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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.

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16.

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 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

ſince

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 acco-

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 precisenesse 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-*

chimedes

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 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

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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 induced not to alter the Title of this Treatiſe, partly becauſe 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, conſiſting 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 unacceptable to the Curious, that the Latter will be not unwelcome 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 requiſite in this caſe, of ſtaying long enough in one place:) notwithſtanding all which difficulties I have by

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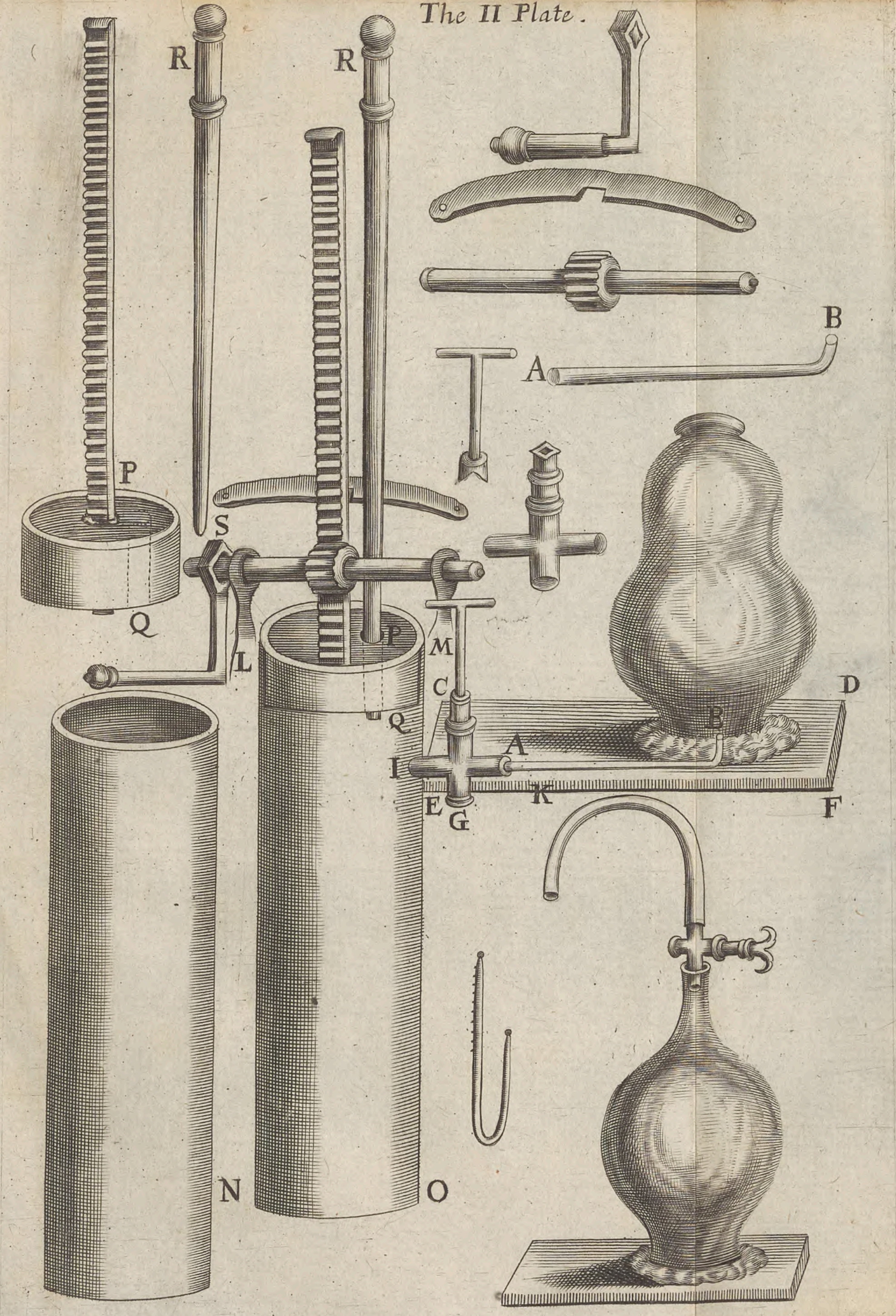
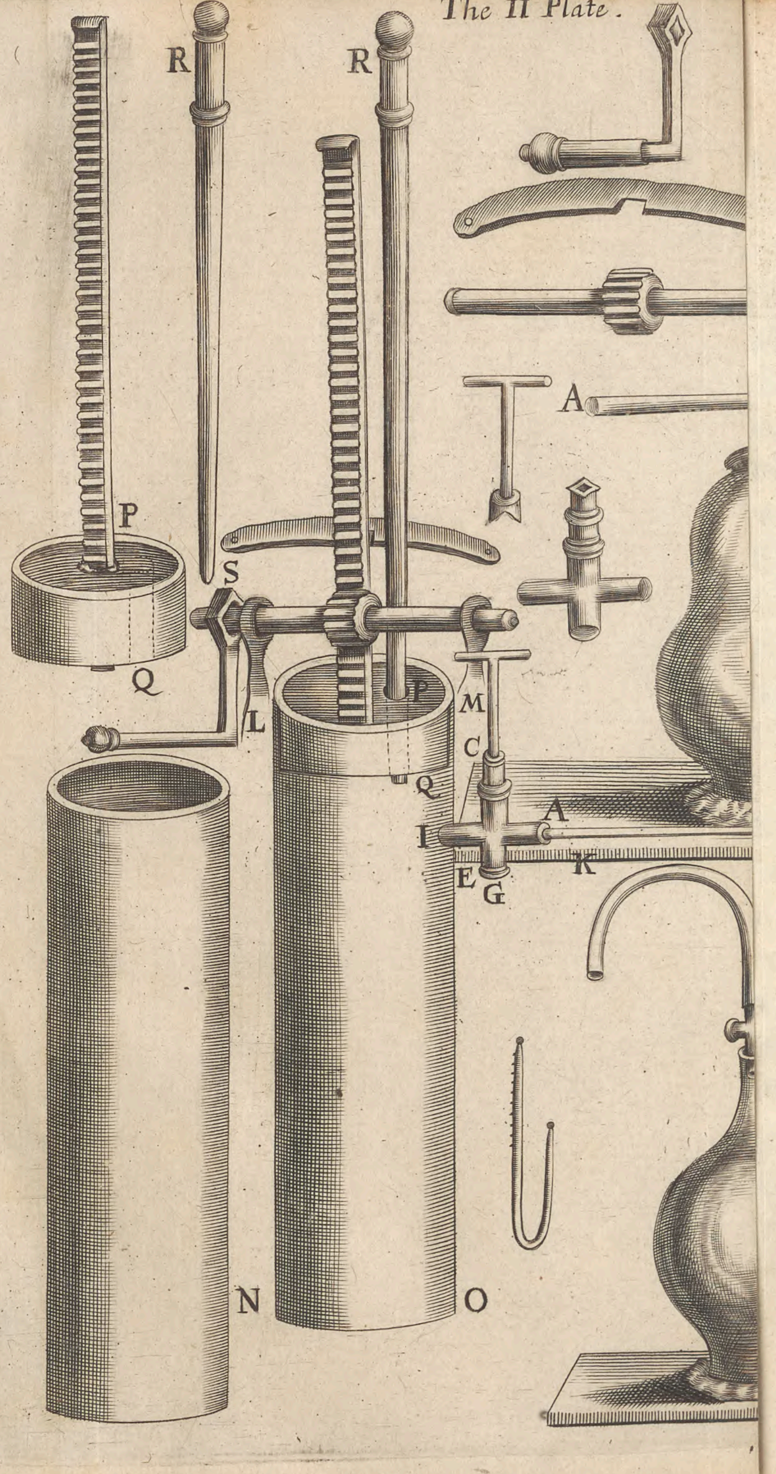
ſnatches been able through God's bleſſing to make forty or fifty of deſigned Tryals, being ſuch as require the leaſt of time to be performed in, though I now think not fit to mention any of them, as well for other reaſons, as becauſe though they be made by the help of our Engine, yet they require a peculiar 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

The II Plate.

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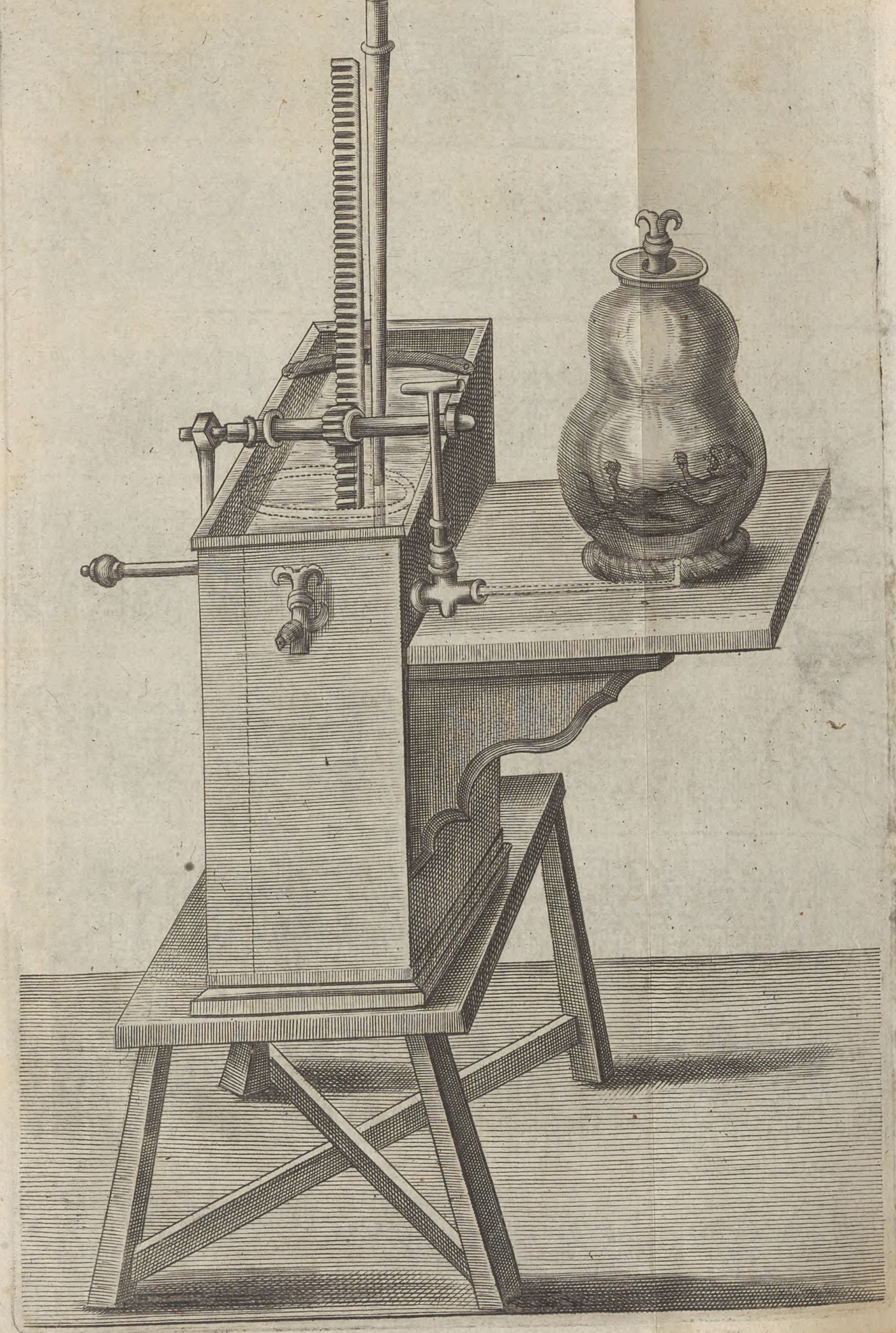
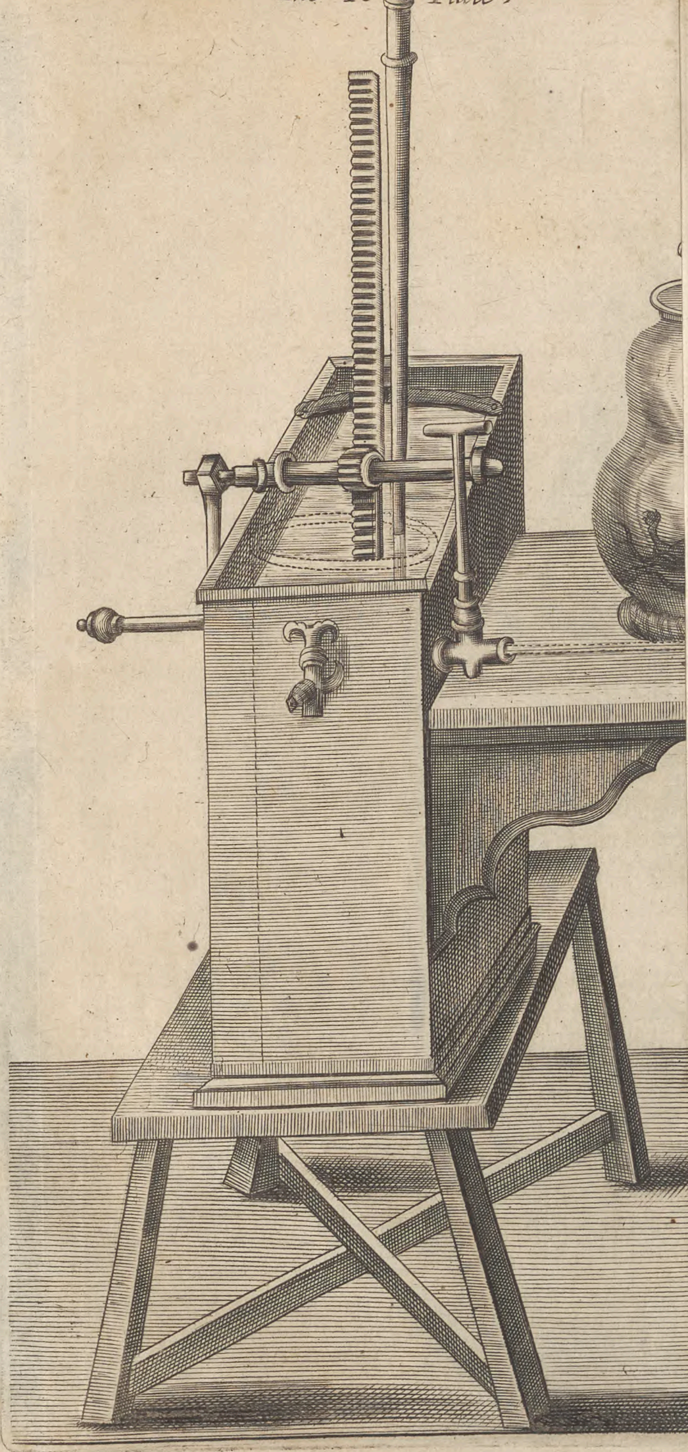


The I. Plate.



Some Advertisements touching the Engine it self.

THough the Engine already *published*, and that which I imploy-ed 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 particu-

larly

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 *I.* 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 *I.* 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

made

made but flender, and the part of it that looks upwards very fhort, it happens not very unfrequently, that when we imploy Receivers with narrow Orifices, where the Cement muft lie clofe to the opening of the Pipe, it happens, I fay, that the Cement, efpecially if it be much foftned by heat, is fuckt (as they fpeak) into the Pipe, and fo choaks it up; or elfe that fome 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 paffage of the Air into the Barrel, againft which inconvenience, to add that upon the by, we ufe amongft other Expedients to place juft about the Orifice of the Pipe a fmall 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 fmall opening or two in the fide, to give the Air of the Receiver free accefs to the Pipe.

The fquare and hollow wooden part of this Engine, difcernable in the firft plate, is fo made, that it may contain not onely the Cylinder, but fo 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 ftill 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 infide of the Barrel, does together with the newly mentioned plumpnefs of the Sucker very much conduce to the exact keeping out of the Air. But this advantage is not without fome 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 fundry Experiments, when the included bodies are fuch that may be fpoiled or impaired (at leaft for the prefent) by that liquor. The fmalnefs of our Cylinder is a convenience in regard of the facility it affords to make and difpatch thofe many Experiments that may be performed in fmall Receivers, though it make thofe more troublefome and tedious, that require the Exhauftion 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.

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.*

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 Instrument 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 3 Figure the 4.

For the better illustration of this Experiment, thus summarily related, but with the like succes, 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, D^r^ *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.

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 2d 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 Dr *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 in 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 difference

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 fuppof'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, elfe

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

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 density 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 succes, 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 thicknes, 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 as wide as that of our Brafs-hoop, the ftraiter were lefs than an Inch in Diameter; You will find, that if this piece of Metal be made ufe of, as the other was in the foregoing Experiment, the flat Glafs cemented on to the Orifice, will be eafily broken, as formerly when tis faftned to the wider Orifice; but if the ftraiter Orifice be turn'd upward, the Glafs that covers it, if it be of a due thicknefs, (though no thicker than the former,) will remain entire, notwithftanding the withdrawing of the Air from beneath it: Which feems fufficiently to argue, that tis not precifely Natures abhorrency of a *vacuum*, that is the caufe why Glaffes are ufually broken in fuch Experiments, fince whether the wider or the narrower Orifice be uppermoft, and cover'd, (the Metalline part of the veffel being the fame, and onely varying its pofture,) the capacity of the exhaufted veffel will be equal; and therefore Nature ought to break the Glafs as well in one cafe as the other, which yet the Experiment fhows fhe does not.

Wherefore this Diverfity feems much better explicable by faying, that when the wider Orifice is uppermoft, the Glafs that covers it muft ferve for the Bafis of a large Atmofpherical Pillar, which by its great weight may eafily force the refiftance of the Glafs: whereas when the fmaller Orifice is uppermoft, there leans upon its Cover but fo flender a Pillar of the Atmofphere, that the natural tenacity or mutual cohæfion of parts in the Glafs is not to be furmounted 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 fufficiently manifefted the ftrength of the Airs Spring upon fluid Bodies, I next thought fit to try, whether the force of a little included Air would alfo 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

the

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 foregoing 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 inconside-

rable

rable 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 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 tis 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.

This

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.

NB. 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 Author's* 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 School-men 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 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 plate the Figure

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 Quicksilver: 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 thē 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

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

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 *See plate the Figure the*

to

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 above mentioned, 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, 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 par-

ticularly

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 eights; 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

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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 returning

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 only 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

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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

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.

Advertisement.

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 Baroſcope 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

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, 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

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 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.

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 should reach; 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 (Mr *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 Mr *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 7^{th} 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 3^{d}, who went up to the top at another time of the Year,) *viz.* That they begun their Journey from *L'oretava* on the 18^{th} 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 *Teneriff*; 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 *Teneriff* 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 Mercury, 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 neverthelefs 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 V[th]. 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 X[th] 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 tbe 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 Pin-hole,) 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

fidently 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 little 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.

EXPERI-

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.

9. Whe-

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 2[d] part of this Continuation, where mention is made of an attempt about it, which did not, I confess, displease me.

EXPERIMENT XXIX.

Of the seeemingly 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 widemouth'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 ſome inches,and that the ſalt did not onely line the inſide of the Glaſs, but, getting over the brim of it, cover'd the outſide of it with a Saline Cruſt: which made them that ſaw how litle liquor remain'd in the Glaſs, admire how it could poſſibly get thither.

And though I have mentioned but the Solution of Vitriol and Sea-ſalt, becauſe they are much eaſier than others to be procur'd, and yet the Experiment ſucceeds better in Them than in ſome other far leſs parable Salts; yet they are not the onely ones by whoſe Solutions the recited *Phænomenon* may be Exhibited.

As for the Cauſe of this odd Effect, though I ſhall not propoſe any thing about it with Confidence, till I have further inquired into it, and eſpecially till I have tryed whether the *Phænomenon* may be produced in an Exhauſted Receiver; yet, by what I have hitherto obſerved, I am inclin'd to conjecture, that it may be referr'd to ſuch a cauſe as that of the Aſcenſion of Liquors in Pipes after ſome ſuch manner as this.

Firſt, I obſerved, that in Water and Aqueous liquors, that part of the Surface which is next the ſides of the Glaſs, is (whatever the reaſon of it be) ſenſibly more elevated than the reſt of the Superficies; and if very litle clippings of Straw or other ſuch minute and light bodies, floating upon the Water, chance to approach near enough to the ſides of the Glaſs, they will be apt (which one wou'd not expect) to run up as twere this aſcent of Water, and reſt againſt the ſides of the Glaſs.

Next we may take notice with the Salt-boylers and Chymiſts, that Sea-ſalt is uſually wont to coagulate at the top of the Water in ſmall and oblong Corpuſcles, ſo that as to theſe tis eaſie to conceive, to them that have conſidered the firſt Obſervation, how numbers of them may faſten themſelves round about to the inſide of the Glaſs. And beſides Sea-ſalt, I have found by tryal divers others, *if their Solutions be ſlowly enough evaporated*, that will, whilſt yet there remains a good proportion of Liquor, afford Saline Concretions at the top of the Water. And the faſtning of Saline particles to the ſides of the Glaſs may perhaps be promoted

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 concave

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 *Phæno-*

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 succefs,) left 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 Syringedid 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 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 glafs of about two inches in length, (which fhould 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 Pedeftal of a convenient height, that the Glafs-pipe might be all feen beneath it, and a very low Viol almoft fill'd with Quick-filver might be fo plac'd underneath the Pipe, that the ftagnant Mercury reach'd a good way above the immerft orifice of the faid Pipe. Thefe things being thus provided, and the Handle of the Syringes Rammer being tied with a ftring to the Turning-key that belong'd to the Brafs-cover of the Receiver, this veffel was cemented on to the Engine, and by it Exhaufted after the ufual manner.

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

NB. I had once before feen the Mercury afcend into the Pipe upon the letting in of the Air into the emptied Receiver, but it feeming fomewhat difficult to me to determine whether the Sucker had been raifed, becaufe there was no mark to guide my Æftimate by, I thought it might be fufpected, that in cafe the Sucker had not been rais'd, the Afcenfion of the Quick-filver might have proceeded from hence, That the Air contain'd in the Glafs-pipe, breaking out through the ftagnant Mercury upon the Exhaufting of the Receiver, the Quick-filver might upon the return of the Air into the Receiver be preft up into the place deferted by the Air, that broke out of the Pipe. Wherefore we caus'd a ftring 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 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,

fied 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 $\frac{4}{5}$,) 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 off, 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 2^{d} 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 *16* 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 thē, 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 down

See Plate the Fig. the

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

was

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

often

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

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 befit 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 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

times

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.

EXPERI-

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: and *See Plate the Fig. the* 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 little 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

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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 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 (vvhich 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 vvas 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 vvould 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 vvas 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 vvhich, there were divers Horizontal Perforations bored here and there in the Pillar it self, to which this Axis belong'd, vvhich 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

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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 e-

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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 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 profecution of this Defign, knowing that hard Sugar, being nimbly fcrap'd with a knife, will afford a fparkling Light, fo that now & then one would think that fparks of Fire fly from it; we caus'd a good lump of hard Loaf-fugar 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 faftned fome pieces of a Steel-fpring, which being not very thick, might in their paffage along the Sugar, grate, or rub forceably againft it, and then the Receiver being diligently exhaufted in the Night-time, and in a dark Room, the vertical Cylinder (whofe lower *Axis* was inferted 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 fteady and ftrong enough. By which means the Irons that came out of the vertical Cylinder, making in their paffage vigorous impreffions upon the Sugar that ftood fomewhat in their way, there were manifeftly produc'd a good number of litle *flafhes*, and fometimes too, though not frequently, there feem'd to be ftruck off litle fparks of Fire.

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

EXPERIMENT XLIV.

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

WE took a large inverted Cucurbite for a Receiver, which being fo well wip'd both within and without as to be very clear, allow'd me to obferve, and to make others do fo too, That when the Pump began to be fet a work, if I caus'd a pretty large Candle to be held on the other fide of the Glafs, upon the turning of the Stop-cock to let the Air out of the Receiver into the Cylinder, the Glafs would feem to be full of Fumes, and there would appear about the Flame of the Candle, feen through them, 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 considerable number of Bubbles, but within about $\frac{1}{4}$ 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 $\frac{1}{4}$ 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 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 with 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 2[d] 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 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

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

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 away 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 75 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 19. 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 $\frac{1}{4}$) 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 V Vater and Air in a heated Eolipile 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 Atmo-

sphere

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 Pressure 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 propos'd, 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 8th 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 upper-

moſt

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 of some lesse heeded Phænomena of the Alterations of the Air, incase 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 themselves, 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 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 the expos'd Concretion had not thaw'd; yet I found its weight to be confiderably diminifhed, and this Experiment I fuccesfully made in more than one Winter, and in more than one place. And tis now but a few dayes fince, expofing not long before midnight, leffe than two ounces of Ice in a good Ballance to a fharply freezing Air, I fent for it before I was up in the morning, and though by the drynefs of the Scales the Ice that was in one of them appear'd not to have thaw'd, yet it had loft about ten Grains of its former weight; fo that here, the Evaporation was made in fpite of a double Cold, of the Ice, and of the Air.

I fhould now proceed to the mention of ponderous and folid Bodies, but before I do fo, it may be expedient to give you notice, that, to make the Proof of what I have propos'd more fatisfactory, and more applicable to our future purpofes, I fhall forbear to give you any Examples of the exhalations of Bodies, where fo potent an Agent as the Fire is made to intervene.

But though I purpofely forbear to infift on fuch Examples, yet it may not be amifs to intimate, that in explicating fome occult Qualities, even fuch Exhalations as are produc'd by the help of the Fire may be fit to be taken into confideration, as we may hereafter have occafion to fhew. And therefore we may obferve in general, that the Fire is able to put the parts of Bodies into fo vehement a motion, that except Gold, Glafs, and a very few more, there are not any Bodies fo fixt and folid, that tis not thought capable to diffipate either totally, or in part. Tis known to thofe that deal in the fufion of Metals, that not onely Lead and Tin, but much harder Bodies will emit copious and hurtful Steams. And there are fome kinds of that Iron, which our Smiths call Cold-fhare iron, about whofe fmell whilft it was red hot, when I made inquiry, the ingeniousfeft Smith I had then met with told me, that he had found it feveral times to be fo ftrong, and rank, that he could fcarce indure to work with his Hammer thofe parcels of Metal whence it proceeded. And even without being brought to fufion, not onely Brafs, 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

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 Experi-

ence

ence 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 another, 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

that

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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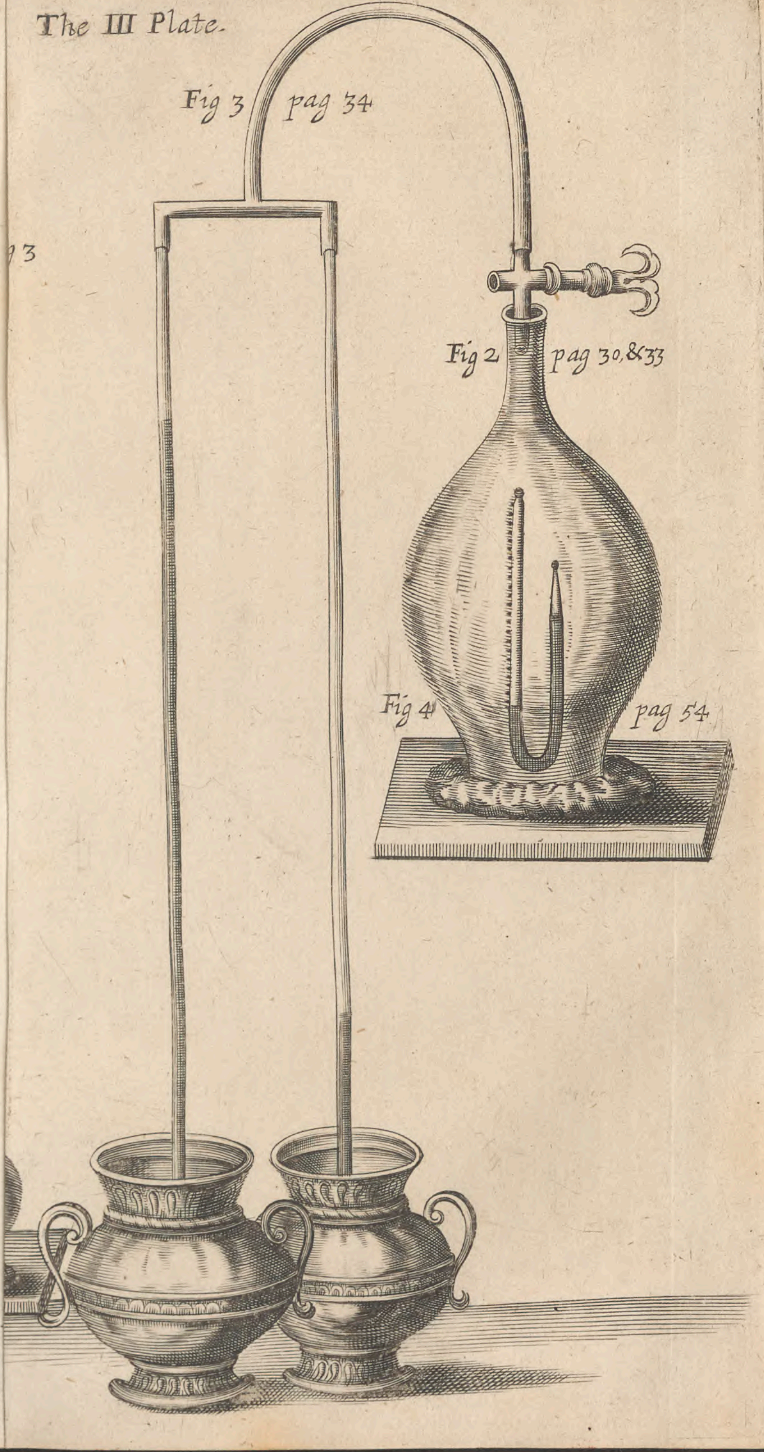

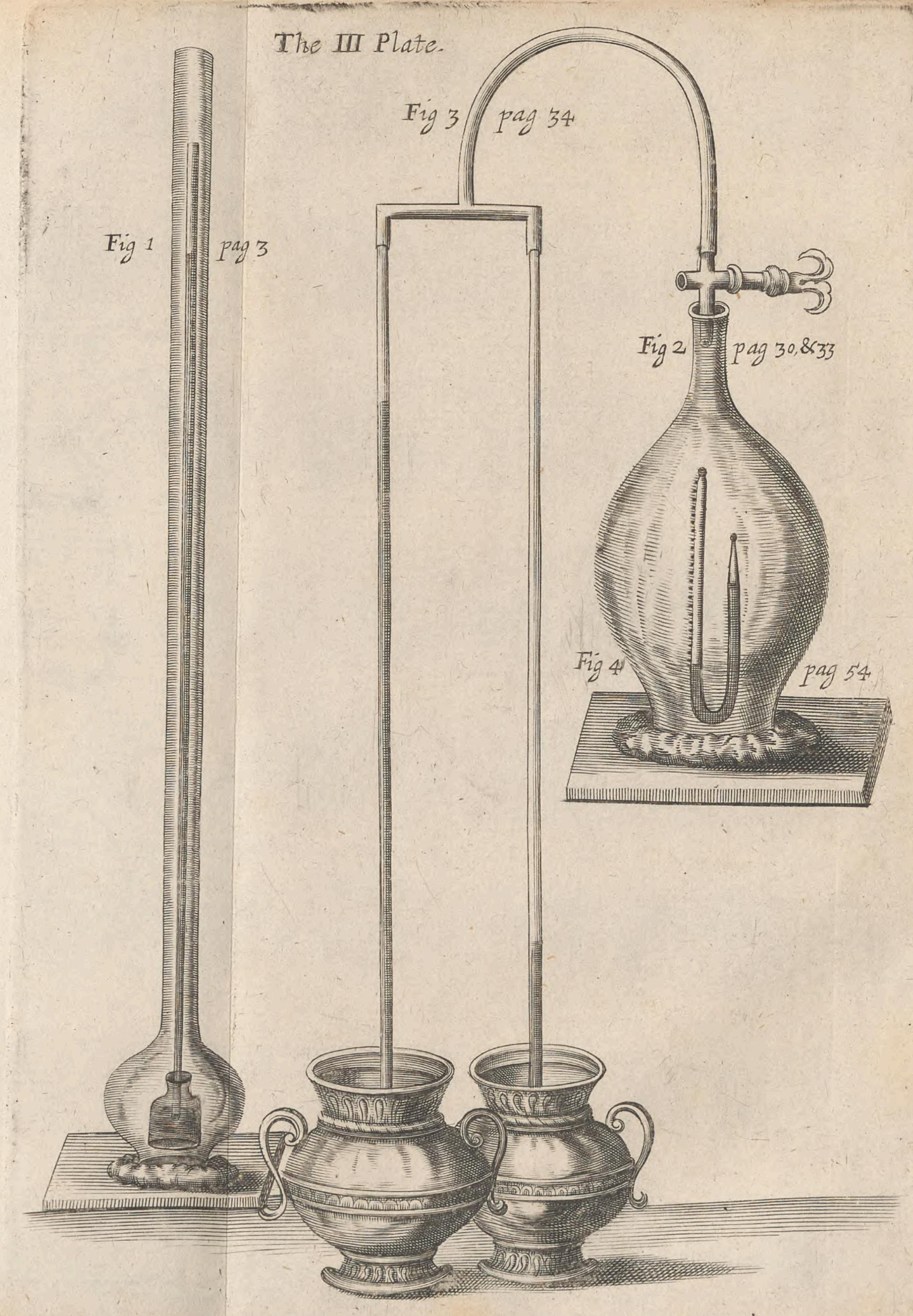
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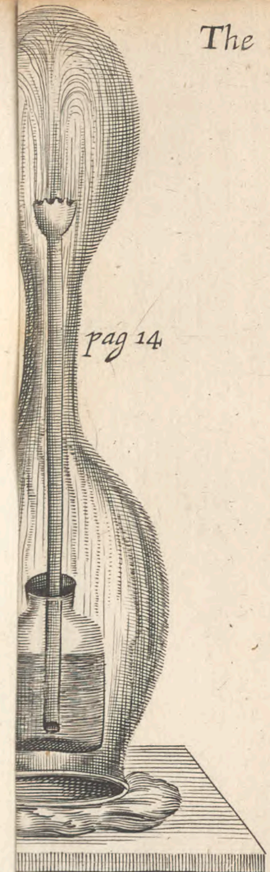

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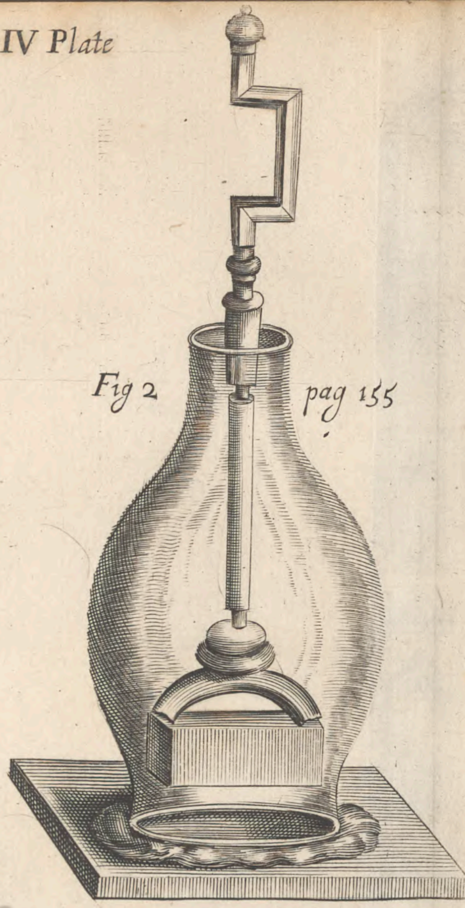

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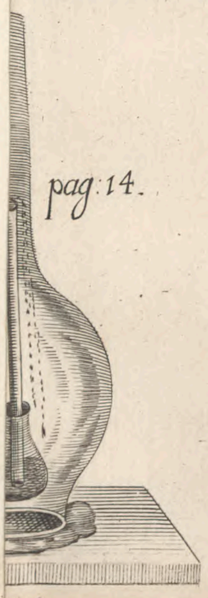

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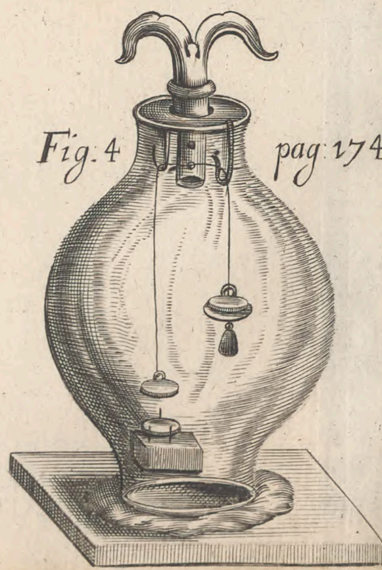

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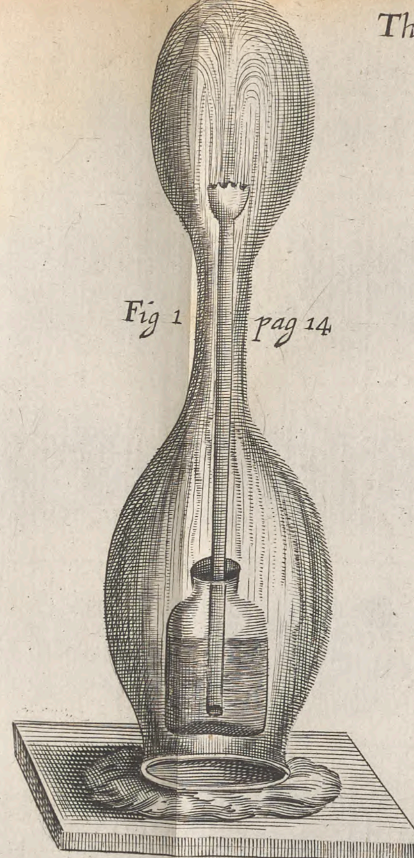

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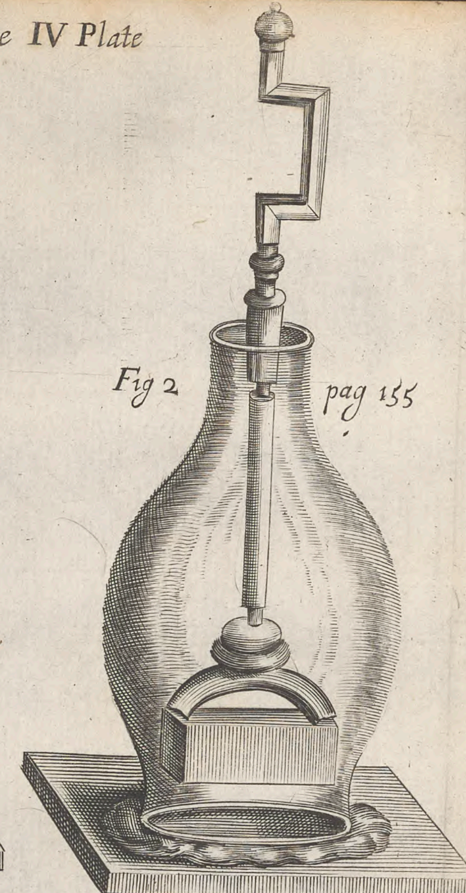

