, before mention'd. But more than this, that it likewiſe *ought to be ſo*, appears from hence, that *the Attractive Power of ſmall Particles of Matter acts only on ſuch Corpuſcles as are in contact with them, or remov'd but at infinitely-little diſtances from 'em.*

Upon which account I think I may ſay, that the *remoter* Surfaces of the Tube, between the *innermoſt* or *concave* one, and the *outermoſt* or *convex* one, do contribute nothing to the Effect; that is, *the Liquid is not influenc'd by any Attraction* of Theirs.

Theſe things premis'd, let us in the next place conſider, how this *Phenomenon* may be accounted for by *Attraction:* That is, *how the Liquid may riſe in a ſmall Tube, by the Attraction of the Parts thereof by the concave Surface of the Glaſs.*

Let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Let A B C D be a *small* Tube, *perpendicularly* immers'd in a *Liquid*, whose *horizontal Surface* is E C D F.

The Parts of the Liquid at *a a*, *b b*, adjoyning to the concave Surface of the Tube, are *strongly attracted* by it, and that in a *Direction perpendicular* to the sides of the *Cylindrick* Glass; or (which is all one) *parallel* to E F, the Surface of the Liquid.

Now the Particles *a a*, *b b*, *gravitating* in Directions *perpendicular* to E F, that is, parallel to A C and B D, the *Sides* of the Tube; by means of the aforesaid Attraction, it comes to pass that the Particles *a a*, *b b*, have all of 'em a much-less *Momentum* or *gravitating Force* than otherwise they would have, were the Attraction *away*. Therefore *the Parts of the Fluid*, *which lie immediately under them*, are *much less press'd upon*, than otherwise they would be.

And altho' the Particles *d d* lie farther out *towards the middle* of the Tube, yet in a very minute and slender one (such as we here speak of) they are near enough to be within the reach of the *powerful Attraction* of the Surface, so far as to be in some measure influenc'd thereby; either *immediately* or *mediately*, by the means of the Particles *a a*, *b b*, which are strongly urg'd towards the Glass, and do *(by the General Law)* attract the neighbouring Particles *d d*, towards themselves.

Upon these accounts, the *Momenta* of all those Particles, *comprehended within the Circumference of the lower Basis of the Tube*, being much lessen'd; the Fluid, which lies directly under 'em, is proportionally *less press'd*.

But the Parts of the Fluid *f f f f*, which lie between the Surface E C D F and the Bottom of the Tube, *at more remote diſtances from the Sides of the Tube* than its *Semidiameter* ; Theſe Particles (I ſay) being out of the reach of any ſuch Attraction, *do gravitate with their whole Force* or *Momentum* on the Parts which lie under 'em. Therefore it appears, that *by the Immerſion of the ſmall Tube into the Liquid*, the *Equilibrium* is deſtroy'd between thoſe Parts of the Liquid *lying within the Circumference* of the lower Baſis, and thoſe which are plac'd *without*. Therefore (by the *Hydroſtatical* Laws) the Liquid muſt riſe within the Surface of the Tube: For the *ſtronger Fluid* will ſtill *preſs in* upon the *weaker*, and *force it away* before it. That is, the Particles about *a a*, *b b principally*; and *next to them*, the Particles about *d d* muſt neceſſarily give way to the Particles below them, which are urg'd on by the *ſuperiour Momentum* of the Particles which come from the aforeſaid *remote diſtances* about *f f f f*. From whence it follows, that thoſe Particles about *a a*, *b b*, muſt neceſſarily aſcend higher in the Tube, as to *e e e e*.

When they are riſen higher, *the Attraction to the Sides of the Tube will take place as before*, and by leſſening their *Momenta*, with reſpect to thoſe *below* 'em, will give a new Occaſion to the *external* Fluid, to inſinuate it ſelf within the Bottom of the little Tube, and conſequently to puſh thoſe Particles ſtill up higher, as to *g g g g*.

Thus, *by the continued Action of the ſame Cauſe*, the ſame *Effect* follows, and the Liquor continues to aſcend in the Tube, till it comes to a *certain determinate Height*, where *it keeps its ſtation*, and that by vertue of the *ſame Laws which brought it thither*.

And

And this, I hope, may be a ſatisfactory Account of the Reaſon of this Phenomenon; and ſerve to ſhew the Manner and Way by which Liquids may be conceiv'd to riſe in ſmall Tubes. I muſt ſay, *it appears to me to be the True one.*

NB. I ſpake but now (with a *Particular remark*) of the Preſſure of thoſe Particles of the Fluid, which lie at more remote diſtances from the Tube, than its *Semi-diameter.* 'Tis to the *Energy and Force* of theſe, that the Aſcent of the Fluid ſeems *chiefly*, if not *entirely*, to be owing. For thoſe Particles nearly adjoyning to the *Convex* Surface, are attracted in ſome degree, as well as thoſe which are approach'd to the *Concave*; And therefore can't be imagin'd to have any ſuch *preponderating Momentum*, as to force thoſe within to aſcend in the Tube. But ſuch Particles as are plac'd at farther diſtances beyond the Convex Surface, ſuffer no attraction from it, and ſo are ſufficient to *preſs the Liquid away before them.*

But it may be ſaid here, that if the ſurrounding parts of the Fluid *without*, on the *Convex* ſide, were attracted as well as thoſe *within*, on the *Concave*; then the Fluid ought (by theſe Principles) *to riſe without*, on the Convex ſurface of the Tube, as well as it does *within*, on the *Concave:* which Experience ſhews that it does not.

But (in *anſwer* to this) the Reaſon *why it ought not to do ſo* (or at leaſt *very inconſiderably*) is obvious, I think, *from the different natures of* Convexity *and* Concavity. Suppoſe we a ſmall Particle of a Fluid, in *contact* with the *Convex* Surface of a Tube: 'Tis plain, that all the *Lineolæ* or *ſtreight Filaments* compoſing this Surface, are *averted*, or *turn'd from* the aforeſaid Particle, except that one ſingle Filament,

 in

in which it touches the Surface. But on the *Concave* side, all the Filaments are *turn'd towards* such a Particle, which we imagine to be in *contact* with the Surface there.

From whence arises an exceeding great Difference between the Attractions of a Particle of Fluid Matter, by the Filaments on the *Convex*, and on the *Concave* side.

For in the *Former* Case, the said Particle must be vastly more *out of the reach of these Attractions*, upon the account of the *averted Position* of the Filaments, than in the *Latter* Case, *where the Incurvation turns the attracting Lineolæ towards it*, and by that means presenting a *much-greater Force*, produces a proportionally-greater Effect.

And for this reason, the Parts of the Fluid *within*, losing beyond comparison more of their *Momentum* than those *without* do; the Fluid ought to rise beyond comparison more on the *Concave*, than on the *Convex* Surface of the Tube. That is, its ascent on the *outside* ought to be *scarce sensible*; and I believe it will be always found to be so.

Thus far we have shewn the Reason why the Liquor must rise in the *small* Tubes.

