Ex Dono. Dni Le ~~[illegible]~~ D. M. P.

A
CONTINVATION
OF
Nevv Experiments
Physico-Mechanical,
Touching the SPRING and VVEIGHT of the AIR, and their Effects.

THE I. PART.

Written by way of Letter, to the Right Honourable the Lord *Clifford* and *Dungarvan*.

VVhereto is annext a short Discourse Of the ATMOSPHERES of *Consistent Bodies*.

By the Honourable ROBERT BOYLE, Fellow of the *Royal Society*.

OXFORD,
Printed by *Henry Hall* Printer to the University, for *Richard Davis*, in the Year 1669.

The PREFACE.

HAving at the beginning of the Treatiſe, whereof This is a Continuation, acquainted my Readers with ſeveral things that belong in common as well to the following Experiments, as to thoſe There publiſh'd; it will not be neceſſary for me to trouble the Reader with a repetition of what he may have met with there already, nor to acquaint him in this Addreſs with any Other particulars then Thoſe that concern the Experiments I am now about to preſent him.

I doubt not but it will be remembred by ſome, that I ſeem'd in the above mentioned Book to have promis'd a Second part of it, or a large Appendix to it: but Intimations of that kind do many times reſpect onely the Thing it ſelf, leaving the Giver of them free in point of Time: and I wanted not ſufficient inducements to delay a while to perform my Promiſe, if I made any. I had indeed, partly before the Book already referr'd to came from the Preſs, and partly ſometime after, made divers other Tryals in order to a Supplement of it: but being oblig'd to make ſome Journeys and Removes, which allowed me no Opportunity to proſecute the Experiments, I had made no very great Progres in my Deſign, before the convening of an Illuſtrious Aſſembly of *Virtuoſi*, which has ſince made it ſelf ſufficiently known under the Title of the *Royal Society*. And having then thought fit to make a Preſent, to perſons ſo like to imploy it well, of the great Engine, I had till then made uſe of in the Phyſico-Mechanical Experiments about the Air; and being unable afterwards to procure another ſo good, I applied my Studies to other Subjects, and gave over for a great while the care of making more Experiments of that kind: and the rather, becauſe that finding by the very favourable reception Thoſe I had publiſh'd had met with among the Curious in ſeveral parts of Europe, that they were like to be Conſidered and Peruſed; I thought I might ſafely leave the Proſecution of them to Others, who would probably come more Freſh and untired to ſuch an Exerciſe of their Curioſity.

But obſerving, that the great Difficulties men met with in making an Engine, that vvould exhauſt and keep out a Body ſo ſubtle as the Air, and ſo ponderous as the Atmoſphere, (beſides perhaps ſome other impediments) vvere ſuch, that in five or ſix year I could hear but of one or two Engines that vvere brought to be fit to Work, and of but one or two Nevv Experiments, that had been added by the Ingenious Owners of Them; I began to liſten to the Perſwaſions of Thoſe that ſuggeſted, That unleſſe I reſum'd this work

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my ſelf, there would ſcarce be much done in it. And therefore having (by the help of Other work-men then Thoſe I had unſuccesfully imploy'd before) procured a new Engine leſſe than the Other, and differing in ſome Circumſtances from it, we did (though not without trouble enough) bring it to work as well as the Other, and, as to ſome purpoſes, better. And having once got This, I made haſt to try with it thoſe Experiments, that belonged to the deſign'd Continuation, and do now make up this Book.

I hope, that to ſuch Readers as the following Papers are principally intended for, I ſhall not need to make an Apology either for the Plaineneſſe of my Style, (wherein I aim'd at Perſpicuity, not Eloquence,) or for my not having adorn'd or ſtufft this Treatiſe with Authorities or Sentences of Claſſick Authors, which I had neither the leiſure to ſeek, nor thought I had any great need to imploy, though it had been far more eaſie then perhaps it would have proved, to borrow from them things that would have been very proper to a Treatiſe where my main Deſign was, to make out by practicable Experiments divers things among other that have not hitherto been advantaged by that way of Probation, nor perchance thought very capable of it; ſo that I ſhall have obtained a great part of what I aim'd at, if I have ſhewn, that thoſe very *Phænomena*, which the School-Philoſophers, and their party urge, and

and ſometimes triumph in, as clear Proofs of Natures abhorrency of a *Vacuum*, may be not onely explicated, but actually exhibited, ſome by the *Gravity*, and ſome alſo by the bare *Spring* of the Air. Which Latter I now mention as a diſtinct thing from the other, not that I think it is actually ſeparated in theſe Tryals, (ſince the Weight of the upper parts of the Air does, if I may ſo ſpeak, bend the Springs of the lower,) but becauſe that having in the already publiſhed Experiments, and even in ſome of Theſe, manifeſted the Efficacy of the Airs gravitation on Bodies, I thought fit to make it my Task in many of Theſe, to ſhew, that moſt of the ſame things that are done by the Preſſure of all the ſuperincumbent Atmoſphere acting as a *VVeight*, may be likewiſe performed by the Preſſure of a ſmall portion of Air, included indeed (but without any new Compreſſion) acting as a *Spring*.

The preſent firſt part of our Continuation might I confeſſe have been not inconveniently divided into two parts. For *firſt* it contains ſome Experiments that are already related in the Printed book, though they be here ſo repeated, as to be confirmed, illuſtrated, or improved, by being reiterated either with better Inſtruments, or with better Succeſſe than when they were made in my large Receiver, which holding (if I miſremember not) about eight Gallons, could not eaſily be ſo well exhauſted as thoſe ſmall Receivers I often

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ſince imployed. And *ſecondly*, the other and far more numerous ſort of Experiments, related in this Firſt part, are new and ſuperadded. And yet I forbear to aſſign each of theſe two ſorts a place by it ſelf, becauſe I could not conveniently ſet down my Tryals otherwiſe then as they came to hand among my Notes; and I conſidered, that in divers places the New ones and the Old ones being mentioned together, might ſerve by their neighbourhood to illuſtrate or confirm each other. And however at another Edition of our Continuation it will be a very eaſie task, if it appear to be a requiſite one, to give the improvements of the former Experiments, and the ſuperadded new ones, diſtinct Titles and Places.

As for the Mechanical contrivances I imployed in making the following Experiments, though moſt of them have had the good fortune to meet with an approbation, and ſome of them with more than that, from no mean *Virtuoſi* and Mathematicians; yet as I expect that Critical Readers will judg, that in ſome Experiments more artificial Inſtruments might have been made uſe of, ſo I hope that they will not look upon thoſe I was reduced to imploy, as alwayes the beſt that ever I could have directed, ſince it ſufficiently appears by diverſe paſſages of the following Experiments, that they were not made at *London*, but in places where the want of a Glaſs-houſe and other aco-

comoaccommodations reduced me to make my Tryals not after the beſt manner I could deviſe, but in the beſt way I could then and there put in practice. And let me add on this occaſion to what I have elſewhere ſaid to the like purpoſe, that tis both a great diſcouragement to many ingenious men, and no ſmall hinderance to the advancement of Natural Philoſophy, that ſome nice Criticks are ſo cenſorious in exacting from Attempters the very beſt Contrivances, and many that would be attempters ſtand too much in awe of ſuch mens judgments; for though in very nice Experiments the exactneſſe of inſtruments is not onely deſireable and uſeful, but in ſome caſes neceſſary; yet in many others, where the production of a new *Phænomenon* is the thing aimed at, they are to be looked upon as Benefactors to the Hiſtory of Nature, that performe the ſubſtantial part of a Diſcovery, though they do it not by the moſt eaſie and compendious wayes deviſeable, or attain not to the utmoſt preciſeneſs that might be wiſhed, and is poſſible. For ſuch performances, notwithſtanding their being ſhort of perfection, make diſcoveries to the World of new and uſeful things; which though others, that are more lucky at Contrivances, and have better accommodations, may compaſſe by more compendious wayes, or with greater preciſeneſſe; yet ſtill the World is beholding to the firſt Diſcovery for the improvements of it, as we are to *Ar-*

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chimedes for the firſt deviſing a way,to find by weigh-ing Bodies in Water, how much Gold or how much Silver a mixture of thoſe Metals does contain, though (if Hiſtorians have not injured that great man in the relation) he went a more laborious and leſſe accurate way to work than modern Hydroſtatians, who (as I elſewhere ſhew) may perform the ſame thing by a far better way, which yet probably we ſhould not have thought of, if that attributed to *Archimedes* had not preceded, and afforded us a fundamental Notion. And that the not being ſo dexterous at contriving the wayes to effect a thing, is no ſure argument that a man has not a true and ſolid knowledge of it, we may eaſily learn from *Euclid*, vvhom our Geometrici-ans generally and juſtly acknowledge to be their Ma-ſter, and to have enriched the World with many uſe-ful Truths, and ſolidly demonſtrated all his Propoſi-tions, though divers of his modern Commentators have found out more compendious wayes for effe-cting ſeveral of his Problems, as vvell as of demon-ſtrating divers of his Theorems, eſpecially ſince the excellent invention of *ſpecious Algebra*, by whoſe help that accurate Mathematician Dr. *Wallis* has, beſides other Specimens upon intricate Propoſitions, clearly demonſtrated the ten firſt and for the moſt part per-plexing Theorems of the ſecond Element, in litle more than as few lines. In ſumme, in Experiments

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that are very nice, accurate Contrivances and Instruments are industriously to be sought, and highly to be valued, and even in such other Experiments as are frequently to be reiterated the most commodious and easie ways of performing them are very desireable, but those practical Compendiums, though very welcome to them that would repeat Tryals, are not so important to the generality of Readers, as being but useful to save pains, not necessary to discover Truths; to vvhich men may oftentimes do good service, without any *peculiar* gift at Mechanical Contrivances, since in most cases They may be lookt upon as promoters of Natural Philosophy, who devise Experiments fit to discover a new Truth if the attempt succeeds, and propose wayes of bringing it to Trial, which though perhaps not the most skilful or expeditious, are yet sufficient and practicable, the increase of Physical knowledg being the product of the things themselves that are discovered, whatever were the Instruments men imploied about making the Discoveries.

As for the Cuts, I endeavoured to make their Relations, and Descriptions of most of the Experiments, so full and plain, as to need as few Schemes as might be to illustrate them: but though I hope, that they who either were verst in such kind of Studies, or have any peculiar facility of imagining, would well enough conceive my meaning onely by words; yet lest my own accustomance to devise such Trials, and to see these made, should make me think them more easily intelligible than most Readers will find them, I advised with a Learned friend or two, fit to be consulted on such an occasion, what Experiments were requisite to be illustrated with Diagrams, and to such I took care they should be annexed. Onely I forbore to adde to the Figure of each Instrument Alphabetical explications of its parts, as judging that troublesome work lesse easie for me, than it would be for such Readers as this Tract is designed for, to understand what is delivered by the help of a litle Attention in conferring the Schemes of the Instruments with the Verbal accounts of the Experiments they relate to. But there is one Particular about the Cuts may require both to be given notice of and excused: which is, that having occasion to alter the method of my Experiments, when I began to foresee that I should be obliged to reserve divers things for another opportunity; aud being my self absent from the Graver for a good part of the time he was at work, some of the Cuts were misplaced, and not graven in the Plates, in which, according to the present *series* of Experiments, they might most properly have been put.

But perhaps I may (for I am not sure of it) more need the Readers pardon for (unknowingly) troubling him in this Continuation with some passages, that he may have already met with in the Book it refers to: which though I had not read over for ſome years before, I chanced not to have at hand, when divers of the following Papers were writen; and though afterwards I recovered it, yet the indiſpoſition of my Eyes made me think it unfit rather to tire them by reading over the whole Book, than to truſt to the Readers good Nature (in caſe I ſhould need it) for the pardon of a few unintended Repetitions.

I doubt not, many Readers will be inquiſitive to know, why this Treatiſe is ſtiled the Firſt part of a Continuation: To give theſe ſome account of the Title, I muſt put them in mind, that in the already publiſhed Experiments I intimated, that two ſorts of Tryals might be made by the help of our Engine: the one, ſuch as needed but a ſhort abſence of the Air, and the other ſuch as required that the Air ſhould not onely be withdrawn for a vvhile, but kept out for a conſiderable time, from the Bodies vvhereupon the trial is made. Of the former ſort of Experiments are theſe this preſent Book does (as vvell as that heretofore publiſhed did) conſiſt of. And though I have been ſo much called upon, and troubled for certain Writings, whereof I had made ſuch mention in thoſe that paſt the Preſſe, as ſome Readers interpreted to be an engagement, that it made me think fit, when I ſatisfied their demands, to be thence forward very ſhy of making the Publick any promiſe; yet I was indu- ced not to alter the Title of this Treatiſe, partly be- cauſe it may intimate to the Curious, that there are yet a great many things to be performed by our Engine, beſides the productions of it I have hitherto preſented them, and partly becauſe, though I ſtill perſiſt in my former averſneſſe to make promiſes to the World; yet tis very poſſible, that if God grant me life and health, I may in due time preſent my Friends with what may ſerve for a Second part of our Continuation, conſi- ſting of Experiments that require a longer abſence of the Air from the bodies to be wrought upon: and I ſhall think, if this Firſt part prove not unaccepta- ble to the Curious, that the Latter will be not unwel- come to them, as being deſigned to conſiſt of Sets of Experiments, which by their being moſt of them New, and ſome of them odd enough, may perchance afford ſome not deſpicable hints to the Speculative. But the very nature of theſe Experiments, requiring that ſome of them ſhould be long in making, my Friends could not reaſonably expect a quick diſpatch of a work of this kind, though I ſhould not meet for the future with ſuch intervening impediments, as have hitherto diſturbed it, (as want of inſtruments, of health, of leiſure, and of the liberty, which is ſo requi- ſite in this caſe, of ſtaying long enough in one place:) notwithſtanding all which difficulties I have by

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ſnatches been able through God's bleſſing to make forty or fifty of deſigned Tryals, being ſuch as require the leaſt of time to be performed in, though I now think not fit to mention any of them, as well for other reaſons, as becauſe though they be made by the help of our Engine, yet they require a peculiar apparatus of Inſtruments, very differing from thoſe we have hitherto mentioned, and not to be intelligibly deſcribed without many words and divers figures. In the mean time, leſt the induſtrious ſhould be diſcouraged by a ſurmiſe, that there is nothing left for them to do by the help of our Engine, at leaſt as to the firſt ſort of Experiments, I ſhall inform them, that I had thoughts to have added divers others of that kind to theſe that now come forth, and particularly two Cluſters of Pneumatical trials, the one about Reſpiration, and the other about Fire and Flame; but ſeveral of my Notes and Obſervations being at preſent out of the way, my having neither health nor leiſure to repair theſe inconveniences, and proſecute Tryals of that ſort with any aſſiduity, makes me chuſe rather to reſerve them for an Appendix, than to make thoſe that now come abroad ſtay for them. Which will not (I preſume) be the more diſliked, becauſe by taking this courſe I may, in delivering of the *phænomena* of Nature, imitate Nature her ſelf, of whom tis the Roman Philoſophers ſaying, *Rerum Natura ſacra ſua non ſimul tradit.*

Seneca quæſt. nat. lib.7.c.31.

SOME

Some Advertisements touching the Engine it self.

THough the Engine already *published*, and that which I imployed in the following Tryals, have the same Uses, & agree both in the ground and the main part of their Construction, yet they differ in some particulars fit to be taken notice of: for after I had presented the great Engine I formerly made use of to the Royal Society, partly the difficulty of procuring such another of that Size and Make, and partly the desire of making some improvements invited me to make some alterations in the Structure; some of them suggested by others, (especially by the Ingenious Mr *Hook*,) and some of them that I added my self, as finding that without them I could not do my work. Wherefore it will not be amiss to point at the chief differences between the former and the latter Engine, and to intimate some of the conveniences and inconveniences that attend them.

As for the Construction of the second Engine it self, since tis presumed, that the Readers of this Book have already perused That of which this is a Continuation, and understood the contrivance of the Instrument that belongs to it, it was presumed sufficient to exhibit in the first Plate the delineation of the entire Engine ready to be set at work; and in the second, the figures of the several Metalline parts that compose it, before they are set together. For though these have not verbal and Alphabetical explications annexed to them, yet the sight of them may suffice to make those that have an imagination fitted to conceive Mechanical contrivances, and are acquainted with the former Engine, comprehend the structure of this; which, Alphabetical explications would scarce make such Readers do, as are not so qualified: onely two things there are, which being of some difficulty, as well as of importance to be conceived, I shall here particularly

larly tak notice of. The first of which is, that in regard the Sucker is to be alwayes under water, and the perforation *p q*, that passes perpendicularly quite through it, and serves together with the stick *r s* for a Valve, is to be stopt at the bottom of the Cylinder, as at *n o*, when tis full of water, twas requisite to make the stick *r p* of a considerable length, as two or three foot: The other and chief thing is that in the second Plate, the Pipe *AB*, whose end *B* bends upward, is made to lie in a gruve or gutter purposely made in the flat wooden Board *c d e f*, on which the Receivers are to rest; which square board I caused to be overlaid with very good Cement, on which I took care to apply a strong plate of iron, of the bigness and shape of the Board, leaving onely a small hole for the erected part of the Pipe to come out at, which I added, not onely to keep the wooden Board the better from warping, but because I knew (what will perhaps be thought strange) that the pressure of the Atmosphere on one side of the Board, when there is no pressure or but very litle on the other side, will enable many Aerial particles to strain through the very wood, though of a good thickness, and imbued with oyl to choak the Pores; to this iron-plate we sometimes fit a Lip turning up about it, to hinder the Water that on some occasions will come from the Receiver from falling on the Room; (and to add that upon the by) though the Stop-cock *g h i k*, that belongs to the hitherto mentioned Pipe, may be inserted at *l.* into the Barrel or Cylinder *l m n o* by the help of Soder, yet we chose as a much better way to have the Branch *l.* of the Stop-cock made like a Screw, which being once firmly screwed in to the Barrel, is not apt to be broken off, and may be more easily mended if any thing happen to be out of order, which the Engine is the most liable to be in or about the Pipe, partly because it may fall out, (though but very rarely if due care be but taken,) that the Air will insinuate it self between the wooden Board and the iron-plate, and so get up (where the Pipe bends upwards) into the cavity of the Receiver, and partly because the Pipe being for a just reason

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made but slender, and the part of it that looks upwards very short, it happens not very unfrequently, that when we imploy Receivers with narrow Orifices, where the Cement must lie close to the opening of the Pipe, it happens, I say, that the Cement, especially if it be much softned by heat, is suckt (as they speak) into the Pipe, and so choaks it up; or else that some part of the body included in the Receiver is drawn to the orifice of the Pipe, and lying upon it as a Cover hinders the free passage of the Air into the Barrel, against which inconvenience, to add that upon the by, we use amongst other Expedients to place just about the Orifice of the Pipe a small cover of Tin, like that of a litle Box, which covers it at the top to hinder any thing from lying immediately upon the Pipe, and has a small opening or two in the side, to give the Air of the Receiver free access to the Pipe.

The square and hollow wooden part of this Engine, discernable in the first plate, is so made, that it may contain not onely the Cylinder, but so much water, as will alwaies keep the Cylinder quite cover'd with that liquor; by which means the Sucker, lying & playing alwaies under water, is kept still turgid and plump, and the water being ready at hand to fill up any litle interval or chink, that may happen to be between the Sucker and the inside of the Barrel, does together with the newly mentioned plumpness of the Sucker very much conduce to the exact keeping out of the Air. But this advantage is not without some inconvenience, for divers times, if great care be not taken in turning the Stop-cock, the water will be impell'd into the Receiver, and much prejudice sundry Experiments, when the included bodies are such that may be spoiled or impaired (at least for the present) by that liquor. The smalness of our Cylinder is a convenience in regard of the facility it affords to make and dispatch those many Experiments that may be performed in small Receivers, though it make those more troublesome and tedious, that require the Exhaustion of large and capacious ones.

The flat Plate (mentioned a litle above) has this great conveniency in many Experiments, that the Receiver needs no Stop-cock of its own; for such a vessel being made all of an entire piece of Glass, and whelmed on upon the Plate well covered with Cement, can better keep out the Air, than if there were a stop-cock, at which the Air does but too frequently get in: but besides that in divers Experiments such Receivers do usually require to be wide-mouthed, whereby a greater compass is to be fenced against the ingress of the Air, several Experiments cannot so conveniently be tryed in this sort of Receivers.

But because, that though this second form of our Engine hath as to several purposes its peculiar conveniences and advantages, yet some *Virtuosi* may be furnished with the other already, and some may conceive it the more clearly of the two, or may judg it preferable for their particular designs; I shall here intimate, that for most of the Experiments, if not all, that follow in this Treatise, they may make use of, or at least make a shift with the first Engine, with a very few alterations; whereof the chief is to be this, That to the upper part of the great Cylinder, on the side opposite to the iron-rack, there is to be fastned such a square Board, and suitable iron-plate, as is used in the second Engine, betwixt which Board and Plate is to be lodged such a Pipe as was lately described, being either a continuation of the outward branch of the Stop-cock, or else firmly fastned to it by sodering or screwing: for by this means, when the Sucker is deprest, the Air will through the Cavity of this Pipe, and the Stop-cock whereto it is annexed, pass freely by virtue of its own Spring out of the Receiver into the exhausted Cylinder; though this, and the Sucker that moves in it, being not kept as in the second form of the Engine under water, the greater care will be needed to keep the Air from insinuating it self between them. A good Cement, to fasten the Receivers to the often mentioned Plate of Iron, is a thing of no small moment in making the following Experiments, of which we imploy differing Compositions for differing purposes, some of which are not necessary to be mentio-

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ned in that part of this work that now comes forth; but that which in almoſt all the following Tryals we chiefly make uſe of, is a well wrought mixture of (yellow) Bees wax and Turpentine, which compoſition as it ſerves better than moſt others to keep out the Air, ſo it has the conveniency, which is no ſmall one, of ſeldome needing to be heated, and ſeldomer to be much ſo; eſpecially if we imploy a litle more Turpentine in Winter than in Summer, in the former of which ſeaſons, as much, or very near as much of that ingredient as of the Wax does well, for as in Summer a mixture of three parts of Wax to about two of Turpentine is more proper.

Errata.

By an overſight a ſhort Paragraph was omitted in the 14. *page, importing, that* the ſecond figure of the 4th. Plate was deſignd onely to make ſome repreſentation of the difference that would appear, if inſtead of making the 4. Experiment with Water, as in the foregoing figure, the Tryal was made with Quick-ſilver.

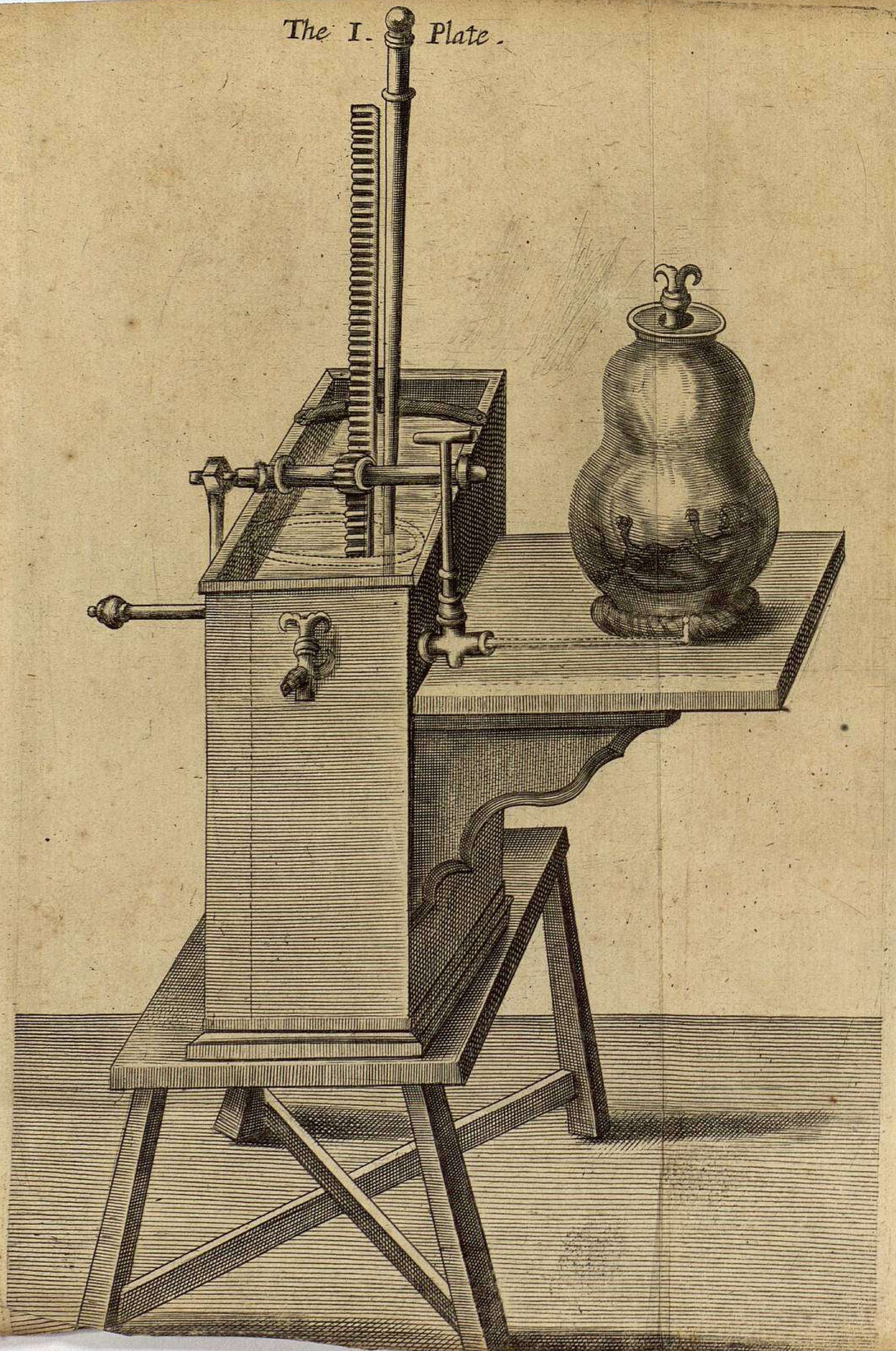
So likewiſe in pag. 104. lin. 4. *and* 8. *for* 14 of the 12 Book *read* 14 of the 11. pag. *ib.* l. 9. *read* Cylinders of equal heights are to one another as their Baſes.

The Reader is desired to perfect with his Pen the marginal Notes referring to the Plates as being defective, and also to insert such others as were wholly omitted, according to the following Directions; which could not otherwise be conveniently supplied, without putting a stop to the Press.

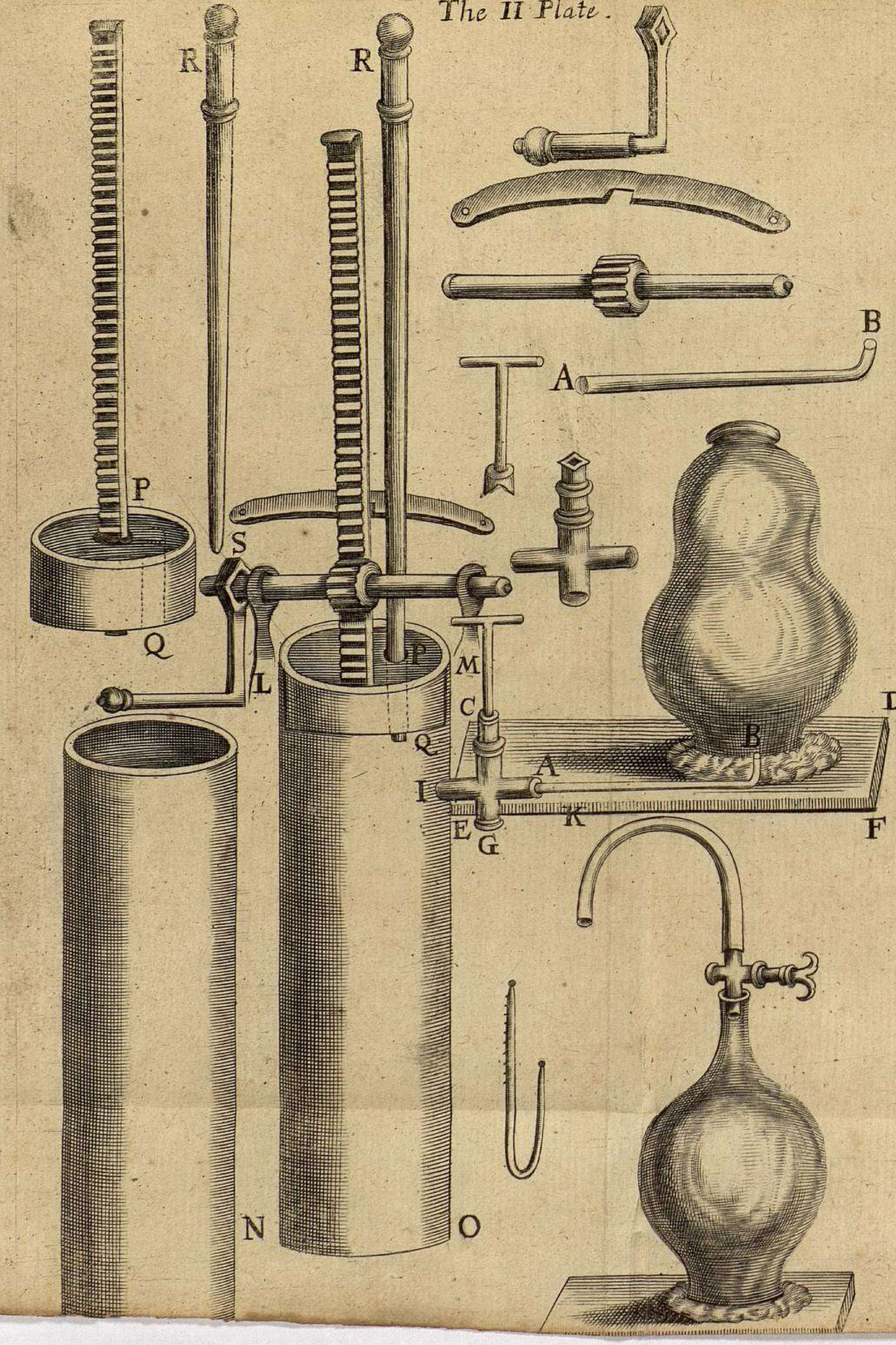
In the Margent of Page the——

3d. read *See Plate the III. Figure the* 1.
14. r. *See plate the IV. figure the* 2.
30. r. *See plate the III, figure the* 2.
33. r. *plate the III. fig. the* 2.
34. *See plate the III. figure the* 3.
43. r. *See plate the V. figure the* 1.
54. r. *See plate the III. figure the* 4.
73. against the 16. line, insert— *See the whole Baroscope delineated Plate the V. fig. the* 2.
87. against the last line but two, insert— *See plate the V. figure the* 3.
88. against the 6. line insert——*See plate the V. figure the* 4.
107. against the 28. line, insert *See plate the VI. figure the* 1.
111. against the 20. line, insert *See plate the VI. fig. the* 2.
113. r. *See the* 2. *figure of the* 7. *plate*: (adding thereto) *which though made primarily for the* 39. *Experiment, may facilitate the conceiving of This.*
120. against the 17. line, insert *See plate the VI. figure the* 3.
122. against the 9. line, insert *See plate the VI. figure the* 4.
123. against the 19. line, insert *See plate the VI. figure the* 5.
125. against the 14. line, insert *See plate the VI. figure the* 6.
130. read *See plate the VI. fig. the* 7.
132. r. *See plate the VII. fig. the* 1.
136. against the 8. line, insert *See plate the VII. figure the* 3.
139. read *See plate the VII. figure the* 4.
144. r. *See plate the VIII. fig. the* 1.
155. r. *See plate the IV. fig. the* 3.
161. r. *See plate the VIII. Fig. the* 2. *and* 4.
165. against the 21. line, insert *See plate the VIII. fig. the* 4.
and against the last line save one, insert *See plate the VIII. fig. the* 3.
166. r. *See plate the VIII. fig. the* 5.
174. Within 3 lines of the bottom, insert *See plate the IV. figure the* 4.

The I. Plate.



The II Plate.



A

CONTINUATION

OF

Nevv Experiments

Physico-Mechanical,

Touching the SPRING and WEIGHT of the AIR, and their Effects.

THE I. PART.

Written by way of Letter, to the Right Honourable the Lord *Clifford* and *Dungarvan*.

My Dear Lord,

SInce I have already in proper places of the *Physico-Mechanical Experiments about the Air*, which I formerly presented your Lordship, giv'n you a sufficient account of several things touching the Scope, Occasion, &c. of my Attempt; it will not be necessary to make a solemn Preface to the ensuing Experiments: And therefore presuming upon an acceptance, which the favourable Entertainment, which your Lordship, as well as the Publick, was pleas'd to give my first Tryals of this kind, encourages me to expect, I shall, without troubling you with any further Preface, immediately fall upon a Continuation; especially since Your Lordship will perhaps wonder, that you have not receiv'd it much sooner, as, indeed, you should have done, if I had been befriended with Accommodations and Leisure.

EXPERIMENT I.

About the raising of Mercury *to a great height in an open Tube, by the spring of a little included Air.*

DIvers ways have been proposed to shew *both* the Pressure of the Air, as the Atmosphere is a heavy Body, and that the Air, especially when compress'd by outward force, has a Spring that enables it to sustain or resist a pressure equal to that of as much of the Atmosphere, as can come to bear against it, *and* also to shew], that such Air as we live in, and is not condens'd by any humane or adventitious force,. has not onely a *resisting* Spring, but an *active* Spring (if I may so speak) in some measure, as when it distends a flaccid or breaks a full-blown Bladder in our exhausted Receiver.

But observing that there seems to want a visible Experiment to convince those that are not so easily satisfy'd with Reasons, though drawn by just consequence from Physical or Mechanical Truths, or even from other Experiments, taking notice, I say, hereof, I made the following Experiments; not so much to prevent or remove a scruple no better grounded, as to have a new way of making an Estimate by some known and determinate measure of the force of the bare Spring of the Air, both in its natural state, (as tis said to be when not compress'd nor rarify'd, more then the free Air we breath,) and according to its several degrees of Expansion.

We took then a Viol, with a neck not very large, and having fill'd about a fourth part of it with Quick-silver, we so erected and fastned a long and slender Pipe of Glass, open at both ends in the neck of the Viol, with hard sealing wax, that the lower end reach'd almost to the bottom of the Quick-silver, and the upper

more

more then a yard above the viol. Then having blown in a little air, to try whether the Instrumeut did not leak, (which tis very difficult to keep such instruments from doing,) we conveigh'd it into a long and slender Receiver, fit for such an use, and having withdrawn the Air as well as we could, we found according to our expectation, that the Spring of the Air, included in the viol, impell'd up the Quick-silver into the erected Pipe, to the height of 27. inches, and having suffer'd the External air to return into the Receiver, the Quick-silver subsided in the Tube, sometimes almost, and sometimes quite as low as the stagnant Quick-silver in the viol. *See plate the Figure the*

For the better illustration of this Experiment, thus summarily related, but with the like success, as to the main, several times repeated, we will subjoyn the following Observations and Notes.

I. That we try'd this Experiment several times, and the last time in the presence of the famous Savilian Geometer, Dr *Wallis*, who saw the Quick-silver in the Pipe impell'd up to 27. inches, being one himself of the measurers; and though at other times we found it to be much about the same height with the last, yet once it seem'd plainly to be a pretty deal higher; which yet we specifi'd not, because a mischance took off the mark, which we had made to measure the height by.

II. Having once, to try the stanchnesse of the viol, blown in so much Air, (without taking out any thing as we use to do in the like case) that the Air in the cavity of the viol rais'd and kept the Quicksilver 3 inches high in the Pipe, when we went on with the rest of the Experiment, according to the way above describ'd, we found, by emptying the Receiver of air, that we were able to raise the Quicksilver in the Cane 30. inches, or somewhat more above that in the viol.

III. Sometimes it may happen, that the Mercury, when taken very-soon out of the Receiver, will not appear to have subsided to its first lownesse, which perhaps 'twill not sink to in some while after: which is not to be wondred at, since in such a Receiver, which contains but little air, the heat of the Cement and the iron, imploy'd to melt it quite round the Receiver, may impart a little warmth to the air in the viol, which will after return to its former Temper. But this Accident is neither constant nor necessary to the Experiment.

IV. 'Tis very remarkable, that if the Receiver be fitly stopt, and slender enough, upon the turning of the Stop-cock, to let out the air at the first exuction, the Mercury will be impell'd up by the spring of the Air in the viol, suddenly flying abroad or stretching it self, so that it will be rais'd several inches above the height it will rest at afterwards, and will make several vibrations up and down before it come to settle, just as the Mercury does in the *Torricellian* Experiment, (the bare pressure of the little air doing here to the Mercury, what the weight of the Atmosphere does there,) and such motions of the Mercury will be made four or five subsequent Exuctions, upon the withdrawing of the air in the Receiver. But as these grow lesser and lesser, as the Spring of the included Air grows fainter, so none of them is any thing near so considerable as the vibrations made upon the first Suck.

V. Agreeable hereunto we observ'd, that at the first Exuction, when the Spring of the included Air was yet strong, the Mercury would be rais'd by our Estimate above half, if not $\frac{2}{3}$ of the whole height, whereto 'twill at length be brought; (though that must be according to the bignes of the Receiver, and other circumstances,) and the subsequent Exuctions do still adde less and less proportions of height to the Mercurial Cylinder, and that for two Reasons: the one, because the more there is of Mercury impell'd into the Tube, the greater weight of Mercury presses upon the included air: and the other, because the air has so much the more room in the viol to expand it self, whereby its spring must be proportionably weakned.

Lastly, when we made most of these Tryals, I had the curiosity to observe the height of the Mercury in a good Barometer, and thereby found, that the Air was then but light; its greatest height

height reaching but to 29 inches, and $\frac{3}{8}$, and its height soon after the Tryal, whereof Dr *Wallis* was a witnesse, amounting but to 29. inches.

To make an estimate of the Quantity of Air, that had rais'd the Quicksilver to 27 inches, we took the viol that was imploy'd about this Experiment; and having counterpois'd it, whilst it was empty, we afterward fill'd it with water, and found the Liquor to weigh 5. Ounces, 2. Drachms, and about 20. Grains; and then having pour'd out the water, till it was sunk to a mark which we had made on the outside of the Glass, to take notice how high the Quick-silver reach'd that we pour'd in: and lastly, weighing the remaining water, equal in bulk to the Quick-silver, we found it to amount to 1. Ounce, 2. Drachms, 14. Grains; so that the air, that had rais'd up the Mercury, possess'd (before its Expansion) in the viol the place but of 4. ounces, and a few odde grains, *i. e.* of about $\frac{1}{4}$ of a Pint of water. And as for the Pipe also, imploy'd about the same Experiment, we found its Cavity to have about $\frac{1}{8}$ part of an Inch in Diameter.

It was one of the Uses I hop'd to make of this Experiment, that by comparing the several degrees of Expansion of air included in the viol, with the respective and increasing heights of the Mercury that was impell'd up into the Pipe, some estimate might be made of the force of the Spring of the Air weaken'd by several degrees of Dilatation; but for want of conveniences I forbore to venter upon such nice Observations, especially because the Pressure of the dilated air, that remains in the Receiver, and is external to the air included in the viol, must also be taken into consideration.

Another Use of our Experiment may be this: That it may supply us with a considerable Argument against some Learned men, who attribute the suspension of the Quick-silver in the *Torricellian* Experiment to a certain rarify'd matter, which some call a *Funiculus*, and whereto others give other names; which rarify'd substance they suppose to draw up and sustain the Quick-silver,

ver, in compliance of Natures abhorrency of a *Vacuum*. For in the Experiment under consideration, the Quick-silver being not onely sustain'd at the height of 27 inches in the Tube, but elevated thither; if the cause of This be demanded, it will be answer'd, according to their *hypothesis*, that the air in the Receiver, external to that of the Viol, being, by reason of the sucking out of some of it by the Pump, more rarified than that in the viol, it draws up to it the Quick-silver in the Cane, and the more it is rarify'd, the higher it is enabl'd to draw it. But then I demand, whence it comes to pass, that though we can, by persevering to pump, more and more rarifie the little remaining air, or the Aëreal substance in the Receiver, That in the viol not appearing to be also rarified, yet the air in the Receiver does not by virtue of its superadded rarefaction, whereby it exceeds that of the air in the viol, pull up the Quick-silver to a greater height in the Tube then 27. inches: For, that this is not the greatest height, to which Mercury may be rais'd by this rarefy'd substance, our Adversaries must not deny, who tell us, that in the *Torricellian* Experiment it sustains a Mercurial Cylinder of 29. inches, and $\frac{1}{2}$, and can raise a Cylinder of 29 inches to 29$\frac{1}{2}$, or higher, in case that the Cylinder be made to vibrate up and down in the Tube.

See the latter part of the following Experiment.

And as for those, that will in such cases, as our Experiment suggests, have recourse onely to that which they call the *Fuga Vacui*, they may please also to consider, that since the Quick-silver remains the same, its ascension in the Tube will not be available for what they think to be Natures purpose; for, whether it reach higher or lower in the Tube, it will adæquately fill no more space in one posture, or in one figure, then in another, in what part soever of the cavity of the Receiver it be plac'd.

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EXPERIMENT II.

Shewing, that much included Air rais'd Mercury *in an open Tube, no higher than the weight of the Atmosphere may in a Baroscope.*

IN the former Experiment, by reason of the smalness of the viol, that was employ'd about it, there was so little Air included, that the Expansion of it so far, as was requisite to impell up the Mercury in the Pipe to the above mentioned height of 27. inches, may be probably suspected to have very much weaken'd its Spring, and therefore it may be thought, that (especially considering the great force that several of our Experiments manifest imprison'd air to have,) if there were a greater Quantity of air included in the vessel, so that the Expansion, sufficient to raise the Mercury to the former height, would not need to be considerable, (because that the capacity of the Tube being but the same, the whole included air will be so much the lesse expanded, by how much the more of it there is,) it seem'd probable that the Spring of the Air, being but a little weakned by so small a dilatation, would remain strong enough to raise a much taller Cylinder of Mercury in the Tube, and perhaps make the Liquor run over into the Receiver.

But though this Suggestion seem probable enough, yet when I consider'd, that the weight of the Atmosphere is able to sustain a Cylinder of Quick-silver but of 30. inches, or thereabouts, (in perpendicular height,) and consequently that the pressure of such a Mercurial Cylinder is equivalent to that of an Atmospherical Cylinder of the same bore; 'twas not difficult to conclude, that since the Air in a viol, before the mouth is clos'd, has a Spring but equal in strength to the weight of the Atmospherical Pillar that leans upon it, (for if the Spring were too strong for the weight that leans on it, some of the air would get out of the viol,) a greater viol, and consequently a greater quantity of included air would not be able by its ſpring to elevate and ſuſtain a longer Cylinder of Mercury, than the weight of the Atmoſphere is able to do; nor indeed altogether ſo much, becauſe of ſome little (though but little) Diminution of the Spring by ſome (though but a ſmall) expanſion, that the included Air ſuffers, by ſucceeding in the place of the Mercury, that is impell'd up.

To clear therefore this matter by an Experiment, we took a ſtrong glaſs-bottle, capable of holding about a Quart of Liquor, and having put into it a convenient quantity of Quick-ſilver, we erected in it a very long and ſlender pipe of Glaſs, open at both the ends, and reaching at the lower end beneath the ſurface of the ſtagnant Mercury, and having faſten'd this pipe in the neck of the Bottle, by choaking up that neck very accurately with good Cement, that none of the included air might be able to get out, we conveigh'd the whole into a Receiver, like that imploy'd about the I. Experiment in ſhape, but much larger, that it might be able to contain ſo great a veſſel; and then the Engine being ſet a work, we quickly rais'd the Quick-ſilver to a greater height than formerly, and when we ſaw it come to a ſtand, we did by the help of ſome marks, made before hand on the pipe, and by the help of a very long and well divided Ruler, meaſure, with as much care and accurateneſs as the figure of the veſſels would allow us to do, the height of the Mercurial Cylinder, which we found to be 29. inches, and about $\frac{7}{8}$, to which abating half an inch, which was rais'd, before the Pump was employed, by ſome air that had been blow'd into the Bottle, to try whether it were ſtanch; deducting, I ſay, this half Inch of Quick-ſilver, which remain'd in the Tube after the external Air was let in, (as well as it had been there before the Receiver was exhauſted,) out of the newly mention'd number there remain'd 29. inches, and neer $\frac{3}{8}$, for the height of the Mercury, rais'd by the Spring of the Air, ſhut up in the Bottle: and then conſulting with the above mentioned Baroſcope, which ſtood in a window in another part of the houſe, I found, that the weight of the Atmoſphere did bear a Mercurial Cylinder of about 29.

inches;

Inches and $\frac{1}{2}$, which was higher by $\frac{1}{8}$ than that to which the Spring had rais'd the Quick-silver in the exhausted Receiver: and the Difference perhaps would have been greater, if the place, where the Experiment was made, had not by its warmth added some little matter to the Spring of the Air, and if also we could have kept the Mercury so long elevated, as to give it leave to discharge its self of those small bubbles, which tis almost impossible in such Experiments as this to free Quick-silver from, without some help from time.

Lastly, though we caus'd the Pump to be ply'd, to try whether we could not, by the more diligent Exuction of the Receiver, raise the Quick-silver above the height of that which the Atmosphere kept sustain'd in the Baroscope, yet our labour gave us but a confirmation, that the Spring of the *Air* would not raise the Mercury higher, then did the *weight* of the Atmosphere, which may not a little confirm the 2^d^ Observation.

N B. This was not the onely nor the first Experiment we made of this kind, but this being carried on without mischances, (with which divers others were attended,) and made with much care, I thought fit to set down This in stead of all, intimating generally about the rest, that they seem'd to agree well for the main with that, which is here recited; onely there is one thing relating to those other Experiments, that seems not altogether unworthy to be taken notice of; which is, that when our Tryals were made in vessels, that contain'd a considerable quantity of Air, though upon the exhaustion of the Receiver the Spring of the included Air could not raise the Quick-silver to the top of the pipe, yet sometimes by other Effects it manifested it self to be very strong, as once or twice by the blowing out or breaking the Cork or Cement, and other matter that was imploy'd to stop the Glass it was shut in; and once by an Accident too memorable to be here past over in silence.

I had one day invited D^r^ *Wallis* to see such an Experiment as I have been relating, made with (not a viol, but) a bottle of Green

Glass, (such as we use now for Wine,) and 4 or 5 pounds of Mercury. After this Learned Person and I had continued Spectators as long as we thought fit, we withdrew into another Room, where we had not sat long by the fire, before we were surpriz'd by a suddain noise, which the person, that occasion'd it, presently came running in to give us an account of, by which it appear'd, that this Ingenious young Man, (whom I often imploy about Pneumatical Experiments, and whom I mention'd to Your Lordship, because *I. M.* has the honour to be somewhat known to You,) being desirous in our absence to satisfie the Curiosity he had to know, whether the Quick-silver could not be rais'd higher in the pipe than I had foretold, plyed the Pump so obstinately, that at length, the Bottle being not, it seems, every where equally strong, the imprison'd air found it more difficult to make the Quick-silver run over at the top of the pipe, than to break the Bottle in the weakest place, and accordingly did not onely throw off a piece of the Bottle, but threw it with such violence against the large and strong Receiver, as broke that also, and render'd it unserviceable for the future. But the Doctor and I laying together the Pipe, which happen'd to be broken into but few pieces, concluded by the place, to which we were told it reacht when this Accident happened, that it had not exceeded, nor indeed fully equall'd the height, to which the weight of the Atmosphere might have rais'd it.

EXPERIMENT III.

Shewing that the Spring of the included Air will raise Mercury to almost equal heights in very unequal Tubes.

HAving shown in the two former Experiments, that the *Active* strength of the Airs Spring is very considerable, I thought good also to examine, whether or no to the other resemblances

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in operation between the weight of the free Air, and the pressure of the included Air, this also may be added, that as the gravitation of the Atmosphere is able (as we shall hereafter prove) to sustain the Mercury at the same height in lesser and greater Tubes, seal'd at the top; so the Pressure of the included Air may be able to sustain the Mercury at the same height in slenderer and in larger Tubes, though in the latter it must sustain a far greater weight of Mercury than in the former; provided allowance be made for the weakning, which the Spring of the included Air must be subject to, by reason that, to succeed in the place of a large Cylinder of Mercury impell'd up into the greater Tube, it must expand it self more, and consequently have its Spring more weakned, than if the Tube were slender.

To prosecute this Experiment, I thought on a peculiar shape of vessels, which, if I had been where there is a Glass-house, I would have caus'd to be blown for the more convenient trying of two Pipes of different bores at the same time. But though I wanted this Accommodation, I thought I might well enough show what I intended by imploying successively two Tubes of very differing sizes, provided the vessel for the including of the Air were the same.

Wherefore taking the Glass bottle, made use of to try the former Experiment, and erecting in it after the manner above described a Cylindrical pipe of Glass, a good deal larger than the former, (if not as large agen,) we prosecuted the Experiment as we had made it, with the slender Tube above mentioned, and found that we were able, by the Spring of the Air in the bottle, to raise the Quick-silver to a considerable height, which, measuring as well as the vessel would allow us, was, by the least estimate that was made of it, (which was mine) 28. inches, and $\frac{1}{8}$, by which it appear'd to want somewhat above an Inch of the height of the Mercurial Cylinder, which the weight of the Atmosphere could have sustain'd, as appear'd by the Barometer, wherein the Quick-silver at that time was about 29. inches, and $\frac{1}{4}$ high; which diffe-

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rence was no more then I expected, confidering that, whereas the weight of the Atmofphere is ftill the fame when the Mercury is at its full height (and that whether the Pipe be great or fmall) in a feal'd Tube; the Spring of our included Air muft needs be weakned the larger the Tube is, and the higher the liquid Metal is impell'd in it; fo that it feem'd a confiderable *Phænomenon*, that the Spring of fo little Air fhould be able to raife the Mercury as high within an Inch or thereabouts in a wider as in a flenderer Tube, fince the Diameter of the Cavity of the former being by our eftimate double to that of the latter, (into which the flender Pipe could eafily be put *as into* a Cafe too big for it:) The greater Mercurial Cylinder may be fuppos'd to have weighed near four times as much as the leffer; I fay, *near*, becaufe there was an Inch difference in their heights: but in cafe thefe had been equal, then the Solidities of the Cylinders would have been to one another as their Bafes; and fince thefe, being Circular, are in duplicate proportion to their Diameters, that is, as the Squares of their Diameters; its plain, that if the Diameters be as one to two, the Squares of them muft be as one to four; and thefe Cylinders confifting of the fame Mercury, their Weights will have the fame Proportions with their Solidities, and confequently would be as one to four, making the abatement formerly intimated for the Inch and a little more of Mercury, by which the larger Cylinder came fhort of the height of the former.