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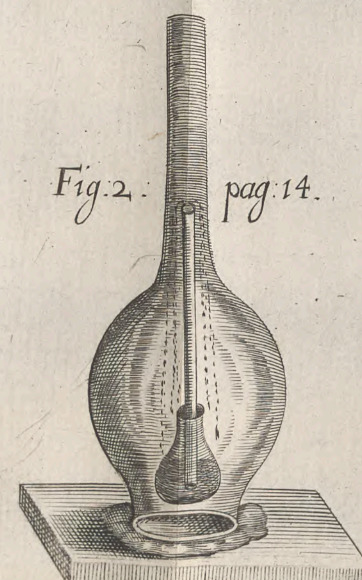

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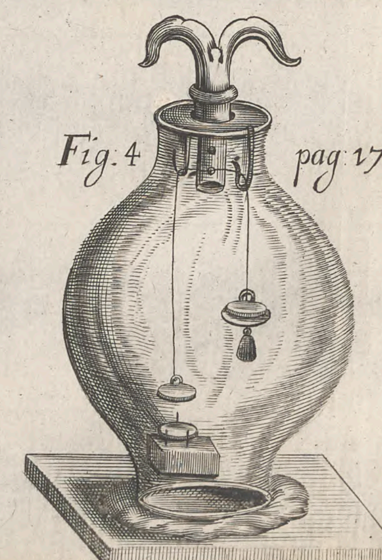

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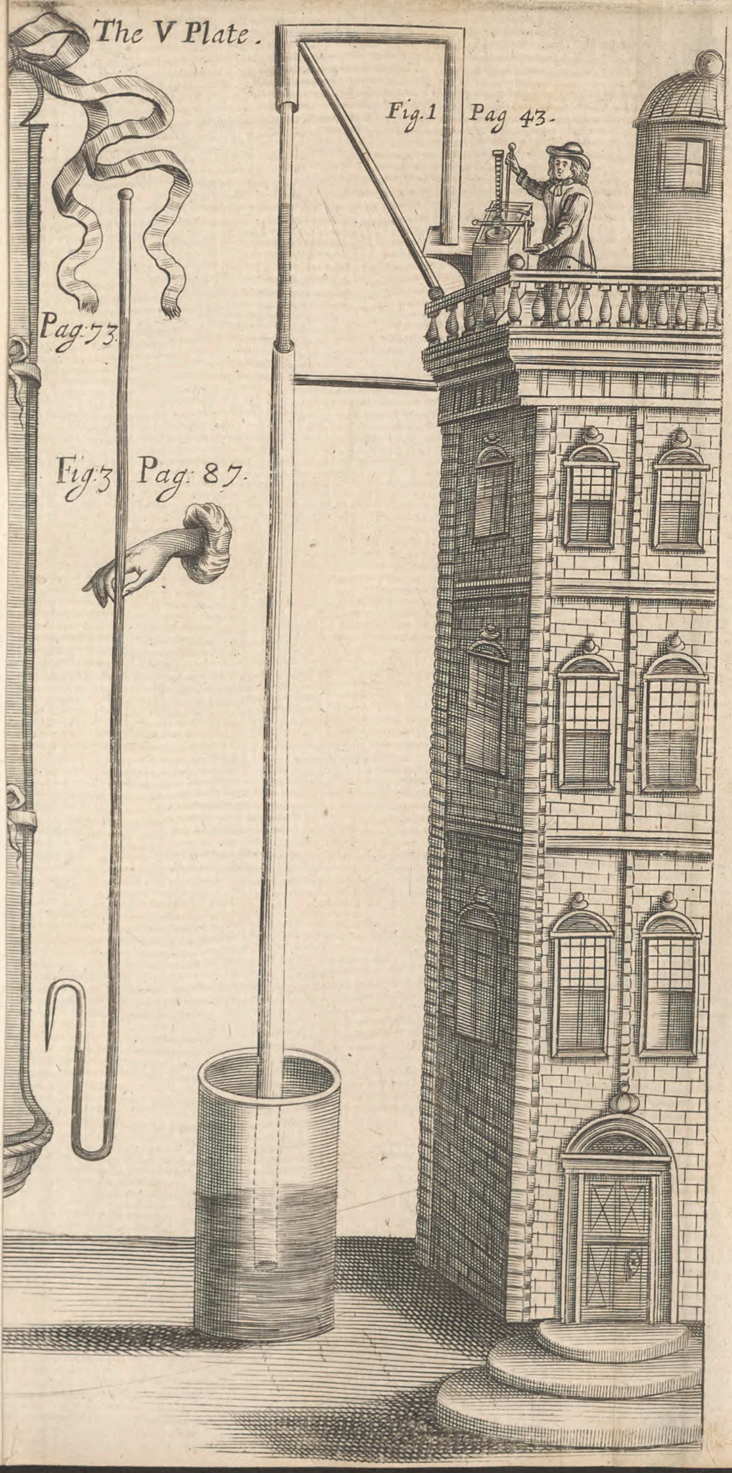
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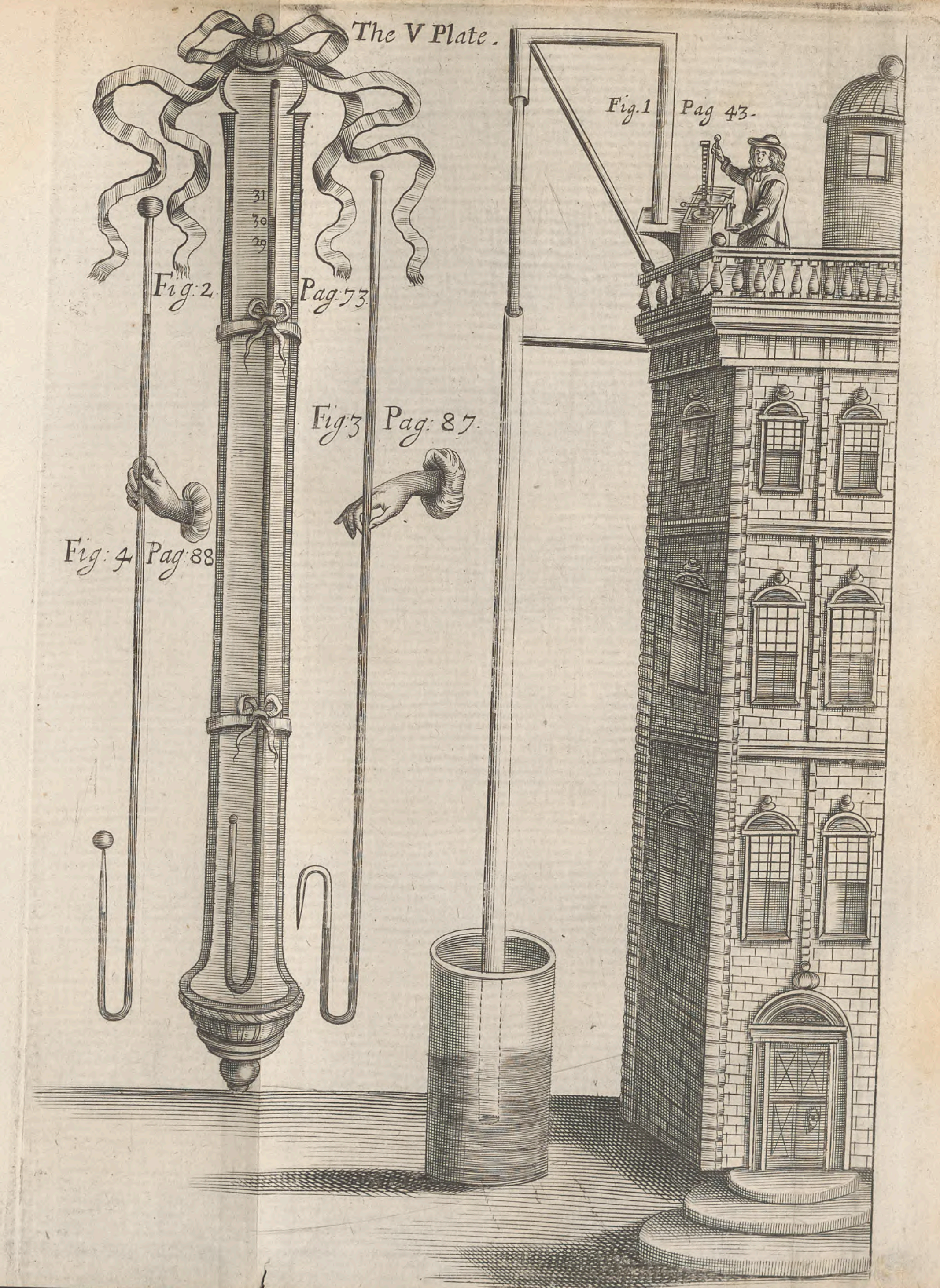
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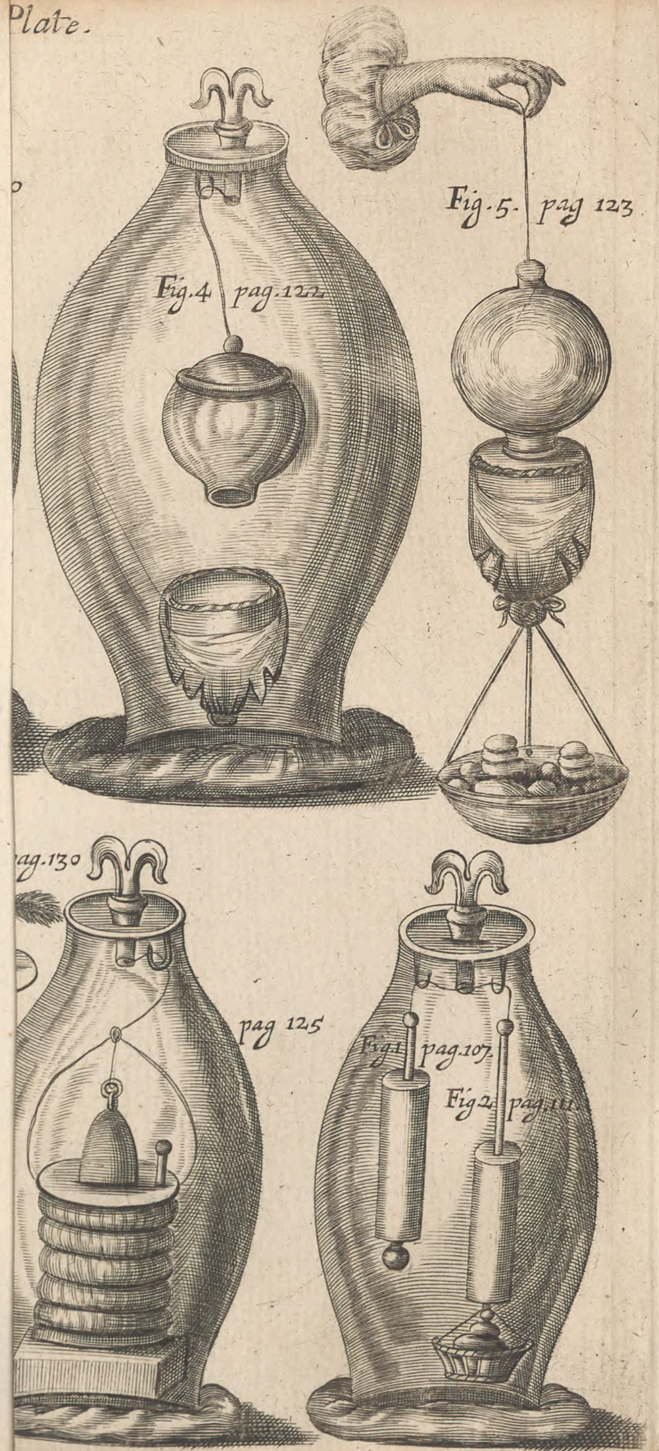
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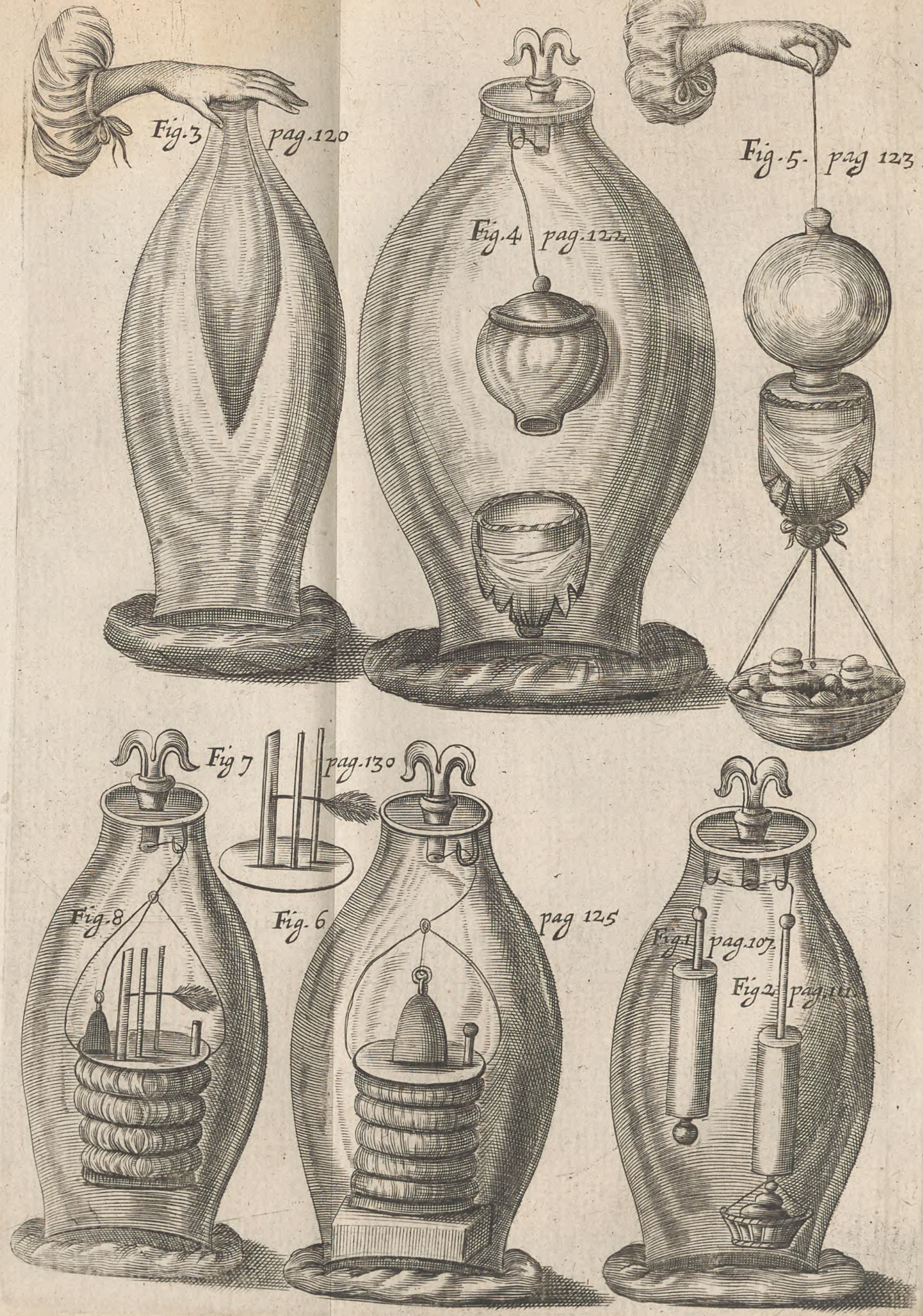

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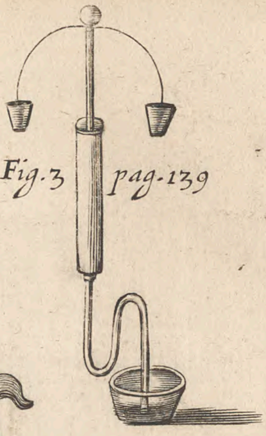

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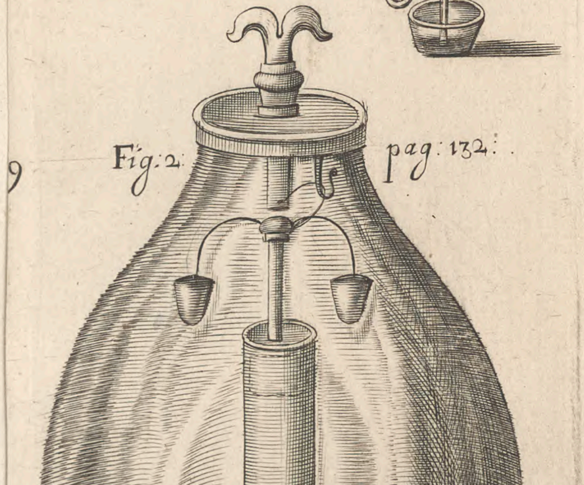

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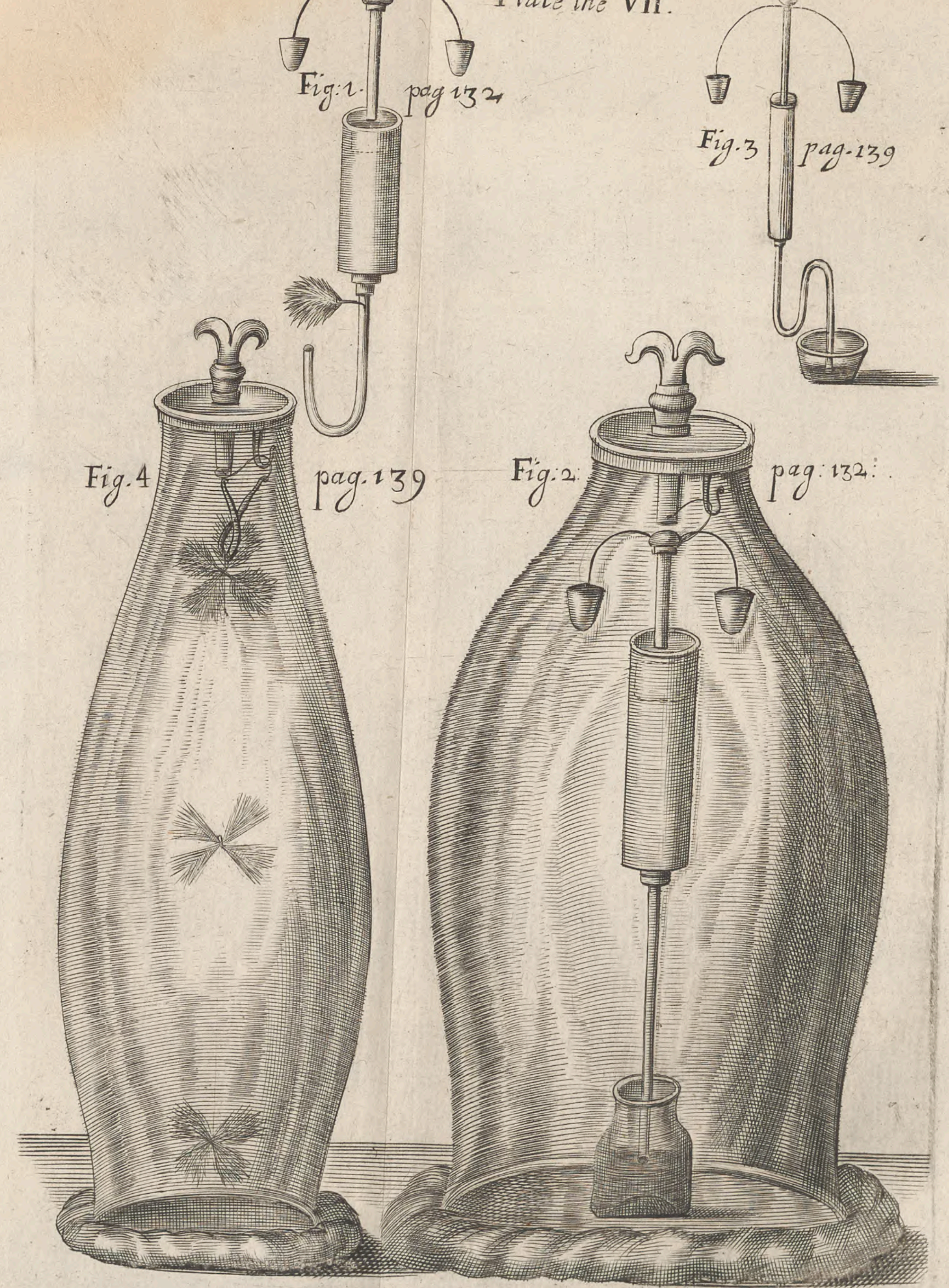

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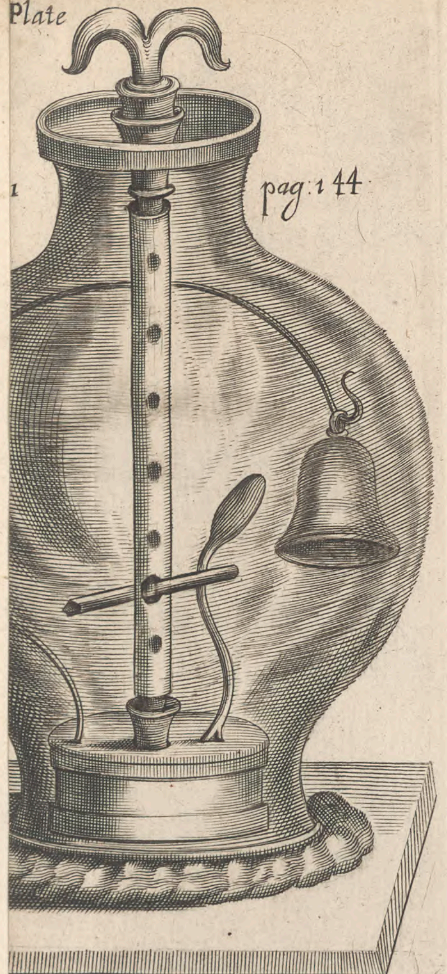

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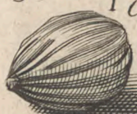

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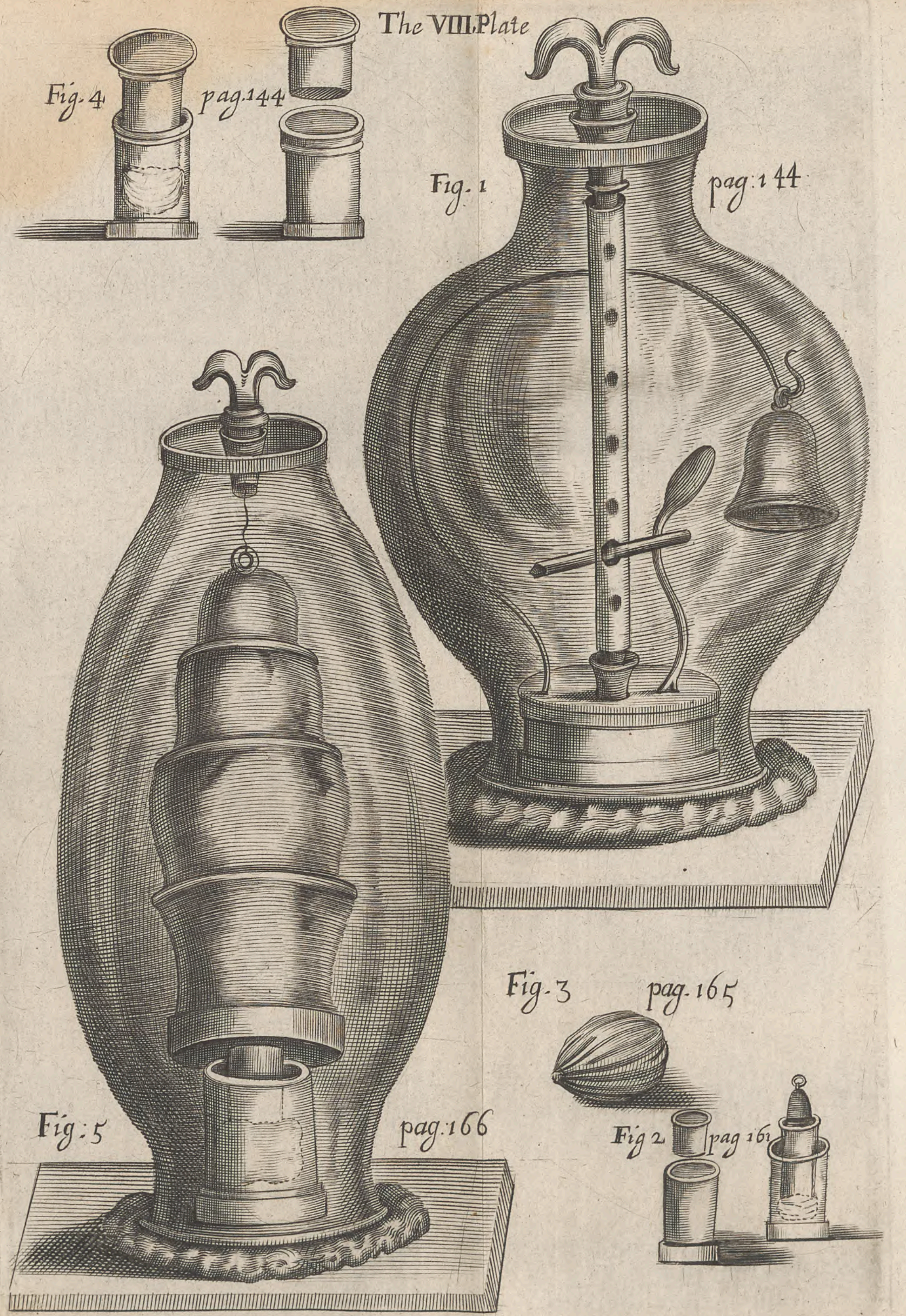
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M^r^ BOYLE'S { CONTINUATION OF EXPERIMENTS of the *Air*.

A CONTINUATION OF NEW EXPERIMENTS PHYSICO-MECHANICAL

Touching the

SPRING and WEIGHT of the AIR,

And their EFFECTS.

The Second Part:

WHEREIN

Are contained divers EXPERIMENTS made both in *compreſſed* and alſo in *factitious* AIR, about FIRE, ANIMALS, *&c.*

Together with

A DESCRIPTION of the ENGINES wherein they were made.

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

LONDON,
Printed by *Miles Fleſher*, for *Richard Davis*, Bookſeller in *Oxford*, *Anno Dom.* MDCLXXXII.

THE PREFACE TO The LATINE Edition.

AFter I had first published my Physico-mechanical Experiments *to the Curious World, and, some years after, the* Continuation *of them*, (*together with a full* Description *of the* Engines, *and lesser* Vessels, *which I used in the making of them*) *I thought it a very venial thing in me, if, superseding any farther labour upon such Subjects, I left that Argument to be studied, and, if they had pleased, cultivated by others. And therefore I was content to annex onely some* Experiments, *occasionally made, concerning* Respiration, *concerning the scarce credible* Rarefaction *of the* Air; *and lastly, concerning the* Preservation *of some* Bodies, *whilest they are defended from the contact of the* Air, *in regard those* Tracts *were of kin* *to other Arguments, which I had occasion to handle at several times. But in seven or eight years space, hearing of very few* Experiments *made, either in the* Engine *I used, or in any other made after the model thereof, I began to reassume some Thoughts, concerning the farther use thereof my self: At which time it happened very opportunely, That a certain* Tract *written in* French, *small in bulk, but very ingenious, containing sundry* Experiments *concerning the* Preservation *of* Fruits, *and some other* Tracts *of a different nature, was brought unto me by Monsieur* Papin, *who had joined his Pains with the eminent Monsieur* Christian Hugenius, *in making the said* Experiments; *And, upon farther discourse with him, finding that he came out of* France *into* England *but a little before, in hopes to obtain some* Place *here, which might be fit for his* Genius, *and, whilest he was in that expectancie, that he was willing to bestow his Pains about* Experimental Philosophy, *upon which, I had an Inclination, at my cost, to gratifie his Curiosity, whilest I also indulged my own. And, seeing he had a* Pneumatick-pump *of his own, made by himself, to the Use of which he was more accustomed, though it differed from the structure of my* Pump, *I gave him the freedom to use his own, because he best knew how to ply it alone, and (if any disorder should happen, from the luxati-*

on of its Parts, or any other casualtie) how to repair it more easily. Though, in his absence, I chose rather to use my own Pump, *both because my Domesticks were better acquainted with it, and also because it was not subject to so many and frequent Inconveniencies, by reason of its more solid structure.*

But, seeing several sorts of Experiments, *long since made on divers* Bodies, *had left me little to doe about the same Subjects; there were only two things, which I chiefly designed to prosecute. One of which contained those* Experiments, *which, when I first published my* Physico-mechanical Experiments, *I had wished in general had been made, not in rarefied or expanded, but in condensed or rather compressed* Air. *The other was to be versant about those Trials which were not to be made, as the former, with natural* Air; *either in its wonted state, or any way rarefied, but with factitious* Air, *(that I may so speak,) such as, in my former* Writings, *I had mentioned to be producible by the help of* Fermentations *or* Corrosions; *The divers waies of producing or extricating that factitious* Air, *and the waies of Trying it, when it was produced, having been some years ago presented to the* Royal Society, *I was invited, by that* Learned Assembly, *to prosecute farther those Disquisitions. Now, although those were the chief kinds of* Experiments *which I applied my mind unto, yet it will* *appear by the following Sheets, that I did not confine my ſelf to them alone.*

But, before I could make any conſiderable progreſs in this Work, it pleaſed the moſt Juſt and Wiſe God, *the Supreme* Arbiter *and* Ruler *of all things, to afflict me with the* Stone *(the Pains whereof do as yet now and then trouble me) ſo that I was enforced to take another courſe of proceeding. For, to eaſe my ſelf, it was judged meet, that Monſieur* Papin *ſhould ſet down in Writing all the* Experiments *and the* Phænomena *ariſing therefrom, as if they had been made and obſerved by his own Skill; and moreover, the Calculation of the Degrees of the Rarefaction and Condenſation of the* Air, *included in our* Mercurial Gage, *was intruſted to his Care. But I my ſelf was alwaies preſent at the making of the chief* Experiments, *and alſo at ſome of thoſe of an inferiour ſort, to obſerve whether all things were done according to my mind. But, as for thoſe* Experiments *which required a longer time in makeing, ſuch as thoſe about the* Conſervation *of* Bodies, *he did from time to time, with great diligence, acquaint me with thoſe Alterations, which happened in them, in my abſence; and he alſo brought the* Glaſs-inſtruments *to me, and declared to me the Effects of the* Experiments, *when they were finiſhed, that ſo I might take into conſideration the Changes made in* *the Materials, when taken out of the Vessels. Yet, I confess, I was purposely somewhat more incurious and remiss about those* Experiments *which were made concerning the* Preservation *of* Fruits, *and of* Flesh *in* Liquors, *which was made chiefly by the help of* Compression; *and also about the* Coction *of* Meat. *For, as some of these later* Experiments *were propounded for Tryal by Monsieur* Papin, *for a particular End of his own, somewhat different from my Design in the other* Experiments; *so I was very willing, that he should use his own method about them; not doubting but he would use his greatest Industry therein, as I found, by the Event, that he had done. Yea, I did judge, that I might more safely acquiesce in his Relations, concerning the* Experiments *about* Flesh, *about* Fruits, *and about* Boiling *of* Meat, *because, as these were some of the last which we made, so I had cause enough to trust his Skill and Diligence used about the former* Experiments; *some of which,* viz. *those which are marked with an* Asterisk, *he himself propounded, as if they had been formed in his own brain, as also not a few of the* Mechanical Instruments, (*especially, the* Double-pump, *and* Wind-gun) *which sometimes were of necessary use to us in our Work, are to be referred to his Invention, who also made some of them, at least in part, with his own hands.*

The Preface to the Latine Edition.

In the following Tract, *the* Reader *will not find the* Reasons *subjoyned, which moved me to make these* Experiments, (*which I usually did in my former* Physico-mechanical Experiments, *and in the Continuation of them*) *for I had neither leisure, nor a mind free from other businesses, to make such a* Preface; *and I did also hope, that the sagacious* Reader *would find out my Sense well enough, though purposely not expressed in plain words, if he did but attentively consider the nature of the things treated of, especially if calling in to his aid those short Corollaries, which he will find annexed to the several* Experiments, *whereby he may fish out my aim. Though, to speak the Truth, some few of those Inferences owe themselves more to my Assistant than to me.*

I am well assured, That very many of the following Experiments *will not be thought weighty enough by many* Readers, *as to deserve to be printed, and indeed I my self was so far of their mind, that I had once thoughts of expunging them out of the following* Collection; *But at last I was more easily persuaded to afford them a place amongst the rest, because, however they may be considered apart, yet, in consort with the rest, they may be, at least, of moderate use.*

I was not very solicitous about the style, because, being infirm in point of Health, and besides, surrounded with many businesses, I was enforced to leave the

the choice of words to Monſieur Papin; *my chief Care being to have the whole Worke diligently read over to me, that ſo no miſtake might paſs by unobſerved about the* Experiments *themſelves. Beſides, ſeeing the things here treated of are meerly* Phyſical, *and their manner of handling but* Hiſtorical, *there is no need of any farther Apology, to excuſe the incomptneſs of the ſtyle: Yet this may be alledged in excuſe thereof; That the Heads of things (or* Memorials *as they are called) being at firſt ſet down, for haſte, by Monſieur* Papin *in his own native Tongue,* ſcil. *the* French, *and afterwards turned into* Latine, (*in which habit they now appear*) *do labour with that inconvenience which doth uſually attend all* Tranſlations, *eſpecially where the* Interpreter *muſt have a greater care of the Propriety of words, than of the Elegancy of them.*

Moreover, he that ſhall attentively conſider the following Experiments, *will not wonder, that they are delivered in a leſs accurate method. For we accounted it ſufficient for our purpoſe, to reduce thoſe* Experiments, *which did differ and had leaſt affinity amongſt themſelves, into ſome certain Heads, to which they ſeemed moſt commodiouſly to be referrable: And, beſides, conſidering the nature of the* Experiments *themſelves, I hope the* Reader *will eaſily grant, that at leaſt many of them ought to have been ſet down in* *the way of a Diary, yet diſtinguiſhed and, as it were, intercalated by frequent intervals, becauſe the Examination of ſome of them was protracted for many days, the nature of the* Experiments *themſelves, and alſo the deſign of the* Experimentators *requiring ſuch Chaſms: Add hereto, That I was more willing to ſet down divers things, with their minute circumſtances, becauſe I was of opinion, that probably many of theſe* Experiments *would be never either re-examined by others, or re-iterated by my ſelf. For though they may be eaſily read, when ſet down with Pen and Ink in Paper-ſheets, yet, he that ſhall really go about to repeat them, will find it no eaſie task.*

For there are ſo many, and ſuch ſundry ſorts of Inſtruments, *both of a greater and leſſer ſize, which are neceſſarily required for uſe herein, ſome of them to be made on purpoſe for the preſent occaſion, reſpect alſo being had to the time and aſſiduity, requiſite and neceſſary for making the* Experiments *and* Obſervations, *in caſes wherein ſo ſubtile and elaſtick a* Body *as the* Air *is, muſt be violently reduced into a preternatural ſtate, and muſt be long kept in that diſpoſition, that, as it is a very difficult thing to prevent thoſe Inconveniences which do attend ſo unuſual* Experiments, *ſo it is far more difficult, to apply Remedies to thoſe Inconveniences, after they have once happened. For theſe, and other Reaſons, ſo much time*

is

is to be ſpent, that I am almoſt aſhamed to tell how much thereof was impended on theſe Trials *which are contained in the preſent* Book, *though but ſmall, to which this* Proeme *is prefixed.*

Neverthelefs, though all theſe things are alledged in excuſe, yet the deficiency of this Collection *is ſo well known to me (there being little to be found therein which may commend* Books *) that I would invite very few* Philoſophers *to the reading of ſo incult and unpolite a* Rhapſodie, *eſpecially from the beginning to the end. For though it may probably happen, that ſome* Experiments, *contained herein, may not be diſallowed by the Curious, yet they may have leave from me, to eſteem this whole* Tract *but as a looſe Heap (or rather Chaos) of Particulars belonging to the* Air, *eſpecially, as conſtituted in its preternatural ſtate, and to the operations of it upon ſome bodies, as clothed with ſuch and ſuch circumſtances; ſo that it is free for them to cull out onely thoſe* Experiments *which pleaſe their Curioſity, or any other of their Concerns beſt, without being obliged to reade over the whole* Book, *no more than a* Lexicon, *which we uſe not to conſult, but now and then, for the ſake of a word. In ſhort, 'Tis not probable, That a* Book *ſo impolite, as this is, will be either wholly read over, or can conciliate any favour from the reading, unleſs with thoſe* Readers *to whom a Book comes ſufficiently* *commended onely upon this accompt, That it contains things* New *and also* True. *For if those two Privileges are enough to obtain Favour, then there is no cause, that the following* Tract *should wholly despair of the* Reader's *benevolence, especially since some* Trials *contained therein do treat of the* Properties *and* Operations *of the* Air; *I say, of the* Air, *which, notwithstanding the laudable Endeavours of some ingenious modern* Writers *in the Explication thereof, yet is a Body which, I fear at present, we have greater use and necessity of than knowledge.*

An

An ADVERTISEMENT of THE PUBLISHER to the READER,

Before the Latine Edition.

SEveral Tracts, made by our Author, printed at *Geneva*, and bound up in one Volume, were not long ſince tranſported into *England*: In which matter, though the Author himſelf doth not complain (which yet he might lawfully doe) of the immoderate Liberty of ſome men, who have preſumed, unknown to him, to bind up ſo many of his Writings together, and to publiſh them. Not to mention the Print, as being but bad, (or at leaſt not accurate) yet there are two things in that Edition, which; in our Author's behalf, cannot be concealed without juſt reprehenſion, for they may empair his Credit much, eſpecially with thoſe to whom his Writings are no otherwiſe known than by that Collection.

For, Firſt, There is no Signification made therein, That any of Mr. *Boyle*'s Tracts were ever written in any other Language than that wherewith they are there clothed, *viz.* The *Latine*, whence it may probably come to paſs, That all the Faults and Defects of Style, which are wont to blemiſh Tranſlations, eſpecially ſuch as are literally made, may, by Readers, who are not otherwiſe enformed, be imputed to the Author himſelf, who, for Reaſons often rendred by him, was induced to write all his Works in the *Engliſh* Tongue: The Verſions of ſome of them into *Latine* being not ſo much as ſeen by him, till, being come from the Preſs, they were put into his hands.