From hence now it follows likewise, that *it must of necessity rise higher in Tubes of a smaller, than those of a larger Bore.*

For suppose there be *two Forces*, each of which is to lift a *several Weight*. Now (from the Principles of *Mechanicks) that Force which bears the greatest Proportion to its Weight, will be able to raise that Weight higher, than that Force which bears a less Proportion to its Weight, will be able to raise that Weight.* Let us also take two Tubes of *different diameters*, and the *same height:*

height: the *attractive Powers are as the* Surfaces; and the *Weights* or Quantities of Liquor to be rais'd into these *two Cylinders*, are as their *ſolid Contents*. Or (becauſe the Heights being the ſame, the *Surfaces* are as the *Peripheries*; and the *Contents*, as the *Areas* of the *Baſes*;) the *Attractive Forces* will be as the *Peripheries*; and the *Weights*, as the *Areas* of the Baſes.

But there is a *greater Proportion* between the *Peripherie* and the *Area* of the Baſe in the *ſmall* Cylinder, than there is between the like quantities in the *great* one. Therefore in the *ſmall* Tube, the *Attractive Force* bears a *greater Proportion* to the *Weight of the Liquid to be rais'd*, than it does in the *great* one: And therefore *the Liquid muſt riſe higher* in the *Former*, than in the *Later*.

From hence likewiſe we may form *a Rule*, *that may help towards determining the Height the Liquid muſt aſcend to*, *in any given ſmall Tube*.

For the Liquid muſt neceſſarily riſe, till it comes to ſuch a Height; *that the Momentum of all the Liquid in the Tube*, *as it is there diminiſh'd by the Attraction of the Surface*, becomes equal to the *undiminiſh'd Momentum* of the *External* Liquid, at *that depth* the Tube is immers'd to. And when it is come to that *particular Height*, it muſt as neceſſarily ſtand, and go no farther.

And in that Caſe, the Proportion will run thus— As the *Diminiſh'd Gravity* of the Liquid *in the Tube* is to the Abſolute Gravity of the *Collateral Cylinder* of External Liquid, ſo is the *Depth of Immerſion*, to the *Height of the Liquid in the ſmall Tube*. For, I ſuppoſe the Cylinder of Fluid *in the Tube* to be balanc'd by one *without*, which has the *ſame* Baſe, and whoſe *Height* is equal to the *Immerſion*; for the Baſes being the ſame, the Heights are as the *Contents* or

Quan-

Quantities of Matter. And to make an *Equilibrium*, or *Equality of Momenta*, the Forces muſt be *reciprocally*, as the *Bulks* or *Quantities*; that is (in this caſe) reciprocally as the *Heights*.

Now, as for the Reaſons inducing me to propoſe a Solution of the Phenomena of *Capillary* Tubes, *on ſuch Principles as I have here done*; I think it not improper here to ſubjoyn them.

'Tis true, the *direct* and *ſeemingly-ſtreight* aſcent of the Liquid, from the *lower* to the *upper* parts of the Tube, would, at firſt view, tempt one to think of no more than only *an Attraction upwards*, or in Directions *parallel* to the ſides of the Tube, inſtead of *perpendicular* thereto; and ſo to derive the Fluid's aſcent *only from this*, without any regard to *Hydroſtratical* Laws, or the *loſs and recovery of an Equilibrium*. But in this way there appear'd ſeveral Difficulties, of that ſtrength, that I could by no means get over them; and the conſideration of theſe, determin'd me to ſolve the Phenomenon the *other way*.

For, 1. I could not ſee any Reaſon to convince my ſelf, why a Particle of the Liquid, as *a* or *b*, which is there at *that point* in *actual contact* with the Glaſs, ſhould not be attracted to and by *that Particle*, rather than by another *above* it, and *remote* from it: Or at leaſt why it ſhould not be *vaſtly-more* attracted by that Particle with which it *is in contact*, than by another above it; and conſequently why the Attraction ſhould not *firſt begin*, in lines *perpendicular*, and not *parallel*, to the ſides of the Tube, whatever be done afterwards: For I do not abſolutely exclude the *former* Attraction from being ſome way concern'd, tho' I make the *latter* the main and *principal Cauſe* of the Aſcent.

2. If

2. If a Body reſting on an *Horizontal Plane* were to be drawn (in a Direction parallel thereto) *againſt an Upright or Vertical Plane* erected upon the ſame Horizontal Plane; it appear'd certain to me, that the *actual Preſſure* of that Body in the *Horizontal* Plane would be diminiſh'd, according to the force wherewith the *Thread* was drawn, *which preſs'd it againſt the vertical Plane.* For it may be drawn ſo hard againſt the upright Plane, that the Horizontal one ſhall have little or nothing at all of the burden of it. And applying this to the Caſe in hand; I concluded, that for this reaſon the Momentum of the Parts of the Fluid *a a, b b*, muſt be abated, with reſpect to the Particles lying immediately under them.

3. When I ſuppos'd the Liquor to aſcend purely by vertue of the Attraction *directly upwards*, in Lines *parallel to the ſides* of the Tube, I could not ſee a reaſon, *why the Liquid ſhould ever ſtop in any Capillary Tube* before it comes to the *very top of all*; which yet *Experience* ſhews it does. For if the Attraction be ſolely from the upper parts, then as long as there is any part of the Surface left unoccupied by the Liquid, *ſo long there is a Cauſe left in being, of the Liquids farther aſcent.* And if there be that Cauſe in being, why ſhould it not exert it ſelf, and make the Liquid riſe, as well when 'tis gotten up 2 or 3 inches, high in the Tube, as when it was below at the bottom? All *Circumſtances here*, are the ſame as *there*, as far as I can with all my Attention diſcover. And that the Liquid *has already poſſeſs'd* ſome part of the Surface, can be no Reaſon why *that part which is not poſſeſs'd*, ſhould not exert its attractive force, and draw up the Liquid, till it is gone as high as it can go; that is, till the whole Tube is full. In a

word,

word, Why ſhould I deny the *upper part* of the Tube that attractive power, which I ſo *freely allow to the lower?* I know no reaſon, *to imagine this* Vertue *to lie ſo unequally ſcatter'd about the Tube*, and if it be not ſo, then I think I ought to expect the ſame Effect ſhould take place in the upper parts of it, that does in all other places.

And therefore, 4. By the Explication which I have here given of this Phenomenon, I found I could give a Reaſon *for the Aſcent of the Fluid to a determinate Height in any Capillary Tube propos'd*; which I ſaw no way, that was *tolerably Philoſophical*, to do by the *other Method*.

For the *Momentum of the external Liquid* being in ſome meaſure taken into this Account, as well as the *Attraction of the Tube*; the *Hydroſtratical* Laws, by which I ſuppoſe the Liquids *within* and *without* to be *balanc'd*, will themſelves *determine the Height to which the Fluid muſt riſe in the Tube*. Otherwiſe (as I hinted before) 'tis not a ſlight Difficulty, to conceive any other Limits to bound the motion of the Liquor upwards, but the very top of the Tube it ſelf: and there indeed it muſt ſtand for a *good Reaſon*. But if it ſettles at any determinate depth *below that*, and this by vertue of the *direct* or *upright* Attractions, I muſt needs ſay 'tis a Riddle, out of the Intricacies of which I have little hope to deliver my ſelf.