NB. 1. This and the two former Experiments tryed by us with Quick-filver, may be alfo tryed with Water; but befides that we could hardly procure Tubes long enough for fuch Tryals, we were not very follicitous about it: for if we attentively enough confider, what has been already deliver'd, and the Proportion in fpecifick gravity betwixt Water and Quick-filver, (whereof the latter is near 14. times as heavy, bulk for bulk, as the former,) 'twill not be difficult to forefee the Event of fuch Experiments, which he, that has a mind to make, fhould be furnifh'd not onely with long Tubes, but with capacious Veffels to fhut up the Air in,

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Else the Air will be so far expanded before the Water has attain'd near the height, to which the weight of the Atmosphere may raise it, that the Experiments will not seem to succeed near so well with Water, as ours did with Quick-silver.

2. We thought it worth trying, whether, when the included Air had rais'd the great Cylinder of Mercury to the utmost height, it could elevate it to, by the Spring it then had; it would not be brought to raise the Quick-silver yet higher, if, notwithstanding the Expansion it had already, there were an agitation made by the heated Corpuscles of the same Air. And in pursuance of this Curiosity having caus'd an hot Iron and a Shovel of kindled Coals to be held near the opposite parts of the Receiver, we perceiv'd after a while, that the Mercury ascended $\frac{1}{8}$ of an inch or better above the greatest height it had reach'd before. But conjecturing that it would have risen higher, were it not that whilst the application of the hot bodies was making, some Particles of Air had unperceivably stolen into the Receiver, I caus'd the Pump to be ply'd again to withdraw the Air, I suspected to have got in, by which means the Mercury was quickly rais'd $\frac{5}{8}$ of an inch, (or better,) by virtue of this Adventitious Spring, (if I may so call it,) which the included Air acquir'd by heat, and I made no doubt, that it might have been rais'd much higher, but I was unwilling by applying a less moderate heat to hazard the breaking of my Glasses, in the place I then was in, where such a mischance could scarce have been repair'd.

EXPERIMENT IV.

About a new Hydraulo-pneumatical Fountain, made by the Spring of uncompress'd Air.

I Shall now add such an application of the Principle whereon the former Experiment was grounded, as I should scarce think worth

worth mentioning in this place, were it not that besides that divers *Virtuosi* seem not a little delighted with it, it may for ought I know prove to be of some Philosophical use (to be pointed at hereafter.)

We took a Glasse-bottle with a convenient quantity of Water in it, and fitted this Bottle with a slender glass-pipe open at both ends, aud about three foot long , which was so plac'd, that the lower Orifice was a good way beneath the Surface of the Water, and the Pipe it self passed perpendicularly upwards through the Neck of the Bottle, which Neck was, by the Pipe and by good hard Cement imploy'd to fill the space betwixt the Pipe and the inside, so well and firmly clos'd, that no Water or Air could get out of the bottle , nor no externall Aire could get into it, but by passing through the Pipe. This Instrument was convey'd into a large Receiver shap'd like a Pear, of which a good part of the blunt end , and a small part of the sharp end are cut off by Sections parallel to the Horizon, and consequently to one another. And because this Receiver was not (nor ought to be) long enough to receive the whole Pipe, there was Cemented on to the upper part of it a smaller Receiver of white Glass, of such a length and bigness, that the upper end of the Pipe might reach to the middle of its Cavity, or thereabouts, and that the motions of the springing water might have a convenient Scope, and so be the better taken notice of.

See plate the Figure the

This double Receiver being cemented on to the Engine, a little of the Air was by one Suck of the Pump drawn out from it, by which the Pressure of the remaining Air being weakned, it was necessary, that since the Air included in the Bottle had not its Spring likewise weakned, it should expand it self, and consequently impell up the water in the same Bottle through the Pipe, which it did so vigorously, as to make it strike briskly at first against that part of the top of the smaller Receiver, which was just over the Orifice of the Pipe. But after it had a while made the Water thus shoot up in a perpendicular line, as the Spring of the Air in the Bottle grew by that Airs dilatation to be weaken'd, the Water would be impell'd up leſs ſtrongly and leſs directly, till the Air in the Bottle being as much expanded as that in the Receiver, the Aſcent of the Water would quite ceaſe, unleſs by Pumping a little more Aire out of the Receiver we renew'd it again.

About the making of this Experiment theſe Particulars may be noted.

1. Tis convenient, that the upper part of the Pipe be made (as it eaſily may be at the flame of a Lamp) very ſlender, that the Water having but a very ſmall Orifice to iſſue out at, may be ſpent but ſlowly, and thereby make the Experiment laſt ſo much the longer.

2. You may, if you pleaſe, in ſtead of making the upper part of the Pipe ſlender, as was juſt now directed, Cement on to it a Top either of Glaſs or Braſs, conſiſting of three or more very ſlender Pipes, with a Pin hole at the end of each, that one of theſe pointing directly upwards, and the others to the right hand and to the left, the Water may ſpin out ſeveral ways at once, by which kind of branched Pipes we have ſometimes imitated the *Jets d' eau* (as the French call them) and Artificial fountains of Gardens and Groto's.

3. In regard that ſo ſhort a Cylinder of Water, as exceeded not the length of our Glaſs pipe, could not make any conſiderable reſiſtance to the expanſion of the included Air, it was thought and found ſafe enough to imploy in ſtead of a ſtrong Glaſs-bottle a much larger Viol, without being ſollicitous about its ſhape, or that it ſhould be very ſtrong, and by this means we could make this pleaſant Spectacle laſt a great while, eſpecially if we alſo made uſe of the expedient to be mentioned in the following Note.

4. If you find that the included Air have by expanding it ſelf too much weaken'd its Spring, whilſt there yet remains with it a good quantity of Water in the Bottle or Viol, you may reinforce the preſſure of the Air by onely turning the Stop-cock, and letting in what air you think fit to the exhauſted Receiver: for upon the admiſſion of this new Air, the Air in the Receiver will preſs

upon the Water in the Pipe, and having driven it into the bottle again, will follow it thither, till the Air in the Bottle, and that in the Receiver have attain'd an equal Spring, and then by Pumping out a convenient quantity of the Air contain'd in the latter, the Air ſhut up in the former will be able to impell up the Water as before, till the ſtagnant Liquor be depreſt to the lower Orifice of the Pipe, at which; when the Air of the bottle can get out, the courſe of the water upwards muſt ceaſe.

The Uſes I made of this new Hydraulo-pneumatical Fountain (for in it I aim not onely at a Ludicrous Experiment) were principally theſe.

The firſt was to make it the more probable, that if we had had convenient Veſſels, we might by the Preſſure of the Air included in the Bottle have rais'd Water about fourteen times as high as we did Quick-ſilver in the former Experiment, ſince upon but a little weakning of the Preſſure of the Air in the double Receiver, the Air in the Bottle was able to impell the Water forcibly enough, and for a pretty while, to the top of a Pipe of about a Yard long, and a good deal higher. (But this is but a ſlight Uſe.)

The next thing therefore we deſign'd to ſhew by this Experiment was, That in thoſe Hydraulo-pneumatical Engines, where Water is plac'd between two parcels of Air, the Water may be ſet a moving as well by the meer dilatation of one of the parcels of the Air, as by giving a new force by heat or compreſſion to the other, and whether this Mechanical Principle of Motion may hereafter prove not altogether uſeleſs in Engines, we refer to further conſideration.

Another Uſe we made of this Experiment was to ſhow ſomewhat relating to the Spring of the Air, which may be worth conſidering, though we ſhall now but barely mention it. If then, when ſome of the Air had been pump'd out of the Receiver, we remov'd that double Veſſel from the Bottle, the external Air would by its weight haſtily depreſs the water in the Pipe, till having driven it to the very bottom, it got up in numerous Bubbles through the water, and joyned it self with the Air incumbent on that Liquor: but that which was here observable was, that all the external Air that was able to get into the Bottle, did not do it suddenly, but after the first irruption we could perceive, that from time to time there would new portions of Air leasurely insinuate themselves through the Pipe into the Bottle, and emerge through the stagnant Water in Bubbles, that succeeded one another so slowly, as to beget some wonder, as if the Spring of the included Air having been once put out of its wonted constitution by its late expansion, could not be reduc'd to it but by degrees by the weight of the Atmosphere, which was still the same: or, rather, as if between the Spring of the included and the Pressure of the external Air counterballancing each other, there happen'd some such thing as is observ'd in an ordinary pair of Scales, of which one is too much depress'd, where the motion (which was swift enough at first) becomes so much the slower, by how much the Weights come nearer to the *Æquilibrium*, which their equality disposes them to rest in.

But the chief Use design'd in this Experiment was, to observe, whether the Lines, made by the water in its effluxions, would be of the same figure, notwithstanding the rarifaction of the Air in the upper part of the Receiver, as if the Air had not been at all rarified: and for this purpose it is best to make ones Observations towards the latter end of the Experiment, because then the Receiver being most exhausted, and consequently having the least of Air left in it, the difference made by the change of the densiry of the *medium*, in which the Beams of Water (if I may so call them) move, is like (in case there be any) to be best discern'd. And this convenience we had by our way of Experimenting, that we could take notice of the Lines describ'd by the Salient water, as the ejaculation of that Liquor grew still fainter and fainter. But though I afterwards invited Dr. *Wallis* to favour me with his Opinion about the Curve Lines of the Salient water, yet for want of an upper Receiver large enough, even he profess'd himself (as

I had done) not satisfied about them. Onely He sometimes (as I also did) observ'd the Salient water to describe part of a line perfectly enough Parabolical, with which sort of Curves he has been particularly conversant.

This made me resolve for further satisfaction to attempt by another contrivance, (of whose success, if I can procure the Implements I need, Your Lordship may expect an account,) what the Figures will be not onely of Salient water, but Mercury, and other Liquors; and that when the Receiver is much better exhausted, then it was necessary it should be in the foregoing Experiment.

EXPERIMENT V.

About a way of speedily breaking Flat Glasses, by the weight of the Atmosphere.

FOr the more easie understanding of some of the subsequent Tryals, it will be requisite in this place to mention among Experiments about the *Spring* of the Air the following *Phænomenon* belonging to its *Weight*.

This is one of those that is the most usually shown to Strangers, as a plain and easie proof both that the Weight of the incumbent Air is considerable, and that the round figure of a Receiver doth much more conduce to make an exhausted Glass support that weight, than if the upper part of the Receiver were flat.

To make this Experiment we provided a Hoop or Ring of Brass of a considerable thickness, whose height was 2 $\frac{1}{2}$, or 3 Inches, and the Diameter of whose Cavity as well at the upper as lower Orifice (should have been just 3. Inches, but through the errour of the workman) was 3. inches and $\frac{2}{10}$. To this Hoop we successively fasten'd with Cement divers round pieces of Glass, such as is used by Glasiers (to whose Shops we sent for it) to make Panes for Windows, and thereby made the Brass-ring with its Glass-

Glass-cover a kind of Receiver, whose open Orifice we carefully cemented on to the Engine; and then we found, as we had conjectured, that usually at the first Exuction (though sometimes not till the second) the Glass-plate would be broken inwards with such violence, as to be shatter'd into a great multitude of small fragments, and (which was remarkable) the irruption of the external Air driving the Glass inwards did constantly make a loud Clap, almost like the Report of a Pistol. Which *Phænomenon*, whether it may help us to discover the cause of that great noise, that is made upon the discharging of Guns, (for the Recoyl seems to depend upon the Dilatation and Impulse of the Powder,) I must not stay to consider.

EXPERIMENT. VI.

***Shewing, that the breaking of Glass-plates in the foregoing Experiment, need not to be ascrib'd to the* Fuga Vacui.**

THough I long since inform'd you, that in the Experiments I then presented Your Lordship, it was not my purpose to deliver my own Opinion whether there be a *Vacuum*, or no, and though I do not in this Tract intend to declare my self either way; yet, that I may on this occasion also show, that the Pressure of the Air may suffice to account for divers *Phænomena*, which according to the vulgar Philosophers must be referr'd to Natures abhorrency of a *Vacuum*, I will illustrate the foregoing Experiment by another, the substance whereof is this.

That if, instead of the above mentioned brass Hoop, both whose Orifices are of equal breadth, you imploy a hollow (but taller) piece of Brass, or (which is more easily made) of Latton, shap'd like a *Conus truncatus*, or a Sugar-loaf, whose upper part is taken off parallel to the bottom; and if you make the two Orifices of a breadth sufficiently unequal, as if the larger being made

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as wide as that of our Brass-hoop, the straiter were less than an Inch in Diameter; You will find, that if this piece of Metal be made use of, as the other was in the foregoing Experiment, the flat Glass cemented on to the Orifice, will be easily broken, as formerly when tis fastned to the wider Orifice; but if the straiter Orifice be turn'd upward, the Glass that covers it, if it be of a due thickness, (though no thicker than the former,) will remain entire, notwithstanding the withdrawing of the Air from beneath it: Which seems sufficiently to argue, that tis not precisely Natures abhorrency of a *vacuum*, that is the cause why Glasses are usually broken in such Experiments, since whether the wider or the narrower Orifice be uppermost, and cover'd, (the Metalline part of the vessel being the same, and onely varying its posture,) the capacity of the exhausted vessel will be equal; and therefore Nature ought to break the Glass as well in one case as the other, which yet the Experiment shows she does not.

Wherefore this Diversity seems much better explicable by saying, that when the wider Orifice is uppermost, the Glass that covers it must serve for the Basis of a large Atmospherical Pillar, which by its great weight may easily force the resistance of the Glass: whereas when the smaller Orifice is uppermost, there leans upon its Cover but so slender a Pillar of the Atmosphere, that the natural tenacity or mutual cohæsion of parts in the Glass is not to be surmounted by a weight that is no greater.

EXPERIMENT VII.

About a convenient way of breaking blown Bladders by the Spring of the Air included in them:

THe foregoing Experiments having sufficiently manifested the strength of the Airs Spring upon fluid Bodies, I next thought fit to try, whether the force of a little included Air would

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alſo upon conſiſtent and even Solid bodies emulate the Operations of the weight of the Atmoſphere. In the proſecution of which Enquiry we thought fit to make two ſorts of Tryals: the one, where the Air is included in the Bodies, on which its Spring does work; and the other, where tis External to them. Of the firſt ſort are this 7th, and the two following Experiments; and of the ſecond ſort are ſome other Tryals, to be comprehended under the 10th Experiment.

Having formerly mention'd to Your Lordſhip, that we were ſeveral times able (though ſometimes not without much difficulty) to make a blown Bladder break with the Spring of its own Air; I ſhould not think it worth while to ſay any thing here about the ſame *Phænomenon*, but that (beſides that it ſeems odd enough, and is not unpleaſant to many Spectators,) it may deſerve not to be wholly neglected, becauſe a Good way to break Bladders in the much Exhauſted Receiver, may ſometimes prove an uſeful Expedient, eſpecially in ſuch caſes where the Experimenter (who ſometimes either is not skilful enough, or well enough furniſh'd with accommodations to regulate the ingreſs of the Air) would very ſuddainly ſupply the Receiver with freſh Air, when it has been much emptied, without danger of letting in too much Air from without. Not to mention, that the Air, included in the Bladder to be broken, may be ſo mingled with ſteams, or imbu'd with divers qualities, as to be much fitter than common Air for ſome particular Purpoſes.

We ſhall then for the affinities ſake between this Tryal and the former, ſubjoyn now the way, by which we ſeldom fail'd of breaking Bladders in our emptied Receivers. For this purpoſe, the blown Bladder that was to be burſt, having the neck very cloſely and ſtrongly tyed, was kept a pretty while in the Receiver, whilſt the Air was pumping out, and then taken out again, that, now the fibres were ſtretcht and relax'd, the Capacity being leſſen'd by a new ligature that I order'd to be ſtrongly made near the Neck, the Bladder might be leſſen'd though the Air were but

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the ſame, and the Membrane being not ſo capable of yielding as before, upon the ſecond exhauſtion of the Receiver the Bladder in it would break, far more eaſily then otherwiſe, and perhaps be oddly enough lacerated.

We ſometimes alſo varied this way of diſpoſing Bladders to be burſt, by omitting the preparatory putting in of the Bladder into the Receiver, and onely taking it in a little near the Neck, that, the Bladder having not been blown very full at firſt, the tenſion of the included Air might be greater. But this laſt way is to be made uſe of, when the thing we deſire is, that the Bladder by breaking at a certain time may part with its Air, and not when tis onely to give an inſtance of the force of the Spring of *uncompreſſ'd* Air againſt the ſides of the Veſſel that contain it.

EXPERIMENT VIII.

About the lifting up a conſiderable Weight by the bare Spring of a little Air included in a Bladder.

YOu will eaſily believe, that the Force imploy'd (in the fore-going Experiment) by the Air, to break the well blown Bladders tis included in, is conſiderable, if I here adde, that a ſmall quantity of Air, which will not fill $\frac{1}{4}$ of a Bladder, will not onely ſerve to blow it quite up, but will manifeſtly ſwell it, though that Effect be oppoſ'd not onely by the reſiſtance of the Bladder it ſelf, but by a conſiderable weight tied to the bottom of it, as in the following Experiment.

We took a middle ſiz'd Bladder (of a Hog or Sheep,) and having preſs'd out the Air, till there remain'd but about a fourth or fifth part (by gueſs,) we cauſ'd the Neck to be very ſtrongly tyed up again: alſo round about the oppoſite part of the Bladder, within about an inch of the bottom, we ſo ſtrongly tyed another String, that it would not be made to ſlip off by a not inconſiderable weight we hung at it. Then fastning the Neck of the Bladder to the turning Key, we convey'd the Bladder and the Weight hanging at it into a large Receiver, in which when it began to be pretty well exhausted,the Air within the Bladder being freed from the wonted Pressure of the Air without it, did by its own Spring manifestly swell, and thereby notably shorten the Bladder that contain'd it, and by consequence visibly lifted up the Weight, (that resisted that change of figure,) which exceeded 15 pound of 16. ounces to the Pound.

After that we took a larger Bladder, and having let out so much Air, that it was left lank enough, we fasten'd the two ends of it to the upper part of the Receiver, (for which else it would have been too long,) and tyed a Weight (but not the same) so as that it hung down from the middle of the Bladder; then exhausting the Receiver as before, though the Bladder, and this new Weight which stretcht it, reach'd so low, as that for a while we could scarce see whether it hung in the Air or no, yet at length we perceiv'd the Bladder to swell, and concluded that it had lifted up its Clog about an Inch; which was confirm'd by the return we permitted of the Air into the Receiver, upon which the Bladder became more wrinkled than before, and the Weight descended, which being taken off, and weighed in a *Statera*, amounted to about 28 Pounds. We would have reiterated the Experiment, but so heavy a Weight having broken the Bladder, we were discouraged from proceeding any farther, especially in regard of the difficulty of bringing by this contrivance the strength of the Airs Spring to any exact computation; though it sufficiently shews what I design'd it should, namely that the Spring of a little included Air may be able even in so slight a contrivance to raise a great Weight.

Whether this Experiment may any way illustrate the motion of Muscles, made by Inflation, Contraction, &c. it belongs not to this place to consider.

EXPERIMENT IX.

About the breaking of Hermetically seal'd Bubbles of Glass by the bare Spring of their own Air.

I Shall premise to the following Tryals an Experiment, wherein Uncompress'd Air is made by its own bare Spring to break the solid body it self tis shut up in. And this I the rather set down before the subsequent Tryals, because in our already publish'd *Physico-Mechanical Experiments* mention has been made of this Tryal, as of one that we could not then make to succeed; we have since, imploying smaller Receivers, made it often enough prosperously, somewhat to the wonder of eminent *Virtuosi*, who confess'd to me they had made frequent and divers attempts to perform the same thing, without ever succeeding in any of them.

Exp. 8. pag. 36.

But it will not be requisite to multiply relations about this Particular, and therefore I shall set down but this one, which I meet with among my loose Notes.

A large Glass Bubble Hermetically seal'd being put into the Receiver, and the Air drawn out as much as in usual Operations, and somewhat more, though I told the Company before hand that I had several times observ'd, that such Bubbles would not break immediately, but somewhile after the withdrawing the Air from about them, yet this continued so long entire after we had left off Pumping, that presuming it had been blown too strong, I began to dispair of the Experiments succeeding; when, whilst we were providing something else to put into the Receiver, and as I guess'd 4. minuts after the Pump had been let alone, the Bubble surpriz'd us with its being broken with such violence by the Spring of the included Air, that the fragments of it were dash'd every way against the sides of the Receiver, and broken so very small, that when we came to take it up, the Powder was by the By-standers compar'd to the small Sand wont to be imploy'd to

dry

dry Papers, that have been newly writ upon with Inck. The Reaſon why the Bubble broke ſo ſlowly I cannot now ſtay to propoſe, no more then to examine whether the difficulty of breaking veſſels of Glaſs, no thicker then theſe Bubbles, proceed from ſome weakning of the Spring of impriſoned Air, by its ſtretching a little the including Glaſs, (for in another caſe we have obſerv'd this Glaſs to be ſtretchable by the preſſure of Air;) or from hence, that 'twas very hard, as I have elſewhere mention'd, to avoid rarifying the Air a little, and conſequently weakning its Spring, by the heat that was neceſſary to be imploy'd about the ſealing up the Bubble.

EXPERIMENT X.

Containing two or three Tryals of the force of the Spring of our Air uncompreſs'd upon ſtable and even ſolid Bodies, (whereto t'is external.)

IN proſecution of the Enquiry propos'd in the Title, we made (among others) the following Tryals.

The I. TRYAL.

1. WE took the Braſs-hoop, mention'd in the 5th Experiment, (whoſe Diameter is ſomewhat above 3. Inches,) and having cauſ'd a Glazier to cut ſome Plates of Glaſs, ſuch as are uſed for making the Quarrels of Windows, till he had brought them to a Size, & a roundneſs fit to ſerve for Covers to that braſs-hoop, we carefully faſten'd one of them with Cement to the upper Orifice of the Hoop or Ring, and then cementing the lower Orifice to the Engine, ſo that the Veſſel, compos'd of the Metal and Glaſs, ſerv'd for a ſmall Receiver; we whelm'd over it a large and ſtrong Receiver, which we alſo faſten'd on to the Engine with Cement after the uſual manner. By which Contrivance it was neceſſary, that when the Pump was ſet on work, the included Receiver (of Braſs and Glaſs) ſhould have its Air withdrawn, and yet the Air in the larger Receiver ſhould not be

pump'd out but by breaking through the Glass, so that the internal Air of the Metalline Receiver (as we may call it for distinctions sake) being pump'd out, the Glass Plate, that made part of that Receiver, must lye expos'd to the pressure of the Ambient Air shut up in the other Receiver, without having the former assistance of the now withdrawn Air to resist the Pressure; wherefore, as we expected, at the first or second Exuction of the Air, included in the small metalline Receiver, the Glass-plate was, by the Pressure of the incumbent Air, contain'd in the great Receiver, broken into an 100 pieces, which were beaten inwards into the Cavity of the Hoop.

The II. Tryal.

2. This done, to shew that there needed not the Spring of so great a quantity of included Air to break such Glasses, we took another Roundish one, which, though wide enough at the Orifice to cover the Brass-Ring & the new Glass-plate that we had cemented on it, was yet so low, that we estimated it to hold but a 6th part of what the large Receiver, formerly imploy'd, is able to contain; and having whelm'd this smaller vessel, which was shap'd like those Cups they call Tumblers, over the Metalline Receiver, and well fasten'd it to the Engine with Cement, we found that though this External Receiver had a great part of its Cavity fill'd by the included one, yet when this Internal one was exhausted by an Exuction or two, the Spring of the little Air that remain'd, was able to break the Plate into a multitude of fragments.

The III. Tryal.

3. Because the Glass-Plates hitherto mention'd seem'd not so thick, but that the Pressure of the included Air might be able to give considerabler Instances of its Force; in stead of the Metalline Receivers hitherto employed, we took a square Bottle of Glass, which we judg'd to be able to contain about a Pint (or Pound) of Water, and which had been provided to keep subtle Chymical Liquors in, for which use we are not wont to choose weak ones.

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This we inverted, and apply'd to the Engine as a Receiver, over which we whelm'd the large Receiver formerly mention'd; and having cemented it on, as in the foregoing Experiments, we set the Pump on work to empty the internal Receiver, (or square Bottle,) by which means the withdrawing of the Air, and the figure of the vessel (which was inconvenient for resisting) suffer'd the Pressure of the Air included in the external Receiver to crush the viol into a great number of pieces.

And to vary this Experiment, as we did that of breaking the metalline Receivers, we took another Glass of the shape and about the bigness of the former, and having apply'd it to the Engine as before, and cover'd it with a Receiver that was little higher than it self, we found, that upon the exhaustion of the Air the second square Glass was likewise broken into many fragments, some of which were of so great a thickness, as mov'd some wonder, that the bare Pressure of the Air was able to break such a vessel, though probably the Cracks, that reacht to them, were begun in much weaker parts of the Glass.

N.B. 1. The bottoms and the necks of both these square Bottles were entire enough; by which it seem'd probable, that the vessels had been broken by the Pressure of the Air against the Sides, which were not onely thinner than the parts above named, but expos'd a larger Superficies to the *lateral* Pressure of the Air, than to the *perpendicular*.

2. We observ'd in one of the two last Experiments, that the Vessel did not break presently upon the last Exuction that was made of the included Air, but a considerable time after, which it seems was requisite to allow the comprest parts of the Glass time to change their places; and this *Phænomenon* I therefore mention, because the same thing that here happen'd in the breaking a Glass inwards by the Spring of the Air, I elsewhere observ'd to have happen'd in breaking a Glass outwards by the same Spring.

3. To confirm, that it is the Spring of the External Receivers Air that is the Agent in those Fractures of Glasses, and to prevent

or remove ſome ſcruples, we thought fit to make this variation in the Experiment. We applyed a Plate of Glaſs, juſt like thoſe formerly mentioned, to the Braſs-hoop; but in the cementing of it on, we plac'd in the thickneſs of the Cement a ſmall Pipe of Glaſs of about an Inch long, whoſe Cavity was not ſo big as that of a Straw, and which being left open at both the ends might ſerve for a little Channel, through which the Air might paſs from the External Receiver to the Internal; over *This* we whelm'd one of the ſmall Receivers above mentioned, & then, though we ſet the Pump on work much longer then would have needed if this litle Pipe had not been made uſe of, we found, as we expected, that the Internal Receiver continued entire, becauſe the Air, whoſe Spring ſhould have broken it, having liberty to paſs through the Pipe, and conſequently to expand it ſelf into the place deſerted by the Air pump'd out, did by that Expanſion weaken its Spring too much, to retain ſtrength enough to break the Metalline (or Internal) Receiver.

But here tis to be noted, that either the Pipe muſt be made bigger than that lately mentioned, or the Exuction of the Air muſt not be made by the Pump as nimbly as we can, or otherwiſe the Plate of Glaſs may be broken notwithſtanding the Pipe; becauſe the Air contain'd in the External Receiver, having a force much greater than is neceſſary to break ſuch a Plate, it may well happen (as I have ſometimes found it do) that if the Air be haſtily drawn out of the Internal Receiver, that Air, which ſhould ſucceed in its room, cannot get faſt enough out of that external Receiver through ſo ſmall a Pipe, and the Air remaining in that external Receiver will yet retain a Spring ſtrong enough to break the Glaſs. To illuſtrate which, I ſhall propoſe this Experiment, That ſometimes, when I have at the flame of a Lamp caus'd Glaſs Bubbles to be blown with exceeding ſlender Stems, if they were nimbly remov'd out of the flame whilſt they were ignited, they would according to my conjecture be either broken, if they cool'd too faſt; or compreſs'd inward, if they long enough retain'd the

ſoftneſs

Softneſs they had given them by Fuſion. For the Air in the Bubble being exceedingly rarified and expanded, whilſt the Glaſs is kept in the flame, and coming to cool haſtily when remov'd from thence, looſes upon refrigeration the Spring the heat had given it, and ſo, if the External Air cannot preſs in faſt enough through the too ſlender Pipe, there will not get in Air enough to reſiſt the Preſſure of the Atmoſphere, and therefore if this Preſſure find the Bubble yet ſoft, it will preſs it a little inwards, and either flatten it,or make a dimple in it, though the Orifice of the Pipe be left open.

EXPERIMENT XI.

Shewing, that Mercury will in Tubes be raiſed by Suction no higher then the weight of the Atmoſphere is able to impell it up.

TIs ſufficiently known, that the common opinion of Philoſophers, and eſpecially of thoſe which follovv *Ariſtotle*,has long been, and ſtill is, that the cauſe of the Aſcenſion of Water upon Suction, and particularly in thoſe Pumps, where the Water ſeems of its own accord to follow the riſing Sucker, is Natures abhorrency of a *Vacuum*. Againſt this receiv'd Opinion divers of the Modern Philoſophers have oppoſ'd themſelves. But as ſome of them were *Vacuiſts*, and others *Pleniſts*, they have explicated the Aſcenſion of Water in Sucking-pumps upon very different grounds; ſo that many Ingenious men continue yet irreſolv'd in this noble Controverſie. Wherefore though I have formerly made, and now renew a ſolemn Profeſſion, that I do not in this Treatiſe intend to declare either for or againſt the being of a *Vacuum*; and though I have * elſewhere occaſionally acknowledg'd my Self not to acquieſce fully in what either the ancient or the modern Philoſophers have taught about the adequate cauſe of Suction ; (in the

**The place here meant is a paſſage in the Authors* Examen *of* Mr. Hobbs *his* Dialogue *about the* Air.

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assigning of which, I think, I have shown them to have been somewhat deficient,) yet since I think some Experiments, of importance to this Controversie, may be better made by the help of our Engine, than they have been by any Instrument I have yet heard of, I shall now adde the Tryals I made, to shew both that whether there be or may be a *Vacuum* or not, there is no need to have recourse to a *fuga vacui* to explicate Suction; and also that whatever other Causes have by *Gassendus* and *Cartesius* been ingeniously propos'd to explicate Suction, it seems to depend clearly upon the Weight of the Atmosphere, or in some cases upon the Spring of the Air; though I deny not, that other Causes may contribute to that Pressure of the Air, which I take to be the grand and immediate Agent in these *Phænomena*.

See Plate the Fig. the :and the Annotations at the close of this Experiment.

We took a Brass-Pipe bended like a Siphon, and fitted at the bigger end with a Stop-cock &c, as is delineated in the Figure, (which Instrument for brevities sake I often call an Exhausting (or Sucking) Siphon,) and to the slender end of this we fastned with good Cement the upper end of a Cylindrical Pipe of Glass, of about fifty inches long, and open at both ends, and having the lower end open into a Glass of stagnant Quick-silver, whose upper Superficies reacht a pretty deal higher than the immerst Orifice of the Glass Cane. These things being thus prepared, we caus'd the Pump to be set on work, whereby the Air being by degrees drawn out of the Exhausting Siphon, and consequently of the Glass-Cane that open'd into it; the stagnant Mercury was proportionably impell'd up into the Glass-pipe, till it had attain'd to its due height, which exceeded not 30. inches. And then, though there remain'd in the upper part of the Pipe above 20 inches unfill'd with Quick silver, yet we could not by further pumping raise that fluid Metal any higher.

By which it seems manifest enough, that whatever many Learned men have taught, or others do yet believe about the unlimited power that Nature would exercise, to prevent what they call a *Vacuum*; yet this power has its bounds, and thoſe depend not ſo much upon the Exigency of that Principle, which the Schoolmen call a *fuga vacui*, as upon the ſpecifick Gravity of the Liquor to be rais'd by Suction. For confirmation of which, we ſubſtituted in ſtead of the ſtagnant Mercury a baſon of Water, and though inſtead of the *many* Sucks we had fruitleſly imploy'd to raiſe the Quick-ſilver above the lately mentioned height, we now imploy'd but *one* Exſuction, (or leſs then a full one,) which did but in part empty the Exhauſting Siphon: yet the Water upon the opening of the Stop-cock was not onely impell'd to the very top of the Glaſs-Cane, but likewiſe continued running for a good while through the Exhauſting Siphon, and thence fell upon the plate of the Engine; ſo that it ſeem'd an odd ſpectacle to thoſe that knew not the reaſon of it, to ſee the Water running very briskly of its own accord as they imagined out of the ſhorter leg of a Siphon; eſpecially that leg being perhaps not above a quarter ſo long as the other. And here I muſt not omit this conſiderable circumſtance, that though ſometimes in the *Torricellian* Experiment I have obſerv'd the Mercury to ſtand at thirty inches, and now and then above it, yet the height of the Mercury elevated in our Glaſs-Cane appear'd not, when meaſured, to reach fully 29. inches and a quarter, which I thought it was not difficult to render a reaſon of, from the varying weight of the Atmoſphere; and accordingly conſulting the Baroſcope, (that ſtood in another room,) I found the Atmoſphere to be at that time ſomewhat light, the Quick-ſilver in it being in height but 29. inches and an eighth, which probably would have been the very height of the Quick-ſilver raiſ'd by the Engine, if it had had time by ſtanding to free it ſelf from Bubbles.

From whence we may conclude, that Suction will elevate liquors in Pumps no higher then the weight of the Atmoſphere is able to raiſe them, ſince the cloſeneſs requiſite in the Pump of our Engine to be ſtanch makes it very unlikely, that by any ordinary Pump a more accurate Suction can be effected.

I have nothing to adde about the related Experiment but this one; that it may afford us a notable confirmation of the argument we formerly propos'd against them, that ascrib'd the elevation and sustentation of the Quick-silver in the *Torricellian* Experiment to a certain rarified Air, which the more highly it is rarified, the greater power it acquires to attract Quick-silver, and other contiguous Bodies; for in our Experiment though by continuing to pump we can rarifie or distend more and more the Air in the Exhausting Siphon, yet we were not able to raise the Mercury above 30 inches, (which exceeds not the height to which the Atmosphere is able to elevate it,) and this, though, the stagnant Mercury being exposed to the free Air, it cannot be pretended (as in some other cases it may, though not satisfactorily, be done) that the Mercury cannot be raised higher, without offering violence to the body incumbent on the stagnant Mercury: for in the Experiment we are considering if Nature should raise the Quick-silver higher and higher in the Pipe, to succeed in the room of the Air that is withdrawn, the formerly Stagnant Mercury, that would on this occasion be rais'd, might be immediately succeeded by the free and undilated Air, so that Nature would be put to offer violence to the Quick-silver onely, which if she were scrupulous to do, what ayl'd her to raise it (as she did in our Tryal) against the inclinations of so ponderous a body, to above 29. Inches high?

Annotation.

Though the Exhausting Siphon, mentioned at the beginning of this Experiment, may be easily enough conceiv'd by an attentive inspection of the Figure, yet because I frequently make use of it in Pneumatical Experiments, twill not be amiss to intimate here once for all these three particulars about it. 1. That though the bending Pipe its self may be for some uses more conveniently made of Glass than of Metal, because the Transparency of the former may inable us to discover what passes in it; yet for the

the most part we choose to imploy Pipes of the latter sort, because the others are so very subject to break. 2. That tis convenient to make the longer leg of the Siphon a little larger at the bottom than the rest of the Pipe usually needs to be, that it may the more commodiously admit the shank of a Stop-cock, which is to be very carefully inserted with Cement; by seasonably turning and returning of which Stop-cock, the passage (for the Air) between the Engine and the Vessel to be exhausted is to be opened and shut. 3. That though we sometimes content our selves to apply immediately the brass Siphon its self to the Engine, by fastning with Cement the external shank of the Stop-cock to the Orifice of the little Pipe, through which the Exuction of the Air is made; yet the bended Pipe alone, if it be not almost constantly held, is so apt to be loosen'd by the motion of the Engine, and the turning of the Stopcock, (which frequently occasions Leaks, and disturbs the Operation,) that for the most part we make use of a Siphon consisting of a brass Pipe, and Stop-cock, and a Glass of 6, 8, or 10 Inches in height, and of some such shape (for it need not be the very same) as that represented in the Figure: for by this means, though the Exhaustion is because of this additional Glass, somewhat longer in making, yet it is more securely and uninterruptedly carried on by reason of the stability, which the breadth of the lower Orifice of the Glass gives to the whole Instrument. Besides which, we have these other conveniences, that not onely the Siphon is hereby much lengthned, which in divers Tryals is very fit; but also that we may commodiously place in the Glassie part of this compounded Syphon a Gage, whereby to discern from time to time how much the Air is drawn out of the Vessel to be exhausted.

See the Figure the plate

EXPERIMENT XII.

About the differing Heights whereto Liquors will be elevated by Suction, according to their several Specifick Gravities.

IF, when I was making the foregoing Experiment, I had been able to procure a Pipe long enough, I had tried to what height I could raise Water by Suction, though I would have done it rather to satisfie Others then my self, who scarce doubted, but that as Water is (bulk for bulk) about 14 times lighter than Quick-silver: so it would have been rais'd by Suction to about four or five and thirty foot, (which is 14 times as high as we were able to elevate the Quick-silver,) and no higher. But being not furnished for the Tryal I would have made, I thought fit to substitute another, which would carry the former Experiment somewhat further. For whereas, in That, we shew'd how high the Atmosphere was able by its whole Gravitation to raise Quick-silver; and whereas likewise that, which appears in Monsieur *Paschals* Experiment, is, at what height the whole weight of the Atmosphere can sustain a Cylinder of Water: by the way that I thought on, it would appear, (which hath not yet (that I know of) been shewn,) how a part of the *Pressure* of the Air would in perpendicular Pipes raise not onely the two mentioned Liquors, but others also to Heights answerable to the degree of Pressure, and proportionable to the specifick Gravities of the respective Liquors.

To make this Tryal the more clear and free from exceptions, I caus'd to be made and inserted to the shorter Leg of the above mentioned Exhausting Siphon a short Pipe; which brancht it self equally to the right hand and the left, as the adjoyning Figure declares. In which contrivance I aim'd at these two conveniences: one that I might exhaust two Glass-Canes at the same time; and the other, to prevent its being surmis'd that the Engine was not equally applied to both the Glasses to be exhausted. This

Plate the Fig. the

additional

additional Brass-pipe being carefully cemented into the Sucking Syphon, we did to each of its two branches take care to have well fastned with the same Cement a Cylindrical Glass of about 42 Inches in length, (that being somewhat near the height of our exhausting Syphon above the floor,) the lower Orifice of one of these two Glasses being immerst in a vessel of stagnant Mercury, and that of the other in a vessel of Water, where care was taken by those I imploy'd, that as the Tubes were chosen near of a bigness, (which yet was not necessary,) so the surfaces of the two different Liquors should be near of a height. This being done, we began to pump warily and slowly, till the Water in one of the Pipes was elevated to about 42 inches, and then measuring the height of the Quick-silver in the other Pipe above the surface of the Stagnant Quick-silver, we found it to be almost 3 Inches; so that the Water was about 14 times as high as the Quick-silver. And to prosecute the Experiment a little further, we very warily let in a little Air to the Exhausting Syphon, and had the pleasure to see the two Liquors proportionably descend, till turning the Stop-cock when the Water was about 14 inches high, we thereby kept them from sinking any lower, till we had measured the height of the Quick-silver, which we found to be about one inch.

We tried also the proportion of these two Liquors at other heights, but could not easily measure thẽ so well as we did at those newly mentioned; and therefore though there seem'd to be some slight variation, yet we lookt upon it but as what might be well imputed to the difficulty of making such Experiments exactly; and this displeas'd me not in these Tryals, that whereas it was observ'd, and somewhat wondred at, that the Quick-silver for the most part seem'd to be somewhat (though but a very little) higher then the proportion of 1 to 14 required, I had long before by particular Tryals found, that though 14 and 1 be the nearest of small *integer* numbers that express the proportion between the Specifick Gravities of Quicksilver and Water, yet the former of those Fluids (or at least that which I made my Tryals with) is not

quite

quite so heavy as this proportion supposes, though I shall not here stay to determine precisely the difference, having done it in another Tract, where the method I imployed in the investigation of it is also set down.

The above mentioned Experiment, made by the help of our Engine, as to Quick-silver and Water being confirmable by Tryals (to be by and by mentioned) made in other Liquors, affords our *Hypothesis* two considerable advantages above the vulgar doctrine of the Schools, (for I do not apply what follows to all the *Plenists*,) who ascribe the ascension of Liquors by Suction to a Traction made *ob fugam vacui*, as they are wont to speak.

For first it is manifestly agreeable to our Doctrine, that, since the Air, according to It, is a Fluid that is not void of Weight, it should raise those Liquors that are lighter, as Water, higher then those that are ponderous, as Quick-silver; and that answerably to the disparity of their Weights. And secondly, there is no reason why, if the Air be withdrawn by Suction from Quick silver and Water, there should be less left a *vacuum* above the one then above the other, in case either of them succeed not in the place deserted by the Air, and consequently when the Air is withdrawn out of both the forementioned Glass-pipes, if there would be no *vacuum* in case no liquor should succeed it, why does Nature needlesly to prevent a *vacuum* make the Water that is an heavy body ascend contrary to its own nature, according to which it tends towards the Center of the Earth? And if the succeeding of a liquor be necessary to prevent a *vacuum*, how chance that Nature does not elevate the Quick-silver as well as the Water, especially since tis manifest by the foregoing Experiment that she is able to raise that ponderous Liquor above 26 inches higher than she did in the Experiment we are now discoursing of.

Perhaps it would not be amiss to take notice, on this occasion, that among other applications of this Experiment it may be made somewhat useful to estimate the differing Gravities of liquors, to

which

wch purpose I caus'd to be put under the bottom of the forementioned Glass pipes two vessels, the one with fresh water, & the other with the like water impregnated with a good proportion of Sea-salt that I had caus'd to be dissolv'd in it, for want of Sea-water, which I would rather have imploy'd. And I found, that when the fresh water was rais'd to about 42 inches, the Saline solution had not fully reacht to 40.

But though this difference were double to that which the proportion and Gravity betwixt *our* Sea-water and fresh water would have required, yet to make the disparity more evident, and also because I would be able the better to guess at the proportion of the dissolv'd Salt by making it as great as I could, I caus'd an unusual Brine to be made, by suffering Sea-salt to deliquate in the moist Air. And having applyed this Liquor and fresh water to the two already mentioned Pipes, and proceeded after the former manner, we found that when the pure water was elevated to near 42 Inches, the liquor of Sea-salt wanted about 7. Inches and a quarter of that height; and when the water was made to subside to the middle of its Pipe, or thereabouts, the Saline liquor in the other Pipe was between 3 and 4 inches lower then it.

I would have tryed the difference between these Liquors and Oyl, but the Coldness of the Weather was unfavourable to such a Tryal: but to shew a far greater Disparity then That would have done betwixt the height of Liquors of unequal Gravities, I took fair Water, and a liquor made of the Salt of Pot-ashes suffered to run in a Sellar *per deliquium*, (this being one of the ponderousest Liquors I have prepar'd,) and having proceeded as in the former Tryals, I found that when the common Water was about 42 inches high, the newly mention'd Solution wanted somewhat of 30 inches; and when the Water was made to subside to the middle of its Pipe, or thereabouts, the deliquated Liquor was between 6 and 7 inches lower then it.

I had some thoughts, when I applied my self to make these Tryals, to examine how well we could by this new way compare

the

the Saltneſs of the waters of ſeveral Seas, and thoſe alſo of Salt-ſprings; and likewiſe whether, and (if any thing near) how far we might by this Method determine the proportion of the more ſimple Liquors that may be mingled in compounded ones, as in the mixture of Water and Wine, Vinegar and Water, &c. but being not provided with Inſtruments fit for ſuch nice Tryals, and a miſchance having impair'd the Glaſſes lately mentioned before the laſt Tryals were quite ended, and having ſoon after broken one of them, I laid aſide thoſe Thoughts.

EXPERIMENT XIII.

About the Heights to which Water and Mercury may be rais'd, proportionably to their ſpecifick Gravities, by the Spring of the Air.

IN proſecution of the Parallel formerly begun, betwixt the Effects of the Weight of the Atmoſphere, and the Spring of included Air, we thought fit after the foregoing to make the following Experiment.

We took a ſtrong Glaſs-bottle, capable to hold above a Pint of Water, and having in the bottom of it lodg'd a convenient quantity of Mercury, we pour'd on it a greater quantity of Water, (becauſe this Liquor was to be impell'd up many times higher than the other,) and having provided two ſlender Glaſs-pipes, each open at both ends, we ſo plac'd and faſtned them, by means of the Cement wherewith we choak'd the upper part of the neck of the Bottle, that the ſhorter of the Pipes had its lower Orifice immerſt beneath the ſurface of the Quick-ſilver, and the longer Pipe reacht not quite ſo low as that Surface, and ſo was immerſt but in the Water, by which contrivance we avoided the neceſſity of having two diſtinct veſſels for our two ſtagnant Liquors, which would have been inconvenient in regard of the ſlenderneſs of the upper part of our Receiver. This done, we conveyed the Bottle into a fitly shap'd Receiver, (formerly describ'd at the first Experiment,) and having begun to pump out the Air, we took notice to what heights the Quick-silver and Water were impell'd up in their respective Tubes, on which we had before made marks from inch to inch with hard Wax, (that they might not be remov'd by wet or rubbing,) and we observ'd, that when the Quicksilver was impell'd up to two inches, the Water was rais'd to about eight and twenty; and when the Quick-silver was about one inch high, the Water was about fourteen. I say, *about*, *partly* because some allowances must be made for the sinking of the Superficies of the Stagnant Quicksilver, and the greater subsidence of that of the stagnant Water, by reason of the Liquors impell'd into the two Pipes; *partly* because that the breadth of the Mark of wax was considerable, when the Quick-silver was but about an inch high, and so made it difficult to discern the exact height of the Metal, when the water was fallen down to fourteen inches: especially in regard that the Quick-silver never ascending so high as the neck of the Bottle, (which the water left far beneath it,) the thickness of the Receiver, and that of so strong a Bottle made it difficult to discern so clearly the station of the Quick-silver as I could have wished.

EXPERIMENT XIV.

About the Heights answerable to their respective Gravities, to which Mercury and Water will subside, upon the withdrawing of the Spring of the Air.

FOr the further illustration of the Doctrine propos'd in the last and some of the foregoing Experiments, about the raising and sustentation of Liquors in Pipes by the Pressure of the Air; I thought it not unfit to make the following Tryal, though it were easie to foresee in this peculiar Experiment a peculiar difficulty.

We caus'd then to be convey'd into a fitly shap'd Receiver two Pipes of Glass very uneven in length, but each of them seal'd at one end, the shorter Tube was fill'd with Mercury, and inverted into a small Glass Jarr, wherein a sufficient quantity of that Liquor had been before lodg'd: the longer Pipe was fill'd with common Water, and inverted into a larger Glass, wherein likewise a fit proportion of the same Liquor had been put.

Then the Receiver being closely cemented on to the Engine, the Air was pump'd out for a pretty while before the Mercury began to subside; but when it was so far withdrawn, that its Pressure was no longer able to keep up a Mercurial Cylinder of that height, that liquid Metal began to sink; the Water in the other Tube, though this were three times as long, still retaining its full height. But when the Quick-silver was fallen so low, as to be but between three & four inches above the surface of the Stagnant Quick-silver, the Water also began to subside, but sooner then according to the laws of meer Staticks it ought to have done, because many Aerial Particles emerging from the body of the Water to the upper part of the Glass, did by their Spring concurr with the Gravity of the water to depress this Liquor. And so when the Quick-silver was three inches above the stagnant Mercury, the water in the other Pipe was fallen divers inches beneath 42, and several inches beneath 28 when the Mercury had subsided an inch lower. But this being no more then was to be expected, after we had caus'd the Pumping to be a while continued, to free the water the better from the latitant Air, we let in the external Air, and having thereby impell'd up again both the Liquors into their Pipes, and remov'd the Receiver we took out those Pipes, and inverting each of them again to let out the Air, (for even that wich held the Quick-silver had got a small Bubble, though inconsiderable in comparison of the Air that had got up out of the Water,) we fill'd each of them with a little of the restagnant Liquor belonging to it, and inverting each Tube once more into its proper liquor, we repeated the Experiment, and found it, as it

seem'd,

seem'd, to require more pumping then before to make the Liquors begin to subside; so that when the Mercury was fallen to three inches, or two, or one, the water subsided so near to the heights of 42, 28, or 14 inches, that we saw no sufficient cause to hinder us from supposing, that the litle differences that appear'd between the several heights of the Quick-silver, and fourteen times as great heights of the Water (which fell somewhat lower than its proportion in Gravity required) proceeded from some Aerial Corpuscles yet remaining, in spite of all we had done, in the water, and by their Spring, though but faint, when once they had emerg'd to the upper part of the Glass, furthering a little the depression of it: not now to mention lesser Circumstances, particularly, that the surface of the stagnant Water did not inconsiderably rise by the accession of the Water lately in the Pipe; whereby the Cylinder of water, rais'd above that surface, became by so much the shorter. However Your Lordship may, if You think fit, cause the Experiment to be reiterated, which I could not so well do, by reason of a mischance that befell the Receiver.

EXPERIMENT XV.

About the greatest height to which Water can be rais'd by Attraction or Sucking Pumps.

SInce the making and the writing of the foregoing Experiments, having met with an opportunity to borrow a place somewhat convenient to make a Tryal to what height Water may be rais'd by Pumping; I thought not fit to neglect it. For though both by the consideration of our *Hypothesis*, to whose truth so many *Phænomena* bear witness; and though particularly by the Consequences deduceable from the three last recited Experiments I were kept from doubting what the event would be, yet I thought it worth while to make the Tryal.

I know what is said to have been the Complaint of some Pump-makers. But I confess the *Phænomenon*, 'twas grounded on, seem'd not to me to be certainly enough deliver'd by a Writer or two, that mention what they complain'd of; and their observation seems not to have been made determinately or carefully enough for a matter of this moment. Since that which they complain of seems to have been in general, that they could not by pumping raise Water to what height they please, as the common Opinion of Philosophers about Natures *fuga vacui* made them expect they might. And it may well have happen'd, that as they endeavoured onely to raise it to the height their occasions required, so all that their Disappointment manifested, was, that they could not raise it to that particular height: which did not determine, whether if the Pump had been a Foot or a Yard shorter, the Water would then have been elevated to the upper part of it or no: but that which I chiefly consider is, that these being but Tradesmen, that did not work according to the Dictates of, or with design to satisfie, a Philosophical Curiosity, we may justly suspect, that their Pumps were not sufficiently stanch, nor the Operation Critically enough perform'd and taken notice of.

Wherefore, *partly* because a Tryal of such moment seem'd not to have yet been duely made by any; and *partly* because the varying weight of the Atmosphere was not (that appears) known, nor (consequently) taken into consideration by the ingenious Monsieur *Paschal* in his famous Experiment, which yet is but analogous to this; and *partly* because some very Late as well as Learned Writers have not acquiesc'd in his Experiment, but do adhere to the old Doctrine of the Schools, which would have Water raiseable in Pumps to any height, *ob fugam vacui*, (as they speak,) I thought fit to make the best shift I could to make the Tryal, of which I now proceed to give Your Lordship an Account.