Secondly,

Secondly, The several Tracts making up that Collection, are all dated in one and the same year; *viz.* 1677. as if they had been all, both writ, and also published, by our Author at once, whereas indeed some of them were made publick 8 or 10 years, some 11 or 12, others 17 or 18 years before ever this Collection saw the Light: Hence an Injury, greater than the former, may be offered to our Author; for those Readers, to whom neither Himself, nor his Lucubrations are known, but from that Volume, may be easily persuaded to believe, that those Experiments, if perhaps they meet with the same which are comprehended in these Books, and are also found in other mens Works printed before 1677, were transferred by our Author out of their Tracts into his own; than which nothing can be imagined or spoken at a greater distance from Truth: For, indeed, if, applying my self for three whole years to manage the Experiments of so Great a Person, and thereby having frequent opportunity to converse with him, I sometimes casually light upon something new, yet who sees not, that Thanks is to be returned to him alone, who afforded me both the Occasion of meditation, and also Leisure to operate; yet such is the Humanity of this Noble Person, that he mentions my Name in the Preface to this Book, as if some things therein were mine: Who then can justly say, that he hath excerped any thing from other Authors, who gives his own freely unto others? But, to make the matter more clear, and also, to satisfie some Ingenious Persons who have earnestly desired a Catalogue of all Mr. *Boyle*'s Works, I will here subjoin it, and also affix to each Tract the time of its Publication; for by this means any Enquirer will be able to perceive, that what was written by our Author for New, hath really been published before the Writings of all the rest. And besides, the Faults of many will be detected; for though some Writers have with Ingenuity enough cited the Name of our Author in their Works, yet more have done otherwise, transferring not a few

of

of his Experiments, together with the Ratiocinations explaining them, after the manner of Plagiaries into their Books, making no mention of his Name at all.

But here I must advertise the Reader of these two things:

1. That those Books, marked with an Asterisk, were long since turned into *Latine*; yea, some of them but a little while after their Editions in *English*; yet without any Additaments in their Versions.

2. The other, which might have been set in the first place, is, to hint the Reason, why this present Tract bears the Title of *Continuation*, &c. *Part the Second.* For you must know, that after the first *New Physico-mechanical Experiments* of our Author were published to the World, some years after, a large Continuation of them *in Quarto* was likewise printed, which was also translated into the *Latine* Tongue, but, by the Death of the person to whom the Charge of publishing it was committed, and other Accidents happening thereupon, that Version could not yet be found; and if no hope do appear of recovering it again, (which we do not wholly despair of) then probably a second Translation may be undertaken, for the sake of the Curious.

A
CATALOGUE
Of all the
PHILOSOPHICAL WORKS
Published by our AUTHOR.

* *NEW Physico-mechanical Experiments concerning the Weight and Spring of the Air; published in* English, Anno Dom. 1660.

* *A Continuation of them, Part I.* 1669.

* *The Defence of the New Experiments,*&c. *against* Franciscus Linus.

The Examen of the Physical Dialogues of Thomas Hobs, *concerning the Air. These two were published,* A.D. 1661.

* *The Sceptical Chymist,* 1661.

* *Physiological Essays, together with the History of Fluidity and Firmness, and some other Tracts, Printed* 1662.

* *The Experimental History of Colours begun,* A. 1663.

Concerning the usefulness of Experimental Philosophy; the first Tome: A. 1664.

The second Tome was printed, 1669.

* *A Tract concerning the Origin of Forms and Qualities,* 1666.

Though this Tract was turned into *Latine* divers years before the *Genevian* Collection was published, yet was omitted therein, whence it appears, that the Publisher was not very cautious, who affirms in his Preface, That all Mr. *Boyle*'s Works are contained in that Volume.

The

THE SECOND CONTINUATION OF PHYSICO-MECHANICAL EXPERIMENTS.

ICONISME I.

The description of the Engine, with a double Tube for the exhausting of the Air.

AA ARE Two Pumps made of Brass.

BB Are Two Plugs hollow within, and open below.

CC Are Two holes in the upper part of the Plugs with Valves opening outwardly, that they may afford passage to the air to go out, and hinder it from coming in.

DDDD Are Iron Rods serving to move the Plugs, and annexed to them, by means of the Gnomons FF.

EE Are Two flat Iron stirrups at the top of the Rods DD, on which the Operator must stand to set a work the Engine.

GGG Is a Cord joyned to the Two Stirrups, and compassing the Pully H.

LL Are Two Valves at the bottom of the Pumps, opening inwardly, for the admiſſion of the Air out of the Tube MM.

MM Is a Tube reaching from both Pumps to the Plate OO, by means of the Curvature PP QQ; which Curvature ought to be of ſo great length, that the Tube P QQ may not hinder the exerciſer of the Pumps, but that he may conveniently ſtand on the ſtirrups EE.

OO Is a Plate bored in the middle, on which the Receivers, to be evacuated, are to be put; as R for example.

Before this Engine can be fit for uſe, it is to be put into a frame of wood to ſupport it, as is ſhewed in the ſecond Scheme, and as much water is to be poured through the hole Q in the Plate OO into the Pumps, as is ſufficient to fill the Cavities of the Plugs, and a little more; and then ſome body muſt ſtand on the two Iron Stirrups EE, and muſt alternately depreſs and elevate them. For by this means it will come to paſs, that the Plugs, following the motion of the Stirrups, in their aſſent will leave the ſpace in the bottom of the Pumps empty, and ſeeing all other paſſage is intercluded from the Air, that Air alone which is contained in the Receiver R is conveighed into the aforeſaid Pumps by the Tube QQ PP M, and opens the Valve L, which being preſently ſhut hinders the ſame Air from making a regreſs: wherefore the Plug, afterwards deſcending, Compreſſeth that Air, whence of neceſſity the Valve C muſt be opened, and all the Air muſt paſs out at it, *viz.* becauſe the water in the bottom of the pumps doth exactly fill all the ſpaces, and doth alſo regurgitate through the Valve C.

Here we may obſerve, That this *double* Engine is upon many occaſions to be preferred before a *ſingle* one (that is moved with the Foot,) for it doth not onely produce a double effect, but performes it alſo much more eaſily; for in thoſe Engines, which are furniſhed but with one Tube, whilſt the

the Plug is drawn up to evacuate the Pump, the whole Pillar of the Air, incumbent on the Plug, is to be elevated by force; and again, when the Plug returns back, it is also by force to be restrained, lest it should be too swiftly impelled by the Air, and so break the bottom of the Engine; but in these double Engines, the Plyer of them is in a manner wholly free from that toyle. For in the First suctions, the Plugs are easily lifted up, because the Air, immediately derived from the Receiver R into the Pumps, presseth the Plugs downwards, almost as strongly as the external Air incumbent on the opposite part; and when the quantity of the internal Air is dimi-nished, it comes to pass that the Plug to be depressed, tends downward with so much the greater force, and so by means of the Cord GGG compassing the Pully, draws the other Plug upwards, and at the same time hinders it from too much velocity of descent. And by this means both Plugs at one and the same time will be helpfull to him that exerciseth the Pumps.

Seeing the Plugs make but a very small resistance, a man may easily judge, that the two Pumps of this Engine may be plyed with greater ease and also with more speed, than one Pump in single Engines can, so that this engine is of great use in order to those Experiments, which cannot be well made, but with velocity and speed.

ICONISME II.

The description of the Mercurial Gage.

THE First description of a Mercurial Gage, to discover the degrees both of the rarefied and condensed Air, may be seen about the beginning of the Continuation of our Physico-Mechanical Experiments; but those Gages which I used in the following Experiments, are declared in the ſubſequent Scheme.

Fig. 1. The whole Gage ABCDE conſiſts of Three Glaſs Tubes, all very well faſtned and cemented together, yet ſo, that a paſſage is open from one to the other; The firſt of theſe Tubes AB being open at the extreme A, is of leſs capacity than the Tube BCD, but of greater than the Tube ED. The Tube BCD is crooked in the middle, and the Tube ED ought to be Hermetically ſealed, at the extreme E, but the part BCD muſt firſt be filled with Mercury.

This Inſtrument thus prepared, if it be put into a Receiver, out of which the Air is afterwards to be extracted, it will come to paſs, that the Air remaining in the part ED, will by its ſpring compreſs the Mercury DCB and force it to aſcend into the part BA, and its ſelfe will be dilated in the Cavity DC. If then the proportions be duely obſerved between the bigneſs and length of the Tubes, as ſhall be declared hereafter, when the Air is extracted, the Mercury will almoſt reach to the top A, and the Air in the other Leg, being ſo dilated, that it cannot ſuſtain a greater body of Mercury, will be kept included in that place.

But that this Inſtrument may exactly tell the quantity of the Air produced in its Receiver, the Tubes AB ED are to be diſtinguiſhed by marks into ſeveral parts; And when the Torricellian Experiment is tryed, above the plain Plate LM of the Pneumatick Engine, as you may ſee in the *Figure*, a Receiver FGE is to be taken, being perforated in the top F, and the Tube HI is to be tranſmitted through the hole, that ſo the Receiver may be applyed to the Plate; and then the Hole F being ſtopped, and the Gage ABCDE being put into the Receiver, the Air is to be exhauſted; the Air then being dilated in the Receiver, the Mercury cannot be ſuſtained ſo high in the Tube HI, but muſt deſcend by degrees; and at the ſame time

time the Air of the Tube ED drives the Mercury by little and little into the Tube AB. When then the Mercury in the Tube HI defcends to the height of 29 Digits (I take Digits for Inches throughout all this Tract) and ftays at that height, if we mark to what height the Mercury hath afcended into the Tube AB, we may know, that as often as the Mercury in our Gage fhall reft at that height, the Air in the fame Receiver will be able to fuftain onely 29 Digits of Mercury; fo that the place in the Gage, or in the Paper femblably divided, muft be marked with the figure 29. And fo further, every Digit of the defcent of the Mercury in the Tube HI may be marked in our Mercurial Gage, and the part AB will be fit to fhew all the degrees of the rarefied Air.

But now if the Air be condenfed in the Receiver above its wonted preffure, and all ways of its efcape be ftopped, you may immediately know it by the Tube ED; for the Mercury will be impelled into it by the incumbent Air, through the open hole fo much the higher, as the compreffion of the Air in the Receiver fhall be the greater; and how great that is, and what an altitude of the Mercury it can fuftain, may eafily enough be found out, if the computation be made after the manner following.

It is evident from the Experiments long fince publifhed by Mr. *Boyle* in his Anfwer to *Linus*, That the fpace poffeffed by the Air, is diminifhed in the fame proportion, as the compreffing force is encreafed, and *vice verfa*.

Let then (for Example) the fpace A be poffeffed by a certain quantity of Air, when (for inftance) the compreffing force is F; if now we encreafe that force by the addition of G, which is equal to it, it will happen, that our felf-fame quantity of Air will be reduced to half its fpace, fo that B the remaining fpace will be the half of the total fpace A, even as the former preffure F is the half of the total preffure F+G. So further, if we encreafe the preffure more by the *Fig. 2.*

addition of H, ſo that the firſt preſſure F is onely $\frac{1}{4}$ of the total preſſure F + G + H, it will come to paſs, that the Air can poſſeſs onely the ſpace C which is $\frac{1}{4}$ of the total ſpace A. And ſo afterwards, the remaining ſpace will be in the ſame proportion to the total ſpace, as the firſt preſſure is to the total preſſure.

The remaining ſpace. The total ſpace : :
The firſt preſſure : The total preſſure.

So that three of thoſe terms or quantities being known, it will be eaſy to find out a fourth by the Rule of proportion. For inſtance, In our Gage let the Tube ED be the total ſpace, in which the Air is compreſſed by the wonted preſſure of the Air, which in *England* is wont to be equivalent to 30 Digits of Mercury, or thereabouts; and therefore the firſt preſſure will be 30 Digits of Mercury. Now if that preſſure be encreaſed, and the Air be reduced into a narrower ſpace, ſuppoſe into the ſpace NE; if I would find out the quantity of this preſſure, I meaſure the remaining ſpace NE exactly, and I conſtitute that, ſuppoſe 6 Digits or Inches, for the firſt term of proportion; the ſecond term will be the total ſpace DE, ſuppoſe 12 Digits; the third term will be the height of 30 Digits of the Mercury, which was the firſt preſſure; and ſo the fourth term or total preſſure will be found to be 60 Digits of Mercury; whence I may conclude, that the preſſure of the Air in the Receiver can ſuſtain the Mercury to the height of 60 Digits: And ſo of the reſt.

From the ſame principle before laid down, it will be eaſy to collect, what ought to be the proportion between the Largeneſs of the Tubes AB and ED. For that depends on the length of the Legs, which the higher they are, ſo much the better they can reſtrain and keep in the Air being but a little dilated in the ſealed part. For inſtance, Let the length AB be of 10 Inches, which height of the Mercury is $\frac{1}{3}$ of the accuſtomed

preſſure

preſſure, it will be ſufficient that the Tube HB be twice as big as the Tube ED; for after the Mercury hath aſcended to the top of the Tube AB, the Air included in the other Leg, expanding it ſelf into the ſpace, forſaken by the Mercury, will poſſeſs three times more than its former ſpace, and ſo $\frac{1}{2}$ of the firſt preſſure, which is 10 Digits, will be ſufficient to curb its ſpring. But if the Legs were of leſs length, then the Mercury would be expelled by the included Air, at leaſt in part. And therefore the bigneſs of the Tube AB ought to have a greater proportion to the bigneſs of the Tube ED, that the aſcending Mercury may afford greater place to the Air to be dilated, and ſo, the ſpring of the Air being weakened, the weight of the Mercury cannot be overcome. And that would happen ſo, if the height of the Gage be to the height of 30 Digits, in the ſame proportion which the firſt ſpace of the Air is in, to the total ſpace, which the Air would poſſeſs *in vacuo*: According to the principle before laid down.

It is better that the height of the Tube be longer than ſhorter; becauſe if it be ſhorter, the Mercury will be expelled in part, and ſo will not be able to ſhew all the degrees of rarefaction; but if it be longer, this onely will happen, that the Mercury will not reach to the top, and ſo the Gage will nevertheleſs indicate all the variations, though they be leſs ſenſible ones.

But the Tube DC ought to contain that quantity of Mercury at the leaſt, which may be ſufficient to fill the Tube AB, before any way of eruption be opened for the Air included in the Tube ED. If the capacity of it be much greater, the matter is not much; nor need we be very ſolicitous concerning the Figure of this Tube.

ICO-

ICONISME II.

A description of the Engine to compress the Air.

Fig. 3. AA Is a Glass Vessel, whose orifice is exquisitely fitted to the plain Plate BB.

BB Is a plain Plate of Brass, made to cover the Vessel AA exactly.

CC Is a small Tube of Brass, passing through the middle of the said Plate, and fastened thereunto.

E Is a little Valve, opening inwardly, to shut the small Tube C aforesaid.

F Is the Spring depressing the Valve E.

GGG Is the Gnomon fastened to the Plate BB, made for restraining the Spring F.

II Is a square Lath, sustaining the Plate BB, and bored through in the middle to transmit the little Tube C.

LLL LLL Are two Iron Wires, which passing through the holes in the Lath I I and compassing the upper part of the Iron Plate KK, do hinder the said Plate, that it cannot be much moved from the Lath.

KK Is an Iron plate with a hole in the middle formed into a Female-screw, to receive the Male-screw MM.

MM Is an Iron Screw, whose use is, straitly to conjoyn the Receiver AA with the Plate BB. And lest the Brass Vessel should be broken, it is convenient to put some wood with Leather between the Screw and the upper part of the Receiver: Also Leather is to be put upon the Plate BB both to prevent the breaking of the Glass, and also for the more exact shutting of the Receiver.

NN Is a Pump fastened to the Tube C below the Plate BB.

OO Is the Sucker or Plug of the Pump NN.

P Is

P Is a little hole in the lower part of the Pump, by which the Air enters into it, when the Plug is brought to the loweſt part thereof.

Now if we would compreſs the Air by the help of this Engine, we put the Bodies, about which the Experiment is to be made, into the Receiver AA; and laying it on the Plate BB, we firmly bind it thereto by the help of the Screw MM. This being done, the Sucker or Plug OO is to be drawn, till the external Air by the hole P, can fill all the upper part of the Pump; then if the Plug be drawn upwards, it will come to paſs, that the Air finding no other way of egreſs, will open the Valve E, and enter into the Receiver AA, from whence there is no regreſs, becauſe the valve E is preſently depreſſed by the Spring F, and doth ſhut the hole C. And ſo we may iterate the compreſſion of the Air into the Veſſel AA, as often as we pleaſe, and the quantity thereof is eaſily known by the Mercurial Gages.

But I am wont ſo to faſhion the Pump, that it may be fitted by a Screw to the Tube C. For ſo when one Receiver is full, we may take away the Pump, and uſe it to fill other Receivers.

Now becauſe in theſe Engines, Mercurial Gages are uſed onely to ſhew the degrees of compreſſion, there is no need of uſing the Gages here, which are deſcribed in the firſt Figure; for they are made with more difficulty, and beſides, they afford but a ſmall ſpace to note the degrees of compreſſion in. And therefore it is better to fold the Glaſs Tube, ſealed at one end, in ſeveral places, as the Figure T ſhews, that a long Tube may be contained in a ſhorter Receiver: ſo that the Mercury being put in, through the open end, as much as will ſuffice to fill the length of one Digit, all the reſt of the ſpace filled with Air, will ſerve for the marking of the degrees of compreſſion, much more ſenſibly than can be done in a ſhorter Tube.

Here we muſt note, That when the Mercury tends downwards in ſuch an inflected Gage, the weight thereof doth help the external preſſure; but when it is impelled upwards, the ſame weight makes reſiſtance: This difference muſt be heeded, if we have a mind to try very accurate Experiments.

ICONISME II.

How mixtures may be made in compreſſed Air.

Fig. 3. LET the Receiver be AA, in which we have a mind to mix either liquors or powders.

Let QQ RR be two Tubes, each of them ſealed at one end, and open at the other.

Let RQS be a Veſſel of Braſs, to be laid upon the orifice of the Tubes, as is ſhewed in the Figure.

The Liquors to be mixed muſt be poured into the Tubes QQ RR, each liquor in his own Tube, and let the Veſſel inverted RQS be laid on the orifices of the Tubes, and in that poſture let all be covered with the Receiver AA, let the Screw be wrung or ſtraitened, and the Air intruded after the manner deſcribed fol. 9. And when you ſhall underſtand by the Gage TT, that the compreſſion is arrived at that degree, which you intend, the Engine is to be inverted, and ſo the Liquors will flow down from the Tubes into the Veſſel RQS, and be mixed there. If you deſire to mix more liquors or powders, then the number of the Tubes is to be encreaſed accordingly.

ICONISME III.

How factitious Air may be transmitted out of one Receiver into another.

I Tryed two ways (principally) to transmit Air out of one Receiver into another; but because the first of them seemed less convenient, I shall *here* onely describe the Latter.

AA Is a plain Plate made of Metal, having an hole in the middle.

BB Is the Stop-cock fastened to the hole in the middle of the Plate AA, one of whose ends is formed into a Male-screw.

DC Is a Copper Funnel open below, with a broad orifice (that so it might be easily set upon the Pneumatick Engine and there stand firm) and in the upper part the orifice D is fashioned into a Female-screw, to receive the Male-screw of the Stop-cock BB.

EE Is a small Tube, open at both ends, both whose orifices are excavated into a Female-Screw, to receive the Male-screw of the Stop-cock BB. *Fig. 2.*

FF Is the Receiver laid on the Plate AA, and exquisitely fitted thereunto. *Fig. 1.*

Now if we would make factitious Air, we must put the matter which is to produce the air, into the Receiver FF, and placing the said Receiver on the Plate AA, by means of the Screw, we must strongly fasten it thereto, after the same manner as hath been described in our Engine for compressing the Air; and the Stop-cock BB we insert into the Female-screw D; then the orifice C, and with it the Receiver, is to be placed upon the pneumatick-Engine, and the Stop-cock B being opened, the Air is to be extracted; when the Receiver FF is emptied of Air, the Stop-cock B is to be ſhut, that ſo all paſſage of external Air into the Receiver may be intercluded, and the Stop-cock being taken out from the Female-ſcrew D, the Receiver is preſently to be immerged in water,ſo that at leaſt the Plate AA with the Stop-cock may be covered therewith; for ſo it will be clear, that no Air from without can find ingreſs, and the Air produced out of the matter included in the Receiver, will be preſerved unmixed, and the degrees of its rarefaction or compreſſion are known after the ſame manner, as hath been deſcribed p.4.

Fig. 3. Now if we would tranſmit that Air into another Receiver; another Receiver FF with another Plate AA, and a Stop-cock BB is to be procured and evacuated after the ſame manner, as was before deſcribed, then by meanes of the ſmall Tube EE we joyn the Stop-cocks BB of both Receivers, as is ſhewn in Fig. 3, and all ſuſpected places are to be ſtop'd with Cement or Turpentine, that no external Air may find admiſſion; then, the Stop-cocks being opened, the Air produced in the former Receiver flows into the latter, and the Stop-cocks being again ſhut and plucked out from the Tube EE the Receivers may be kept apart; and if there be any matter included in the latter Receiver, we may eaſily view what influence the factitious Air hath upon it.

But becauſe the Mercurial Gages deſcribed fol.4. are ſpoiled if they be inverted, and the Gages, mentioned fol. 9. do preſently expel their Mercury, if the Air be rarefied in their Receivers; and ſeeing the operation, here deſcribed, cannot be perfected, but both Receivers muſt be inverted, and both likewiſe emptied of Air; we muſt make Gages of another ſort after the manner following. See Fig. 4.

AA Is a Glaſs Phial filled with Mercury to the Superficies DD or thereabout.

BB Is a Glaſs Tube very well cemented, in the orifice of the Phial.

CC Is

CC Is another Tube transmitted through the Tube BB, and reaching to the bottom of the Glass. This Tube must be sealed above and open below; neither must it so exactly fill the Tube BB, but that passage may be opened to the external Air within the Glass AA.

Now if you put this Instrument into a Receiver, from which the Air must be afterwards extracted, it will come to pass, that both Tubes will be exhausted of Air, and when you invert the Receiver, to take in new Air, as in Fig. 3 is declared; the Mercury will flow down to the orifices of the Phial, and will be there kept below the orifice of the Tube BB; and the new Air entring, will easily fill both Tubes and Phial: Then the Receiver being erected, the Mercury will again be stagnant in the bottom of the Phial, and the orifice of the Tube CC will be found demersed in it. Then if any Air be produced, out of the bodies included in the same Receiver, it will come to pass that the Mercury will ascend into the Tube CC, and there, reducing the Air into a narrower place, will shew the degrees of compression.

Note that almost all the kinds of factitious Air in the beginning are in part destroyed, and therefore the degrees of compression cannot here be so exactly known, unless we know by Experiments, what part of the Air is wont to be destroyed.

ICONISME IV.

An Instrument by which Air may be filtrated through Water.

AA Is a Glass Receiver, whose orifice, laid upon the Plate BB, agrees exquisitely therewith. *Fig. 1.*

BB Is a plain Plate with an hole in the middle, to transmit the Tubes CC DD.

CC DD Are two Tubes cemented to the Plate BB, one of which is no higher than the Plate, but the other reacheth almoſt to the Top of the Receiver.

EEEE Is a Stop-cock, to whoſe holes the Extremities of the Tubes CC DD are faſtned.

FF is the Key of the Stop-cock unperforated, wherein onely one chink GG is excavated.

HH Is the Receiver, compaſſing the end of the Stop-cock, and faſtned to it, ſerving againſt the ingreſs of the outward Air, and communicating with the Pump II.

LL Is a Glaſs Veſſel.

M Is a hole in the top of the Receiver, whoſe Stopple is faſtned with a Screw.

In the ſecond Figure there is exhibited a Stop-cock, cut tranſverſly, that the two Tubes CC DD may be the better diſtinguiſhed, and their inſertion into the Stop-cock be perceived.

This Inſtrument is *thus* to be uſed: We put the thing, about which the Experiment is to be made, into the Veſſel; and the Receiver AA being laid on the Plate BB, we pour water into the hole M till the Receiver be half full, or thereabouts, and the Veſſel LL, with the matter contained therein, do ſwim on the top thereof; then we ſtop the hole exactly, and faſten it with a ſcrew, in the ſame manner us hath been deſcribed in the firſt Scheme. Theſe things being thus prepared, the Key is to be ſet in that poſture that the chink GG may communicate with the Tube CC; then the Plug being brought to the loweſt part of the Pump, the Air of the Receiver AA, entring through the upper Orifice of the Tube CC, will flow down through the chink GG into the Receiver HH, and into the Pump. Then the Key being inverted, ſo that the chink GG doe anſwer to the inſertion of the Tube DD, the Plug is to be impelled upward, and then the Air will be expelled from thence, and, finding no other paſſage, will be driven through the chink GG, into the Tube DD; and from thence will emerge to the

upper

upper part through the water ſtagnant in the Receiver. Iterating this labour, we ſtrain the Air through the Water, as often as we pleaſe; and by this means, we know whether it be clothed with any new qualities, in reſpect of the body included with it.

ICONISME IV.

How the ſame Numerical Air may be ſometimes condenſed, ſometimes rarefied.

LET the Receiver AA be placed upon the Plate BB and ſcrued in, as is deſcribed fol. 8. *Fig. 3.*

CC Is the Stop-cock, faſtned to the hole in the midſt of the Plate BB.

DD Is a pump joyned to the Stop-cock C with a ſcrew.

E Is a Veſſel of that bigneſs, that it may fluctuate in the Receiver AA without danger of inverſion.

Let ſome Animal be put into the Veſſel E, and let the Receiver AA be put upon it and ſcrewed to it, as the Scheme ſhews. Then let the Pump be filled with water, and by a Screw fitted to the Stop-cock; the Stop-cock being then opened, let the Plug P be forced upwards, then the Water aſcending through the Stop-cock will, in part, fill the Receiver AA, and will reduce the Air, contained therein, into a narrower ſpace, without any addition of new Air; if then you draw the Plug downwards, the ſame numerical Air will be again rarefied. Thus you may both condenſe and rarefie the ſame Air as often as you pleaſe; and by this means you may find out, whether the condenſation of the Air do contribute any thing to prolong the life or health of Animals, yea or no?

ICO-

ICONISME II.

The description of a Wind-Gun.

AA IS a Copper Globe, hollow within.

BB Is a Tube, faſtned to the Globe.

F Is a Valve opening inwardly, and ſhutting the Globe BB.

G Is the Spring depreſſing the foreſaid Valve.

H Is a Gnomon affixed to the Globe AA, and making faſt the Spring G.

CC Is a Tube of Iron, faſtned to the Tube BB and the Globe AA.

DD Is a Plug exactly fitted to the foreſaid Tube.

EEE Is another Plug fitted alſo to the Tube BB with an Iron Wyre, reaching almoſt to the Valve F.

R Is the protuberance of the Tube CC, ſomewhat hollowed above to receive the end of the Iron LL.

LL Is a crooked Iron, moveable about the Extremity in R, ſo that it is like a leaver to lift up the Plug EEE.

OPO Is a crooked Iron, faſtned in M, that the Thumb ſticking in the Angle P, the reſt of the Fingers may attract the Leaver L, and ſo force the Plug EEE upwards. But the Curvature is made for this uſe, that the one end O might be applyed to the ſhoulder, if it be thought fit to aim at any mark.

TT Is a rectangle of Iron, compaſſing the Leaver LL and the Iron OPO, to keep the Leaver in that poſture, which the preſent Scheme holds forth; for otherwiſe the Plug EEE, would be thruſt out far away, whileſt we intrude the Air into the Globe AA.

II Is an elliptick hole in the upper part of the Globe very well ſhut with a Valve, opening inwardly; whoſe uſe is to give

give liberty of inſpection, and of amending what is amiſs; for the Valve may be drawn through the hole by reaſon of its elliptick Figure.

SS Is a metalline plate tranſverſly placed above the hole II, and perforated to tranſmit the Screw V, by whoſe help the Valve ſhutting, the hole II is ſuſtained and is applyed cloſely to the hole.

Q Is an hole in the inferiour part of the Tube CC, by which the Air enters into the Tube, whileſt the Plug D is brought to the loweſt part of the Tube.

The Air is thruſt into this Engine after this ſort, I tread with my foot upon the crooked end of the Plug DD, that it may not be removed from the ground, and I lift the Engine upward, till the upper part of the Plug be found below the hole Q, and then the Air entring through the foreſaid hole, doth wholly fill the Tube CC.

Then I forceably depreſs the Engine, and ſo the Air, contained in the Tube CC, opens the Valve F, and is thruſt into the Globe AA; whence it cannot return, becauſe the ſaid Valves preſently ſtop the paſſage; and thus by iterated turns, we may condenſe the Air in the Globe, untill the force of its Spring cannot be overcome by our ſtrength.

Now if we would diſcharge the Air, ſo condenſed, the Plug DD is wholly to be drawn out, and a bullet of Lead to be put into the bottom of the Tube CC: Then by means of the Leaver LLL the Plug EEE is to be impelled upward, as we ſaid before, and then the extremity of the Iron-wire opens the valve B, and the air breaking out therefrom, expels the Leaden Bullet through the Tube CC with great violence.

Note that before the plug DD is again put into the Tube CC for the compreſſion of the Air, about half an ounce of water is to be poured into the ſaid Tube. For by this means no Air at all can eſcape out by the Plug, and moreover, that

D water

water exactly filling the upper part of the Tube CC, will Cause that the whole Compressed Air will be intruded within the Cavity AA, and so the condensation will be perfected much sooner, than if, at every turn, part of the compressed Air did remain below the Valve F.

This Engine is much better than any Wind-Guns hitherto mentioned in Print.

1. Because that seeing one onely Valve serves, both for the letting in, and discharging forth of the Air, it is less subject to be spoiled or impaired, than if two Valves were used for that purpose.

2. If any disorder happen in other Guns, the Engine remains useless, but here by the Elliptick hole, a man may take out the Spring and the Valve, and so mend whatsoever is amiss.

3. In other Guns the Valves being covered with Leather were put in before the Engine was on every side shut, and therefore Silver-solder could not be used in cementing the parts, but onely Lead-solder by which the Air, being much compressed could by no means be restrained; but here all things are well cemented with Silver solder, without danger of burning, in regard the Valve covered with Leather is put in afterward through the Elliptick hole II.

4. But this Engine is chiefly to be preferred before others on this accompt, because we immit several bodies into the Receiver, through the Elliptick hole, and so make many Experiments in highly-compressed Air.

ICONISME V.

An Instrument to distill in vacuo.

AA Is a Brass Vessel, shut below and open above. *Fig. 1.*

BB Is a Diaphragma or Midriff of Tin, whose edges are so polished on both sides that they exquisitely do agree and suit with the edges of the Vessells AA DD, which are also polished, and so keep the external Air from Ingress.

CC Is a Tube fastened to a hole in the middle of the Diaphragma BB.

DD Is a Brass Vessel whose aperture is applyed to the Diaphragma BB.

EE Is a Stop-cock fastned to the hole of the Diaphragma BB.

FF Is a Tube reaching from the Stop-cock EE to the hole for suction in the Pneumatick Engine.

GG Is a metalline Vessel shutting in the commissures of the Vessels with the Diaphragma, and also the Stop-cock, that it, being filled with water, may keep all safe from the external Air. This Vessel is to be soldred to the Vessel AA.

We use this Engine after the following manner, Taking away the Diaphragma BB, we put the things to be boiled into the Vessel AA, and so set it in a convenient place, that it be not shaken, whilest it is evacuated, then putting on the Diaphragma BB and the Vessel DD, we put to the Pneumatick Engine, and making use of the Tube FF, the Air is pumped out of the Vessels, the Vessel GG being yet first filled with water. Then the Stop-cock is to be shut, and taking away the Tube FF, we may place the evacuated Engine on the Fire, and the Vapours ascending through the Tube CC, are condensed

denſed in the upper Veſſel, and ſo we have a liquor diſtilled *in vacuo*; and the quantity of the generated Air, is known by the Mercurial Gage H, but that muſt be kept up in the Top of the Receiver, leſt the Mercury do exhale, by reaſon of too much heat.

Note that round pieces of Paper, perforated in the middle, are to be laid over the orifices of the Veſſels AA DD, to the end they may be better joyned with the Diaphragma; and the commiſſures of the Tube FF with the Stop-cock and Pneumatick Engine are to be fortified with cement, and the Stop-cock EE is ſo to be diſpoſed with the Veſſel GG that part of the Key may be prominent without the Veſſel through the hole, that ſo it may conveniently be turned, and yet nevertheleſs, the Stop-cock, with the Diaphragma, may be taken out of the Veſſell GG, whilſt the Veſſell EE is to be filled with fleſh or any other matter. And that is very eaſily done in this manner, The Key conſiſts of two parts, one of which M is turned in the Stop-cock it ſelf, by means of a certain chink, which receives the ſmall protuberance of the other part OO, which other part doth exactly fill the ſmall Pipe NN, faſtned to the Veſſel GG, and being prominent outwardly may eaſily be turned in it, and communicate its motions to the other part M, but it is drawn outward whilſt the Diaphragma BB is to be taken out of the Veſſell GG.

Fig. 2. Shews you another Inſtrument, herein differing from the former, that it is almoſt all of Glaſs and affords a longer paſſage for the vapours.

BB Is not a Diaphragma, but onely a ſmall Tube, poliſhed at both ends, that it may exquiſitely ſuit with the orifices of the Veſſells A and D.

AA DD Are two Glaſs Veſſels, whoſe orifices are applied to the Tube BB, and ſo the Vapours are eaſily tranſmitted from the one to the other.

EE FF GG I have the ſame Uſe as in the former Scheme, and the whole Inſtrument is to be evacuated after the ſame manner, and placed upon the Fire, except that here the Veſſel AA, as being made of Glaſs, muſt not be put on an open Fire, but *in balneo Mariæ*, or on Sand, and the Vapours will be condenſed in the Veſſel DD.

ARTICLE I.

Several waies uſed to help the Production of the Air.

EXPERIMENT I.

July 11. 1676.

BEcauſe it appears by the new Experiments publiſhed at *Paris*, in the year 1674. and which are to be ſold by *John Cuſſon* in St. *James* Street, That Bread alone can produce no Air *in vacuo*, we were willing to try whether yet it did not contain ſome Air, which might come forth ſome other way. I therefore included a little Piece of Bread, very moiſt and a little kneaded, *in vacuo* with a Mercurial Gage.

July 12.

In ſix hours ſpace no Air was produced yeſterday, but this night a little brake into the Receiver, as much as did ſuffice to ſuſtain three digits of Mercury; the reaſon was, becauſe I had neglected to fortifie the Cover with Turpentine.

Towards the Evening, I found the Mercury higher by one inch or thereabout, and I am very certain that nothing had entred from without.

July 13.

This night alſo the Mercury aſcended higher, but my Gage was not of that ſort as exactly to diſcover many degrees.

July 26.

This day the Piece of Bread disjoined its Receiver from the Cover, by the force of the produced Air, and the Smell of it was acid.

Hence it follows, That Water is a fit Diſſolvent to draw forth Air out of Bread.

EXPERIMENT II.

July 11.

I tried another way to extract Air from Bread, for by the help of a Burning-glaſs I burnt Bread *in vacuo*, and ſo I found that the Bread did generate much Air, and that Air did ever and anon break out, as by Fulmination; whence it ſeems probable, that Air is contained in Bread, but it is ſo cloſely coarctated therein, that no eaſie operation can give it a diſcharge; but if any thing could diſſolve and looſe that knot, it may then produce great effects.

EXPERIMENT III.

Sept. 22.

I took eight ounces of dryed Grapes, and, with ſeven ounces of Water, included them in a Receiver, able to hold 22 ounces of Water, the Grapes were bruiſed.

Sept. 23.

The Receiver was demerſed under the Water all this night, yet the Mercury aſcended two whole inches.

Sept. 30.

In ſeven daies ſpace, the Mercury came to the height of thirteen inches.

October 5.

In five daies ſpace, the Mercury ran up twelve inches, and was now 25 inches high.

Octob.

Octob. 18.

The Mercury did not proceed to aſcend with the ſame ſwiftneſs, and the Air began to paſs out of the Receiver, but not before this day; yet theſe Grapes produced much more Air than thoſe which I had included without Water. See *Art.* IX. *Exper.* I.

EXPERIMENT IV.

July 12.

I included of Raiſins of the Sun bruiſed ten ounces *in vacuo*, with a ſufficient quantity of Water to promote Fermentation.

July 14.

In 2 daies ſpace the Raiſins had produced ten inches of Air.

About the evening the Mercury was about fifteen inches high: the fifteenth day, the Mercury had almoſt reached to its accuſtomed height.

July 16.

This day, in the morning, I found the Receiver ſevered from its Cover, and the Air breaking forth through the Water, in which it was demerged: I included the ſame Raiſins again *in vacuo*.

July 18.

This day, in the morning, I found the Air again breaking out.

July 19.

I ſhut up the ſame Raiſins in the ſame *empty* Receiver.

July 21.

This day I found the Receiver full, and the Air breaking out of it.

I again ſhut in the ſame Raiſins in the ſame exhauſted Receiver.

July 23.

Yeſterday about noon I found the whole Receiver almoſt

full

full of Air, and this day in the morning I perceived the Air to paſs out very often. From the I. Experiment of *Artic.* IX. it appears, that Grapes, without Water, can generate but little Air: ſo that it is manifeſt hereby, that Water is a fit *medium* to elicit Air out of them: 'tis alſo evident that the Production of Air is not begun preſently upon the Affuſion of Water; but it proceeds on with greater ſwiftneſs, after that the parts of the Water in five or ſix days time have more deeply ſunk into, and pervaded the Grapes.

EXPERIMENT V.

Auguſt 13. 1677.

I included Pears in two Receivers *in vacuo*; and Plums in another.

Aug. 16.

In three days ſpace all my Receivers were filled with Air, newly generated; yea, one of them, which included the Pears, becauſe I had left it expoſed to the Raies of the Sun, in the ſpace of 24 hours, was ſeparated from its Cover, whence we may conjecture, that the Production of Air is very much promoted by the Heat of the Sun.

EXPERIMENT VI.

Octob. 16. 1677.

I took two ounces of Grapes bruiſed, and ſecured them from the ingreſs of Air, in an exhauſted Receiver, capable of containing twenty ounces of Water.

Octob. 17.

The Mercury roſe higher about one half-inch.

Octob. 18.

Theſe laſt 24 hours the Mercury ran up about another half-inch.

Octob.

Octob. 20.

The height of the Mercury was two inches.

The 22 it was almoſt 4.

The 27 it was almoſt 6 inches.

Jan. 2. 1678.

The Mercury as yet came not to the height of 10 inches.

Octob. 16. 1677.

I put 3 ounces of bruiſed Grapes, with half an ounce of Spirit of Wine into a Receiver able to hold 30 ounces of Water, and then I exhauſted the Air.

Octob. 17.

The Mercury aſcended but a very little.

Octob. 18.

The Mercury came not up to the height of one quarter of an inch.

Octob. 20.

The Mercurial Gage was out of order.

Jan. 2. 1678.

I this day found my Receiver filled with Air, and alſo, when ſome of the Liquor was poured out, ſome Bubbles were formed in the Turpentine about the Orifice, and were broke outwardly.