For the remaining part of the *Concave* Surface lying above the Liquor in the Tube; either has, or has *not*, the Power of Attraction, like the other parts of the *Surface below*.

If not, *by what Law is a Tube ſo divided* into *attractive* and *non-attractive* Segments? How is the Limit between theſe two very Heterogeneous Parts determin'd?

Was

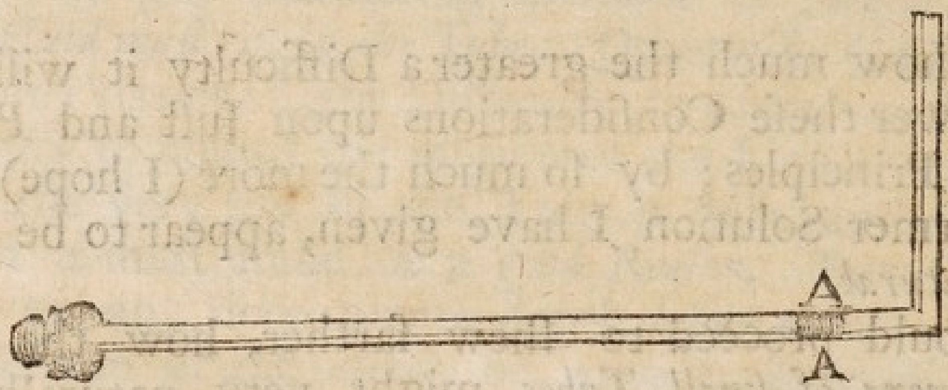
Was there *originally* and *always* ſuch a diſtinction? Or did it commence when the Glaſs was firſt *blown* *to the Form* of a Capillary Tube? Or did it begin to take place only when the *Tube was actually im-mers'd* in the Liquid? In ſhort, does this ſtrange pro-perty owe its riſe to *Nature*, or to the *Fire*; the laſt which made it a Tube, or to the *Water 'tis plung'd in*, when the Experiment is to be perform'd? If it be not to one of theſe, I am at a loſs for its original. On the other hand; if the remaining part of the Tube, *above the Surface* of the Liquor where it *ſettles*, be endow'd with an attractive force *ſimi-larly* and *proportionally* to the reſt, why does it not exert it ſelf, *when the Liquor* is fairly preſented *with-in the Sphere of it*; in like manner as the attracti-on of the other *parts below* did, *when the Fluid was brought within their Sphere?*

By how much the greater a Difficulty it will be, to anſwer theſe Conſiderations upon Juſt and *Philo-ſophick* Principles; by ſo much the more (I hope) will the former Solution I have given, appear to be *clear* and *natural*.

I could proceed to ſhew farther, how the *other Phenomena of ſmall Tubes* might very naturally be ſolv'd from *theſe Principles*; but it being *very eaſie* to make *that Application*, I ſhall wave all Diſcourſe of thoſe matters, and with that I have now ſaid, con-clude this preſent Subject.

An Account of an Experiment concerning the different Denſities of the Air, *from the* Greateſt *degree of* Heat, *to* that *of* Cold, *in our* Climate.

I Took a Glaſs Tube about 2 Foot *long*, and of about $\frac{1}{5}$ in *diameter*, which I bent into the form of a *Rectangular Syphon*, at nearly the diſtance of 6 inches from one end, thus:



At that Extremity of it which was *fartheſt* from the *Angle*, I *cemented* on a *Braſs Screw* with a ſmall Perforation in it; by which means, when I had put a little *Quickſilver* in at the *ſhorter* Leg, by inclining the *longer* Leg *this* or *that* way, I could bring the *Mercury* to reſt any where at pleaſure; as at A A. The little Column of Mercury I here made uſe of, was about $\frac{1}{2}$ inch in length, as being ſufficient for the purpoſe of this Experiment. Then having ſcrew'd a *Cap* on to the longer

longer or *Horizontal* Leg of the *Syphon*, and the Mercury being fix'd and ſettl'd in a *certain* part; I convey'd it into a *Trough*, together with a *Thermometer*, and pour'd on as much *warm* Water as would cover the *Ball* of the Thermometer: Thus the *Syphon* lay, with its longer Leg *under* the Water, in an *exact Horizontal Poſition*, and its *ſhorter* Leg *upright* and *above* the Water. And the Reſult of this Contrivance, was the Prevention of ſome Inconveniencies, which would have follow'd upon placing the Syphon ſo, as that the Water ſhould have found its way into it. Beſides, the Action and Power of the *External Air* could not have been ſo certainly argued and determin'd, in *that* caſe, as it might in *this*; where it had liberty to preſs as it would, and that *Immediately*, without oppoſition or hinderance from an *Intervening Body*. The *Spirit* in the *Thermometer*, being quickly influenc'd by the *warm* Water, I ſuffer'd it to riſe up as high as the *little Ball* on the Top of that *Inſtrument*, and indeed to paſs into it; that ſo I might make my *Obſervations on its Deſcent* with more exactneſs. For I imagin'd, that by that time the Spirit was fallen to ſome *convenient* degree, deſign'd to begin the Account at, it might have acquir'd a pretty equal degree of Heat in all its Parts.

Accordingly I began my Obſervations, when it had deſcended to 130 Degrees above the Freezing Point; at which time I found the length of the Column of Air, from the cloſed end of the Syphon to the neareſt Surface of the Quickſilver, to be juſt 144 tenths of Inches. After the Spirit had deſcended 10 Degrees lower, the Air, which before poſſeſs'd 144 Parts, lack'd one of them now; and ſo on ſucceſſively at every 10 Degrees deſcent of the Spirit, the Column of the contain'd Air was leſſen'd in its length one exact tenth. When it had deſcended to 30 Degrees above the Freezing Point, the Air was found to poſſeſs but 134 of the 'forementioned Parts. So that from hence it will be eaſie to conclude, that at the Freezing Point, the Air in the Syphon would be reduc'd to 3 tenths leſs than at the laſt Obſervation; and conſequently at 50 Degrees below the Freezing Point, (which I am inform'd is the greateſt Degree of Cold that has happen'd in our Climate,) it would be reduc'd to 126 Parts of the whole, and in that ſtate would be one *eighth* more denſe, than when at the greateſt Degree of our Natural Heat. And the Reaſon why I could not prove this later part by Experiment, was, that when I came to expoſe the Thermometer and Syphon in the open Air, or freezing Mixture, the Syphon would inſtantly receive the Impreſſion of the Cold, and the Air contain'd in it be conſiderably contracted, before the Thermometer gave any ſign of ſuch Alteration. But ſeeing the former part of the Experiment ſucceeded ſo very regularly as it did, I think there can be no doubt of the truth of the whole Calculation; nor do I yet ſee how it could be better perform'd. I ſhall add a Table of the different Degrees of the Air's Denſity at every 10 Degrees, from 130 above the Freezing Point, to 50 Degrees below it.