The place I borrowed for this purpose was a flat Roof about 30 foot high from the ground, and with Railes along the edges of it. The Tube we made use of should have been of Glass, if we could have procured one long and strong enough. But that being exceeding difficult, especially for me, who was not near a Glass-house, we were fain to cause a Tin-man to make several Pipes of above an inch bore, (for of a great length 'twas alleadg'd they could not be made slenderer,) and as long as he could, of Tin or Laton, as they call thin Plates of Iron Tinn'd over; and these being very carefully soder'd together made up one Pipe, of about one or two and thirty foot long, which being tied to a Pole we tried with Water whether it were stanch, and by the effluxions of that Liquor finding where the Leaks were, we caus'd them to be stopt with Soder, and then for greater security the whole Pipe, especially at the Commissures, was diligently cas'd over with our close black Cement, upon which Plaister of Paris was strewed to keep it from sticking to their hands or cloaths that should manage the Pipe. At the upper part of which was very carefully fastned with the like Cement a strong Pipe of Glass, of between 2 and 3 foot in length, that we might see what should happen at the top of the water. And to the upper part of this Pipe was (with Cement, and by the means of a short elbow of Tin) very closely fastned another Pipe of the same Metal, consisting of two pieces, making a right Angle with one another, whereof the upper part was parallel to the Horizon, and the other, which was parallel to the Glass-pipe, reacht down to the Engine, which was plac'd on the flat Roof, and was to be with good Cement sollicitously fastned to the lower end of this descending part of the Pipe, whose Horizontal leg was supported by a piece of Wood, nail'd to the above mentioned Rails; as the Tube also was kept from overmuch shaking by a board, (fasten'd to the same Rails,) and having a deep Notch cut in it, for the Tube to be inserted into.

This *Apparatus* being made, and the whole Tube with its Pole erected along the Wall, and fastned with strings and other helps, and the descending Pipe being carefully cemented on to

See the plate the Figure.

to the Engine, there was plac'd under the bottom of the long Tube a convenient veſſel, whereinto ſo much Water was poured, as reach'd a great way above the orifice of the Pipe, and one was appointed to ſtand by to pour in more as need ſhould require, that the veſſel might be ſtill kept competently full.

After all this the Pump was ſet on work, but when the water had been raiſed to a great height, and conſequently had a great Preſſure againſt the ſides of the Tube, a ſmall Leak or two was either diſcovered or made, which without moving the Tube we caus'd to be well ſtopt, by one that was ſent up a Ladder to apply ſtore of Cement where it was requiſite.

Wherefore at length we were able after a pretty number of Exuctions, to raiſe the Water to the middle of the Glaſs-pipe abovementioned, but not without great ſtore of bubbles, (made by the Air formerly conceal'd in the pores of the water, and now emerging,) which for a pretty while kept a kind of Foam upon the ſurface of it, (freſh ones continually ſucceeding thoſe that broke.) And finding the Engine and Tube as ſtanch as could be well expected, I thought it a fit ſeaſon to trie what was the utmoſt height to which Water could by Suction be elevated; and therefore though the Pump ſeem'd to have been plyed enough already; yet for further ſatisfaction, when the Water was within few inches of the top of the Glaſs, I caus'd 20 Exuctions more to be nimbly made, to be ſure that the water ſhould be raiſed as high as by our Pump it could be poſſibly. And having taken notice where the Surface reſted, and caus'd a piece of Cement to be ſtuck near it, (for we could not then come to reach it exactly,) and deſcending to the Ground where the ſtagnant water ſtood, we caus'd a ſtring to be let down, with a weight hanging at the end of it, which we applied to a mark, that had been purpoſely made at that part of the (Metalline) Tube, which the ſuperficies of the ſtagnant water had reſted at, when the water was elevated to its full height: and the other end of the ſtring being, by him that let it down, applied to that part of the Glaſs, as near as he could gueſs, where

where the upper part of the Water reacht, the Weight was pull'd up; and the length of the ſtring, and (conſequently) the height of the Cylinder of Water was meaſur'd, which amounted to 33 foot, and about 6 inches. Which done, I return'd to my lodging, which was not far off, to look upon the Baroſcope, to be informed of the preſent weight of the Atmoſphere, which I found to be but moderate, the Quick-ſilver ſtanding at 29 inches, and between 2 and 3 eights of an inch. This being taken notice of, it was not difficult to compare the ſucceſs of the Experiment with our *Hypotheſis*. For if we ſuppoſe the moſt received proportion in bulk between Cylinders of Quick-ſilver and of Water of the ſame weight, namely that of 1 to 14, the height of the water ought to have been 34 foot and about two inches, which is about 8 inches greater than we found it. But then Your Lordſhip may be pleaſed to remember, that I formerly noted (before ever I made this Experiment) that I did not allow the proportion betwixt Mercury and Water (at leaſt ſuch water as I made my Tryals with) to be altogether ſo great, and though in ordinary Experiments we may with very litle inconvenience make uſe of that proportion to avoid fractions, yet in ſo tall a Cylinder of Water as ours was, the difference is too conſiderable to be neglected. If therefore in ſtead of making an Inch of Quick-ſilver equivalent to 14 inches of Water, we abate but a quarter of an inch, which is but a 56 part of the height of the Water, this abatement being repeated 29 times and a quarter, will amount to 7 inches, and above a quarter, which added to the former height of the Water, namely 33 Foot and 6 inches, will make up 34 foot and above an inch; ſo that the difference between the height of the Mercury ſuſtain'd by the weight of the Atmoſphere in the Baroſcope, and that of the Water rais'd and ſuſtain'd by the Preſſure of the ſame Atmoſphere in the long Tube did not appear to differ more than an Inch or two from the proportion they ought to have had, according to the difference of their ſpecifick Gravities. And though in our Experiment the difference had been

greater,

greater, provided it exceeded not 8 or 10 Inches, it would not have been ſtrange: *partly*, becauſe of the difficulty of meaſuring all things ſo exactly in ſuch an Experiment, *partly* becauſe as Waters are not all of the ſame weight, ſoa little diſparity of it in ſo long a Cylinder may be conſiderable, and *partly* (and perhaps chiefly) becauſe the Air flying out of the bubbles, that roſe out of ſo great a quantity of water, and breaking at the top of it, and ſo near that of the Tube, might by its Spring (though but very weak) aſſiſting the weight of ſo much water, ſomewhat (though not much) hinder the *utmoſt* elevation of that Liquor. But our Experiment did not make it needful for me to inſiſt on theſe conſiderations, and the inconſiderable difference that was betwixt the height of the water we found, and that which might have been wiſh'd, did rather countenance then at all disfavour the thing to be made out by our Experiment, ſince by no Pumping we could raiſe the Water *quite* ſo high (though I confeſs it wanted but very little) as the weight of the Atmoſphere was able to keep up a Cylinder of Mercury proportionable to it in height, and equivalent in weight: and yet I preſume, Your Lordſhip will eaſily grant, that there was at leaſt as much care uſed in this Experiment, to keep the things imploy'd about it tight, as has been wont to be uſed by Tradeſmen in their Pumps, where tis not ſo eaſie either to prevent a little inſinuation of the Air, or to diſcern it.

Tis not that I am ſure, that even all our care would have kept the water for any long time at its full height; but, that the Air was ſufficiently exhauſted for our purpoſe, when we determin'd the height of the water, I was induc'd to conclude by theſe Circumſtances.

1. As well the conſtruction of the Engine, as the many (formerly related) Experiments, that have been ſucceſsfully tryed with it, ſhew that tis not like it ſhould be inferiour in cloſeneſs to the great Water-Pumps, made by ordinary Tradeſmen: and particularly

ticularly the XI. Experiment foregoing, manifeſts, that by this Pump Quick-ſilver was rais'd to as great a height, as the Atmoſphere is able to ſupport in the *Torricellian* Experiment.

2. The ſtanchneſs of the Pipe appear'd by the Diminution (as to number) of Bubbles, that appear'd at the top of the Water, and by their ſize too, for when there was a leak, (though but ſo very ſmall, that the water could not get out at it in the Tube,) it might uſually be taken notice of by the attentive ear of him that ſtood to watch upon the Ladder, erected by the ſide of the Tube; and the Air that got in, did eaſily diſcover it ſelf to the Eye by large Bubbles, manifeſtly differing from thoſe that came from the Aerial particles belonging to the water; and if the leak were not ſo very ſmall, the Air that got in would ſuddenly lift up the water above it, and perhaps fill with it the deſcending Pipe.

3. Though there had been ſome imperceptible Leak, yet that would not have hindred the ſucceſs of the Experiment for the main. For in leaks that have been but ſmall, though manifeſt enough, we have often, by cauſing the Pump to be ply'd leſs nimbly then it now was, been able to proſecute our Tryals; becauſe the Pump carried off ſtill more Air than could get in at a leak that was no greater.

4. And that litle or no (intruding) Air was left in the upper part of our Tube, was evident by thoſe marks, whereby it was eaſie for them that are well acquainted with the Pump, to eſtimate what Air is left in the veſſel it ſhould exhauſt, and particularly towards the end of our operation I obſerv'd, that when the Sucker was depreſt, there came out of the Water that cover'd the Pump, ſo very few bubbles, that they might be imputed to the Air afforded by the Bubbles, ſpringing from the water in the Tube; whereas if any adventitious Air had got into that Cylinder of water, it would have appear'd in the water that cover'd the Pump.

5. Laſtly, it were very ſtrange, that if the water was but caſu-

ally hindred by ſome Leak from aſcending any higher, it ſhould be ſo eaſy to raiſe it to the very number of feet that our *Hypotheſis* requires, and yet we ſhould be unable by obſtinate Pumping to raiſe it one foot higher.

Note, 1. as ſoon as we had made our Experiment,and thereby found, that what was requiſite to it was in order; I ſent to give notice of it to Dr *Wallis*,and Dr *Wren*, as Perſons whoſe curioſity makes them as well delighted with ſuch Tryals, as their deep knowledg makes Them moſt competent Judges of them. But before They could be found, and come, it being grown ſomewhat late and windy, I that was not very well, and had tired my ſelf with going up and down, could not ſtay with them ſo long as I intended, but leaving the reſt of the Repeated Experiment to be ſhewn them by *I. M.* (who had been very induſtrious in fitting and erecting the Tube) they and their Learned friend (whom they brought with them) Doctor *Millington*, told me a while after, that they alſo had found the greateſt height, to which they could raiſe the water, to be 33 foot and an half.

2. When the Water began firſt to appear in the Glaſs, the Bubbles would be, as I had foretold, exceeding numerous; ſo as to make a froath of near a foot high, if the water were newly brought, and had never been rais'd in the Tube before. But if the Pumping were long continued, the number and height (or at leaſt one of the two) of the Aggregate of Bubbles, would (as there remain'd fewer and fewer Aerial particles in the water) be leſſer and leſſer; but their emerging did never that I remember wholly ceaſe.

3. At the beginning alſo there would appear great vibrations of the water in the upper part of the Tube; the riſing and the falling amounting ſometimes to a foot, or near half a yard: but theſe grew leſſer and leſſer, as thoſe of the Quickſilver in the *Torricellian* Experiment uſe to do.

4. One may uſe an ordinary Pail to hold the ſtagnant water; but we rather imploy'd a veſſel of Earth made (for another purpoſe

pose) somewhat slender, and of a Cylindrical shape, because in a narrow vessel tis more easie to guess by the rising and falling of the Liquor, how the Pump is ply'd, and to perceive even smaller Leaks.

5. I must not forget to take notice, that though the newly nam'd Gentlemen came to me (when they had seen the Experiment tryed) within less than an hour after the time I had look'd upon the Baroscope, and observ'd the Quick-silver to stand somewhat beneath 29 inches, and 3 eighths; yet when presently upon their return I consulted the same instrument again, the Mercury appear'd to be sensibly risen, being somewhat (though but very litle) above 9 and 20 inches, and 3 eights, and 5 or 6 hours after (at bed-time) I found it to be yet more considerably risen. Which may keep Your Lordship from wondring at what I intimated a little above, touching Monsieur *Paschal's* Experiment, as well as touching the disappointment of the Pump-makers endeavours. For tis not onely possible, that (as I have elsewhere noted) Water may be raised in the same Pump (though we suppose it still equally stanch) higher at one time than at another: but 'twas contingent, that, in Monsieur *Paschal's* noble attempt to imitate the *Torricellian* Experiment with Water in stead of Quick-silver, the proportion betwixt the heights of those two Liquors in their respective Tubes answer'd so well to their specifick Gravities. For, the varying weight of the Atmosphere being not then (that appears) known, or consequently taken into consideration; if Monsieur *Paschal*, having tryed the *Torricellian* Experiment, when the Air was for instance very heavy, had tryed his own Experiment, when the Atmosphere had been as light as I have often enough observ'd it to be, he might have found his Cylinder of Water to have been half a Yard or two foot shorter than the formerly measur'd height of the Quick-silver would have required.

I have now no more to adde about this 15th Experiment, but that it may serve for a sufficient confirmation of what I note in another Treatise, against those Hydraulical & Pneumatical Writers, who pretend to teach wayes of making Water pass by inflected Pipes, and by the help of Suction, from one side of a Mountain to the other, be the Mountain never so high. For, if the Water be to ascend as 'twere spontaneously above 35 or 36 foot, a Sucking Pump will not ordinarily, at least here in *England*, be able to raise it.

And now I speak of Mountains, it will not be altogether impertinent to add, that if it had not been for unseasonable weather, I had thought fit to make the foregoing 11th Experiment (of elevating Mercury by Suction) to be tryed at the top of an Hill, not far from the place I then was at. For by what has been already delivered, it appears, that we might have estimated the height, to which the Water may be there elevated by Suction, without repeating the Experiment with a thirty five foot Tube, (which we could not hope for conveniency to do,) by the utmost height to which our Engine could have rais'd Mercury: and it may be of some use to be able from Experiments to make *some* estimate (for it can scarce be an accurate one) how much it may be expected, that Pumps shall (*cæteris paribus*) loose of their power of elevating Water by Suction, by being imploy'd at the top of an Hill, in stead of being so at the bottom, or on a Plain. Remembring always what I lately intimated, that even in the same place Liquors will be brought to ascend by Suction to a greater or less height at one time than another, according to the varying Gravity of the Atmosphere.

EXPERIMENT XVI.

About the bending of a Springy Body in the Exhausted Receiver.

THe cause of the Motion of Restitution in Bodies, and consequently of that which makes some of them Springy, which far the greater part of them are not, has been ingeniouſly attempted by ſome Modern Corpuſcularians, and eſpecially *Carteſians*; but ſince divers Learned and Judicious men do ſtill look upon the cauſe of Elaſticity, as a thing that needs to be yet farther enquired into; and becauſe I am not my ſelf ſo well ſatisfied as to blame their Curioſity, I held it not unfit to examine by the help of our Engine their Conjecture, who imagine that the Air may have a great ſtroak in the making of bodies Springy; and this I the rather did, becauſe I had * elſwhere ſhewn, that there is no need to aſſert, that in all Bodies, that have it, the Elaſtical power flows immediately from the Form, but that in divers of them it depends upon the Mechanical ſtructure of the Body.

* *In Notes about the hiſtory of Elaſticity.*

To make ſome Tryal therefore, whether the Air have any great Intereſt in the Motion of Reſtitution, we took a piece of Whalebone of a convenient bigneſs and length, and having faſten'd one end of it in a hole made in a thick and heavy Trencher, to be placed on the Plate of the Engine, we tyed to the other end a Weight, whereby the Whalebone was moderately bent, the weight reaching down ſo near to a Body plac'd in a level poſition under it, that if the Spring were but a little weaken'd, the weight muſt either lean upon, or at leaſt touch the Horizontal plain: or if on the other ſide the Spring ſhould grow ſenſibly ſtronger, it might be eaſily perceiv'd by the diſtance of the weight, which was ſo near the plain, that a litle increaſe of it muſt be viſible.

This done, we convey'd theſe things into the Receiver, and order'd thoſe that pump'd to ſhake it as litle as they could, that the weight might not knock againſt the Body that lay under it, or ſo ſhake it, as to hinder us from diſcerning whether or no it were depreſs'd by the bare withdrawing of the Air.

And when the Air had been well pump'd out, I watcht attentively whether any notable Change in the diſtance of the weight from the almoſt contiguous plain would be produc'd upon its being let in again: for the weight was then at reſt, and the return-

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ing Air flowing in much more ſpeedily than it could before, be drawn out, I thought this the likelieſt time to diſcover whether the abſence of the Air had ſenſibly altered the Spring of the Whalebone. But though the Experiment were made more than once, I could ſatisfie my ſelf onely in this, that the depreſſion or elevation of the Weight, that was due to the true and meer change of the Spring, was not very *conſiderable*, ſince I did not think my ſelf ſure, that I perceiv'd any at all: for though it be true, that ſometimes, when the Receiver was well exhauſted, the Weight ſeem'd to be a little depreſt, yet That I thought was very litle, if any thing more than what might be aſcrib'd to the abſence of the Air, not conſider'd as a Body that had any thing to do directly with the Spring, but as a Body that had ſome (though but a litle) Weight; upon which account it made the *medium*, wherein the Experiment was tried, contribute to ſupport the Weight that bent the Spring; which Weight, when the Air was abſent, muſt (being now in a lighter *medium*) have its Gravitation increas'd by as much weight, as a quantity of the exhauſted Air, equal to it in bulk, could amount to. But this Experiment being tried onely with VVhalebone, and in a Receiver not very Great, may deſerve to be further tryed in taller Glaſſes, with Springs of other kinds, and by the motions of a VVatch, and other more artificial Contrivances.

EXPERIMENT XVII.

About the making of Mercurial, and other Gages, whereby to eſtimate how the Receiver is exhauſted.

BEcauſe the Air being inviſible, it is not always eaſie to know whether it be ſufficiently pump'd out of the Receiver that was to be exhauſted; we thought it would be very convenient to have ſome Inſtrument within the Receiver, that might ſerve for

for a Gage, or Standard; whereby to judge whether or no it were sufficiently exhausted.

To this purpose divers Expedients were thought on, and some of them put in practise; which, though not equally commodious, may yet all of them be usefully imploy'd, one on this occasion, and another on that.

The First (if I misremember not) that I propos'd, was a Bladder, (which may be greater or less, according to the Size of the Vessel it is to serve for) to be very strongly tied at the neck, after having had onely so much Air left in the folds of it, as may serve to blow up the Bladder to its full dimensions, when the Receiver is very well exhausted, and not before. But though Your Lordship will hereafter find that I yet make use of small Bladders on certain occasions, in which they are peculiarly convenient, yet in many cases they do, when the Glasses are well exhausted, take up too much room in them, and hinder the Objects, included in the Receiver, from being observ'd from all the sides of it.

Another sort of Gage was made with Quick-silver, pour'd into a very short Pipe, which was afterwards inverted into a litle Glass of stagnant Quick-silver, according to the manner of the *Torricellian* Experiment. For this Pipe being but a very few inches long, the Mercury in it would not begin to descend, till a very Great proportion of Air was pump'd out of the Receiver; because till then, the Spring of the remaining Air would be strong enough to be able to keep up so short a Cylinder of Mercury. And this kind of Gage is no bad one. But because, to omit some other litle inconveniences, it cannot easily be suspended, (which in divers Experiments 'tis fit the Gage should be,) and the Mercury in it is apt to be too much shaken by the motion of the Engine, there was another kind of Gage by some Ingenious man (who ever he were) substituted in its place, consisting of a kind of Siphon, whose shorter leg hath belonging to it a large Bubble of Glass, most commonly made use of at an Illustrious meeting of *Virtuosi*; where Your Lordship having seen it, I shall not need to describe it more particularly.

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But none of the Gages I had formerly us'd, nor even this last, having the conveniences that some of my Experiments require; I was fain to devise another, which is That I most make use of, as having advantages, some or other of which each of the Gages already mentioned wants; for even that with Spirit of Wine, not to mention lesser disadvantages, hath a Bubble too Great to let it be useful in vessels so slender, as for some purposes I divers times imploy; and this short Cylinder of so light a Liquor as spirit of Wine, makes the subsidence of the Liquor be indeed a good sign that the Receiver is well exhausted, but gives us not an account what Quantity of Air may be in the Receiver; 'till it be arriv'd at that great measure of Rarefaction; and the same Liquor, being upon a very small leak. (such as would not be prejudicial to many Experiments) impell'd up to the top of the Gage, we cannot afterwards by this Instrument take any measure of the Air that gets in at the Leak. But now there are divers Experiments where I desire to see the *Phænomena* that will happen, not onely (or perhaps not at all) upon the uttermost Exhaustion of the Air, but when the Pressure of it is withdrawn to such or such a measure, and also when the Air is gradually readmitted.

To make the Gage we are speaking of, take a very slender and Cylindrical Pipe of Glass, of 6, 8, 10, or more Inches in length, and not so big as a Goose-quill, (but such as we imploy for the Stems of seal'd Weather-Glasses,) and having at the flame of a Lamp melted it, but not too near the middle, to make of it by bending it a Siphon, whose two Legs are to be not onely parallel to one another, but as litle distant any where from one another as conveniently may be. In one (which is usually the longer) of these Legs, there is to be left at the top, either half an inch, or a whole inch, or more or less than either, (according to the length of the Gage, or the scope of the Experimenter) of Air in its natural state, neither rarefied, nor condens'd; the rest of the longer leg, and as great a part of the shorter as shall be thought fit, being to be fill'd with Quick-silver. This done, there may be Marks

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plac'd at the outſide of the longer (or ſealed)leg, whereby to meaſure the Expanſion of the Air included in the ſame leg, and theſe marks may be either litle Glaſs Knubs, about the bigneſs of Pins heads, faſten'd by the help of a Lamp at certain diſtances to the longer leg of the Siphon, or elſe the diviſions of an Inch made on a liſt of Paper, and paſted on either to the Siphon it ſelf, or to the ſlender Frame, which on ſome occaſions we faſten the Gage to.

This Inſtrument being convey'd into a Receiver, (which for expedition ſake we chooſe as ſmall as will ſerve the turn,) the Air is to be very diligently pump'd out, and then notice is to be taken to what part of the Gage the Mercury is depreſt, that we may know, when we ſhall afterwards ſee the Mercury driven ſo far, that the Receiver, the Gage is plac'd in, is well exhauſted. And if it be much deſired to know more accurately (for one may arrive pretty near the truth by Gueſs) what ſtations of the Mercury in the Gage are anſwerable to the degrees of the Rarefaction of the Air in the Receiver; that may be compaſſed either by Calculation, (which is not ſo eaſie, and ſuppoſes ſome *Hypotheſes*,) or (though not without ſome trouble) by letting in the water as often as is neceſſary, into a Receiver, whoſe intire capacity is firſt meaſured, and in which there may be Marks made to ſhew when the water to be let in ſhall fill a fourth part, or half, or three quarters &c. of the Cavity. For if (for inſtance) when the Quickſilver in the Gage is depreſt to ſuch a Mark, you let in the water, and that Liquor appears to fill a fourth part of the Receiver, you may conclude, that about a 4th part of the Air was pump'd out, or that a 4th part of the Spring, that the whole included Air had, was loſt by the Exhauſtion, when the Quick-ſilver in the Gage was at the Mark above mentioned; & if the admitted water do conſiderably either fall ſhort of, or exceed the quantity you expected, you may the next time let in the water either after the Mercury has a litle paſt the former Mark, or a litle before it is arriv'd at it. And when once you have this way obtain'd one pretty

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long and accurate Gage, you will not need to take ſo much pains to make others, ſince you may divide them by the help of that one; for this being plac'd with any other in a ſmall Receiver, when the Mercury in the Standard-Gage (if I may ſo call it) is depreſt to any of the determinate diviſions obtain'd by obſervation, you may thence conclude how much the Air in the Receiver is rarefied, and conſequently by taking notice of the place where the Mercury reſts in the other Gage, you may determine what degree of Exhauſtion in a Receiver is denoted by that ſtation of the Mercury in this Gage.

Perhaps I need not tell your Lordſhip that the Ground of this contrivance was, that whereas in divers other Gages, when the Pump came to be obſtinately ply'd, the Expanſion of the included Air would be ſo great, that it would either drive out the Liquor, eſpecially if it were light, or in part make an eſcape through it: I judg'd that in ſuch an Inſtrument, as that newly deſcrib'd, thoſe inconveniences would be avoided, becauſe that the more the Air ſhould come to be dilated, the greater weight of Quickſilver it would in the ſhorter Leg have to raiſe, which would ſufficiently hinder it from making that heavy liquor run over; and the ſame ponderouſneſs of the Liquor, together with the ſlenderneſs of the Pipe, would likewiſe hinder the included Air from getting through in Bubbles.

NB. 1. For moſt Experiments, where exact meaſures are not required, it will not be ſo neceſſary to mark the Gage at any other ſtation of the Quick-ſilver then that which tis brought to by the Exhauſtion of the Receiver, for by that alone we may know when the Air is well pump'd out of the Receiver, wherein the Gage is included: and when one is a litle uſ'd to ſome particular Gage, one may by the ſubſidence of the Mercury gueſs at the degree of the Airs rarefaction, ſo near as may ſerve the turn in ſuch Experiments. But when this Inſtrument is to be us'd about nice Tryals, where it may be thought requiſite to have it divided according to one of the ways formerly propoſed, it will on divers

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occaſions be more ſecure (in caſe the maker of the Gage has skill to do it,) to put to the Diviſions rather by litle Knubs of Glaſs, than by Paper; becauſe this will on ſuch occaſions be in danger either to be rubb'd off, or wetted. And if Glaſs-marks be us'd, it will be convenient that every fifth, or tenth, or ſuch Ordinal number as ſhall be judg'd fit, be made of Glaſs of a differing colour, for diſtinction ſake, & the more eaſie reckoning. We ſometimes for a need apply, in ſtead of theſe Glaſs-knubs, little marks of hard ſealing Wax, which will not be injur'd by moiſture, as thoſe Papers will that are paſted on; but theſe of Wax, though in many caſes uſeful, are not comparable to the other in all, ſince if they be very ſmall, they are eaſily rubb'd off, and if large, they make not the Diviſion exact enough, and often hide the true place of the Quick-ſilver.

I ſhall here about the Mercurial Gages add onely this Hint, that what I propos'd to my ſelf in that Contrivance, was not onely to eſtimate the Air pump'd out of the Receiver, or that remaining in it; but alſo, by the help of this Inſtrument (as elſewhere by another Experiment) to meaſure (ſomewhat near) the ſtrength of the Spring of rarefied Air, according to its ſeveral degrees of Rarefaction; and by this Obſervation, in concurrence with other things, I hoped we might (according to what I have elſewhere inſinuated) be aſſiſted to eſtimate, by the Cylinder of Mercury rais'd in the open leg, the Expanſion of the Air included in the ſealed leg: but of theſe things I deſign'd in this place to give but an Intimation.

3. That leg of the Gage that includes the Air, may be ſeal'd up either at the beginning, before the Pipe be bent into a Syphon, or (which is much better) after the following manner. Before you bend the Pipe, draw out the end of it, which you mean to ſeal, to a ſhort and very ſlender Thread; then having made the Pipe a Siphon, pour into the leg, which is to remain open, as much Quick-ſilver as you ſhall judg convenient, which will riſe to an equal height in the other leg; out of which by gently inclining

the Siphon, you may pour out the superfluous Mercury, (if there be any,) and when you see that there is an inch, or half an inch (or what part you design'd to leave for Air) unfill'd with Mercury, next to the end that is to be clos'd; and that the rest of that leg, and as much (as you think fit) of the other is full of Quick-silver, you may, by keeping the Siphon in the same posture, and warily applying the slender *Apex* above mentioned to the upper part of the flame of a Lamp, blown Horizontal, easily seal up that *Apex* without cracking, or prejudicing the open leg, or considerably injuring the Air hole, that was to be seal'd up in the other. And this sealing of one leg must (as tis evident) keep the Mercury suspended in it, though it be higher by divers inches than that in the open leg, till the withdrawing of the external Air enable the included, by expanding it self to depress the Mercury in the seal'd leg, and raise it in the open.

4. How the length of these Mercurial Gages is to be varied, according to the Bigness and Shape of the slender Receivers they are to be imploy'd in, and how they may easily be made either to stand upright at the bottom of the Receiver, or be kept hanging in the middle, or near the top of it (as occasion may require,) and how the open end may be made to secure the Mercury, in cases where that is needful, belongs not so properly to this Treatise, as to the Second part of the Continuation; where, if ever I trouble Your Lordship with it, the Usefulness of this sort of Gages, and the Circumstances that may advantage them, will best appear.

5. There being some Experiments, wherein it is not desir'd that the Receiver should be neer exhausted, but rather that the degrees of the Airs rarefaction, which ought not to be very great, should be well measur'd; we may in such cases make use of Gages shap'd like those hitherto describ'd, but made as long as the Receiver will well admit, and furnish'd in stead of Quick-silver *either* with spirit of Wine coloured with Cocheneel, *or else* with the tincture of red Rose-leaves, drawn onely with common Water,

made

made ſharp by a litle either of the Oyl, or the ſpirit of Vitriol, or of common Salt. For the lightneſs of theſe Liquors in compariſon of Quick-ſilver will allow the Expanſions of the Air included in the Gage to be very manifeſt, and notable enough, though not half, or perhaps a quarter of the Air be pump'd out of the Receiver.

6. You may alſo in ſuch caſes as theſe, where the Receiver is large enough, and is not to be quite exhauſted, make uſe of a Mercurial Gage, differing from thoſe above deſcrib'd onely in this, that the ſhorter leg need not be above an inch, or half an inch long, before it expand it ſelf into a Bubble of about half an inch, or an inch in Diameter; and having at the upper part a very ſhort and ſlender unſeal'd Pipe, at which the Air may get in and out: by which Contrivance you may have this Convenience, that You need not include ſo much Air, as otherwiſe would be requiſite, at the top of the longer Leg, becauſe the Mercury in the ſhorter cannot, by reaſon of the breadth of the Bubble, whereinto the Expanſion of the Air drives it, be conſiderably rais'd: Upon which account it becomes more eaſie to eſtimate by the Eye the degrees of the included Airs Rarefaction, which may be done almoſt as eaſily, as if there were water in ſtead of Mercury: provided it be remembred, that Quick-ſilver by reaſon of its ponderouſneſs, does far more aſſiſt the dilatation of the Air, then ſo much Water would do.

EXPERIMENT XVIII.

About an eaſie way to make the Preſſure of the Air ſenſible to the Touch of thoſe that doubt of it.

THough ſeveral of our Experiments ſufficiently manifeſt to the Skilful, that the Preſſure of the Air is very conſiderable; yet becauſe ſome of them require peculiar Glaſſes, and o-

ther

ther Inſtruments, which are not always at hand, and becauſe there are many that think it ſurer to eſtimate the force of Preſſure by what they immediately feel, than by any other way; I was invited for the ſake of ſuch to imploy an eaſie Experiment, which uſually proved convincing, becauſe it operated on that Senſe, whereon they chiefly rely'd.

I caus'd then to be made a hollow (but ſtrong) piece of Braſs, not above two or three inches high, (that it might be in a trice exhauſted,) and open at both ends, whoſe Orifices were Circular and parallel, but not equal, (the Inſtrument being made tapering, ſo that it might be repreſented by an excavated *Conus truncatus*, or a Gigg, with the lower part cut tranſverſly off.) This piece of Braſs being cemented on, as if it were a ſmall Receiver to the Engine, the Perſon, that would not believe the Preſſure of the Air to be near ſo conſiderable as was repreſented, was bidden to lay the Palm of his Hand upon the upper Orifice; and being ordered to lean a little upon it, that ſo the lower part of his hand might prove a cloſe Cover to the Orifice, one Exuction of the Air was made by the help of the Pump: and then upon the withdrawing of the greateſt part of the Preſſure of the internal Air, that before counterballanc'd that of the External, the Hand being left alone to ſupport the weight of the Ambient Air, would be preſſed inwards ſo forceably, that though the ſtronger ſort of men were able (though not without much adoe) to take off their Hands, yet the weaker ſort of Tryers could not do it, (eſpecially if by a ſecond Suck the litle Receiver were better exhauſted,) but were fain to ſtay for the Return of the Air into the Receiver to aſſiſt them.

This Experiment being deſign'd rather to convince than to puniſh thoſe that were to make it, we took care not onely that the Braſs ſhould be ſo thick, and the Orifices ſo ſmooth, that no Sharpneſs nor Roughneſs of the Metal ſhould offend the Hand; but alſo that the narrower Orifice (which was the oftneſt made uſe of) ſhould be but about an inch and a quarter in Diameter.

But

But if any were desirous of a more sensible conviction, 'twas very easie to give it him by making the larger Orifice the uppermost, which was the reason why the Instrument was, as we formerly noted, made tapering. But yet this larger Orifice ought not to exceed 2 Inches, or 2 Inches and ½ in wideness, least the great Weight of the Air endanger the breaking or considerably hurting the Hand of the Experimenter. Which Caution I am put in mind of giving, by remembring that I once much endangered my own Hand, through the mistake of him that manag'd the Pump, who unawares to me set it on work, when, for another purpose, I had laid my Hand upon the Orifice of an Instrument of too great a Diameter.

THe famous Experiment of Torricellius, *mentioned in the* 17th *of our already published Tryals, is of that Noblenesse and Importance, that though divers Learned men have (but upon very differing principles) discours'd of it in Print, which gives me the lesse mind to insist long upon it here, yet I shall not scruple to subjoin some Notes concerning Tryals that I made, (though for want of opportunity I could not repeat them according to my custom,) which I had not met with in Others, and which may serve to confirm the* Hypothesis *made use of in this Continuation, and the Treatise it belongs to.*

EXPERIMENT XIX.

About the Subsidence of Mercury in the Tube of the Torricellian Experiment to the level of the stagnant Mercury.

A Baroscope being included in a Receiver, made of a long Bolt head with the lower part of the Ball cut Circularly off, upon the first Exuction of the Air, the Quick silver that before stood at 29 inches, (the Atmosphere appearing then by a constant Baroscope very light,) would fall so low as to rest at 9 or 10 inches,

ches, (for once I measur'd the Subsidence beneath its former Elevation,) and in about three Sucks more it would be brought quite down to the Level of the Stagnant Quick-silver, and somewhat below, (as tis the property of Quick-silver, quite contrary to Water, to rise less in a slender Pipe than in a wide.) The Air being let into the Receiver, the Quick-silver would be impell'd up slowlier or faster, as we pleas'd, to the former height of 29 inches, or thereabouts.

NB. 1. That if the Air were suffer'd to go hastily out of the Receiver, the Mercury would, by virtue of the accelerated motion acquir'd in its descent, at the very first Suck descend till it reacht within an inch or two of the stagnant Mercury, though it would presently after a few risings and fallings settle at the height of 9 or 10 inches, till the next Suck brought it down lower.

2. If when the Mercury was reimpell'd up to its due height, those that manag'd the Pump did, in stead of rarifying the Air, a little compress it, the Quick-silver would by the compress'd Air be easily made to rise an inch or more above the former standard of 29 inches. Which Circumstance I mention, not as a new thing, but to confirm (what some think strange) a Passage printed, page the 59th, where I mention, that if the Air in the Receiver, in stead of being rarify'd in the Engine, were a litle comprest by it; the Pressure of the included Air, being somewhat increas'd by having its Spring thus bent, would sustain the Mercury in the *Torricellian* Tube at a greater than the wonted Height.

And to confirm another passage in the same Page, where I observ'd, that if the Pressure of the Air upon the stagnant Mercury be not so great as tis wont to be, the *Mercury* will begin to subside in a (fill'd and inverted) Tube, which wants of the usual height; we took a Glass Cane, (seal'd at one end,) much shorter than the due length, and having fill'd it with Mercury, and inverted it into a Glass full of stagnant Mercury, we placed all in the former Receiver; where the Mercurial Cylinder for want of the requisite height remain'd totally suspended, but upon the first or

second

ſecond Suck it would ſubſide, and in two or three Sucks more it would fall to the levell of the ſtagnant Mercury, or a little below it. Upon the letting in of the Air it would be impell'd to the very Top of the Tube, bating an Aerial bubble, which ſeem'd to come from the Mercury it ſelf, and was ſo litle, as not to be at all diſcernable, ſave to a very attentive Eye.

This Experiment I ſhould not think fit here to relate, ſince I formerly acquainted Your Lordſhip with the Subſidence of the Mercury upon the withdrawing of the Air from the Receiver, were it not that, in the mention of that Tryal, I remember I confeſs'd to You, that I could not ſo free the great Receiver I then us'd from Air, but that the litle that remained or leak'd in, made me unable to bring the Mercury in the Tube totally to ſubſide, or fall much nearer than within an Inch of the Surface of the ſtagnant Mercury, with which in our preſent Tryals that in the Tube was brought to a Level.

Exper. the XVII. pag. the 54, and 55.

EXPERIMENT XX.

Shewing that in Tubes open at both ends, when no fuga Vacui *can be pretended, the weight of Water will raiſe Quick-ſilver no higher in ſlender than in larger Pipes.*

BEcauſe I find it, even by Learned and very Late Writers, urg'd as a clear and cogent Argument againſt thoſe that aſcribe the *Phænomena* of the *Torricellian* Experiment to the weight of the External Air; That tis impoſſible, that the Air, though 'twere granted to be a heavy Body, could ſuſtain the Quick-ſilver at the ſame height in Tubes of very differing bigneſs, ſince the ſame Air cannot equally counterpoiſe Mercurial Cylinders of ſuch unequal weights: and becauſe this Objection is wont very much to puzzle thoſe that are not well acquainted with the Hydroſtaticks, I preſume Your Lordſhip will allow me, till I can ſhew you ſome Hydroſtatical Papers, by which the Objection may appear to be but ill grounded upon the true Theoremes of that Art, to annex the Tranſcripts of a couple of Experiments, (that I once made to remove this, ſuppoſedly inſuperable, Difficulty,) juſt as I find them regiſtred in my Note-books.

The 1. Tryal.

Sept. the 2. 1662.

We took a very large Glaſs-Tube, Hermetically ſeal'd at one end, and about two Foot and a half in Length. Into this we poured Quick-ſilver to the height of 3 or 4 fingers. Then we took a couple of Cylindrical Pipes of very unequal ſizes, (the wider being as big agen as the ſlenderer) and open at both Ends. The lower Ends of theſe two Pipes we thruſt into the Quick-ſilver, and faſten'd them near their upper Ends to the Tube with ſtrings, that they might not be lifted up, nor mov'd out of their poſture, in which the convex Surface of the Mercury in both the Pipes ſeem'd to lie almoſt in a Level, the Tube alſo it ſelf being plac'd upright in a Frame. This done, by the help of a Funnel we poured in Water by degrees at the top of the Tube, and obſerv'd, that as the Water gravitated more and more upon the ſtagnant Mercury, ſo the included Mercury roſe equally in both the Pipes, till the Tube being almoſt fill'd with Water, the Mercury appeared to be impell'd up to and ſuſtain'd at as great a height in the Big Tube, as in the Leſſer, being in either raiſed about two Inches above the Surface of the Stagnant Quick-ſilver.

NB. 1. Having caus'd about half the Water (having no conveniency to withdraw any more) in the Tube to be ſuck'd out at the Top, we obſerv'd the Quick-ſilver in both the Tubes to ſubſide uniformly, and to reaſcend alike upon the reaffuſion of the Water.

2. We endeavoured to try the Experiment (for their ſake who have not the Conveniency to have ſuch Tubes purpoſely made)

made) in a wooden veſſel, into which, when it was fill'd with water, we let down a flat Glaſs furniſht with ſtagnant Mercury, whereinto the Ends of the two Pipes were immerſ'd. But the Opacouſneſs of the Cylinder (which reduced us to ſee onely from the Top the Reflection of the ſtagnant Mercury,) and other Impediments, diſabled us to perceive the Motions and Stations of the Mercury in the Pipes, though we once made uſe of a Candle the better to diſcern them.

The II. Tryal.

We took a very wide Tube of Glaſs, of about a Foot long, and into it poured a convenient Quantity of Quick-ſilver. We took alſo two Pipes of about equal length, and of that diſparity in Bigneſs that we newly mentioned, (thoſe Pipes lately deſcribed being indeed cut off from theſe we are now to ſpeak of,) and theſe being fill'd with Quick ſilver (after the manner of the *Torricellian* Experiment) were by a certain Contrivance let down into the Tube, and unſtopt under the Surface of the ſtagnant Mercury, and then the Quick-ſilver in the Pipes falling down to its wonted Station, and reſting there, we poured into the Tube about a foot height (by Gueſs) of Water, whereupon the Quick-ſilver as it before ſtood, as it were, in a Level in both the Pipes, ſo it was, for ought appear'd to us, equally impell'd up beyond its wonted Station, and ſuſtain'd there both in the ſlender and in the bigger Pipe, and upon the withdrawing of ſome of the Water it began to ſubſide alike, as to ſenſe, in them both, falling no lower in the bigger than in the ſlenderer. And Water being a ſecond time poured down into the Tube, the Mercury did in both Pipes riſe uniformly as before. By which and the former Experiment it ſufficiently appeared, that a Gravitating Liquor as Air or Water, may impell or keep up Mercury to the ſame height in Tubes that are of very differing Capacities: And that Liquors ballance each other according to their Altitude, and not barely according to their Weight. For in this laſt Experiment, the Additional Cylinder of one Inch of Mercury was manifeſtly rais'd and kept up by the Water incumbent on the ſtagnant Mercury, (the other Cauſe, whatever it were, of the Mercury's Suſpenſion, being able to ſuſtain but a Cylinder ſhorter by an Inch.) And the ſame parcel of Water did counterpoiſe in the differing Pipes two Mercurial Cylinders, which though but of the ſame Altitude, (namely about an Inch) were of very unequal Weight.

EXPERIMENT XXI.

Of the Heights at which pure Mercury, and Mercury Amalgam'd with Tin, will ſtand in Barometers.

CONſidering with my ſelf, that if the Suſtentation of the Quick-ſilver in the *Torricellian* Experiment at a certain height, depends upon the *Æquilibrium,* which a Liquor of that Specifick Gravity does at ſuch a height attain to with the External Air, if that peculiar and determinate Gravity of the Quick-ſilver be altered, the height of it, requiſite to an *Æquilibrium* with the Atmoſphere, muſt be altered too: (Conſidering this I ſay) I thought it might ſomewhat confirm the *Hypotheſis* hitherto made uſe of, if a *Phænomenon* ſo agreeable to it were actually exhibited. This I ſuppoſed performable two differing wayes, namely by mixing or (as Chymiſts ſpeak) Amalgamating Mercury either with Gold, to make it a mixture more heavy, or with ſome other Metal that might make it more light than Mercury alone is. But the former of thoſe two ways I forbore to proſecute being where I then was unfurniſhed with a ſufficient quantity of refined Gold, (for that which is Coyn'd is generally allayed with Silver, or Copper, or both,) and therefore Amalgamating Mercury with a convenient proportion of pure Tin, (or, as the Tradeſmen call it, Block-Tin,) that the mixture might not be too thick to be readily poured out into a Glaſs-Tube, and to ſubſide in it, we fill'd with this Amalgam a Cylindrical Pipe, ſealed

led at one end, and of a fit length, and then inverted it into a litle Glaſs furniſhed with the like mixture. Of which Tryal the Event was, that the Amalgam did not fall down to 29, nor even to 30 inches, but ſtopt at 31 above the ſurface of the ſtagnant Mixture.

Note 1. That though one may expect, that the Event of the Experiment would be the more conſiderable, the Greater the Quantity is that is mingled of the light Metal, yet care muſt be taken that the Amalgam be not made too thick, leaſt part of it ſtick here and there (as we did to our trouble find it apt to do) to the inſide of the Pipe, by which means ſome Aerial Corpuſcles will meet with ſuch convenient Receptacles, as to make it very difficult, if not almoſt impoſſible, to free the Tube quite from Air.

2. It may perhaps be worth while to try, whether by comparing the height of the Amalgam, to what it ought to be upon the ſcore of the ſpecifick Gravities of the Mercury, and the Tin, mingled in a known Proportion in the Amalgam, any diſcovery may be made whether thoſe two Metals do penetrate one another after ſuch a manner (for there is no ſtrict Penetration of Dimenſions among Bodies) as Copper and Tin have, as I elſewhere note, been (by ſome Chymiſts) obſerv'd to do, when being melted down together they make up a more cloſe and ſpecifically ponderous Body, than their reſpective Weights ſeem'd to require.

3. That by comparing this 21. Experiment with the 18th of thoſe formerly publiſhed, it may appear, that the height of the Liquor, ſuſpended in the *Torricellian* Experiment, depends ſo much upon its *æquilibrium* with the outward Air, that it may be varied by a change of Gravity in either of the two Bodies that counterballance each other, whether the change be of weight in the Atmoſphere, or of Specifick Gravity in the ſuſpended Liquor.

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I Should here acquaint Your Lordship with what I have since tried in reference to the 18th *of the Printed Experiments, where I mention, that I observed, by long keeping the same Instrument with which I once made the* Torricellian *Experiment in the same place, that the height of the suspended Mercury would vary according as the weight of the Atmosphere hapned to change. But though about the Barometer (as others have by their imitation allowed me to call the Instrument hitherto mentioned, put into a Frame) I made in the year* 1660 *several Observations, that would not perhaps be impertinent in this place, yet having long since left them with a Friend, who lives far off, and not having them now in my power, I must beg Your Lordships permission to reserve them for a part of the Appendix, which I doubt I shall be engaged to adde to this Epistle. And in the mean time I shall not forbear to present Your Lordship those other Papers that I have by me, relating to the Barometer; some of which will, I presume, sufficiently confirm my lately mentioned conjecture about the cause of the Variation observed in the Height of the suspended Mercury.*

EXPERIMENT XXII.

Wherein is propos'd a way of making Barometers, that may be transported even to distant Countries.

THinking it a desireable thing (as I have elsewhere intimated) to be able to compare together, by the help of Barometers, the weight of the Atmosphere at the same time, not onely in differing parts of the same Country, as of *England*, but in differing Regions of the World; I could not but foresee that 'twould be very difficult to accomplish my desire without altering the form of the Barometers I had hitherto made use of. For as these be unfit

unfit to be transported far, because that stagnant Mercury would be so apt to spill. So the procuring them to be made in the places where they are to be used, though it be no bad expedient, and such as I have divers times made use of, is liable to this inconvenience; that, besides that few will take the pains, and have the skill, requisite to make Baroscopes well, though they be sufficiently furnished with Glasses and Mercury for that purpose, besides this, I say, except men be more than ordinarily diligent and skilful, (and perhaps though they be,) 'twill be very difficult to be sure that the Baroscope newly made in a remote Country, is as Good (and but as good) as that which a man makes use of in this; in regard that at the making of the former, they are supposed to have no other Barofcope to compare it with; and to be sure, they have not the same with which it is to be compared Here.

Being by these considerations invited to attempt the making of Portable or Travailing Baroscopes, (if I may so call them,) I thought it requisite to endeavour these three things: The *first*, to make the vessel that should contain both the sustained and the stagnant Mercury all of one piece of Glass, of a like bigness: The *next*, to place this vessel, when fill'd, in such a Frame, as may be easie to be transported, and yet in a reasonable measure defend the Glass from external violence, no part of it standing quite out of the Frame, as in all other Baroscopes: And the *third*, so to order the vessel, that it may not be subject to be easily broken by the violent motion of the Mercury contain'd in it.

The first of these will not seem practicable to those that imagine (without any warrant from the Hydrostaticks) that tis as well necessary as usual, that the stagnant Mercury should have a vessel much wider than the Tube, wherein the Mercurial Cylinder is sustain'd; but to us the difficulty seem'd much less to make the Glass part of our Tube of one piece, and of a convenient shape, than afterwards to fill it.

But to do both, we took a Glass Cylinder seal'd at one end, and of a convenient length, (as about 4 or 5 foot,) and caus'd it by the flame of a Lamp to be so bent, that, to those that did not take notice 'twas sealed at one end, it seem'd to be a Syphon of very unequal Legs, the one being 3 or 4 times longer than the other; by virtue of which Figure the shorter Leg may serve in stead of the distinct vessel usually imployed to contain the stagnant Mercury. To fill this, which is not easie, one may proceed after this manner. Take a small Funnel of Glass, with a long and slender Shank, so that it may reach 3 or 4 Inches, or further, into the shorter Leg of our Barometrical Syphon (if I may so call it;) and by this Funnel pour into this shorter Leg as much Mercury as may reach about 2 or 3 Inches in both Legs; then stopping the Orifice with your finger, and slowly inclining the Tube, the Mercury in the longer Leg will gently fall to the sealed end; and the Air that was there before, will pass by it, and so make it room. The Mercury in the shorter Leg (which Leg ought to be held uppermost) will by the same inclination of the Tube fall towards the Orifice, but, being by the finger that stops that, kept from falling out, if you do slowly reerect the Glass, and then make it stoop again as much as before, the Mercury will pass out of the shorter Leg into the longer, and joyn with that which was there before; and if all the Mercury do not so pass, the Orifice is to be stopt again with your Finger, and the Tube inclin'd as formerly. This done, the Tube is to be erected, and by the help of the Funnel more Mercury is to be poured in, and the foregoing process of stopping the Orifice, inclining the Tube &c. is to be repeated, till all the Mercury pour'd into the shorter Leg, be brought to joyn with that in the longer; and then the open Leg is to be furnisht with fresh Mercury, observing this, that the nearer the longer Leg comes to the being fill'd, the less you must raise it from time to time, when you pour Mercury into the shorter; as also, that when you see the longer Leg quite full of Mercury, (though there be but litle in the shorter,) you need not pour in any more, if the longer do much exceed a Yard; because upon the restoring of the Tube to an erected posture there will

subside

ſubſide from the taller leg into the other a pretty quantity of Mercury, by reaſon of the ſpace at the ſeal'd end, which will be deſerted by the Mercury that was there. But becauſe tis difficult by this way, as well as by that practiſed already, to fill a Tube with Mercury without leaving any viſible bubbles; to free it from ſuch (if any happen to be) you muſt once more ſtop the Orifice with your finger, and incline, and reerect the Tube divers times, till you have thereby brought moſt of the ſmaller bubbles into one greater; (which you may if you pleaſe increaſe, by letting in a little Air:) for by making this Great bubble paſs leiſurely two or three times from one end of the Tube to the other, it will in its paſſage as it were lick up all the ſmall Bubbles, and unite them to its ſelf; which may afterwards by one inclination more of the Tube be made to paſs into the ſhorter Leg, and thence into the free Air.

But there is another ſort of Funnels, which if one have the skill and conveniency to make, (as *I. M.* eaſily doth,) one may very expeditiouſly fill the bended Tubes of our portable Barometers. For if you make the ſlender part of the Funnel not ſtreight but bended, in the form of an Obtuſe Angle, and of ſuch a length, that the part which is to go into the ſhorter Leg of our Siphon may reach to the Flexure (of the Siphon;) then you may, by ſo holding the Tube that the ſealed end be ſomewhat lower than the other, and by pouring in Mercury at the Obtuſe end of the Angular Funnel, eaſily make it run over the Flexure into the longer Leg of the Siphon; provided you do now and then, as occaſion requires, erect a litle and ſhake the Tube, to help the Mercury to get by the Air, and expell it.

By ſuch wayes as theſe we have found by Experience, that tis poſſible (though not eaſie) to do in ſuch a bended Glaſs, as our purpoſe requires, what, beſides a very late Learned Writer, the Diligent *Merſennus* himſelf, admoniſhes his Reader, that tis not a practicable thing to do in the Ordinary Glaſses of the *Torricellian* Experiment, viz. to free the Mercury of a ſtraight Tube from

Air and Bubbles, (ſo as to be able by inclining the Glaſs to make the Liquor aſcend to the very top.)