From this Experiment, made in two Receivers together, it ſeems to follow, that Spirit of Wine doth much advance the Production of Air *in vacuo*, though in common Air, it wholly hinders it. See the II. VIII. and XIV. *Experiments* of the II. *Article.*

EXPERIMENT. VII.

July 19. 1678.

I put Muſt, expreſſed from Grapes bruiſed, and kept for 10 months in a Veſſel, ſtopt with a Screw, into the ſame Receiver, being alſo ſtopped with a Screw.

July 21.

The Mercury had not aſcended at all.

23. The height of it was 3.

24. The height was 5.

25. In the morning it was 104.
Towards the evening the height was 137; and the Muſt got out.

26. The Muſt was almoſt all got out of the Receiver; and although the Air now did poſſeſs double the ſpace it did yeſterday, yet it kept up the Mercury in the ſame height.

27. About half of the remaining Muſt brake forth this night, becauſe I had omitted to *ſet* the Screw, leſt the Receiver ſhould have been broken in pieces.

From this Experiment it follows, that Grapes kept ſo long a time, do rather *acquire* than *loſe* a fermentative Virtue.

EXPERIMENT VIII.

Jan. 30.

I put two quantities of Apples, boiled the day before, into two Receivers ſtopp'd with a Screw; with one of them I mixed one third part of Sugar, the other had no Sugar at all.

N. *All theſe Receivers were quite full.*

Jan. 31.

I included raw Apples bruiſed in three Receivers; in one of them I mixed one third part of Sugar; the ſecond was without Sugar, and ſo was the third, but it differed herein from the ſecond, that it was ſix times as big: For by this means we may know, whether the capacity of the Veſſel, or the mixing of Sugar, or the crudity of the Fruit, can promote or retard the Production of Air.

Febr. 10.

In that Receiver onely which contained the raw Apples with Sugar ſome Air was produced.

Febr.

Febr. 14.

The raw Apples with Sugar had impelled the Mercury up to 30 inches; those that were boiled with Sugar, to two onely; in the other Receivers no Air was produced.

Febr. 18.

In the Receiver, containing the raw Apples with Sugar, the Mercury came to the height of 56 inches; in that containing the boiled Apples with Sugar, the height was 3. in the other Receivers there was also some Air produced, except in that wherein the boiled Apples without Sugar were put. I opened that Receiver in which the Apples had produced so great a quantity of Air; yet the Apples seemed hardly to be fermented, but were endued with a most pleasant Taste.

Febr. 21.

The boiled Apples without Sugar had lost some of their Juyce; and, opening the Receiver, I found the Cover to be broke, and yet the Apples were not rotten at all.

March 1.

In the great Receiver, containing the raw Apples, the Mercury was 25 inches high; in the little one, onely 7; but in that where the Apples were boiled with Sugar, the Mercury had ascended to 9 inches.

March 8.

In the great Receiver the height of the Mercury was 29; in the lesser 22 $\frac{1}{2}$; and where the boiled Apples with Sugar were, the altitude abode at 9 digits.

March 17.

The Juyce got out of the great Receiver; in the little one the height was 67; where the Apples were boiled with Sugar, it was 15 digits.

From this Experiment it seems inferrable, that Sugar, the Crudity of the Fruit, and the Largness of the Receiver, do all contribute to the Production of Air.

ARTICLE II.

Several waies to hinder the Production of Air.

EXPERIMENT I.

Decemb. 21. 1678.

I made Paſte of Bread-corn-meal, without Leaven, and put it into an empty Receiver, and then I put the Receiver in a certain Apartment, with Fire, which there kept a greater heat than is wont to be in the middle of Summer; yet the Dough or Paſte produced no Air in 10 hours ſpace; whence it ſeems to follow, that if Dough hath once ſuffered too much Cold, it can ſcarce recover its faculty of Fermenting; for, ſome years ago, when I made Dough without Leaven, in the Summer time it produced very much Air *in vacuo* in a ſhort time.

EXPERIMENT II.

May 23.

I included 3 ounces of Dough, kneaded with Leaven, in a Receiver capable of holding 50 ounces of Water; I alſo poured upon it ſome quantity of Spirit of Wine, to try whether Fermentation would be hindred by that means.

May 24. The Mercury was 3 inches high.
26. Little change.
27. No change.
May 29. No change.
June 2. It ſeemed to have aſcended a little higher.
14. No change.

Decemb. 14.

No more Air being produced from the Dough, I took it out from the Receiver, and found the ſmell of it not gratefull, but ſubacid: I put it into an empty Receiver, and there it roſe or ſwelled to double its accuſtomed ſpace, and made a little Ebullition.

May 23.

I included 3 ounces of Dough kneaded with Leaven in a Receiver able to hold 50 ounces of Water, but here I mixed no Spirit of Wine.

May 24. The Mercury was $19\frac{1}{2}$ inches high.

May 26. 'Twas 38 inches high.

27. There was no change.

Dec. 14.

The Mercury perſiſted in the ſame height; and this day, opening the Receiver, I found the Dough of a moſt acid ſmell.

From which Experiment it ſeems to follow, that Spirit of Wine, even in Dough kneaded with Leaven, doth hinder the Production of Air.

EXPERIMENT III.

Auguſt 29.

I included Pears, with a Mercurial Gage, in a Receiver full of Water, and then I intruded Air into it, till the Mercury ſtaid at 26 inches higher than it was wont; within a quarter of an hour, one of the Pears was broken, and afterwards almoſt all of it was reduced to the conſiſtence of a Pultis.

Aug. 30.

In 24 hours ſpace, the Pears ſeemed to have afforded no Air; but on the contrary, the Mercury in the Gage was depreſſed an inch and half.

Aug. 31.

I this day found no change in the height of the Mercury.

Sept. 1.

Now the Pears began to produce Air, and the Mercury was almoſt 27 digits high.

Sept. 2.

In 24 hours the Mercury aſcended more than 8 digits, and now 'twas 35 digits high.

Sept. 3.

The height of the Mercury was increaſed 17 digits, ſo that now it was 52 digits high or thereabout.

Sept. 4.

Within thoſe 24 hours the Mercury roſe 7 digits higher, and reſted then in 59.

Sept. 5.

It was 64 digits high; a Pear, being broken, was become black.

Sept. 6.

Three digits and more being added to the height of the Mercury, it came now to the 67 digits and $\frac{1}{4}$ beyond what it was accuſtomed.

Sept. 7.

It deſcended 3 digits, and reſted again in 64.

Sept. 8.

This day the Mercury was depreſſed to the 58 digit, and ſome of the Water had broke out; and therefore I ſtraitned or *ſet* the Receiver with a Screw.

Sept. 9.

The Mercury aſcended full 3 digits, and now ſtuck ſuſpended above 67.

Sept. 10.

In 24 hours it mounted $1\frac{1}{2}$, and ſtopped almoſt in 69.

Sep. 11.

Now it began to deſcend again, and was no higher than 67 digits; yet I am certain, nothing had eſcaped out of the Receiver, but it was a ſharp cold night.

Sept. 12.

No change did evene.

Sept. 13.

The height of the Mercury did again decreaſe; it was not above 64 digits: the Cold increaſed.

Sept.

Sep. 14.

In 24 hours it became higher by 6 digits, reaching to 70.

Sept. 16. It was 69 digits high, or thereabouts.
19. It remained in the ſame place.

Sept. 20. It again reached to 71.
23. The Mercury was again depreſſed to 69.

Octob. 1.

It came now to the height of 75 digits.

Octob. 3.

Yeſterday I found no change at all in the Mercury; but this day it ſtuck in 70; and the Cold was very bitter.

Octob. 5.

Yeſterday the Mercury did abide in the ſame place; but this day it reached to 75: it was a rainy day.

Octob. 7.

It continued rainy; and the Mercury continued in the ſame place.

Octob. 10.

Hitherto the Mercury was not changed; but this day I found it had deſcended to 69 digits; though the Rain ceaſed not.

Octob. 12.

Yeſterday the Mercury ſtood ſtill; but this day it was depreſſed to 65 digits: and the cold weather returned.

Octob. 13. The height of the Mercury was 64.
14. } The height } 69.
15. } was } 74.
24. The height was 68. It was a cold ſeaſon.
Nov. 2. The height was 64. The Cold encreaſed.

Nov. 5. The height was $80\frac{1}{2}$. The Cold abated.
2. The height was 65. It was a hard Froſt.
27. The height was 68. It was a Thaw.
Decem. 6. The height was 61. It was a very bitter Froſt.

From the former Experiment we may learn, That Fruits in a great Compreſſion of the Air, cannot produce ſo great a quantity of Air; for when I made an eſtimate of the quantity

of

of the Fruits, and of the ſmall ſpace which is to be filled with Air; I found, that that quantity of Air was not $\frac{1}{8}$ part of that which had been produced in an empty and a large Receiver: yet the Cold of the Water might alſo give ſome Impediment to the Generation thereof, as the following Experiment will confirm.

'Tis alſo farther manifeſt, that the Air is produced by iterated turnes, and as it were by reciprocations, even as all bodies in motion by the force of their gravity or of their ſpring are carried beyond their point of reſt, and ſo ſuffer many vibrations, or goings and returnings: Now although Cold and Heat are not the ſole cauſes of ſuch reciprocations, yet they ſeem to contribute much thereunto.

EXPERIMENT IV.

Febr. 22. 1677.

I included 10 ounces of Paſte in a Receiver capable of holding 22 ounces of Water, and afterward I thruſt as much Air into it as was ſufficient to ſuſtain 73 digits of Mercury, beſides the wonted Preſſure. In two hours ſpace I perceived no ſenſible change.

Febr. 23.

In 18 whole hours the Mercury ran up 7 digits onely, its height being 80.

In 6 hours ſpace it was now aſcended 3 digits; its height was 83.

Febr. 24.	Its height was	90
25.		97
26.		101
27.		105
28.		$107\frac{1}{2}$
March 1.		112

And Water ſeemed to be expreſſed out of the maſs.

March 2.	Its height was	120
3.		121

4, & 5. It ſtayed at 121

March

March 8.

These 2 or 3 last daies, the Frost being dissolved, the Mercury ran up 4 digits: the height thereof was 125.

March 10.

Yesterday the Mercury persisted in the same height; but this day, mounting 6 digits, it stayed in 131.

March 21.

By reason of the long cold season, no Air was produced: but in the three last daies the Mercury ascended 7 digits, and stayed in 138.

April. 4.

Yesterday I perceived the Mercury had ascended, but I deferred exactly to measure the quantity till this day: But in this very night one of the Iron-wires, that straitned the Receiver was broken, and so the Receiver was ejected to 4 or 5 foot distance.

From this Experiment we may conjecture, that the Compression of the Air did very much hinder the Production thereof; for *that* is wont to be perfected in Paste in 2 or three daies space. Moreover, Cold doth much hinder the same Production.

EXPERIMENT V.

March 1. 1677.

I included two ounces of Raisins of the Sun with six ounces of Vinegar in an empty'd Receiver, and Bubbles in a sufficient quantity did break forth: the Raisins were bruised.

March 2.

The Mercury in 24 hours space ascended not to the height of half a digit: yet some Bubbles still appeared.

March 25.

The Vinegar did alwaies appear interspersed amongst some of the Bubbles, yet the Mercury ascended not to the height of one digit.

By this Experiment it appears, That Vinegar doth hinder the Production of Air and Fermentation; ſeeing otherwiſe Raiſins are wont to afford much Air.

EXPERIMENT VI.

Apr. 7. I included 10 ounces of Paſte in a Receiver capable of holding 22 ounces of Water; afterwards I intruded Air into it, as much as ſufficed to ſuſtaine 128 digits of Mercury, beſides its accuſtomed height.

In 6 hours ſpace the Mercury mounted up 4 digits, and ſtaid in 132.

Apr. 8. In 16 hours the Mercury ran up 9 digits higher; it ſtaid in 141.

Nine hours after the Mercury was not changed.

Apr. 9. This day, in the morning, I perceived ſome Air had broke forth, and the Mercury was depreſſed to 130 digits, and therefore with a Screw I ſhut the Receiver more cloſely, and thruſt in 11 digits of new Air: the height was 141.

Apr. 10.	The height was	151	*Apr.* 14.	The height was	183
11.		158	15.		183
12.		168	16.		187
13.		176	17.		191

Apr. 27.

For eight whole daies the Mercury kept its ſtation in the ſame place, but theſe two laſt daies it aſcended 7 digits, and ſtayed in 198 above its wonted height.

Apr. 30.

Perceiving the Mercury to perſiſt in the ſame height, I a little relaxed or eaſed the Screw, that ſome Air might break forth; and when I ſaw that the Mercury had ſo far deſcended, that it exceeded its accuſtomed height onely 50 digits, I preſently *ſet* the Screw, that ſo I might know whether that remiſſion of the Spring of the Air would afford any place for new Air to be generated; and truly in two or three minutes

time

time I found the Mercury to have ascended sensibly higher.

Three hours after, making an Admeasurement, the Mercury was found 12 digits higher; for it came to 62.

In 5 hours space it ascended 1 digit and $\frac{1}{2}$ and no more.

May 1.

In 15 hours the Mercury gat higher onely one digit.

May 3.

Yesterday the Mercury persisted in the same height, but this day 'twas higher by $1\frac{1}{2}$, and remained in 66.

May 4.

The Mercury was not changed at all, and therefore I suffered all the Air to escape; but somthing hindred, that I could not quickly *set* the Screw, whence it is probable, that very much Air, which at that time was produced, got out of the Receiver; yet neverthelesse, after the Receiver was again straitly stopp'd, I perceived that two digits of Air and more had been produced in 5 or six minutes time.

May 7.

The Mercury in 3 daies, again mounted 2 digits.

May 8.

The Mercury was higher by $\frac{1}{2}$ a digit.

May 11.

Those two last daies the Mercury again ran up half a digit, and not much more. I included this mass, almost unfit, as it seemed, for producing of Air, *in vacuo*; and then in 5 hours space the Mercury ascended to the height of one digit.

May 21.

It did not yet ascend quite 3 digits.

May 30.

The Mercury staid at the height of 4 digits and $\frac{1}{2}$.

By this Experiment it appears, that all the Air producible from Paste, may be in a manner generated in a great Compression; yet it is somewhat restrained by that hindrance, which at length in a lesser Compression will break forth in a short time.

Moreover, we have a confirmation by this Experiment, that Air is producible by repeated turns and operations; alſo, that it is produced more ſlowly in compreſſed than in free Air: For ſuch a Production in free Air is wont to be perfected in two or three daies time.

EXPERIMENT VII.

July 30. 1677.

Artificial Air.

I included Plums and Apricocks, many of them being cut aſunder, in an empty Receiver, and afterwards I immitted as much Air, produced out of Cherries, into the ſame Receiver as was ſufficient to ſuſtain 64 digits of Mercury.

Aug. 1.

Our Fruits had produced no Air, but grew yellow by reaſon of their overmuch Ripeneſs, more than thoſe which were in Common Air. See p. 37.

Aug. 3.

This day I found the Mercury a little higher, and that Apricock which remained whole, ſeemed to be full of ſome drops of Water.

Aug. 7.

The whole Apricock grew more and more ſoft; the Mercury was 59 digits high above its wonted Preſſure.

Aug. 8.	The height of it was	61	*Aug.* 13.	The height of it was	78
9.		65	14.		80
10.		71	15.		80
11.		74	16. and the days following it abode at the ſame height.		

24. The height of it was 77. Though I certainly knew that nothing had iſſued or eſcaped out of the Receiver.

29. Seeing I found that neither the Fruits nor the height of the Mercury were changed any more, I opened the Receiver

Receiver and perceived that the Apricocks had kept their colour very well, but the fleſh of them was ſpongeous, and their taſte ſubacid; many bubbles had broke forth from them, at the time they were freed from the *circumſtant* preſſure.

July 30. 1677.

Common Air.

I included the half parts cut off from the Fruits aforeſaid, in a Receiver full of Common Air; and with them alſo ſome Fruits of the ſame kind uncut.

July 31.

I found the Mercury had attained 8 digits high.

Auguſt 1.

At 6 a Clock in the Evening the Mercury was 21 digits high; in the other Receiver it was not moved.

Auguſt 3.

Our Fruits kept their firmneſs much better than thoſe which were included with Artificial Air. The height of the Mercury was 35 digits.

Auguſt 4.

The height of the Mercury was 42 digits.

Auguſt 6.

Our whole Apricock ſeemed not at all to be altered. The height of the Mercury was 57.

Aug. 7	The height of it was	81
8		95
Aug. 9	The height of it was	113
10		124

The colour of the whole Apricock yeſterday began, and now proceeded to wax yellow. No moiſture appeared.

Aug. 11	The height of it was	131
13		157
14		163
Aug. 15	The height of it was	171
16		171

17 and the days following the ſame height remained.

Aug. 27. The height was 182.

29. When I ſaw that neither the Fruit nor the height of the Mercury were changed any more, I opened the Receiver, and found the Apricocks of a more acid and leſs acceptable taſte, than the others in facti-tious air; yea, their pulp was of a very good colour, but ſpongie: they ſent forth many bubbles, as the others did.

From this Experiment made in two Receivers together, 'tis probably collected, that the artificial Air of the Cherries was a great hindrance to the Apricocks, that they could not produce air; yet notwithſtanding, it doth advance the alteration of their colour and firmneſs; and is alſo good to preſerve their taſte.

EXPERIMENT VIII.

Octob. 10. 1677.

Grapes without ſpirit of Wine.

I ſhut in an ounce and half of Grapes unripe and bruiſed, in a Receiver that would hold 10 ounces of Water; I drew out no Air.

Octob. 11. The Mercury aſcended a little.

12. There was but a ſmall change.

13 The height was ½ a digit.

17 The height was 1 digit.

18 The height 1 ½

19 The height almoſt 4 digits.

20 The height the ſame, but ſome finew or mouldineſs appeared in their ſuperficies.

21 The height was 4½

22, 23, 24 The height remained the ſame, but the mouldineſs or finew encreaſed.

26, 27, 30, *Nov.* 2 The height of it was 5½, 6, 6½, 7½

Nov.

Date	The height of it was	Date	The height of it was
Nov. 6	9	*Nov.* 18	23
8	10	21	26
9	12	*Dec.* 8	36 ½
12	15	12	39
14	17	27	39

*Jan.*6. 1678. The height was 36. The air broke out.

Octob. 10. 1677.

Grapes with ſpirit of Wine.

I made the ſame Experiment in another Receiver, obſerving the ſame circumſtances, ſave that here I mixed 2 drachms of ſpirit of Wine with the Grapes.

Octob. 11. The Mercury was not changed.

12. There was no change.

13. The Mercury was not moved.

Oct. 17. It aſcended a little.

18. The height of it was not yet a quarter of an inch.

19. It was moved but a very little.

Jan. 6.

The Grapes during all the time elapſed, had produced no air.

By this Experiment made in a double Receiver, it appears that ſpirit of Wine doth hinder Fermentation.

EXPERIMENT IX.

Octob. 17. 1677.

I put one Peach into an emptied Receiver, with ſome quantity of ſpirit of Wine, which yet could not touch the Peach, unleſs it were elevated into vapours.

March 27. 1678.

I drew out the Peach, which had kept its colour, onely it had loſt its firmneſs. Though the Receiver was but ſmall, yet it was not filled with air, for when it was opened, the air ſeemed

to

to rush into it: The Peach being softned, was so depressed, that the lower part of it did a little touch the spirit of Wine; it also came to pass, that the superiour part had almost contracted the taste of the spirit of Wine, as well as that which was immerged in it.

EXPERIMENT X.

Octob. 17.

Air with spirit of wine.

I included 5 Peaches in an unexhausted Receiver, and together with them, some spirit of Wine, which could not touch the Peaches, unless it were elevated in form of Vapours.

*Octob.*18. The Mercury ascended not at all.

20. The height of the Mercury was $3\frac{1}{2}$

		digits
21	The height of it was	$5\frac{1}{2}$
22		$7\frac{1}{2}$
23		9
26		$9\frac{1}{2}$
Nov. 2		12
Nov. 6	The height of it was	14
12		16
14	It kept the same height.	
16		
Dec. 8	The height of it was	18
16		$19\frac{1}{2}$
27		$20\frac{1}{2}$

*Jan.*6. 1678. it was 23

March 28.1678. it was $31\frac{1}{2}$

Octob. 17.

Air without spirit of Wine.

I included 5 Peaches in a Receiver full of Common Air, without spirit of Wine.

Octob. 18.

The Mercury ascended not at all

Octob. 20.

The height of the Mercury was 5 digits.

Octob.

		The height of it was (digits)			The height of it was (digits)
Octob.	21	8	*Nov.*	12	20
	22	10		14	20
	23	11		16	21
	26	12	*Decemb.*	8	26
Nov.	2	15		16	26 $\frac{1}{2}$
	6	17 $\frac{1}{2}$		27	28 $\frac{1}{2}$

Jan. 6. 1678. The height was 32

March 28. 1678. The height was 33 $\frac{1}{2}$.

April. 15.

The Liquor in the lower part of the Receiver had broke all out, and the air followed it; so that I took out the Peaches.

By this Experiment we learn, That the very Vapours of spirit of Wine do somewhat hinder fermentation, yet much less than the spirit it self.

EXPERIMENT XI.

April 27. 1678.

Paste with Leaven or Ferment.

I included an ounce and half of Paste, mixed with leaven with common air in a Receiver, able to hold 23 ounces and half of water.

April 28.

The height of the Mercury in the Gage was 2 $\frac{1}{2}$.

April 30.

The height of it was 3 $\frac{1}{4}$.

May 4.

The Mercury was depressed, though no air broke forth, and the Paste was mouldy. The height of it was 2 $\frac{1}{2}$.

		The height of it was			The height of it was
May	6	2 $\frac{3}{4}$	*May*	17	4 $\frac{1}{2}$
	8	3		20	5
	10	3 $\frac{1}{2}$		24	6
	14	4		28	8

		digits			digits
June 2	The height of it was	9	*July* 5	The height of it was	13 ½
6		10	19		15
14		10 ½			

April 27. 1678.

Paſte without Leaven.

I included an ounce and half of Paſte, without Leaven, with common air, in a Receiver capable of holding 23 ounces and an half of Water.

April 29.

Hitherto the Mercury had not aſcended; but this afternoon I found its height to be a quarter of a digit.

April 30.

There was no change.

May 4.

The Mercury aſcended but very ſlowly, and the Paſte was finewed or mouldy.

May 6.

The height of the Mercury was 4 digits.

May 8	The height of it was	5 ½	*May* 24	The height of it was	16
10		7 ½	28		18 ½
14		10 ½	*June* 2		20 ½
17		12 ½	6		21 ½
20		13 ½	14		25

By this Experiment, made in two Receivers at once, it ſeems clear, That Leaven doth rather hinder than help the production of Air, if the Paſte be not made in a place hot enough.

EXPERIMENT XII.

May 23.

Paste with spirit of wine.

I included an ounce and half of Paste, without Leaven, in a Receiver capable of holding 25 ounces of Water, and I poured spirit of wine on the Paste.

May 24. The Mercury was 1 digit high.

May 26. It was almost 2 digits high.

27. It was $2\frac{1}{2}$.

31. There was no change.

June 1	The height of it was	$3\frac{1}{2}$
6		4
10		$4\frac{1}{2}$

July 19 No change.

December 14.

When the height of the Mercury was no more changed, I opened the Receiver, and the Paste affected my Nostrils with a subacid smell.

May 23.

Paste without spirit of wine.

I included one ounce and an half of Paste, without Leaven, in a Receiver capable of holding 25 ounces of Water; but I added no spirit of Wine.

May 24.

There was no ascension of the Mercury.

May 26. It was 3 digits high.

May 27	The height of it was	$4\frac{1}{2}$
28		$5\frac{1}{2}$
29		7
31		$9\frac{1}{2}$
June 2		12

June 6	The height of it was	17
10		22
July 4		30

July 19. The Mercury little exceeded 30 digits. This day I found that the Air had broke out, and therefore I *set* or ſtraitned the Screw.

December 14.

The Mercury came again to the height of 15 digits, but this day I opened the Receiver, and found the Paſte very acid.

From theſe Experiments, made with Paſte, in a four-fold Receiver at one and the ſame time, it ſeems to follow, That ſpirit of Wine doth very much prejudice the production of Air; and the rather if the Paſte be wrought with Ferment; beſides, it is clear, that Paſte *without* Ferment in tract of time, will produce no leſs Air than Paſte *with* Ferment.

EXPERIMENT XIII.

Octob. 11.

I included new Ale in a Receiver, exactly filled by the help of my Pneumatick Engine, that ſo no air might be left: And I included another quantity of the ſame Ale, in another Receiver, wherein ſome room was allowed for the Air.

Octob. 12.

I this day found the Cover of that Receiver in which I had left ſome Air, to be broken, and therefore I transfuſed the ſame Ale into another Receiver, in which there was room large enough left for the Air. In the Receiver exactly full, the Mercury aſcended a little,

October 13.

In the Receiver exactly filled, the height of the Mercury was 12 digits, in the other Receiver 13 digits, though it had been ſhut up a ſhorter time, and a much larger ſpace was left therein, in which the Air newly produced might have been dilated.

October 14.

In the full Receiver the height was 13; in the other Receiver, 18. Towards Evening I found the full Receiver to work

work with greater swiftness, for the height of the Mercury in it, was 22; and in the other 20.

October 15.

In the full Receiver the height of the Mercury was 42 digits; in the other 26. Besides we must mark, that some bubbles of Air, which in the full Receiver had possessed its upper part, now did wholly vanish; and besides the Ale did occupy a long space in the Mercurial Gage, wherein before it was not found.

October 16. In the full Receiver the height was 60 digits.
In the other 30.
18. In the full Receiver the height was 90.
In the other 40.
22. In the full Receiver the height was 90.
In the other 42.
23. In the full Receiver the height was 108.
In the other 50.
26. In the full Receiver the height was 108.
In the other 60.
28. In the full Receiver the height was 133.
In the other 63.

The bubbles which were vanished, appeared again, yet nothing flowed out.

Nov. 8.

The full Receiver had lost much Ale, wherefore I opened it, and thereupon all the Ale seemed as if it would have vanished into Froth, unless I had suddenly shut the little hole, which I had opened: I tried it many times, that if the hole were opened in the Gage, the Mercury presently descended; but if the hole were again shut, it would speedily ascend; as if the compression, being abated, had afforded some facility for the production of Air. The Ale had a most pungent taste.

Nov. 9.

I opened the other Receiver, and observed in a manner the same circumstances. From

From this Experiment it ſeems to follow, That Ale if th Air be wholly excluded from the Veſſel will ferment mor ſlowly than if ſome Air were left with it: yet in tract o time, it makes a greater compreſſion, if no place be left for it dilatation.

EXPERIMENT XIV.

June 27.

Peaſe with ſpirit of wine.

I put green Peaſe into an emptied Receiver, with ſpirit of Wine. Towards the Evening the Receiver ſeemed to admit the external Air, and the Mercury came to the height of 18 digits; and therefore I firmed the Cover with Turpentine.

June 30.

I perceived no more change in the height of the Mercury.

July 7.

No Air was produced, even in the moſt vehement heat.

June 27.

Peaſe without ſpirit of Wine.

I put new Peaſe into an emptied Receiver, without ſpirit of Wine. The Receiver and the quantity of the Peaſe were the ſame, as in the laſt mentioned Experiment.

June 28.

The Receiver was full of Air, for I think it was not exactly ſhut; and therefore I again included the ſame Peaſe. Towards Evening the height of the Mercury was 5 digits.

June 29	The height of it was	10	*July* 5	The height of it was	26
30		16	7		30
July 1		19			

July 8. The Air got out of the Receiver being too much filled.

From

From this Experiment, made in two Receivers at once, it appears, That ſpirit of Wine doth alſo hinder the production of Air in Peaſe.

ARTICLE III.

The Effects of Artificial Air are different from the Effects of Common Air.

EXPERIMENT I.

June 19. 1677.

I Put Cherries into an evacuated Receiver. In 6 hours time the Mercury came to the height of 5 digits and an $\frac{1}{2}$.

June 20.

The aſcenſion of the Mercury was 3 $\frac{1}{2}$.
Towards the Evening it was 2.

N. *The Aſcenſions are always to be underſtood, as added to the former.*

June 21	The aſcenſion was	1 $\frac{1}{2}$
22		1 $\frac{1}{2}$
23		2
24		1 $\frac{1}{2}$
25		1 $\frac{1}{2}$
June 26	The aſcenſion was	3
27		3
28		5
30		1 $\frac{1}{3}$
July 1	The aſcenſion was	3
2		4
3		2
July 4	The aſcenſion was	2 $\frac{1}{2}$
5		3

The height was 48; but I tranſmitted the Air into another Receiver, and the Mercury was depreſſed to the height of 35 digits.

July

July 6. The aſcenſion of the Mercury was 4 digits in one nights ſpace.

7. The aſcenſion of it was 5 $\frac{1}{2}$ in 24 hours ſpace.

8. The aſcenſion of it was 5.

9. The aſcenſion of it was 5.

10. The aſcenſion of it was 6.

11. The aſcenſion of it was 12. in the ſpace of 34 hours.

12. The aſcenſion of it was 7.

13. The aſcenſion of the Mercury was 3. the height about 92 digits; but the Air being tranſmitted into another Receiver, the Mercury ſtaid in the height 50.

14	The aſcen-ſion was	14	16	The aſcen-ſion was	13
15		11	17		5

18. The aſcenſion of the Mercury was 9. the height of it 102.

19. The height of the Mercury was 92. *viz.* becauſe I tranſmitted part of the Air into another Receiver.

20 The aſcenſion of the Mercury was 15.

22. Some Air got out, and the height of the Mercury was 63 $\frac{1}{2}$.

23. The aſcenſion of it was 12 $\frac{1}{2}$.

24. The aſcenſion of the Mercury was 4. the height of it was 79 digits; but the Air being tranſmitted into another Receiver, the height ſtaid at 62.

25	The aſcen-ſion was	8	27	The aſcen-ſion was	4
26		9	28		5

30. The aſcenſion of it was 10. the height was 98. Part of the Air being tranſmitted into another Receiver, the height ſtaid at 64.

31. The aſcenſion was 6.

Aug. 1. The aſcenſion of the Mercury was 9. digits.

2. The aſcenſion of it was 4.

3. I tranſmitted the Air into another Receiver, and the Mercury abode in the height 68.

4. I

Aug. 4. I tranſmitted the Air again into another Receiver, and the Mercury reſted in the height 54.

6. The aſcenſion of the Mercury was 7.

7. The aſcenſion of it was 4.

8. There was no aſcenſion thereof.

9. The aſcenſion thereof was 3 digits.

The Receiver being opened, I found the Cherries of a whitiſh colour, and of very little taſte; but the taſte they had, was not ungrateful: their fleſh or pulp was ſpongie.

From this Experiment it ſeems to follow, that Cherries contain much Air in them, and that they produce it very irregularly.

EXPERIMENT II.

July 13. 1677.

I put Cherries into an empty Receiver, and then I tranſmitted into the ſame Receiver, as much Air produced from other Cherries, as was ſufficient to ſuſtain 50 digits of Mercury.

July 15.

Yeſterday the Mercury had not aſcended at all; but this day it was two digits higher, *viz.* in 22 above its wonted height.

July 16. The height of the Mercury was $23\frac{1}{2}$.

July 17 The height of it was 25.

26. The height of it was 43. Some Air got out.

27. The height of the Mercury was 45. Some more Air made an eſcape.

30. The height of it was 52.

31. The height of it was 61 digits.

Auguſt 1.

The height of the Mercury perſiſts in a manner the ſame, but the Air brake out.

Auguſt 27.

The Air had all broke out for ſome time before; I took out the Cherries, and found them not to have loſt their colour, as they had in the former Experiment; and beſides they had contracted no putrefaction nor mouldineſs, but had a taſte a little more acid than they were wont to have; and being opened, there were many cavities in their pulp, like fermented paſte or dough, but not quite ſo thick.

From this Experiment compared with the former, it may probably be inferred, that in Artificial air, fruits do produce leſs Air, and ſo they keep their colour and their taſte better; for the Cherries in the former Experiment remained included in a Receiver, not much longer than thoſe in *this*.

EXPERIMENT III.

September 10. 1677.

Common Air.

I put 6 ounces of unripe Grapes into a Receiver, capable of containing 25 ounces of Water; and I ſtop'd it firmly by the help of a Screw, with Common Air.

September 11. The Mercury aſcended not at all.

September 12. The Mercury ſtop'd a little below one digit.

Sept. 13	The height of it was	$3\frac{1}{2}$	*Sept.* 18	The height of it was	16
14		7	19		18
15		10	20		20
16		$12\frac{1}{2}$	21		22
17		14	22		$23\frac{1}{2}$

September 23. The height of it was 27. The Grapes were not altered.

September 24. The height was 30.

25. The height was 31. The Grapes now began to be yellow.

Sept.

Sept. 26, 27. The height of it was $32\frac{1}{2}$, 34. | *Sept.* 29, 30. The height of it was 35, 35.

October 1. The height remained at 35.

*Octob.*2. The height was 36. 5, 6. The height stayed at 36. | *Octob.*10 The height was 35. 13 The height of it was $32\frac{1}{2}$. The Air got not forth, but the Cold began to come on and encreaſe.

Novemb. 9. The ſame height remained.

Decemb. 19.

I found the Air almoſt all to have made an eſcape.

Decemb. 20.

I took out the Grapes, and I found that by their Smell and their Taſte, they had contracted ſome mouldineſs, though the ſame was not diſcernable by the eye. Their firmneſs was encreaſed.

Septemb. 10. 1677.

Factitious Air.

I included two ounces of crude Grapes in a Receiver capable of holding 8 ounces of Water; and to the Common Air, I ſuperadded Air produced out of Pears, until the Mercury did ſtay 10 digits above its wonted preſſure.

Septemb. 11.

The Mercury deſcended, its height was 8 digits.

Septemb. 12.

The height of it was 11. the aſcenſion of it was 3.

Sept. 13, 14. The height of it was 16, 20. | *Sept.* 15, 16. The height of it was 23, 24.

*Septemb.*17. The height was 28. the Grapes turned yellow.

Sept. 18, 19, 20, 21. The height of it was 29, 30, 31, 33. | *Sept.* 22, 23. The height of it was 35, 20. Becauſe ſome air had broke out: The Grapes were alſo of a Yellow colour.

Sept. 24. The height of the Mercury was 21 digits.
25. The height was 22.
26. The height almoſt the ſame.
27. The height abode in 22.
29. The height was 27.
30. The height was 28.
Octob. 1 & 2. The height ſtay'd at 28.

Octob. 5, 6	The height of it was	30, 31	*Octob.* 10, 13	The height of it was	$31\frac{1}{2}$, 31

Novemb. 9. The height was 13. Some Air had got out.
December 19. The height of the Mercury was 20 digits.

Decemb. 20.

I took out the Grapes, and their Smell and Taſte were more grateful than of others, and their Firmneſs was rather increaſed than diminiſhed.

By this Experiment, made in two Receivers at once, we learn, That Factitious Air ſeems fit to alter Colour, and to preſerve Taſte; but the Firmneſs might be increaſed here, as it is augmented in Turpentine; *viz.* the Spirits in tract of time being exhaled.

EXPERIMENT IV.

July 18.

I took two pieces of Orange, and by the help of my Screw I ſtopped them in faſt in my Receiver, with Common Air, and then into the ſame Receiver I put Air, produced out of Cherries, as much as was ſufficient to ſuſtain 12 digits of Mercury. At the ſame time I put another piece of the ſame Orange into another Receiver, with common Air alone, and that not compreſſed.

July 20.

The Orange in the common Air began to contract mouldineſs; the other ſeemed not at all to be altered.

July

July 23.

The mouldineſs of the Orange in the common Air increaſed; the other remained ſound.

July 16.

The Orange in the common Air, did not proceed to increaſe its mouldineſs, but ſeemed wholly rotten: the other alſo began to putrifie, but remained free from mouldineſs.

Aug. 1.

Perceiving that the Oranges were no more ſenſibly changed, I opened the Receivers, and though the Air, wherewith I had mingled artificial Air, was ſo compreſſed in its Receiver, that now it could not ſuſtain 26 digits of Mercury above its wonted preſſure, yet the Fruits were far better preſerved in it, than in the other; onely ſomething in the ſuperficies ſeemed to have loſt its juice, but all the inner parts, with the Rind, or Pill, were very well-coloured, well-taſted, and firm: In the other Receiver, the whole Orange ſeemed almoſt rotten, not excepting the Rind. In the *Exper.* X. of *Artic.* IV. the Orange was more corrupted in the compreſſed Air, becauſe as it ſeems, no factitious Air had been mixed with it.

Here alſo it ſeems worthy our obſervation, That the ſame Air, generated from Cherries, is apt to produce different effects, upon Fruits of a different kind; for here it retarded the alteration of colour and firmneſs, which in *Exper.* VII. of *Artic.* II. where I included Air with Apricocks, it accelerated and haſtened.

EXPERIMENT. V.

July 20. 1676.

Factitious Air.

I included a ſmall piece of Beef in an emptied Receiver, and then I put Air, produced from Cherries, into the ſame Receiver, as much as ſufficed to ſuſtain 27 digits of Mercury.

July

July 21, 22, 23, 25. The Mercury persisted almost in the same height, and came not to its wonted pressure.

July 26. This day the Beef had removed the Receiver from its Cover; and because it stunk very much, we threw it away.

July 20. 1676.

Common Air.

I put a piece of Beef into a Receiver full of Common Air, and I carefully stopped and firmed it in, by the help of the Screw.

July 21. The Mercury had not at all ascended in the Gage.
July 22. The height of the Mercury was 1 digit.
23. The height of it was $5\frac{1}{2}$.
25. The height of it was $9\frac{1}{2}$.
26. The height of it was $14\frac{1}{2}$. In the Evening 18.
27. The height of it was $21\frac{1}{2}$. In the Evening 25.
28. The Screw, not being firm enough, suffered the Air to break forth.

By this Experiment, made in 2 Receivers at once, it appears That Air produced from Cherries, is a great hindrance to the production of Air from Flesh.

EXPERIMENT VI.

March 14. 1676.

Common Air.

I put two Onions into a Receiver, full of Common Air, with a Mercurial Gage; and I fastned the stopple with a screw, to see whether Vegetation would increase the quantity of the Air, or diminish it.

March

March 28.

Two days after, the Mercury ſeemed depreſſed $\frac{1}{4}$ of a digit; but afterward it recovered its former height, and 2 digits more; and now the Air brake forth, and the Roots grew longer.

April 28.

About 10 or 12 days ſince I perceived the Roots to be corrupted; and indeed now they were wholly putrified.

May 9.

The Mercury perſiſted in the ſame height, becauſe the Air had broke forth; and therefore I took out the Onions, and found their Roots putrified, but they were not mouldy at all.

March 17. 1676.

Factitious Air.

I included two Onions in an empty Receiver, and afterward put Air, produced from Paſte, into the ſame Receiver.

March 28. My Onions took root, at leaſt as well, as thoſe which I kept in the Common Air.

April 28.