This Experiment was made *February* the 11th, 1708; the Mercury in the Barometer (at the ſame time) ſtanding at 30 inches.

	Degrees.	Parts.	
Above.	130	144	
	120	143	$\frac{1}{144}$
	110	142	$\frac{1}{72}$
	100	141	$\frac{1}{48}$
	90	140	$\frac{1}{38}$
	80	139	$\frac{1}{28,8}$
	70	138	$\frac{1}{24}$
	60	137	$\frac{1}{20,5}$
	50	136	$\frac{1}{18}$
	40	135	$\frac{1}{16}$
	30	134	$\frac{1}{14,4}$
	20	133	$\frac{1}{13,09}$
	10	132	$\frac{1}{12}$
Freezing Point.	0	131	$\frac{1}{11,08}$
Below.	10	130	$\frac{1}{10,3}$
	20	129	$\frac{1}{9,6}$
	30	128	$\frac{1}{9}$
	40	127	$\frac{1}{8,4}$
	50	126	$\frac{1}{8}$

This Table shews the Difference of the Air's Density at every 10 Degrees, from 130 above the Freezing Point, to 50 below it. As, supposing the Spirit in the Thermometer should stand at 40 Degrees above the Freezing Point; I find right against it, in the third Column $\frac{1}{16}$: Its State being then so much more dense than when the Spirit is elevated to 130 Degrees. And so of all the rest.

The second Column shews the Extent of the Air at the several Stations, from the Greatest Heat, to the Greatest Cold.

NB.

NB. For the better underſtanding of the foregoing Experiment, I would note ſomething concerning the *Poſition* and *Motion* of the little Column of *Mercury* in the *Horizontal* Leg of the *Syphon.*

This body of Quickſilver being fix'd in a certain part of the Tube (as ſuppoſe *thereabouts*, where it *now appears* to be, in the *Fig.*) was afterwards, by the Rarefaction of the Air (contain'd between it, and the *end cover'd with the Cap*) driven farther towards the *Angle of the Syphon* ; for the Mercury when *put in*, neceſſarily *forcing the Air along* before it, there muſt needs be a Column of Air, *included between it* and the *end of the Syphon* guarded with the braſs Cap: and *that Air* muſt as *neceſſarily be rarefied* by the heat of the *warm Water*, and *that Rarefaction or Expanſion* will force the Mercury towards the Angle of the Syphon, where it has only the Preſſure of the External Air (thro' the *open ſhorter Leg* of the Syphon) to encounter with, as an *Impediment* to its motion that way. Now the *firſt Expanſions* of the *included* Air, by the *Heat*, are ſufficient to overcome the contrary Preſſure of the *External Atmoſphere :* And by this means the Column of Mercury is preſs'd towards the *Angle of the Syphon* ſo far, till the Rarefaction and the outward Preſſure come to *balance* one-another. Then, as the Water *cools*, (and the Heat growing *leſs*, the expanſive Force of the included Air by conſequence abates too) the Preſſure of the Atmoſphere thro' the ſhorter Leg of the Syphon begins to prevail, and conſequently forces the Mercury more inwards, or *farther from the Angle of the Syphon.* And thus the Rarefaction ſtill diminiſhing, and the weight of the Atmoſphere gaining more and more upon it, the Mercury is ſtill driven farther from the Angle of the Syphon, and ſo the length

or

or diſtance between it and the *end cover'd with the Cap*, becomes continually leſs and leſs. And *theſe Diſtances* are expreſs'd in that *Column of Parts* in the *Table*, which anſwer to the Degrees of the *Spirit's* deſcent in the *Thermometer*.

This Experiment it ſelf proves the Spring of the Air, as it ſhews us the *External* Atmoſpheric Column firſt *giving way* to the more *powerful* Expanſion of the *included* Air, and then by degrees *recovering it ſelf*, and forcing the Mercury away before it, towards the other end of the Tube.

We ſee likewiſe the Ground *gotten* by the *External* (which is the ſame with that *loſt* by the *included* Air) to anſwer exactly to *the Abatements of Heat* indicated by the *deſcending* Spirits in the *Thermometer*; ſo that the Liquor *there* always gave an exact and perfect account of the *contraction* or *ſhortning* of the *Column of included Air*; that is, of its *Denſity*.

The *abundant Uſefulneſs* of *which Obſervation*, I may ſome time or other more largely diſcourſe of.

EXPERIMENTS
Concerning the Refraction *of the* Air.

ABout Ten Years ſince, that Curious and Ingenious Member of the ROYAL SOCIETY, Mr. JOHN LOWTHORP, contriv'd an *Apparatus* to demonſtrate ſenſibly the *Refractions* of the *Air*, which hitherto had been perceiv'd only by the ſubtile and nice Diviſions of Aſtronomical Inſtruments. He made a *Vacuum* between two inclined Planes of Glaſs by by the help of *Quickſilver*; thro' which an Object view'd with a *Teleſcope* was ſeen, upon readmiſſion of the Air, very ſenſibly to change place: An Account of which Experiment is at large in *Phil. Tranſ.* N° 257, and in the *French Memoirs* for the Year 1700, to which I referr.

Mr. *Caſſini* the Son having been preſent when Mr. *Lowthorp* made his Experiment before the *Royal Society*, made a Report thereof to the *Royal Academy* of Sciences of *France*; and, upon his return home, thoſe Sçavans thought it worth their while to re-examin the matter: But tho' themſelves thought it very ſtrange, yet, as they manag'd the Trial, they declare in the Hiſtory of their Academy for the Year 1700, That *it did not ſucceed*; and, that the Beams of Light paſſing thro' ſuch a *Vacuum*, ſuffer'd no alteration by Refraction.

How well they made their *Vacuum*, tho' they ſay it was *bien exactement*, may juſtly be queſtion'd; or rather, the thing being ſo evident, it will not ſeem malicious, if we ſuppoſe ſome little Senſe of Emulation might incline them to deny the Honour of an Improvement of ſuch Conſequence to *Aſtronomy*, to a foreign *Academy*.

The *Royal Society* (whoſe Glory it is to be as unwilling to deceive as to be deceived) being inform'd that this Experiment was call'd in queſtion by the *French Academy*, were deſirous that it might be put paſt diſpute, by repeated and fully-atteſted Trials: Accordingly I was order'd to make an Inſtrument for the purpoſe, by the direction of Mr. *Halley*, R. S. S. and Profeſſor of Geometry in *Oxford*. It conſiſted in a ſtrong Priſm of Braſs, two ſides of which had Sockets, to receive Glaſſes as truly plane and poliſh'd as could be gotten; and the third ſide had a Pipe

with

with a Stop-cock, whereby to apply both the Exhausting and Condensing Engine. The Glasses were firmly fix'd and cemented in, so as to bear both an inward and outward Pressure, and the whole turn'd upon an Axis, that it might be made to receive the Rays with any Obliquity desir'd: And, to be the more secure, I affix'd a Mercurial Gage, to discover any the least Defect in the Cement, that might happen; the Angle contain'd between the two Glass Planes being very near to 64 degrees. And this Instrument, thus prepar'd, we fitted to a Telescope of about 10 foot long, so as the Axis of the Telescope might pass thro' the middle of the Prism; and in the *Focus* of the *Telescope* a very fine Hair was adapted, for direction to the Sight.