The Firſt of our 3 above mentioned Scopes being thus attained, it was not difficult to compaſs the Second, by the help of a ſolid piece of Wood, which is to be ſomewhat longer than the Tube, and a good deal broader in the lower part than in the upper, that it may receive the ſhorter Leg of the Siphon. In ſuch a piece of Wood, which was about an Inch thick, we caus'd to be made a Gutter or Channel, of ſuch a depth and ſhape, that our Siphon might be placed in it ſo deep, that a flat piece of Wood (like a plain'd Lath) might be layd upon it, without at all preſſing upon or ſo much as touching the Glaſs; ſo that this piece of Wood may ſerve for a Cover to defend the Glaſs, to be put on when the Inſtrument is to be tranſported, and taken off again when tis to be hung up to make Obſervations with; the Channel-piece of wood ſerving both for a part of a Caſe, and for an entire Frame; which may for ſome uſes be a litle more commodious, if the Cover be joyned (as it may eaſily be) to the reſt of the Frame, by 2 or 3 litle Hinges and a Haſp, by whoſe help the Caſe may be readily opened and ſhut at pleaſure.

The 3^d^ thing we propoſed to our ſelves is nothing near ſo eaſie as the 2^d^, nor have we yet had opportunity to try, whether the way we made uſe of will hold, if the Barometer be tranſported into very remote parts, though by ſmaller Removes we found cauſe to hope that 'twill ſucceed in Greater.

The Grand difficulty to be obviated was this; That though 'twere eaſie to hinder the ſpilling of the Mercury, by ſtopping the Orifice of the ſhorter Leg of our Siphon, yet that would not ſerve the turn; for the uppper part of the Tube being deſtitute of Air, if the Mercury be by the motion of the Inſtrument put to vibrate, it will be apt (for want of meeting with any Air in the upper part of the Tube to check its motions) to hit ſo violently againſt the Top of the Glaſs as to beat it out, or to crack ſome of the neighbouring parts.

To

To obviate this great inconvenience our way is, to incline the Tube, till the Mercury be impell'd to the very top of it, and yet there will remain a competent quantity in the ſhorter leg of the Glaſs, if that be not at firſt made too ſhort. This done, the remaining part of the ſhorter Leg is to be quite fill'd up either with Water or Mercury, and the Orifice of it is to be very carefully and firmly ſtopt, (for which purpoſe we uſe our ſtrong black Cement:) for by this means the Mercury in the longer Leg, having no room to play, cannot ſtrike with violence as before, againſt the top of the Glaſs. But though by many times ſucceſſively ſhaking the Baroſcope we did not perceive that 'twas very like to be prejudiced by the ſhakes it muſt neceſſarily indure in Tranſportation to remote places, if due care be had of it by the way, yet till further Tryal have been made I ſhall not pretend to be certain of the Event. But thus much of conveniency we have already found in this Contrivance, that we ſent it ſome miles off to the top of a Hill, and had it brought home ſafe again, the *phænomena* at the top and bottom of the Hill being anſwerable to what we might have expected if we had imployed another Baroſcope.

When the Inſtrument is to be ſent away, the height of the Mercurial Cylinder (to be meaſured from the ſurface of the ſtagnant Mercury in the ſhorter Leg) being taken for that place, day, and hour, and compar'd (if it may be) with that of another good Baroſcope, which is to continue in that place; as much of the Gutter as is unfill'd by the Glaſs may be well ſtuffed with Cotten, or ſome ſuch thing, to keep the Glaſs the more firm in its poſture; and that the Tube be not ſhaken or preſs'd againſt the Wood, ſome of the ſame matter may be put between the reſt of the Frame and the Cover, which ought to be well bound together: And when the Inſtrument is arriv'd at the remote place where tis to be imployed, (for if it be to be ſent but a litle way, it may be carried ſafely without uſing any adventitious Liquor,) the Water that is added, may be taken off again, by ſoaking it up with pieces of Sponge, Linnen, &c. but if in ſtead of Water you put in Mercury

cury, as it ought to have been put in by Weight, ſo it is to be taken out, till you have juſt the Weight that was put in: and tis not difficult to take out the Mercury by degrees, by the help of a ſmall Glaſs-pipe, ſince You may either ſuck up litle by little as much as remains of the additional Mercury, when by erecting the Barometer, and warily unſtopping the Orifice of the lower Leg, as much Mercury as will of its ſelf flow out is efflux'd; or elſe you may take out the ſuperfluous Mercury, by thruſting the lower end of the litle Pipe into that Liquor, and when it has taken in enough, ſtopping the upper end cloſe with your finger, to keep it from falling back again when you remove the Pipe.

NB. If it ſhould happen in a long voyage, that by the numerous Shakings of the Inſtrument there ſhould from the additional Water or Mercury in the ſhorter Leg get up into the longer any litle Aerial Bubble, which ſeems the onely (but I hope not likely) danger in this Contrivance, he that is to uſe the Inſtrument, at the end of the Voyage may, if he be skilful, free the Mercury from it by the ſame way, that we lately preſcrib'd to free it from Air, when the Inſtrument was firſt fill'd.

I preſume I need not tell Your Lordſhip, that the chief uſe of this Travailing Baroſcope is, That he that uſes it in a remote part, keeping a Diary of the heights of the Mercury, by comparing theſe heights with thoſe at which the Mercury ſtood at the ſame times in the Barometer that was not remov'd, the Agreement or Difference of the weight of the Atmoſphere in diſtant places may be obſerved. To which this may be added, the Conveniency, which the ſtructure of theſe Inſtruments gives them to be ſecurely let down into deep Wels or Mines, and to be drawn up to the top of Towers and Steeples, and other elevated places: not here to conſider, whether by a convenient addition, theſe, as well as ſome other Barometers, may not be made to diſcover even very minute Alterations of the Atmoſpheres Preſſure.

Whether this Travailing Baroſcope, being furniſh'd at its upper end with a very good Ball and Socket, and at the lower end with a great weight, (which way of keeping things ſteady in a Ship has been happily uſed by the *Royal Society* on another occaſion,) whether, I ſay, our Inſtrument may by this Contrivance, or ſome other that might be ſuggeſted to the ſame purpoſe, be made any thing ſerviceable at Sea, notwithſtanding the differing motions of the Ship, I have had no opportunity to try: but whether it may or may not be uſeful in ſpite of the rolling of the Ship, it may at leaſt be made uſe of in flat Calms, (which divers times happen in long Voyages, eſpecially to the *Eaſt Indies*, and to *Africk*,) and then the Inſtrument, which at other times may lie by without being at all cumberſom, may be made uſe of, as long as the Calm laſts, to acquaint the Obſerver with the weight of the Atmoſphere in the Climate where he is, and that upon the Sea: which may give ſome welcome Information to the Curioſity of Speculative Naturaliſts, and perhaps prove either more directly or in its conſequences of ſome uſe to Navigators themſelves, as by enabling them by its ſuddain changes to foretell the end of the Calme. Beſides that, having one of theſe Inſtruments ready at hand, where ever they ſet foot on ſhore, though it be but upon a ſmall Iſland, or a Rock, they can preſently and eaſily take notice of the Gravity of the Atmoſphere in that place; which whether or no, if compared with other Obſervations, it may in time prove not altogether uſeleſs to the Gueſſing whereabouts they are, and the foreſeeing ſome aproaching changes of Weather, I leave to future Experience, if it ſhall be thought worth the making, to determine.

Beſides the ordinary Baroſcope, and this Travailing one, I have imployed 2 or 3 other Inſtruments of quite differing kinds, to diſcover the varying Gravities of the Atmoſphere; but though they have hitherto ſucceeded well (for the main,) yet being willing to make further Obſervations about them, I reſerve one of them for another opportunity, and think fit to leave the other in a Tract it belongs to. A

A Post-script Advertisment.

Since the writing of the foregoing and the following Experiments about the Travailing Baroscope, having had occasion to make one at a place about 50 miles distant from that where I was when I writ them, I took notice, that the Mercury in the Travailing Baroscope was not by ¼ of an Inch so high as that in another Baroscope made the ordinary way; and yet 'twas not easie to perceive, that the former had been less carefully fill'd than the latter. So that I yet know not well to what cause to impute the Difference, unless it should perhaps depend upon this Circumstance; That the Pipe, whereof the Travailing Baroscope was made, was very slender, and much more so than the Tube of the other; and I have already elsewhere observed, that Mercury, contrary to what happens in Water, is lesse apt to rise in very slender Pipes. And though I remember that, at the Place where I writ the Experiment, to which this Postscript belongs, in the Tube I then imployed to make the Travailing Baroscope, the Mercury ascended as high as in a noted one made the common way, yet not being in the other place furnished with a Tube long and big enough, I think my self oblig'd, till I can clear the Doubt by further Tryal, to give Your Lordship this Advertisement, lest either the Cause already suspected, or some other unheeded thing may in some cases make these Travailing Baroscopes somewhat differing from others. But though they should prove to be so, yet it would not follow that they cannot be made serviceable: for keeping a pretty while that Instrument, which suggested the Scruple to me, just by the other with which I had compar'd it, and carefully taking notice of the respective heights at which the Mercury rested in both, I observ'd that when it rose or fell in the other Barometer, it did also rise and fall in the Portable one; and when it rested at its first station in the Former, it did so in the Later; and though there seem'd to be an inequality in the quantity of the Ascent, and subsidence of the Mercury in the two Instruments, yet that seem'd to be accountable for by some Circumstances,

ſtances, eſpecially the very unequal breadth of the veſſel that contain'd the ſtagnant Mercury in the other Barometer, and that ſhorter Leg which anſwer'd to that veſſel in the Travailing Barometer. But till the formerly propoſed Scruple be by further Obſervation removed, the ſafeſt way will be to make the Barometer to be ſent to remote places, as like as may be (in bigneſs, and length of the Tube) to another Portable one kept at home; that ſo when they are once adjuſted, the Collations may be made betwixt two Inſtruments of the ſame kind, whereof that which is kept at home may alſo, if it be thought fit, be compared, when the Obſervations are made, with a Baroſcope made the ordinary way.

EXPERIMENT XXIII.

Confirming, that Mercury in a Barometer will be kept ſuſpended higher at the top, than at the bottom of a Hill. On which occaſion ſomething is noted about the height of Mountains, eſpecially the Pic of Tenariff.

TO give Your Lordſhip ſome Inſtance (till I can preſent You with a Nobler one) of the Uſe of our Travailing Barometer, I ſhall now adde: That when I writ the foregoing Experiment, chancing to be within 2 or 3 miles of a Hill, which, though not high, was the leaſt low in that Countrey, I thought our Inſtrument might be ſafely, and not altogether uſeleſly, carried on Horſe-back to the top of it, which was too remote from the bottom to be conveniently reacht by me on foot in the midſt of Winter. This Tryal therefore I reſolv'd to make, becauſe, though I formerly told You of a conſiderable one that had been made in *France* by ſome Eminent *Virtuoſi* of that Country, yet I was willing, not onely to have a Proof how ſafely our Baroſcope might be tranſported, but to confirm to Your Lordſhip upon our own Obſervation, made in another Region, ſo conſiderable an an Argument, as these kind of Experiments afford to our *Hypothesis*.

And though when I came to try the Experiment, I hapned to have an Indisposition that forbid me to do it all my Self, yet having carefully mark'd on the edge of the Frame the height to which the suspended Quick-silver reach'd, and compar'd it with a good Baroscope made the ordinary way, I committed our Instrument to a couple of Servants, that I had often imployed about Pneumatical and Mercurial Experiments, giving them particular Instructions what to do. And the Instrument being such as might be safely carried on Horseback, I had in two or three hours an Account brought me back, the Summe of which was: That they found the suspended Mercury fall a litle as they ascended the Hill, at whose Top they gave the Liquor leave to setle, and carefully took notice by a mark of the Place it rested at; which was; as I afterwards found, $\frac{1}{4}$ of an Inch, or somewhat better beneath the Mark I had made, and this notwithstanding the Hill was not high, and the Air and Wind seem'd to them to be much colder at the top of it, than beneath. But though, as they descended more and more, they observ'd the Mercury to rise again higher and higher, (as being press'd against by a taller column of the Atmosphere,) and though consequently the Experiment agreed very well with our *Hypothesis*, and may serve for a Confirmation of it; yet by reason of the small height of the Mountain the Decrement of the height of the Mercurial Cylinder was not so considerable, but that I shonld perhaps have omitted the mention of this Tryal, if it did not shew that our Travailing Baroscopes may be fit to be imployed about such Experiments. And therefore, when I can recover some of my scatter'd Papers, I shall by way of *Appendix* subjoin to this some other Observations, that I procur'd to be made by Ingenious men, who had the Opportunity of living near higher Mountains.

Some further Tryals I have recommended to be hereafter made by some other inquisitive Persons; and to make them the more

more inſtructive, I could wiſh that others would do what I ſhould have done, if Opportunity had befriended me. For I deſign'd to make the Experiment at the bottom, the top, and the intermediate part of the hill, at three differing conſtitutions of Air; viz. when it ſhould appear by a good ordinary Baroſcope, that the Atmoſphere was very heavy, when it ſhould be found to be very light, and when it ſhould have a moderate degree of Gravity: And I hoped, that if ſagacious Experimenters ſhould make theſe diverſify'd Obſervations on diſtant and unequal Hils, good Hints may reſult from the Collations that may be made of the varying Decrements of the Mercurial Cylinders height, according to the differing Gravities of the Atmoſphere at ſeveral Times, and the differing heights of the Hils and Stations where the Obſervations ſhould be made.

I alſo indeavoured to get a Baroſcope carried down to the bottoms of deep Mines; partly, to try whether the Atmoſpherical Pillar being longer There then at the Top, the Mercury in the Tube would not be impell'd up higher; and partly, in order to other Diſcoveries. But ſome Impediments in the ſtructure of thoſe Mines made it not very Practicable to imploy Barometers there; which yet makes me not deſpair of Succeſs in ſome other Mines, where the Shafts or Pits are ſunck more perpendicularly.

Perhaps I told Your Lordſhip already by word of mouth, that I have been ſollicitouſly endeavouring to get the *Torricellian* Experiment tried upon the Pic of *Teneriff*, but hitherto I have had no Account of the ſucceſs of my Endeavours; for which I am the more concern'd, becauſe of the Eminent (if not Matchleſs) height of that Mountain, of which You may receive ſome Satisfaction, by what I am going to ſubjoin about it.

M *An Appendix*

An Appendix about the height of Mountains.

FOrasmuch as on the one hand not onely *Kepler*, but divers other modern Writers of Note, do endeavour to straiten the Atmosphere, and make it lower by half than the least height to which, according to our Estimation, it shouldreach; and to countenance their Opinion, will not allow the Clouds to be often above a Mile high, (nor even the highest Mountains to exceed two miles.) And forasmuch as on the other side other Learned men seem to make the Clouds and the Mountains of a stupendous height; we, who take a middle way of estimating the height of the one and the other, hold it not unfit to subjoyn on this occasion some uncommon Observations, in favour of our Opinion, that we have obtain'd from inquisitive Travellers.

But first I will subjoyn a Passage I have somewhere met with in *Ricciolus* his *Almigestum novum*, where he(if I well remember) relates, that the *Rector Metensis* (as he calls him) of the Jesuites Colledg affirm'd to him some years since, that he had measured the height of many Clouds, without having found any of them higher than 5000 paces: which argues, that he met with some so high,though indeed the height of Clouds must needs be very various, according to the Gravity or Lightness, Density or Thinness, Rest or Agitation of the Air, and the condition of the Vapors & Exhalations they consist of. And if either that be true which we have formerly had occasion to mention concerning *Maignan's* Observation, or if it be true that *Sublunary* Comets (for I speak not of *Celestial* ones) are Generated of Exhalations of the Terrestrial Globe, we may well conjecture that the Atmosphere, (especially if its height be not uniform,) and even Clouds (especially those that have most Fumes, and fewest Vapors) may reach much higher than *Cardan*, *Kepler*, and others have defin'd.

But of the height of Clouds (which we have sometimes attempted to take Geometrically) we may have elsewhere occasion

to

to speak again; and therefore I shall now proceed to what I have to say concerning the Height of Mountains. Which being an Enquiry curious and difficult enough in it self, and of some Importance in the Disquisition about the height of the Atmosphere, (it being evident that That must reach at least as high as the tops of Mountains, upon whose tops men can live;) I hope it will not be unacceptable to Your Lordship, if having a while since (as I was intimating) had the opportunity to discourse with some credible Persons that have been upon the top of exceeding high mountains, particularly of the Pic of *Tenariff*, (and especially with one Gentleman, who was a few dayes before brought to satisfie the Curiosity of our Inquisitive and Discerning Monarch, by giving him an Account of his Journey,) I acquaint You with those of the Particulars, which I learn'd from thence, that are the most pertinent to our present purpose. First then whereas divers late Mathematicians will not allow above two miles or half a German league (and some of them not half so much) to the height of the highest Mountain; the Mountain we speak of, in the Island of *Tenariff*, one of the *Canaries* or Fortunate Islands, is so high, that, though perhaps I think those Travellers I have taken notice of, speak with the most when they write, that the top of this Mountain is to be seen at Sea 4 degrees off, *i. e.* at least threescore German Leagues; yet having ask'd the ingenious Gentleman lately mentioned, Mr. *Sydenham*, from what distance the top of the *Sugar-loaf* (or highest part of the Hill, so called from its Figure) could be seen at Sea, according to the common opinion of Seamen? he answer'd, that that Distance was wont to be reckon'd 60 Sea-leagues, of 3 miles to a League: adding, that he himself had seen it about 40 leagues off, and yet it appear'd exceeding high, and like a blewish Pyramid, manifestly a great deal higher than the Clouds. And what he related to me about the Distance, was afterwards confirmed by the Answers I receiv'd from observing men of differing Nations, who had sail'd that way; and particularly by a Noble *Virtuoso*, skill'd in the Mathematicks, who was then Admiral of a brave *English* Fleet: And the above mentioned Gentleman (M^r^ *S.*) also told me, that sometimes men could from thence see the Island of *Madera*, though distant from it 70 leagues; and that the Great *Canary*, though 18 leagues off, seem'd to be very near them that were on the top of the *Sugar-loaf*, as if they might leap down upon it: Thus far M^r^ *Sydenham*. By whose Relation it appears, that this *Pic* must be far higher than *Kepler* and others allow Mountains to be: for else it could not be seen at Sea from so great a Distance. And the Learned *Ricciolus* supposing it to be (as some Navigators report it to be) discoverable at Sea 4 degrees off, calculates its height measur'd by a Perpendicular line, and allowing too for Refraction, to amount to Ten miles, which Altitude also the accurate *Snellius* assigns it. But I fear this Learned man may have been somewhat misinform'd by the Navigators he relyes on, or else that the way of allowing for Refractions is not yet reduc'd to a sufficient Certainty. For I do not find by those who have purposely gone to the top of it, that the Mountain is so high as his Calculation makes it. And whereas the same Eminent Writer resolutely ponounces that the Height of mount *Caucasus*, Deduction being made for Refraction, is 51 Bolonian miles, (which are considerably greater than the Roman miles,) I doubt that here likewise, though I question not his Supputations if You grant him the Grounds of them, he makes this Mountain far higher than indeed it is. For the Passage of *Aristotle*, on which he founds his Opinion, is obscure enough; and *Aristotle* himself does sometimes take up Reports upon Hear-say, without over-strictly examining their Truth or Probability; whereas all the Navigators and Travellers I have hitherto met with, (and Your Lordship knows, that I have upon a Publick Account the opportunity of meeting often with such men,) do almost unanimously agree, that the Pic of *Teneriff* is the highest Mountain hitherto known in the World, and yet that is so far from being 15 leagues high, (as some Eminent and even late Writers would perswade us,)

that

that it is scarce a 7th part so high as *Ricciolus* computes Mount *Caucasus* to be. For having ask'd Mr *Sydenham*, and others, what was the Estimate made by the most knowing Persons of the Island of the height of the Hill, he told me that his Guides accounted it to be one and twenty mile high from the Town called *L'oretava*, seated on the lower part of the Hill; from which town to the Sea there is 3 miles of way alwayes descending. But in regard that the way, which amounted to 21 miles in length, is, as other wayes whereby steep places are wont to be ascended, made to wind and turn for the conveniency of Travellers; I can scarce deduct less than 2 thirds for the Crookedness of the way: and accordingly having ask'd him, whether the Perpendicular height of it had been accurately taken by any with Mathematical Instruments, he answered, that he could say nothing to that upon his own knowledg, but that a Sea-man with great confidence affirmed himself to have accurately enough measur'd it by Observations made in a Ship, and to have found the Perpendicular height of the Hill to be about 7 miles. Which Estimate agrees well enough with the Calculations of *Ricciolus* and *Snellius*, if we lessen the Distance from which the top of the Hill is to be discovered, from 60 German leagues of 4 miles to a League, to the like number of common Leagues at 3 miles to a League.

And because eminent Writers have so confidently deliver'd prodigious things touching the height of this Mountain, I will here, to confirm the Estimate already made, adde these Particulars, which I took from the Gentleman's own mouth, (and which were afterwards confirm'd to me by another that went with him, and partly also by a 3d, who went up to the top at another time of the Year,) *viz.* That they begun their Journey from *L'oretava* on the 18th of August, about 10 of the Clock at night, and travell'd till Five in the Afternoon on the Munday following, resting two Hours by the way, and travelling about 10 miles of their way upon Mules, which afterwards they were forc'd to leave, and betake themselves to their feet. Resting upon Munday till mid-

midnight, they resum'd their journeying, and travell'd till about Nine the next morning, at which time they arriv'd at the top of the Sugar-loaf, or highest Pile of the Mountain; so that they travell'd in all but 26 hours, in which, considering the steepness and ruggedness of the ways, and that they were forc't to goe above half way on foot, to which they were unaccustomed, tis likely enough that the length of the way did not much, if at all, exceed the Computation of the Guides.

We have since endeavour'd, but without yet knowing what will be the success, to have the height of this Mountain carefully taken by skilful men. In the *interim* I shall not deny, but that if what *Aristotle* and other Authors report of Mount *Caucasus* be true, there may be far higher Mountains than the Pic of *Te-nariff*; especially since there is one Consideration, which perhaps You will not think despicable, that I find not taken notice of by those that have written of the height of Mountains; *viz.* That of two Mountains that, measur'd by Geometrical Instruments, may appear to be of the same height, there may yet be a Great inequality; because the Measurer measures onely from some plain piece of Ground at the bottom of the Hill to the top, whereas it may be, that the Country, wherein one of those Mountains stands, may be exceedingly much higher than that wherein the other is plac'd: which difference of heights in the several Countreys, he that is to measure onely the height of one of the Mountains, is not wont to take any Notice of; and consequently though in respect of the Plains, adjacent to the feet of the Mountains, their Altitudes may be equal, yet in respect of the Level or Superficies of the Terraqueous Globe, consider'd as having no Mountains at all but those two, the height of the one may far exceed that of the other; and so the Pic of *Te-nariff* being look'd upon from the Level of the Sea, may be much less high than some other Hils, but may appear much higher than some other Hils, which yet protuberating above the level part of some Country which is it self generally exceeding high, may have its

The like Consideration I since found to have been had, before me, by the learned Ricciolus.

its top more remote from the Centre of the Earth, than that of the Pic, and would appear higher than it, if as well the one as the other were look'd upon from the same Superficies of the Sea.

But to return to the height of the Atmosphere; in order to the making an Estimate of what we have consider'd as to the height of Mountains, I shall adde, that though by what has been already said touching the height of the *Pic*, and other Hills, it appears, that the Atmosphere reaches far higher than many learned men would hitherto allow, yet we are not to think that the Atmosphere may not reach almost incomparably higher than the tops of Mountains. Nor do I suffer my self to be concluded by what many Commentators of *Aristotle* and other Writers are wont to teach touching the distinct narrow Extent they allow to that Sphere, within whose Limits they would have the Steams of the Terrestrial Globe to produce Meteors. How far the Height of Mountains may make the Air at the tops of them inconvenient for Respiration, shall be (God permitting) consider'd, when I come to acquaint Your Lordship with my loose Tryals about Respiration.

EXPERIMENT XXIV.

Shewing that the Pressure of the Atmosphere may be exercis'd enough to keep up the Mercury in the Torricellian Experiment, though the Air press upon it at a very small Orifice.

BY a very slight variation of the foregoing 22th Experiment we may both confirm one of the most important and the least likely Truths of the Hydrostaticks, and remove an Objection, which, for want of the knowledg of this Truth, is wont to be urg'd against our *Hypothesis* even by Learned men. For divers of these, when they see the same *Phænomena* happen in the *Torricellian* Experiment, whether it be made in the open Air, or in a Chamber,

Chamber, are forward to object, That if it were, as we ſay tis, the weight of the Air, incumbent on the ſtagnant Mercury, which keeps that ſuſpended in the Tube from falling down, the Mercury would not be ſuſtain'd at any thing near the ſame height in the open Air, where the Pillar that is ſuppos'd to lean upon the ſtagnant Mercury, may reach up to the top of the Atmoſphere, as in a cloſe room, where they imagine that no more Air can preſs upon it, than what reaches directly up to the Roof or Sealing. And when to this tis anſwer'd, that though if a Room were indeed exactly clos'd, the Suſtentation of the Mercury ought to be aſcrib'd to ſome other cauſe than the weight of the Impriſon'd Air, (which other Cauſe I have elſewhere ſhewn to be its Spring;) yet in ordinary Rooms there is ſtill a Communication between the internal and external Air, either by the Chimney, or, if the Room have none, by ſome Crevice in the Window, or by ſome Chink between the Wall and the Door, or at leaſt by the Key-hole. And when to this tis objected, that the Orifice of the Keyhole is much narrower than the Superficies of the ſtagnant Mercuryr, and conſequently, though the Atmoſphere were not reduc'd to to preſs obliquely on the Mercury, yet, entring at ſo ſmall an Orifice, it could not preſs ſufficiently upon it; when, I ſay, in anſwer to this Objection I have alleadg'd that Hydroſtatical Theoreme, That the Preſſure, in ſuch caſes as ours, is to be eſtimated by the heights of the Liquors and not the breadths, the Aſſertion has been thought unlikely and precarious.

To confirm therefore this Hydroſtatical Truth, one may take the bended Tube, mention'd in the 22th Experiment; and inclining it till the greateſt part of the Mercury paſs from the ſhorter Leg into the longer, the upper end of this ſhorter Leg may by the flame of a Lamp be drawn out ſo ſlender, that the Orifice of it ſhall not be above an 8th or 10th part (not to ſay as much leſſe) as big as 'twas before. For this being done, and the Tube erected again, if the tall Cylinder of Mercury be of the uſual or former height, as we have found it, 'twill appear congruous to our

Hypotheſis,

Hypotheſis, that the weight of the external Air may exerciſe as much Preſſion upon the ſtagnant Mercury through a little hole, as when all the upper Superficies of that Mercury was directly expos'd to it.

And if one have not the conveniency to draw out the ſhorter Leg as is preſcrib'd, one may nevertheleſs make the Tryal, by carefully ſtopping up the Orifice with a Cork and Cement, leaving onely (or afterwards making) a very ſmall hole for the Air to paſs in and out. If I had not wanted a fit Inſtrument, I would have tried to exemplifie the Truth of what has been delivered, by adding to the Glaſſes we imploy'd to make the Vth. Experiment, ſuch a Cover, as might be cemented on to the Edge of the Glaſs, having onely a very ſmall hole in the midſt, at which the Atmoſphere would be reduc'd to exerciſe its Preſſure; and the like Cover I would have made uſe of in the Xth Experiment, about the breaking of Glaſs-plates in the unexhauſted Receiver, by the bare Spring of the Air.

EXPERIMENT XXV.

Shewing that an Oblique preſſure of the Atmoſphere may ſuffice to keep up the Mercury at the wonted height in the Torricellian Experiment, and that the Spring of a little included Air may do the ſame.

BY adding a couple of litle Circumſtances to the Tryals lately propos'd, we may confirm two conſiderable Articles of our *Hypotheſis*. For 1. if, in ſtead of drawing the ſhorter Leg of our Barometrical Syphon (if I may ſo call it) directly upwards, or parallel to the longer Leg as in the foregoing Experiment, You make the ſlender part bend off ſo, as that, if it were continued, it would make a right Angle with the longer Leg of the Syphon, or elſe an acute Angle tending downwards; this being done, I ſay, if when the Tube is erected the Mercury rest at its wonted station, 'twill appear, that the Pressure of the Atmosphere may be exercis'd upon it as well obliquely, when the Pipe that conveyes it is either Horizontal, or opens downwards.

And 2. if in stead of bending this slender Pipe, one seal it up Hermetically, the continuance of the Mercurial Cylinder at the same height will shew, that the Spring of a very litle Air, shut up with the Pressure of the Atmosphere upon it, (though no more than what the Air here below is ordinarily expos'd to by the weight of the incumbent Air,) is able to support as tall a Cylinder of Mercury as the weight of the whole Atmosphere, *i.e.* of as much of it, as can come to exercise its Pressure against the Mercury.

NB. If when the shorter Leg of the Baroscope is seal'd up, you move the Instrument up and down, the Mercury will vibrate, by reason of the somewhat yielding Spring of the imprisoned Air; but because of the resistance of the Spring, the motion will be diversified after an odde and pretty manner: which may be easily perceiv'd by the Impression it makes upon the Hand, but not so easily describ'd. And because that, when the shorter Leg is drawn out slender enough, after the Instrument is furnish'd with Quick-silver, tis easie to seal it up with the flame of a Candle, without the help of any Instrument at all, I shall here take notice to Your Lordship, (which I could not reasonably do before,) that it may on some occasions be convenient to seal up the Barometer, before it be transported, and, in some cases, to incline the Tube beforehand, till the Quick-silver have quite fill'd the longer Leg; by this means the vibrations of the Quick-silver will be less than otherwise they would be, and 'twill be no trouble at all, when the Instrument is brought to the design'd place, to break off the slender *Apex* of the shorter Leg, and so expose again the Mercury to the Pressure of the Atmosphere.

As about the former Experiments, so about these two this Advertisement may be given; *viz.* That the same Tryals, for the

the main, may be made without confining ones self to the propos'd wayes of making them.

1. For the First of these new Tryals may be made by Cementing very carefully on to the Orifice of the shorter Leg (which need not be alter'd) a short Pipe of Glass, whose upper end may be drawn out very slender, and bent either Horizontally or downwards; which is far easier to be done, than to draw out the shorter Leg when the Glass is furnish'd with Mercury.

2. And as for the 2d Tryal, that may be well enough made, by carefully stopping the unalter'd Orifice of the shorter Leg with a good Cork, and our close Cement, or with the later onely; and when you would afterwards use this Instrument as a Baroscope, You need but heat a Pin or slender Wire red hot, and so burn a hole through the Stoppel.

And this Expedient, which I could not conveniently advertise Your Lordship of sooner, may be of Use when a Travailing Baroscope is to be often remov'd: because having once stopt the whole Orifice well, tis far more easie to stop and open a Pin-hole accurately, than to close and unstop the whole Orifice of the Tube.

Note, I endeavoured to confirm more than one of the foregoing Particulars by this one Experiment. Having caus'd a Portable Barometer to be made with the shorter Leg of a somewhat more than ordinary length, I afterwards caus'd the upper part of this Leg to be drawn out very slender, (as in this 25th Experiment;) and lastly I caus'd the same shorter Leg to be either about or somewhat above the middle bended downwards, so that the small Orifice of the slender *Apex* pointed towards the Ground. This done, I was to have measur'd the height of the suspended Mercury, but not having a fit Ruler at hand, I then deferr'd, and afterwards forgot to do it; but I remember, that neither I, nor some others vers'd in such Experiments, to whom I shew'd it, took any notice that the Mercury was less high than in ordinary Barometers; whence 'twas concluded, that the Atmoſphere could exerciſe his Preſſure not onely at a very ſmall Orifice, (which in our Experiment did litle, if at all, exceed a Pinhole,) but when the Air muſt at this little Orifice preſs upwards to be able to preſs upon the Surface of the ſtagnant Mercury:

EXPERIMENT XXVI.

About the making of a Baroſcope (but of litle practical uſe) that ſerves but at certain times.

TO ſhew ſome Ingenious men by a *Medium*, that has not hitherto (that I know of) been made uſe of, That the not ſubſiding of Quick-ſilver in an inverted Tube, that is a litle ſhorter than 30 inches, or thereabouts, does not proceed from ſuch a *fuga Vacui* as the Schools aſcribe to Nature, but from the Gravity of the external Air, I deviſed the following Experiment.

Having made choice of a time, when it appear'd by a good Baroſcope, (which I had frequently conſulted for that purpoſe,) that the Atmoſphere was conſiderably heavy, I caus'd a Glaſs-pipe, Hermetically ſeal'd at one end, and in length about 2 foot and a half, to be fill'd with Quick-ſilver, ſave a very litle wherein ſome drops of Water were put, that we might the better diſcern the Bubbles, if any ſhould be left after the inverſion of the Tube into an open Glaſs with ſtagnant Mercury in it. Having by this means (though not without difficulty) freed the Tube from bubbles, we ſo order'd the matter, that the Quick-ſilver and the litle water that was about it, fill'd the Tube exactly, without leaving any interval that we could diſcern at the top, and yet the Mercurial Cylinder was but very little higher than that of our Baroſcope was at that time.

This done, the newly fill'd Pipe was left erected in a quiet place, where the Liquors retain'd their former height for divers dayes. But though an ordinary School-philoſopher would confidently fuently have attributed this sustentation of so heavy a Body to Nature's fear of admitting a *Vacuum*, yet it seems, that either she is not alwayes equally subject to that fear, or some other cause of the *Phænomenon* must be assign'd; for when (a pretty while after) I had observ'd by the Baroscope, that the Atmosphere was grown much lighter than before, repairing to my short Tube, I found that according to my expectation the Quick-silver was not inconsiderably subsided, and had left a Cavity at the top, which afterwards grew lesser, according as the Atmosphere grew heavier.

NB. 1. The Tube imployed about this Experiment, may be brought to the requisite shortness, either by wearing off a litle of the Glass at the Orifice of it, or by increasing the height of the stagnant Mercury, into which it hath been inverted.

2. When the Quick silver in our short Tube was much subsided, there appeared in the Water that swam upon it a litle Bubble, about the bigness of a small Pins head, but, considering how careful we had been to free the Tube from bubbles before we set it to rest, it may very well be, that this so small a Bubble was not produc'd till after the subsiding of the Quick-silver, whereupon the Aerial Particles in the Water became less compress'd than before; not to mention that the Bubble (such as it was) appear'd very much greater than it would have done, if the Pressure of the Atmosphere had not been kept from it by the weight of the subjacent pillar of Mercury.

EXPERIMENT XXVII.

About the Ascension of Liquors in very slender Pipes in an Exhausted Receiver.

WHat I related to Your Lordship in the 35th of the publish'd Experiments, (pag. 138.) about the seemingly spontaneous Ascension of Water in slender Pipes, has occasion'd the

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making of many Tryals by the Curious, whereby that Experiment has been not a little diversify'd; but because among those I have yet heard of none have been made in our Engine, it may not be amiss to adde the following Tryal, which may be of use in the *Examen* of one or two of the chief Conjectures that have hitherto been propos'd about the cause of that odde *phænomenon*.

We ting'd some spirit of Wine with Cocheneel, which being put into the Receiver, and the Air withdrawn, did exceedingly bubble for a pretty while. Then little hollow Pipes of differing Sizes being put into it, the red Liquor ascended higher in the slenderer than the others, but upon the withdrawing of the Air there scarce appear'd any sensible difference in the heights of the Liquor, nor yet upon the letting it in again.

Afterwards two such Pipes of differing Sizes, being fasten'd together (at a distance) with Cement, were let down into the same spirit of Wine when the Receiver was well exhausted, notwithstanding which the Liquor ascended in them, for ought we could plainly see, after the ordinary manner; onely when the Air was let in again, there seem'd to be some little (and but very litle) rising at least in one of the Pipes. In this Tryal this *Phænomenon* was noted: That though there appear'd no Bubbles at all in the vessel'd spirit of Wine, (notwithstanding that we continued to pump,) yet there did for a pretty while arise bubbles in that part of the Liquor that was got into the slender Pipes; which I guess'd to proceed from the sustentation (in part) of the spirit of Wine, made by the inside of the Pipe whereto it adher'd.

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EXPERIMENT XXVIII.

About the great and seemingly spontaneous Ascension of Water in a Pipe fill'd with a compact body, whose Particles are thought incapable of imbibing it.

VPon occasion of the (seemingly) spontaneous Ascension of Water in slender Pipes of Glass, I consider'd that 'twould be easie by another way to make it rise to a far Greater height than hitherto had been done; for since we had found by Observation that, *cæteris paribus*, the slenderer the little Pipes were that we imployed, the higher the Liquor would rise in them; and since the *Hydrostaticks* had taught us, that often times even in very crooked Pipes Water would be made to ascend by the same wayes (of raising it) to the same perpendicular height (or thereabouts) as in straight ones; I thought, that I might well substitute a Powder, consisting of solid Corpuscles heap'd upon one another, and included in a Glass-Cane in stead of the litle Pipes I had hitherto used. For I consider'd the litle intervals, that would necessarily be left between these differingly shap'd and confusedly plac'd Corpuscles, would allow passage to the Water as did the Cavities of the little Pipes, and yet would in many places be straiter than the slenderest Pipes I had us'd. And though beaten Glass, or fine Sand, &c. might have been imployed about this Experiment, yet I judg'd it far more convenient to make use of some Metalline Calx, because the Operation of the Fire, making a more exquisite Comminution of Solid bodies than our Pestles are wont to do, is fit to supply us with exceeding minute Granes, that intercept proportionable Cavities between them.

Upon this Consideration therefore (besides others to be hereafter hinted) I took a strait pipe of Glass, open at both ends, and of a moderate wideness, (for it need not be very slender,) and having tyed a Linnen-rag to one end of it, that the Water might have

have free paſſage in, and the Powder not be able to fall out, we carefully and as exactly as we could, fill'd the Cavity with Minium, (which is Lead calcin'd, without addition, to Redneſs;) and then having erected the Tube, ſo that the bottom of it reſted upon that of a ſomewhat ſhallow and open mouth'd Glaſs, containing Water enough to ſwim an Inch or two above the bottom of the Tube; into whoſe cavity it did, as I expected, inſinuate it ſelf by degrees, as appear'd by a litle change of colour in that part of the Minium which it reacht, till (the open Glaſs being from time to time ſupplied with freſh liquor) it attain'd to the height of about 30 inches. And then, our Society expreſſing a Curioſity to ſee it, and have it plac'd among better things, I was hinder'd from making any further Obſervations with that particular Glaſs.

This was (if I forget not) about the later end of the year 1662.

Wherefore taking afterwards another Tube, and ſome Minium carefully prepared, I proſecuted the Experiment ſo as to make the Water riſe in the Pipe about 40 inches above the ſurface of the ſtagnant Water; and I gueſs'd it had riſen higher, but, by reaſon that at the upper part of the Minium the difference of colour was ſo ſmall, as not to be eaſily diſtinguiſhable with certainty, I forbore to allow a greater height to the Aſcenſion of the Water: nor could I, where I then was, much promote the Experiment, for want of ſuch Accommodations as I deſir'd; but about the Experiment, as I try d it, I ſhall take notice of the following particulars.

I tryed ſome other Powders beſides red Lead, (as beaten Glaſs, pieces of fine Spunge, Putty, &c.) but did not find any of them do ſo well; which ſucceſs was yet perhaps but accidental, and therefore the Tryal may be repeated, eſpecially with Putty, becauſe that being a Metalline Calx as well as Minium, conſiſts of very ſmall Grains, and by reaſon of its Great whiteneſs receives a Greater change of colour by wetting than Minium does; in which, eſpecially if it be very fine, the diſcoloration that Water makes toward the upper part of the Tube, is ſometimes not ſo eaſie to be clearly diſcern'd.

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2. I did indeed endeavour to remedy this inconvenience, by uſing, in ſtead of meer Water, tincted Liquors, as Ink, tincture of Safron, &c. but they ſeem'd not to riſe near ſo high as Water alone, as if the diſſolv'd ingredients did by degrees choak the pores of the Minium.

3. To have the Grains of our Powder more minute and the ſmaller intervals between them, I choſe not onely to uſe the fineſt ſort of Minium I could procure, but alſo to ſift it through a very fine Searce, and to put it but by litle and litle into the Tube, that by ramming it from time to time it might be made to lie the cloſer; which Expedients ſucceeded not ill.

4. It ſeem'd by a Tryal or two (for I am not ſure the obſervation will alwayes hold,) that if the Tube were very ſlender, (as about the bigneſs of a Swans quill,) the Experiment ſucceeded not well.

5. It may be worth while to obſerve in what times the Water aſcends to ſuch and ſuch heights; for at the beginning twill aſcend much faſter then afterwards, and ſometimes twill continue riſing 24 or 30 hours, and ſometimes perhaps much longer.

6. One of the ſcopes I propos'd to my ſelf in this Experiment was to diſcover a miſtake in the Explication that ſome Learned modern Writers have given us of the cauſe of Filtration; for whereas they teach that the parts of Filtre that touch the Water, being ſwell'd by the ingreſs of it to their pores, are thereby made to lift up the Water, till it touch the ſuperiour parts of the Filtre that are almoſt contiguous to them; by which means theſe being alſo wetted, and ſwell'd, raiſe the Water to the other neighbouring parts of the Filtre, till it have reacht to the top of it, whence its own Gravity will make it deſcend. But in our caſe we have a Filtre made of ſolid Metalline Corpuſcles, where twill be very hard to ſhew that any ſuch intumeſcence is produc'd, as the recited Explication requires.

7. Water aſcends ſo few inches even in very ſlender Pipes, as to ſeem much to favour their Judgment, who diſallow the

conjecture lately entertain'd by some ingenious men, (particularly Mr *H.*) about the raising of the Sap in Trees after the like manner that Water is raised in slender Pipes; but without fully delivering yet my thoughts of that Speculation, I may take notice, that in the last Tryal above recited, I made Water to ascend near, if not above, 3 foot ½; and if by so sleight an Expedient, Water may be made to rise as high as is necessary for the Nutrition of some thousands of Plants, (for such a number there is, that exceed not 3 foot ½ in height,) one may without absurdity ask, why tis not possible that Nature, or rather the most wise Author of it, may have made such Contrivances in Plants, as to make Liquors ascend in them to the Tops of the tallest Trees; especially since, besides divers things that we may already suspect, (as Heat, and something equivalent to well plac'd Valves,) many others, that perhaps are not yet dreamt of, may probably concur to the Effect.

8. As I formerly made, by bending the slender Pipes we have been talking of, short Syphons through which the Water runs, without being at first assisted by Suction, so I thought fit to try, whether I could not in larger Pipes, by the help of Minium, make much longer Syphons. But though when the Orifices were turn'd upwards, fine Minium were ramm'd into both the Legs, and the Orifices were both of them clos'd, yet when they came to be again turn'd downwards, the weight of the Minium would somewhere or other (and for the most part at or near the flexure) make some such chink or discontinuation, as to hinder the farther progress of the Water. Which impediment, though I judg'd it superable enough, (especially by making at the Flexure a little Pipe or Socket, by which both Legs might be closely fill'd) yet for want of Accommodations and leisure it was left unsurmounted. Upon which account also I did not satisfie my self about the success of some former Tryals, as of the Ascension of Water into pieces of Wood of differing sorts, the operation of the Vicissitudes of the Suns beams, and the absence of them upon liquors ascending in Tubes fill'd with Minium, &c.

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9. Whether the Preſſure of the outward Air be the cauſe of the Aſcenſion of Liquors in our Tubes furniſht with Minium, is a Probleme, in order to whoſe Solution I could acquaint Your Lordſhip with a Contrivance, wherewith to make ſome Tryals in our Engine. But ſince it can ſcarce be well deſcrib'd without many words, unleſs You expreſs a particular Curioſity to know it, I ſhall not trouble You with it: and the rather, becauſe the beſt way I know of examining this difficulty belongs to the 2d part of this Continuation, where mention is made of an attempt about it, which did not, I confeſs, diſpleaſe me.

EXPERIMENT XXIX.

Of the ſeemingly ſpontaneous Aſcenſion of Salts along the ſides of Glaſſes, with a conjecture at the Cauſe of it.

TO the ſame Cauſe (or the like) with that of the Aſcenſion of Water in ſlender Pipes may be probably referr'd an odde *Phænomenon*, which though I remember not to have been mentioned by any Chymical or other Writer, I have not unfrequently obſerved as well by chance as in Tryals purpoſely made to ſatisfie my ſelf and others about the truth of it.

The *Phænomenon*, in ſhort, was this. That having in wide-mouth'd Glaſses (which ſhould not be very deep) expoſ'd to the Air a ſtrong Solution of common Sea-ſalt or of Vitriol, which reacht not by ſome inches to the top of the Glaſs; and having ſuffered much of the aqueous part to exhale away very ſlowly, the coagulated Salt would at length appear to have lin'd the inſide of the Glaſs, and to have aſcended much higher, not onely than the place where the ſurface of the remaining Water then reſted at, but than the place to which the Liquor reacht when 'twas firſt poured in. And if the Experiment were continued long enough, I ſometimes obſerved this Aſcenſion of the Salt to amount to fome inches, and that the falt did not onely line the infide of the Glafs, but, getting over the brim of it, cover'd the outfide of it with a Saline Cruft: which made them that faw how litle liquor remain'd in the Glafs, admire how it could poffibly get thither.

And though I have mentioned but the Solution of Vitriol and Sea-falt, becaufe they are much eafier than others to be procur'd, and yet the Experiment fucceeds better in Them than in fome other far lefs parable Salts; yet they are not the onely ones by whofe Solutions the recited *Phænomenon* may be Exhibited.

As for the Caufe of this odd Effect, though I fhall not propofe any thing about it with Confidence, till I have further inquired into it, and efpecially till I have tryed whether the *Phænomenon* may be produced in an Exhaufted Receiver; yet, by what I have hitherto obferved, I am inclin'd to conjecture, that it may be referr'd to fuch a caufe as that of the Afcenfion of Liquors in Pipes after fome fuch manner as this.

Firft, I obferved, that in Water and Aqueous liquors, that part of the Surface which is next the fides of the Glafs, is (whatever the reafon of it be) fenfibly more elevated than the reft of the Superficies; and if very litle clippings of Straw or other fuch minute and light bodies, floating upon the Water, chance to approach near enough to the fides of the Glafs, they will be apt (which one wou'd not expect) to run up as twere this afcent of Water, and reft againft the fides of the Glafs.

Next we may take notice with the Salt-boylers and Chymifts, that Sea-falt is ufually wont to coagulate at the top of the Water in fmall and oblong Corpufcles, fo that as to thefe tis eafie to conceive, to them that have confidered the firft Obfervation, how numbers of them may faften themfelves round about to the infide of the Glafs. And befides Sea-falt, I have found by tryal divers others, *if their Solutions be flowly enough evaporated,* that will, whilft yet there remains a good proportion of Liquor, afford Saline Concretions at the top of the Water. And the faftning of Saline particles to the fides of the Glafs may perhaps be promoted

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ted by the Coldneſs that may be communicated to the Corpuſcles contiguous to the Glaſs, by reaſon of the coldneſs which the Glaſs may be ſuſpected to have, upon the ſcore of its Denſity, in compariſon of Water. But to proceed: I conſider, that by the Evaporation of the aqueous parts of the Solution, the ſurface of the remaining liquor muſt neceſſarily ſubſide, and thoſe Saline particles, that were contiguous to the inſide of the Glaſs and the more elevated part of the Water, having no longer enough of Liquor to keep them diſſolv'd, will be apt to remain ſticking to the ſides of the Glaſs, and upon the leaſt farther Evaporation of the Water will be a litle higher than the greater part of the Superficies of that Liquor; by which means it will come to paſs, that, by reaſon of the litle inequalities that will be on the internal ſurface of the adhering Corpuſcles of the Salt, and perhaps alſo on the internal Superficies of the Glaſs, there will be intercepted between the Salt and the Glaſs litle Cavities, into which the Water contiguous to the bottom will aſcend or be impell'd upon ſuch an account as that, whereon tis rais'd in ſlender Pipes. And when the Liquor is thus got to the top of the Salt, and comes to be expoſed to the Air, the Saline part may, by the evaporation of the Aqueous, be brought to coagulate there, and conſequently to increaſe the height of the Saline filme, (if I may ſo call it;) which by the like means may be at length brought to reach to the very top of the Glaſs, whence it may eaſily be brought over to the outſide of the veſſel, where the natural weight of the Solution will facilitate its progreſs downwards; and the skin of Salt, together with the contiguous ſurface of the Glaſs, may (at length) conſtitute a kind of Syphon.

To this Explication it agrees well, that I have uſually obſerved the Saline filme hitherto mentioned to be with great eaſe ſeparable from the Glaſs in large Fleaks; which argues, that they did not ſtick cloſe to one another except in ſome few places, but had a thin Cavity intercepted between them, through which the water might aſcend.

Nor

Nor is it repugnant to this Explication, that in case the Water ascended, it should, as it seems, dissolve the Salt. For the Liquor being already upon the point of Concretion, is so glutted with Salt, that it can dissolve no more. Whence we may also render a reason, why, when the Saline filme chances to reach to the outside of the Glass, the Liquor (divers times) does not run down to the bottom, but is coagulated by the way. And I have also had a suspicion, (though I could not seasonably take notice of it before now,) that when the Concretion is once begun, the Film may be raised and propagated, not onely by the motion of the Liquor between the inside of it and the Glass, but by the same Liquor's insinuating it self on the outside of the Film into the small Chinks and Crevises, intercepted between the Saline Corpuscles, as Ink (especially if somewhat thin) rises into the Slit, and along the sides of the Nib of a Pen, though nothing but its very point be dipt in the surface of the liquor. And by this means the impregnated Solution may as it were climb up to the top of the saline Concretion, and by coagulating there adde to its height.

Some other Circumstances I have noted of our *Phænomenon*, that agree with the propos'd Explication, but perhaps it would not be worth while to spend more time about it. Not to examine here whether what has been related, so as to make it probable that ascending Water may carry up wherewithall to heighten and increase the Pipes or vessels through which it rises, may contribute any thing more then was suggested in the former 28th Experiment, towards the Explication of the Rising and diffusing of the Sap in Trees.

EXPERI-

EXPERIMENT XXX.

About an attempt to measure the Gravity of Cylinders of the Atmosphere, so as that it may be exprest by known and common Weights.

VVHilst I was making the former Experiments, 'twas more than once my wish, that by knowing the just weight of a Cylinder of Quick-silver of a determinate Diameter, and of 29 or 30 inches high, which is near the height that the Air does usually counterballance, I might the better estimate the weight of a Cylinder of the Atmosphere of that Diameter, and consequently make the better Guesses how near the effects of the Spring of the Air (as well as of its Weight,) produc'd by the help of our Engine, approach'd to the utmost of what might have been expected, in case all the instruments imployed had been perfect, and all concurrent circumstances had been favourable: And upon this account I several times regretted my want of a long Instrument of Steel or hardned Iron, wherewith I many years since made an Observation, that was more carefully registred than preserved, of the weight of a Mercurial Cylinder of a determinate height as well as Diameter; which weight I did not think it so safe to determine by the help of Glass-Tubes, because tis very difficult to have them uniformly Cylindrical, and to know that they are so, in regard that they are form'd but by blowing and drawing out, and, besides the inequality that may happen to the Cavity upon other accounts, tis very difficult to make the sides of the Glass equally thick, and to examine whether they be so or no.