The ends of the Roots began to putrifie, yet they were in far better caſe, than thoſe who are ſurrounded with Common Air. Perhaps the cauſe of this difference is to be fetched from hence, That a greater quantity of Water was included with Artificial Air. The Mercury mounted higher 9 or 10 digits.

May 18.

Hitherto the Onions ſeemed not all to be corrupted, but this day I found one of them to have contracted ſome corruption, which may be called a Syderation or Planet-ſtriking, and differs from a mouldineſs.

From this Experiment, made in 2 Receivers at once, we may gather, That Artificial Air doth not at all hinder Vegetation: It appears alſo thereby, That not onely the ſenſible bigneſs of the body, but alſo the quantity of the Air, is increaſed by Vegetation.

EX-

EXPERIMENT VII.

August 25.

Common Air.

I included 6 ounces of unripe Grapes in a Receiver capable of holding 25 ounces of Water, but I did not exhauſt the Air.

Auguſt 26. The Mercury aſcended a little.
27. The height of the Mercury was 1 digit.
28. The height of it was 1 ¼.
29. The height of it was 1 ¼.

Auguſt 30.

The Mercury ſeemed to have deſcended rather than aſcended. The colour of the Grapes was leſs altered here, than in the Receiver, into which Air produced out of Pears, had been immitted.

Auguſt 31.

The Receiver was broken, and I left the Grapes expoſed to the free Air.

Septemb. 7.

The Grapes being left in the free Air, did ſtill keep their green colour, and were of a taſte grateful enough, though leſs pungent than before.

Auguſt 25.

Factitious Air.

I included 2 ounces of unripe Grapes in a Receiver capable of holding 8 ounces and ½ of Water: and having ſtopped it cloſe with a Screw, I filled it further with Air, which I immitted, produced from Pears, as much as ſufficed to ſuſtain 15 digits of Mercury.

Auguſt 26.

Some Air eſcaped out, and therefore I immitted new Air, pro-

produced out of the ſame Pears, untill the Mercury ſtaid at 17 digits above its wonted preſſure.

Auguſt 27.

The Mercury was depreſſed below the 16 digit; and yet no Air had brake forth. Towards Evening, I found the Mercury had again aſcended to 17.

Aug. 28	The height of it was 19	*Aug.* 31	The height of it was 23 ½
29	21	*Septemb.* 1	24
30	22	2	24

September 4.

The ſame height continued at 24. and the Grapes had all contracted a yellow colour.

Septemb. 5.

The Air broke out.

September 7.

The Air proceeding to get out by degrees, I took out the Grapes, and found them very inſipid, and of an unacceptable taſte.

This Experiment, made in 2 Receivers at once, doth confirm to us the efficacie of Artificial Air, to alter the colour of Fruits. 'Tis alſo very obſervable, That in this Experiment it did prejudice the preſervation of the taſte, and promoted the production of the Air, contrary to what had happened in the former Experiments. It would be worth the while to try, whether the ſame ſucceſs would evene with all unripe Fruits.

EXPERIMENT VIII.

Auguſt 2. 1676.

Factitious Air.

I ſhut up one Gilliflower in a Receiver, with Air produced from Paſte made with Meal, and not mixed.

Auguſt 4.

Our Flower began to change colour and to be moiſt.

August 9.

The Gilliflower was little altered.

August 12.

The moiſture increaſed by little and little, but no mouldineſs appeared.

August 31.

The Gilliflower was little altered, yet it was leſs freſh than thoſe which were kept *in vacuo.*

August 2.

COMMON AIR.

I ſhut up one Gilliflower in a Receiver, with Common Air, not mixed.

August 4.

Our Flower was not changed.

August 9.

The Gilliflower was madid, and had almoſt loſt all its colour.

August 12.

Now a great mouldineſs covered all the Flower.

August. 2.

VACUUM.

I included two Gilliflowers *in Vacuo*; and took ſpecial care, that no humidity ſhould be included with them.

August 4. 1676.

One of the Gilliflowers began to appear madid.

August 31. 1677.

During the whole elapſed Year, the Gilliflowers had ſuffered no mutation.

By this Experiment, inſtituted in 3 Receivers at once, it ſeems probable, That Factitious Air doth render the change of colour more ſpeedy, yet it prevents mouldineſs, even as *Vacuum* doth the ſame. EX-

EXPERIMENT IX.

July 24.

COMMON AIR.

I put Apricocks, and ſome Plums, of which divers were cut in pieces, into a Receiver full of common Air, and ſtopped it firmly with a Screw.

July 25.

The Mercurial Gage was ſpoiled, and therefore I could not by any means perceive the quantity of the Air to be generated.

July 30.

The Fruits ſeemed not at all to be altered, ſaving that one of the diſſected Plums had contracted ſomething of mouldineſs.

Auguſt 2.

I opened the Receiver, and found all the Fruits firm, of a good colour, and of a grateful taſte.

July 24.

ARTIFICIAL AIR.

I made the ſame Experiment in another Receiver, with the ſame circumſtances, ſave onely that into this laſt Receiver I intruded Air, produced from Cherries, as much as was ſufficient to ſuſtain 22 digits of Mercury.

July 25.

I found the Mercury to have deſcended 3 digits, it ſtaid in 19. Toward the Evening it recovered its former height, it ſtaid in 22.

July 26, 27. The height of it was 28, $34\frac{1}{2}$ | *July* 28, 29. The height of it was 36, 40

July 30. The height was 44. The Apricocks which were cut, began to moiſten, and to be diſſolved into water.

July 31. The height was 51.

Aug. 1. The height was 60.

August 2. The height was 65. Towards Evening, when I found ſome liquor had eſcaped out of the Receiver, I ſcrewed it more ſtraitly, but one of the iron Wires being broken, all the Air got out. Wherefore I took out the Fruits, and found them very ſoft, eſpecially thoſe whoſe lower parts were immerged in the Water; for the reſt they were a little more firm; but all of them retained a grateful taſte.

From this Experiment made in 2 Receivers, it ſeems to be inferrable; That Air produced from Cherries, doth promote the alteration both of colour, and alſo of firmneſs in Apricocks.

It appears alſo, That ſome part of ſuch Air is deſtroyed in the beginning.

EXPERIMENT X.

July 30. 1676.

I put Plums, cut aſunder, into 3 Receivers, of which one was full of Artificial Air, produced from Gooſberries; the ſecond was full of Common Air, the third was *Vacuous.*

Auguſt 2.

In the Artificial Air, the Plums were not changed. In the Common Air, they began to be mouldy; but in the *evacuated* Receiver, they retained their colour, but were ſoft.

Auguſt 5.

In the Artificial Air the Plums had contracted a red colour, humidity, and ſoftneſs; In the Common Air, they ſeemed black and mouldy, yet retaining their firmneſs: In the *evacuated* Receiver, they were almoſt melted or diſſolved.

Auguſt 7.

In the Common Air the Plums now began to ſoften.

August 8.

In the Common Air, the Plums seemed to have lost their black colour, and to have contracted a red one; even as it happened 3 days ago to the Plums in the Artificial Air.

In this Experiment, Artificial Air seems to have promoted alteration.

EXPERIMENT XI.

September 24.

I put 5 Peaches into a Receiver, with Common Air mixed with Air produced from Grapes, and I included the Grapes themselves in the same Receiver; that the Common Air might be the better saturated with the Artificial.

September 25. The height of the Mercury was 21 digits.

Sept. 26	The height of it was	23	*Sept.* 29	The height of it was	42
27		31	30		45
28		39	*Octob.* 1		48

Octob. 2. The same height continued.

3. The height of it was 52 ½.

5. The height the same; but the Peaches seemed somewhat madid.

6. The height of it was 58.

7. The height of it was the same.

8. The height of it was 61.

11. The Mercury ascended a little.

19. The height of it was 65.

25. The height of it was 61. The cold was sharp.

27. The Cold abated and the Mercury ascended.

30. The height stay'd at 61. and a little more.

Novemb. 2. The height of the Mercury was 59. 'Twas bitter cold weather.

6. The height of it was 61. The Frost broke and was dissolved.

Nov.

Nov. 7. The Mercury ſeemed ſomewhat higher.
9. The Mercury perſiſted in the ſame height.
Dec. 9. In one Months ſpace the Mercury aſcended by little and little, its height was 80 digits.

April 1. 1678.

The Mercury came to 96 digits above its wonted height, And I opened the Receiver, and whileſt the Air was breaking out, the Peaches did emit many bubbles through their skin, not without violent noiſe, and the skin in ſome of them was broken; They had preſerved their taſte pleaſant enough and the colour of their pulpe was commendable, but they had loſt their firmneſs, as if they had been boiled; being left in the Air for 3 hours ſpace, they were all rotten.

This Experiment proves, That Common Air doth corrupt bodies, yet it doth *ſo* much leſs, if it be mixed with Factitious Air.

EXPERIMENT XII.

Auguſt 4.

The First Receiver.

I cut 5 Pears, each of them into four parts, and I put one part of each into a Receiver full of Common Air, and ſtopped it cloſe with a Screw.

Auguſt 6.

The colour of theſe Fruits was altered little leſs than of others: The Mercury aſcended not at all.

Auguſt 7.

The Pears were little altered, The Mercury was higher by a little.

Auguſt 8.

The Pears underwent no great mutation. The height of the Mercury was 4. digits.

Auguſt 9. The height of it was $4\frac{1}{2}$.

Aug.

Aug. 10	The height	6	*Aug.* 13	The height	16
11	of it was	10	14	of it was	20

The Pears began to be ſoftned.

Aug. 15. The height of it was 21.

16. The height of it was 19. I believe the Air had got out.

17. Now I found the Air had eſcaped out.

18. When the Air had almoſt all got out ſince yeſterday in the Evening, and I ſaw the Fruits to look worſe than before, I took them out, and found them putrified.

Auguſt 4.

THE SECOND RECEIVER.

I took one quarter of each of the aforeſaid Pears, and included it after the ſame manner; and afterwards I immitted Air, produced out of Cherries, till the Mercury poſſeſſed 23 digits above its wonted preſſure.

Auguſt 6.

Thoſe Fruits had altered nothing, but their colour a little.

Auguſt 7.

The Pears, almoſt all, ſeemed rotten. The Mercury perſiſted in the ſame height.

Auguſt 8.

The Pears were not altered much more. Something hindered, that I could not ſee the Mercury.

Auguſt 10.

The Pears wax'd more and more ſoft. Now looking upon the height of the Mercury, it was 40 digits more than its wonted height.

Aug. 11	The height	51	*Aug.* 14	The height	67
13	of it was	61	15	of it was	73

Aug. 16. The Mercury deſcended; yet I know aſſuredly that nothing had got out.

Aug.

August 17.

The Mercury exceeded not 67 digits in height, yet the Air could by no means escape out.

August 18.

The Mercury persisted at the same height, but I suffered the Air to break forth; it affected my Nostrils with a sharp odour: moreover the taste of the Fruits seemed very acid, and their pulpe exceeding soft.

August 4. 1677.

THE THIRD RECEIVER.

I put a quarter of each of the foresaid Pears into a Receiver, not exactly shut.

August 6.

The Pears seemed to change their colour.

August 7.

One of our pieces of Pears began to lose its firmness: but in the Artificial Air another piece of the same Pear did yesterday seem wholly rotten.

August 8.

One piece was mouldy, the rest were soft.

August 9.

The Pears grew more and more rotten.

August 11.

The Pears were wholly mucid and rotten.

This Receiver compared with the first, shews, That Corruption doth not begin in *Free* Air sooner than in *included* Air; but when it is begun, it is much more, yea, and more speedily increased, *viz.* because the included Air might be satiated.

August 4. 1677.

THE FOURTH RECEIVER.

I included one quarter of each of the said Pears *in Vacuo.*

August

August 6. The height of the Mercury was 5.

August 7	The height of it was 8		*August* 13	The height of it was 20
8	10		14	23
9	12		15	25
10	14		17	28
11	16			

20. Hitherto the Pears had undergone no alteration, but this day they began to be soft: The Mercury ascended not.

August 26. Neither the Pears, nor the height of the Mercury were altered at all.

This production of the Air seems very regular.

By this Experiment, made in 4 Receivers at once, we find the aptitude of Artificial Air for the softning of Fruits.

And that the production of Air was here promoted by Artificial Air, is very probable; yet it had succeeded otherwise with Apricocks, *Artic.*II. *Exper.*VII.

EXPERIMENT XIII.

August 21. 1677.

THE FIRST RECEIVER.

I divided 6 Apricocks, each into 4 parts, and I put one piece of each into a Receiver full of Common Air, and stopped it firmly with a Screw.

*Aug.*22.

The Apricocks seemed riper this day than yesterday; but no Air was produced by them.

August 23.

One piece, contiguous to the Water, began to be mouldy, the rest inclined to putrifaction: the Mercury seemed to have ascended a little.

Aug. 24.

A piece next the Water, was covered with a great deal of mouldineſs, another piece, more remote from the Water, was ſomewhat mouldy alſo; but all were rotten.

Aug. 25.

The Fruits contracted no more mouldineſs; but the putrifaction more and more increaſed. The height of the Mercury was 7 digits.

Aug. 26. The height of the Mercury was 15. digits.

28. The height of it was 30.

29. The ſame height continued.

30. The height of it was 33. The Fruits were almoſt all diſſolved.

31. The height of it was 38.

Septemb. 1. The height of the Mercury was the ſame.

2. The ſame height ſtill.

3. The Mercury aſcended a little.

Septemb. 4	The height of it was 41	*Sept.* 7	The height of it was 45
5	43	8	46

Septemb. 9. The ſame height continued.

Sept. 22. Little or no change was made in the height of the Mercury; but the Fruits were almoſt melted into water.

Octob. 1.

When the Mercury continued in the ſame height, and the Fruits were almoſt all vaniſhed, I opened the Receiver, and found the Apricocks very much impaired, and ſoft, yet they had retained a taſte, not ungrateful, but ſubacid.

Auguſt 21. 1677.

THE SECOND RECEIVER.

I covered one quarter of each of the foreſaid Fruits, the Receiver not being fortified againſt external Air.

Aug. 22.

The Apricocks were *flaccid* or quailed, as if they had been dry or withered.

*Aug.*23.

Many of our Fruits appeared rotten and mouldy.

Aug. 24.

The Apricocks were wholly infected with putrefaction and mouldinefs.

August 21.

THE THIRD RECEIVER.

I included firmly by the help of a Screw, one quarter of each of the forefaid Fruits, in an unexhaufted Receiver; to which I after added Air produced from Pears, as much as fufficed to fuftain 20 digits of Mercury.

Aug. 22.

The Mercury afcended not at all; but the Fruits feemed to have acquired a greater degree of maturity than thofe which are included in Common Air.

Aug. 23.

Thefe Fruits feemed lefs altered than they which were in Common Air.

Aug. 24.

The Fruits were not altered.

Aug. 25.

The Fruits did begin to produce Air, but I could not difcern the quantity.

Aug. 26.

Little alteration in the Fruits.

Aug. 28.

The Apricocks began to moiften, yet they were far lefs altered than thofe which remain in Common Air.

Aug. 30.

The Mercury did this day emerge above the bodies by which it was hid. Its height above the wonted preffure, was 30 digits.

Aug. 31. The height of the Mercury was 40 digits.
Sept. 1. The height of it was the same.
2. The same height continues.
3. The height thereof 45.
8. The height was little changed.
9. The height was 40. and yet no Air got out.
11. The height was 38.
12. The Mercury continued to descend.
13. The height of it was 33.
Sept. 14. The Mercury was so depressed, that it appeared no more.
Sept. 22. The Mercury did emerge again, its height was 33. The Fruits were covered with a kind of *mucor* or Finew.

Octob. 1.

When the height of the Mercury, nor the Apricocks, were any more altered, and the Finew vanished away, I opened the Receiver, and found the Apricocks not impaired, but of a colour laudable enough, but their pulp was spongy and soft, and of a subacid taste.

August 21.

THE FOURTH RECEIVER.

I took a quarter of each of the aforesaid Fruits, and shut them up firmly with a Screw in an unexhausted Receiver, into which afterwards I intruded Air, till the Mercury came to 90 digits above its accustomed pressure.

Aug. 22.

Our Receiver broke into an hundred pieces by the force of the Air compressed within it: whereupon I put the Fruits into another Receiver, and added onely such a quantity of Air as was able to sustain 60 digits of Mercury.

Aug. 25.

The Apricocks had contracted no mouldiness, I added new Air.

August 26.

The Apricocks were wholly infected with mouldineſs, and rottenneſs,

This Receiver, if compared with the former, doth ſhew, That the quantity of corruption, doth depend on the quantity of the Air.

By this Experiment made in 4 Receivers at once, we have a confirmation, That in Factitious Air alteration is made quicker; but in tract of time, the corruption is far greater in Common Air.

ARTICLE IV.

The Effects of Compreſſed Air, are different from the Effects of Common Air.

EXPERIMENT I.

March 21. 1677.

I Put 2 Onions into a Receiver, which was to be ſtopped cloſe with a Screw, and I intruded ſo much Common Air thereinto, that raiſed the Mercury 60 digits above its wonted preſſure.

March 28.

My Onions took root as well as other Onions which I had included in Common Air at the ſame time.

April 28.

The Onions included in Common Air 8 days ago, were covered with mouldineſs, though in the beginning they had put forth roots numerous enough: The Onions in the other Receiver began to contract corruption at the ends of their roots, but the compreſſed Air 10 days before had found a gradual

paſſage

passage out, and now was almost all escaped. And therefore I put in new Air, till the Mercury had attained to the height of 60 digits above its accustomed pressure.

April 29.

The Onions in the compressed Air, were all over covered with mouldiness.

From this Experiment it seems to follow, That a little compression doth not prejudice those bodies which are to be expanded by vegetation.

Moreover the new Air, which was intruded, seems to have promoted the mouldiness, though in the beginning it is probable that the compression of the Air did retard both the mouldiness, and also the corruption.

EXPERIMENT. II.

May 9.

I put 2 equal quantities of Tulips and Lark-spurs into 2 Receivers of an equal bigness, and stopped them up firmly with Screws: I left one of them with Common Air onely, but I compressed the other with the intrusion of new Air, till the Mercury did exceed its wonted height by 70 digits.

May 11.

Two Tulips in the Common Air contracted mouldiness, but all things remained unaltered in the compressed Air.

May 12.

A third Tulip, in the Common Air, began to be finewed; but there was no such thing in the compressed Air.

May 14.

This day I perceived one Tulip in the compressed Air to be infected with some *mucor* or finew, but those which remained in the Common Air, were all very mucid, and also one of the Lark-spurs in the Common Air, had contracted a *mucor*.

May

May 17.

Three of the Tulips in the compreſſed Air had indeed contracted a Finew, but not half ſo much as Tulips in the Common Air were covered with. And moreover 2 of the Lark-ſpurs in the Common Air appeared finewed alſo; but thoſe ſhut up in compreſſed Air, were preſerved freſh, and wholly free from mouldineſs or finew.

May 21.

The Flowers in the Common Air were all rotten and putrified; but the other in the Compreſſed Air, received no further alteration: and beſides, the Tulips, which had contracted ſome finew, ſeemed rather to loſe *that*, than to acquire *new*.

May 30.

When the Flowers in the common Air, being wholly putrid, were diſſolved into water, I took them out, and kept the liquor in the Veſſel to try whether any Inſects would breed therein. In the compreſſed Air the Flowers ſuffered no more ſenſible alteration; and therefore I took them out, and found them madid, and infected with a ſubacid odour.

By this Experiment it ſeems plain, That compreſſed Air doth hinder putrefaction and mouldineſs in ſome plants.

EXPERIMENT III.

May 21. 1677.

I cut an Orange into two equal parts, and one of the halfs I ſtopped up in a Receiver with Air ſo compreſſed, that it would ſuſtain 100 digits of Mercury above its wonted preſſure; but I left the other half in another Receiver, well ſhut, onely with common Air.

May 25.

Each half of the Orange had contracted mouldineſs, but that which was in the common Air was much more mucid than the other.

May

May 26.

This day I perceived that the compreſſed Air had almoſt all got out, and therefore I put in new.

May 30.

Every day I perceived ſome Air had got forth, and therefore I made a dayly ſupply by adding new. And it came to paſs that the Orange by receiving new air, ſo often admitted, had contracted a *mucor* notwithſtanding the compreſſion much more than the other piece of Orange that was always left in the ſame air without preſſure.

June 1.

I took out the two half Oranges, and that which remained in the compreſſed air, ſeemed to have contracted a corruption at leaſt three times greater than that which had continued in the common air.

By this Experiment, The aptitude of compreſſed air, to retard corruption, is confirmed; yet in progreſs of time 'tis very probable, that the quantity of corruption doth depend upon the quantity of the air. See *Exper.* 1.

EXPERIMENT IV.

May 31. 1677.

I included two equal quantities of Roſes in 2 Receivers, which I ſtopped by the help of Screws, into one of which I intruded as much air as would ſuffice to ſuſtain 90 digits of Mercury, beſides its accuſtomed preſſure; but I left the other onely with common air.

June 11.

The Roſes in the common air were free from mouldineſs, onely they ſeemed to have loſt ſomething of their colour; but thoſe which were ſhut up in the compreſſed air had almoſt all contracted a yellow colour, as if they had withered in the *open* air, and yet they were not mucid or finewed.

June

June 18.

This laſt Week the Flowers in the common air admitted not the leaſt change; but thoſe in the compreſſed air grew more and more yellow. I opened both Receivers, and found the Roſes to have kept their ſmell, yet it was ſomewhat altered, neither of them were dry nor withered: I kept them apart in the open air, and found that the Roſes, taken out from the compreſſed air, were not ſo ſoon altered by the contact of new air, as thoſe which had remained in the air not compreſſed.

From this Experiment it ſeems to follow, That compreſſed air is ſometimes fitter for the alteration of colour than common air. And perhaps it may not be unworthy of our notice, that Roſes ſo included, contract not a mouldineſs, but onely a yellow colour; but in Tulips and Larkſpurs the matter ſucceeded otherwiſe. See *Exper.* II.

EXPERIMENT V.

June 1. 1677.

I put the 2 halfs of the ſame Orange in 2 Receivers; In the one I increaſed the quantity of air till it ſuſtained the Mercury 100 digits above its wonted height; but I left the other uncompreſſed, onely exactly ſhut.

June 6.

Each half of the Orange was infected with mouldineſs, eſpecially that, whoſe ambient air was compreſſed. But note that new air was every day to be ſupplied thereunto; for the compreſſed air in 24 hours ſpace had almoſt all got out. But in *Exper.* III. it had remained very well ſhut in for 6 whole days.

June 11.

The Orange in the common air contracted no more mouldineſs; but in the compreſſed air, the *mucor* or mouldineſs was more and more increaſed.

June 18.

Finding the mouldineſs of the Orange in the common air to be leſſened rather than increaſed, I took it out; and perceiving further, That in compreſſed air the Orange was not more mucid, after I had ceaſed to intrude new air; I was willing to trie, whether the new air did ſuppeditate new ſtrength to the Orange to exert and thruſt out its mouldineſs; therefore I made the Mercury in the Gage, by reaſon of the air I intruded, to exceed its wonted height 80 digits.

June 20.

Two days after I had intruded new air into the Receiver, the mouldineſs of the Orange appeared to be manifeſtly augmented.

From this Experiment we may gather, That the quantity of the mouldineſs doth depend on the quantity of the air.

EXPERIMENT VI.

June 17. 1677.

I put 2 Shrew-Mice into 2 Receivers, of equal bigneſs, and ſtopped them up carefully; In one of them I left onely common air; into the other, I intruded air, till the Mercury was higher than its wonted preſſure 30 digits: But the Mouſe in the common air was included about 5 and 52′, 6′ after the other.

The Mouſe in the compreſſed air ſeemed to loſe his ſtrength much ſooner than the other, the motion of his breaſt being leſs frequent. Yet notwithſtanding about 6 and 18′, the Mouſe in the common air, which ſeemed the ſtronger, fell into convulſive fits and died; but the Mouſe in the compreſſed air, ſeemed then, and ſome time after, to be as well, as it was an hour and half before.

About 11 of the Clock, the mouſe in the compreſſed air did as yet breath; but about 4 in the morning he was found dead in in the ſame poſture, wherein he was 7 hours before; whence we may conjecture, that he was free from convulſive fits.

I muſt not here omit to relate, that the Mouſe in the common air had conſumed ſomething of that air, ſo that the Mercury ſtood at 29 digits, which, when the Receiver was opened, preſently aſcended to 30.

From this Experiment we learn, That compreſſed air ſeems fitter than common air, for the prolongation of Life, ſeeing the one Mouſe lived 24′ and no more, but the other lived about 15 turns longer, though onely a double quantity of Air was included in his Receiver.

EXPERIMENT VII.

June 13. 1677.

I put 4 Flies into a Receiver, into which I afterwards intruded air, till the Mercury did occupy 60 digits above its wonted height; and at the ſame time I included 3 other Flies in another Receiver, with common air not compreſſed.

July 14.

This day in the morning all the Flies were well. In the afternoon I found 2 of them dead in the compreſſed air, but in the common air they were all alive. About 5 of the clock one of the Flies in the compreſſed air was alive and three in the common air.

June 15.

This morning I found all the Flies in the common air dead; but that ſingle one which remained alive in the compreſſed air, ſeemed ſtill to be very well, and being taken out of the Receiver, flew ſpeedily away.

From this Experiment it ſeems to follow, That Flies are not very ſenſible of the compreſſion of the air; and that they die more for hunger than for default of air: for the Flie which was ſo long well, fed upon the carcaſſes of thoſe which were

 dead,

dead, ſo that ſhe ſeemed to be affected with no diſtemper. Yet I iterated the Experiment. See *Exper.* VIII.

EXPERIMENT VIII.

June 15.

I repeated the former Experiment, onely including 4 Flies in each Receiver, and compreſſing the air ſomewhat more.

June 16.

This morning I found 2 of the Flies in the common air dead, and but one in the compreſſed air.

About 2 in the afternoon the 4 Flies in the common air ſeemed to be dead, but in the compreſſed air, the 3 were alive.

June 17.

All the Flies died, except one in the compreſſed air.

From this, and the former Experiment, a man may conjecture, That the compreſſion of the air is of ſmall conſequence to Flies; and indeed they are not prejudiced by the rarefaction of the air, but with great difficulty, unleſs there be almoſt a compleat *vacuum.*

EXPERIMENT IX.

June 18.

I included 2 Frogs in 2 Receivers, and ſtopped them by the help of Screws; the one onely with common air, the other with air compreſſed to ſuſtain 70 digits of Mercury.

June 19.

Both the Frogs were alive; and the height of the Mercury in both Receivers remained the ſame.

June 20.

Neither of the Frogs were dead, and they ſeemed to me rather to diminiſh than increaſe the air, but the difference was ſo ſmall, that I dare not be poſitive therein.

June

June 21.

In the morning both the Frogs were alive; but towards evening the Frog in the common air was found dead.

June 22.

At evening the Frog in the compressed air was alive.

June 23.

In the morning I found the Frog dead.

It must be found out by iterated Experiments, whether the greater length of life was to be ascribed to the compression of the air, or to the disposition of the Frogs.

EXPERIMENT X.

June 18. 1677.

I shut 2 half parts of the same Orange in 2 Receivers, and stopped them by the help of Screws; the one with common air, the other with air compressed to sustain 90 digits of Mercury.

June 22.

This morning I found the Orange in the common air, to be infected with mouldiness, but the other was sound.

At 3 of the clock in the afternoon, the Orange in the compressed air seemed also to have contracted some *mucor*.

June 23.

I found the Orange in the common air far more mucid than the other.

June 24.

The Orange in the common air did not increase his mouldiness, but the other was covered all over with it.

June 28.

The mouldiness produced in the common air was now wholly vanished; In the other Receiver, I saw no further alteration in the Fruit.

June 30

June 30.

Perceiving that the Fruits persisted in the same state, I took them out. The half Orange, which was kept in common air, seemed half rotten; but the other besides its finew, appeared wholly putrified.

By this Experiment we have a confirmation, That the quantity of the mouldiness or finew doth depend on the quantity of the air.

It seems also worthy of observation, That the mouldiness, or hoariness did appear a little later in the *compressed* air than in the *common*, though afterwards it increased much more.

EXPERIMENT XI.

June 29. 1677.

I included Roses in 2 Receivers, stop'd by the help of Screws; I left one with common air onely, but I filled the other with so much air intruded by force, that the Mercury ascended to 90 digits above its wonted pressure.

July 14.

Four or five days ago I found the Roses in the compressed air to wither and to degenerate into a yellow colour. There was not the least alteration in the other Receiver.

July 17.

When I perceived that this present Experiment proceeded after the same manner, as That mentioned *p.* 72. I took out the Roses. Those kept in the compressed air, were very much corrupted, and of a very ungrateful smell; but the others were little altered; and their smell not unpleasant.

Hence we have a further confirmation, That the quantity of corruption doth depend on the quantity of the air.

EX-

EXPERIMENT XII.

July 4.

I cut a Limon asunder, and put both halfs into two Receivers, to be stopped by the help of Screws: The one I left with common air onely, but the other I filled with so much compressed air, that it sustained 90 digits of Mercury above its wonted pressure.

July 7.

This day both parts of the Limon seemed to grow mouldy at the same time.

July 17.

The part of the Limon in the compressed air, had contracted much more of hoar or finew, than the other: And perceiving no further alteration in them, I took them out, and found the Limon in the compressed air far more putrid than the other.

By this Experiment, it is confirmed, That the quantity of corruption doth depend on the quantity of the air.

It seems also, That a triple compression of the air, in respect of a Limon, is too weak sensibly to retard the production of mouldiness or finew.

EXPERIMENT XIII.

July 18. 1677.

I included 2 parcels of Gilliflowers, equal in number, in 2 equal Receivers, and stopped them close with Screws. I filled the one with compressed air, till it sustained 100 digits of Mercury above the wonted pressure; but the other was left with common air alone.

July 23.

In the compressed air, the Gilliflowers were bedew'd with some hoariness or mould; the others appeared onely moist: but

But the Mercury exceeded its wonted height onely 70 digits, becauſe ſome of the air had got forth.

July 25.

In the compreſſed air, the Gilliflowers proceeded to be much more corrupted than the others: They had wholly loſt their colour.

July 26.

In the compreſſed air, the Gilliflowers were wholly putrified, and covered with an hoary finew; the others were moiſt onely in ſome places.

Auguſt 1.

Perceiving no farther alteration in the Gilliflowers, I took them out of their Receivers; thoſe which were kept in compreſſed air were rotten, and did ſtinke; but the other kept their colour, and their ſmell was not offenſive, but they were moiſt.

This Experiment confirms, That the quantity of the air doth increaſe corruption.

We may alſo obſerve, That the mouldineſs or hoarineſs is not produced, but in compreſſed air; neither is it probable that this happened by chance, ſeeing in each Receiver there were 4 Gilliflowers included, or three at leaſt.

EXPERIMENT XIV.

July 21. 1677.

I included a Shrew-Mouſe in a Recipient, with common air, and ſhut it in firmly with a Screw, to trie whether he would produce or conſume air.

After 2 hours the Mouſe died, and ſome air was conſumed, but a leſs quantity than in the Experiment mentioned *p.* 74.

July 24.

Hitherto I found no change in the height of the Mercury.

Towards evening it ſeemed a little higher.

July 25.

This day in the morning much air was produced *de novo.*

July 26.

The quantity of the produced air increaſed more and more.

By this Experiment we have a confirmation, That *living* Animals do conſume air, but *dead* ones produce new.

EXPERIMENT XV.

Auguſt 31.

COMPRESSED AIR.

I put Pears into a Receiver, whereto, after it was well ſtopped, I added as much Air, as ſufficed to ſuſtain 30 digits of Mercury above the wonted preſſure.

September 1.

The Mercury was depreſſed, as it happened fol. 37.

Sept. 2.

The height of the Mercury decreaſed: it exceeded not 25 digits.

Sept. 3.

This day the Mercury proceeded one digit higher; it ſtaid in 26.

Sept. 4.

The height thereof was 28.

Sept. 8.

Becauſe the Receiver did afford ſome efflux to the air, I therefore put in new: And this day, opening the Receiver, to compare the taſte of theſe Fruits with the taſte of the others, I found that 5 of the Pears had loſt their firmneſs, but 2 had retained it.

August 31.

COMMON AIR.

I included Pears of the ſame kind in another Receiver, with common air onely, not compreſſed.

September 1.

The Mercury was a little depreſſed, as if it had been in compreſſed air: The cauſe whereof I judge attributable onely to the Cold.

*Sept.*2. The Mercury was not changed.

Sept. 3.

The height of the Mercury was one digit above the wonted preſſure.

Sept.	The height of it was	Sept.	The height of it was
4	4	6	$6\frac{1}{2}$
5	$6\frac{3}{4}$	7	12

September 8.

The height of the Mercury was 20. The Pears being taken out of the Receiver, had preſerved their taſte much better than thoſe which were included *in vacuo*. They alſo retained their firmneſs.

Auguſt. 31.

VACUUM.

I included Pears of the ſame ſort *in vacuo*, but ſome external air brake in, and the height of the Mercury was 1 digit.

Sept.	The height of it was	Sept.	The height of it was
1	4	5	19
2	3	6	23
3	12	7	27
4	16	8	30

The Pears, being taken out, had kept their firmneſs, but had loſt much of their taſte.

From this Experiment, made in 3 Receivers at once, it ſeems to follow, That in a greater compreſſion, a leſs quantity of air is produced.

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EXPERIMENT XVI.

December 7.

I ſhut up a ſmall Bird in a Receiver, capable of holding 20 ounces of Water. The Bird began to be ill, before I had *ſet* the Screw; but, after I had intruded ſo much air, as could ſuſtain 30 digits of Mercury above its wonted height, ſhe ſeemed to recover again; but in ſome ſpace of time after, ſhe began again to be ſick, and therefore I intruded air the ſecond time, till the Mercury ſtaid in 45 digits above its wonted height, and then the Bird was again reſtored to health, but a little time after ſhe began to gaſp again; then opening the Receiver, after ſhe had ſtaid in it 28 minutes, ſhe got out, and was very well.

EXPERIMENT XVII.

January 20. 1678.

I put a Shrew-Mouſe into the Receiver of my Wind-Gun, whoſe elliptick aperture was ſcituate in its upper part, the Figure of it is ſet down *p.*16,17. Then as quick as I could, I ſo far condenſed the air there, till it was reduced to the twentieth part of its ſpace, or thereabouts; and then I preſently diſcharged that Air, and the elliptick hole being opened, I ſuſpected that the Mouſe had been onely a little convulſive; but when he was taken out, there were no ſigns of life in him. And therefore 'tis left to enquiry, Whether the cauſe of his death were to be aſcribed to the Narrowneſs of the Receiver, or to the Compreſſion of the Air?

Wherefore I put another Mouſe into the ſame Receiver, and the air being reduced to a third or fourth part of its ſpace, I opened the Receiver, but not ſo carefully as I had done in the former Experiment; yet the Mouſe, taken out therefrom, was found to be very well.

I afterward repeated the ſame Experiment, the air being about 7 or 8 times condenſed, and the Mouſe ſeemed to ſuffer no inconvenience thereby.

I tried the ſame Experiment again, in Air compreſſed 7 times, and left the Mouſe included for 24 minutes, which time being elapſed, I diſcharged the Air, and the hole being opened, I perceived the Mouſe to fetch many deep groans, as it were; yet, being taken out, he could not recover his health again.

By theſe Experiments it is manifeſt, That a great compreſſion of Air is noxious, yea mortiferous to Animals.

EXPERIMENT XVIII.

January 28. 1678.

I put a Shrew-Mouſe into a Glaſs, to whoſe neck I tied a bladder ſtopping the orifice. Theſe things being thus prepared, I put them into a Receiver for the compreſſing of the Air. A little time after, when the Mouſe began to be ſick, I compreſſed the Air, and the bladder was ſtraitned, and ſo the Mouſe was found in compreſſed Air, though no new Air could penetrate to him: Then he ſeemed to be much better, and his heart did not pant ſo often; and opening the Receiver, in a ſhort time, he was as well as ever.

I iterated the ſame Experiment, and the Mouſe was left there ſo long, that he could hardly breath, whileſt I began to compreſs the Air; and the compreſſion ſeemed again to abate his reſpiration; the Receiver, being opened, and ſo the Mouſe expoſed to the Air, could not breath much more freely; but if I blew the Air on him by Bellows, he ſeemed to be ſomething relieved; but being again committed to the compreſſed Air, he breathed leſs frequently, and at laſt died.

March 25.

Becauſe in the former Experiment it was not clearly manifeſt, whether the Air did enter through the ligature of the bladder,

der,

der, I used the Instrument described *p.* 15. And when I perceived that the Mouse was sick, and breathed seldom, I intruded Water into the Receiver, so that the Air was reduced to the half of its space, and then the Mouse breathed more rarely; but if, extracting the Water, I left the whole space entire for the Air, his respiration seemed more vivid, and the Air being thus many times contracted and dilated, the sick Mouse seemed to me to breath more lively in the common Air, than in the compressed. Whence I conjectured, That the Air is to Animals, like Food, the quantity whereof ought to bear some proportion with their strength: and that I might more certainly know it, I put the same Mouse into my pneumatick Engine, and rarified the Air, so that it possessed more than double the space it was wont; whilest the Air was raresying, presently the Mouse began to be better; yet a little while after he seemed to be sick, and when the Air was restored, it brought no sensible commodity or inconvenience to the Mouse. I thus repeated the rarefaction three times, and the same success followed; but at last the Mouse died.

ARTICLE V.

The Effects of Artificial Air upon Animals.

EXPERIMENT I.

May 5. 1677.

I Put a Bee, with Vinegar distilled, and pulverized Coral, into an emptied Recipient, and the Air being wholly exhausted, I ordered the matter so, that the Coral fell down into the Glass of Vinegar: But the Air, produced from thence, did not restore

reſtore any power of motion to the Bee; but when ſhe was expoſed to the open Air, in a little time after ſhe began to move her ſelf.

Hence a ſuſpicion doth ariſe, That Artificial Air is unfit for the life of Animals.

EXPERIMENT. II.

Auguſt 12. 1676.

I put 2 Flies into a Receiver, and exhauſting the Common Air, I ſubſtituted Air, produced from Gooſberries, in its place, as much as could ſuſtain 26 digits of Mercury.

Afterwards I put 2 other Flies alſo *in vacuo*; but with this difference, that I reſtored common Air to theſe latter Flies, onely in that quantity, as could ſuſtain 23 digits of Mercury.

Within a quarter of an hour; theſe latter Flies, upon the reſtitution of the Air, recovered that power of motion which they had loſt *in vacuo*, and did flie in the rarefied Air; but the former lay without any motion, though they had received a greater quantity of Air.

Auguſt 13.

The Flies in the artificial Air, ſeemed ſtill dead; but the others were luſty.

The Flies taken out of the artificial Air, and expoſed to the common air, remained ſo all this whole day, and yet did not recover any life.

Auguſt. 18.

I renewed the ſame Experiment, with the ſame ſucceſs, though I had reſtored a greater quantity of artificial air.

Hence we have an high confirmation, That artificial air is noxious to the life of Animals.

EXPERIMENT III.

June 22. 1677.