Having chosen a proper and very distinct erect Object, whose distance was 2588 feet, *June* 15. *S.V.* 1708, in the Morning, (the *Barometer* being then at 29.7½, and the *Thermometer* at 60) we first exhausted the Prism, and then applying it to the *Telescope*, the horizontal Hair in the *Focus* cover'd a Mark on our Object distinctly seen thro' the *Vacuum*, the two Glasses being equally inclin'd to the visual Ray: Then admitting the Air into the Prism, the Object was seen to rise above the Hair gradually, as the Air entred, and in the end the Hair was found to hide a Mark 10¾ inches below the former Mark. This often repeated, as often succeeded.

This done, we applied the Condensing Engine to the Prism, and having pump'd in another Atmosphere, so that the Density of the included Air was, by the Mercurial Gage, double to that of the outward; we again plac'd it before the *Telescope*, and then letting out the Air by the Cock, the Object, which before ſeem'd to riſe, now appear'd gradually to deſcend, and the Hair at length reſted on an Object higher than before by the ſame Interval of $10\frac{1}{4}$ inches. And this likewiſe often repeated, never fail'd.

We again crouded in another Atmoſphere, and upon diſcharging the condens'd Air, the Object was ſeen near 21 inches lower than the Hair; but in this, the great Preſſure forcing the Cement would not permit us to make ſo frequent Repetitions as in the former.

And theſe Experiments have been ſhewn before the Preſident; and, at times, to moſt of the principal Members of the *Royal Society*. So that 'tis hoped the Fact may no longer be queſtion'd.

Now the *Radius* being 2588 feet, ten inches and a quarter ſubtend an Angle of one minute and eight ſeconds; and the Incidence of the Viſual Ray being 32 Degrees, by reaſon the Angle of the Glaſs Planes was 64 Degrees, it follows, from the known Laws of Refraction, that as Sine of 32° to S. of $31^{\circ}.59'.26''$, ſo Sine of any other Incidence to the Sine of its refracted Angle; and ſo is *Radius*, or 1000000, to 999736; the Logarithm of which *Ratio* is —0001145: whence the Refraction of the Air may readily be computed at any other Angle of Incidence.

By theſe Experiments it plainly appear'd, that the Refraction of the Air was, as far as the Eye could diſtinguiſh it, exactly proportion'd to its Denſity; the Refraction being the ſame from the common Air to a *Vacuum*, as from a double Denſity to the com-

common Air, and the Refraction from a treble Density to the common Air exactly double to that from the common Air to a *Vacuum*. Whence the Density of the Air, in respect of the incumbent Atmosphere, being always as the height of the Mercury in the Barometer, the Refraction also will be *cæteris paribus* in the direct proportion of the heights of the Mercury.

But this Density of the lower Air is considerably varied by Heat and Cold, as appears by the Table, in *page* 173; wherein we have shewn by Experiment, that the same Air which when the Thermometer marked 130 degrees, (being the greatest Summer Heat) occupied 144 spaces, by extremity of Cold, or at 50 degrees below the freezing point, was reduced to 126 of the same spaces; but at the freezing point to 131: it being very remarkable that the Air and Spirit of Wine did proportionable contract themselves during the whole Experiment. Hence by help of the aforesaid Table, we are enabled to give a rule to estimate the Refraction of the Air at all times; having the height of the Barometer and Thermometer: for with the same Heat, the Refraction is as the height of the Barometer directly, and under the same Pressure, it is as the spaces the same Air occupies reciprocally.

Now our Experiment being made when the Mercury was at 29, 7½, and the Thermometer at 60, which gives the space in the Table 137; let it, for Example sake, be required to find what would be the Refraction when the Barometer is at 29 inches, and the Thermometer at the Freezing point, or the Air occupying but 131 parts. I say, the Density of the Air at such time will be to the Density at the time of our Observation, as 137 times 29 to 131 times 29, 7½,

 that

that is as 15892 to 15589; wherefore the Refraction of the Air, at ſuch time, will be in that ſame *Ratio* to what it was *June* 15, 1708. How to apply this rule to Aſtronomical purpoſes, and how to correct the Errors occaſioned by the Air's Refraction, in the Obſervations of the Stars, will ſhortly be ſet forth in a more proper place.

An Account of an Experiment concerning the different *Weights of the* ſame ſorts *of Bodies, but of very unequal Surfaces, in* Water, *which were of* equal *Weight in Common Air.*

IT is very well known, by many Experiments, that the minute Parts of Bodies, which are *ſpecifically* heavier than ſome *Menſtruums*, may, notwithſtanding their *exceſs of Gravity*, be ſuſpended and held up therein.

This is ſeen in the *Diſſolution* of *Gold* in *Aqua Regia*, and of *Silver* in *Aqua Fortis*, and many other *Chymical* Experiments beſides.

Now theſe Phenomena have hitherto been us'd to be ſolv'd from the conſideration of the great *encreaſe of Superficies* (in ſmall bodies) in proportion to their *bulk*. For *theſe Metals*, or other Bodies, (ſay thoſe who go upon this Hypotheſis) being divided into extreamly-*minute* parts, by the action of the *Menſtrua*, a vaſt *encreaſe of Superficies*, in proportion to the *bulk* or *weight*, is an immediate Conſequent thereupon.

And

And the Refiftance from the Liquid, being greater or lefs, according to the Superficies, it comes to pafs that Particles of Matter, fpecifically heavier than a Fluid propos'd, may by that great excefs of *refiftance* above their *gravity*, come to be *fufpended* and *float* therein.

Now from hence 'twas eafie to infer, that if this was the reafon of the Phenomenon, fomething of this mighty difference muft needs appear *by weighing equal quantities of Matter*, and therefore *equally heavy*, but of very *unequal Superficies*, in *Water*, or fome other *Liquid*; and then feeing how much the one exceeded the other in weight there. Accordingly I took a piece of *Sheet-Brafs* of an exact *inch fquare*, and in weight juft 482 *grains*. I then cut as many Square inches of Brafs *Tinfel*, as were equal in *weight* to the former, *viz.* 482 grains; and thefe pieces were 255 in *number*. Now here being fo very great a difference of Superficies, I concluded there would be fome very confiderable difference found, arifing upon the weighing of thefe Materials in Water. But to my great furprize (being indeed prepoffefs'd *in favour of the common Opinion*) *I found but two grains difference*; the *fingle piece* weighing in the Water about 422 grains, and the other *feparate* ones hardly two grains lefs. And this, upon two or three repeated Trials, (made with all the caution imaginable) *fucceeded much the fame*; fo that the difference is not worth mentioning. Now here the proportions of the Surfaces were as 1 to 255 (for I reckon the fides of all the *Tinfel-Laminæ* to be equal to the fides of the *fingle Brafs-Lamina*) and notwithftanding that in one cafe there was 254 times *more Superficies*, than in the other; yet there was fcarce a $\frac{1}{211}$ part *less weight* with all that Superficies, than with the former; which decreement of Weight may, without scruple, be attributed to some small Bubbles of Air, which adhered to them unperceiv'd.