But at length lighting upon (what I had too often wanted in the foregoing Experiments) a dexterous Artificer, that chanced to come for a while to the place where I then was, I indeavour'd to repair my loss, as well as he could help me to do it, by causing him

him to turn very carefully a Cylindrical piece of Brass, of an inch in Diameter, and 3 inches in length, and open (that it might be the better wrought) at both ends, to one of which was exactly fitted a flat bottom of the same Metal, fastned very close to it with little Screws on the outside; this being judg'd a better way, than if it had been turn'd all of a piece.

This instrument being diligently counterpois'd in a trusty pair of Scales, was carefully fill'd with Mercury, which (for greater caution) we took out of a new parcel, that we had not yet imployed about other Experiments, and finding it to weigh XVII Ounces, one Dram, 45 Gr: Troy weight, (or 137 dr: 45 gr:) multiplying that by 10, there will come for the weight of a Mercurial Cylinder, of one inch in Diameter, and 30 inches in height, (and so high I have divers times seen the Mercury to be in a good Barometer,) about 14, 2*l*, (*i.e.* 14*l*, 2 Ounces, and above three drams, Troy-weight; and almost 11, 8*l*. Haberdupoise weight, (*i.e.* 11*l*, 12 Ounces, and above 6 Drams,) which is a greater weight than without such a Tryal one would easily imagine that so short a Cylinder of Mercury, and much less that a Cylinder of so light a Body as Air, being neither of them above an Inch Diameter, could amount to.

Note First, to examine at the same time the weight of the Mercury, and its proportion to Water, we did, before the Mercury was pour'd into the Brass-vessel, fill it with Water, (after which we wip'd it dry before the Mercury was put into it;) and this liquor weighing 10 drams, and 15 gr: the proportion between the Mercury and the Water appeard to be that of 13 $\frac{18}{41}$ to 1: which though it seem somewhat of the least, yet Your Lordship may remember, that I formerly told You I had several times found the receiv'd proportion of 14 to 1, between Mercury and Water, to be somewhat too great; and besides that, in a vessel whose orifice was no lesse than an inch in Diameter, tis exceeding difficult to be sure when tis precisely full either of Water or Mercury; because the former has a Superficies considerably con-

cave

cave, and the other one that is notably convex, and though we us'd some litle Artifices (which would be troublesome here to mention) to estimate the protuberance of the one liquor, and the deficience of the other, as near the truth as could be, yet I am not sure but there may have been a few Mercurial Corpuscles more than there should have been, and that consequently some small abatement may have been made of the weight newly attributed to the whole Mercurial Cylinder of 30 inches.

2. I had thoughts of making use of the Barrel of a Gun, of a convenient length, to find the weight of a Mercurial Cylinder of 2 foot and ½, but I preferr'd the Instrument already made use of (especially not being where I could have one bored after a peculiar way,) not onely because I could not meet with one whose Diameter was a just inch, and consequently as convenient for calculations, and because that the Barrels of Guns are often bor'd a litle Tapering; but because a skilful Artificer confest to me, that they scarce ever bore such Barrels, but with a four-square Bit, (as they call it,) which leaves the Cavity too Angular, or too imperfectly round; whereas if an Hexahedrical Bit be imploy'd it will, as he affirm'd, make the Cavity almost as Cylindrical as can be reasonably desired. I say nothing here of making use for our purpose of a Trunk, as they call a hollow Cylinder of Wood, because I elsewhere shew, that Wood (at least such as the Trunks to shoot Pellets with are wont to be made of) is not of a Texture close enough for such an use.

3. Because in Cylinders of Mercury, 30 inches is a height which the Atmosphere is seldome heavy enough to be able to counterpoise, and because 29 inches is somewhat nearer the middle between the greatest and the least heights, at which I have observed the Mercury at differing times to stand in good Barometers. Your Lordship may, if You please, abate a 30th part of the weight assign'd above to a Mercurial Cylinder of 30 inches, (though I take 29 and ¼, or thereabouts, to be somewhat a more usual height of the Mercury, than precisely Nine and twenty.)

4. The Weight of a Mercurial Cylinder in an *Æquilibrium* with the Atmosphere, and of one inch in Diameter being thus setled, we may, by the help of the doctrine of Proportions, and a few Propositions, especially the 14th of the 12th book of *Euclides* Elements, easily enough calculate the weight of a Cylinder of Mercury of another Diameter, and consequently the force of the Pressure of an Atmospherical Pillar of the same Diameter. For since according to the forenam'd 14th Proposition of the 12th, Cylinders of equal Bases are to one another as their Heights; and since by the 2d Proposition of the same 12. Element, Circles such as are the Bases of Cylinders) are to one another, as the Squares of their Diameters; and since lastly we suppose, that Mercury being a Homogeneous body, at least as to sense, the Mercurial Cylinders will have the same proportion to each other in Weight that they have in Bulk; since, I say, these things are so, if, for instance, we desire to know what will be the weight of a Cylinder of 30 inches high, whose Diameter is two inches, the Rule will be this.

As the square of the Diameter of the Standard Cylinder, (as I call that whose weight is already known) is to the square of the Diameter of the Cylinder propos'd, so will the bulk of the former Cylinder be to that of the later, and the weight of that to the weight of this.

According to which Rule, the square of 1 inch (which is the Diameter of the standard Cylinder) being but 1, (whereby Your Lordship may perceive how much the measure I pitcht on facilitates Computations,) and the square of 2 (which is the Diameter of the propos'd Cylinder) being 4, the bulk or solid Contents of this later Cylinder, and consequently its Weight, will be 4 times as great as those of the standard Cylinder; and so, since the lesser has been already suppos'd to weigh 11, 8*l* Haberdupoise, the Mercurial Cylinder of two inches in Diameter, will weigh 47, 2*l* of the same weight.

EXPE-

EXPERIMENT XXXI.

About the Attractive virtue of the Loadſtone in an Exhauſted Receiver.

SOme Learned modern Philoſophers, that have attempted to explicate the cauſe and manner of Magnetical Attraction or Coition, give ſuch an account of it, as ſuppoſes, that the Air between the two Magnetical Bodies, being driven away by their Effluviums from between them, preſſes them on the parts oppoſite to thoſe where the Contact is to be made; and upon ſome ſuch ſcore (for I muſt not now ſtay to deliver their Theories Circumſtantially) the Air is ſuppos'd to contribute very much to the Attraction and Suſtentation of the Iron by the Loadſtone: wherefore partly to examine this Opinion, and partly for ſome other Purpoſes (not neceſſary now to be mentioned) we thought fit to make the following Exptriment.

We took a ſmall but vigorous Loadſtone, cap'd and fitted with a looſe plate of Steel, ſo ſhap'd, that when it was ſuſtained by the Loadſtone, we could hang at a litle Crook, that came out of the midſt of it, and pointed downwards, a Scale, wherein to put what Weights we ſhould think fit. Into this Scale we put ſometimes more and ſometimes leſs weight, and then by ſhaking of the Loadſtone as much as we gueſs'd it would be ſhaken by the motion of the Engine, we found the greateſt weight, that we preſum'd it would be able to ſupport, in ſpite of the Agitation 'twould be expoſed to, which prov'd to be, beſides the Iron-plate and the Scale, VI Ounces Troy weight, to which if we added half an ounce more, the whole weight appear'd too eaſie to be ſhaken off. This done, we hung the Loadſtone, with all the weight it ſuſtain'd, at a Button of Glaſs, which we had procur'd to be faſtned on to the top of the inſide of a Receiver, when 'twas firſt blown, and though in about 12 Exuctions we uſually emptied ſuch Receivers as as much as was requisite for most Experiments; yet this time, to exhaust it the more accurately, we continued pumping till we had exceeded twice that number of Exuctions, at the end of which time shaking the Engine somewhat rudely, without thereby shaking off the Weight that hung at the Loastone, the Iron seem'd to be very near as firmly sustain'd by it as before the Air began to be pump'd out. I said very *near*, rather than altogether, because that the withdrawing of the Air, though it be not suppos'd to weaken at all the Power of the Loadstone precisely considered, yet it must lessen its power to sustain the Steel, because this in so thin a *medium* must weigh heavier, than in the Air, by the weight of as much Air, as is equal in bulk to the appended Body.

Some other Magnetical Tryals (and also some Electrical ones) I remember I attempted to make by the help of our Engine, but not having the Notes I took of them now at hand, I shall suspend the mentioning them, till I can give Your Lordship a more punctual Account of them.

EXPERIMENT XXXII.

Shewing, that when the Pressure of the External Air is taken off, tis very easie to draw up the Sucker of a Syringe, though the Hole, at which the Air or Water should succeed, be stopp'd.

HAving taken notice, that some learned Opposers of the Modern Doctrine about the weight of the Atmosphere think themselves more than ordinarily befriended by the difficulty we find in drawing up the *Embolus* or Sucker of a Syringe, when the hole, at which the Air or Water should succeed, is stopt, and by the violence, with which, as soon as tis let go, tis, as they imagine, drawn back. And supposing the reason of this confidence of theirs to be, that Men have not yet been able in these *Phænomena*

mena (as in some others) to prove the interest of the Atmosphere's Gravity by direct or confessedly analogous Experiments; I presum'd it will not be unwelcome to Your Lordship, if I here fortifie the Speculations that have been or may be propos'd to explicate these things according to the *Hypothesis* of the weight of the Air, by what we tried to that purpose, among others, when we were making use of a Syringe in our Engine.

The I. Tryal.

We took a Syringe of Brass, (that Metal being closer and stronger then Pewter, of which such instruments are usually made,) being in length (in the Barrel) about 6 inches, and in Diameter about 1 inch $\frac{3}{8}$; and having, by putting a thin Bladder about the Sucker, and by pouring a litle Oyl into the cavity of the Cylinder (or Barrel,) brought the instrument to be stanch enough, and yet the Sucker to move to and fro without much difficulty, we thrust this to the bottom (or Basis) of the Barrel to exclude the Air, and having unscrew'd and laid aside the slender Pipe of the Syringe (which in this and some other Tryals was like to prove not onely needless, but inconvenient) we carefully stopt the Orifice, to which the Pipe in these instruments is wont to be screw'd, and then drawing up the Sucker we let it go, to judg by the violence, with which it would be driven back again, whether the Syringe were light enough for our purpose, and finding it to be so, we fastned to the Barrel a ponderous piece of Iron to keep it down, and then fastning to the handle of the Rammer (or Axletree of the Sucker) one end of a String, whose other end was tied to the often mentioned turning-key: We convey'd this Syringe, and the weight belonging unto it, into a Receiver; and having pump'd out the Air, we then began to turn the Key, thereby to shorten the String that tied the handle of the Syringe to it; and, as we foretold, that the Pressure of the Air, lately included in the Receiver, being withdrawn, we should no more find the wonted resistance

resistance in drawing up the Sucker from the bottom of the Cylinder, so we found upon Tryal that we could very easily pull it up without finding any sensible resistance.

However having thought fit to repeat the Experiment, (which we did with the like success,) lest it might might be objected, that this want of resistance might proceed, as partly from our imploying the Turning-key to raise the Sucker, so principally from some unperceived Leak, at which the Air may be suppos'd to have got into the cavity of the Cylinder; I thought fit *not onely* to examine by Tryal, after the Receiver was remov'd from off the Pump, whether the Syringe were not stanch, (upon which I found that I could not, without some straining, draw up the Sucker even a litle way, and that it would be violently beaten back again,) *but also* in one of these Experiments to make this variation; That when, the Receiver being exhausted, we had drawn up the Sucker almost to the top of the Barrel by such a string as was purposely chosen somewhat weak, we kept the parts of the Syring in that posture, till we had open'd a passage to the outward Air, upon whose ingress the Sucker was (as we intended it should be) so forceably deprest, that it broke the String by which it was tied to the Turning-key, and was violently driven back to the lower part of the Barrel, & that notwithstanding these two disadvantageous Circumstances; *one*, that the string was not so weak, but that one, whom I imploy'd to try it before it was fastned to the Syringe, made it sustain a lump of Iron that weighed between four and five pound; and the *other*, that yet this string was broken long before all the Air, that flowed in to fill the Receiver, had got in: so that the pressure of all the admitted Air would doubtless have broken a much stronger string, if we had imploy'd such a one to resist the depression of the Sucker, which will yet be more evident by a *phænomenon* of our Syringe, that I shall presently have occasion to relate.

II. Tryal

The II. TRYAL.

Containing a Variation of the foregoing.

We took the Syringe imploy'd in the foregoing Experiments, and having found by Tryal that it was, though not perfectly, tite, (nor altogether so much so as before,) yet enough so for our present purpose, (since, when the Orifice of the vent in the Basis was stopt, if the Sucker were more forceably drawn up a litle way, and then let go, it would hastily return, or rather violently be impell'd back towards the bottom of the Barrel,) we made it serve us as well as we could for the following Experiment. Of this Syringe we did very carefully with a Cork and our Cement close the vent; and then having tied to the barrel of the Syring a Weight that hapned to be at hand, (and to amount to 2 Pound, and as many Ounces,); we suspended the Rammer of the Syringe by a string in a large Receiver; and then causing the Pump to be applied, we made 11 or 12 Exuctions of the Air, without any appearance of change in the Syringe: but because I had judg'd the above mentioned Weight sufficient, and suppos'd that the little Air still remaining in the Receiver, had yet too strong a Pressure to be surmounted by it, I caus'd the Pumping to be continued, and within 2 or three Exuctions more I perceiv'd the Cylinder to begin to be drawn down (though but very slowly) by the Weight hanging at it, (assisted by its own Gravity:) and likewise tried (after having purposely stopt a while the working of the Pump) that just upon a fresh Suck the descent would be manifestly accelerated. And when we had suffer'd the Barrel and Weight to slide down as far as we thought fit, we let in the External Air, which (as was to be expected) rais'd them both again much faster than they had subsided.

NB. There would not have needed any thing near so great a Weight to depress the Barrel of the Syringe, but that it is difficult

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in ſuch an inſtrument to make the Sucker fill it accurately enough, without making it ſomewhat uneaſie to be mov'd to and fro: Upon which account twas neceſſary that a Weight ſhould be added, not onely to ſurmount the Preſſure of the Air remaining in the Receiver, (which was not, nor needed to be diligently exhauſted in this Experiment,) but to overcome that reſiſtance, which we juſt now noted the inequalities of the inſide of the Cylinder and thoſe of the Sucker to give to the motion of the one in or over the other. And yet for all this tis not eaſie, though it be not impoſſible, to make one of theſe Syringes very Tight, eſpecially when the Noſe is well ſtopt, and the Sucker drawn up; there being often ſome litle Air that ſtrains in between the Sucker and the Barrel, and ſome that will be harbour'd between the Sucker (though thruſt home) and the bottom of the Barrel, beſides what may lurk between the ſame Sucker and the Cork that ſtops the orifice of the Vent. Nor were we confident, that our Syringe did not at length let ſome Aerial particles inſinuate themſelves into the Cavity, which the depreſſion of the Barrel had made betwixt the Baſes of that Barrel and the Sucker: and in ſuch caſes we ought not to wonder, if upon the return of the Air the Barrel and Weight be not impell'd up all together to the ſame height they reſted at, when they were firſt ſuſpended in the Receiver.

2. It agreed very well with our Doctrine, that as the Cylinder and Weight began not to fall, till a great quantity of Air had been pump'd out of the Receiver, ſo they did not begin to move upwards preſently upon the freedom that was allow'd the Air to return into the Receiver. For till it had continued a pretty while flowing in, there was not enough of it entred to reſtore by its preſſure the Cylinder and the annexed Weight to their former ſituation.

3. What has been deliver'd about our Experiment may be confirm'd by this Variation which we made of it: That having ſubſtituted a far heavier Weight inſtead of that lately mention'd, the

the depreſſion of the Barrel of the Syringe ſucceeded 2 or 3 times one after another much ſooner than formerly, *viz.* about the ſixth, or at moſt, the ſeaventh Exuction.

EXPERIMENT XXXIII.

About the opening of a Syringe, whoſe Pipe was ſtopt in the Exhauſted Receiver, and by the help of it making the Preſſure of the Air lift up a conſiderable Weight.

THough the Trial I am about to relate, had not all the ſucceſs I deſir'd, yet perhaps it will not be impertinent to make mention of it, becauſe there is not any ſort of Experiments, that is wont ſo much to perſwade the Generality of Spectators, of the great force of the Preſſure of the Air, as thoſe, wherein they plainly ſee heavy and ſolid Bodies made to aſcend, (upon the operation of the Air on them,) without ſeeing any other thing lift them up.

We took the often mention'd Syringe, and having cloſ'd up the Hole at the bottom with good Cement, we ty'd to the Barrel a hollow piece of Iron, that ſerv'd us for a Scale, into which we put divers Weights one after another, trying from time to time whether, when the Sucker was forceably drawn up, and held ſteddily in its higheſt ſtation, the Weight tyed to the Barrel (which was held down, whilſt the Sucker was drawn up, and afterwards let go) would be conſiderably rais'd. And when we perceiv'd, that the addition of half a Pound, or a Pound more, would make the Weight too Great to be ſo rais'd, we forbore to put in that increaſe of weight; and having tied the Handle of the Rammer to the Turning-key, we convey'd the Syringe together with its clog into a Receiver, out of which a convenient quantity of Air being pump'd, we were thereby enabled eaſily to draw up the Sucker without the Cylinder; after which having let in the Air, the by-ſtanders concluded, that the weight was rais'd a litle, which yet I would not have allow'd, if we had not been able, by inclining the Engine and the Receiver, to make the Syringe and Weight a litle to ſwing. But to make the effect more evident, I caus'd a two pound weight to be taken out, and then the Receiver being ſomewhat exhauſted, and the Air readmitted, the Clog, when all the Air was come in, was ſwiftly raiſed, and as it were ſnatch'd up from the midle to the upper part of the ſuſpended Rammer.

It is no eaſie matter to meaſure, with any certainty and exactneſs by a Syringe, the weight of an Atmoſpherical Pillar equal to it in Diameter, eſpecially if there be any imperfection in the Syringe, either becauſe the Sucker does not go cloſe enough, in which caſe it can ſcarce be ſtanch, or becauſe by its Preſſure againſt the inſide of the Barrel (which often happens if it be too cloſe) it hinders the Sucker and Barrel from ſliding without reſiſtance by one another, and conſequently there is an undue reſiſtance made to the endeavour of the Atmoſphere, to raiſe the Barrel and Weight. And therefore, though our Syringe being, upon the account of ſome ill accident, leſs in order than it was in ſome of the foregoing Experiments, I muſt not conclude that a Cylinder of the Atmoſphere of the ſame wideneſs with it, is equipollent to no greater a weight, than that which was taken up in our Trial, yet we may ſafely conclude that ſo ſlender a Pillar of the Atmoſphere is able to raiſe by a Syringe at leaſt ſuch a Weight, as in our Experiment it actually lifted up, which amounted to about ſixteen pound (Haberdupoiſe weight,) for it exceeded fifteen pound and three quarters, beſides the weight of the Syringes barrel it ſelf.

EXPERI-

EXPERIMENT XXXIV.

Shewing, that the cauſe of the Aſcenſion of Liquors in Syringes is to be deriv'd from the Preſſure of the Air.

I Shall not here trouble Your Lordſhip with what I have elſewhere propos'd about the explicating of Suction: but as by the lately recited Experiments (I mean the 31, 32, and 33) it has appear'd, that tis to the Preſſure of the External Air that we ſhould aſcribe the difficulty of drawing up the Sucker of a Syring, *when the Pipe* (or the Vent) *is ſtopt*; ſo I ſhall now endeavour to ſhew, that the Aſcenſion of Liquors, which follow the Sucker when tis drawn up, the *Pipe being open*, depends alſo upon the Preſſure of the Air, (incumbent on that Liquor.)

If I had been furniſh'd with very tall Receivers, and ſuch other Glaſſes as I could have wiſh'd, I had tried the following Experiments with Water, as well as Quick-ſilver, but for want of thoſe Accommodations I was reduc'd to make my Experiment with the later onely of thoſe Liquors, which yet will I hope ſufficiently make out what was intended.

The I. Tryal.

We took a ſmall Receiver, ſhap'd almoſt like a Pear, cut off Horizontally at both ends, (being the ſame cap'd Glaſs that is elſewhere mentioned in the accounts of other Experiments:) we alſo took the Syringe formerly deſcrib'd, and having faſtned on to it with good Cement, in ſtead of its own Braſs-pipe, a ſmall Glaſs pipe of about half a foot in length, we put this Syringe in at the narrow end of the Receiver; to whoſe Orifice was (afterwards) carefully cemented on the Braſs-cap with the Turning-key, whereto was tied by a ſtring the handle of the Rammer. Then having conveniently plac'd upon the Engine a very ſhort thick Glaſs ſhap'd like a Sugar-loaf, (which was made uſe of for want of a better,) with a ſufficient Quantity of Quick-ſilver in it; we

See the fig. of the plate

ſo placed the Receiver over it, that the lower end of the Pipe of the Syringe reacht almoſt to the bottom of this Glaſs, and conſequently was immerſt a pretty way beneath the ſurface of the Quick ſilver. We had alſo poured a litle Water in the upper part of the Syringe, that no Air might get in between the Sucker and the Cylinder, notwitſtanding that by ſome Accident or other the Syringe was become ſomewhat leſs Tite than before. And laſt of all we cemented the Receiver to the Engine after the uſual manner.

That which now remained, being to try the Experiment it ſelf, in order to which all this had been done, the Air was pump'd out of the Receiver, (and conſequently out of the litle Glaſs that held the Mercury,) and then the Sucker being warily drawn up, we could not ſee the Quick-ſilver aſcend to follow it, though a litle Water, which it ſeems the outward Air had thruſt in between the Sucker and the Cylinder, was either rais'd or ſtopt in the Glaſs-pipe of the Syringe, (whereof yet much the greateſt part remain'd unfill'd;) of which the reaſon according to our *Hypotheſis* was manifeſt, namely, that the Air being pump'd out of the Receiver, the litle that remain'd had not ſtrength enough to preſs up ſo ponderous a Liquor as the Quick-ſilver into the Pipe, (though even that litle unexhauſted Air might have Spring enough left to raiſe a litle water.) And ſince it appear'd by this, that *without* the Preſſure of the Air the Quick-ſilver would not be elevated, we thought it ſeaſonable to ſhew, that *by* the Preſſure of the Air it would. Whereupon the Air being let ſlowly into the Receiver, the Mercury was quickly impell'd up at leaſt to the top of the Glaſs-pipe, (though by reaſon of ſome unperceiv'd leak it was not long ſuſtain'd there.)

And for further ſatisfaction, when the Experiment was to be tried over again, we order'd it to be ſo made, that it might plainly be obſerved, that though when, the Receiver not being yet exhauſted, the Sucker was drawn up but one inch, the Mercury would be rais'd to the upper part of the Glaſs-pipe of the Syringe, yet

yet after the exhausting of the Receiver; though the Sucker was drawn up twice as high, there appear'd no ascension of the Mercury in the Pipe, (whose lower part onely was darkned by the litle Glass which contain'd that fluid Metal.)

Before I dismiss this Experiment, I must, to make good a promise I made Your Lordship, acquaint You with a *Phænomenon*, which does not a litle confirm our Doctrine, according to which it was easie both to foresee and to explain it: The *phænomenon* was, That if when the Air was diligently pump'd out of the Receiver, the Sucker were endeavour'd to be pull'd up, it could not be so, without much difficulty and resistance, such as was formerly found when the Vent of the Syringe was stopt, of which in our *Hypothesis* the reason may be clearly this; That there being no common Air in the Receiver to assist by its Pressure (whether immediate or mediate) the raising of the Sucker, this could not be raised but by a force great enough to surmount the Weight of the external Air or Atmospherical Pillar that lean'd upon it. So that as the other *Phænomena* of our Experiments manifest, that the raising of Liquors by a Syringe, which is commonly ascrib'd to Attraction, depends upon the Pressure of the Air; so by this *Phænomenon* it appears, that the difficulty of opening a Syringe, whose Pipe is stopt, need not be attributed to such a *fuga vacui* as vulgar Philosophers refer it to; since in our case the same difficulty was found, though the Pipe were open, and the Liquor 'twas immerst in, might have had free access to the place deserted by the Sucker.

The II. Tryal

Being a Prosecution of the former Attempt.

To vary as well as confirm the foregoing Experiment, we caus'd the Syringe to be tied fast to a competently ponderous Body that might keep the Cylinder unmov'd, when the Sucker should be drawn up. We also cemented on to the vent or screw at the bottom of the Syringe a Pipe of glaſs of about two inches in length, (which ſhould have been longer, but that then there would not have been room in the Receiver for the pulling up of the Sucker,) and having plac'd the heavy Body whereto the Syringe was tied upon a Pedeſtal of a convenient height, that the Glaſs pipe might be all ſeen beneath it, and a very low Viol almoſt fill'd with Quick-ſilver might be ſo plac'd underneath the Pipe, that the ſtagnant Mercury reach'd a good way above the immerſt orifice of the ſaid Pipe. Theſe things being thus provided, and the Handle of the Syringes Rammer being tied with a ſtring to the Turning-key that belong'd to the Braſs-cover of the Receiver, this veſſel was cemented on to the Engine, and by it Exhauſted after the uſual manner.

When this was done, we look'd upon the Syringes Glaſs-pipe above mentioned, and being able to ſee through it, (whereby we were certain that it was not yet full of Quick-ſilver) we did by the ſtring draw up the Sucker to a good height, but could not perceive the Pipe to be fill'd with any ſucceeding Mercury. Wherefore warily letting-in ſome Air, we quickly ſaw the Mercury impell'd to the very top of the Pipe; and we concluded from the quantity of Quick-ſilver that was rais'd, that a pretty deal was alſo driven into the cavity of the Cylinder.

NB. I had once before ſeen the Mercury aſcend into the Pipe upon the letting in of the Air into the emptied Receiver, but it ſeeming ſomewhat difficult to me to determine whether the Sucker had been raiſed, becauſe there was no mark to guide my Æſtimate by, I thought it might be ſuſpected, that in caſe the Sucker had not been rais'd, the Aſcenſion of the Quick-ſilver might have proceeded from hence, That the Air contain'd in the Glaſs-pipe, breaking out through the ſtagnant Mercury upon the Exhauſting of the Receiver, the Quick-ſilver might upon the return of the Air into the Receiver be preſt up into the place deſerted by the Air, that broke out of the Pipe. Wherefore we caus'd a ſtring to be tied about the Rammer, as near as we could to the

the top of the Cylinder, by which means, when the Receiver was the next time exhausted, we perceiv'd, that by drawing up the Sucker vve had rais'd it about two inches, if not more, and yet vve could not discern any Mercury to follow it, (the Glass-pipe still continuing transparent,) till we had let some Air return into the Receiver.

This Experiment joyn'd with those we have formerly related to have been tried with our Syringe, may teach us, that if a Syringe were made use of above the Atmosphere, neither the stopping of the Pipe vvould hinder the easy drawing up of the Sucker, nor the drawing up of the Sucker, though the Pipe vvere not stopt, vvould raise by suction the Liquor vvhich the Pipe was immerst in.

Postscript.

Since the last recited Experiment was made, and written, finding some of our Instruments to be in better order than they were when that Tryal was made, vve thought fit to endeavour by that which follows, to repair an omission or two, that formerly we could not well avoid.

Having then caus'd such a Glass-pipe, as has been lately mentioned, to be vvell cemented on to the Syringe, (vvhose Sucker did now move more easily, and yet fill the Barrel more exactly, than before,) I order'd (being to be absent for a while my self) that the Pipe should be fill'd with spirit of Wine tincted with Cocheneel, that the liquor and its motions might be the better discern'd, and that the Pipe being fill'd, that Air might be excluded, which vvould else be harboured in the Pipe, (which Caution was omitted in the foregoing Experiment.) And this the Person, to whom I committed it, affirm'd to have been carefully done, though when he inverted the Pipe thus fill'd into the rest of the red Liquor, that was put into a Viol, he could not possibly do it so well, but that a bubble of Air got into the Pipe, and took up some (though but a litle) room there. By that time, I was call'd upon, to see the

the Event of the Tryal, and could come to look upon it, the Receiver was almoſt quite exhauſted; vvherefore after I had made the pumping be continued a litle longer, and perceived that the tincted ſpirit was fallen down out of the Pipe, and that which lay in the Viol ſeem'd almoſt to boyl at the top, by reaſon of the emerſion of numerous Bubbles, I caus'd the Sucker to be, by the help of the Turning-key, drawn up (by our æſtimate) about two inches and a half, notwithſtanding which vve could not perceive the ſpirit of Wine to riſe in the Pipe, (though the Pumping were before left off.) For vvhich reaſon I order'd the Air to be let in very leiſurely, upon which vve could plainly ſee that the red ſpirit was quickly driven up to the top of the Pipe, and that it was ſo likewiſe into the Cavity of the Barrel, appeared, when the Receiver was removed, by the ſmall Quantity of Liquor that remained in the viol, and the plenty of it which came out of the Syringe.

NB. That if I had not vvanted dexterous Artificers, to work according to a Contrivance I had deſign'd, I had attempted to imitate, by the help of the bare Spring of the Air, ſuch Experiments, as in the lately recited Tryals vvere made to ſucceed, by the help of the Preſſure exercis'd by the Air upon the account of its Weight.

EXPERIMENT XXXV.

Shewing, that upon the Preſſure of the Air depends the ſticking of Cupping Glaſſes to the fleſhy parts they are apply'd to.

'Tis ſufficiently known, that if the Air within a Cupping Glaſs be rarified by the flame of Tow, Flax, or the like, (burn'd for a litle while in it,) and the Glaſs be preſently clapt upon ſome fleſhy part of a Mans body, there will quickly enſue a painful and viſible ſwelling of the part cover'd by the Cupping Glaſs;

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Tis also known, that this Experiment is wont to be urg'd by the Schools as a clear proof of that abhorrence of a *Vacuum* they ascribe to Nature; for, say they, the reason of this *phænomenon* is plainly, that the internal Air of the Cupping Glass, præternaturally rarified by heat when the Instrument is applied, That heat after a while ceasing, the succeeding Cold must again necessarily condense the Air; and so this contracted Air being no longer able to fill the whole space it replenished before, there would ensue a *vacuum*, if the flesh covered by the Cupping Glass, or adjoyning to it, did not swell into the Cavity of it, to fill the place deserted by the Air.

Those Moderns that assert the Weight of the Atmosphere, do thence ingeniously endeavour to deduce the *phænomenon*. And indeed if to their *Hypothesis* about the Airs Weight, the consideration of its Spring be added, 'twill be easie enough to explicate the *phænomenon*, by saying, That when the Cupping Glass is first set on, though much of the Air it formerly contain'd were a litle before expell'd by the heat, yet the same heat, increasing the pressure of the remaining Air, is the cause that the absence of the Air driven out of the Glass, does not immediately occasion so sensible a pain: but, when that adventitious agitation of the included Air ceases, that Air having now, because of the paucity of its Corpuscles, but a weak Spring, can no longer press upon the part covered by the Cupping Glass neer so strongly, as the outward Air does by its Weight press upon all the neighbouring parts of the flesh: by which means (according to what we have more than once explicated already) some of the yielding flesh (or other body covered by the skin) must be forceably thrust into the cavity of the Cupping Glass, where there is less Pressure, then at the outside of it. And the fibres and membranous parts being thus violently stretcht, there must needs follow a sensible Pain as well as Tumour. Which Tumour yet does not fill up the Cupping Glass, not onely because of the resistance of the skin to be so far distended, but also, if the included Air have not been much rarified becaufe of the Spring of the imprifoned Air, (which grows fo much the ftronger, by how much the fwelling flefh reduces the Air into lefs room,) as I have fometimes tried, by applying a Cupping Glafs to Quick-filver, or even to Water, which will rife in it but to a certain height.

But though by this, or fome fuch Explication, the Argument urged by the Schools in favour of the *fuga vacui* may be fufficiently enervated; yet it fuited better with the defign of this Treatife to propofe fome new Experiment, to illuftrate our *Hypothefis*; and though it feem'd to be far more difficult to do it in reference to Cupping Glaffes, than to other fubjects, yet I pitcht upon two different wayes of Experimenting; whofe fuccefs not difappointing me, I fhall now give Your Lordfhip an account of them,

We took a Glafs of about one Inch and a half in Diameter, but a good deal longer, than an ordinarily fhap'd Cupping Glafs of that breadth would have been, that there might be the more room for the flame to burn in it, and rarifie the Air. We alfo provided a Receiver fhap'd almoft like a Pear, this Receiver was open at both ends; at the fharper whereof there was but a fmall orifice, but at the obtufe end there rofe up a fhort neck, whofe Orifice was wide enough to admit with eafe the newly mentioned Cupping Glafs without touching the fides of it, and we were not willing it fhould be much larger, left it fhould not be fo exactly cover'd by the Palm of the hand that fhould be laid upon it, and left alfo the hand fhould be broken or hurt by the too great weight of the Atmofphere, when the included Air fhould be withdrawn from under it.

Thefe things being thus prepared, and the fmaller Orifice of the Receiver being faftned with Cement to the Engine, I caufed the Cupping Glafs to be faftned, with the mouth upwards, to the Palm of the hand of a Youth, (whom your Lordfhip may remember to have feen with me,) whofe hand feem'd fram'd by Nature for this Experiment, being broad, ftrong, and very plump. And

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having pull'd the Glass, to try whether it stuck well on, I caus'd him to put it into the Receiver, and lay his hand so upon the Orifice lately mentioned, that it might serve for a Cover to it, and hinder any Air from getting in between them.

That which we pretended was, that the Receiver being but small, (that it might be quickly exhausted, and so not put the Youth to a long pain,) upon an Exuction or two made with the Pump, of the Air about the Cupping Glass, the remaining Air should have its Pressure so far weakned, as not to be able to support the Cupping Glass; especially since if the Air without the Cupping Glass (but yet in the Receiver) should be more rarified by the removal of that which had been pump'd out, than the Air included in the Cupping Glass was by the precedent Heat; this last mentioned Air having a stronger Spring (or tendency to expand it self) than the External Air of the Receiver, the Glass must needs fall down, or rather be thrust off, though, in case there had been no Air at all left in the Cavity of the Cupping Glass, the Air in the Receiver would by its Pressure sustain a far Greater weight.

The Event of our Trial agreed very well with our conjecture. For upon the first Suck the Cupping Glass fell off, the weight of the Atmosphere pressing so hard upon the Young mans hand, that, though he be more than ordinary strong, he complain'd he could very hardly take it off the Glass it was almost thrust into, and, a while after, that his hand was very sore. But this last inconvenience became not so quickly very sensible, but that we had time to repeat our Experiment, by fastning the Cupping Glass more strongly than before; so that he complain'd that it drew in his hand very forceably, and though that part be not wont to be fleshy, yet the Tumour occasioned by the Cupping Glass was manifest enough to the eye: but as before, so now, at the very first turning of the Stop-cock, (to let out the Air of the Receiver,) the Cupping Glass fell off.

EXPERIMENT XXXVI.

About the making, without heat, a Cupping Glaſs to lift up a great Weight.

THe other Experiment I lately told Your Lordſhip we had made, to illuſtrate our Doctrine about the cauſe of the ſticking of applied Cupping Glaſſes, was tried after the following manner.

We took the Braſs-hoop or Ring, mentioned in the 5th and 6th Experiments, and cover'd it with a Bladder, (which was wetted to make it the more limber,) and was ſo tied on to it, (which was eaſie to do,) that the bottom of the Bladder covered the upper orifice of the Hoop, and was ſtretcht (though not ſtrongly) upon it, almoſt like the Membrane that makes the head of a Drumm; and the neck of the Bladder was tied with a ſtring near the middle of the lower Orifice of the Hoop, and in this lower part of the Bladder we made two or three ſmall Holes for the Air to paſs in and out at. Then having plac'd at the bottom of the often mentioned capp'd Receiver a thick piece of Wood, that had a hole in it, to receive the neck of the Bladder, we ſo plac'd the cover'd Hoop upon this piece of Wood, that the upper part of the Bladder lay parallel to the Horizon. This done, we ſuſpended, at the Turning-key belonging to the Cap of our Receiver, a blind head (as Chymiſts call it) of Glaſs, which for want of a true Cupping Glaſs we were fain to ſubſtitute, and which indeed was not very unlike one either for ſhape or ſize; and to the upper part of this Glaſs we faſtned a large Ring of Metal, the better to depreſs it, and make it lean ſtrongly on the Bladder.

Theſe things being thus made ready, and the Receiver cemented on to the Engine, we did by help of the Turning-key let down the Cupping Glaſs, (for ſo we ſhall hereafter call it,) till it came almoſt to touch the level Superficies of the Bladder; and when

when the Receiver was as far exhausted as we thought fit, (but not near as far as it might have been,) we let down the Cupping Glaſs a litle lower, ſo that it lean'd upon the Bladder, and touch'd it with all the parts of its orifice: ſo that the Cupping Glaſs with the ſubjacent Bladder was become an internal Receiver (if I may ſo call it,) whoſe Air was conſiderably expanded, and conſequently weakned as to its Spring. All this being done, we warily let the Air into the Receiver, and thereby the Air, that did ſurround the Cupping Glaſs, (which we juſt now called the Internal Receiver,) having now a ſtronger Preſſure than the Air in the Cupping Glaſs could reſiſt; the Bladder, on which the Cupping Glaſs reſted, was as we look'd for, thruſt up a pretty way into the cavity of the Glaſs, in which it made a conſpicuous Tumor; and was made to ſtick ſo cloſe to the orifice of it, that one would have thought that the Bladder had been violently drawn in, as the skin is wont to be in the ordinary applications of Cupping Glaſſes.

And becauſe we took notice, that though this Glaſs were not capacious, (for it ſcarce held a Pint of Water,) yet the orifice of it was not very narrow, (being in Diameter an inch and ¼,) we thought fit in repeating the Experiment to adde ſomething that ſeem'd odd enough, and was fit to manifeſt that Cupping Glaſſes may, without heat, by the bare Preſſure of the external Air, be more ſtrongly faſtned, than for ought we know they are by the help of flame. Having then reiterated the former Experiment with this onely variation, that we exhauſted the Receiver further than before, we took out the Cupping Glaſs and the Bladder, which together with the included Braſs-hoop was hanging at it; and then having tied the Glaſs to the Hook of a good *Statera*, and tied a large Scale to the neck of the Bladder, we put in by degrees Weights into the Scale, till we had loaded it enough to force off the Bladder from the Glaſs; which hapned not till the whole Weight, that tended to draw down the Bladder, amounted to 35 Pound (if not better,) of ſixteen ounces in the pound. Nor did we doubt, but that the Preſſure of the Atmoſphere would in our Experiment have kept up a much greater Weight, if we had, before we let in the outward Air, diligently exhauſted the Receiver; which we had purpoſely forborn to do, for fear the too diſproportionate Preſſure of the external Air ſhould break the Bladder: which puts me in mind of adding, upon the by, That as more Weight was put into the Scale, the Bladder (ſtretcht more and more by the Weight on one ſide, and the Air on the other,) appear'd to ſwell higher in the cavity of the Glaſs.

EXPERIMENT XXXVII.

Shewing, that Bellows, whoſe Noſe is very well ſtopt, will open of themſelves, when the Preſſure of the external Air is taken off.

IT is wont by the Peripateticks and others to be made a great argument for the *fuga vacui* which they attribute to Nature, That if the Noſe of a pair of Bellows be well ſtopt, one cannot open them by raiſing the upper board from the lower. But of this another reaſon may be eaſily aſſigned, without determining whether there be a *vacuum* or no, namely the Weight and Preſſure of the Air: for when the Noſe of a pair of Bellows, that are Tite enough, is well ſtopt, no Air being able to inſinuate it ſelf upon the disjoining of the boards into the Cavity made by that disjunction, This cannot be effected, but by ſuch a force as is *almoſt* able (I ſay *almoſt*, becauſe ordinary Bellows cannot be ſo well ſhut, but that there will remain ſome Air in them, whoſe Spring will facilitate the opening of them) to raiſe an Atmoſpherical Pillar, whoſe *Baſis* ſhall be the upper board, vvhich is commonly ſo large, that a leſs force may ſerve to break common Bellows, then to raiſe ſo great a Weight: but if they vvere made ſtrong enough, and there vvere applied a ſufficient force to lift ſo Great a vveight, as the newly mentioned Pillar of the Atmoſphere, the ſides might be be disjoyn'd, how close and ſtanch ſoever the Inſtrument vvere made.

Thus far one may argue upon the bare principle of the weight of the Air, but taking in the Spring of it too, I thought one might proceed ſo much further, that I ventur'd to foretell divers ingenious men, that if the Preſſure of the ambient Air were taken oft, not onely it would be eaſie to open the Bellows in ſpite of their being carefully ſtopt at the noſe, but that they would fly open as it were of their own accord, without the application of any external force at all. And 'twas partly to juſtifie this prediction, as well as to make a Trial, I thought more conſiderable, that we made the following Experiment.

We caus'd (then) to be made a pair of Bellows, differing from ordinary ones in theſe particulars. Firſt, that the Boards were circular, (and ſo without handles,) and of about *6* inches in Diameter: 2. That there was no Clack or Valve: 3. That the noſe was but an inch long, or leſs, (being to be lengthned if occaſion required vvith a Pipe:) 4. That the Leather (which vvas not ſpar'd, that the inſtrument might be the more capacious) was not horny or very ſtiff, but limber. The Reaſon of the firſt and third diverſity was, that the Bellows might be capable to be conveyed into our Receiver; (for vvhich purpoſe alſo, if there had appear'd need, the noſe might have been made in the uppermoſt of the two Boards:) the reaſon of the 2d variation was, that the inſtrument might be the more ſtanch: and of the 4th, that the *baſes* of the Bellows might (as in Organ-bellows) be clapt cloſer together, and harbour leſs Air in the wrinkles and cavity. So that when the Bellows vvere opened to their full extent, by drawing up the upper Baſis at a button purpoſely made in the midſt of it, the Bellows look'd like a Cylinder of 1*6* or 18 inches high; upon which reſemblance I take the liberty to call both the Boards (as Geometricians do both the circular parts of a Cylinder) Baſes.

But though theſe were made by an Artificer, otherwiſe dexterous, yet it not being his Trade to make Bellows, nor any other

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mans in the Town I then was in, he could not make them ſo Tite, but that in ſpite of our oyling the Leather, and choaking the Seams with good Cement, there was ſome litle and unperceived hole or cranny, whereby ſome Air had paſſage when the noſe was accurately ſtopt: but this was not ſo conſiderable, but that if we drew up the upper Baſis from the lower, the external Air would on all ſides preſs the Leather inwards, and ſo make the ſhape of the inſtrument very far from being ſo Cylindrical, as it would be if the noſe were left open.

Wherefore concluding, that notwithſtanding this imperfection the Bellows would ſerve, though not for both the Experiments I deſign'd, yet for one of them, we carefully ſtopt the noſe, after we had approach'd the Baſes to one another, and conveying them into a large Receiver, it quickly appear'd, when the Pump was ſet on work, that at every Exſuction of the incumbent Air, the Air harbour'd in the folds of the Leather, and the reſt of the litle Cavitie that could not but be left between the Baſes, made the upper of thoſe Baſes manifeſtly riſe, though its weight (becauſe of the thickneſs and ſolidity of the Wood) would ſoon after depreſs it again, either by driving out ſome of the Air at ſome place where the inſtrument was not ſufficiently Tite, or by making it as it were ſtrain'd through the Leather it ſelf; and if the Pump were agitated ſomewhat faſter than ordinary, the Expanſion of the internal Air would be greater than could be rendred quite ineffectual by ſo ſmall a Leak, and the upper part of the Bellows would be ſoon raiſ'd to a conſiderable height, as would appear more evidently if we haſtily let in the external Air, upon whoſe ingreſs the Baſes would be clapt together, and the upper of them a good vvay depreſt. So that the imperfection of the Bellows made the Experiment rather more than leſs concluding; for ſince there was no external force applied to open them, if notwithſtanding that ſome of the included Air could get out of thẽ, yet the Spring of the internal Air was ſtrong enough to open the Bellows when the ambient Air was withdrawn, much more

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would the effect have been produc'd, if the Bellows had been perfectly ſtanch.

EXPERIMENT XXXVIII.

About an Attempt to examine the Motions and Senſibility of the Carteſian Materia ſubtilis, *or the* Æther, *with a pair of Bellows (made of a Bladder) in the exhauſted Receiver.*

I Will not now diſcuſs the Controverſie betwixt ſome of the Modern Atomiſts, and the *Carteſians*; the former of whom think, that betwixt the Earth and the Stars, and betwixt theſe themſelves there are vaſt Tracts of Space that are empty, ſave where the beams of Light do paſs through them; and the later of whom tell us, that the Intervals betwixt the Stars and Planets (among which the Earth may perhaps be reckon'd) are perfectly fill'd, but by a Matter far ſubtiler than our Air, which ſome call Celeſtial, and others *Æther*. I ſhall not, I ſay, engage in this controverſie, but thus much ſeems evident, That if there be ſuch a Celeſtial Matter, it muſt make up far the Greateſt part of the Univerſe known to us. For the Interſtellar part of the world (if I may ſo ſtile it) bears ſo very great a proportion to the Globes, and their Atmoſpheres too, (if other Stars have any as well as the Earth,) that it is almoſt incomparably Greater in reſpect of them, than all our Atmoſphere is in reſpect of the Clouds, not to make the compariſon between the Sea and the Fiſhes that ſwim in it.

Wherefore I thought it might very vvell deſerve a heedful Enquiry, whether we can by ſenſible Experiments (for I hear what has been attempted by Speculative Arguments) diſcover any thing about the Exiſtence, or the Qualifications of this ſo vaſt Æther: and I hoped our Curioſity might be ſomewhat aſſiſted by our Engine, if I could manage in it ſuch a pair of Bellows as I deſign'd. For I propos'd to my ſelf to faſten a convenient weight to the upper Basis, and clog the lower with another, great enough to keep it Horizontal and immoveable, that when by the help of the Turning-key, frequently above mention'd, the upper Basis should be rais'd to its full height, the cavity of the Bellows might be brought to its full dimensions. This done, I intended to exhaust the Receiver, and consequently the thus open'd Bellows with more than ordinary diligence, that so both the Receiver and they might be carefully freed from Air. After vvhich I purpos'd to let go the upper Base of the Bellows, that being hastily deprest by the incumbent Weight, it might speedily enough fall down to the lower Basis, and by so much, and so quickly lessening the Cavity, might expell thence the Matter (if any were) before contain'd in it, and that (if it could by this way be done) at the hole of a slender Pipe, fasten'd either near the bottom of the Bellows, or in the upper Basis: against or over the orifice of which Pipe there was to be plac'd at a convenient distance either a Feather, or (if that should prove too light) the Sail of a litle Wind-mill made of Cards, or some other light body, and fit to be put into motion by the impulse of any Matter that should be forc'd out of the Pipe.

By this means it seem'd not improbable, that some such discovery might be made, as would not be altogether useless in our Enquiry. For if notwithstanding the absence of the Air, it should appear by the Effects that a stream of other Matter, capable to set visible bodies a moving, should issue out at the Pipe of the comprest Bellows; it would also appear, that there may be a much subtiller Body than common Air, and as yet unobserv'd by the Vacuists, or (their Adversaries) the Schools, that may even copiously be found in places deserted by that Air; and that it is not safe to conclude from the absence of the Air in our Receivers, and in the upper part of those Tubes where the *Torricellian* Experiment is made, that there is no other body left but an absolute Vacuity, or (as the Atomists call it) a *vacuũ coacervatũ*. But if on the other side there should appear no motion at all to be produc'd duc'd, so much as in the Feather, it seem'd that the Vacuists might plausibly argue, that either the Cavity of the Bellows was absolutely empty, or else that it would be very difficult to prove by any sensible Experiment that it was full, and, if by any other way of probation it be demonstrable, that it was replenish'd with Æther, we that have not yet declar'd for any party, may by our Experiment be taught to have no confident expectations of easily making it sensible by Mechanical Experiments; and may also be inform'd, that tis really so subtle and yielding a Matter, that does not either easily impell such light bodies as even Feathers, or sensibly resist as does the Air it self the motions of other bodies through it, and is able without resistance to make its passage through the Pores of Wood, and Leather, and also of closer bodies, which we find not that the Air doth in its Natural or wonted state penetrate.

To illustrate this last Clause I shall adde, that to make the Trial more accurate, I wav'd the use of other Bellows, (especially not having such as I desired,) & caus'd a pair of small Bellows to be made with a Bladder, as a Body, which some of our former Experiments have evinc'd to be of so close a Texture, that Air will rather break it than passe through it: and that the Bladder might no where loose its entireness by Seams, we glued on the two Bases, the one to the bottom, and the other to the opposite part of it, so that the Neck came out at a hole purposely made for it; in the upper Basis, and into the Neck it was easie to insert what pipe we thought fit, binding the Neck very close to it on the outside. We had likewise Thoughts to have another pair of Tite Bellows made with a very light Clack in the lower Basis, that by hastily drawing up the other Basis, when the Receiver and Bellows were very carefully exhausted, we might see by the rest, as the lifting up of the Clack, whether the subtle Matter that was expell'd by the upper Basis in its Ascent, would, according to the Modern Doctrine of the Circle made by moving Bodies, be impell'd up or not.

We alſo thought of placing the litle Pipe of the Bladder-bellows (if I may ſo call them) beneath the ſurface of Water exquiſitely freed from Air, that we might ſee whither upon the Depreſſion of the Bellows by the incumbent Weight, when the Receiver was carefully exhauſted, there would be any thing expell'd at the Pipe, that would produce Bubbles in the liquor, wherein its Orifice was immerſt.