I put Paſte into 3 Receivers, out of which I afterwards exhauſted the Air.

June 23.

When my 3 Receivers did this day regurgitate with Air produced from the Paſte, I kindled a perfumed Cone, and thus kindled, I put it into one of my Receivers, which being preſently ſtopped, the Fire, within one minute of time, went out. Then by blowing, I expelled the artificial Air from the Receiver, and put in fire to it, as before; and then it burned bright for a pretty long time, though I had ſhut the Receiver as ſpeedily, and as accurately as before.

I tried another Experiment, after the ſame manner, with a Fly, and in the artificial Air ſhe was preſently dead as it were, but afterward, being expoſed to the Sun, ſhe in a ſhort time grew well again. Then I blowed in common Air into the Receiver, which being done, the Fly included as before, ſuffered no inconvenience thereby.

I iterated the ſelf-ſame Experiment with the ſame Fly in our third Receiver, being filled with Artificial Air, and the ſame ſucceſs followed, ſave onely that this Fly, when it was taken out from the artificial Air, could not be reſtored to health, but in a longer time, *viz.* becauſe ſhe was left there longer.

By theſe Experiments it appears, That factitious Air is prejudicial to Fire, as well as to the life of Animals.

EXPERIMENT IV.

June 25. 1677.

I put Paſte into 4 Receivers, and exhauſting the Air wholly

from

from two of them, I pump'd out onely half the Air from the other two.

June 26.

I found the 2 Receivers which I had left half full with common Air, to be quite filled with Air newly produced; neither dare I aſſert, whether they had for ſome time regurgitated or no, ſo that the quantity of common Air was much diminiſhed. However the matter was, I put 2 Flies at once into one of the Receivers, after the manner before deſcribed; and they, as ſoon as they touched the bottom of the Receiver, in a very little while after remained without motion. I put a third Fly into the Receiver, after the ſame manner, and found ſhe lived a little longer there than the former. A fourth Fly, being thruſt in, maintained her life longeſt of all, yet at laſt, ſuffering ſome convulſion, ſhe lay unmoved and reſupine. All the Flies, after ſome ſtay in the artificial Air, being taken out from thence, and expoſed to the common, grew well in a ſhort time.

I made the ſame Experiments in another Receiver half full of artificial Air, and in a manner with the ſame ſucceſs; but the Flies, in that Receiver, to which onely common Air was blown in, recovered the power of motion and their ſtrength in a ſhort time.

June 27.

I found one of the Receivers, which was wholly evacuated of common Air, to be full of artificial Air; but it being caſually thrown down upon the ground, ingreſs was thereby afforded to the external Air: yet I put a Frog into it, which ſeemed not to be very ſick therein.

June 30.

My fourth Receiver, by the power of the produced Air, ſeemed at length forced away from his Cover. I put a Frog into it, in manner aforeſaid, and ſhe fell into high Convulſions for five minutes ſpace, and then lay without motion. After four minutes were elapſed, I opened the Receiver, and taking

out

out the Frog, for 46 minutes ſhe remained without motion; but afterwards in four or five minutes more ſhe grew very well.

By theſe Experiments, it is evident, That artificial Air is very hurtful to the life of Animals; but if it be mixed with common Air, it doth not ſo readily produce its effects.

EXPERIMENT V.

June 28. 1677.

I put Paſte into 4 Receivers, 3 of which I cauſed to be wholly exhauſted of common Air, but the fourth was left half full of Air.

June 29.

One of the Receivers which were wholly exhauſted, was found full of Air newly produced; and a Frog being put into it for 4 or 5 minutes, had ſtrong Convulſive fits; then for one minute it lay ſtill without motion, whereupon I took the Frog out, and in 5 minutes ſhe began to move, and a while after became well again.

I took another Receiver, filled with artificial Air, and putting a Frog into it, 7 minutes were elapſed before ſhe ceaſed to be convulſive. And afterward, when ſhe had lain 1 minute there without motion, I opened the Receiver, and taking out the Frog, found that ſhe began to ſtruggle and move, yet I judged thoſe motions to be the relicks of her Convulſions; for after that ſhe remained unmoved for a whole half hour and more; yet at laſt ſhe grew well again.

As for that Receiver, from which I had exhauſted onely half of the Air, it had ſo long regurgitated with produced Air, that it is very credible, much common Air had got out together with it. A Frog being caſt into it, ſeemed to be vehemently moved, and convulſive for 10 minutes, as the reſt did, and then ſhe ſeemed quite dead; but after a full minute was elapſed, I opened the Receiver, and the Frog, being exposed to the open Air, within a quarter of an hour began to recover motion again.

I put a Frog into a Recipient, full of common Air, to trie, whether, the Paste being now taken out, the Frog would continue her life any longer time there?

July 1.

In the afternoon, I found the Frog dead, in the morning she was alive and breathed, so that she lived about 48 hours.

June 30.

I cast a Frog into my fourth Receiver, which was wholly filled with artificial Air; for 7 minutes and an half she was vehemently convulsive, and at last died; then after 2 minutes, she was taken out of the Recipient, and yet recovered no motion at all.

July 1.

Perceiving the Frog to remain in the same posture, I threw her away.

We have a confirmation by these Experiments, That artificial Air is so much the more hurtful to Animals, by how much the freer it is from common Air.

EXPERIMENT VI.

June 30.

I included Paste in two Receivers, and then I exhausted the Air.

July 4.

I would have put a Shrew-Mouse, being taken by the tail, into one of my Receivers, filled with artificial Air, but the little Vermine, with his fore-feet, did so catch at the edges of the Receiver, that he could not then be thrust into it; and by this means the Receiver, being for a while open, afforded ingress to the external Air; yet I shut it again, till I had bound the legs

of

of the Mouse, and then he was easily put in, and there suffered vehement Convulsions, and after the elapse of one minute, died, I presently took him out, and exposed him to the common Air; but his life being wholly gone, no power of motion could be recovered.

Then I took the other Receiver, and putting a Snail into it, did with some wonder observe, that he continued to be moved very strongly for a whole quarter of an hour; but afterwards his motion was slower, untill about another quarter of an hour being elapsed, he lay still, as if he were dead; but then being taken out of the Receiver, and exposed to the Air, in a short time he grew well.

I put Flies into the same Receiver; but now it had admitted too great a quantity of external Air, for the Flies suffered no prejudice.

By this Experiment we gather, That artificial Air doth kill Animals by some venemous quality, and not onely by the defect of common Air; for the Snails lived a longer time *in vacuo*. See *Artic.* VI. *Exper.* III.

EXPERIMENT VII.

July 5. 1677.

I took a Receiver, filled with Air produced from Cherries, and then transmitted that Air out of *that* into another Receiver, full of common Air, in which a Frog was kept: Matters were so ordered, that the Water gave place onely to the artificial Air entering in, and the Water it self flowed out: And thus the Frog, being included in pure artificial Air, for a quarter of an hour and more suffered Convulsions, and at last lay still without motion: yet being after taken forth, and exposed to the open Air, she grew quickly well.

It seems probable by this Experiment, That Air produced from Cherries, is less hurtful to Frogs than *that* produced from Paste. See *Exper.* V.

EXPERIMENT VIII.

July 9. 1677.

I put Goosberries into three empty Receivers.

July 20.

I found one of my Recipients severed from his Cover by the force of the produced Air; I cast a Flie into it, which died in one *punctum* of time; a second Flie being likewise cast into the Receiver, presently also died: a third Flie put into the same Receiver, seemed a little while to be convulsive there; but less than a fourth Flie, which I included there, which yet before one quarter of a minute was elapsed, lay unmoved; afterward I dispelled the artificial Air out of the Receiver, by blowing, and in a little time the Flies grew well.

July 24.

I took another Receiver, filled with Air produced from Goosberries, and putting a Shrew-Mouse into it, found that he died there in the space of one half minute.

From this Experiment, it seems inferrable, That Air produced from Fruits, is less hurtful to Animals than Air produced from Minerals. For the 20 day of *July* I tried, that a Mouse did not live above a quarter of a minute in Air produced out of Gunpowder.

EXPERIMENT IX.

July 5. 1677.

I included Paste in 4 Receivers, having the Air exhausted from them.

July 6.

One of those Receivers, being filled with factitious Air, was forced from its Cover, which I again stopped, yet not so suddenly, but some *common* air might mix with the *artificial*: yet I put

put a Shrew-Mouse into it, who was presently highly convulsive, and after one minute and an half remained unmoved; and, being presently taken out, he seemed to make some convulsive motions, but died notwithstanding.

July 7.

I took a second Receiver, filled with artificial Air, and having put a little Bird into it, I suddenly stopped it; she presently fell into convulsive motions, and within a quarter of a minute, or a little more, died; I took her out, but it was too late, for she never stirred more.

I blew out the artificial Air from the Receiver, and then, another Bird of the same kind, being put into it, was very well, yet she staid there 4 minutes.

July 9.

I took a third Receiver full of artificial Air, and put that Bird into it, which in the former Experiment had continued well, and yet seemed to be lively and sound; before she had been there a full quarter of a minute, she lay without motion, and being presently taken out, there appeared no sign of life in her.

In the afternoon I put an Adder into my fourth Receiver, and within 2 minutes he began to be ill, and to gape and pant; yet he was not wholly deprived of motion till after 24 minutes. Then after 6 minutes more, which made up half an hour, I took the Adder out of the Receiver, motionless as he was, and exposed him to the free Air, yet he did not Recover life.

July 10.

The Adder remained in the same state, and gave no hope of reviviscence.

EXPERIMENT X.

July 12. 1678.

I put a Bird into a Receiver full of Air produced out of Raisins

ſins of the Sun; ſhe died in $\frac{1}{4}$ of a minute,and though I took her out preſently, yet ſhe never ſtirred more.

July 18.

I likewiſe put a Shrew-Mouſe into a Receiver full of Air produced from Raiſins of the Sun; but a thred left on the edge of the Receiver, hindered me from ſtopping it cloſe; yet the Mouſe preſently began to be very ill, and after 2 minutes he lay, as it were without any motion; yet being taken out, in 2 or 3 minutes time he was well again.

EXPERIMENT XI.

October 1. 1678.

About 10 of the Clock in the morning, I included a Shrew-Mouſe with common Air, in a Receiver, fortified againſt the external Air; about 11 the Mouſe was brought to ſuch ſtraits, that he could hardly breath: I threw in another ſtrong and luſty Mouſe into the ſame Receiver, and preſently put on the ſtopple again: But becauſe the firſt Mouſe had conſumed ſome of the Air, it came to paſs that the external Air was forcibly impelled into the Receiver, and ſo was able to diſpel a great part of the Air ſtagnant there; and indeed, when this was done, the firſt Mouſe ſeemed to be much better, neither did it die much ſooner than the other, but both of them died about noon. About 4 in the afternoon, I thruſt a freſh ſtrong Mouſe into the ſame Receiver, and leſt the external Air might again expel the included Air, I put him in very ſlowly and lieſurely; The iſſue was, that this third Mouſe lived not 3 minutes entire.

Whence we may conjecture, That that portion of Air which hath once ſerved the reſpiration of Animals as much as it could, is no longer uſeful for the reſpiration of another Animal, at leaſt of the ſame kind.

EX-

EXPERIMENT XII.

April 28.

This day in the morning I put ſo great a quantity of Paſte into an empty Receiver, that in the afternoon I found the Receiver full of factitious air; whereupon I thruſt down a Snail into it, which preſently frothed very much, and did very often expand and again contract it ſelf; but at length after 4 minutes were elapſed, he ceaſed to move at all, yet I took him not forth, till he had ſtaid in the Receiver an whole quarter of an hour, and then, being extracted, he ſeemed as if he had been quite dead; for though he were pricked with a pin, yet he diſcovered no ſign of life; yet after another quarter of an hour, being alſo pricked with a pin, he made a little motion.

I blew out the factitious air from my Receiver, and then thruſting in another Snail after the ſame manner, as I did the former, he was very well in the Receiver, and did not froth at all.

We have a confirmation by this Experiment, That factitious air is a greater enemy to Animals, than a *vacuum* is.

EXPERIMENT XIII.

June 22. 1678.

This day in the morning I put green Peaſe into an empty Receiver, and towards evening the Mercury had almoſt attained to the height of 10 digits.

June 23.

The height of the Mercury was almoſt 30 digits.

June 24.

The Mercury did not as yet exceed 30 digits in height: The Cover did no longer ſtick to the Receiver, yet hitherto nothing had eſcaped out of it.

June

June 26.

I included the ſame Peaſe in the ſame empty Receiver.

June 29.

When I now found that the Receiver was filled with factitious air, I thruſt a Snail into it, who put forth much ſpume or froth, and did very often expand and contract his horns; but after 6 minutes were elapſed, he lay ſtill, as if he had been dead, for 2 or 3 minutes; then the Receiver being opened, and the Snail taken out, moved himſelf a little, if he were pricked; whence it ſeems to follow, that air produced from Peaſe is leſs prejudicial to Snails than air from Paſte. See *Exper.* XII, XI. I blew new air into the Receiver, and a Snail then put into it did very well.

In this Experiment it ſeems obſervable, That Peaſe do quickly produce air *in vacuo*; but in the wonted compreſſion of air they generate but little.

ARTICLE VI.

Animals in Vacuo.

EXPERIMENT I.

June 22. 1676.

I Put a Butterflie into an empty Receiver, and it was almoſt 3 hours before ſhe was wholly deprived of her faculty of motion; at length, perceiving him to lie unmoved, I let in the air into the Receiver, and in a little time the Butterflie recovered his motion. Then I bound him by one of his horns with a thred, and ſo hanged him in the Receiver, and then he was carried very freely from one part of it unto the other, by clapping his wings; but after the air was extracted, the clapping of of her Wings was in vain, for ſhe could not move the thred in the leaſt, from being perpendicular.

EXPERIMENT II.

July 12. 1676.

Yeſterday I put 2 Flies into a Receiver, in which I left $\frac{1}{3}$ of air, (*i.e.*) as much as would ſuſtain 10 digits of Mercury; The biggeſt of the Flies ſeemed to die preſently, but the other, which was a ſmall bodied one, lived almoſt 24 hours.

When both the Flies lay, as if they were dead, I ſuffered ſome air to enter in, till the Mercury was 15 digits high; and then the leſſer Flie began to move her feet, but the other continued ſtill without motion.

Hence it appears, That air highly rarefied may ſerve for Inſects to breath in, and that it doth not kill them ſo ſoon as artificial air.

EXPERIMENT III.

May 1.

I put 2 Snails into an emptied Receiver, and for an whole hour they ſeemed to be well enough, and crept up to the top of the Receiver; but in 2 hours time, they fell down from thence, and lay without motion.

Six hours after they were firſt put in, I took them out *è vacuo*, and within half an hour they began to move a little. During the time they were included, they produced near as much air as ſufficed to ſuſtain the Mercury in the height of $\frac{1}{4}$ of a digit.

Theſe Snails lived longer *in vacuo* than the others included in artificial air. *Artic.* V. *Exper.* VI.

EXPERIMENT IV.

August 12. 1676.

I put Fly-blowings, or the Eggs of Flies, into an empty Receiver, to trie, whether they would produce Worms there or no.

Aug. 14.

I saw the Worms were formed, but the air had crept into the Receiver, so that it could sustain 15 digits of Mercury.

Hence it appears, That Insects may be produced, and may live, if not *in vacuo*, yet at least in air very highly rarefied. See *Exper.* VI, and VIII.

EXPERIMENT V.

March 17. 1677.

I put 2 equal quantities of Frog-spawn into 2 Vessels of Glass, of equal bigness, I left the one included in an empty Receiver, exposed to the Sun; but the other, being in a Receiver full of common air, I fortified against the access of the external air. The Frog spawn *in vacuo* did all swell into bubbles.

May 2.

No Frogs were produced in either Receiver, and that Seed or Spawn which was kept *in vacuo*, remained still full of bubbles; but about 3 days ago all the bubbles vanished, and the Spawn was changed into a certain green liquor.

July 2.

Our Receivers remained in a Window exposed to the Noonday Sun; and so some Water that was mixed with the Frog-spawn, all *in vacuo*, and the very Spawn it self was elevated into vapours, and afterwards sticking to the sides of the Receiver, out of its own Vessel, was there condensed; but the Vessel kept in the common air, still contained all its Water, together with the Seed or Spawn.

EXPERIMENT VI.

August 16. 1677.

I put Flies-Egs into an empty Receiver.

Aug. 29.

When no Worms were produced out of them, I gave admission to the Air to enter into the Receiver, and left all things in the same posture, to trie, whether the Eggs had lost their faculty of producing Worms.

Septemb.9. The Eggs produced nothing.

This Experiment, if it be compared with *Exper*.IV. seems to shew, That Insects may be generated, and may live in air highly rarefied, but not at all *in vacuo*.

EXPERIMENT VII.

June 15.

I shut in a Frog in an emptied Receiver, at about 7 of the Clock in the evening, about 9 the Frog died.

June 16.

I repeated the same Experiment, and again perceived that the dead Frog in 2 hours space, had produced some air, rather than consumed it.

June 18.

The Frog, left hitherto *in vacuo*, was swollen very much; but the air now entering, made her far more flaccid and lank than she was wont to be.

We are instructed by this Experiment, That a Receiver void of artificial air, is less hurtful to the life of such kind of Animals. See *Exper*. IV. and VII. of *Artic*.V.

EXPERIMENT VIII.

August 3. 1678.

I put Flie-blowings ſticking to Fleſh, into an emptied Receiver.

Aug. 12.

No Worms were generated from them.

Aug. 15.

Perceiving no change in the Eggs, I opened the Receiver, to trie, whether they would yet be generated in the free air.

Sept. 15.

Nothing was produced from them.

We have a confirmation by this Experiment, That Animals, which may be generated and live in highly rarefied air, yet are killed *in vacuo*. See *Exper.* IV.

EXPERIMENT. IX.

Auguſt 22. 1678.

I included Vinegar full of ſmall Eels, or Vinegar-worms in an emptied Receiver.

Aug. 29.

The Worms were ſtill moved, yet they were fewer than in the beginning.

September 6.

Yeſterday ſome of thoſe Worms did ſtill move in our Vinegar, but this day I could not ſee one; whereupon taking a Microſcope, I found them all dead; but in the Vinegar, which I had left in the open air, the Eels made as brisk motions as at the beginning.

Hence it appears, That thoſe, even very diminutive Animals, are alſo affected with the preſence and abſence of the air.

ARTI-

ARTICLE VII.

Fire in Compressed Air.

EXPERIMENT I.

May 14.

I Took a perfumed Cone, of that nature, that being once kindled in the Free air, 'tis wont by degrees wholly to be consumed; and put it into a Receiver firmly stopped with a Screw; and I intruded air into it, till the Mercury came to 120 digits above its wonted height, and then putting to my Burning glass, I kindled the Cone, which presently darkned all its Receiver with Smoke, and after some time $\frac{7}{8}$ parts of 1 digit thereof in length were reduced to ashes; yet taking out the Cone, and blowing away the ashes, I found onely the superficies thereof consumed, but the inner parts were untouched.

I included another Cone of the same sort in a much greater Receiver, but I did not compress the air therein: The Cone, fired by the same Burning-glass, was not taken out, till all the Fumes were abated and fallen down; yet much less of this Cone was burnt than of the other.

EXPERIMENT II.

May 11.

I weighed a perfumed Cone exactly, and then firmly included it in a Receiver with common air, and I kindled it by the help of my Burning glass; when the Fumes were condensed, I took

took the Cone out of the Receiver, and weighed it again, the loſs of its weight was almoſt one grain. Then I got me many pieces of Paper, each of them of the ſelf-ſame weight, which I preſume to call *Paper-grains.*

Afterwards the ſame Cone, obſerving the ſame circumſtances, was again included and kindled, but firſt I had intruded air into its Receiver, as much as could ſuſtain 90 digits of Mercury, and thus by means of a pair of Scales, I found the loſs of weight this time was 4 times more than of the former, for the Cone was lighter by 4 Paper-grains.

From this Experiment it ſeems to follow, That the conſumption of matter is ſo much the greater, by how much the greater quantity of air is contained in the Receiver.

EXPERIMENT III.

May 17. 1677.

I included a perfumed Cone in a Receiver firmly ſtopped by the help of a Screw; and, the air being compreſſed to ſuſtain 60 digits of Mercury above its wonted preſſure, I ſet fire to it with my Burning glaſs; the Cone being afterwards taken out, had loſt 3 Paper-grains and an half in weight.

I repeated the ſame Experiment, but in air, ſo compreſſed, that the Mercury reached to 120 digits above the wonted preſſure, then the Cone was 7 ¼ Paper-grains lighter; and ſo though the quantity of the air was not double, yet the conſumption of the matter by the fire, was more than twice as much as that was in the former Experiment.

May 17.

I iterated the ſame Experiment in air, compreſſed to ſuſtain 97 digits of Mercury, and then the loſs of weight ſeemed to be 6 Paper-grains.

By all theſe Experiments we are taught, That the matter is ſo much the more conſumed by the Fire, by how much the com-

compreſſion of the air in the Receiver is the greater; yea, the conſumption ſeems to have a greater proportion to the conſumption, than the compreſſion hath to the compreſſion.

May 18. 1677.

I included a perfumed Cone as before, in a Receiver 7 times larger than that which I uſed in the former Experiments, and I immitted no air at all into it. The Cone kindled there, loſt 3 $\frac{1}{4}$ Paper-grains of its weight, and no more; whereas in the ſame quantity of air, if it had been reduced to a 5 part of its ſpace, the Cone would have loſt 10 grains, *viz.* by obſerving the proportion of the conſumption made before in air, ſuſtaining Mercury to 120 digits above its accuſtomed height, (*i.e.*) air reduced to a 5 part of its ſpace.

From this Experiment it ſeems to follow, That the ſame quantity of air, if it be reduced to leſs than its accuſtomed ſpace, on that account alone cauſeth a greater conſumption, than if it had remained in its wonted expanſion.

EXPERIMENT IV.

May 19. 1677.

I repeated the Experiment laſt deſcribed, in the ſame Receiver, cloſely ſtopped with a Screw, that nothing might go out, or in. The Cone loſt 1 paper grain and a quarter onely of its weight, whence I ſuſpect that it was not well kindled.

May 21.

I made the ſame Experiment, after the ſame manner. This day the Cone was lighter by 4 Paper-grains; whence I more certainly collected, That it was not well ſet on fire in the former Experiment.

May 23.

I repeated the ſame Experiment twice, but do ſuſpect that the

the Cone was not well kindled, ſeeing at one time it loſt onely $\frac{3}{4}$, and at another time 1 Paper-grain of its weight.

May 24.

I tried the ſame Experiment again, and this day alſo the loſs of weight was found onely 1 Paper grain and a quarter. Then I opened my Receiver, and having wiped and cleanſed away the Soot, I iterated the Experiment, and then the Cone took fire very well, for the loſs of its weight amounted to 6 Paper-grains and an half.

I tried the ſame Experiment again in an uncleanſed Receiver, and then the Cone loſt onely 3 Paper-grains in weight.

May 25.

I iterated the ſame Experiment in a Receiver well waſhed, and the Cone was lighter by 6 Paper-grains and an half.

I made the ſame Experiment in the like manner, and in a well cleanſed Receiver, and the Cone loſt 7 grains and an half of its weight.

I tried the ſame Experiment again, in an unwaſhed Receiver, and then I could not ſufficiently kindle the Cone.

May 26.

I tried the ſame Experiment in an unwaſhed Receiver about the middle of the day, the Sun being clear, and clouded with no miſts; and I removed not my Burning-glaſs from kindling the Cone a long time, ſo that it took fire very well, and became 8 Paper grains lighter.

By theſe Experiments it is manifeſt, That the quantity of a Cone to be conſumed in the ſame quantity of air, is not fixed and certain, but ſometimes greater, ſometimes leſſer, as the Cone ſhall be more or leſs kindled: Beſides the imperfect mixture of the matter may cauſe ſome difference; yet it ſeems certain that fire is more eaſily kindled in compreſſed air, than in common; and the conſumption will be the greater in a certain quantity of air, if that air be reduced into a narrower ſpace, than if it enjoyed its wonted expanſion.

EX-

EXPERIMENT V.

May 22.

I put a perfumed Cone into a Receiver made for compresſing the air; and intruding the air till the Mercury ſtaid in 30 digits above its wonted preſſure: I kindled the Cone, and found its weight to be abated 1 ¾ of a Paper-grain.

May 23.

I made the ſame Experiment again, after the ſame manner, and in effect with the ſame ſucceſs.

I tried the ſame Experiment again, but the Cone took not fire well. Whence we have a confirmation, that Fire is more eaſily kindled in air much compreſſed, than in common air, or that which is but a little condenſed.

I iterated the ſame Experiment, and after I had removed my burning-glaſs from kindling the Cone, whileſt I was intent to ſee, whether the Cone would proceed to be conſumed, the Receiver brake into 100 pieces, ſome of which ſtruck my head and wounded it: which paſſage I mention, that ſo no man may be confident his Glaſs will not break, whileſt he is about theſe Experiments, becauſe he hath found that at other times it hath reſiſted a greater preſſure. For this very Glaſs of mine, had contained air 4 times more compreſſed, very well. See *Exper.*III. Yea in *Exper.*VI. of *Artic.*II. it had reſiſted Air, ſuſtaining 198 digits of Mercury above its wonted height; yet now it was broken by a preſſure more than 6 times leſs: and therefore whilſt a man looks into ſuch Receivers, his head had need be fortified with ſome perforated or pellucid muniment and defence to preſerve it from a blow.

ARTICLE VIII.

Fire used to produce Air.

EXPERIMENT I.

June 4. 1676.

I Burnt Paper, besmeared with Sulphur *in vacuo*, and found that it produced some Air, which Air was not at all diminished for 2 whole days.

That Air is to be ascribed to the Paper, for no Air is produced out of Sulpher alone.

EXPERIMENT II.

June 15.

I burnt Harts-horn *in vacuo*, and found that the Fumes issuing therefrom, did contain some Air in them.

June 17.

These 2 last days, I iterated the same Experiment, and always observed, That, Air produced from Harts-horn, was in a short time in part destroyed; but that, which preserved the elastick nature of Air for a full hour after the Burning-glass was removed, seemed afterwards not to lose it at all.

June 19.

I took the Harts horn out of the Receiver, and found no volatile Salt, but onely a fœtid Oil to be produced therefrom.

EXPERIMENT III.

June 21.

I burnt Amber *in vacuo*, and at firſt I could not find that the Fumes did aſcend above the height of one digit; and yet in a Receiver full of Air, they would be carried up to the top of the Receiver, and from thence be reflected downwards; yet afterwards, even in the *vacuum* it ſelf the Fumes reached almoſt to the top of the Receiver, but the Mercury was not at all changed in its Gage.

June 22.

This night, a great deal of that Water, in which I had immerſed the Receiver, found a paſſage into it, though the Cover was ſo well fitted to the aperture, that I never perceived any water to get in betwixt them before. Hence a ſuſpicion aroſe in me, that ſome volatile Salt had probably attracted (if I may ſo ſpeak) the aqueous parts, by reaſon of the congruity betwixt them.

July 8.

I ſtill kept the Receiver immerged in Water, but no more Water entered in, as if, the Salts being waſhed away, the external Water, being deſtitute of aſſiſtance, could no longer creep in: But that agreement between the Fumes of the Amber, and the parts of the Water had need of a confirmation by a great many more Experiments.

Hence it appears, That Amber produceth no Air, no not though it be burnt.

EXPERIMENT IV.

Jan. 18. 1677.

I put 2 drachms of Camphire into an empty Receiver, and the commiſſure of the Cover with the Receiver, being fortified againſt external Air. I put the Camphire on a digeſting Furnace.

Jan. 19.

The Camphire was ſublimated into Flowers, but no Air was produced.

EXPERIMENT V.

May 24. 1676.

I included *Sulphur vivum* in an exhauſted Receiver, and melted it by the help of my burning-glaſs, but found that the Fumes produced therefrom, did contain no Air in them, becauſe the Mercury did aſcend to the aperture of its Gage, as it uſeth to do while the Receiver is evacuating: yet when the Receiver was cooled, the Mercury returned to its former height; and therefore I think that change proceeded onely herefrom, becauſe the Air included in the ſealed leg of the Gage, was rarefied, and drove the Mercury into the other part.

EXPERIMENT VI.

July 19.

Having included Paſte 9 days agoe *in vacuo*, and perceiving that it now contained no more air; I endeavoured to fire it with my burning glaſs. The ſubſiding Fumes had tinged the ſuperficies of the Paſte, with a curious yellow colour; and beſides I conjectured, That ſome Air was produced, becauſe the Receiver, which before was ſtraitly joyned to its cover, was now with eaſe plucked therefrom.

ARTI-

ARTICLE IX.

Concerning the Production of Air in Vacuo.

EXPERIMENT I.

September 9. 1676.

I Exhausted the Air out of a Receiver half full of dried Grapes, and fortified it against the external Air.

Sept. 10.

In 24 hours time the height of the Mercury was $\frac{1}{2}$.

*Sept.*12. In two days time, the ascension of it was $\frac{1}{2}$.

14. The ascension of the Mercury was $\frac{3}{8}$.

17. The ascension of it was $\frac{3}{8}$.

22. The ascension of it was $\frac{5}{8}$.

27. The ascension was $\frac{5}{8}$. The height 3 digits.

October 11.

The height of the Mercury was now about 6 digits.

September. 9. 1676.

I put dried Figs into a Receiver, and filled about half of it with them, and then I extracted the Air, till the Mercury staid in the height of 3 digits.

Sept. 10. No Air was produced.

Sept. 17.

Perceiving no Air to issue out of the Figs, I opened the Receiver.

By this Experiment we learn, That dried Fruits, put into an exhausted Receiver, do produce very little Air with any regularity.

EX-

EXPERIMENT II.

August 5. 1676.
I included Pears and Apricocks *in vacuo.*

Aug. 6.

In 18 hours time the Mercury reached 2 digits; in 10 hours more it reached the third digit. Its height was 3 digits.

Aug. 7. The height of it was 5 digits.

8. The height of it was $6\frac{1}{2}$.

9. In 14 hours space, the Mercury mounted $\frac{1}{4}$. Its height was $7\frac{1}{4}$.

Aug.	The height of it was	*Aug.*	The height of it was
10	$8\frac{3}{4}$	18	25
11	$10\frac{3}{4}$	19	29
12	$12\frac{1}{4}$	20	$31\frac{1}{2}$
13	$14\frac{1}{4}$	21	$32\frac{1}{2}$
14	16	22	34
15	18	23	35
16	20	26	$38\frac{1}{2}$

Aug. 29. The height of the Mercury was 41.

*Sept.*1. The height of the Mercury was $42\frac{1}{2}$.

4. The height of it was 44.

7. The three days last past, being hotter than the foregoing, the ascension of the Mercury was $2\frac{1}{4}$. Its height was $46\frac{1}{4}$.

*Sept.*10. The height of the Mercury was $47\frac{1}{2}$.

13. The Mercury was depressed, its height was onely 44 digits.

23. The Mercury was by degrees again mounted to the 48 digit.

27. The height of the Mercury was $50\frac{1}{2}$.

Nov. 5. The Mercury ascended by degrees to the height of $52\frac{1}{2}$.

Nov.

Nov. 28.

The Apricocks were reduced to Water; the skin was severed from the Pulp, yet no more Air was produced.

Jan. 10. 1677.

Whilest it was a very hard Frost, the Mercury came to the height of 57 digits: but when the Thaw came, it was depressed to 23. Whether the strength of the Frost opened some way for the Air to get out, I know not.

March 3.

The Mercury could ascend no higher, because the Air was got out. This day I found the Receiver tumbled on the ground, and the Apricocks, when the Frost was broke, were putrified, and had lost their colour.

From this Experiment it seems to follow, That Apricocks do produce Air almost as easily in their wonted pressure, as *in vacuo.*

EXPERIMENT III.

June 20. 1676.

I put sowre Cherries into 2 empty Receivers, and observed altogether the same circumstances in them both; save that in the one, the Cherries were *whole*, in the other, *cut* asunder. In 2 hours space the *whole* Cherries had impelled the Mercury into the Gage to the height of 10 lines; and the dissected Cherries, to about 20.

June 21.

In 24 hours space, the Mercury, which was in the Receiver, containing the *whole* Cherries, came to the height of 3 digits; but in the other Receiver the Mercurial Gage was spoiled.

June 26.

The *whole* Cherries had not yet produced so much Air that could sustain 15 digits of Mercury; but the dissected Cherries had wholly filled their Receiver with Air.

July

July 9.

This day the Receiver of the whole Cherries was removed from his Cover: I did eat one of the Cherries, and its taſte ſeemed pleaſant enough. I included the reſt again *in vacuo*, many of them were broke, and in one hours ſpace they impelled the Mercury to aſcend to the height of about 2 digits.

July 10.

Theſe laſt 24 hours the Mercury aſcended not; whether the Gage was prejudiced, I am not certain.

July 15.

This day I found the Cover ſevered from his Receiver, and ſo it was clear, that the Gage was ſpoiled or hurt.

This Experiment gives us a probable conſequent, That ſome *diſſected* Fruits do ſooner produce their Air, than *whole* and undivided ones.

EXPERIMENT IV.

June 9. 1676.

I put Cherries (not acid ones) into an empty Receiver, and within one hour I found as much Air produced from them, as ſufficed to ſuſtain $\frac{1}{4}$ of a digit of Mercury.

June 10.

In 18 hours the Mercury ſeemed to have come to the height of 11 digits.

June 11.

Our Fruits produced Air, leſs, and leſs copiouſly; ſo that this day, towards the evening, they came not up to the height of 15 digits.

June 12. Now the Mercury was a little higher than 15 digits.

13. The height of the Mercury was 22 digits.

16. The Mercury yet came not up to 30 digits.

18. Perceiving no more Air to be produced from my Fruits, I opened the Receiver.

Such

Such a small production of Air seemed very observable to me, because I had found by experience, that Fruits of the same kind in *France*, had filled their Receiver in 2 days time; it may probably come to pass, that Fruits of the same kind, in several Countries, may differ much amongst themselves.

EXPERIMENT V.

June 12. 1676.

I put Cabbages cut in pieces into an empty Recipient, with a Mercurial Gage, and in one hours space the Mercury had made one line.

June 13. The Mercury was now come almost to the height of 10 digits.

17. The Mercury was come almost to the top of its Gage, and the Receiver being opened, I found the Cabbages little altered.

19. The Cabbages being left 2 days in the open Air, were wholly corrupted and blackish. I put them again *in vacuo*, to trie, whether the putrefaction begun, would promote, or else retard the production of Air.

June 19. The Mercury in half an hour ran up $\frac{1}{2}$ of a digit.

22. For three whole days the Mercury got higher onely 10 lines. Its height was 1 and $\frac{1}{3}$ of a digit.

23. Finding that the Cabbages produced no more Air, I took them out of the Receiver, their Smell was very bad. Hence a suspicion arose within me, That Bodies, when they putrefie, have already produced almost all their Air.

EXPERIMENT VI.

May 29. 1676.

I took pieces of Orange weighing 4 ounces, and put them into a Receiver capable of holding 10 ounces of Water, and I exhausted the Air.

June 10.

This day the Receiver was removed from his Cover, by the force of the produced Air; ſo that I took out the Oranges, and preſently put them into another empty Receiver capable of containing 8 ounces of Water, and the Mercury within half an hour, was elevated to the height of one half digit.

June 13.

That ſudden aſcenſion of the Mercury was not durable, for it *yet* came not to the height of 2 digits.

June 16.

The Mercury, the laſt 24 hours aſcended about 3 lines.

June 21.

The Mercury, theſe laſt 24 hours, did not aſcend the ſpace of one line.

July 18.

I perceived no more alteration was made in the height of the Mercury; but ſome mouldineſs appeared, though I am certain that no Air from without, had found any ingreſs into the Receiver.

EXPERIMENT VII.

April 27. 1676.

I put a Tulip into an empty Receiver, with a Mercurial Gage, but before it was fortified againſt the external Air, ſome Air had got in, enough to ſuſtain 2 digits of Mercury.

May 2.

The Tulip, which firſt ſeemed ſtriped with ſundry colours, was now wholly changed into a dark red, and was moiſt, It produced very little Air.

EXPERIMENT VIII.

April 22. 1676.

I put half of a Limon into an empty Receiver, with a Mercurial

curial Gage, ſo ſhort, that the Mercury could not run up the ſpace of 3 digits.

April 24. In 2 days ſpace the Mercury came to the height of one digit and an half.

25. The Mercury was now 2 digits high.

27. Yeſterday the Mercury made 4 lines, but this day onely one.

29. The 2 laſt days, the Mercury mounted higher by one line.

May 3.

In 4 days ſpace the Mercury aſcended one line and a little more.

May 3. 1677.

The Mercury came to the top of its Gage, yet no Air got out; but the Limon was little altered.

*Jan.*1. 1678.

As yet no Air eſcaped out of the Receiver; but the Limon had contracted a yellow colour, and moiſture therewith.

EXPERIMENT. IX.

March 16. 1677.

I put 2 Apples, of the ſame ſort, in 2 empty Receivers, one of the Apples began to putrifie before, the other was onely bruiſed with a few blows.

May 15. 1677.

As yet the Fruits were in very good caſe; but this day that Apple which was bruiſed, appeared wholly rotten, and the Receiver was forced from his Cover; the other Apple remained without any change.

Auguſt 20. 1677.

That Apple which before began to be rotten, ſuffered no farther alteration; but this day finding that the Receiver was pulled from his Cover, and fearing leſt the Apple would be ſpeedily putrified, I took it out; its taſte was grateful, but ſubacid, as if it had been fermented; but the pulp inclined to the conſiſtence of meal.

From this Experiment it ſeems to be confirmed, That Fruits have produced the greateſt part of their Air, when putrefaction begins to alter them; ſeeing the putrid Apple did not fill its Receiver but in a much longer time than the other Apple. See *Exper.* V. of this Article.

EXPERIMENT X.

May 17. 1676.

I poured 2 equal quantities of Milk into 2 Glaſs Receivers, of equal bigneſs; the one I left in the Free Air, the other I included to be kept in an emptied Veſſel, with a Mercurial Gage.

May 18.

The Cream did ſwim on the top of that Milk, which was left in the Free Air; but that which was *in vacuo*, was onely covered with Bubbles; and the Gage was not changed at all.

May 19.

The Bubbles ſwelled more and more, and the Mercury in the Gage was a little higher.

May 20.

The Bubbles *in vacuo* ſwelled yet more, and that Milk ſeemed curdled; but the other in the Free Air was manifeſtly curdled. The Mercury *in vacuo* came almoſt to the top of its Gage.

May 22.

The Milk *in vacuo* proceeded to generate Air more and more, and now it evidently appeared to be curdled; whence it is manifeſt, that the coagulation of Milk, when the Air is taken away, is retarded. Now almoſt all the Bubbles were broke.

June 20.

The Milk *in vacuo* was no longer covered with Bubbles, and remained ſtill coagulated in the ſame ſtate. But the Milk in the Free Air, ſtank filthily, and was full of Worms: when it was put on the Engine, and the Air extracted, it did emit ma-

ny

ny very great bubbles for a long time; and the Worms did move themſelves very vehemently, but not one of them died in 4 hours ſpace.