And from hence I am strongly induc'd to conclude, that some other Cause must be found out to solve this Phenomenon by, since the disproportion between Superficies, and Bulk or Weight of Matter is not sufficient to do it. For suppose a small *Metallick* Particle, or one of some *other* Body *specifically heavier* than a *Liquid*; and suppose this to sink by its own weight, if put into this Liquid. According to the foregoing Experiment, tho' this Particle were *divided* so, as to have 254 *times more Superficies* than now it has, yet its loss of Weight would be so inconsiderable, that no *suspension in the Liquid were to be expected from thence.* And there is a pretty remarkable Confirmation of this, to be drawn from an *Experiment* I made with the *Powder of Fine Flint* Glass. This Glass made use of, was of that sort which is of all others the clearest and *freest* of *Blebs.* Farther; to have the parts of the Glass as *minute* as well might be, after it was reduc'd *to Powder*, I pass'd it thro' a *Lawn Sieve.* And that there might be no Errour arising from the want *of a just Quantity of Matter*, to make the Trial with; I weigh'd an Ounce of this fine Powder against the like quantity of *solid Glass.*

And here likewise (as in the former Experiment) the weight of this so finely-powder'd Glass, in *Water*, differ'd by *such a Trifle*, from the *Counterbalance* of the solid Piece in the *same Element*, that it was by no means worth taking notice of: Especially too, since some parts of it *remain'd floating* in the Water, and never

se-

ſettled, or ſunk down into the *Bucket* of the *Hydroſtatical* Balance at all.

But what this way of arguing from the greatneſs of the Surfaces of Bodies, and the *reſiſtance by the Liquid ariſing* therefrom, will not do (with reſpect to the accounting for that Suſpenſion in a *ſpecifically-lighter* Medium) I believe may be done by another Method, and that *effectually*. In ſhort, the ſuſpenſion of the heavier Particles of Matter in Liquids, I attribute to the *ſame Cauſe that keeps the Liquors ſuſpended in ſmall Tubes:* I mean *Attraction*.

The *minute* Parts of Bodies conſiſting of *plane* Surfaces, *being ſtrongly attracted by the Parts of a Fluid, in which they are plac'd*, (and therefore reciprocally attracting the Parts of that Fluid again) *may, by the Action of theſe Forces, be held ſuſpended therein.* And what little Bodies are not, or will not be, *held ſuſpended* in a Liquid, but are *let fall* therein to the bottom of the containing Veſſel, I believe to be ſo, upon one of theſe two accounts: Either that the Parts of the Liquid *do more ſtrongly attract one-another*, than they do thoſe *little Bodies interſpers'd* amongſt them (which therefore *ſubſide* upon that ſcore;) or elſe, that they do by their *own Attractions* form themſelves into little cluſters, whoſe bulk and *ſuperiour Momentum* help to precipitate them downwards. This being laid down as the *true* Cauſe, of the *Suſpenſion* of ſmall ponderous Particles of Matter in Liquids; I believe our common Notions of *Corroſion* and *Diſſolution*, may alſo be rectify'd from the ſame Principles. A *Corroſive* Liquor or *Diſſolvent*, in the vulgar ſenſe, is a very *unintelligible* thing. For (not to mention *other Incongruities)* 'tis not to be conceiv'd what ſhould carry *the parts of a Liquid with ſo prodigious an Impetus into the Pores of a ſolid Body*, ſo as to diſſolve the whole Texture

ture of it, and reduce it into *insensibly-small Parts.* But an *Attractive Force* in that solid Body will do this; by which the Particles of the Fluid are forc'd into the Interstices of it, *with a Momentum superiour to that of the Cohesion of its Parts.* For this suppos'd; its Parts will be separated from one-another; that is, the Body will be *dissolv'd.* But the time perhaps will come, when this *wonderful Law of Attraction* (as it obtains in the smaller Portions of Matter) will be more fully and clearly understood, and some new Effects of it discover'd, which now are not suspected to proceed from that Cause.

AN

AN APPENDIX,

Containing ſome General Remarks *on ſome of the foregoing Experiments.*

THO' there are none of the Experiments related in the *foregoing Tract*, but what will (I hope) be of ſome uſe to the *Intelligent Philoſophical* Reader, (ſerving at leaſt to excite him to make farther Improvements himſelf, in *Experimental Knowledge*, if they don't give him all the Information he needs or deſires) yet there are ſome of them which being (I think) *quite new*, and moreover *very ſurprizing*, I thought it might not be amiſs to fill up a few Pages here with ſome Enquiries into, and Reaſonings upon them.

The Experiments I *principally* referr to, are thoſe of *Electricity* and *Light* produc'd by *Attrition*; of which the Reader may find a large account from *pag.* 17. to *pag.* 69. and theſe relating to *various ſorts* of Bodies, and in various *Mediums* too.

I begin with the Phenomena of *Electricity*.

There are ſome of theſe ſo ſtrange in their Circumſtances, that I confeſs I am apt to think there are *not many in nature*, more ſurprizing then they are.

But, tho' the diſcovery is yet but *young*, and has not been made long enough, to be *throughly and perfectly diſcuſs'd*; yet ſome things which are either *plain* and *certain*, or probable and likely, may be advanc'd in the mean time; and as all Circumſtances and Varieties in thoſe Experiments come to be more accurately examin'd, 'tis to be hop'd we may arrive at more *Poſitive Concluſions*, about *the Reaſons of* the Phenomena.

The Four following Propoſitions relate to the Attrition of Tubes.

Prop. 1. *Within the Body of the Glaſs, are contain'd and lodg'd certain Parts of Matter, of conſiderable Force and Activity, which by their Motions and Percuſſions are the Cauſes of all theſe Effects.*

That there is an emiſſion of ſome Matter conſequent on the Friction, I think is too plain to be queſtion'd; for 'tis obvious almoſt to every one of our Senſes: To the *Eye*; by the Motions of the Leaf-Braſs, and by the Light produc'd, when the Tube was rubb'd in the Dark: To the *Feeling*; by the ſenſible ſtrokes and puſhes, made upon the Face, when the Tube was held near it: To the *Ear*; by the *Noiſe* and Crackings, the Eruption was accompanied with, which might be heard at the diſtance of Seven or Eight Foot.