To bring now our Conjectures to ſome Trial, we put into a capp'd Receiver the Bladder accommodated as before is mentioned, and though we could have wiſh'd it had been ſomewhat larger, becauſe it contain'd but between half a Pint and a Pint, yet in regard it was fine and limber, and otherwiſe fit for our Turn, we reſolv'd to try how it would do; and to depreſs the upper Baſis of theſe litle Bellows the more eaſily and uniformly, we cover'd the round piece of Paſtboard, that made the upper Baſis, with a Pewter-plate, (with a hole in it for the neck of the Bladder;) which nevertheleſs upon trial prov'd not ponderous enough, whereby we were oblig'd to aſſiſt it by laying on it a Weight of Lead. And to ſecure the above mentioned Feather, (which had a ſlender and flexible Stem, and was left broad at one end, and faſtned by Cement at the other, ſo as to ſtand with its broad end at a convenient diſtance juſt over the Orifice of the Pipe,) from being blown aſide to either hand, we made it to move in a perpendicular ſlit in a piece of Paſtboard, that was faſtned to one part of the upper Baſis, as that which the Feather was glued to was to another part. Theſe things being thus provided, the Pump was ſet a work, and as the ambient Air was from time to time withdrawn, ſo the Air in the Bladder expanded it ſelf ſo ſtrongly, as to lift up the metalline Weight, and yet in part to ſally out at the litle Glaſs-pipe of our Bellows, as appear'd by its blowing up the Feather, and keeping it ſuſpended till the Spring of the Air in the bladder was too far weakned to continue to do as it had done. In the mean time we did now and then, by the help of a ſtring faſten'd to the Turning-key, and the upper Baſis of the Bellows, let

See Plate the Fig. the

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down that Baſis a litle, to obſerve how upon its ſinking the blaſt againſt the Feather would decreaſe, as the Receiver was further and further exhauſted. And when we judg'd it to be ſufficiently freed from Air, we then let down the Weight, but could not perceive that by ſhutting of the Bellows the Feather was at all blown up, as it had been wont to be, though the upper Baſis were more than uſually depreſt. And yet it ſeems ſomewhat odd, that when, for Curioſity, in order to a further Trial, the Weight was drawn up again, as the upper baſis was rais'd from the lower, the ſides of the Bladder were ſenſibly (though not very much) preſt, or drawn inwards. The Bellows being thus opened, we let down the upper baſis again, but could not perceive that any blaſt was produc'd; for though the Feather, that lay juſt over and near the orifice of the litle Glaſs Pipe, had ſome motion, yet this ſeem'd plainly to be but a ſhaking and almoſt vibrating motion (to the right and left hand,) which it was put into by the upper baſis, which the ſtring kept from a ſmooth and uniform deſcent; but not to proceed from any blaſt iſſuing out of the cavity of the Bladder. And for further ſatisfaction we caus'd ſome Air to be let into the Receiver, becauſe there was a poſſibility, that unawares to us the ſlender Pipe might by ſome accident be choak'd: but though upon the return of the Air into the Receiver, the baſes of the Bellows were preſt cloſer together, yet it ſeem'd that, according to our Expectation, ſome litle Air got through the Pipe into the cavity of the Bladder: for when we began to vvithdraw again the Air we had let into the Receiver, the Bladder began to ſwell again, and upon our letting down the Weight, to blow up and keep up the Feather, as had been done before the Receiver had been ſo well exhauſted. What conjecture the opening and ſhutting of our litle Bellows, more than once or twice, without producing any blaſt ſenſible by the raiſing of the Feather, gave ſome of the by-ſtanders, may be eaſily gueſs'd by the preamble of this Experiment; but whilſt I was endeavouring to proſecute it for my own further information, a miſchance that befell

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the Instrument, kept me from giving my self the desir'd satisfaction.

EXPERIMENT XXXIX.

About a further attempt to prosecute the Inquiry propos'd in the foregoing Experiment.

CONsidering with my self, that by the help of some contrivances not difficult, a Syringe might be made to serve, as far as our present occasion required, in stead of a pair of Bellows; I thought it would not be improper to try a differing, and, in some regards, a better way to prosecute an attempt, which seem'd to me to deserve our Curiosity.

See plate the Figure the

I caus'd then to be made, for the formerly mentioned Syringe, in stead of its streight Pipe, a crooked one; whose shorter Leg was parallel to the longer. And this Pipe was for greater closeness, after 'twas screw'd on carefully, fastned with Cement to the Barrel; and because the Brass-pipe could scarce be made small enough, we caus'd a short and very slender Pipe of Glass to be put into the orifice of the shorter Leg, and diligently fasten'd to it with close Cement. Then we caus'd the Sucker (by the help of Oyl, Water, and moving it up and down) to be made to go as smoothly as might be, without lessening the stanchness of the Syringe. After this, there was fastned to the handle of the Rammer a Weight, made in the form of a Ring, or Hoop, which by reason of its figure might be suspended from the newly mention'd handle of the Rammer, and hang loose on the outside of the Cylinder, and which both by its Figure and its Weight might evenly and swiftly enough depress the Sucker, when That being drawn up the Weight should be let go. This Syringe thus furnished, was fastned to a broad and heavy Pedestal, to keep it in its vertical posture, and to hinder it from Tottering, notwithstanding the Weight that clogg'd it. And besides all these things, there

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was taken a Feather, which was about two inches long, and of which there was left at the end a piece about the breadth of a mans Thumb-naile, (the reſt on either ſide of the ſlender ſtalk (if I may ſo call it) being ſtript off) to cover the hole of the ſlender Glaſs pipe of the Syringe; for which purpoſe the other extreme of it was ſo faſtned with Cement to the lower part of the Syring, (or to its Pedeſtal,) that the broad end of the Feather was plac'd (as the other Feather was in the foregoing Experiment) juſt over the litle orifice of the Glaſs, at ſuch a convenient diſtance, that when the Sucker was a litle (though but very litle) drawn up and let go again, the Weight would depreſs it faſt enough to blow up the broad part of the Feather, as high as was permitted by the reſiſtance of the Stalk, (and that was a good way,) the Spring of which would preſently reſtore the whole Feather to its former poſition.

All theſe things being done, and the handle of the Rammer being tied to the Turning-key of a capp'd Receiver, the Syringe and its Pedeſtal were incloſed in a capacious Receiver, (for none but ſuch a one could contain them, and give ſcope for the Rammers motions,) and the Pump being ſet on worke, we did, after ſome quantity of Air was drawn out, raiſe the Sucker a litle by the help of the Turning-key, and then turning the ſame Key the contrary way we ſuffer'd the Weight to depreſs the Sucker, that we might ſee at what rate the Feather would be blown up; and finding that it was impell'd forceably enough, we caus'd the pumping to be ſo continued, that a pretty many pauſes were made, during each of which we rais'd and depreſs'd the Sucker as before, and had the opportunity to obſerve, That as the Receiver was more and more exhauſted of the Air, ſo the Feather was leſs and leſs briskly driven up, till at length, when the Receiver was well emptied, the uſual elevations and depreſſions of the Sucker would not blow it up at all that I could perceive, though they were far more frequently repeated than ever before; nor was I content to look heedfully my ſelf, but I made one whom I had

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often imploy'd about Pneumatical Experiments to watch attentively, whilst I drew up, and let down the Sucker, but he affirm'd that he could not discern the least beginning of Ascension in the Feather. And indeed to both of us it seem'd, that the litle and inconsiderable motion that was sometimes (not alwayes) to be discern'd in the Feather, proceeded not from any thing that issued out of the Pipe, but from some litle Shake, which twas difficult not to give the Syringe and Pedestal, by the raising and depressing of the Sucker.

And that which made our *Phænomenon* the more considerable, was, that the Weight that carried down the Sucker being still the same, and the motions of the Turning-key being easie to be made equal at several times, there seem'd no reason to suspect that Contingencies did much (if at all) favour the success; but there hapned a thing, which did manifestly enough disfavour it. For I remember, that before the Syringe was put into the Receiver, when we were trying how the Weight would depress it, and it was thought that though the Weight were conveniently shap'd, yet it was a litle of the least; I would not alter it, but foretold, that when the Air in the Cavity of the Syringe (that now resisted the quickness of its descent, because so much Air could not easily and nimbly get out at so small a Pipe) should be exhausted with the other Air of the Receiver, the elevated Sucker would fall down more easily, which he, that was imploy'd to manage the Syringe whilst I watch'd the Feather, affirm'd himself afterwards to observe very evidently. So that when the Receiver was exhausted, if there had been in the cavity of the Syringe a matter as fit as Air to make a Wind of, the Blast ought to have been Greater, because the celerity that the Sucker was deprest with was so.

After we had long enough tried in vain to raise the Feather, I order'd some Air to be let into the Receiver; and though when the admitted Air was but very litle, the motions of the Sucker had scarce if at all any sensible operation upon the Feather, yet when the quantity of Air began to be somewhat considerable, the Feather began to be a litle mov'd upwards, and ſo by letting in Air not all at once but more and more from time to time, and by moving the Sucker up and down in the intervals of thoſe times of admiſſion, we had the opportunity to obſerve, that as the Receiver had more Air in it, the Feather would be more briskly blown up.

But not content with a ſingle Tryal of an Experiment of this conſequence, we cauſed the Receiver to be again exhauſted, and proſecuted the Tryal with the like ſucceſs as before, onely this one Circumſtance, that we added for confirmation, may be fit to be here taken notice of. Having, after the Receiver was exhauſted, drawn up and let fall the Sucker divers times ineffectually; *though* hitherto we had not uſually rais'd it any higher at a time, than we could by one turn of the hand, both becauſe we could not ſo conveniently raiſe it higher by the Hand alone, and becauſe we thought it unneceſſary, ſince that height ſuffic'd to make the Air briskly toſs up the Feather; yet *ex abundanti* we novv took an inſtrument that was pretty long and fit ſo to take hold on the Turning-key, that we could eaſily raiſe the Sucker between two and three inches (by our Æſtimate) at a time, and nimbly depreſs it again; and for all this, which would much have increas'd the Blaſt, if there had been a Matter fit for it in the Cavity of the Syringe, we could not ſenſibly blow up the Feather, till we had let a litle Air into the Receiver.

To be able to make an æſtimate of the Quantity of Air pump'd out, or let in, when the Feather vvas ſtrongly or faintly, or not at all rais'd by the fall of the Sucker; vve took off the Receiver, and convey'd a Gage into it, but though for a vvhile vve made ſome uſe of our Gage, yet a miſchance befalling it before the Operation was quite ended, I ſhall forbear to adde any thing concerning that Tryal, and proceed to ſay ſomething of another Attempt, wherein though I foreſaw and met with ſuch difficulties, as kept me from doing altogether what I deſired, yet the ſucceſs being almoſt as good as could be expected, I ſhall venture

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to acquaint Your Lordſhip with the Tryal, which was this.

In ſtead of the hitherto imploy'd Pipe of Braſs, there was well faſtned (with Cement) to the Syringe a Pipe of Glaſs, whoſe figure differ'd from that of the other in this particular, that the ſhorter (or remoter) Leg of our new Pipe, after it had for a while been carried parallel to the other Leg, was bent off ſo, that above an inch and a half of it tended downwards, that the orifice of it might be immerſt into Water contain'd in a ſmall open Jarr. The deſign of which contrivance was, that when the Receiver ſhould be well exhauſted, we might (according to what I told Your Lordſhip vvas at firſt deſign'd) try vvhether by the raiſing and depreſſing of the Sucker any ſuch Matter would be driven out at the noſe of the Pipe, as would produce bubbles in the incumbent Water, which, Air (though highly rarefied, perhaps to ſome hundreds of times beyond its wonted Dimenſions,) is capable of doing. And I chooſe to imploy rather Water than Quick-ſilver, becauſe though by uſing the later I might hope to be leſs troubled with bubbles, yet the ponderouſneſs and opacity of it ſeem'd to outweigh that convenience.

I need not tell Your Lordſhip, that in other reſpects this Experiment was made like the former, ſo that I ſhall mention onely its peculiarities, which were, That as the Air was pump'd out of the Receiver, that in the Glaſs pipe made its way through the Water in Bubbles, and a litle Air having once by a ſmall Leak got in, and forc'd ſome of the Water out of the Jarr into the pipe, when the Receiver was again vvell emptied, both that Water and even the litle quantity of ſtagnant Water, that was contain'd in the immerſt part of the Pipe, produc'd ſo many bubbles of ſeveral ſizes, as quite diſturb'd our Obſervations. Wherefore we let alone the Receiver, exhauſted as it was, for 6 or 7 hours, to give the Water time to be freed from Air, and then cauſing what Air might have ſtolen in to be again pump'd out, till we had perceiv'd by the Gage that the Receiver was well exhauſted, we caus'd the Sucker (of the Syringe) to be rais'd and depreſt diverſe

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times, and though even then a Bubble vvould now and then make our Obſervations troubleſome, and leſs certain, yet it ſeem'd to us, that when we were not thus confounded, we ſometimes obſerved that the elevation and fall of the Sucker, though reiterated, did not drive out at the Pipe any thing that made any diſcernable bubbles in the incumbent Water; for though there would appear now and then ſome ſmall bubbles on the ſurface of the Water, yet I could not perceive that the Matter that made them, iſſued out at the Pipe; and ſome of them manifeſtly proceeded from Aerial Particles, till then lurking in the Water, as I concluded from the place and time of their riſing. But this Non-eruption of bubles at the noſe of the Pipe, vvas not that which gave me the moſt of ſatisfaction. For at length both I and another had the opportunity to obſerve the Water in the immerſt part of the Pipe, which was very ſlender, to be about an inch higher than the reſt of the ſtagnant Water, and to continue at that height or place in the Pipe, though the Sucker vvere divers times together rais'd and depreſſ'd by Gueſs between 2 and three inches at a time. Which ſeem'd to argue, either that there was a *vacuum* in the cavity of the Syringe, or elſe that if it were full of *Æther*, that body vvas ſo ſubtle, that the impulſe it received from the falling Sucker vvould not make it diſplace a very litle Thread (perhaps not exceeding a Grain in Weight) of Water that vvas in the ſlender Pipe, though it appear'd by the bubbles, that ſometimes diſclos'd themſelves in the Water, after the Receiver had been exhauſted, that far more Water vvould be diſplac'd and carried up by a ſmall bubble conſiſting of ſuch rarified Air, that according to my Æſtimate the Aerial particles of it did not, before the Pump vvas begun to be ſet on vvork, take up in the Water a five-hundredth part of the quantity of a Pins head.

But whilſt we were conſidering what to do further in our Tryal, a litle Air, that ſtrain'd in at ſome ſmall undiſcoverable Leak, drove the Water into the emptied part of the Pipe, and put an end for that time to our Tryal, which had been too toylſome to invite us then to reiterate it.

I had indeed thoughts of prosecuting the Enquiry, *by* dropping from the top of the exhausted Receiver light Bodies conveniently shap'd, to be turn'd round, or otherwise put out of their simplest motion of Descent, if they met with any resistance in their fall; and *by* making such Bodies move Horizontally and otherwise in the Receiver, as vvould probably discover whither they were assisted by the *medium*: and other contrivances and wayes I had in my thoughts, whereby to prosecute our Enquiry, but vvanting time for other Experiments, I could not spare so much as was necessary to exhaust large Receivers so diligently, as such nice Trials would exact; and therefore I resolv'd to desist, till I had more leisure than I then had, (or have since been Master of.)

In the interim, thus much we seem to have already discovered by our past Tryals, that if when our Vessels are very diligently freed from Air, they are full of Æther, that Æther is such a body, as will not be made sensibly to move a light Feather by such an impulse as would make the Air manifestly move it, not onely whilst tis no thinner than common Air, but when tis very highly rarified, (which, if I mistake not, it was in our Experiment so much, as to be brought to take up above an hundred times more room than before.)

And one thing more we gain'd by the Tryal made with water, namely a clear confirmation of what I deliver'd in the 34th Experiment, about the cause of the Suction that is made by Syringes; for Your Lordship may remember, that at the close of the Experiment we have all this while been reciting, I observ'd, that when the external Air was so very well withdrawn, the pulling up of the Sucker would not make the stagnant Water, that the Pipe of the Syringe was immerst in, to ascend one inch, or so much as the tenth part of it.

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EXPERIMENT XL.

About the falling, in the Exhauſted Receiver, of a light Body, fitted to have its motion viſibly varied by a ſmall reſiſtance of the Air.

PArtly to try whether in the ſpace deſerted by the Air, drawn out of our Receivers, there would be any thing more fit to reſiſt the motion of other light Bodies through it, than in the former Experiment we found It to impell them into motion; and partly for another purpoſe to be mention'd by and by, we made the following Tryals.

We took a Receiver, which, though leſs tall than we would have had, was the longeſt we could procure: and that we might be able, not ſo properly to let down as, to let fall a Body in it, we ſo faſtned a ſmall pair of Tobacco-Tongs to the inſide of the Receivers Braſs-Cover, that by moving the Turning-key, we might by a ſtring tied to one part of them, open the Tongs, which elſe their own Spring would keep ſhut. This being done, the next thing was to provide a Body, which vvould not fall down like a Stone, or another dead Weight through the Air, but would in the manner of its deſcent ſhew, that its motion was ſomewhat reſiſted by the Air; vvherefore that vve might have a Body that vvould be turn'd about Horizontally (as it were) in its fall, we thought fit to joyn Croſs-wiſe four broad and light Feathers (each about an Inch long) at their Quils with a litle Cement, into vvhich vve alſo ſtuck perpendicularly a ſmall Label of Paper, about an 8th of an inch in breadth, and ſomewhat more in height, by vvhich the Tongues might take hold of our light Inſtrument vvithout touching the Cement, which elſe might ſtick to them.

By the help of this ſmall piece of Paper, the litle Inſtrument, of vvhich it made a part, vvas ſo taken hold of by the Tongs, that it hung as Horizontal as ſuch a thing could well be plac'd: *See Plate the Fig. 1*

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and then the Receiver being cemented on to the Engine; the Pump vvas diligently ply'd, till it appear'd by a Gage, which had been conveyed in, that the Reciver had been carefully exhausted: Lastly, our eyes being attentively fix'd upon the connected Feathers, the Tongs were by the help of the Turning-key open'd, and the litle Instrument let fall, which, though in the Air it had made some turns in its descent from the same height it now fell from, yet now it descended like a dead Weight, without being perceiv'd by any of us to make so much as one Turn, or a part of it: notwithstanding which I did, for greater security, cause the Receiver to be taken off, and put on again, after the Feathers were taken hold of by the Tongs , whence being let fall in the Receiver unexhausted, they made some Turns in their descent, as they also did being a second time let fall after the same manner.

But when after this, the Feathers being plac'd as before, we repeated the Experiment by carefully pumping out the Air, neither I nor any of the By-standers could perceive any thing of Turning in the descent of the Feathers; and yet for further security we let them fall twice more in the unexhausted Receiver, and found them to turn in falling as before; whereas when we did a 3^{d} time let them fall in the well exhausted Receiver, they fell after the same manner as they had done formerly, when the Air, that vvould by its resistance have turn'd them round, vvas remov'd out of their vvay.

Note 1. though (as I intimated above) the Glass, vvherein this Experiment was made, were nothing near so tall as I would have had it, yet it was taller than any of our ordinary Receivers, it being in height about 22 inches.

2. One that had had more leisure and conveniency, might have made a more commodious Instrument than that we made use of: for being accidentally visited by that Sagacious Mathematician D^{r} *Wren*, and speaking to him of this matter, he was pleas'd with great dexterity as well as readiness to make me a litle Instrument of Paper, on which, when twas let fall, the resi-

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ſtance of the Air had ſo manifeſt an operation, that I ſhould have made uſe of it in our Experiment, had it not been caſually loſt when the ingenious Maker was gone out of theſe parts.

3. Though I have but briefly related our having ſo order'd the matter, that we could conveniently let fall a Body in the Receiver when very well exhauſted, yet to contrive and put in practice what was neceſſary to perform this, was not ſo very eaſie, and it would be difficult to deſcribe it circumſtantially without very many words; for which reaſon I forbear an account, that would prove too tedious to us both.

4. What has been hitherto related, was done in proſecution of but one of the two Deſigns I aim'd at in the foregoing Contrivance, by which I intended, if I could have procured a Receiver tall enough, to try whether Bodies (ſome very light, and ſome heavier) being let fall when the Air was very diligently pump'd out, would not deſcend ſomewhat faſter than if the Receiver were full of Air. But though I had provided a *Pendulum* that vibrated quarters of Seconds, yet the Glaſs being no higher than it was, the Deſcent even of our Feathers took up ſo litle time, that even this *Pendulum* was of no uſe; onely it ſeem'd to all of us that were preſent at making the above recited Tryals, that when the Feathers were let fall at ſuch times as the Air (that would have turn'd them round in their deſcent) was removed, they came to the bottom ſenſibly ſooner than at other times. But when we ſhall have opportunity to repeat the Experiment in taller Glaſſes, and to make ſome variation of it, I hope to be able to give Your Lordſhip a fuller ſatisfaction about this Particular. And in the mean while I ſhall forbear to examine whether the Air might ſomewhat retard the deſcent of the Feathers upon ſome other account, or meerly upon that of its being a *medium* not quite devoid of Gravity.

Annotations.

1. But here I muſt be ſo ſincere as to inform Your Lordſhip, that this 40th Experiment ſeem'd not to prove ſo much as did the fore-

foregoing made with the Syringe: for being ſuſpicious that, to make the feathered body above mentioned turn in its fall, there would need a reſiſtance not altogether inconſiderable, I caus'd the Experiment to be repeated, when the Receiver was by our Æſtimate (which was not made at random neither) litle or nothing more than half exhauſted, and yet the remaining Air was too far rarified to make the falling Body manifeſtly turn.

2. And yet perchance it would have hapned otherwiſe, if the Receiver had been tall enough; which though I had not then leaſure and conveniency to make it, yet it will not be amiſs to let Your Lorſhip know by what means we did, that it might be ſomewhat fit to make the recited Experiment and ſome others, bring it to the height it had, which did conſiderably exceed that of the talleſt Glaſs we could then procure.

To lengthen our Receiver therefore, we thought fit to try, whether we could not cloſe enough faſten to the bottom of it with very good Cement a Cylindrical Pipe of Laton, whoſe upper orifice ſhould have neer the ſame breadth with the bottom of the Glaſs. And though this Contrivance ſeem'd liable to a couple of not mean difficulties; The one, that the Laton being every where bended, and in ſome places neceſſary to be ſouder'd, it would be very hard (as indeed we found it) to avoid ſome ſmall cracks and leaks: And the other, that if the metalline Pipe were wide enough, ſo great and heavy a pillar of the Atmoſphere would come to bear againſt it, as to preſs it inwards, if not alſo to break it; yet we hoped we ſhould be able to obviate both of theſe inconveniences. Againſt the firſt of which our Remedy was, to Coat over very carefully the whole Pipe with the ſame cloſe Cement, wherewith we faſtned it to the Glaſs Receiver. And againſt the Second, we provided a litle Frame, conſiſting of divers ſmall Iron Bars faſtned together; which Frame (though twere not too wide to go into the Cylinder of Laton, yet it) was wide enough to be ſo neer it on the inſide, that (though the weight of the Atmoſphere ſhould, as we feared, preſs the Laton ſo as to make

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it yield inward, yet) it could make it bend no further than the Iron-frame would permit; which was not far enough to ſpoile either the Receiver or the Experiment. And this not unpleaſant *phænomenon* would ſomewhat ſurpriſe unaccuſtomed Spectators, that when after the Receiver had been very well exhauſted, the external Air was permitted to return, there would be heard during ſome time, from the metalline part of the Receiver, divers Sounds briſk enough, which would make an odd Cracking noiſe proceeding from the Laton-plate, which having been forceably, though but ſlowly, bent inwards by the predominant Preſſure of the Atmoſphere, was now aſſiſted by the Preſſure of the returning Air, to regain its former Figure. And as I thought not fit to omit this Circumſtance, becauſe it confirms the practicableneſs of the Remedy propos'd againſt the 2d Inconvenience; ſo I thought fit to mention this way of enlarging and heightning Receivers, becauſe what we have related ſeems to give Grounds of hoping that this Contrivance may be made good uſe of in divers other Tryals, and particularly in attempts to make Receivers capacious enough to contain larger Animals, and perhaps even a Boy, or a Man. In order to ſome of which purpoſes we indeavoured to get an improvement made of our Metalline Cylinder by additional contrivances; but could not (where we then were) get Artificers, that would perform what was directed.

EXPERIMENT XLI.

About the propagation of Sounds in the Exhauſted Receiver.

TO make ſome further Obſervation than is mention'd in the *Publiſh'd Experiments, about the Production and conveying of Sounds in a Glaſs whence the Air is drawn out, we imploy'd a Contrivance, of which (becauſe we make uſe of it in di- ** Page the 105. 106.*

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vers other Experiments) it will be requisite to give Your Lordship here some short description.

We caus'd to be made at the Turners a Cylinder of Box, or the like close and firme Wood, and of a length suitable to that of the Receiver it was to be imploy'd in. Out of the lower Basis of this Cylinder (which might be about an inch and a half in Diameter) there came a smaller Cylinder or Axle-tree not a quarter so thick as the other, and less than an inch long: this was Turn'd very true, that it might *move* to & fro (or, as the Tradesmen call it, Ride) very smoothly in a litle Ferrule or Ring of Brass, that was by the same Turner made for it in the midst of the fixt Trencher, (as we call a piece of solid Wood shap'd like a Milstone,) being 4 or 5 inches (more or less according to the wideness of the Receiver) in breadth, and between one and two in thickness; and in a large and round Groove, or Gutter, purposely made in the lower part of this Trencher, I caus'd as much Lead as would fill it up to be plac'd and fasten'd, that it might keep the Trencher from being easily mov'd out of its place or posture, and in the upper part of this Trencher it was intended that Holes should be made at such places as should be thought fit, to place bodies at several distances as occasion should require. The upper Basis of the Cylinder had also coming out of the midst of it another Axletree, but wider than the former, that, into a Cavity made in it, it might receive the lower end of the Turning-key divers times already mentioned, to which twas to be fastned by a slender peg of Brass, thrust through two correspondent holes, the one made in the Key, and the other in the newly mentioned Socket (if I may so call it) of the Axletree. Besides all which, there were divers Horizontal Perforations bored here and there in the Pillar it self, to which this Axis belong'd, which Pillar we shall to avoid ambiguity call the Vertical Cylinder. The general use of this contrivance (whose other parts need not to be mentioned before the Experiments where they are imploy'd) is, that the end of the Turning-key being put into the Socket, and the lower Axis of the

See plate the Figure the

the Vertical Cylinder into the Trencher, by the motion of the Key a Body faſten'd at one of the holes to the Cylinder may be approach'd too, or remov'd from, or made to rub or ſtrike againſt another Body faſtned in a convenient poſture to the upper part of the Trencher.

To come now to our Tryal about Sounds, vve caus'd a Hand-Bell (vvhoſe Handle and Clapper were taken away) to be ſo faſtned to a ſtrong Wire, that, one end of the Wire being made faſt in the Trencher, the other end, vvhich vvas purpoſely bent downwards, took hold of the Bell. In another hole, made in the circumference of the ſame Trencher, vvas vvedg'd in (vvith a wooden Peg) a Steel-ſpring, to whoſe upper part was tied a Gad of Iron or Steel, leſs than an inch long, but of a pretty thickneſs. The length of this Spring was ſuch, as to make the upper part of the Hammer (if I may ſo call the piece of Iron) of the ſame height with the Bell, and the diſtance of the Spring from the Bell was ſuch, that when it was forc'd back the other way, it might at its return make the Hammer ſtrike briskly upon the outſide of the Bell. *See the Figure laſt referr'd to.*

The Trencher being thus furniſht and plac'd in a Capp'd Receiver, (as You know, for brevity ſake, we uſe to call one that is fitted with one or other of the Braſs Covers, often mentioned already,) the Air was diligently pump'd out; and then, by the help of the Turning-key, the Vertical Cylinder was made to go round, by which means as often as either of a couple of ſtiff Wires, or ſmall Pegs, that were faſtned at right Angles into holes, made not far from the bottom of the Cylinder, paſs'd (*under* the *Bell*, and) by the lately mentioned Spring; they forceably did in their paſſage bend it from the Bell, by which means, as ſoon as the Wire was gone by, and the Spring ceas'd to be preſs'd, it would fly back with violence, enough to make the Hammer give a ſmart ſtroak upon the Bell. And by this means we could both continue the Experiment at diſcretion, and make the percuſſions more equally ſtrong than it would otherwiſe have been eaſie to do.

The event of our Tryal was; That, when the Receiver was vvell emptied, it sometimes seem'd doubtful, especially to some of the By-standers, whether any Sound were produc'd or no; but to me for the most part it seem'd, that after much attention I heard a Sound, that I could but just hear; and yet, vvhich is odd, me thought it had somewhat of the nature of Shrilness in it, but seem'd (which is not strange) to come from a good way off. Whether the often turning of the Cylindrical Key kept the Receiver from being so stanch as else it vvould have been, upon vvhich score some litle Air might insinuate it self, I shall not positively determine: but to discover vvhat interest the Presence or the absence of the Air might have in the Loudness or Lowness of the Sound, I caus'd the Air to be let into the Receiver, not all at once but at several times, with competent intervals between them; by which Expedient it was easie to observe, that the Vertical Cylinder being still made to go round, when a litle Air vvas let in, the stroak of the Hammer upon the Bell (that before could now and then not be heard, and for the most part be but very scarcely heard) began to be easily heard. And when a litle more Air was let in, the Sound grew more and more audible, and so increased, till the Receiver was again replenished with Air; though even then (that we omit not That *phænomenon*) the Sound was observ'd to be much less loud than when the Receiver was not interpos'd between the Bell and the Ear.

And whereas in the already publish'd Physico-Mechanical Experiments I acquainted Your Lordship with what I observ'd about the Sound of an ordinary Watch in the Exhausted Receiver, I shall now adde, that That Experiment was repeated not long since, with the addition of suspending in the Receiver a Watch, with a good Alarum, which was purposely so set, that it might, before it should begin to ring, give us time to cement on the Receiver very carefully, exhaust it very diligently, and settle our selves in a silent and attentive posture. And to make this Experiment in some respect more accurate than the others we made

made of Sounds, we ſecur'd our ſelves againſt any leaking at the Top, by imploying a Receiver that was made all of one piece of Glaſs, (and conſequently had no Cover cemented on to it,) being furniſh'd onely within (when twas firſt blown) with a Glaſs-knob or Button, to which a ſtring might be tied. And becauſe it might be ſuſpected, that if the Watch were ſuſpended by its own Silver Chain, the tremulous motion of its ſounding Bell might be propagated by that Metalline Chain to the upper part of the Glaſs; to obviate this as well as we could, we hung the Watch, not by its Chain, but by a very ſlender Thread, whoſe upper end was faſtned to the newly mentioned Glaſs-button.

Theſe things being done, and the Air being carefully pump'd out, we ſilently expected the time when the Alarum ſhould begin to ring, which 'twas eaſie to know by the help of our other Watches; but not hearing any noiſe ſo ſoon as we expected, it would perhaps have been doubted whether the Watch continued Going, if for prevention we had not order'd the matter ſo, that we could diſcern it did not ſtand ſtill. Wherefore I deſir'd an ingenious Gentleman to hold his Ear juſt over the Button, at which the Watch was ſuſpended, and to hold it alſo very near to the Receiver, upon which he told us that he could perceive, and but juſt perceive ſomething of Sound, that ſeem'd to come from far; though neither we that liſtned very attentively near other parts of the Receiver, nor he, if his Ears were no more advantaged in point of poſition than Ours, were ſatisfied that we heard the Watch at all. Wherefore ordering ſome Air to be let in, we did by the help of attention begin to hear the Alarum; whoſe Sound was odd enough, and, by returning the Stop-cock to keep any more Air from getting in, we kept the Sound thus low for a pretty while, after which a litle more Air, that was permitted to enter, made it become more audible; and when the Air was yet more freely admitted, the by-ſtanders could plainly hear the noiſe of the yet continuing Alarum at a conſiderable diſtance from the Receiver.

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From what has hitherto been related we may learn what is to be thought of what is delivered by the Learned *Merſennus*, in that Book of his *Harmonicks*, where he makes this to be the firſt Propoſition. *Sonus à Campanis, vel aliis corporibus non ſolùm producitur in illo vacuo (quicquid tandem illud ſit,) quod fit in Tubis Hydrargyro plenis, poſteaq; depletis, ſed etiam idem acumen, quod in Aere libero vel clauſo penitus obſervatur & auditur.* For the proof of which Aſſertion, not long after, he ſpeaks thus: *Porro variis Tubis, quorum extremis lagenæ vitreæ adglutinantur, obſervari Campanas in illo vacuo appenſas, propriiſq; malleis percuſſas idem penitus acumen retinere, quod in Aere libero habent: atq; ſoni magnitudinem ei ſono, qui fit in Aere quem Tubus clauſus includit, nihil cedere.* But though our Experiments ſufficiently manifeſt that the preſence or abſence of the common Air is of no ſmall importance as to the conveying of Sounds, and that the interpoſition of Glaſs may ſenſibly weaken them; yet ſo diligent and faithful a Writer as *Merſennus* deſerves to be favourably treated: and therefore I ſhall repreſent on his behalf, that what he ſayes may well enough have been true, as far as could be gathered from the Tryals he made. For Firſt, tis no eaſie matter, eſpecially for thoſe that have not peculiar and very cloſe Cements, to keep the Air quite out for any conſiderable time in veſſels conſiſting of divers pieces, ſuch as he appears to have made uſe of. And next, the bigneſs of the Bell in reference to the capacity of the exhauſted Glaſs, and the thickneſs of the Glaſs, and the manner whereby the Bell was faſtned to the inſide of the Glaſs, and the Hammer or Clapper was made to ſtrike, may much vary the Effect of the Tryal, for Reaſons eaſie to be gather'd out of the paſt Diſcourſe, and therefore not needful to be here inſiſted on. And upon this Account we choſe to make our Experiment, with ſounds that ſhould not be ſtrong or loud, and to produce them after ſuch a manner, as that as litle ſhaking as could be might be given by the ſounding Body to the Glaſs 'twas included in. The Propoſal made by the ſame *Merſennus*, to have thoſe that have induſtry enough

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nough, try whether a Bag-pipe will be made to afford the ſame Sound as in the open Air, in ſuch Veſſels as he uſed for his Bels, though he ſeems to think it would ſucceed, is that which Your Lordſhip will not, I preſume, ſollicite me to make Tryal of, if You remember what is related in the almoſt immediately forego-ing Experiments, ſhewing, That we could make nothing come out of the Cavity of a pair of Bellows, that had force enough to blow away a Feather, when that Cavity was freed from Air, as the Bagpipe would be by the ſame operation, that empties the Glaſs that contains it, or elſe the Sound would not be made in ſuch a *Vacuum* as the ſcope of the Experiment requires.

If I had had Conveniency, I would have made ſome Tryals by conveying a ſmall ſtring'd Inſtrument (perhaps ſome ſuch as they commonly call a *Kit*) exactly tun'd, into a large Receiver, and then upon briskly ſtriking the String of a bigger Inſtrument, (tuned, as they ſpeak, to an Uniſon to (or with) that of the ſmaller Inſtrument) I ſhould have taken notice, whether the Sound would have been ſo uniformly propagated, notwithſtanding the Inter-poſition of the Glaſs Receiver, as ſenſibly to ſhake the included String; in order to the diſcerning of which, a bended piece of Straw, or Feather, or ſome ſuch light body, was to be hors'd up-on the String to be ſhaken. I alſo intended, in caſe the ſtring *were* made to move, to make the like Tryal after the Receiver was diligently exhauſted. And laſtly I deſign'd to try, whether two Uniſon ſtrings of the ſame Inſtruments, or of a couple to be plac'd in the ſame Receiver, would, when the Air (which is the uſual *medium* of Sounds) was well pump'd out, yet maintain ſuch a Sympathy (as tis call'd,) that upon the motion of the one, the other would alſo be made to ſtir: Which Tryals may be varied, by imploying for the external Inſtrument another in ſtead of a ſtringed one.

And becauſe Contraries (as is vulgarly noted) ſerve to illuſtrate each other, I thought to ſubjoyn, to the Tryals above related, about the propagation of Sounds in a *thinner medium* than the Air,

Air, ſome obſervations about the conveyance of them through that *thicker medium*, Water; but having unluckily mislaid my Notes upon that Subject, I cannot at preſent acquaint Your Lordſhip with what I intended, but muſt defer the doing it, till I ſhall have recovered Them.

EXPERIMENT XLII.

About the breaking of a Glaſs-drop in an Exhauſted Receiver.

YOu know, that among the Cauſes that have been propos'd of the ſtrange flying of a Glaſs-drop into a multitude of pieces, when the ſlender Stem of it comes to be broken off, One of the leaſt improbable was taken from the Preſſure of the Air: as if that within the poreous (and as 'twere honey-comb'd) inſide of the Glaſs, being highly rarified when the drop of melted Glaſs fell into the Water at its firſt formation, it was forc'd to continue in that præternatural ſtate of Expanſion by the hardneſs and cloſeneſs of the external Caſe of Glaſs, that inclos'd the Pithlike part (if I may ſo call it;) ſo that upon the breaking off a part of this ſolid Caſe at the Stem, the external Air gaining acceſs, and finding in the Spungy part very litle reſiſtance from the highly rarified and conſequently weaken'd Air included there, ruſhes in with ſuch violence, as to ſhiver the Glaſs-drop into a multitude of pieces.

I ſhall not now trouble Your Lordſhip with the mention of what may be alleadg'd to queſtion this *Hypotheſis*, eſpecially if it be compared with that accurate Account of the *Phænomena* of ſuch Glaſs-drops, which was ſometime ſince preſented to the Society by that great Ornament of it, S^r^ *Robert Moray*. But I ſhall onely ſay in this place, that when I conſider'd, that if the Diſſilition of the Glaſs would ſucceed when the Air was pump'd out of it, it, it would be hard to ascribe that Effect to the irruption of the external Air, I thought fit to try what would happen, if a Glass-drop were broken in our exhausted Receiver. And accordingly did, though not without some difficulty, so order the matter, that the blunter part of the Glass-drop was fastned to a stable Body (convey'd into the Receiver,) and the crooked Stem was tyed to one end of a string, whose other end was fastned to the Turning-key; by which means, when the Air had been diligently pump'd out, the Stem was (by shortning the string) broken off, and the Glass-drop was shatter'd into a thousand pieces.

This Experiment was long after repeated with the like success, and having at that time no Gage to try how far the Air had been drawn out, we let the external Air impell up the Water out of the Pump into the Receiver, and thereby found, that That vessel had not been negligently exhausted.

EXPERIMENT XLIII.

About the production of Light in the exhausted Receiver.

I Presume, I need not put Your Lordship in mind, that divers attempts were made to try, whether either a Flame, or kindled Coals would be made to continue for sometime burning in our Receiver: But those Tryals making it evident, that it would be either impossible, or very difficult to produce any durable Light, without the presence of the Air, by the burning of bodies; I thought it not amiss, considering the Nobleness of Light, to make trial, whether it might be otherwise produc'd in our exhausted Receiver; since whether or no the Attempts should prove successful, the Event would probably be instructive. For as tis the property of Light, when tis produc'd, to be discoverable by it self; so in such a Tryal as we intended, it would teach something concerning Light, to find that the absence of the Air would or would not hinder it from being produc'd. In prosecution of this Design, knowing that hard Sugar, being nimbly scrap'd with a knife, will afford a sparkling Light, so that now & then one would think that sparks of Fire fly from it; we caus'd a good lump of hard Loaf-sugar to be conveniently and firmly placed in the cavity of our capp'd Receiver, and to the vertical Cylinder formerly mentioned we caus'd to be fastned some pieces of a Steel-spring, which being not very thick, might in their passage along the Sugar, grate, or rub forceably against it, and then the Receiver being diligently exhausted in the Night-time, and in a dark Room, the vertical Cylinder (whose lower *Axis* was inserted into the often mentioned Trencher) was made for a pretty while to move round by the help of the Turning-key, manag'd by a hand steady and strong enough. By which means the Irons that came out of the vertical Cylinder, making in their passage vigorous impressions upon the Sugar that stood somewhat in their way, there were manifestly produc'd a good number of litle *flashes*, and sometimes too, though not frequently, there seem'd to be struck off litle sparks of Fire.

The Contrivance here mentioned may be conceiv'd, by considering the Figure belonging to the 41. Experiment.

EXPERIMENT XLIV.

About the production of a kind of Halo, *and Colours in the Exhausted Receiver.*

WE took a large inverted Cucurbite for a Receiver, which being so well wip'd both within and without as to be very clear, allow'd me to observe, and to make others do so too, That when the Pump began to be set a work, if I caus'd a pretty large Candle to be held on the other side of the Glass, upon the turning of the Stop-cock to let the Air out of the Receiver into the Cylinder, the Glass would seem to be full of Fumes, and there would appear about the Flame of the Candle, seen through them,

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a kind of *Halo*, that at firſt commonly was between Blew and Green, and after ſome Sucks would be of a Reddiſh or Orange colour, and both very vivid. The production of this *Meteor* (if I may ſo call it) was, according to my conjecture, made on ſome ſuch ſcore as this. That the Cement being ſomewhat ſoft and new (as is convenient for this Experiment) abounds with Turpentine, and having a litle (as well to faſten on the Receiver, as for the other purpoſe) apply'd to it a hot Iron, whereby the Cement was both ſoftned and heated, it ſeem'd rational to expect, That upon the withdrawing of the Air in the Receiver, the Aerial Particles in the Cement, freed from their former Preſſure, would extricate themſelves, and with the looſer ſteams of the Turpentine and perhaps of the Bees-wax would with a kind of Exploſion expand themſelves in the Receiver, and by their interpoſition between the Light and the Eye exhibit thoſe delightful Colours we had ſeen. To confirme which, I afterwards found, that by watchfully obſerving it I could plainly enough perceive the colouring ſteams, juſt upon the turning of the Stopcock, to fly up from the Cement towards the top of the Glaſs; and if we continued Pumping, the Receiver would grow clearer, and the Colours more dilute, (till we had occaſion to put on the Receiver, and heat the Cement afreſh:) of which the reaſon might be, partly that the Aerial and Volatile Particles of the upper part of the Cement did in that tract of time ſpend themſelves more and more; and partly, becauſe the Agitation they receiv'd from the heat communicated by the Iron did continually decay: Not to mention, that when the Receiver is more exhauſted, the want of Air makes it more difficult for Steams to be ſupported, and as it were ſwim up and down in it.

But for farther Confirmation, I caus'd ſome Cement to be put into a ſmall Crucible, warm enough to melt it; and conveying this into a clear Receiver of a convenient ſhape and ſize, I caus'd the Pump to be ſet a work; whereupon it appear'd manifeſtly enough, That upon the opening of the Stop-cock to let out the

Air, the Steams would copiously be thrown about from the Crucible into the capacity of the Receiver, and would, after having a litle play'd there, fall down again. But in these apparitions the Vividness, and sometimes the Kind of the exhibited Colours seem'd much to depend upon divers circumstances, such as the degrees of Heat, the bigness and shape of the Receiver, the quantity of Air that yet remain'd unpump'd out, and the nature of the Cement its self; which last particular *I* the rather mention, because, though I were hinder'd from doing it, I had thoughts to try a suspicion I had, that by varying the Materials expos'd to this kind of operation, some pretty variety might be made in the *phænomena* of the Experiment.

Whether or no the Apparition of Whiteness, or Light, that we sometimes hapned to take notice of divers years agoe, and have mentioned in the already * publish'd part of our Physico-mechanical Experiments, may be *partly* (though not entirely) referr'd to some of the Cements I then imploy'd, differing from those I now use most, and to the unheeded temper of those Cements, as to Warmth, and degrees of Softness, is a Doubt that further Observation may possibly enable us to determine.

* *pag.* 156. &c.

EXPERIMENT XLV.

About the production of Heat by Attrition in the Exhausted Receiver.

THe opinion that ascribes the Incalescence of solid Bodies, struck or rubb'd hard against one another to the attrition or vehement agitation of the intercepted Air, is famous and received enough to seem worthy of a particular Examination. But I confess to Your Lordship, that twas not any thing relating to this Opinion that chiefly induc'd me to make the Experiment I am now about to give an account of; for I thought it might be useful full to more purposes than one, to be able to produce by Attrition a somewhat durable Heat even in our exhausted Receiver: and therefore though 'twere easie to foresee, that it would prove no easie task, yet we thought fit to attempt it in spight of the difficulties met with at our first Tryal. In what way and with what success we afterwards made this attempt, I now proceed to relate.

Cross the stable Trencher, formerly often mentioned, there was fastned a pretty strong Spring of Steel or Iron, shap'd almost like the Lathe of a Cross-bow, and to the midst of this Spring was strongly fastned on the outside a round piece of Brass hollow'd almost like a concave Burning-glass, or one of those Tools wherein they use to grind Eye-Glasses for Telescopes. To this piece of Brass, which was not considerably thick, nor above 2 inches Diameter, was fitted a convex piece of the same Metal, almost like a Gage for a Tool to grind Glasses in, which had belonging to it a square Handle, whereinto as into a Socket was inserted a square piece of Wood, proceeding from the Basis of a square wooden Pillar, which we made use of on this occasion in stead of our vertical Cylinder. By the help of another piece of Wood coming from the other Basis of the same Pillar, the Turning-key was joyned to this Pillar, which was made of such a length, that when the Turning-key was forceably kept down as low as the Brass Cover, it was a part of, would permit; the convex piece of Metal lately describ'd did depress the concave piece a pretty way, notwithstanding a vigorous resistance of the subjacent Spring.

See Plate the Fig. the

Besides these things, a litle fine powder of Emery was put between the convex and concave pieces of Brass, to make them more congruous, and facilitate the motion that was to be made; and there was fastned to the upper part of the Turning key a good Wimble, without which we presum'd the turning of the Key would not produce a sufficient motion: in order to the making of which, it was, after the first Tryal, judged requisite to have a strong man, that was us'd to exercise his hands and armes in Me-

chanical

chanical labours, upon which account we ſent for a certain Lock-ſmith, that was a luſty and dexterous fellow.

All things that were thought neceſſary being thus in readineſs, and a Mercurial Gage being convey'd into the Receiver, we caus'd the Air to be diligently pump'd out; and then the Smith was order'd to turn the Wimble, and to continue to lean a litle on it, that he might be ſure to keep the Turning-key from being at all lifted up by the formerly mentioned Spring. Whilſt this man with much nimbleneſs and ſtrength was moving the Wimble, I watch'd the Gage, to obſerve whether the agitation of the Stop-cock, and conſequently the Engine, did not prejudice the Experiment; and for greater caution I caus'd the Pump to be almoſt all the while kept at work, though that ſeem'd not ſo neceſſary.

When the Turner of the Wimble was almoſt out of Breath, we let in for haſt the Air at the Cover of the Receiver by lifting up the Turning-key, and nimbly removing the Receiver we felt the pieces of Braſs, betwixt whom the Attrition had been made, and, as we expected, found both of them very ſenſibly warm.

But being willing to confirm the Experiment by a ſecond Tryal, which we hoped might, after the Experience taught us by the firſt, be ſomewhat better performed, we caus'd the Smith, after he had well refreſh'd himſelf with reſt and drink, to lay hold of the Wimble again, when the Gage made it appear that the Receiver was well exhauſted, ſo that by further Pumping the Quick-ſilver ſeem'd not to be further depreſt. And in this 2^d Tryal the nimble Smith plaid his part ſo well, (the Pump in the mean while not being neglected,) that when we did as before haſtily let in the Air, and take out the Bodies that had been rubb'd againſt one another, they were both of them (eſpecially the uppermoſt) ſo hot, that I could not endure to hold my hand on either of them, and they did for a conſiderable time retain a not inconſiderable degree of Warmth.

The ſame day I caus'd to be made at the Turners two bodies of of Wood, for ſize and ſhape like thoſe of Braſs we had juſt before imploy'd; the upper of theſe was of hard Oak, the other of Beech, (ſuch a difference between Woods, to be heated by mutual Attrition, being thought to be an advantageous circumſtance;) but though the Wimble was ſwiftly turn'd as before, and that by the ſame Perſon, nevertheleſs the Wood ſeem'd not to me (for all the By-ſtanders were not of my opinion) to have manifeſtly acquired any Warmth; and yet that there had been a conſiderable Attrition, appear'd by the great Poliſh which part of the Wood had evidently acquir'd, vvhich made me ſuſpect, that though the Wood ſeem'd dry enough, yet it might not really be ſo, notwithſtanding the contrary was affirm'd to me: but not being willing to ſit down with a ſingle Tryal, I caus'd the Experiment to be repeated with more obſtinacy than before; the effect of which was, that the Wood, eſpecially the upper piece of it, vvas brought to a Warmth unqueſtionably ſenſible.

EXPERIMENT XLVI.

About the ſlaking of Quick-Lime in the Exhauſted Receiver.

THe ſeveral Scopes I aim'd at in making the following Tryal are not neceſſary to be here particularly taken notice of. But one of them may be gueſs'd at by the ſubſequence of this Experiment to that immediately foregoing, and the *phænomena* of it may be mentioned in this Epiſtle upon the account of their being exhibited by our Engine.

We took in an Evaporating Glaſs a convenient quantity of Water, and having convey'd it into a Receiver, and well drawn out the Air, we let down into it by the Turning-key a lump of ſtrong Lime, about the bigneſs of a Pipin; and obſerv'd not that at the firſt immerſion, nor for ſome while after, there appear'd any ny considerable number of Bubbles, but within about ¼ of an hour, as I guess'd it, the Lime began (the Pump having been and being still ply'd from time to time) to slack with much violence, and with bubbles wonderfully great, that appear'd at each new Exuction, so that the inside of the Receiver (though pretty large) was at length lin'd with Lime-water, and a great part of the mixture did from time to time overflow the vessel, that had purposely been but little fill'd; nor did any thing but our weariness put a period to the bubling of the mixture, whose heat was sensible even on the outside of the Receiver, and which continued considerably hot in the Evaporating Glass for ¼ of an hour (as I conjectured) after the Receiver was removed.

Note, That the Lime imployed about this Experiment was of a very good and strong kind (made of hard stones,) and not such Lime, made of Chalk, as is commonly used at *London*, which probably would not have been strong enough to have afforded us the same *phænomenon*.

EXPERIMENT XLVII.

About an attempt made to measure the force of the Spring of included Air, and examine a Conjecture about the difference of its strength in unequally broad mouth'd Vessels.

THough several of the foregoing Tryals have sufficiently manifested that the Spring of the Air in its natural or wonted state, hath a force very considerable, and indeed much Greater than men seem to have hitherto believed; Yet I could not hope by any of these Experiments to determine by any known weight, how Great that force is, so as to conclude that it is equivalent to such a Weight, as so many Pounds, Ounces, &c. and to no more. Wherefore among the Uses I had design'd to make of our Syringe, formerly often mentioned, it was One, to try if by the help

of

of that Inſtrument, we could determine ſomewhat near (for no more was to be expected) how much Weight a Cylinder of uncompreſt Air included in it, and conſequently of the ſame Diameter vvith the cavity of the Barrel, would be able to ſuſtain or alſo to lift up.

In order to this Tryal, 1. we provided a ſtable Pedeſtal, or Frame, wherein the Syringe might be kept firm, and erected. Next, vve alſo provided a Weight of Lead ſhap'd like our Braſs-hoop, or Ring,* formerly deſcrib'd, that by the advantage of its figure it might be made to hang down by ſtrings from the top of the Handle of the Rammer, and ſo preſs evenly enough on all ſides, without making the upper part of the inſtrument top-heavy. 3. We took care to leave, between the bottom of the Syringe (which was firmly clos'd with ſtrong Cement) and that part of it where the Sucker was, a convenient quantity of Air, to expand its ſelf, and lift up the Weight, when the Air external to that included Air ſhould be pump'd out of the Receiver: And laſtly, the Handle of the Rammer (from which the Annular weight lately ſpoken of depended) was ſo faſtned to the Turning-key of the Cover of the Receiver, that the Weight might not compreſs the Air included in the Syringe, but leave it in its natural ſtate or wonted Laxity, till the Air were withdrawn from the Receiver.

* *Expe.* the Vth.