May 19. 1677.

Three or four Moneths ago, ſome Whey *in vacuo* was poured out of a Veſſel into a Receiver, and it ſeemed clear and limpid, like Water; yet there was Whey enough left in the Veſſel, to ſeparate the Butyrous from the Caſeous part, at a ſufficient diſtance.

This day the Milk ſtagnant in the Receiver, ſeemed to have got out of it; ſo that it is clear, that the Air in the Receiver, was of greater force than the external Air, for the Cover alſo was forced from the Receiver. Towards night, I took that Milk out of the Receiver, and found it to be acid, both in ſmell and taſte, yet it was not unacceptable to the palate; but after a ſhort time, the Whey, which hitherto had remained limpid between the Caſeous and Butyrous part, began to diſappear, and to be blended with the reſt.

May 24.

This day the Butyrous part was wholly vaniſhed though as yet it had ſuffered no ſenſible mutation; but the Milk began to ſmell amiſs.

June 1.

Our Milk had not yet contracted the worſt of ſmell, neither had it produced any Worms, but it grew dry by degrees; and this night the Mice eat it up, as perhaps they had done the Butyrous part before.

This is the Story of my Preſerved Milk, in which theſe 4 things ſeem moſt obſervable. Firſt, That the Coagulation of Milk, when Air is extracted therefrom, is ſomewhat retarded. Secondly, The weight of Butter, or of Whey, or Cheeſe, is not the ſame in the Air, as it is *in vacuo*; for in the Air they are mixed one with another confuſedly: but *in vacuo* one ſwims

on

on the top of the other. Thirdly, The putrefaction of Milk, when Air is extracted, is hindered, or very much retarded. Fourthly and lastly, Milk by long continuance *in vacuo*, is made unfit to generate Worms, even in Common Air.

EXPERIMENT XI.

September 5. 1677.

I took the same Receiver, and the same Vessel, which I used before to preserve Milk *in vacuo*, and I included Urine therein, after the same manner, as I had done Milk before. The quantity of Urine was 3 ounces and 3 drachms, or thereabouts; and the Receiver was onely capable of holding 10 ounces of Water.

Sept. 7.

The Mercury reached to the height of almost 2 digits.

Sept. 8.

The Mercury was this day somewhat higher than yesterday.

December 5.

The Mercury ascended not above 3 digits in height, and for the whole moneth past was not changed at all. The Urine seemed not at all to be altered.

Decemb. 6.

I set other Urine under a Receiver, not fortified against the external Air.

Decemb. 16.

The Urine *in vacuo* still kept unaltered, but the other, in 10 days time seemed turbid, and to have contracted some mouldiness in its superficies.

This Experiment, compared with the former, gives us a probable inference, That Urine, which is an excrementitious humour, contains less Air in it, than Milk which is alimental.

Moreover, The efficacy of the Air to corrupt Urine, seems very observable.

EX-

EXPERIMENT XII.

May 19.

I took Paste very much diluted, and without Leaven, and put it in a Glass Vessel into an empty Receiver; and though the Vessel, which contained it, were not half full, before all the Air was exhausted, yet the Paste had swollen above the brims of the Vessel.

May 20. The Paste continued to swell more and more, and was interspersed with many cavities.

May 22.

This day the Paste was much more tumid than before, and much Air was generated therefrom.

May 23.

This day in the morning I found the Cover severed from his Receiver, by the force of the produced Air, and some of the Paste was spread above the edges of the Receiver, yet its swelling was somewhat abated. In the afternoon, its tumidness was much more abated, yet it took up twice more room than it did before it was put into the Receiver. The taste of it was not acid, and therefore I think that Bread, thus made, is very light.

EXPERIMENT XIII.

July 20. 1676.

I took a quantity of Beef, and put it into an exhausted Receiver, fortified against the external Air; and likewise I put another equal quantity of Beef into a Receiver, neither exhausted, nor closely stopped.

July 21.

In 30 hours space, the exhausted Receiver was all filled with Air, so that I suspected some Air had got in; and therefore I in-

included the ſame Beef again, and ſo cloſed it, that there was no fear of the ingreſs of any external Air.

July 22.

In 14 hours ſpace the Mercury came to the height of 15 dig.

July 25.

For 3 whole days and more, the Beef did not produce ſo much Air, as would fill one half of the Receiver.

July 26.

This day the Receiver was ſevered from his Cover; and in one hours ſpace, I perceived that the Beef, being again included *in vacuo*, had produced Air, which ſufficed to ſuſtain 10 digits of Mercury.

July 28.

I found the Receiver again filled with Air, and re-exhauſting it, much Air was in a ſhort time again produced from the Beef.

July 30.

The Receiver being again filled, I included the Beef again *in vacuo*, and found, that the Air produced from it in one hours ſpace, was able to ſuſtain 10 digits of Mercury.

Auguſt 1.

The Receiver being this day filled again, the Beef ſtank ſo filthily, that we threw it out of doors.

Hence it appears, That Fleſh, whileſt it putrifies, doth produce much more Air, than before it putrifies; but 'tis otherwiſe with Fruits. See *Exper.* IX. of this *Artic.*

EXPERIMENT XIV.

July 18. 1676.

I put ſome Gooſberries, which I had kept long in Receivers to produce Air, into a vacuous Receiver.

Within half an hour the Mercury aſcended to the height of one digit.

In an hour and halfs time, the Mercury mounted another digit.

July

July 19.

In 24 hours time, the Receiver was almost all filled with Air.

July 20.

The Cover was forced from his Receiver, and much juice had run out of the Receiver.

July 29.

I left the same Goosberries in a Receiver, not hitherto fortified against the external Air; but this day I included them again *in vacuo*, to trie, whether they could produce any more Air.

July 30.

In 16 hours time, the Goosberries drave up the Mercury a digit and $\frac{1}{2}$ into the Gage.

July 30. 1677.

The Goosberries could not wholly fill their Receiver; and they always remained in the same state, but a while since they had almost lost their red colour, and inclined to white.

From this Experiment it seems to follow, That these Fruits, after they have produced all their Air, admit very little alteration; as if that Air it self were the cause of corruption.

EXPERIMENT XV.

August 23.

I put Pears into a *vacuous* Receiver with a Mercurial Gage; and before the Receiver could be well fortified against the ingress of the Air, the Mercury was come to the height of one digit and an half.

In 2 hours space the Mercury ascended 4 digits; its height was almost 6.

August 24. The height of the Mercury was 12 digits.

25. The height thereof was 16.

Aug. 26 27	The height of it was	18 21	*Aug.* 28 31	The height of it was	23 30
Sept. 1 2 3	The height of it was	32 35 $38\frac{1}{3}$	*Sept.* 4 5 6	The height of it was	$44\frac{1}{3}$ $45\frac{1}{3}$ 50

*Sept.*7. The height of it was the same, because some Air had escaped, but I prevented that for the future.

8. The height of the Mercury was $53\frac{1}{2}$.

9. The height of it was $54\frac{1}{2}$.

10. The height of it was 58.

Septemb. 12.

Yesterday the Mercury persisted in the same height; but this day it seemed to be depressed: whence I conjecture, that some Air had got out. The height of it was $53\frac{1}{2}$.

Sept. 13.

I transmitted the Air into another Receiver: the height of it was $32\frac{1}{2}$.

Sept. 16.

I perceived that the Air had got out; and opening the Receiver, I found the Pears very rotten.

These Pears produced their Air irregularly enough, sometimes quicker, sometimes more slowly.

EXPERIMENT XVI.

September 17.

I put dried Plums into an evacuated Receiver.

*Sept.*19. The Mercury seemed to have ascended a little.

22. I perceived not that the height of the Mercury was any more altered.

Novemb. 8.

When I saw that the Plums produced no more Air, I opened the Receiver.

By

By this Experiment, we have a confirmation, That dri'd Fruits are very unfit to produce Air.

EXPERIMENT XVII.

Septemb. 28.

I put fresh Nut-kernels, cut into pieces, having thrown away their shells, into an evacuated Receiver with a Mercurial Gage.

29. The Mercury ascended a little.

30. The height of it was 2 digits.

Octob. 5.

The Mercury proceeded to ascend by degrees: the height of it exceeded 6 digits.

Oct. 15. The height thereof was 10 digits.

22. The height of it was 15.

Nov. 28.

The Mercury was come to the height of 20 digits, or a little more; but this day the Receiver was cast down and broken, and the Nut-kernels thrown about; they were kept very well, both as to colour and taste.

Hence we may conjecture, That Air without sensible putrefaction may be produced from Fruits, even of an hard consistence.

ARTICLE X.

Concerning the Production of Air above its wonted Preſſure.

EXPERIMENT. I.

June 22.

I Included new Peaſe in a Receiver with a Glaſs full of Raiſins of the Sun bruiſed, and mixed with Water, I did not exhauſt the Air.

Towards Evening the Mercury had mounted to 12 digits, but a great part of that Air was produced from the Raiſins, not from the Peaſe.

June 23. The height of the Mercury was 49.

June 24	The height of it was	75	*June* 26	The height of it was	90
25		90	28		100

The Peaſe did as it were ſweat, and grow yellow.

30. The height of the Mercury was 110.

July 1. The Mercury aſcended not, yet no Air eſcaped out.

4. The height of the Mercury was 124.

7. The height of it was 140.

July 10.

The height remained the ſame, but the liquor which diſtilled, or ſweat out from the Peaſe, got out.

July 12.

New liquor was produced from the Peaſe, but the Mercury continued in the ſame height.

July 13.

The liquor got out of the Receiver, and ſome Air beſides; where-

whereupon I *set* the Screw, and new liquor being in a ſhort time collected, did fortifie the Cover within.

July 15.

This day the Receiver was broken in pieces; but the Peaſe being ſofter than ordinary, were eaſily ſtript of their husks, as if they had begun to be boiled: they kept their ordinary taſte.

EXPERIMENT II.

Sept. 15. 1676.

I put unripe Plums into a vacuated Receiver; but before the Receiver could be guarded againſt the external Air, the Mercury had already aſcended to the height of one digit.

Sept. 16.

In 24 hours time the Mercury ran up 5 digits, its height was 6 digits.

Sept. 17. The height of the Mercury was 8.

Sept. 18	The height of it was	10	*Sept.* 23	The height of it was	18
19		12	24		19
20		14	26		23
22		18	28		26

Octob: 1. The height of the Mercury was 30.

4. The height of it was 31. 'twas ſomewhat cold.

Octob. 5	The height of it was	32	*Octob.* 9	The height was 33 ½.
7		33	11	The ſame height ſtill.

Octob. 15. Theſe 2 laſt days, the Cold being abated, the Mercury aſcended more ſpeedily; its height was 37.

Octob. 17	The height of it was	38	*Octob.* 29	The height of it was	45
19		39 ½	*Nov.* 2		46
22		41	5		47
26		43	20		53

In this Experiment, the Air ſeems to be produced ſometimes regularly enough, and at other times Anomalouſly.

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EXPERIMENT III.

July 6. 1676.

I put Goosberries into an emptied Receiver, but before it could be guarded against the external Air, it had entered in, and impelled up the Mercury to the height of half a digit; and afterwards in half an hour, the Air produced from the Goosberries, had impelled it up to another semi digit.

In 7 hours time the Mercury ascended 4 digits higher: it staid in 5.

July 7. In 14 hours space the ascension of the Mercury was 2 digits and ½.

In 10 hours space, the ascension of it was 2 ½.

July 8. In 14 hours the ascension of the Mercury was 1 ½.

In 10 hours the ascension of it was 2 digits.

July 9. In 14 hours the ascension of the Mercury was 2 ½.

In 10 hours its ascension was 1 ¼.

July 10. In 14 hours the ascension of it was 1 ¾.

In 10 hours the ascension of it was 3.

July 11. In 24 hours the ascension of the Mercury was 4.

July 12. In 24 hours the ascension of the Mercury was 4.

Now the Mercury was brought to its wonted pressure.

July 13.

This day in the morning, I found the Cover to be broken, and because it was fastned by a Screw, that it might not be severed from the Receiver, I suspected that it was broken by the force of the internal Air; I substituted another Cover in its place.

July 14, 15, 16, 17, 18.

I perceived no change in the height of the Mercury, because the Cover was not exactly shut; and therefore I took out the Fruits, and put some part of them into another evacuated Receiver, and the rest I stopped up closely with common Air, that nothing might get out.

In

In 4 hours the aſcenſion of the Mercury was 4 digits.

July 19. In 14 hours the aſcenſion of the Mercury was 1 $\frac{1}{2}$. but, ſuſpecting the Air to have eſcaped, I *ſet* the Screw.

In 9 hours the aſcenſion of the Mercury 11 digits.

The Cover was broke, and the Air made an eſcape.

This Experiment ſeems to prove, That Gooſberries contain much Air in them, which, as ſoon as it is freed from the wonted preſſion of the Air, doth more readily break forth, than when it is reſtrained by ſome ambient Air, until the Gooſberries begin to be fermented, for then Air is produced in a far larger quantity, even in a great compreſſion.

EXPERIMENT IV.

July 8. 1676.

I included Paſte in an exhauſted Receiver, and, before it was guarded againſt the external Air, the Mercury was come to the height of 3 digits, by reaſon of the Air making an irruption from without; whence it came to paſs, that the Paſte, which was much ſwollen, loſt about the third part of its tumidity.

A little while after it ſwelled again, and within half an hour the Mercury mounted higher by 2 digits.

In one hours time the aſcenſion of the Mercury was 2 $\frac{1}{2}$. and the Paſte continued to ſwell or riſe more and more.

In another hours ſpace the aſcenſion of the Mercury was 3 digits and $\frac{1}{2}$.

In 1 hours time the aſcenſion of it was 4 $\frac{1}{2}$ digits: it ſtaid in 16.

July 9.

In 14 hours ſpace, the aſcenſion of it was 21 digits. The height of the Mercury was 37. Moreover I ſuſpected that ſome Air had got out; when I *ſet* the ſcrew, the Cover brake, and upon the ingreſs of the external Air, the Paſte, which always did *riſe*, now did abate about 2 digits of its tumidity, though it was now found in a leſs compreſſion than before.

In

In 5 hours ſpace the aſcenſion of the Mercury was 15 digits.

But when I again endeavoured to *ſet* the Screw, the Cover brake, ſo that the Air eſcaped; the Paſte did preſently ſomewhat pitch, and was depreſſed.

In 4 hours ſpace the aſcenſion of the Mercury was 10 digits, the Paſte did again ſwell or riſe, as before; but being willing to ſubſtitute a better Screw in the place of the other, I permitted an egreſs to the Air, yet this time the Paſte did not pitch or ſubſide, as before it had done.

July 10.

This night the Paſte *roſe* again, yet it ſeemed to have produced no Air.

In 4 hours ſpace there was no aſcenſion of the Mercury.

In 7 hours ſpace the aſcenſion of it was 4 digits.

July 12 I perceived no aſcent of the Mercury.

13. It ſeemed to have aſcended a little.

17. Seeing no more Air was produced, I took out the Paſte and found it to be of a ſubacid ſmell,

This Experiment ſeems to prove, That Air may be produced out of Paſte, in *compreſſed* Air, as well as *in vacuo.*

But the Paſte was twice depreſſed, becauſe the compreſſed Air ſuddenly finding out a way of eruption, was ſo much dilated, as it is wont to happen in all Springs, when they are carried beyond their point of reſt: but, when that Air was immediately repelled by the external Air, the Paſte did pitch and was depreſſed.

EXPERIMENT V.

July 13. 1676.

I included ſome Beans, of that ſort which are given to Horſes for Provender, *in vacuo*, with ſome Water; ſome of them which were *bruiſed*, ſeemed to ſwell much; but thoſe which were left *whole*, ſuffered no ſenſible alteration.

In

In 2 hours ſpace I ſaw no Air produced, though the Beans continued to ſwell.

July 14. In 24 hours the aſcenſion of the Mercury was 7 digits.

July 15. In 16 hours the aſcenſion of the Mercury was 3 digits and $\frac{1}{2}$.

In 8 hours the aſcenſion of it was 1 $\frac{1}{2}$. the height of it was 12.

July 16. In 14 hours the aſcenſion of it was 3.

17. In 26 hours the aſcenſion of it was 6.

18. In 24 hours the aſcenſion of the Mercury was almoſt 9.

19. I ſtopped the Receiver firmly with a Screw, becauſe the Air had got out. In 9 hours ſpace the aſcenſion was 1 digit.

20. In 24 hours ſpace, the aſcenſion was 3 $\frac{1}{2}$.

21. In 24 hours ſpace the aſcenſion was 5 $\frac{1}{2}$.

22. In 14 hours the aſcenſion of the Mercury was 2 digits.

23. In 24 hours the aſcenſion of the Mercury was 18 digits.

24. In 14 hours the aſcenſion of the Mercury was almoſt 5. The height of it was 35 above the wonted preſſure.

25. The Receiver could not ſuſtain a greater preſſure. I found the Beans of a fœtid ſmell, not much unlike the ſmell of putrified Fleſh.

From this Experiment it ſeems to follow, That Beans contain much Air in them, and that, that Air is produc'd in a moderate preſſure, as well as *in vacuo*, ſometimes more ſpeedily, ſometimes more ſlowly.

Eſpecially, that great inequality, which happened *July* 23. is to be taken notice of.

EXPERIMENT VI.

July 23.

I included Gooſberries *in vacuo*, and fortified them very well againſt the external Air.

In 2 hours ſpace the Mercury aſcended 1 digit.

July 24. The height of the Mercury was 7 digits $\frac{1}{2}$.

July 25	The height of it was	12	*July* 27	The height of it was	20
26		17	28		$24\frac{1}{2}$

July 29. The height of it was almoſt 30.

30. The height of it was almoſt 31. I tranſmitted ſome Air out of this Receiver into another evacuated Receiver, and ſo the height of the Mercury was 26.

31. The height of the Mercury was 35.

Auguſt 1.

The height of the Mercury was 39. But ſome Air had eſcaped out; and going about to ſtop the Receiver cloſe, I ſuffered ſome more Air to get out.

The height of the Mercury was 30.

Aug. 2. The height of the Mercury was 39. I tranſmitted ſome Air into another Receiver.

The height of the Mercury was 31.

Aug. 3. The height of the Mercury was 39.

4. The height of the Mercury was 41.

5. The height of the Mercury was 43. I tranſmitted the Air into another Receiver.

The height of the Mercury was 30 digits.

6. The height of the Mercury was 43.

7. The height thereof was 47.

8. The height thereof was 48. But the Air being tranſmitted into another Receiver, the height of it was 36.

9. The height of the Mercury was 41. Fourteen hours were paſt.

Aug. 10. The height of the Mercury was 47. the Air being transmitted into another Receiver, the height of it was 35. 24 hours were elapsed.

11. The height of the Mercury was 38½. Fourteen hours were elapsed.

12. The height of the Mercury was 42. twenty four hours were passed. I extracted the Air, and the height of the Mercury was 26.

13. The height of the Mercury was 33. twenty four hours were elapsed.

14	The height of it was	36	hours 24.	17	The height of it was	44	hours 24.
15		39		18		47	
16		41½		19		50	

I transmitted the Air into another Receiver, and the Mercurial Gage was spoiled. I took out the Goosberries, and found that they had lost their colour, and also almost all their acidity.

From this Experiment we may infer, That Goosberries do produce their Air regularly enough, unless something be extracted out of the Receiver, for then they acquire strength to produce new Air more speedily.

EXPERIMENT VII.

September 12.

I put crude Grapes into an emptied Receiver, but before they could be fortified against the external Air, some thereof had got in, as much as could sustain 3 digits of Mercury.

Sept. 13	The height of it was	5	*Sept.* 17	The height of it was	19
14		10	19		23
16		17	20		25

Sept. 22. The height of the Mercury was 30. I stopped the Receiver with a Screw.

23 The height of the Mercury was about 30½.

24 The height thereof was 32.

Sept. 26	The height of it was	34½	*Octob.* 2	The height of it was	39½
27		36¼	4		39½
28		36¼	5		40½
29		37¼	7		41½
30		37¾	9		42½

Octob. 15. The height of the Mercury was 46. It ascended chiefly these 2 last days, when the Frost was dissolved.

Nov. 2. The height of the Mercury was 54 digits.

5. The height was 58.

Jan. 10. 1677.

Now the Mercury was come to the height of 70 digits; and yet I perceived no sensible mutation in the Mercurial Gage, even when the Cold was most fierce, though the Grapes and their Juice were concreted into Ice.

September 21.

Hitherto the Grapes seemed not altered: but the Mercury had ascended a little, because the Air had found a passage out. This day I opened the Receiver, and when the Air brake forth, many of the Grains seemed to be contracted into wrinkles. The Grapes had kept their taste but much more pungent; but their Juice continued to be tinged with a curious red colour.

This Experiment seems to inform us, that Grapes produce not all their Air, but in a long tract of time.

EXPERIMENT VIII.

August 10. 1677.

I put Pears cut in two, into a vacuous Receiver. Towards Evening the Mercury was come up to the height of 10 digits.

Aug. 11	The height of it was	20	*Aug.* 15	The height of it was	55
13		38	16		60
14		48	17		68

The

The Air being transmitted into another Receiver, the height of the Mercury remained at 53 $\frac{1}{2}$.

Aug. 18	the height	61	*Aug.* 20	the height	70
19	of it was	64	21	of it was	72

The Air being transmitted into another Receiver, the Mercury remained in the height of 61.

Aug. 22	the height	68	*Aug.* 24	the height	79
23	of it was	74	25	of it was	81

The Air being transmitted into another Receiver, the height of the Mercury was 61.

Aug. 26. The height of the Mercury was 56. because some Air had got out, yet I transmitted the Air into another Receiver, and the Mercury remained in the height 52.

Aug. 27	the height	60	*Au.* 30	the height	83
28	of it was	68	31	of it was	88
29		75	*Sept.* 1		93

Septemb. 2. The height of it was 100.

Sept. 3. The height of it was 89, because some Air had escaped out, which made me cautious to prevent the like for the future.

Sept. 4. The height of the Mercury was 100.

5. The same height continued.

7. The same height still continued, though no Air at all had any egress.

9. The height of the Mercury was 107.

10. The height of the Mercury was the same.

The Air being transmitted into another Receiver, the Mercury staid in the height 99.

Sept. 11. The Mercury moved not.

13. The height of the Mercury was 105.

October 8. I this day found that the Air had got out.

This Experiment seems to inform us, that Pears do produce their Air, as it were by Paroxysms, or Fits.

ARTI-

ARTICLE XI.

Various Experiments.

EXPERIMENT I.

March 16.

I Melted down Lead with a fire in a Braſs Veſſel, whoſe Diameter was an inch and half; but before the Lead was concreted by cold, I put it into a Receiver, out of which I exhauſted the Air with great ſpeed; whence it came to paſs, that the figure of the concreted Lead, was concave, and the parts of it were ſo much the more depreſſed, by how much they were the nearer to the Center: whereas, on the other ſide, Lead congealed in common Air, doth exhibit a convex figure, except in the middle, where a little cavity doth appear.

I made the ſame Experiment with Tin, and had the ſame ſucceſs: though both Metals being liquid, and very hot, had remained long enough *in vacuo*, yet no bubbles ſeemed to emerge from either of them; whereas all other hot liquors do ſend forth numerous bubbles *in vacuo.*

EXPERIMENT II.

September 2.

I put Water ſaturated with diſſolved Salt, *in vacuo*, to trie whether it would be there converted into Chryſtals, and the Salt be carried above the plain, or ſuperficies of the Water, as it is wont to happen in the Free Air.

*Sept.*15. The Water with the diſſolved Salt, abiding in the ſame

ſame ſtate, I opened the Receiver; ſeeing no vapours could eſcape out of the evacuated Receiver, 'tis conſentaneous to Reaſon to judge, that the Salt could not there be converted into Chryſtals.

EXPERIMENT III.

Auguſt 8. 1676.

I put Air produced from Gooſberries, into an evacuated Recipient, furniſhed with a Mercurial Gage.

March 1. $167\frac{6}{7}$. When I perceived that no change was made in the height of the Mercury, I opened the Receiver.

EXPERIMENT IV.

Auguſt 8.

I took a Phial which was able to hold 7 ounces, 5 drams, and 3 grains of Water, and exhauſted the Air out of it; and when in a ballance it was ſuſpended in an *æquilibrium* with another weight, I pierced the bladder which covered the orifice, with a Needle, and then, the phial being filled with Air, appeared heavier by 4 grains and $\frac{1}{2}$, which latter weight to the former, is in the ſame proportion as 1 to 814; whence it follows, that Water is about 800 times more ponderous than that Air of an equal bulk. Yea, 'tis probable, that the proportion is with the leaſt, becauſe this day the Air was hot and clear, and beſides ſome Air was always left in the Receivers after the exhauſtion.

EXPERIMENT V.

Jan. 16. 1677.

I put *Aqua Fortis* with fixed Nitre into a Receiver, and, having exhauſted the Air as much as I could, I poured in one of them on the other, and found much Air produced. I marked the height of the Mercury in the Gage.

March

March 5. Finding that the produced Air was not deſtroyed, and that the Mercury perſiſted in the ſame height, I opened the Receiver, and found Nitre produced *in vacuo* from the mixture.

EXPERIMENT VI.

May 12. 1677.

I filled a Phial, of a long and very narrow neck with Oil up to the middle of the neck; and thus filled, I put it into a Receiver firmly ſtopped by the help of a Screw; into which afterwards I intruded Air till it could ſuſtain 120 digits of Mercury above its wonted height. And the Oil in the neck of the phial, appeared depreſſed toward the phial about one quarter of an inch; the cauſe whereof I judge attributable to the compreſſion of the Air; and yet having eaſed the Screw, and thereby ſuffered the Air to break in and be dilated, the Oil did not aſcend at all; ſo that I judge it was condenſed onely by cold.

Auguſt 5. I made the ſame Experiment after the ſame manner, onely uſing Water inſtead of Oil; and yet I could perceive no change of the height of the Water in the neck of the Glaſs, though the heat being moderate, might have produced a ſenſible effect.

*Jan.*14. 1678. Becauſe I found by ſome Experiments, that compreſſed Air did enter into the pores of the Water, and did pierce even to the bottom, a ſuſpicion might ariſe, that the Water was not condenſed by the compreſſed Air, for this reaſon, becauſe the Air entering into the pores, did make the preſſion within equal to the preſſion from without. And to be ſure of this, I filled the Glaſs aboveſaid with Spirit of Wine, leaving onely the length of 3 digits in the top of the neck thereof, which was filled with Air onely. Then my hands being applied to the Glaſs, the Spirit of Wine, being heated, in a ſhort time, filled the whole neck even to the top. Then the Glaſs being inverted into a

Veſſel

Vessel full of Mercury, I removed my hands, which being done, the Spirit of Wine being soon cooled; afforded space to the Mercury to fill 3 digits in height. I put the Vessel and the Glass in that posture, into a Receiver, into which I afterwards compressed the Air, till the Mercury exceeded its wonted height 90 digits, and yet there was no sensible condensation of the Spirit of Wine, nor any ascension of the Mercury; however it is certain, that no Air had crept in, because the Mercury hindered it; and the Receiver being opened, when the Air, that compressed from without, was dilated, no bubbles appeared in the Spirit of Wine.

In this Experiment, it seems worthy our Enquiry, how it comes to pass that Spirit of Wine was so sensibly condensed by a moderate cold, and not at all by a great compression of the Air.

EXPERIMENT VII.

May 12. 1676.

I poured Spirit of Wine into a Glass Vessel, and superadded some drops of Oil of Turpentine thereto, which swimming upon the Spirit of Wine, began to be whirl'd about by motion, hither and thither, as it is wont to come to pass. I put the Glass Vessel on the Pneumatick Engine, and covered it with a Receiver, and yet the bubbles did not at all cease to be moved up and down. Then I pump'd out the Air, till the Spirit of Wine did onely not bubble; and it came to pass, that the bubbles emerging from the Spirit of Wine, did adhere to the drops of Oil, and carried them with themselves to the sides of the Vessel, and there retained them; yet 2 drops, free from such bubbles, proceeded to have further motion: Afterwards I wholly exhausted the Receiver, and some drops were emitted to the top thereof, by the force of the bullient Spirit of Wine; but the remaining drops proceeded on to be moved a little, and in a little time after they rested. The Air being immitted, the drops began again to renew their motion, but it was a slow one, and it quickly ceased.

I iterated the same Experiment, with Spirit of Wine and Oil of Turpentine, cleansed from Air; and no ebullition was then made, yea no bubble appeared at all, but the drops of the Oil of Turpentine were moved *in vacuo*, as in the open Air.

Hence it seems to follow, that the cause of the motion of the drops is not to be ascribed to the dissolution, for all the dissolutions *in vacuo*, have hitherto seemed to me to produce bubbles.

EXPERIMENT VIII.

May 19. 1676.

I left yesterday 2 Radishes *in vacuo*, one of them I hanged up, the root being upside down, the other in a contrary posture; both of them cut transversly did hang over a subjacent Vessel, which contained red Wine. All these being left a whole night *in vacuo* seemed well purged from their Air. Opening the Receiver, I added 2 other Radishes to the former included ones, cut after the same manner and from which I had further detracted their thick skin. Then exhausting the Receiver, I immerged the cut part of all the Radishes at once, into the subjacent Wine: and then many bubbles seemed to arise out from them, as it came to pass in those little Glass-Tubes of *Experiment* IX. yea more bubbles were emitted from those Radishes, which were purged from Air the whole night, than from those which had not remained above half an hour *in vacuo*; and from whom I had taken away their skin.

This Experiment seems to afford us a confirmation, that Bubbles are formed of particles of Air, swimming in Water; and because in the skin there are some Canales, fit to retain parts of Air, it came to pass that the Radishes, from which I had detracted their skin, afforded no opportunity for the forming of so many Bubbles.

The

The liquor aſcended no leſs into thoſe Radiſhes which hanged with their roots upwards, than into thoſe of a contrary poſture.

EXPERIMENT IX.

May 4. 1676.

I immerged one end of a ſmall Glaſs tube, open at both ends, into Water ſtagnant *in vacuo*, and preſently the Water aſcended up into it, as it is wont to do in common Air, and even to the ſame height; but a little while after, many Bubbles being formed there, lifted the Water higher, and kept it ſuſpended in 3 different places, diſterminated by many Bubbles; and many other Bubbles ſeemed to paſs out from that end, which was immerſed in Water.

Then I ſealed the other end of the tube Hermetically; and ſo the Experiment being made in common Air, the Water could not aſcend up into the tube by the open end. But *in vacuo* the matter ſucceeded far otherwiſe; for the Water aſcended up into the tube, no otherwiſe, than if it had been open at both ends; and many Bubbles formed in a ſhort time, did diſtinguiſh the Water, contained in the tube, by great intervals, as before, whileſt the mean time, many other Bubbles ſeemed inceſſantly to paſs out from the end of the tube, immerſed in Water, yet in progreſs of time, they appeared leſs frequent.

But this circumſtance I much admired, that the Water being ſuſpended higher in the tube, ſeemed to be filled with no Bubbles, whereas the end onely did emit ſo many.

Then I took out that end from the Water, and no Bubbles did any more appear, though that end was wholly filled with a Cylinder of Water.

May 5. I repeated the ſame Experiment; but before I had immerged the end of the tube in Water, a drop of Water which ran over from the ſuperiour aperture of the Receiver, flowed down to the open end of the tube, and pierced up into it the height of 2 lines, neither was any Bubble formed there in a full half hours time: that being paſſed, I immitted the end of the tube into the Water of the Veſſel, and not long after, Bubbles began to be formed, as before, of which ſome followed others within half a minute; yet afterwards they came forth leſs frequent. Furthermore, iterating this Experiment many times, I perceived, that when the Water was extracted from the tube, no Bubbles appeared: but if it were immerged in Water, Bubbles would cleave to the end of it, either ſooner or later.

May 6. I tried the ſame Experiment, with the infuſion of Nephritick-wood, and the ſucceſs was wholly alike, but that the Bubbles could emerge and pierce the liquor, before they had acquired any bigneſs, for being yet very ſmall, they pervaded the liquor, contained in the tube, and were carried to the upper part thereof: whence we may conjecture, that that liquo is very thin, and hath no viſcoſity to reſiſt the pervading Body.

May 10. I iterated the ſame Experiment with Spirit of Wine, mixed with a certain Oil, made *per deliquium*: yet I found no new event, but that the aſcenſion of the liquor into the tube, was not ſo high.

From theſe Experiments it ſeems to follow, that the Bubbles are formed, in the extremity of the tube of aerial particles, ſwimming in the Water, which finding ſome impediment at that end, cannot paſs by, and ſo, new ones coming upon them, they ſwell into a Bubble.

EXPERIMENT X.

July 18. 1676.

Two days ago I took ſome Beans, ſuch as are given to Horſes for Provender, and included them in an iron tube cloſely ſtopped; yet I firſt affuſed Water on the compreſſed Beans, till the tube seemed wholly full; to try whether the force of the swelling Beans would be enough to break the tube. This day the tube seemed not to be altered at all, but the stopple being plucked back, some quantity of Air brake out; and much Water fell upon the ground, which was not sucked up by the beans; then a certain noise, as it were, of bubling Water, was heard for a whole hour and more.

July 25. I left the iron tube in the same posture, but this day one of the ends of it being unstopped, and some Beans taken out, the murmur of the bullient Water was heard, as before.

From this Experiment it seems to follow, that Beans do contain Air in them, which in a great compression cannot escape out; but if it be freed from the force compressing it, then it makes an eruption.

EXPERIMENT XI.

March 4. 1677.

I put a Glass half full of Spirit of Sal-Armoniack and *limatura Cupri*, into a Receiver exhausted as much as I could, and there stopped it in. And it came to pass, that in 15 minutes space the liquor had contracted a certain blew colour, but very much diluted; but, the Air being immitted, in 3 minutes, the blew colour appeared vivid and thick. I put the liquor so tinged again *in vacuo*, to trie, whether in tract of time that colour would vanish.

April 4. The blew colour was almost quite vanished, but upon the admission of the Air, it quickly returned.

EXPERIMENT XII.

May 8.

I put a certain Oil made *per deliquium*, with Spirit of Wine into

into an exhaufted Receiver, and the Spirit always fwam on the top; now left the Spirit might be fpilt by bubling above the edges of the Veffel, I extracted the Air by degrees, and in the beginning great Bubbles came from the Spirit, and but very fmall ones from the Oil; but after one hours time, the Oil did emit great Bubbles, which being fmall at bottom, in their afcent did fill the whole latitude of their Veffel; and after another hour, fome Bubbles brake out with fo great force, that they hit againft the top of the Receiver.

May 9. I iterated the former Experiment in a Glafs fomewhat long and narrow, that I might the better perceive the motion of the Bubbles; and then I faw the Bubbles paffing out of the Oil into the Spirit of Wine, without any great increafe of their quantity; but being diftant onely 1 quarter of an inch from the fuperficies, they were fuddenly expanded.

EXPERIMENT XIII.

May 3. 1676.

I mixed a certain quantity of *Aqua Fortis* with a quantity of *Spirit of Wine* fomewhat greater; and then I diftributed that mixture equally into 3 Glafs Veffels, and put three equal pieces of Iron into them, to each Veffel one. This being done, I included one of the 3 Veffels *in vacuo*, and there many great ebullitions were made. Then after a quarter of an hour, I took out the Veffel, and found the liquor black and turbid, whereas the other two Veffels had their liquor not altered in colour, but onely fome black powder did appear in the bottom of the liquor.

Of thefe 2 Veffels, I put one *in vacuo*, and then there arofe ebullitions, great indeed, but much leffer than the former: when one quarter of an hour was elapfed, I took the Veffel *è vacuo*, and found the liquor black indeed, yet fomewhat lefs fo than the former; but the liquor which was left always in the Air, did in a manner remain unchanged

May 4. This day in the morning the liquors in the 2 Vessels, put *in vacuo*, appeared cleansed and green, and had no other operation.

But the liquor which was not put *in vacuo* did bubble more strongly than yesterday, and exhibited a red colour. I put the 3 Vessels together *in vacuo*, and perceived no eminent ebullition, onely some Bubbles appeared larger in the red liquor, than in the other two.

From this Experiment it seems to follow, that Spirit of Wine *in vacuo* doth accelerate *ebullition*.

EXPERIMENT XIV.

Jan. 21. 1678.

I kept a Glass half full of Sal Armoniack, and *filings of Copper*, the hole thereof being so exactly stopped, that the blew colour, which was induced into that liquor, from the contact of the external Air, (*See Philosophical Transactions, Num.*120.) did wholly now disappear. The stopple was made of Leather, prepared after a special way and manner.

I put that Glass *in vacuo* with Paste not yet fermented.

I did it to this end, that the Receiver, being full of Air from the Paste, I might perforate the leather that stopped the Glass, with an Iron Wire prepared for that purpose; and that I might trie, whether the contact of the Air generated from the Paste, would also communicate some colour unto the liquor.

Jan. 22. There was no need to perforate the Leather, for this day I found the liquor already tinged; so that it is probable, that Air produced from Paste, is endued with such minute particles, that it can penetrate Leather which is impervious to common Air.

Yet I will keep the Glass, not touching its ligature, to trie, whether that colour may vanish again.

Jan. 25. Now the liquor became almost colourless, whence it it appears, that common Air is too thick to penetrate all paſſages, which are pervious to Air, produced from Paſte.

Feb. 2. I put the ſame phial *in vacuo*, but did not fortifie the commiſſure of the Receiver with the Cover, with Turpentine, ſo that the Air making a gradual ingreſs, in 24 hours filled the Receiver, even as it was leiſurably filled, with the Air produced from Paſte, yet the liquor remained ſtill colourleſs.

Feb. 15. I put the ſame Glaſs again *in vacuo* with ſome quantity of Paſte; but this time the Air produced from thence, did not pervade the Leather, as it had done before, and the liquor was not tinged at all.

EXPERIMENT XV.

April 2. 1678.

I put a Shrew-mouſe into the Engine deſcribed *p.*13,14. and when I perceived he was reduced to extremity, I began to ſtir the Pump, that the Air might penetrate, and be, as it were filtrated through the Water. The Mouſe a while after, ſeemed to be better, yet he could not be wholly reſtored to health. Now becauſe he had been long kept faſting, I am uncertain whether he died for want of Aliment, or of new Air.

April 12. I iterated the ſame Experiment with a ſmall and weakly Mouſe, that had been kept a long time faſting. And finding that this Experiment had the ſame ſucceſs with the former, I took out the Mouſe, before he was dead; and though he then enjoyed the Free Air, yet he recovered not; ſo that we have need of more Experiments, that we may attain to a certain knowledge of the effect of that Filtration.

EXPERIMENT XVI.

May 2. 1678.

Six Weeks ago, I included Frog-Spawn in 3 Recipients; the

the first of which was *vacuous* ; the second contained common Air ; and into the third, I intruded so much Air, that the Mercury staid in 60 digits above its wonted height.