That this Matter emitted, is alſo emitted from or by the Tube; I take to be as plain as the former. For how elſe ſhould the rubbing of the Tube, ever be an occaſion of this Matter's diſplaying and exerting it ſelf? If it came not from thence, the Attrition of the Tube could not fetch it from any other Body diſtinct from the Tube. But the Teſtimony of Senſe aſſures us of this likewiſe: For all the motions of the Leaf-Braſs are directed to, or from, or about the Tube; and therefore tis beyond all diſpute, *that the courſe of the moving Matter is from thence.* And

I

I believe there's hardly any one but will allow, that this Matter, if it *came from the Tube, was certainly repos'd and lodg'd there before.*

Prop. 2. *The Motion of this Matter is not equable and regular, but disorderly, fluctuating and irregular.*

This appears from part of the Fact related. For the little Bodies sometimes would be drawn to, sometimes thrown from the Tube with violence; sometimes be suspended for a small time in the Air, and at other times slip along the sides of the Tube. They would repeat these Leaps and Boundings for several times together, and flutter up and down almost like so many Animals, rather than pieces of lifeless Matter. Now this Variety cannot be the Effect of an even and regular Motion. It plainly shews the moving Force to exert it self (as it were) by fits; and to be *propagated every way about in a confus'd irregular Orb.* For if Bodies once put in motion, can't of themselves alter their Direction, but are overrul'd by a foreign Force whenever they do it; and if the diversities of their Motions must needs infer just as great a diversity in the Impulses of the Bodies that move them; then since our pieces of Leaf-Brass (in this Experiment) were so very odd and extravagant in their Motions, 'tis plain, that the Effluvia (which alone can be the moving Bodies here,) must themselves also be *hurried after a very irregular manner.*

Prop. 3. *The Air contiguous to the inner Surface of the hollow Tube, has an Influence on Operations of the Effluvia.* This plainly follows, because when the Tube was exhausted, and the *contain'd Air drawn out*, the Leaf-Brass would scarce be stirr'd at all, tho' with a much more forcible Attrition, and at a much leſs diſtance, then when the Tube was full of Air. And beſides, when the Air was let into the Tube again; the attractive power (which was before almoſt loſt) was ſtrangely and ſuddenly recover'd again. Which is an undeniable Proof, that the Preſence of that contiguous Air did ſome way or other contribute to the more powerful and effectual operation of the Effluvia. Neither is it an objection of any moment againſt this; that the attraction is as powerful in the *caſe of the ſolid Tube*; where there being no *Cavity*, there can conſequently be no *contiguous Air*. For this only proves that there is as ſtrong an attraction in a ſolid Tube, as in a hollow one; but it does not prove, that the Air was of no advantage in the caſe of the hollow Tube. *To prove that an Effect may be the ſame, in two very different Circumſtances; is not the ſame thing as to prove that it has no manner of relation to this or that particular Cauſe in one of thoſe Circumſtances.* And therefore to argue from the Leaf-Braſs being ſtirr'd as vigorouſly by the Effiuvia, when the ſolid Tube was uſed; will not be ſufficient to ſhow that the Air has no manner of influence in the Circumſtance of the hollow Tube.

For the Propoſition does not aſſert, that the Effluvia can in no caſe exert themſelves with vigour, without the concurrent aſſiſtance of the Air: but it aſſerts, *that the contiguous Air had ſome advantageous Influence in the Caſe of the hollow Tube.* And this is as evident, as that the Air is ſerviceable to the vital Functions of Animals, or that they cannot live and breath without it. For as upon the depriving an Animal of the benefit of this Element, *all the Powers flag; the Springs of motion become feeble*

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and drooping; and at last sink away into a fatal inactivity: So here, if the Tube be exhausted of Air, *the Effluvia loose all that brisknefs* which wrought such surprizing Effects before; and continue (as it were) impotent and dead, till a fresh return of Air inspires them again.

And to add a Demonstration of the Airs power, with respect to the operation of the Effluvia, which will (I think) admit of no Exception; I desire it may be consider'd, that the Effluvia will not be excited by any Friction, to produce any Effects, if the Attrition of the Tube be made in *Vacuo:* and that, whether it be a closed *hollow* Tube replete with Air, or even a solid Tube it self: To either of which, I can give what degree of Friction soever is necessary, in an exhausted Receiver. The contiguous Air, I say, being removed, the Electrical force seem'd to be quite gone; and continued so to be, till the presence of the Air was restor'd. Now this is a plain Proof of the necessity of the Air, to the operations of this attractive matter. Wherein that necessity lies, or what assistance tis which the Air contribut's, I don't here determine; but that the thing is so, *is so manifest, that I cannot expect to see any thing more clearly prov'd by Experiment than this is.*

Prop. 4. *It does not seem that the Air included in the Cavity of the Tube can have any Influence (with respect to the Action of the Effluvia) but one of these two ways:* Either, by the *forcible Endeavour of its Spring* against the *contiguous* Body of Glass, helping to push and impell that *active Matter* outwards, which is already prepar'd and dispos'd by the *Attrition* for such a *Remove*; or else, as (by vertue of the *same Principle*) it *hinders the Electrical Matter from retiring*

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inwards, by acting as an *Impediment against* it; and ſo only *occaſionally* cauſes the more ſenſible and remarkable Effects of that Matter, upon little Bodies plac'd in its way *without* the Tube.

It may be, that the *Heat* produc'd by the *Motion* and *vigorous Attrition* of the Glaſs, may produce ſome *degree of Rarefaction* in the Air contiguous to the *convexe* or *outward Superficies*, And then, in *that Caſe*, there being not the like Rarefaction in the Air contiguous to the *concave* or *inner* Surface, (for the *rubbing* cannot produce that Heat upon a *diſtant* Surface, that it does on that which is *immediately* rubb'd;) the Electrical Matter will, with *much-more difficulty*, *retire in* towards the Cavity of the Tube, than it *will go outwards*: becauſe the *Equilibrium being loſt on the outſide*, it will neceſſarily be carried that way where it meets with the leaſt Oppoſition. And certainly, the Spring of the *leſs-rarefied* Air *within*, is ſuperiour to the *Preſſure* of the *more-rarefied* Air *without*.

And therefore, on the other hand, *when the Tube is exhauſted of its Air*, and conſequently the *Balance* loſt on the *inſide*; all the Attrition that can be given will not be ſufficient to *bring the Effluvia out* againſt an *incumbent Preſſure*, as long as the inward Cavity is *clear* of Air, and there is no *Counter-force* to oppoſe their *Conatus* or *Tendency* that way.

Prop. 5. *As the* internal *Air is neceſſary to the Action of the Effluvia*, ſo is the *external too*: Becauſe, tho' the Tube were full of Air, yet if rubb'd *in Vacuo*, the attractive Power was quite loſt.

Prop. 6. *As therefore the* internal *Air ſeems neceſſary, either to aſſiſt the Electrical Matter in its Motion outwards, or at leaſt to prevent its retiring inwards*; *ſo the* external

Air

Air appears to be as neceſſary to carry the little Bodies (which we ſay are attracted) *towards the Tube.*

For if by the Heat and Rarefaction, conſequent upon the Attrition, *the Medium contiguous to the Tube* be made *ſpecifically lighter*; then of courſe, *to keep up the balance, the remoter Air*, which is *denſer*, muſt preſs in towards the Tube, and ſo carry away (in the Torrent) the little Bodies lying in its way, thither alſo.