But notwithſtanding all this, when we actually tryed the Experiment, That hapned which I feared. For though by this method the included Air would well enough lift up a Weight of 7 or 8 pound, yet when the Rammer came to be clogg'd with ſo conſiderable a Weight, as my ſcope in making the Experiment required, the Inſtrument prov'd not ſo ſtanch, but that it was eaſier for ſome particles of Air to force themſelves a paſſage, and get away between the Sucker and the inſide of the Barrel, than to heave up ſo great a Weight. And yet I have thought fit to relate the Experiment thus particularly, becauſe, if an exact Syringe can be procured, (which I fear will be very difficult, but do not think impoſſible, this ſeems to be one of the likelieſt and leaſt exceptionable wayes I know, of meaſuring the force of the Airs Spring.

But deſpairing to get ſuch a Syringe, as I deſir'd, in the place where I then was, I bethought my ſelf of another way, by which I hop'd to be able (though not to arrive at an exact knowledge of the full force of the Airs Spring, yet) at leaſt to approach nearer it than I have been able to do by the help of the Syringe. For this purpoſe conſidering with my ſelf, that if a convenient quantity of Air were included in a fine ſmall Bladder, the ſides of it would hinder the Air from getting away, and the limberneſs of them would permit the Air to accommodate it ſelf and the Bladder to the Figure of a Cylindrical veſſel, into which it might be put.

Wherefore with much adoe I procured to be made by a perſon exerciſed in Turning a couple of hollow Cylinders, whoſe ſides were of a ſufficient thickneſs, (that they might reſiſt the preſſure of the Air to be impriſoned in them,) and of ſuch differing breadths, that the firſt had but one inch in Diameter, and the 2d two; their depths being alſo unequal, that the one might receive a much larger Bladder than the other.

With the leſſer of theſe (which was very carefully Turned) I made a diligent Tryal; whoſe Circumſtances I cannot now acquaint Your Lordſhip with, the Paper, wherein they vvere amply recorded, having been vvith other Notes belonging to this Continuation unluckily loſt: but the moſt conſiderable things in the Event were, That twas very difficult to procure a Bladder ſmall and fine enough for that litle Cylinder; and that one, which at length we procured, would not continue ſtanch for many Tryals, but would after a vvhile part with a litle Air in the well exhauſted Receiver, when twas clog'd with the utmoſt Weight it could ſuſtain: but whilſt it continued ſtanch vve made one fair Tryal vvith it, from vvhence vve concluded, that a Cylinder of Air of but an inch in Diameter, and leſſe than two inches in length, was able

able to raiſe viſibly (though but a litle) a Weight of above ten Pounds, (I ſpeak of Averdupoiz vveights, vvhere a Pound contains 16 ounces.) The manner of making this Experiment, and the cautions us'd in judging of it, Your Lorſhip may learn by the recital of the ſubſequent Tryal; my Notes about which were not ſo unfortunate as thoſe that concern'd the former.

Into a hollow Cylinder of Wood of four inches in depth, and two in Diameter, furniſhed with a broad and ſolid bottom or Pedeſtal, to make it ſtand the firmer, was put a Lambs or Sheeps bladder very ſtrongly tyed at the Neck, on vvhich vvas put a Wooden Plug, markt with Ink where the Edg of the Cylinder vvas contiguous to it; this Plug being loaded with Weights, amounting to 35 pound, (the uppermoſt of vvhich Weights was faſtned to the Turning-key, to keep it upright, and to help to raiſe it at firſt,) the Receiver vvas exhauſted, till the Mark appeared very manifeſtly above the brim of the Cylinder; and then, though the ſtring were by turning the Key quite ſlackned, yet the mark on the Plug continued very viſible: and vvhen ſo much Air was let into the Receiver, as made the Weight depreſs the Plug quite beneath the Mark, upon the repumping out of the Air the Weight was without the help of any Turning-key lifted up, and by degrees all the mark on the Plug was raiſed about $\frac{3}{8}$ above the edge of the Cylinder.

See plate the Figure the

Wherefore we ſubſtituted for a 7 pound weight one that was eſtimated at 14, (for then we had not a Ballance ſtrong enough to weigh it with,) and uſing the ſame Bladder we repeated the Experiment, onely having a care to ſupport a litle the uppermoſt Weight by the Turning-key, till the Bladder had attained its expanſion; and then the Weight being gently let go, depreſs'd not the Plug ſo low, but that we could yet ſee the mark on it, (which yet was all we could do,) though that part of the Plug, where the mark vvas, vvere manifeſtly more depreſt than the other.

For the clearing up of ſome particulars relating to this Tryal, we will ſubjoyn the following Notes.

1. The Plug is to be so fitted to the Cavity of the Cylinder, as easily to slip up and down in it, without Grating against the sides of it, lest it needlesly increase the resistance of the Weight to be rais'd. And this Plug ought to be of a convenient length, as about an inch and ½ at least, that it may be the fitter ~~to help~~ to reduce the Bladder by compression into a somewhat Cylindrical shape, and yet that it may not be thrust in too deep by the incumbent Weight; and that the Weight might rest more firmly upon it, there was a broad and strong Ledge made at the top of it, by which it might lean on every side upon the brim of the hollow Cylinder.

2. Before the Instrument was conveyed into the Receiver, the Bladder (which ought to be of a just size, and not full blown, and of a fine and limber contexture) was put into the Cylinder, and by divers gradual (but not immoderate) compressions was reduc'd to conform its self, as much as might be, to the Cylindrical shape of the containing vessel. And then the Weight being put on, and taken off again, there was a mark (in the form of an horizontally plac'd Arch) made with Ink, where the edge of the brim of the hollow Cylinder did almost touch the Plug. This we thought necessary to do, to avoid a mistake; for we must not judg, that all the Weight, that might be rais'd by our Bladder, may pass for the Weight sought after by our Experiment; since the Air in the Bladder is by reason of the incumbent weight more comprest than twas before, and consequently its being able to heave up a Great weight will not infer, that our common Air is able in its natural state (as they call it) to exert so Great a strength; that Weight being onely to be lookt on as rais'd or sustain'd by the uncomprest Air, that is rais'd or sustain'd when the Plug is lifted up to the mark, since till then the Spring of the Air does but bring it back from its new state of adventitious compression to its natural or wonted Laxity.

3. When, after the operation was ended, we took the Bladder out of the vessel, it had obtain'd a form Cylindrical enough,

and though it could be but 2 inches in Diameter, yet it was so litle as to be but half an inch more long than broad.

4. The reason why I chose to have the two Cylinders made of the unequal Diameters above mentioned, was to examine, as far as by this way I could, a conjecture I had, that the force of the Spring of differing Cylinders of Air to lift up solid Weights, would, *at the very first raising of the Weights*, be in duplicate proportion to the Diameters of their Cylinders, (those Diameters being proportionable to the Areas of the plain Superficies, against which the Air does immediately press,) without very much considering the inequality that may be between the quantity of the several parcels of Air, whose pressures are compared. But tis to be remembred, that I said *at the very first raising of the weights*, because presently after That, the quantity of the parcels of Air may be very considerable: for, as I have shewn in another Treatise, two very unequal quantities of Air being made by their Expansion to possess two equal spaces, the lesser quantity of Air must be much more rarified in proportion than the greater; and consequently, (to bring this home to our present Argument) though both be lifted up $\frac{1}{4}$ or $\frac{1}{2}$ of an inch, the Spring of a very litle Air must be much more weakned than that of a very considerable Quantity, and so it cannot *continue* to lift up its weight, as the above mentioned proportion would (if it were not for this Advertisement) seem to require.

Taking then our conjecture in the sense now declared, the success of our Tryals is agreeable to it, inviting us to conclude, that the Air in the Bladder, which was but two inches in Diameter, was able by its Pressure to countervaile the weight of 42 pound, which is about four times the weight that we lately observ'd the Spring of a Cylinder of Air of one inch in Diameter to be able to lift up. For though, according to what we have formerly said, of a duplicate proportion, 42 pound seems to be somewhat more than ought to have been lifted up in the Cylinder of two inches bore, when that of one inch lifted up not much above 10 pound; yet

yet this disagrees not with the *Hypothesis*, if we consider that the substance of the Bladder straitens the cavity of the smaller Cylinder in a Greater proportion than that of the bigger.

5. Though we have thus (as far as the Instruments we were able to procure would assist us) measured the Pressure of included Air, yet I must not forbear to advertise Your Lordship, that considering what I formerly observ'd to You about the weight of an Atmospherical Pillar of an inch in Diameter, I cannot but think, that if a Cylinder, or other convenient instrument, exactly Tite, can be procured, the Spring of an Aerial Cylinder will appear to be Greater than we found it by the foregoing Tryals; in which I consider that, not to mention the resistance of the Bladder its self, the membraneous substance that lin'd the Cylinders (though twere very thin and fine) could not but somewhat straiten their Cavities, and consequently somewhat (though not much) lessen the Diameters of the included Aerial Cylinders.

6. To all these Notes I must adde this Advertisement, That it may be therefore the more difficult in such Tryals as ours to *ascertain* the force of the Airs Spring, because, that Air its self when tis included, being shut up with the Pressure of the Atmosphere upon it, tis probable, that since that Pressure (as we have shewn) is not at all times the same, the Spring of the included Air will accordingly be varied. And, if my memory fail me not, when the lately recited Experiments were made, our Barometer declared the Atmosphere to be somewhat light.

From what has been hitherto delivered, this may result; that tis likely, that the Spring of an Aerial Cylinder an inch broad, may be able to sustain, if not raise, a pretty deal more than ten pound Weight; and that the past Tryals, without determining that the Air can raise no more than in them it did, do, at least, prove that it can raise up as much Weight as we have related, since we actually found it to do so.

EXPE-

EXPERIMENT XLVIII.

About an easie way of making a small quantity of included Air raise in the exhausted Receiver 50 *or* 60 *pound, or a greater weight.*

I Would very willingly have further prosecuted the foregoing Tryals, to see how far the lately propos'd Conjecture or *Hypothesis* would hold; but was hindered by the want of Receivers tall and capacious enough to contain the Weights, that such an attempt required: but remembring that there were not any Experiments made in our Engine, that appear'd more strange to the Generality of Spectators, and serv'd more to give them a high opinion of the Airs Spring, than those wherein they saw solid Bodies actually lifted up by it, and remembring, that I had lying by me a Brass vessel, (which had been bespoken for another Experiment, for which the Workmen had not made it fit,) I thought it not amiss to imploy it about making a Tryal very easie, and yet fit to be shewn to Strangers, to convince them, that the Spring of the Air is a much more considerable thing than they imagined.

We took then a Brass vessel made like a Cylinder, and having one of his Orifices exactly covered with a flat Plate very firmly fastned to it, the other Orifice being wide open. The depth of this vessel was 4 inches, and the Diameter should have been precisely (but wanted about a quarter of an inch of) 4 inches. To this hollow Cylinder we fitted a wooden Plug, like one of those described in the foregoing Experiment, save that it was not quite so long, and that it was furnished with a Rimme or Lip, which was purposely made of a considerable breadth, that it might afford a stable Basis to the Weight that should lean upon it. And then taking a middle siz'd and limber Bladder, strongly tyed at the Neck, but not near full blown, we press'd it by the help of

the

the Plug into the Cylinder to make it the better accommodate it self to the figure of it. Then taking notice by an inky mark how much of the Plug was extant above the orifice of the vessel, we laid the Weights upon the Plug, (whose Rimme or Lip hinder'd it from being deprest too deep into the cavity of the vessel;) and having convey'd them into the Receiver, we found as we expected, that if we had loaded the Plug but with a single Weight, (as to avoid trouble, and the danger of breaking the Glass we usually thought fit to do,) though that were a common half hundred weight, (which You know amounts to 56 pounds,) it would very quickly be manifestly heav'd up by the Spring of the included Air. For confirmation of more than which, I shall subjoyn the ensuing Tryal, as I find it recorded among my loose Notes.

See Plate the Fig. the

The Weight that was lifted up by the Bladder in the Cylinder 4 inches broad, was $7\frac{1}{2}$ pound; this Weight was lifted up till the wooden Plug *disclos'd* the Mark, that was to shew the height, at which the Air kept the said Plug before it was comprest: *disclos'd* it I say *visibly* at the 5th Exuction, and at the 7th that mark was $\frac{1}{8}$, or rather $\frac{3}{16}$ above the Edge of the Cylinder. In the Gage where the Mercury in the open Air was wont to stand about $\frac{1}{8}$ above the uppermost Glass-mark, it was deprest till it was $\frac{1}{8}$ below the second mark. When the Air was let in, it was a pretty while before the Weight did manifestly begin to subside; the Bladder being taken out, and the place it had possess'd in the Cylinder being supply'd with a Sleeve, or some such thing, and the Weight laid again upon the Plug, we found that at 24 Exuctions the Mercury was deprest to the lowest Mark of the Gage; and it was the 34 or 35th Exuction before the Receiver appear'd to be so exhausted, as to put an end to the sinking of the Mercury, which was then above $\frac{1}{8}$ beneath the lowest mark.

Your Lordship will easily believe, that most of the Spectators of such Tryals thought it somewhat strange to see a small quantity of Air, which was not onely uncomprest in the Bladder, but did

did not near fill it, (and left it very ſoft and yielding to the leaſt touch,) lift up ſo eaſily by its bare Spring ſuch Great Weights as indeavoured to oppreſs it. But this not being any thing near a ſufficient Tryal, how far the conjecture or *Hypotheſis* formerly propos'd will hold, I thought fit to make the utmoſt Tryals the talleſt Receivers I could procure would admit: and having caus'd leaden weights to be purpoſely caſt flat like Cheeſes, and as broad as we could conveniently put into the Receiver, that by the advantage of this ſhape we might be able to pile up the more of them, without much danger that any of them ſhould be ſhaken down; we laid divers of them one upon another, and then the upper part of the Receiver growing too narrow to admit more of them, we added a leſs broad Weight or two; and then exhauſting the Receiver, till we perceiv'd by the Gage that the Air was manifeſtly withdrawn, we found (as near as we could meaſure) by the help of a Mark and a pair of Compaſſes, that the Plug was ſo far rais'd, as that twas concluded, that the Elevation vvould have been much Greater, if the included Air, being put upon ſo Great a *Conatus*, had not found it eaſier to produce ſome Leak at the neck of the Bladder, than to lift up ſo Great a Weight, which by our Reckoning came to about 100 pound of 16 ounces to the pound. But this laſt Experiment, for want of ſome requiſite accommodations, vve vvere hinder'd from repeating and promoting; though the above mentioned *Hypotheſis* made me preſume, that a far Greater weight might this way have been rais'd if the Bladder had been ſtanch, and the Receiver high enough.

I need not tell Your Lordſhip, that if a larger Bladder be imploy'd and included in a Braſs veſſel of a ſufficiently wide Orifice, a far Greater weight may be lifted up by the Spring of the internal Air. But yet it will not be amiſs to give Your Lordſhip on this occaſion this Advertiſement, which may be fit to be taken notice of on divers others: That care muſt be had not to make Receivers, that ought to be well emptied, too large, and eſpecially too wide at the Orifice; for otherwayes they will be expos'd to ſo great a Preſſure of the Atmoſphere, that they need be of an extraordinary ſtrength to reſiſt it; and even Receivers, that ſeem'd thick enough proportionably to their bulk, and which held out very well till the cloſe of the Operation, yet when they came to be very diligently exhauſted, they did, by reaſon of the wideneſs of their Orifices, begin to crack at the bottom.

EXPERIMENT XLIX.

* *viz. the XXXVI.* IN one of my publiſh'd Experiments * I long ſince told Your Lordſhip, that when I endeavoured, by the help of a ſeal'd bubble, weigh'd in an exhauſted Receiver, to compare the Gravity of Air and Water, I was hinder'd by the caſual breaking of the Glaſs from compleating the Experiment. Wherefore I afterwards thought fit to repeat the Tryal; and though when I had done ſo twice or thrice, having given away the large Receiver I had made uſe of about them, and not being able ever ſince to procure a Good one, that was capacious enough for the tender Scales I thought ſo nice an Experiment required, I did not proſecute that Attempt ſo far as I intended; yet this very difficulty I met with to procure the Requiſites of making the Tryal, invites me to ſubjoyn the two following Notes, which I find among my looſe Papers.

April the 29. 1662. We weigh'd a Bubble in the Receiver, which we found to weigh above half a Grain heavier, when much of the Air was exhauſted, than when it was full. Afterwards we took out this ſeal'd Bubble, and weighing it found it to weigh 68 Grains and a half; then breaking off the ſmall tip of it under water, we found that the heat, by which it was ſeal'd up, had rarifi'd its included Air, ſo that it admitted 125 Grains of Water, for the admitted Water and Glaſs weighed $193\frac{1}{2}$ Grains. Then filling it full with Water, we found it to contain in all 739 Grains of Water, for it weighed $807\frac{1}{2}$ Grains: whence tis evident, that the difference between the

the weight of Water and Air was leſs than 1228 to 1.]

We weighed in the Receiver a Bubble, the Glaſs of which weighed 60 Grains: the Air that fill'd it weighed *in vacuo* $\frac{27}{32}$ of a Grain: the Water that fill'd it weighed 720$\frac{1}{4}$ Grains: So that by this Experiment the proportion of the weight of Air to Water is as (one) to (853$\frac{17}{27}$.) *May. 26. 1662.*

The Tryals mentioned in theſe Notes, though they were too few for me to acquieſce in, yet being made in a nevv vvay, and which has ſome advantages above thoſe that have been hitherto imployed to weigh the Air, may yet ſerve to keep us from the contrary Extremes, that have not been avoided by ſuch eminent Mathematicians as *Galileo* and *Ricciolus*; the former of which makes Water to be but about 400 times as heavy as the Air; and the later, whoſe conjecture is much remoter from the Truth, 10000 times heavier.

But it is ſo deſireable a thing, and may prove of ſuch importance, to know the proportion in Weight betwixt Air and Water, that I ſhall not ſcruple to acquaint Your Lordſhip with an attempt or two that I made to diſcover it by another way: For, though at firſt ſight this Experiment may ſeem to be the ſame with one publiſh'd a pretty while ago in the learned *Schottus* his *Mechanica Hydraulico-pneumatica*; yet Your Lordſhip will eaſily perceive this difference between them: That, whereas the induſtrious Author of that Experiment contents himſelf to ſhew, by the diminution of the weight of a Glaſs, when the Air has been drawn out of it, that the Air, before twas drawn out, was not devoid of Gravity; the following Tryal does not onely perform the ſame thing, and by a ſuperadded circumſtance confirm the Truth to be thereby prov'd, but it indeavours alſo to ſhew the Proportion in Gravity betwixt the Air and Water. The Tryals themſelves were regiſtred among my *Adverſaria* as follows.

A ſmall Receiver being exhauſted of Air by the Engine, and counterpois'd whilſt it continued ſo, the Stop-cock was turn'd, and the Air readmitted, which made it weigh 36 Grains more than it did before: and to prevent Jealousies, we caus'd it to be applied the second time to the Engine, by which the Air being emptied once more, the Glass was put into the other Scale of the former Ballance, and so counterpois'd; and then the External Air being readmitted, (which rush'd in as formerly with a whistling noise), there was found 36 Grains or better, requisite to restore the Ballance to an *Æquilibrium*.

We took a small Glass Receiver fitted with a Stopcock, and having exhausted it of the Air, and counterpois'd it, and let in the outward Air, we found the vveight of the Vessel to be increased by that admission 36 Grains. This done, we took the Receiver, after having well counterpois'd it, out of the Scale; and having apply'd it the second time to the Engine, we once more withdrew the Air, and then turning the Stop-cock to keep out the external Air, vve took care that none of the Cement, imploy'd to joyn it to the Engine, should stick to it, as we had diligently freed it from adherent Cement before we last apply'd it to the Engine. Then weighing it again, we found it to weigh either 35 or 36 Grains (but rather the former) heavier than it did, when twas last counterpois'd in the same Ballance. This being also done, we immers'd the Stop-cock into a Bason of fair Water, and let in the Liquor, that we might find how much Water would succeed in place of the Air vve had drawn out. When no more vvater vvas impell'd in, vve turned the Stop-cock once more, to keep it from falling out, and then weighing it in the same Scales, (after we had wip'd the Stop-cock, that no Water might stick to it on the outside,) we found the water (without computing the vessel) to weigh 47 ounces, 3 drachms, and 6 Grains, vvhich divided by 35 Grains, (which I took to be the weight of the Air, that vvas equal in Bulk to this vvater that succeeded it,) the Quotient was (wanting a very litle) 650 Grains, for the proportion of the vveight between Air and Water of the same bigness, *at the time when the Experiment was made:* vvhich circumstance I therefore take notice of, because the Atmosphere appear'd appear'd by the Baroſcope (wherein the Mercury ſtood then at 29 inches and ¾) to be very heavy; which made me the leſs wonder to find this proportion not ſo Great, as at other times I had obſerved it to be between Water and Air in point of weight: though I ſuſpected, that becauſe this odd Experiment cannot be nimbly diſpatched, ſome litle Air may have got in at the Stop-cock; beſides the Air that diſclos'd it ſelf in numerous bubbles in the vvater that vvas admitted, vvhere though it lay in ſuch ſmall particles as not to be diſcerned before; yet theſe particles, by this opportunity to expand themſelves, extricated themſelves from the vvater, and by getting together might *ſomewhat* reſiſt the Ingreſs of more; vvhich is a difficulty, vvhere to the meaſuring the proportion between VVater and Air in a heated Eoliple is liable. But the Stealing in of any Air, before the vvater vvas let in, is mentioned but as a Suſpicion.

Your Lordſhip may perhaps think it ſomewhat ſtrange, that I ſhould preſent You Tryals, whoſe Events do not ſo vvell agree together, as perchance You expected. But this very Diſagreement vvas one of the motives that induc'd me to acquaint You vvith them: for all thoſe compris'd in theſe Experiments being made faithfully, and not without (at the leaſt) an ordinary diligence, as they ſeem to make it probable, that one may without any Great errour eſtimate the proportion of our *Engliſh* Air to VVater to be as (One) to ſome number betwixt 600 and 1100; ſo tis not to be expected, that the Proportion, vvhatever it be that ſhould be pitch'd upon, ſhould be accurate and ſtable. For though Learned men ſeem to have hitherto taken it for granted, that it may ſuffice once for all diligently to inveſtigate the proportion betwixt thoſe two Bodies, yet, not onely I am apt to believe that a Determinate quantity of Air (as a Pint or Quart) may be unequally heavy in diſtant Countreys, and even in differing places of the ſame Countrey; but what I have taken notice of in the 17th of the printed Experiments, and afterwards frequently obſerv'd of the Great inequalities of the vveight of the Atmoſphere

ſphere, inclines me to think, that in the ſelf ſame place two Experiments may be made with the ſame Inſtruments, and equal diligence, and yet the weights of the Air may be found differing enough; which may keep Your Lordſhip from much wondering, that in the 36th printed Experiment, made when I had the variations of the Atmoſpheres Gravity in my Eye, I found the Air to be leſs ponderous in reference to Water, than in theſe later Tryals. But of this I hope I ſhall, if God permit, make further Tryals with the ſame veſſels, at times when I ſhall perceive by the Baroſcope, that the Gravity of the Atmoſphere is very Great and very Small. And I wiſh the Curious would make the like Tryals in other Regions. I do not forget, that not onely the School-philoſophers, but moſt of the Moderns deny, that Air hath any weight in Air, no more than Water in Water; but having [a] elſewhere declared and explained my ſenſe about this received Opinion, I ſhall not here ſpend any of the litle time I have remaining, to juſtifie my Diſſent; for which Your Lordſhip may find ſufficient Grounds in the newly related Experiments, eſpecially if You pleaſe to conſider, that though the Opinion I diſallow have been chiefly and generally grounded upon ſome Arguments ſuppoſed to evince, that vvater has no vveight in vvater, I have [b] elſewhere ſhewn thoſe Proofs not to be cogent, and taught a Practical way of weighing vvater in vvater with a pair of ordinary Scales. [c]

a In the Hydroſtatical Paradoxes.

b In an Appendix to thoſe Paradoxes.

c This method was omitted in the Engliſh Edition of the newly mentioned Appendix, but not in the Latin Verſion.

EXPERIMENT L.

About the disjoyning of two Marbles (not otherwiſe to be pull'd aſunder without a great weight) by withdrawing the preſſure of the Air from them.

In our formerly *publiſh'd* Experiments about the Air*, I did, if I miſremember not, acquaint Your Lordſhip with an Attempt

* *Experiment the XXXI. See alſo the cauſe of this Phænomenon diſcours'd of in the Authors Hiſtory of* Fluidity *and* Firmneſs.

I had made to make a couple of coherent Marbles fall asunder, by withdrawing the Air from them; but though I then esteem'd that their Cohæsion depended upon the Presure of the Air, yet not being at that time furnish'd with all the accommodations requisite to make an Experiment not easie to be perform'd succeed, I thought fit, when I had afterwards opportunity, to prosecute what I then began, and add some circumstances that I could not then make Tryal of; and yet whose success will not I presume be unwelcome, since it supplies us with no less than matters of fact; whence we may argue, that this Experiment of coherent Marbles (which not onely the *Aristotelian* Plenists have of late much triumph'd in, but which some recent Favourers of our *Hypothesis* have declar'd themselves to be troubled with) is not onely reconcileable to our Doctrine, but capable of being made a confirmation of it ; notwithstanding what has lately been publish'd (upon the supposition of a case, which at first Blush may seem somewhat of kin to our Experiment,) by a very learned * Writer, to whose objection against our *Hypothesis*, though as well confidently as very civilly proposed, an Answer may in due place, if your Lordship desire it, be return'd.

* Dr. H. M. *in the 2d. chap. of the 2d. Book of the new Edition (in folio) of his* Antidote against Atheism.

We took two flat round Marbles, each of them of two inches and about 3 quarters in Diameter, and having put a litle Oyl between them to keep out the Air, we hung at a Hook fastned to the Lowermost a Pound weight to surmount the Cohæsion, which the tenacity of the Oyl and the imperfect Exhaustion of the Receiver might give them. Then having suspended them in the cavity of a Receiver, at a stick that lay (Horizontally) a cross it; when the Engine was fill'd, and ready to work, we shook it so strongly, that those that were wont to manage it, concluded, it would not be near so much shaken by the Operation. Then beginning to pump out the Air, we observ'd the Marbles to continue joyned till it was so far drawn out, that we began to be diffident whether they would separate. But at the 16th Suck, *upon the turning of the Stop-cock*, (which gave the Air a passage out of

the

the Receiver into the Pump,) the ſhaking of the Engine being almoſt, if not quite, over, the Marbles ſpontaneouſly fell aſunder, wanting that Preſſure of the Air, that formerly had kept them together: which Event was the more conſiderable, not onely becauſe they hung parallel to the Horizon, but adher'd ſo firmly together when they were put in, that having try'd to pull them aſunder, and thereby obſerv'd how cloſe they ſtuck together, I foretold it would coſt a good deal of pains ſo far to withdraw the Air, as to make them ſeparate: which Conjecture Your Lordſhip will the leſs wonder at, if I adde, that a weight of 80 and odd pounds, faſtned to the lowermoſt Marble, may be drawn up together with the uppermoſt, by vertue of the firmneſs of their Coheſion.

NB. This is not the onely time that this Experiment ſucceeded with us. For ſometimes, when they were not ſo cloſely preſs'd together before they were put in, the Disjunction was made at the 8[th] Suck, or ſooner, and we ſeem'd to our ſelves to obſerve, that when we hung but half a pound weight to the lower Marble, it requir'd a Greater exhauſtion of the Receiver to ſeparate them, than when we hung the whole Pound.

After, having proceeded thus far with the Inſtruments we then had, meeting with an Artificer that was not altogether unskilful, we directed him to make (what we wanted before in that place) ſuch a Braſs-plate to ſerve for a Cover or Cap to the upper orifice of Receivers open at the top, as we have divers times had occaſion to mention already in giving accounts of ſome of the foregoing Tryals: by the help of which Contrivance we proſecuted the newly related Experiment much further than we could do before, as may appear by the following account.

We faſten'd to the lowermoſt of the two Marbles a weight of a very few ounces, (for I remember not the preciſe number,) and having cemented the capp'd Receiver with the Marbles in it, as before, to the Pump, we did by a ſtring, whereof one end was tied to the bottom of this Turning-key, and the other to the uppermoſt

moſt Marble, and which (ſtring) paſt through the Crank or Hook belonging to the Braſs-Cover; we did, I ſay, by the help of this ſtring, and by turning round the Key, draw up the ſuperiour Marble, and by reaſon of their coherence the lowermoſt alſo, together with the weight that hung at it: by which means being ſure, that the two Marbles ſtuck cloſe together, we began to pump out the Air that kept them coherent; and after a while, the Air being pretty well withdrawn, the Marbles fell aſunder. But we having ſo order'd the matter, that the lowermoſt could fall but a litle way beneath the other, we were able by inclining and ſhaking the Engine to place them one upon another again, and then letting in the Air ſomewhat haſtily, that by its Spring it might preſs them hard together, we found the Expedient to ſucceed ſo well, that we were not onely able by turning the abovementioned Cylindrical Key, to make the uppermoſt Marble take up the other, and the annexed weight; but we were fain to make a much more laborious and diligent Exhauſtion of the Air to procure the disjunction of the Marbles this ſecond time, than was neceſſary to do it at the firſt.

And for further prevention of the Objections or Scruples that I foreſaw ſome Prepoſſeſſions might ſuggeſt, I thought fit to make this further Tryal, that when the Marbles were thus aſunder, and the Receiver exhauſted, we did, before we let in the Air, make the Marbles fall upon one another as before; but the litle and highly expanded Air that remained in the Receiver, having not a Spring near ſtrong enough to preſs them together, by turning the Key we very eaſily rais'd the uppermoſt Marble alone, without finding it to ſtick to the other as before. Whereupon we once more joyn'd the Marbles together, and then letting in the external Air, we found them afterwards to ſtick ſo cloſe, that I could not without inconvenience ſtrain any further, than I fruitleſly did, to pull them fairly aſunder; and therefore gave them to one that was ſtronger than I, to try, whether he could do it, which He alſo in vain attempted to perform.

And now, my Lord, though I had thoughts of adding divers other Experiments to those I have hitherto entertained You with; yet (upon a review) finding These to amount already to fifty, I think it not amiss to make a Pause at so convenient a Number. And the rather, because an odd Quartainary Distemper, that I slighted so long, as to give it time to take Root, is now grown so troublesome, that I fear it may have too much influence upon my Style; which Apprehension obliges me as well to avoid abusing, or distressing Your Lordship's Patience, as to allow my self some seasonable Refreshment, to reserve the mention of the design'd Additions till they can with less trouble to us both be presented You by

My Dear Lord

Your Lordship's most humble Servant, and Affectionate Uncle,

Oxford, March the 24.
1667.

ROBERT BOYLE.

NOTES &c.

About the ATMOSPHERES of *Consistent Bodies* (here below.)

SHEWING,

That even HARD and SOLID BODIES (and ſome ſuch as one would ſcarce ſuſpect) are capable of emitting EFFLUVIA, and ſo of having ATMOSPHERES.

An Advertisement.

He that shall take the pains to peruse the following Paper, will easily believe me, when I tell him, that twas not design'd to come abroad with the Experiments, in whose company it now appears. But the Stationer earnestly representing that divers Experiments being reserved by me for another occasion, the remaining ones alone would not give the Book a Thickness any thing proportionable to its Breadth; I consented, at his sollicitation, to annexe to them the following Observations, because of some affinity between the small Atmospheres of lesser Bodies, and the great Atmosphere that surrounds the Terrestrial Globe; in which the other, that do at least help to compose it, are lost and confounded, as Brooks and Rivers are in the Ocean. And to save the Reader the pains of making Guesses to what kind of Writing the ensuing Discourse may belong, I shall here intimate, that tis dismembred from certain Papers about Occult Qualities in general, which make part of the Notes I long since designed, and also partly published, about the Origine of Qualities, *of which Notes those that concern'd* Effluviums, *being the most copious, I referr'd them to four general Heads; whereof the first onely is treated of in the following Discourse, the others being withheld, as having not affinity enough with the Atmosphere to accompany This, whereon they have no such absolute Dependance, but that they may well enough spare it. And I make the less Scruple to let it appear without them, because the Inducements already mentioned are not a litle strengthned by this superadded Consideration, That the following Notes may give light to several of the Observations I have*

made

made of some lesse heeded Phænomena of the Alterations of the Air , in case they be allowed to enter into the Appendix *to this* Continuation.

OF

Of the Atmospheres of Consistent Bodies:

THe School Philosophers, and the Vulgar, in considering the more abstruse Operations and *Phænomena* of Nature, are wont to run into Extremes; which, though opposite to one another, do almost equally contribute to keep men ignorant of the true causes of those Effects they admire. For the Vulgar, being accustomed to converse with sensible objects, and to conceive grosly of things, cannot easily imagine any other Agents in Nature, then those that they can see, if not also touch, and handle; and as soon as they meet with an Effect, that they cannot ascribe to some palpable, or at least sensible Efficient, they are, and stick not to confess themselves utterly at a loss. And though the vulgar of Philosophers will not acknowledg themselves to be pos'd by the same *phænomena* with the vulgar of Men, yet in effect they are so. But the School-philosophers on the contrary, do not onely refuse to acquiesce in sensible Agents, but to solve the more Mysterious *Phænomena* of Nature, nay and most of the Familiar ones too, they scruple not to run too far to the other side, and have their recourse to Agents that are not onely invisible, but inconceivable, at least to men that cannot admit any save Rational and consistent Notions: they ascribe all abstruse Effects to certain substantial Forms, which however they call Material, because of their dependence on Matter, they give such Descriptions to, as belong but to Spiritual Beings: as if all the abstruser Effects of Nature, if they be not perform'd by visible Bodies, must be so by immaterial substances: whereas betwixt visible bodies and Spiritual Beings there is a middle sort of Agents, invisible Corpuscles; by which a Great part of the difficulter *phænomena* of Nature are produc'd, and by which may intelligibly be explicated

explicated those *Phænomena*, which 'twere absurd to refer to the former, and precarious to attribute to the latter. Now for methods sake I will refer the Notes, that occur to me about Effluviums, to four Heads; whereof the first is mentioned in the Title of this Paper, and each of the other three shall be successively treated of in as many distinct ones.

That Fluid Bodies, as Liquors, and such as are manifestly either moist, or soft, should easily send forth Emanations, will I presume be granted without much difficulty; especially considering the sensible Evaporation that is obvious to be observ'd in Water, Wine, Urine, &c. and the loose contexture of parts that is suppos'd to be requisite to constitute soft Bodies, (as Flowers, Balsomes, and the like:) but that even Hard and ponderous Bodies, notwithstanding the Solidity and strict cohesion of their component parts, should likewise emit Steams, will to many appear improbable enough to need to be solemnly prov'd.

Whether you admit the Atomical *Hypothesis*, or prefer the Cartesian, I think it may be probably deduc'd from either, that very many of the Bodies we are treating of, may be suppos'd exhaleable as to their very minute parts. For according to the Doctrine of *Lucippus*, *Democritus*, and *Epicurus*, each indivisible particle of Matter hath essentially either a constant actual motion, or an unlooseable endeavour after it; so that though it may be so complicated in some Concretions, with other minute parts, as to have its Avolation hindred for a while; yet it can scarce otherwise be, but by this incessant Indeavour of all the Atomes to get loose, some of them should from time to time be able to extricate them selves, and fly away. And though the Cartesians do not allow Matter to have any innate motion, yet according to them both Vegetables, Animals, and Minerals, consist of litle parts so contexed, that their Pores give passage to a Celestial Matter; so that this Matter continually streaming through them, may well be presum'd to shake the Corpuscles that compose them: by which continued concussion now some Particles, and then others, will be be thrown and carried off into the Air, or other contiguous Body, fitted to receive them. But though by these, and perhaps other considerations, I might indeavour to shew *à priori*, as they speak, that tis probable Consistent Bodies themselves are exhaleable, yet I think it may be as satisfactory, and more useful, to prove it *à posteriori*, by particular Experiments, and other Examples.

That then a dry and consistent form does not necessarily infer, in the Bodies that are endowed with it, an indisposition to send forth Steams, which are as it were litle Colonies of Particles, is evident, not onely in the leaves of Damask Roses, whether fresh or dried; as also in Wormwood, Mint, Rue, &c: but in Ambergreece, Musk, Storax, Cinamon, Nutmegs, and other odoriferous and spicy bodies. But more eminent Examples to our present purpose may be afforded us by Camphire, and volatile Salts, such as are Chymically obtain'd from Harts-horn, Blood, &c. for these are so fugitive, that sometimes I have had a considerable Lump of volatile Salt (either of fermented Urine, or of Harts-horn) fly away by litle and litle out of a Glass, that had been carefully stopt with a Cork, without leaving so much as a Grain of Salt behind it. And as for Camphire, though by its being uneasie to be powder'd, it seems to have something of Toughness or Tenacity in it; yet I remember, that having for tryals sake counterpois'd it in nice Scales, even a small lump of it would in a few hours suffer a visible loss of its weight, by the avolation of strongly sented Corpuscles, and this, though the Experiment were made both in a North Window, and in Winter.

But I expect you should require Instances of the Effluviums of Bodies of a close or solid Texture; wherefore I proceed to take notice, that Amber, Hard wax, and many other Electrical bodies do, when they are rubb'd, emit Effluviums. For though I will not now meddle with the several Opinions about the cause and manner of Electrical Attraction, yet besides that almost all the Modern Naturalists, that aim at explicating things intelligibly, ascribe the Attraction we are speaking of to Corporeal effluxes;

 and

and besides that I shall ere long have occasion to shew you, that there is no need to admit with Cartesius, That because some Electrical bodies are very close and fixt, what they emit upon rubbing is not part of their own Substance, but somewhat that was harbour'd in their Pores: besides these things, I say, I have found that many Electrical bodies may by the very Nostrils be discovered, when they are well rubb'd, to part with store of Corpuscles, as I have particularly, but not without attention, been able to observe in Amber, Rosin, Brimstone, &c.

I know not whether it will be worth while to take notice of the great Evaporation I have observ'd, even in Winter, of Fruits, as Apples, and of Bodies that seem to be better cover'd, as Eggs, which notwitstanding the closeness of their Shels, did daily grow manifestly lighter and lighter; as I observ'd in them, and divers other bodies, that I kept long in Scales, and noted their Decrements of weight: but perhaps you will be pleas'd to hear, that having a mind to shew how considerable an Evaporation is made from Wood, I caus'd a thin Cup, capable of holding about a Pint, or more, to be Turn'd of a Wood, that was chosen by the Turner as solid and dry enough, though it were not of the closest sort of Woods, such as are *Lignum vitæ*, and Box. And as I caus'd the shape of a Cup to be given it, that it might have a greater Superficies expos'd to the Air, and consequently might be the fitter to emit store of Steams into it; so the Success did not onely answer my Expectation, but exceed it: for though the Tryal were made some time in Winter, there was so quick and plentiful an Evaporation made from the Cup, that I found it no easie matter to counterpoise it; for whilst Grains were putting into the opposite Scale, to bring the tender Ballance to an *Æquilibrium*, the copious avolation of invisible Steams from the Wood (which had so much of Superficies contiguous to the Air) would make the Scale that held it sensibly too light. And I remember, that for further satisfaction, being afterwards in a City where there were both good Materials and workmen, I order'd to be made a Boule, about the

the same bigness with the former, of well season'd wood, which being suspended in the Chamber I lay in, (which circumstance I therefore mention, because the Weather and a litle Physick I had taken obliged me to keep a fire there,) it quickly began manifestly to loose of its weight; and though the whole Cup wanted near two Drams of 2 Ounces, yet in 12 hours, *viz.* from 10 a clock in the morning to the same hour at night, it lost about 40 Grains, (for twas above *39*:) but of such Experiments, and the Cautions belonging to them, I may elswhere speak farther.

It were not difficult for me to multiply Instances of the continual Emanation of Steams from Vegetable and Animal Substances; but I am not willing to enlarge my self upon this Subject, because I consider that there are other Bodies which seem so much more indispos'd to part with Effluviums, that a few instances given in such, may evince what I would prove, much more then a multitude produc'd in other bodies. And since I consider that those Substances are the most unlikely to afford Effluvia, that are *either* very cold, *or* very ponderous, *or* very solid and hard, *or* very fixt; if I can shew you that neither of these Qualifications can keep a Body from emitting Steams, I hope I shall have made it probable, that there is no sort of Bodies here below that may not be thought capable of affording the Corporeal Emanations we speak of.

And first I remember, that I have not onely taken Eggs, and in a very sharp Winter found them, notwithstanding the coldness of the Air where I kept them, to grow sensibly lighter, in a faithful pair of Scales, in not very many hours: but because Ice is thought the coldest visible Body we know, I thought fit to shew that even this Body will loose by Evaporation; for having counterpois'd a convenient quantity of Ice in a good Ballance, and forthwith expos'd it therein to the cold Air of a Frosty night, that the Evaporations should be from Ice not from Water, I found the next morning, that though the Scale wherein the Ice were put was dry, which argued as well as the coldness of the Weather

 that

that the expos'd Concretion had not thaw'd; yet I found its weight to be considerably diminished, and this Experiment I succesfully made in more than one Winter, and in more than one place. And tis now but a few dayes since, exposing not long before midnight, lesse than two ounces of Ice in a good Ballance to a sharply freezing Air, I sent for it before I was up in the morning, and though by the dryness of the Scales the Ice that was in one of them appear'd not to have thaw'd, yet it had lost about ten Grains of its former weight; so that here, the Evaporation was made in spite of a double Cold, of the Ice, and of the Air.

I should now proceed to the mention of ponderous and solid Bodies, but before I do so, it may be expedient to give you notice, that, to make the Proof of what I have propos'd more satisfactory, and more applicable to our future purposes, I shall forbear to give you any Examples of the exhalations of Bodies, where so potent an Agent as the Fire is made to intervene.

But though I purposely forbear to insist on such Examples, yet it may not be amiss to intimate, that in explicating some occult Qualities, even such Exhalations as are produc'd by the help of the Fire may be fit to be taken into consideration, as we may hereafter have occasion to shew. And therefore we may observe in general, that the Fire is able to put the parts of Bodies into so vehement a motion, that except Gold, Glass, and a very few more, there are not any Bodies so fixt and solid, that tis not thought capable to dissipate either totally, or in part. Tis known to those that deal in the fusion of Metals, that not onely Lead and Tin, but much harder Bodies will emit copious and hurtful Steams. And there are some kinds of that Iron, which our Smiths call Cold-share iron, about whose smell whilst it was red hot, when I made inquiry, the ingeniousest Smith I had then met with told me, that he had found it several times to be so strong, and rank, that he could scarce indure to work with his Hammer those parcels of Metal whence it proceeded. And even without being brought to fusion, not onely Brass, and Copper will, being well

well heated, become strongly sented, but Iron will be so too, as is evident by the unpleasing smell of many Iron Stowes. And on this occasion I might not impertinently adde here a Tryal we made to observe, whether the Steams of Iron may not be made, though not immediately visible, yet perceptible to the Eye it self, though the Metal had not a Red, much less a White heat. But having elsewhere related it at large, in a Discourse You may command a sight of, I shall rather refer You to it, than loose the time 'twould take up to transcribe it.

These things premis'd, I proceed now to the mention of ponderous Bodies; and concerning them, to represent, that if You will admit what almost all the Corpuscularians assert, and divers of the Peripateticks do not now think fit to deny, that the Magnetical operations are perform'd by Particles issuing forth of the body of the Loadstone, or other Magnetical Agent: I shall not need to go far for an Instance to our present purpose, since I have Hydrostatically found, that some Loadstones (for I have found those Minerals very differing in Gravity) are so ponderous, as to exceed double the weight of Flints, or other Stones of the same bulk.

But not to insist on Loadstones, Stone-cutters will inform You, (as they did me,) that black Marble, and some other solid and heavy stones will, upon the attrition they are expos'd to, when the Workmen are polishing them, (especially without water,) emit, and that without the help of external heat, a very sensible smell, which I found to be much more strong and offensive when, to make it so, I had the curiosity to cause a piece of solid black Marble to have divers fragments struck off from it with a Chizel and a Hammer: for the stroaks succeeding one another fast enough to make a great concussion of the parts of the black Marble, (for in white, which is not so solid, the Tryal will not succeed well,) there quickly follow'd as I expected a rank and unpleasant smell; and you will grant me I know, that Odours are not diffus'd without corporeal Emanations. I remember also, that having procur'd some of those acuminated and almost Conical

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cal ſtones, that paſs among the vulgar for Thunder-ſtones, by rubbing them a litle one againſt the other, I could eaſily according to my expectation excite a ſtrong Sulphureous ſtink. I have alſo tried upon a certain Mineral Maſs, that was ponderous almoſt as a Metal, but to Me it ſeem'd rather an unuſual kind of Marchaſite, that I could in a trice without external heat make it emit more ſtrongly ſented Exhalations, than I could contentedly endure: to which I ſhall adde this Example more, that having once made a Chymical mixture of a Metalline body, and coagulated Mercury, which you will believe could not but be ponderous, though this Mixture had already endur'd as violent a fire as was neceſſary to bring it to Fuſion, in order to caſt it into Rings; yet it was ſo diſpos'd to part with corporeal Effluxes, that a very ingenious Perſon that practis'd Phyſick, and was there when I made it, earneſtly begg'd a litle of it of me for ſome Patients troubled with diſtempers in the Eyes, and other parts remote enough from the hand; which he affirm'd himſelf to have very happily cured, by making the Patient wear a Ring of this odde Mixture, or wearing a litle of it as an *Appenſum* near the diſaffected part. If you make a *vitrum Saturni* with a good quantity of *Minium* in reference to the Sand or Chryſtal, which it helps to bring to Fuſion, you ſhall have a Glaſs exceeding ponderous, and yet not devoid of Electricity: And I remember, that having ſometimes caus'd Braſs it ſelf to be Turn'd like Wood, that I might try, whether ſo Great (though inviſible) a Concuſſion of all the parts would not throw off ſome Steams that might be ſmell'd, I was not reduc'd to foregoe my Expectation; but yet becauſe it was not fully anſwer'd, and becauſe alſo there is great difference of Braſs upon the ſcore of the *Lapis Calaminaris*, whereof together with Copper tis made, I enquired of the Workman, who us'd to turn great quantities of Braſs, whether he did not often after find it more ſtrong; and he inform'd me that he did, the ſmell being ſometimes ſo ſtrong, as to be offenſive to Strangers, that came to his Shop, and were not uſ'd to it.

I proceed now to the Effluviums of solid and hard Bodies, of which, if most of our Corpuscularian Philosophers, and divers others be not much mistaken, I may be allow'd to give instances in all Electrical Bodies, which, as I have already noted, must according to their Doctrine be acknowledged to operate by substantial Emanations. Now among Electrical Bodies I have observ'd divers, that are of so close a Texture, that *Aqua fortis* its self, nor spirit of Salt will work upon them, and to be so hard, that some of them will strike fire like Flints: Of the former sort I have found divers Gems (which I nam'd in my Notes about Electricity,) and even the Cornelian it self, which I found to attract Hairs, though it be thought to be of a much slighter Texture than precious Stones, did yet resist *Aqua fortis*, as I tried in a large Ring, (brought out of the East-Indies,) which I purposely broke, and reduc'd some part of it to powder, that I might make these and some other Tryals with it. Rock Chrystal also, though it have a very manifest attractive virtue (as they call it,) I have yet found it so hard, as to strike fire rather better than worse than ordinary Flints. And to shew that no hardness of a Body is inconsistent with its being Electrical, I shall adde, that though Diamonds be confest to be the hardest Bodies that are yet known in the world, yet frequent Experience has assur'd me, that even These, whether raw or polish'd, are very manifestly (and sometimes vigorously enough) Electrical.

And to let you see, that I need not to have recourse to this kind of Bodies, to prove, that very solid ones are capable of *Effluvia*; I will, to what I have formerly noted about the Odour of black Marble, subjoin two or three Examples of the like nature.

The first shall be taken from a sort of Concretions very well known in divers parts of *Italy* by the name of *Cugoli*, because of the great use that is made of it by the Glass-men. These Concretions you will easily believe are very hard, as other Minerals of that sort are wont to be; and yet being invited by my Conje-

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ctures about the Atmospheres of Bodies, to try them by rubbing them one against the other, I found as I expected, that they afforded not onely a perceptible, but a very strong smell, (which was far from that of a Perfume.)

And this brings into my mind, that having met with some Stones cut out of Humane bladders, whose Texture was so close, that I could not with Corrosive Menstruums make any sensible Solution of one whereon I made my Tryal; though to facilitate the Liquors operation, part of it were reduc'd to fine Powder, yet by a litle rubbing of one of these so closely contexed Stones, it would presently afford a rank smell, very like the stink of stale Urine.

I remember I have caus'd Iron to be turn'd with a Lath, to examine whether by the internal commotion, that would by that operation be produc'd in the corpuscles of the Metal, even that solid as well as ponderous Bodie would not become capable of being smell'd; and though by reason of the nature of that parcel of iron whereon we made our Tryal, or some accidental disposition, which was at that time (being Winter) in my organs of Smelling, the Odour seem'd to me but very faint; yet upon the enquiry I made of the Artificers, whether in Turning greater pieces of iron they did not find the smell stronger? they told me, that they often found it very strong, and sometimes more so than they desired.

And this brings into my mind, what I have carefully observ'd in Grinding of iron; for there are many Grindstones so qualify'd, that in case iron instruments be held upon the Stone, whilst it is nimbly turn'd under it, though the water that is wont to be us'd on such occasions stifles (if I may so speak) the Smell, and keeps it from being commonly taken notice of; yet if you purposely cause (as I remember I have done) the use of Water to be forborn, your Success will not be like mine, if you do not find that store of fœtid Exhalations will be produc'd. And though it be not always so easie to discern by the smell, from which of the

two

two Bodies they issue, or whether they proceed from both; yet it seems probable enough, that some of the Steams come from the iron, and tis more than probable that if they proceed not from that Metal, they must from a Body that is so hard as to be able to make impressions in a trice upon Iron and Steel themselves.

The last Example I shall name under this head, is furnish'd me by Marchasites, some of which would after a short concussion without external heat be made to exhale for a pretty while together a strong Sulphureous odour, and yet were so hard, that when struck with a Steel-hammer, (which would not easily break them) they afforded us such a number of Sparks, as appear'd strange enough. And tis known, that tis from their disposition to strike fire, (which yet I dare not attribute to all sorts of Marchasites,) that this kind of Mineral is, by a name frequently to be met with in Writers, call'd *Pyrites*. And in this Example we may take notice, that a Body, capable of being the source of corporeal Emanations, may be at once both very solid and very ponderous.

It remains now that I manifest, that even the Fixedness of Bodies is not incompatible with their disposition to emit *Effluviums*.