Prop. 7. The *various Irregularities* in the *excitation, or the emiſſion and diſcharge, of the Electrical Matter from the Tube* (which will be follow'd with proportional Irregularities, in the Motion and *Tendency of the denſer Air*, towards the Tube, by the *Hydroſtatical* Laws) *may be ſufficient to account for the various uncertain Motions of the little Bodies carried towards the Tube.*

I ſhall now add ſomething concerning the Effects of the Electricity of the Glaſs *Globe* and *Cylinder.*

Prop. 1. The *Preſence of the Air* is *neceſſary* to this *Phenomenon, of the regular Direction of the Threads*; as well as to *that* of the *Attraction of the Tube.*

Becauſe, if the ſemi-circular Hoop of Threads were plac'd *in Vacuo*, that Property of their regular Direction to a Center would be quite loſt, even tho' the *Globe* or *Cylinder* were *full of Air.*

Prop. 2. The reaſon therefore, *why the Threads are not directed in this caſe*, does not ſeem to be, *becauſe there is no Electrical Matter* diſcharg'd from the Glaſs (by the Attrition) to draw and direct them thither; but becauſe there *wants a current of External Air*, to put them into the aforeſaid *central Direction.*

For the *external* Air being *abſent*, and the *internal preſent*; the Matter ſhould find a vaſtly-eaſier paſſage outwards

wards, than *inwards*; and therefore ought to be discharg'd that way. But then, becaufe the external Air is remov'd, there is *no room for the lofs and recovery of an Equilibrium* to take place; and confequently no *Flux* of a *Circumjacent Medium* that way, and fo, *no Direction of the Threads*. For,

Prop. 3. *If the Electrical Matter be emitted* in *Phyfical Lines, every where diverging from the Center of that Circle in which the Attrition is made* (or in the *Plane* of which the *Hoop of Threads* ftands) *towards the Circumference of the fame Circle*; then by the *Rarefaction* of the *Medium contiguous to the Glafs*, and the *neceffary Preffure of the more remote and denfe Medium*, into the Plane of that fame Circle, with *Directions contrary* to thofe in which the Effluvia are emitted: *by this means* (I fay) the Threads may be regularly directed to the Center of that Circle, in whofe *Plane* the Hoop to which they are fix'd is plac'd.

For the *Flux of the denfe Medium* will be in *Directions contrary* to thofe according to which the *Rarefaction* is made. But the Effluvia are (by the *Hypothefis*) emitted in *Phyfical* Lines, *diverging from the Center towards the Circumference*. Therefore the *Rarefaction* of the *adjacent Medium* is according to the *fame* Directions. And therefore the *Flux* of the *remote denfer* Medium, is in Lines *converging* from the *Circumference* towards the *Center*. And all this (by the *Hypothefis*) being in the *Plane of Attrition*; that is, in the Plane, *wherein the Hoop of Threads ftands*: therefore the Threads are in the fame Plane, wherein the Flux of the denfe Medium paffes *in Lines converging from the Circumference towards the Center*. And therefore by *the Action of the faid Medium*, the *Threads may be forc'd into a regular Central Direction*.

Prop. 4. For the fame reafon; If the *Plane of Attrition be different from that Plane wherein the Threads*

are

are fix'd; the Threads ought to form themselves into a sort of *conical* Surface; or rather the Surface of a *Trunk* of a *Cone*, whose *Vertex* would be some point in the *Axis* of the *Globe* or *Cylinder*; were the *discharge of Electrical Matter* every way *equable* and *uniform*. And we find it matter of Fact, that the Threads *did actually form themselves into this sort of Figure.*

So that if there were two Hoops of Threads, plac'd one on one side, and t'other of the other side the *Plane of Attrition*, there would be two *Curti-cone* Surfaces form'd; of which the *more Acute* would be that which is *farthest* from the Plane of Attrition; and the more *Obtuse*, that which is *nearest* thereto. For when the *Plane of Attrition*, and the *Plane wherein the Threads are plac'd*, do co-incide; then the *Conic Surface* is chang'd into the *Area of a Circle:* because then the Threads lie all in *one and the same Plane.*

Thus much concerning the *Electricity*. I would now subjoyn some few things concerning the *Lights* produc'd in these Experiments.

Prop. 1. *Tho' the Electrical Quality necessarily requir'd the presence both of the External and Internal Air, in order to its shewing it self*; yet the *Light* requir'd the presence but of *one* of 'em, *viz.* either the *inward* or the *outward* Air, in order to its appearance.

For either a Glass Globe *full of Air*, rubb'd in *Vacuo*, or with its Air *exhausted*, and rubb'd in *Pleno*, would *either way* produce a very *considerable Light.*

Prop. 2. There seems therefore to be *a real difference between the Electrical and Luminous Effluvia* (at least in *some* cases:) For by the 'foregoing *Prop.* these Qualities require *different Circumstances* with respect to the *Circumjacent Medium*, in order to their discovering themselves. And more than that; a *stronger Attrition*, which generally heightens the Effects of the *Electricity*, does not at all *contribute to the encreaſe of the Light.* Nay, *Light is producible by the* Effluvia *of one Glaſs falling on another*; but the *Electrical Matter* is not to be brought forth, by any ſuch *feeble Strokes* or *Impulſes* as thoſe are.

Prop. 3. *Thoſe* Lights (in *ſome* Circumſtances at leaſt) *are leſs-ſenſibly affected by the return of the Air, which are produc'd upon an Attrition of* exhauſted *Glaſs in* Pleno, *than thoſe produc'd by the Attrition of Glaſs* full of Air *in* Vacuo.

For, in the former caſe, no great alteration was found in the *Light* or *Colour*, till a *certain quantity of Air* was let into the *inſide* of the *exhauſted* Glaſs. But in the latter caſe, both *Light* and *Colour* were ſenſibly chang'd, at *every* admiſſion of Air, *on the outſide* of the *full* Glaſs.

Prop. 4. *Of the various Lights produc'd from various Bodies by Attrition, or* (which is equivalent thereto) *the Concuſſion and Agitation of their Parts*; *ſome are much more confin'd to a* particular Medium, *as a neceſſary Condition of their Appearance, than others are.*

That of *Culinary Fire*, is abſolutely limited to ſuch a Medium as *Common Air*.

Thoſe of *Amber*, *Woollen*, *Oyſter-ſhells*, &c. require a *Vacuum*, or the neareſt *approach* to it, and utterly diſappear in a *groſſer* Medium.

The *Mercurial* Lights are yet *more unlimited*, as to the *condition of the Medium* in which they appear.

For, as they are producible in *Vacuo*, and in a *rarefied* Medium *approaching* thereto; ſo I have alſo ſhewn, That a Light of this kind may be made to appear even in *Common Air* it ſelf.

And thus much for the *Phenomena* of *Electricity* and *Light*, produc'd by *Attrition*. From all put together, I hope, ſomething may ariſe, that may be ſerviceable to the Deſign, of *gaining ſome true Knowledge, of the Cauſes of ſo ſurprizing Appearances.* And if any one ſhould luckily *Improve* theſe *ſhort Hints* for that purpoſe, I ſhall have obtain'd my End.

F I N I S.