I might alleadg on this occasion, that the *Regulus* of Antimony, and also its Glass, though they must have endur'd Fusion to attain their respective Forms; yet they will without heat communicate to Liquors Antimonial Expirations, with which those Liquors being impregnated become Emetick and Purgative. I might also adde, that divers Electrical Bodies are very fixt in the fire, and particularly that Chrystal, as we have more than once tried, will endure several Ignitions and Extinctions in water, without being truly Calcin'd, being indeed but crackt into a great multitude of litle parts; but because the above named Antimonial bodies will after a while fly away in a strong fire, and because the Effluviums of Chrystal are not so sensible as those which can immediately affect our Eyes or Nostrils, I will here subjoyn one instance, such as I hope will make it needless for me to adde any more, it being of a Body which must have sustain'd an exceeding vehement fire, and is look'd upon by most of the Chymists as more undestroyable then Gold it self, and that is Glass, which is able as you know to endure so great a brunt of the fire, that you did not perhaps imagine I should of all Bodies name it on this occasion. But my conjectures about the Atmospheres of Bodies leading me to think, that Glass it self might afford me a confirmation of them; I quickly found, that by rubbing a very litle while two solid pieces of it (not, as I remember, of the finer sort) one against the other, they would not onely yield a sensible Odour, but sometimes so strong an one, as to be offensive. By which you will easily perceive why I told you above, that I did not acquiesce in the Cartesian Argument against Electrical Bodies performing their operations by Emanations of their own substance, drawn from hence, that Glass does attract light Bodies, (as indeed it does, though but weakly,) and yet is too fixt to emit *Effluviums*, the contrary of which Supposition the lately mentioned Experiment (and by us often repeated) does sufficiently evince.

From what other solid Bodies, and that will endure the fire, I have, or have not been able to obtain such odorous Steams, it is not necessary to declare in this place, but may perhaps be done in another.

You may I presume have taken notice, that according to what I intimated a while agoe, I have forborn in the precedent Examples to mention those *Effluvia* of solid Bodies, that need the action of the Fire to be obtain'd. But since the Sun is the grand Agent of Nature in the Planetary world, and since during the Summer, and especially at Noon, and in Southern Climates, his Heat makes many bodies have litle Atmospheres, that we cannot so well discern that they have constantly; I see not why I may not be allow'd to ascribe Atmospheres to such Bodies, as I have observ'd to have them when the Sun shines upon them, and also to think that the like may be attributed at least sometimes to such other bodies,

Bodies, as will do the things usually perform'd by *Effluviums*, when yet they are excited but by an external heat, which exceeds not that of the hot Sun.

Of these two sorts of Bodies I shall for brevities sake name but two or three Examples, and then hasten to a Conclusion.

The first of these I must make bold to borrow from my Observations about Electricity, among which this is one, that to shew that the particular and usual manner of exciting such Bodies, namely by rubbing them, is not alwayes necessary; I took a large piece of good Amber, and having in a Summer morning, whilst the Air was yet fresh, tried that it would not without being excited attract a light Body I had expos'd to it; I remov'd it into the Suns beams, till they had made it moderately hot, and then I found according to my expectation that it had acquir'd an Attractive virtue, & that not onely in one particular place, as is usually observ'd when tis excited by rubbing, but in divers and distant places at once; at any of which it would draw to it the light body plac'd within a convenient distance from it: so that even in this Climate of ours a solid Body may quickly acquire an Atmosphere by the presence of the Sun, and that long before the warmest part of the day.

The next instance you will perchance think somewhat strange, it being that when for want of an opportunity to make the like Trial in the warm Sun, I took a litle but thick vessel made of Glass, and held it near the fire till it had got a convenient degree of heat, (which was not very great, though it exceeded that which I had given the Amber,) I found as I had imagin'd that the heat of fire had made even this Body attractive, as that of the Sun had made the other.

What degree of heat I have observ'd to be either necessary, or the most convenient to excite Electrical bodies according to their different natures, (for the same degree will not indifferently serve for them all,) this is not the properest place to declare, and it will be more to our present purpose to make some short reflection on what has been hitherto delivered.

It seems then probably deduceable from the foregoing Experiments and Observations, that a very great number if not the greatest part even of Consistent bodies, whether Animal, Vegetable, or Mineral, may emit Effluviums, and that even those that are solid may (at least sometimes) have their litle Atmospheres, though the neighbouring Solids will often keep the Evaporations from being every way ambient in reference to the Bodies they issue from.

For as the instances hitherto alleadg'd (which are not all that I could have nam'd) do plainly shew that divers Bodies (and some that have not been thought very likely) are such as we speak of, so several things induce me to believe, that there may be many more of the like nature.

For first, very few if any have (that I know of) had the curiosity to make use of nice Scales, (which such Tryals require,) to examine the Expirations of inanimate bodies, which if they shall hereafter do, I make litle doubt but they will light on many things, that will confirm what we have been proposing, by their finding that some Bodies, which are not yet known to yield Exhalations, do afford them, and that many others do part with far more copious ones than is imagin'd. For one would not easily have thought, that so extremely cold a Body as a solid piece of Ice should make a plentiful Evaporation of its self in the cold Air of a freezing night; or that a piece of Wood, that had long lain in the house, and was light enough to be conveniently hung for a long time at a Ballance, that would loose its *Æquilibrium* with (as I remember) half a quarter of a Grain, should in less than a minute of an hour, send forth steams enough to make the Scales manifestly turn, and that in Winter.

But supposing (which is my second Consideration) that Tryals were made with good instruments for weighing, though it will follow, that in case the exposed body grow lighter, something exhales from it; yet it will not follow, that if no diminution of weight be discover'd by the instrument, nothing that is corporeal recedes

recedes from it. I will not urge that tis affirm'd, not onely by the generality of our Chymists, but by learned modern Physitians, that when either Glass of Antimony, or *Crocus metallorum* impregnate Wine with Vomative and Purgative Particles, they do it without any decrement of their weight; because the Scales in Apothecaries Shops, and the litle accurateness wont to be imployed in weighing things, by those that are not vers'd in Statical affairs, make me (though not deny the Tradition which may perchance be true, yet) unwilling to build upon observations, which to be relyed on are to be very nicely made; and therefore I shall rather take notice, that though the Loadstone be concluded to have constantly about it a great multitude of Magnetical *Effluvia*, (which may be call'd its Atmosphere,) yet it has not been observed to loose any thing of its Weight by the recess of so many Corpuscles. But because if the Cartesian *Hypothesis* about Magnetisms be admitted, the Argument drawn from this instance will not be so strong as it seems, and as it otherwise would be: I shall add a more unexceptionable Example, for I know you will grant me that Odours are not diffus'd to a distance without Corporeal Emanations from the Odorous body: and yet, though good Amber Greece be, even without being excited by external heat, constantly surrounded by a large Atmosphere, you will in one of the following Discourses find cause to admire how inconsiderable the wast of it is.

If it be said, that in Tract of time a Decrement of weight may appear in Bodies, that in a few hours or dayes discovers not any; the Objection, if granted, overthrows not our Doctrine, it being sufficient to establish what we have been saying, if we have evinc'd that the *Effluvia* of some Bodies may be subtle enough not to make the Body by their avolation appear lighter in Statical Trials, that are not extraordinarily (and as it were obstinately) protracted. And this very Objection puts me in mind to adde, that for ought we know the Decrement of Bodies in Statical Experiments long continued, may be somewhat Greater than even nice

Scales

Scales discover to us; for how are we sure that the weights themselves, which are commonly made of Brass, (a Metal very unfixt,) may not in Tract of time suffer a litle Diminution of their Weight, as well as the Bodies counterpois'd by them: and no man has I think yet tryed whether Glass, and even Gold may not in tract of time loose of their Weight, which in case they should do, it would not be easily discover'd, unless we had Bodies that were perfectly fixt, by comparison to which we might be better assisted, than by comparing them with Brass weights, or the like, which being themselves less fixt, will lose more than Gold and Glass.

My third and last consideration is, that there may be divers other wayes, besides those furnish'd us by Staticks, of discovering the *Effluvia* of solid Bodies, and consequently of shewing, that tis not safe to conclude, that because their Operation is not constant or manifest, such Bodies do never emit any *Effluvia* at all, and so are uncapable to work by their intervention on any other Body, though never so well dispos'd to receive their Action. And this I the rather desire that you would take notice of, because my chief (though not onely) design in these Notes is (you know) to illustrate the Doctrine of *occult Qualities*; and it may conduce to explicate several of them, to know that some particular Bodies emit *Effluvia*, though perhaps they do it not constantly, and uniformly; and though perchance too, they do not appear to emit any at all, if they be examin'd after the same manner with other exhaleable Bodies, but onely may be made to emit them by some peculiar way of handling them, or appear to have emitted them by some determinate operation on some other single Body, or at most small number of Bodies.

Perchance you did not think, till you read what I lately told you about Glass, that from a Body that had endured so violent a fire, there could, by so sleight a way as rubbing a litle while one piece against another, be obtain'd such steams, as may not onely affect but offend the Nostrils. Nor should we easily believe, if Experience did not assure us of it, that a Diamond, that is justly reputed the hardest known Body in the World, should by a litle rubbing be made to part with Electrical *Effluvia*. Nay, (that I may give some kind of confirmation to that part of the last Paragraph that seems most to need it,) I shall adde, that I once had a Diamond not much bigger than a large Pea, which had never been polish'd or cut, whose Electrical virtue was sometimes so easily excited, that if I did but pass my fingers over it to wipe it, the virtue would disclose it self; and if as soon as I had taken it out of my Pocket, I applied a hair to it, though I touch'd not the Stone with my fingers, that I might be sure not to rub it, that Hair would be attracted at some distance, and many times one after an other, especially by one of the sides of the Stone, (whose surface was made up of several almost triangular Planes,) and though this excitation of the Diamond seemed to proceed onely from the warmth that it had acquir'd in my Pocket, yet I did not find that That warmth, though it seem'd not to be alter'd, had alwaies the same effect on it, though the wiping it with my finger fail'd not (that I remember) to excite it. Something like this uncertainty I always observ'd in another Diamond of mine, that was much nobler than the first, and very well polished, and in a small Ruby, that I have yet by me, which would sometimes be considerably Electrical without being rubb'd, when I but wore the Ring it belong'd to on my litle finger; and sometimes again it seem'd to have lost that virtue (of operating without being excited by friction,) and that sometimes within a few minutes, without my knowing whence so quick a change should proceed. But I must insist no longer on such particulars, of which I elsewhere say something; and therefore I proceed to take notice, that we should scarce have dream'd, that when a Partridg, or a hunted Deer has casually set a foot upon the ground, that part where the Footstep hath been (though invisibly) impress'd, should continue for many hours a Source of Corporeal Effluxes; if there were not setting Dogs, and Spaniels, and Bloud-hounds, whose noses can take notice at

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that distance of time of such Emanations, though not onely other sorts of Animals, but other sorts of Dogs are unable to do so.

I saw a stone in the hands of an Academick, an Acquaintance of mine, which I should by the Eye have judg'd to be an Agate, not a Blood stone, and consequently I should not have thought that it could have communicated Medicinal *Effluvia* appropriated to excessive Bleedings, if the Wearer of it had not been subject to that Disease, and had not often cur'd both himself and others, by wearing this stone about his neck; which if he left off, as sometimes he did for Trials sake, his exceedingly sanguine complexion (to which I have rarely seen a Match) would in a few daies cast him into Relapses. What I have elsewhere told you about the true virtues of some Stones, (for I fear that most of those that are wont to be ascrib'd to them are false,) may give some confirmation to what I have been delivering, which I cannot now stay to do, being to draw to a Conclusion as soon as I have put you in mind, that it would not probably have ever been expected that so ponderous and solid a Body as the Loadstone should be invironed by an Atmosphere, if Iron had been a scarce Mineral, and had not chanc'd to have been plac'd near it.

And with this instance I shall put an end to these Notes, because it allows me to make this Reflexion; that since solid Bodies may have constant Atmospheres about them, and yet not discover that they have so, but by their operation upon one particular Body, or those few which participate of That; and since there are already (as we have seen) very differing wayes whereby Bodies may appear to be exhaleable, it is not unlikely that there may be more and more Bodies (even of those that are solid and hard) found to emit *Effluvia*, as more and more wayes of discovering that they do so, shall either by chance or industry be brought to light.

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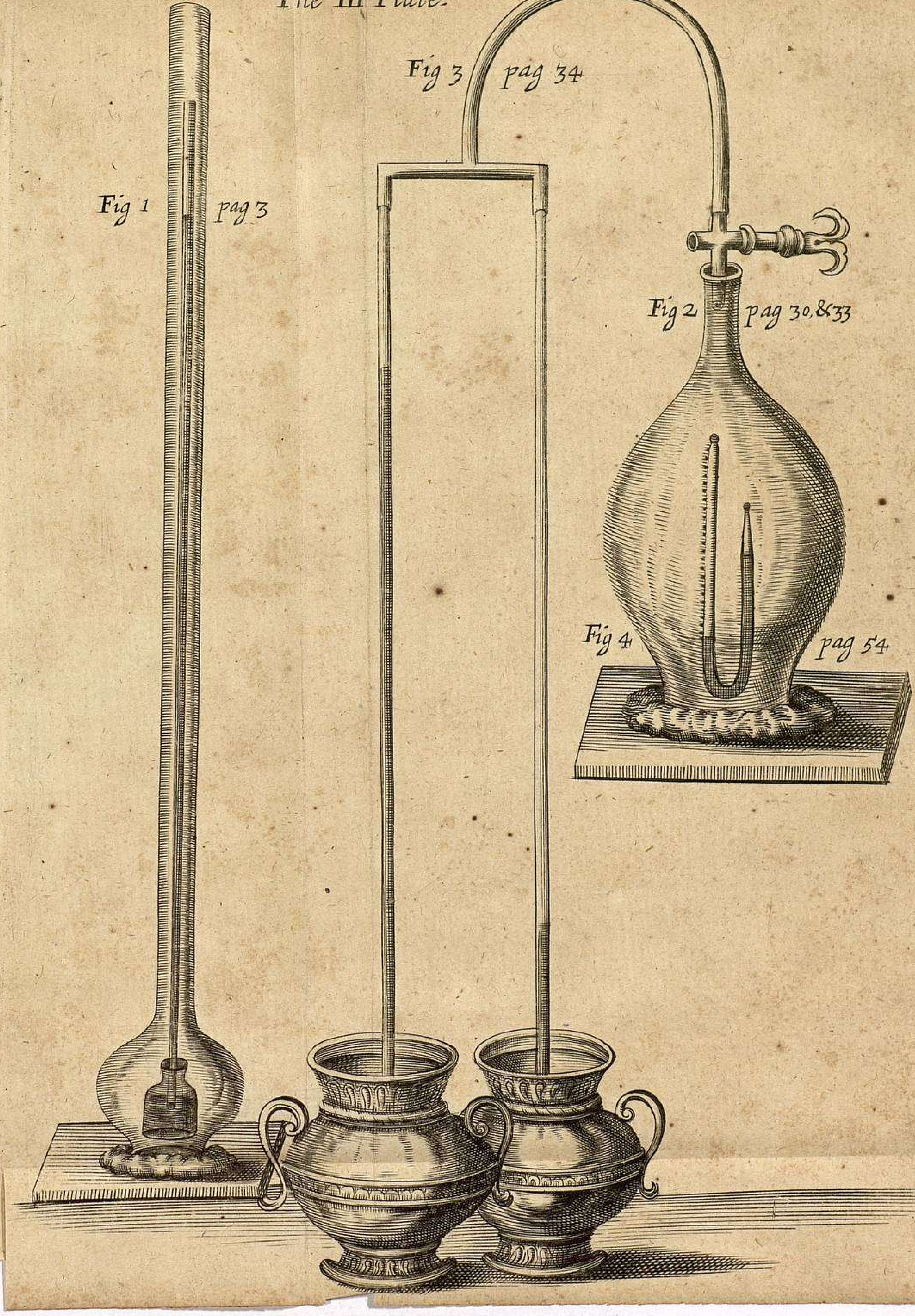

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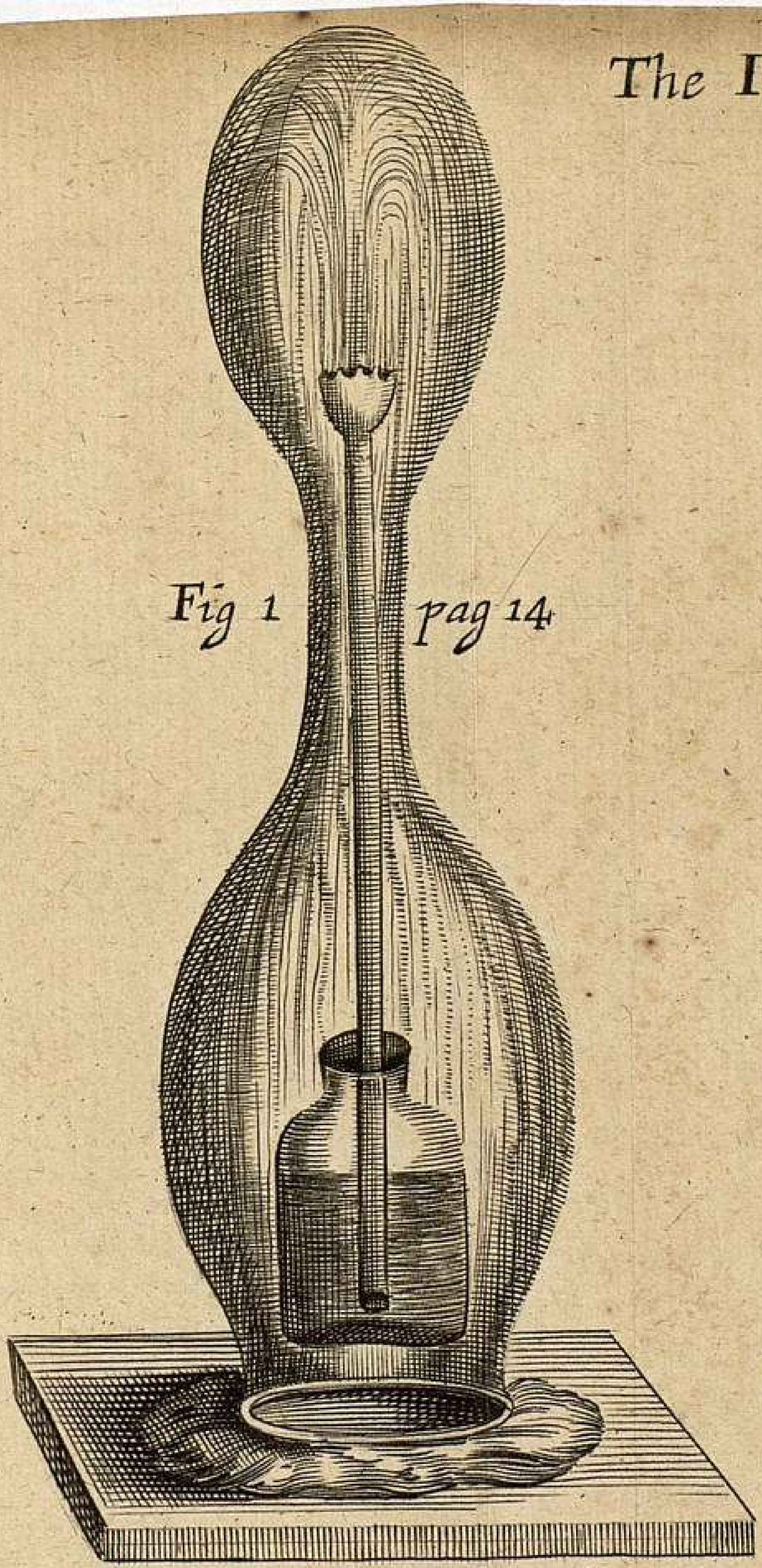

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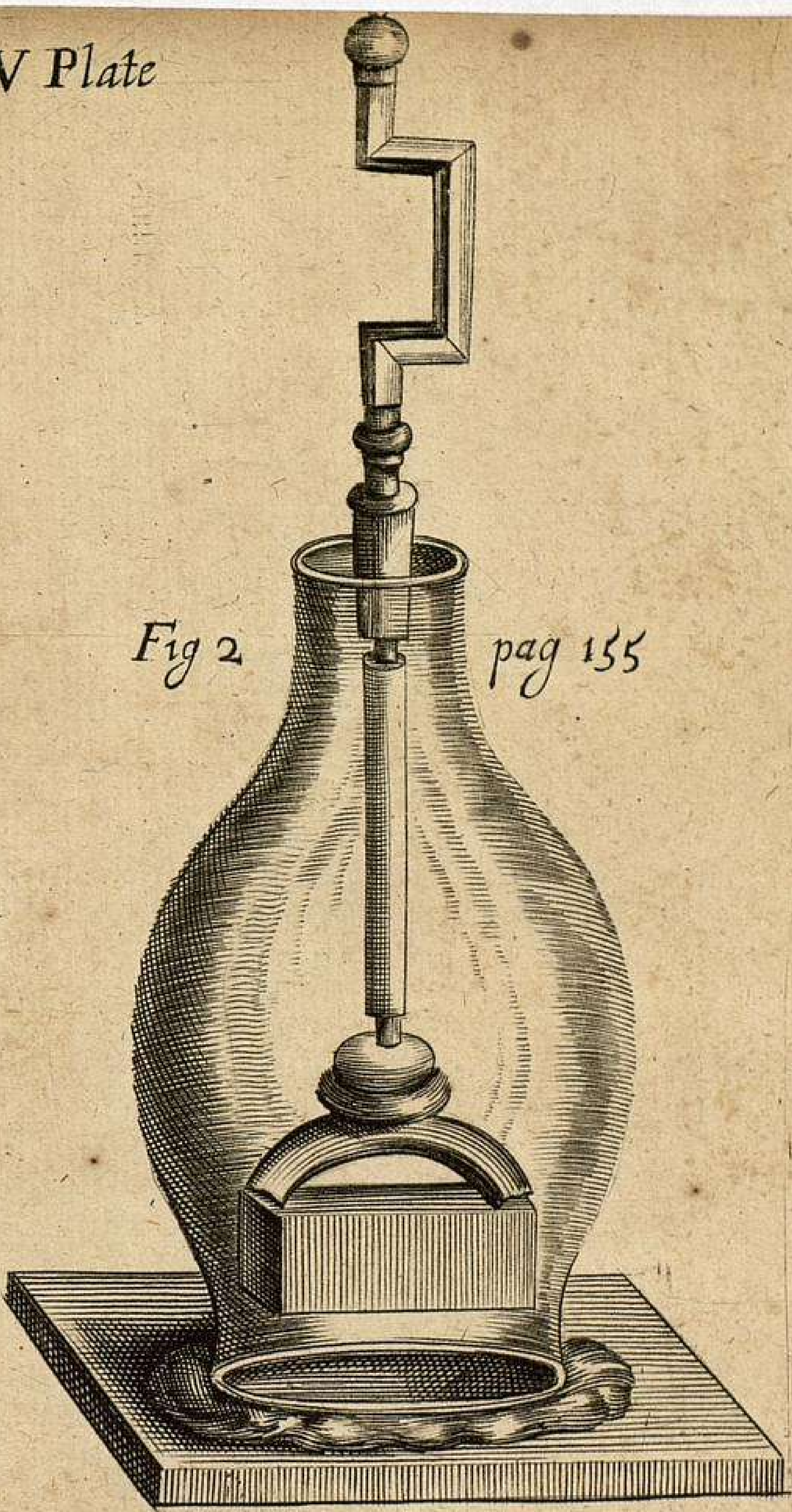

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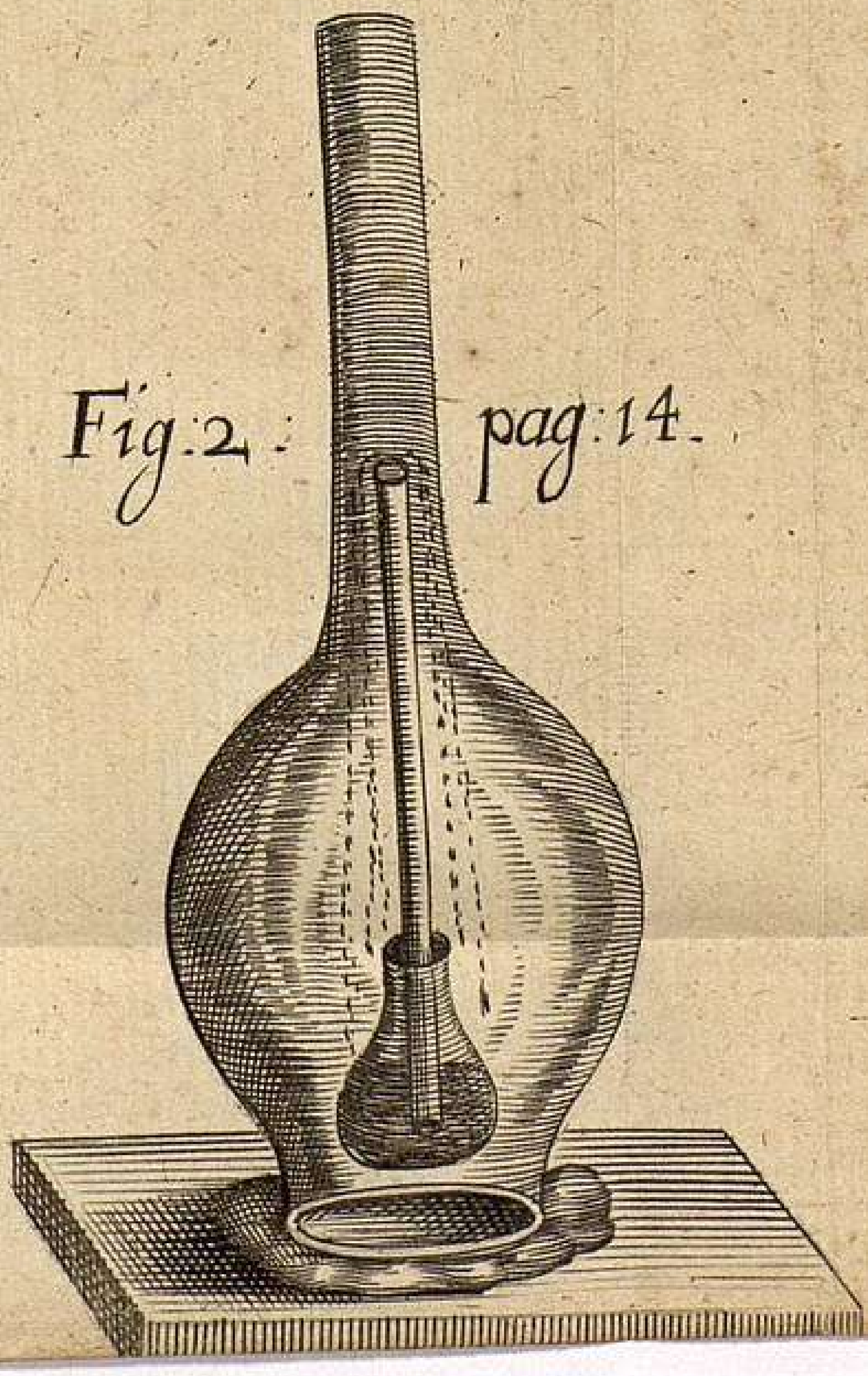

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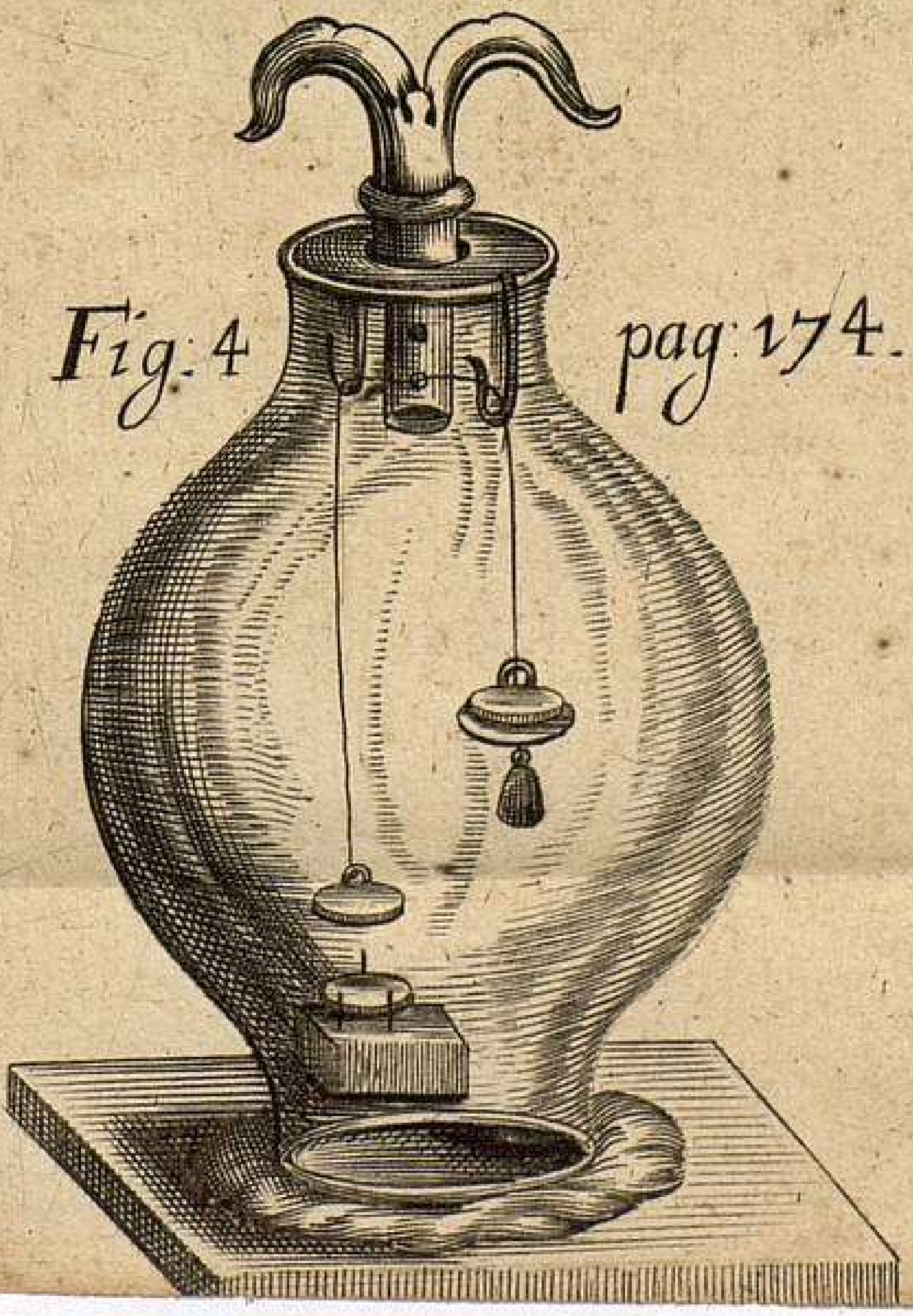

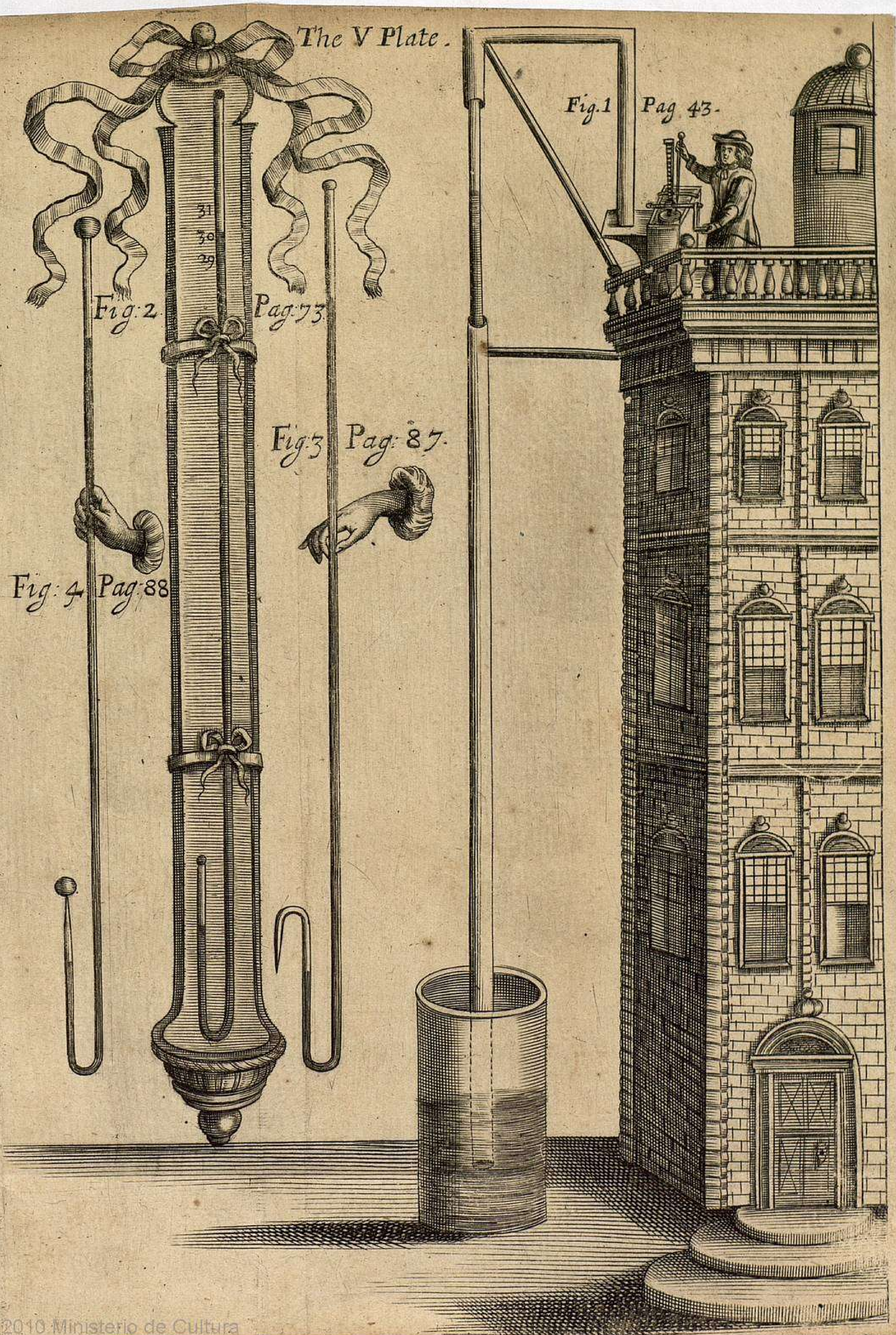
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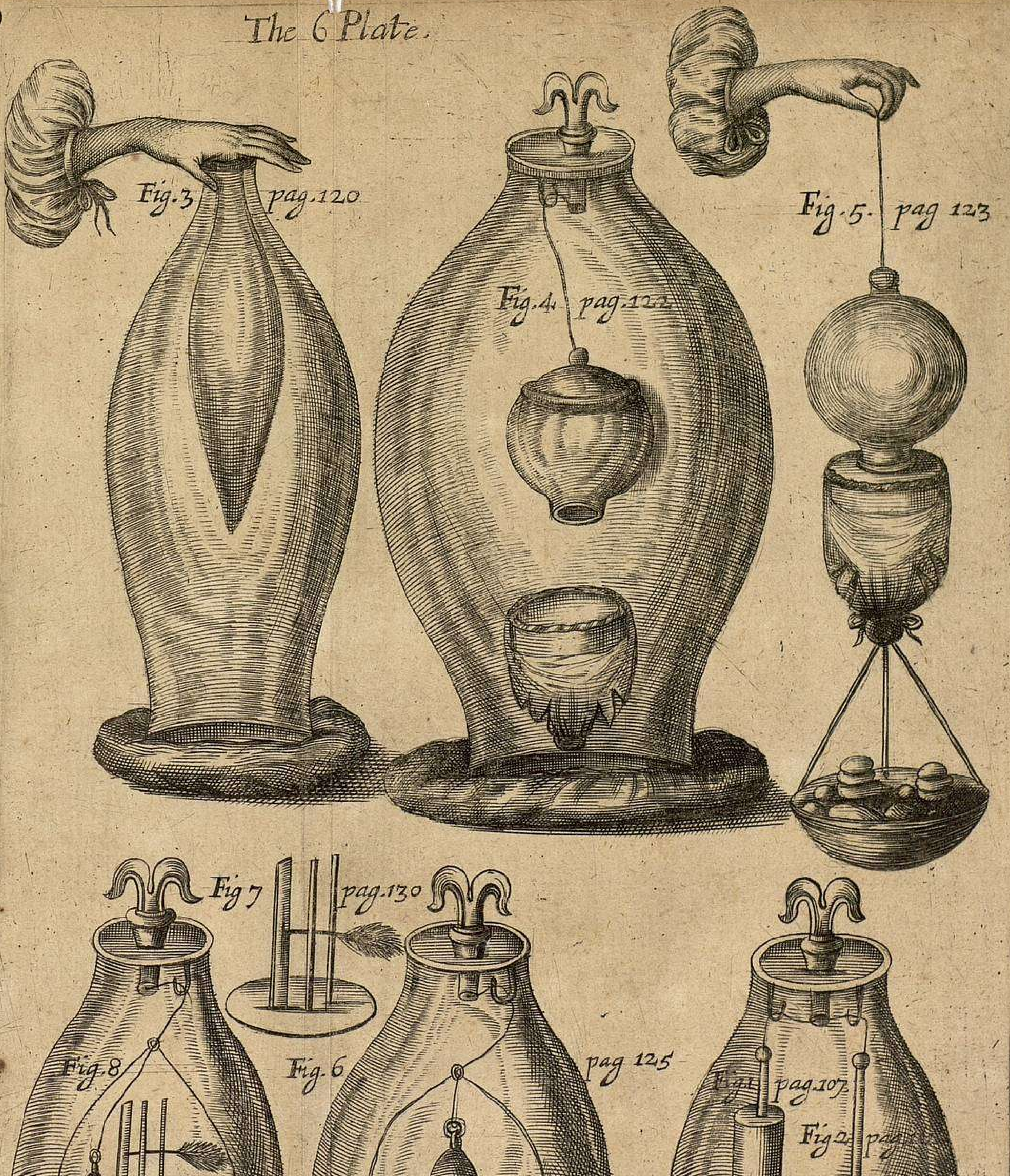
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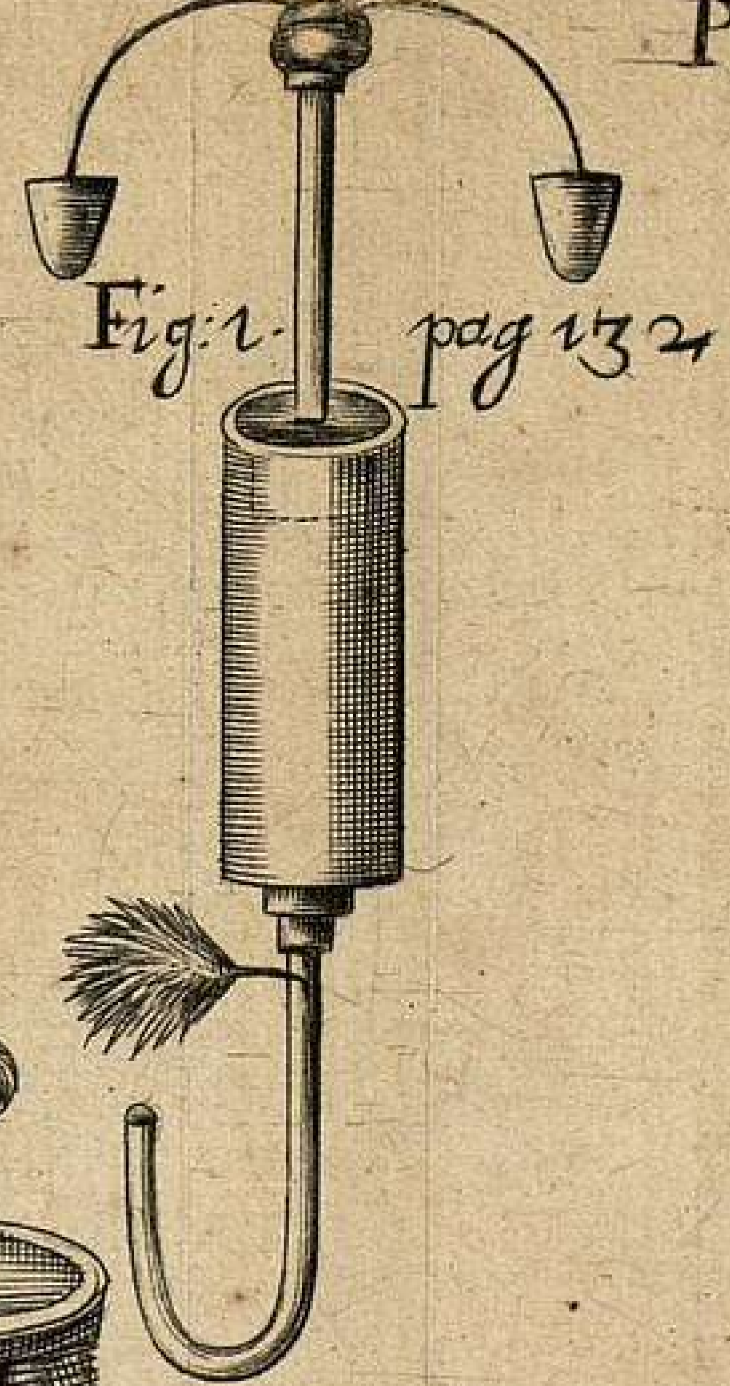

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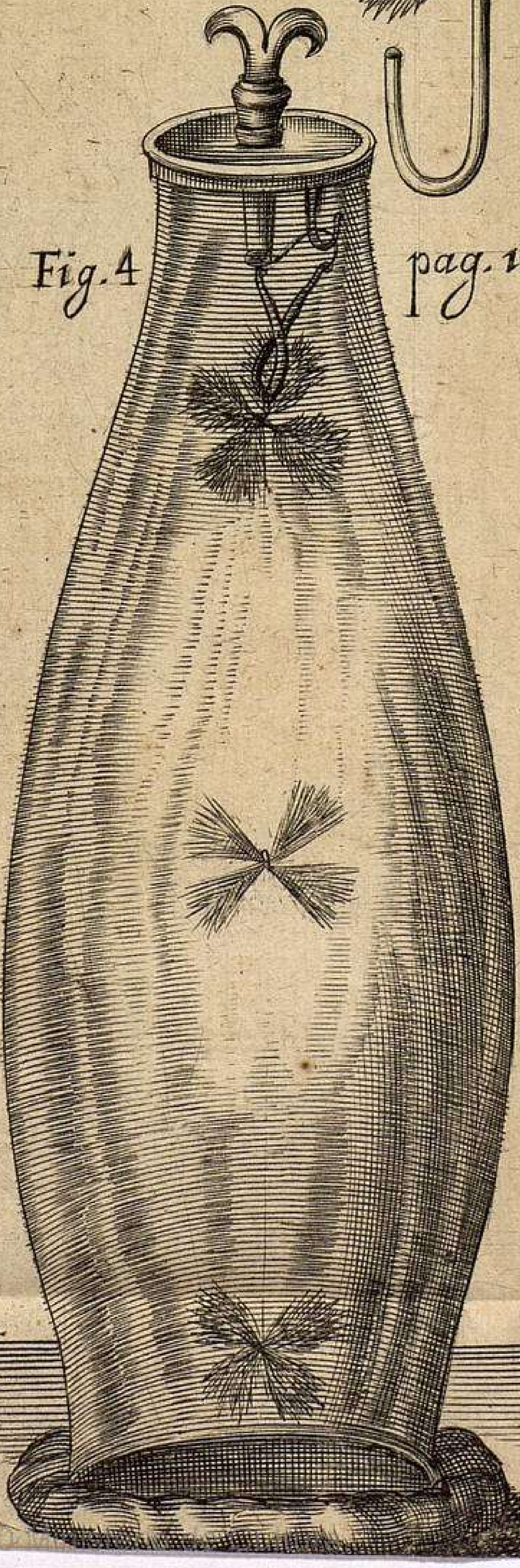

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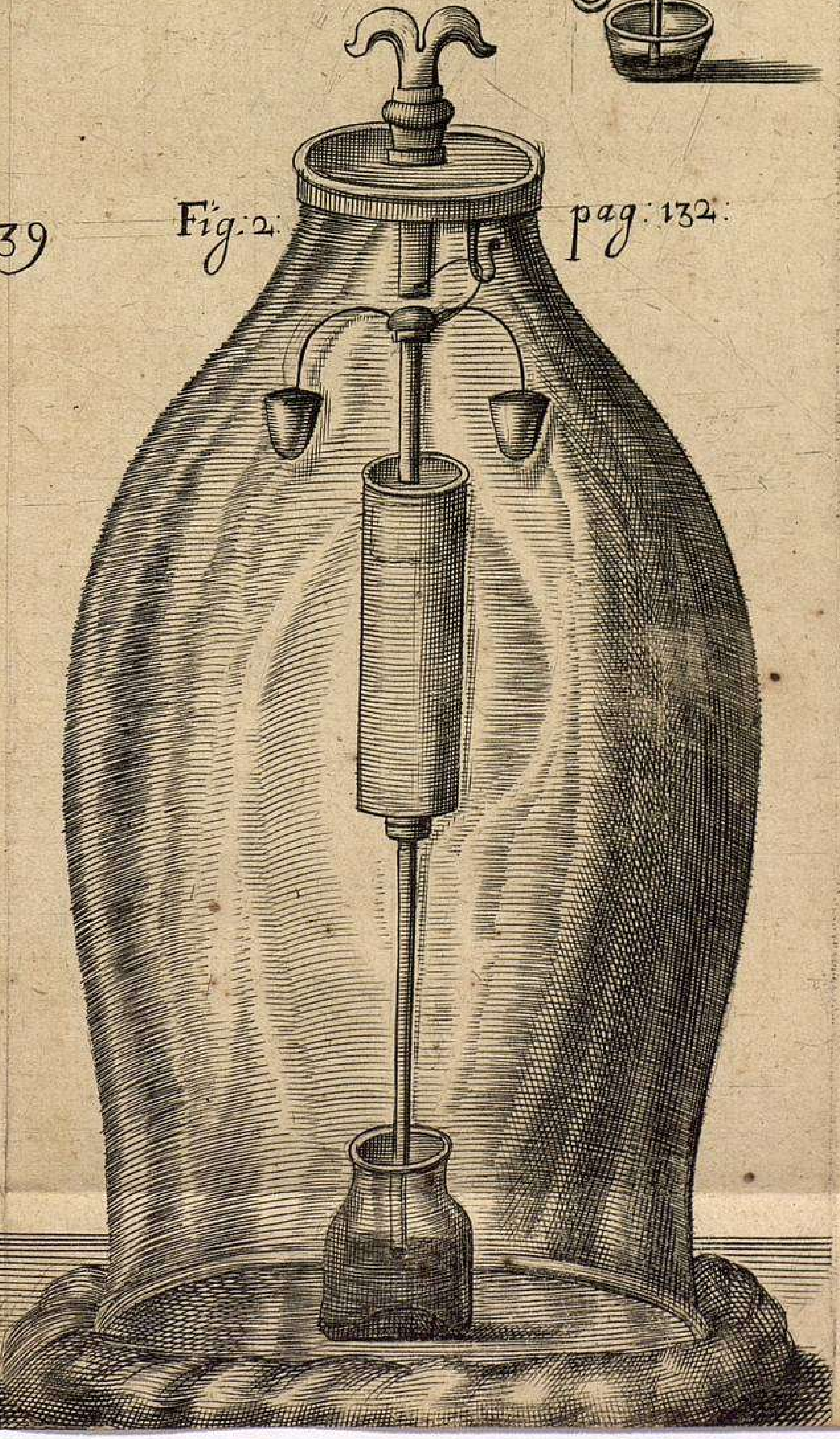

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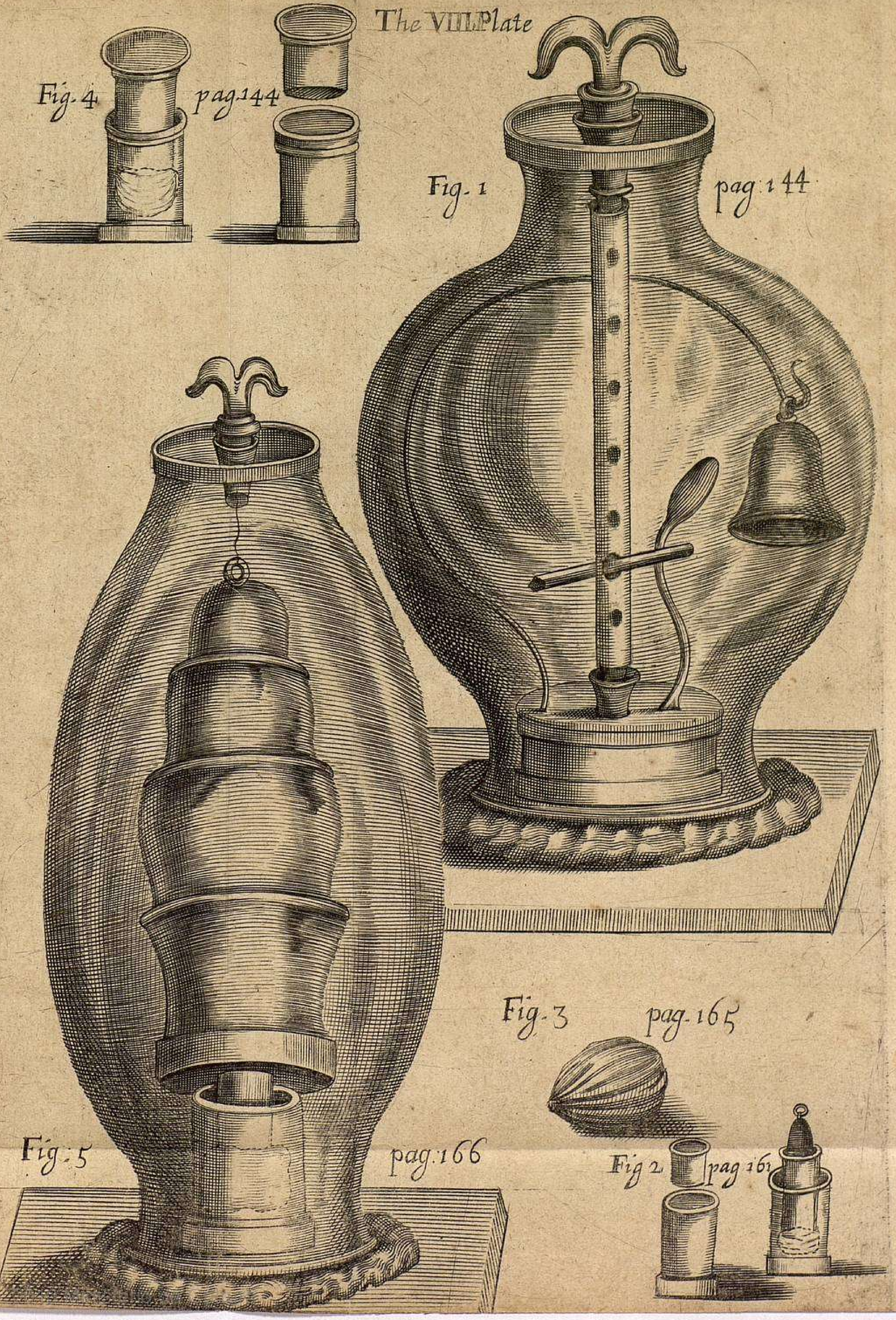

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