In 15 days the Mercury in the evacuated Receiver came to the height of 1 digit. The Spawn in the common Air seemed corrupted and of a blackish colour ; but that in the compressed Air, remained unaltered in colour; but no Frogs were generated.

After an whole month was elapsed, the Sperm *in vacuo* had not changed its colour, excepting the black round spots, but seemed reduced into Water: the colour of that in the common Air was very black, but in the compressed Air the Spawn began to be reddish.

As yet no change was perceived, neither in that Spawn *in vacuo*, nor that in the common Air; but in the compressed Air, the Spawn waxed more and more red.

May 22. The Sperm *in vacuo* was not changed ; in the compressed Air it remained red ; but in the common Air it became again colourless.

June 23. The Sperm *in vacuo* and in common Air was tinged with no colour, but in the compressed Air it inclined to greenness.

Octob. 15. I took out all the Spawns ; that which was kept *in vacuo* was almost exhaled out of its Vessel, and was stagnant in the Receiver, like clear Water: In the common Air, the Sperm remained colourless ; but that in the compressed Air kept still its red colour.

EXPERIMENT XVII.

May 9. 1678.

Six days ago, I included two pieces of the same Orange in 2 Receivers, not quite of equal bigness, but in the greater Receiver, there was left some quantity of Water, so that no less space was left for the Air in that, than in the leſſer. The iſſue was, that the Orange included with Water, though it were not touched by it, yet was 4 times more mouldy than that which was kept without Water.

And therefore in iterating this Experiment, I put 2 pieces of the ſame Orange into 2 Receivers, but I filled the third part of one of them with Water, yet ſo, that it did not reach the Orange.

June 15. Neither of the Oranges had contracted any mouldineſs.

May 16. I repeated the ſame Experiment with the ſame ſucceſs, yet neither Orange had acquired any mouldineſs in the ſpace of more than a month, though in former Experiments all ſuch Oranges would be mouldy.

The cauſe of the difference ſeems to be attributable to ſome diſpoſition of the Air.

EXPERIMENT XVIII.

June 1. 1678.

I put a ſmall Glaſs-tube, half full of Venice Turpentine, into the Wind-gun, deſcribed p. 16. and I had ſcarce reduced the Air to the tenth part of its wonted ſpace, but the Leather, ſpread over the Elliptick valve, was driven out; ſo that the Air having made an eſcape, I drew the Glaſs-tube out of the Engine, and found many Bubbles formed in the ſuperficies of the Turpentine; and therefore I ſuſpected that the Air had pervaded the Turpentine, and that it would have penetrated more deeply into it, if they had remained longer thus encloſed together: and therefore I re-immitted the ſame Tube into the ſame Gun, and there left it in Air reduced to about the 15 part of its ſpace.

June 3. I opened the Engine, and, taking out the Tube, found the Turpentine almoſt free from Bubbles, yet by degrees many were

were formed therein, in the parts remote enough from the superficies.

June 4. I threw away the former Turpentine, and put new in the same Tube, and included it *in vacuo*, that the Turpentine might be the better purged from all Air; then I poured Water upon it, and shut up all in the Wind-gun.

June 8. I opened the Engine, and at first sight, both the Water and the Turpentine in the Tube, seemed to be very free from Bubbles; but a little while after I perceived, that Bubbles were formed in the Turpentine, and that they ascended by degrees; yea, some of them seemed to be made almost in the very bottom, about half an inch below the superfices of the Turpentine. So that we may conjecture, that all the Water, and so great an height of the Turpentine, were penetrated by the Air, which formed those Bubbles.

EXPERIMENT XIX.

August 11. 1678.

I included Spirit of Sal Armoniack, with a Mercurial Gage *in vacuo*; and after that the Spirit ceased to emit any Bubbles, I mixed Filings of Copper therewith, which caused many Bubbles to break forth again; but they were so far from producing any Air, that they contrariwise consumed that which was there before. As it hath been already observed in the Philosophical Transactions, N. 120. But the liquor was made greenish and turbid.

Decemb. 5. The Spirit was almost all exhaled out of the Vessel, in which it was contained, and being condensed in the Receiver, remained still turbid, by reason of much filth which was included there: but that which was not exhaled out of the Vessel, appeared clear like Water. Also the Mercury was wholly expelled out of the Gage. Whence I conjecture, that the Air in the Receiver, was more and more consumed.

EXPERIMENT XX.

September 2. 1678.

I put 2 Cylinders, one of Tin, the other of Lead, *in vacuo*; but their loweſt parts were immerſed in Mercury; and at the ſame time I immerſed 2 other Cylinders, like the former, after the ſame manner in Mercury: but theſe latter were left in the free Air.

Sept. 6. I opened the *vacuous* Receiver, and the Mercury in the Tin Cylinder, was come to the height of 4 digits and an half above the ſuperficies of the ſtagnant Mercury; and cutting the Cylinder tranſverſly, in the middle of that height, the Amalgama ſeemed to have penetrated into the Cylinder, about half a line. And cutting the Cylinder tranſverſly again, in that part which was diſtant onely 1 digit, from the ſuperficies of the ſtagnant Mercury, I found the thickneſs of the Amalgama to equal one line.

In the Lead Cylinder the Mercury came to the height of 2 digits and $\frac{1}{2}$, but only as far as the ſuperficies, and that very part which was immerſed in the Mercury, was not penetrated by it to any ſenſible thickneſs.

Sept. 7. I took out the Tin, left in the Air, out of the Mercury in which it was immerged, and found the Mercury to have aſcended to the height of 5 digits.

Sept. 10. The ſame Cylinder being left in the Mercury, ſeemed to be beſmeared therewith up to the very top, 6 digits and more, above the ſuperficies of the ſtagnant Mercury. When the Cylinder was tranſverſly cut in ſeveral places, it appeared that the Mercury had pierced ſo much the higher into the Tin, by how much it came nearer to the ſtagnant Mercury; ſo that in the part near to the foreſaid Mercury, almoſt the whole diameter of the Cylinder, 3 lines broad was penetrated thereby.

In

In the Lead Cylinder, the Mercury exceeded not the height of 3 digits and $\frac{1}{2}$, neither had it penetrated to any ſenſible thickneſs. Whence it appears, that the weight of the Air, contributes little or nothing to the aſcenſion of Mercury into Metals.

EXPERIMENT XXI.

Decemb. 12. 1678.

I took a ſmall Whiting, and having cut off his head, I divided him tranſverſly into 5 pieces. The firſt whereof I included *in vacuo*. The ſecond in common Air. The third in Air ſo compreſſed, that it could ſuſtain Mercury 50 digits above its wonted height. Theſe 3 Receivers were cloſed with Screws. The fourth piece was put into a Receiver, full of Air produced from Paſte, which was preſently ſtopped. The fifth was left in the Free Air.

Decemb. 15. This day in the Morning, that part of the Whiting, which was left in the free Air, began to ſhine; and towards Evening it ſent forth ſomewhat a more vivid light.

Decemb. 16. In the Morning, the Whiting left in the Free Air, gave over ſhining; but towards Evening it ſhone again.

Decemb. 17. This Morning the ſame part of the Whiting ſhined a little, yet leſs than it did yeſterday in the Evening.

Decemb. 18. In the Morning there appeared no light, though I fixed my eyes a long time upon the Receiver in a dark place; but the Night coming on, the light appeared again.

Decemb. 20. Hitherto the ſame part of the Whiting left in the Air, proceeded to ſhine; but all the other parts did not yet begin to ſhine.

Decemb. 22. Yeſterday the light of the Whiting left in the Air, had not quite ceaſed, but this day it appeared no more.

Decemb. 24. The part of the Whiting in the Free Air, gave over its ſhining quite; but that which was included with com-

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mon Air, did yesterday send forth a faint light; but this day it proceeded not to shine.

Decemb. 26. No shining appeared any more in the common Air: but the three other pieces did not so much as begin to shine.

Jan. 26. 1679. I perceived no more shining in any one of the Receivers.

ARTICLE XII.

Artificial Air destroyed.

EXPERIMENT I.

August 3. 1677.

I Transmitted Air produced from Cherries, into a Receiver full of Common Air, but so stopped with a Screw, that the Mercury ascended to 25 digits above its wonted pressure.

Aug. 4. The Mercury was depressed about 2 digits. The height of it this day was onely 23 digits.

Aug. 6. The height thereof was reduced to 20 digits.

Aug. 7. The height thereof was the same.

Aug. 8. The Mercury was somewhat depressed.

Aug. 10. The height of it was $19\frac{1}{2}$ above its wonted height. When I perceived little or no alteration more, I opened the Receiver.

From this Experiment we have a confirmation, that Air produced from Fruits, at the beginning is in part destroyed; but the rest can keep the form of Air very long.

EXPERIMENT II.

May 26. 1676.

I took 6 grains of Sal Armoniack, and put them into a Receiver, with a sufficient quantity of Oil of Vitriol: then the Air being exhausted, I forced down the Salt into the Oil; whereupon a great ebullition presently followed, and the Mercury ascended into the Gage, almost to its wonted height; but presently after it sunk again, and returned to its former state.

May 27. I repeated the same Experiment, but this time the Salt remained 10 hours *in vacuo*, before it was put into the Oil; but the ebullition followed, as in the former Experiment; yet the Air was produced much more slowly, neither could it wholly be destroyed, but in 7 or 8 hours time; yet at last the Mercury descended to the very bottom.

May 29. I tried the same Experiment again, leaving the Materials 24 hours *in vacuo*: This time the ebullition seemed much less, and the Air was produced both in a lesser quantity, and more slowly than before. I observed also, that whilest the Materials staid *in vacuo*, before their mixture, that the Mercury came nearer to the open end of the Gage, as if some Air had been either extracted or destroyed.

July 8. I put Oil of Vitriol alone into a Receiver, in which I left onely a fifth part of common Air, to trie whether this Oil, without Sal Armoniack, would diminish the elastical force of the Air: but it fell out contrary, that the force of the Air was increased, and the Mercury in one hours space, seemed to have ascended a little into the Gage; but afterwards for 24 hours space no change was made.

This Experiment doth confirm, that some Artificial Airs, may be destroyed; but why this destruction happens sometimes sooner, sometimes slower, it may perhaps seem worthy of a further enquiry.

ARTI-

ARTICLE XIII.

Experiments concerning the different celerity of Air produced in vacuo, *or in common Air.*

EXPERIMENT I.

COMMON AIR.

July 10. 1676.

I Put Paste, kneaded two days before, and sowerish, into a Receiver, and stopped it firmly with a Screw.

In one hours space the height of the Mercury was one digit.

In 7 hours space the height of it was 6 digits.

July 11. The height of it was 11 digits.

12. The height of the Mercury was 24.

13. The height thereof was 30.

14. The height of the Mercury was sensibly greater.

15. The Mercury ascended a little. Measuring its height exactly this day, I found it 38 digits.

19. No more Air was produced from the Paste.

VACUUM.

July 10. 1676.

I put another quantity of the same Paste, much less than the former, into an exhausted Receiver.

Though the quantity of the Paste was less, yet in one hours time, the height of the Mercury was 2 digits.

In

In 7 hours time, the Mercury came almoſt to the top of the Gage, but it was a ſhort one.

July 19. The Paſte was not able to remove the Receiver from his Cover, though at the beginning it had produced a greater quantity of Air than the Paſte in common Air. I endeavoured to fire it with a burning-glaſs, and the Fumes, elevated therefrom, afterward falling upon the Paſte, did tinge the ſuperficies thereof, with a pleaſant yellow colour; and that Air was thus produced, I conjectured hereby, becauſe the Cover was afterwards eaſily ſevered from its Receiver.

From this Experiment made in 2 Receivers at once, we learn, that Air is ſometimes generated much more eaſily *in vacuo* than in common Air.

EXPERIMENT II.

COMMON AIR.

Auguſt 20. 1676.

I put Paſte, kept for 24 hours, into a Receiver full of common Air; to which I further added new Air, ſo that the Mercury exceeded its wonted height 4 digits and $\frac{1}{2}$.

In 6 hours ſpace the Mercury gained almoſt 4 digits. Its height was 8 digits.

Auguſt 21. The aſcenſion of the Mercury was 4 digits and $\frac{2}{3}$.

Aug. 22. The aſcent of it was about 1 digit.

23. The aſcent of it was $\frac{1}{2}$ a digit.

26. For 3 whole days the aſcent of the Mercury was onely $\frac{1}{2}$ a digit.

27. There was no aſcent of it at all.

29. The Paſte taken out of the Receiver, affected our Noſtrils with an acid ſmell.

VACUUM.

Auguſt 20. I put another quantity of the ſame Paſte into an empty Receiver, and kept the ſame proportion between the quantity of the Paſte and the capacity of the Veſſel, as in the former Experiment.

The Mercury ſeemed to have aſcended in a ſhort time. Its height was 2 digits.

Aug. 21. The aſcent of the Mercury was 5 digits.
22. The aſcent of it was 3 digits.
23. The aſcent of the Mercury was 1 digit.
26. For three whole days the aſcent of it was 2 digits.
27. There was no aſcent of the Mercury.
28. I took out the Paſte exhauſted of its Air, from the Receiver.

This Experiment confirms to us, that Air is ſometimes more eaſily produced *in vacuo*, than in common Air.

EXPERIMENT III.

VACUUM.

Septemb. 4. 1677.

I put the Kernels of Filberds into an exhauſted Receiver.

Sept. 5. The height of the Mercury was 5 digits.

Sept. 6	The height of it was	10	*Sept.* 11.	The height of it was	18
7		10	12		23
8		12	13		27
9		15	14		29

Sept. 15. The height of it was almoſt the ſame.
17. The height of it was 30.
18. This day the Air began to eſcape out of the Receiver, for ſome Bubbles appeared in the Turpentine, which ſtrengthened the Commiſſure of the Receiver and Cover.

Com-

Common Air.

September 4.

I put Kernels of Filberds into a Receiver with Common Air.

In the Afternoon the quantity of Air ſeemed to be leſſened.

Sept. 5. The height of the Mercury, was leſs than half a digit.

6. The height of it was the ſame.
7. The height of it was 1 digit.
8. The ſame height ſtill continued.
18. The ſame height continued.

This Experiment gives us a confirmation, that ſometimes Air is produced much more eaſily *in vacuo* than in Common Air.

EXPERIMENT IV.

September 15. 1677.

I included 8 ounces of Raiſins of the Sun, bruiſed and diluted with a little Water, in an exhauſted Receiver, able to hold 22 ounces of Water.

Sept. 16. The height of the Mercury was 6 digits.

Sept. 17	The height of it was	10	*Sept.* 19	The height of it was	29
18		15	20		29

Sept. 21. This day I found the Receiver forced from his Cover.

Sept. 24. I took out ſome of the Raiſins; but thoſe that remained, I encloſed in the ſame evacuated Receiver.

Sept. 25. The Raiſins forced the Receiver, now full of Air, from his Cover.

September 15. 1677.

I put 8 ounces of Raiſins of the Sun, bruiſed and diluted with a little Water, into a Receiver, able to hold 22 ounces of of Water; but I did not exhaust the Air at all.

Sept. 16. The height of the Mercury was $\frac{3}{4}$ of a digit above what was accustomed.

Sept. 17. The height of the Mercury was $1\frac{1}{2}$.

18. The height of it was 3.

Sept. 19	The height of it was	5	*Sept.* 22	The height of it was	11
20		7	23		12
21		9	24		15

Being about to put Peaches into the Receiver, I permitted the Air to break forth; and then many Bubbles did emerge from the Raisins.

This Experiment doth further teach, that Air is sometimes much more easily produced *in vacuo* than in common Air.

EXPERIMENT V.

VACUUM.

February 17. 1677.

I put 3 Onions into an emptied Receiver.

Febr. 19. The ascension of the Mercury was 1 digit.

21. The ascent thereof was again 1 digit. The Onions were not altered.

25. The whole ascent of the Mercury was 9 digits The Onions not altered.

May 4. The Onions had yet undergone no alteration.

18. Neither were they yet altered.

June 19. I this day found the Receiver, forced from his Cover, and the Onions rotten.

RARIFIED AIR.

Febr. 17. I inclosed 3 Onions in Air so rarified, that it could ustain onely 10 digits of Mercury. *Feb.*

Feb. 19. There was no aſcent of the Mercury.

21. There was yet no aſcent thereof. The Onions did not germinate, but contracted a mouldineſs.

25. The aſcenſion of the Mercury was about 7 digits. The Onions received no further alteration.

May 4. The Onions were not altered.

18. The Onions were not yet altered, but the Receiver, by the force of the produced Air, was removed from his Cover.

Common Air.

February 17. I put three Onions in a Receiver not exactly ſhut.

21. The Onions contracted no mouldineſs, but did germinate.

25. The Onions put forth root more and more.

May 4. The Onions began to be mouldy.

This Experiment gives us a likely proof, that *ſome* Bodies do produce their Air not much more eaſily *in vacuo*, than in rarefied Air.

And beſides it hereby appeareth, that Vegetation is hindred, not onely by the evacuation, but alſo by the rarefaction of the Air.

It ſeems alſo worthy our obſervation, that the Onions, as long as they emitted roots, did contract no mouldineſs.

ARTI-

ARTICLE XIV.

The difference betwixt whole, or entire, and bruised Fruits.

EXPERIMENT I.

Bruised Fruits.

August 23. 1677.

I Put Pears bruised into a vacuous Receiver, with a Mercurial Gage.

August 25. The height of the Mercury, was 5 digits.

Aug. 26	The height of it was	10	*Aug.* 29	The height of it was	21
27		14	30		25
28		18	31		28

Sept. 1. The height of it was 30.
2. The Receiver was found forced from his Cover.

Whole, or Entire Fruits.

August 23. I put whole Pears into a *vacuous* Receiver, and I took care that the quantity of the Pears, and the capacity of the Receiver, might be the same with those which I mentioned before.

Aug. 25. The height of the Mercury was 11.

Aug. 26	The height of it was	17	*Aug.* 28	The height of it was	28
27		25	29		30

Aug. 30. The Mercury ascended no higher, because the Receiver was forced from his Cover.

This

This Experiment ſeems to prove, that Bruiſed Fruits do not produce air as ſoon as Entire ones.

EXPERIMENT II.

Entire Fruits.

Auguſt 24.

I encloſed whole Apples *in vacuo* with a mercurial Gage.

Auguſt 25. The height of the Mercury was 5 digits.

Aug. 26	The height of it was	9	*Aug.* 29	The height of it was	19
27		12	30		25
28		15	31		28

September 1. The height of it was 29.

2. The height of it was 30.

3. The Receiver was forced from his Cover.

Bruiſed Fruits.

Auguſt 24. I put an equal quantity of bruiſed Apples into a vacuated Receiver, of the ſame capacity with the former.

Aug. 25. The height of the Mercury was 1 digit.

26. The height of it was 3 digits.

27. The height of it was 4.

Sept. 3. The Mercury continued in the ſame height.

25. The Mercury aſcended not at all.

This Experiment ſeems to inform us, that *bruiſed* Fruits do produce air, ſlower than *whole* or entire ones.

EXPERIMENT III.

Bruiſed Fruits.

Aug. 25. 1677. I put unripe Grapes bruiſed, into a vacuated Recipient.

Aug.

Aug. 26. The height of the Mercury was 1 digit.
27. The height of it was 2 digits.
28. The height of it was 2 digits and an half.
29. The height of the Mercury was the same.

Sept. 15. The Mercury did not ascend at all, but its height remained at 2 $\frac{1}{2}$.

WHOLE FRUITS.

August 25. 1677. I put unripe Grapes, not bruised, into a vacuated Receiver.

Aug. 26. The height of the Mercury was 3 digits.
27. The height of the Mercury was 5 digits.

Aug. 28	The height of it was	7	*Aug.* 30	The height of it was	12
29		10	31		13

Sept. 1. The height of the Mercury was 15.
2. The height of it was 16.
3. The height of it was 18.
4. The height of it was the same.

Sept. 5. The height of the Mercury continued the same; but all the Grapes had almost contracted a yellow colour.

Sept. 7. The Mercury rested in the same height; but all the Grapes were yellow.

Sept. 15. The height of the Mercury was 20.

This Experiment gives us a further confirmation, that *whole* Fruits do produce air, more readily then bruised ones.

EXPERIMENT IV.

FRUITS WHOLE AND ENTIRE.

September 10. 1677.

I put 2 ounces of ripe Grapes, but not bruised, into a Receiver able to hold 10 ounces of Water.

Sept.

Sept. 11. The height of the Mercury was 6 digits.

Sept.	The height of it was	*Sept.*	The height of it was
12	9	15	20
13	12	16	25
14	15	17	28

Sept. 18. The height of the Mercury was 30. The Grapes were not altered at all.

Sept. 19. The height of the Mercury was the ſame.

20. The Receiver was not yet forced from his Cover. The Grapes were not altered, but appeared onely a little riper.

21. The Receiver was forced from his Cover, though as yet nothing had made any eruption out.

22. This day in the Morning, I found the Grapes begin to rot, and therefore I included them again *in vacuo.*

Sept. 23. The height of the Mercury was 5 digits.

Sept.	The height of it was	*Sept.*	The height of it was
24	9	27	20
25	14	29	27
26	17	30	28

Octob. 10. The Receiver was not forced from his Cover, till this day: the Grapes by their colour ſeemed rotten, yet they had kept their firmneſs.

BRUISED FRUITS.

Sept. 10. 1677. I included two ounces of ripe and bruiſed Grapes in a Receiver capable of holding 10 ounces of Water.

Sept.	The height of it was	*Sept.*	The height of it was
11	4	15	15
12	7	16	18
13	10	17	20
14	12	18	25

Sept. 19. The Grapes had ſevered the Receiver from his Cover, and much juice was ſpilt.

Sept. 20. I again put the ſame Grapes into the ſame Receiver; but becauſe they had ſpilt their juice by ebullition, I did not exhauſt all the Air, but the Mercury ſtaid in the height of 5 digits.

Sept. 21. This day in the Morning, the Receiver, being now full of Air, did no longer ſtick to his Cover; ſo that I took out the Grapes, and tranſmitted them into another Receiver, which I ſtopped cloſe with a Screw, but extracted no Air from it.

Sept. 22. The height of the Mercury was 11 digits, though the Receiver was able to hold 26 ounces of Water.

Sept. 23. The height of the Mercury was 19.

24. The height of it was the ſame.

30. The height of it was 20.

Octob. 3. When the Grapes produced no more Air, I took them out, and found them of a bitter taſte, becauſe they were not yet come to their perfect ripeneſs.

This Experiment, if you compare it with *that*, which I related before concerning unripe Grapes, doth ſeem to intimate, that unripe Grapes do produce leſs Air when they are bruiſed, than when unbruiſed; but ripe Grapes do the contrary.

EXPERIMENT V.

Nov. 19. 1678.

I put Apples into 3 vacuated Receivers. In the firſt was a ſound Apple; in the ſecond, an Apple bruiſed, and repoſited looſly in the open Veſſel: In the third was alſo a bruiſed Apple, and repoſited in the Veſſel, but the Cover was ſo fitted to the Veſſel, that it did ſtraitly compreſs the parts of the Apple. For I was deſirous to know, whether the *bruiſed* Apple would produce Air *in vacuo*, as well as the *ſound* one, provided his parts were narrowly conjoined; but the iſſue was, that in the exhauſting of the Receiver, the Air, formed between the parts of the Apple, did expel all the juice.

Nov. 21. In the firſt Receiver the height of the Mercury

was

was 5 digits; in the second, 3 digits; in the third, none at all.

Nov. 23. In the first Receiver the height of the Mercury was 7: in the two others there was no change.

Decemb. 7. In the first Receiver the height of the Mercury was 11 digits. There was no alteration in the other two.

Jan. 23. The first Receiver was now severed from his Cover, by the force of the Air produced anew. In the two others there was no Air generated.

May 20. 1679. This day the third Receiver was found forced from his Cover: whereas the second had produced no Air.

This Experiment informs us, that *bruised* Fruits do produce less Air *in vacuo*, than *sound* ones; contrary to what happens in common Air. The reason whereof may perhaps be this, that Fruits bruised are very much rarefied *in vacuo*, and so the several principles, of which they consist, cannot act upon one another: but unbruised Fruits, by reason of the entireness of their ambient skin, undergo less rarefaction.

ARTICLE XV.

Air is sometimes found unfit to produce mouldiness.

EXPERIMENT. I.

July 12. 1678.

I Put Roses into two Receivers, which were to be stopped with Screws. One of them contained common Air uncompressed; but I intruded so much Air into the other, as sustained the Mercury 60 digits above its wonted height.

August 2. The Roſes in the common Air; 4 days ago, were turned into a yellow colour, as if they had been withered: but thoſe in the compreſſed Air kept their colour very well.

Febr. 10. 1679. The Roſes in the compreſſed Air, as yet retained their freſh colour.

This Experiment, compared with that which was made the Year before with Roſes, doth inform us, that the Air at divers times is diverſly affected; ſo that ſometimes it hath a power to hinder corruption, and ſometimes to promote it. See *Artic.* IV. *Exper.* IV.

EXPERIMENT II.

May 22.

Fifteen days ago I included two equal quantities of Flowers, in two Receivers: Into one of them I thruſt ſo much Air as ſuſtained the Mercury 60 digits above its wonted height; but in the other, I left common Air incompreſſed. The Flowers were Tulips and Larkſpurs.

Since that time no mouldineſs appeared, except onely that 10 days ago, one half of a Tulip, being cut in two, in the common Air, ſeemed ſomewhat mouldy: but this day, the other half of the ſame Tulip in compreſſed Air, ſeemed to be infected with ſome mouldineſs.

As for the Flowers, ſome of them ſeemed as freſh, as when they were firſt put in; eſpecially thoſe in the common Air; for in the compreſſed Air, they ſeemed more moiſt.

June 22. No more mouldineſs appeared: whence we have a confirmation of the Inference drawn from the former Experiment, *viz.* That the Air is ſometimes unfit to produce mouldineſs; ſeeing the year before, all thoſe kind of Flowers had contracted a great deal of mouldineſs.

ARTICLE XVI.

Experiments concerning the change of weight, made by the Beams of the Sun, even in Vessels sealed Hermetically.

EXPERIMENT I.

Sept. 4. 1678.

I Exposed one drachm of Minium, in an open Glass to the Sun Beams concentrated in a Burning glass, and I found that it had lost $\frac{3}{4}$ of a grain of its weight, though much of the *Minium* had not been touched by the Solar-rays.

EXPERIMENT II.

September 6.

I took Coral, already calcined in fire, and endeavoured to calcine it further by the Beams of the Sun, in a sealed Glass, but I could scarce produce any good effect; yet the whiteness of the *calx* of the Coral was somewhat increased.

Sept. 10. I exposed the same Coral again to the Sun-Beams in the same Glass Hermetically sealed, for two whole hours; and weighing the Glass: found that the loss of its weight, was about $\frac{1}{16}$ part of a grain, since the time it was first sealed.

EXPERIMENT III.

May 23.

I put *Calx* of Tin in a light glass phial, sealed Hermetically

cally, and weighed it exactly: afterwards I exposed it to the Beams of the Sun for a long time, by the help of a large Burning-glass; then the Glass, being again weighed, seemed to have lost $\frac{1}{64}$ part of a grain of its weight.

May 29. I repeated the same Experiment, onely using *Minium* in stead of *Calx* of Tin, and the loss of weight came to $\frac{1}{32}$ part of a grain.

May 30. I endeavoured to burn the same Minium again, but such plenty of Air was produced, that the Glass broke into an hundred pieces, and made a great noise at its dissilition.

June 6. I tried the same Experiment again with *Minium*, and then $\frac{1}{64}$ part of a grain was abated of the weight.

When I attempted again to burn the *Minium*, the Glass broke a second time.

July 15. I took Coals made of Wood for the same Experiment, but the Sun did not affect them at all.

July 20. I exposed *Vive Sulphur* to the Beams of the Sun, after the manner before described; and though it was easily melted, and did emit many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the same phial still with the Flower of Sulphur, and exposed it often to the fire of my Burning-glass, without danger of being broken, *viz.* because Sulphur produceth no Air; but the Fumes were emitted, as at the first, and the Sulphur bubbled up; but the weight seemed not to be changed.

ARTI-

ARTICLE XVII.

The Preservation of Bodies in compressed Liquors.

EXPERIMENT I.

August 3. 1678.

I Included two Apricocks in two Receivers, one of which was exactly filled with Raisins of the Sun bruised, and with Water; but in the other, there were onely some Raisins enclosed, yet so that the Apricock was not touched, neither by the Raisins, nor by the Water.

Sept. 10. I took out the Apricock, inclosed with the Water; and whilest the Air did break forth, the Fruit did bubble very much: the Raisins had lost almost all their taste, but the Apricock had preserved a pleasant relish; yea, it seemed more pleasant than the taste of such Fruits bought at that time of the Year useth to be.

Feb. 10. 1678. The Apricock, inclosed without Water, as yet kept its colour and figure, onely seemed to have lost its firmness.

This Experiment informs us, that the taste of some Fruits may be preserved in an Infusion of Raisins of the Sun; at least in Vessels which are able to contain a great compression of the Air.

EXPERIMENT II.

Sept. 17. 1678.

I included Peaches, with an Infusion of Raisins, in 2 Receivers, shut with a Screw.

Sept.

Sept. 21. Too great a quantity of Air produced in one of my Receivers, expelled ſome part of the liquor out of it. The other Receiver as yet retained its liquor.

Sept. 25. The Receiver, out of which the liquor was expelled, loſt ſome more thereof, ſo that its fifth or ſixth part now ſeemed empty: but *ſetting* the Screw, the liquor was then preſerved. The other Receiver was not altered.

Sept. 26. The ſame Receiver began again to leak and run over, ſo that I *ſet* the Screw again.

Nov. 27. Our Receiver ſeemed hitherto to be ſhut exactly enough, but this day I opened it, and, whileſt the Air was getting out, the Peaches bubbled very much; one of them, of the ſort of thoſe, to which the Stone, or Kernel uſeth to ſtick, had preſerved its firmneſs, and afforded a taſte pleaſant enough; but the other, being of that ſort, which are of a yellow colour, was very ſoft, yet the taſte thereof ſeemed to be more pleaſant than the taſte of the other. The liquor was very pleaſant and grateful.

Decemb 28. As yet the other Receiver ſeemed unaltered; but when I opened it, an innumerable company of Bubbles did immerge from the Liquor, and from the Peach. The Peach on one ſide had preſerved its firmneſs, on the other it had loſt it; but the whole Peach was acceptable to the Palate, yet ſomewhat ſharp.

This Experiment ſeems to teach us, that Liquors may grow ſowre, though no Spirits have evaporated from them.

EXPERIMENT III.

September 20.

I included Peaches, with unripe Grapes, in two Receivers, and weighed them exactly. In the one were Apples bruiſed to the conſiſtency of a Pultis: In the other, an Infuſion of Raiſins of the Sun.

Sept.

Sept. 25. The Receiver filled with pulp of Apples, hitherto seemed unaltered; but in the other, the Air which was generated, had extruded the half of the contained Liquor, and impelled the Mercury into the Gage, to the height of 100 digits; wherefore I opened the Receiver, and the Peach, whilest the Air was getting out, was almost reduced to the consistency of a Pultis; the taste of it was pleasant enough.

I put another Peach into the same Receiver, and substituted a new Infusion of Raisins of the Sun, instead of that which was lost.

Sept. 26. The Mercury was now come to 30 digits above its wonted height.

Sept. 27. The height of the Mercury was 72.

28. The height of it was 90. The Liquor did work out.

30. The same height remained, but the Liquor was all gone out.

October 1. I now perceived that all the Air had also escaped; Wherefore opening the Receiver, I found the Peaches very soft, yet of a pleasant taste.

Octob. 3. The Receiver filled with the pulp of Apples, had as yet lost nothing; but this day I perceived that almost all the juice of the Apples had run out, I opened the Receiver, and found all therein very much fermented. The Peach was very soft, but in taste not unpleasant.

This Experiment informs us, that Fruits cannot be long kept in pulp of Apples, by reason of the great production of Air; though that happens a little later in the Infusion of Raisins.

EXPERIMENT IV.

Sept. 23. 1678.

I included Peaches with crude Grapes in two Receivers, one of which was exactly filled with pulp of Apples, the other with unripe Grapes bruised.

Octob. 1. The Receiver filled with pulp of Apples, seemed as yet to have received no alteration; but the other was this day found emptied of his Wine: this therefore I opened, and found one of the Peaches to have retained its firmness, and its taste; but the other had lost its firmness, yet retained a grateful taste.

Feb. 5. 1679. The Receiver containing the Pulp of Apples, hitherto seemed unaltered; yet I opened it, and the great ebullition thereupon, did manifest, that a mighty compression of the Air was in it. The pulp of Apples and the Peach had kept a grateful taste, but somewhat more pungent than ordinary.

This Experiment shews us, that juice of crude Grapes cannot conveniently be used for the preservation of Fruits, by reason of the production of too much Air.

EXPERIMENT V.

Sept. 25. 1678.

I included two Pears, called Butter Pears, in a Receiver exactly filled with pulp of Apples.

Sept. 28. Hitherto I perceived no alteration in the height of the Mercury.

Octob. 5. The Mercury was now come to the height of 15 digits.

Octob. 6. The height of the Mercury was 16 digits and more.

Octob. 12. The Mercury was not changed.

Octob. 20. Three days ago the Mercury was depressed, though nothing had escaped out.

Octob. 26. This day my Receiver was found cracked, though I did not find that the Air was compressed within, but perhaps the Screw was *set* too high. The pulp of the Apples was of a very grateful taste; so were the Pears, but they were very soft, and one of them seemed to incline to rottenness.

Per-

Perhaps the crack in the Receiver was the cause why so little Air was produced in this Experiment.

EXPERIMENT VI.

Octob. 1. 1678.

I inclosed Peaches in two Receivers, one of which was filled with pulp of Apples, and the other with unripe Grapes bruised.

Octob. 5. Much Air was produced in the second Receiver, yet some of the Wine ran out. The height of the Mercury was 64 digits.

Octob. 6. The Wine proceeds to run out: the height of the Mercury was 70.

*Octob.*8. Now the Wine was all run out of the Receiver, and the height of the Mercury was 86.

Octob. 12. The height of the Mercury abode at 86.

*Octob.*18. That Receiver, out of which all the Wine was run, yet held the Air very well; and the height of the Mercury in it, staid at 86. The other Receiver, filled with pulp of Apples, had for these five last days suffered some juice to flow out.

Decemb. 4. I opened the Receiver filled with pulp of Apples, and though all the juice was got out, yet it still contained the Air, very much compressed; and many Bubbles brake forth, not without some noise, after the Receiver was quite opened. The Peach was very soft, and of a pungent taste, like to that of inebriating Wine.

*Jan.*28. 1679. After the effusion of the Wine in the other Receiver, the Mercury staid in the same height. I opened the Receiver; the Peaches did emit many Bubbles, and were wrinkled, but their colour was little changed: their sapor was most pungent, and inclining to acid.

This Experiment doth confirm the Conclusions of the former.

EXPERIMENT VII.

Octob. 4. 1678.

I put Peaches into three Receivers; The firſt of which was filled with Ale, or Beer without Hops; the ſecond with Beer Hopped; the third with Wine.

Octob. 5. The height of the Mercury in the firſt Receiver was 15 digits; in the ſecond, 10; in the third 9 digits.

Octob. 6. The height of it in the firſt Receiver was 25 digits; in the ſecond, 15; in the third, 20.

Octob. 8. The height of the Mercury in the firſt Receiver, was 35 digits; in the ſecond, 15; in the third, 20.

Octob. 12. The height in the firſt Receiver was 63 digits; in the ſecond, 15; in the third, 28.

15. The height of the Mercury in the firſt Receiver was 81 digits; in the ſecond, 15; in the third, 30.

16. There was no more change perceived in any of the three Receivers.

18. The Mercury rather deſcended than aſcended, in all the three Receivers.

22. In the Wine onely, the Mercury aſcended or deſcended according to the heat and the cold.

24. The height of the Mercury in the firſt Receiver was 96 digits; in the ſecond, 15; in the third, 30.

30. The height in the firſt Receiver was 115 digits; in the ſecond, 20; in the third, 30.

Nov. 3. The height in the firſt Receiver was 117 digits; in the ſecond, 20; in the third, 30.

6. The height in the firſt Receiver was 120 digits; in the ſecond, 31; in the third, 31.

11. The height of the Mercury in the firſt Receiver was 105 digits; in the ſecond 31; in the third, 28. It was cold weather.

Nov. 16. The height of the Mercury was the ſame. The Peach, which hitherto was demerſed, now mounted up to the upper part of the Liquor in the ſecond Receiver; all the reſt ſtaid in the bottom.

Nov. 25. The height in the firſt Receiver, was 140 digits; in the ſecond, 47; in the third, 32.

Nov. 28. The height in the firſt Receiver, was 96 digits; in the ſecond, 36; in the third, 28. It was very cold weather.

Decemb. 13. The height in the firſt Receiver was 96 digits; in the ſecond, 47; in the third, 33. I opened the third Receiver and found the Peach firm, and of a laudable colour, but it had contracted much of taſte from the Wine, which yet was capable of being amended by Sugar, ſo that a very pleaſant and edible diſh might be made thereof. The Wine alſo was grateful to the palate.

Decemb. 30. The height of the Mercury in the firſt Receiver was 96 digits; in the ſecond, 47. I opened the firſt Receiver, and the Peaches, which had lain till then at the bottom of the liquor, did preſently emerge to the upper part thereof; they emitted many Bubbles: the taſte of the Ale, of which they had contracted much, was made pleaſant with Sugar.

This Experiment informs us, that fermented Liquors may be uſeful for the preſervation of Fruits, as being unfit to produce Air.

EXPERIMENT VIII.

Sept. 5. 1678.

I included one Peach not cut, with another, cut into pieces, in a Receiver; into which I after poured old Wine, till it was exactly filled, and then ſhut it with a Screw. I hoped the iſſue would have been, that if the Wine did extract any tincture from the Peach, that the cut Peach would eaſily ſupply it; and ſo the whole Peach would keep its full taſte.

Nov.

Nov. 20. As yet nothing ſeemed to be altered; but this day I perceived, that ſome of the Wine did run out.

Nov. 30. The third part of the Wine was loſt.

Decemb. 8. Seeing the Wine begin again to run out, and that there was little of it left, I opened the Receiver, and found the Peaches very much fermented, yet endued with a grateful, but moſt pungent taſte. The Wine alſo was pleaſant.

By this Experiment, if it be compared with the third Receiver in the former Experiment, we may conjecture, that Wine doth hinder the fermentation of Peaches, if it be in a ſufficient quantity; but here the Wine was not ſufficient, becauſe the pieces of that Peach which was cut, did fill the whole Receiver, ſo that no room was left for the Wine, but in the interſtices.

EXPERIMENT IX.

Octob. 11. 1678.

I put two Peaches, one whole, the other cut in pieces, into a Receiver filled with hopped and fermented Beer.

Octob. 12. In one nights ſpace the Mercury aſcended 3 digits.

Octob. 15. The height of the Mercury was 15 digits.

16. The height of it was 15.

18. The height of it was 12. It was very cold.

20. The height of it remained at 12.

22. Now the Mercury aſcended again. The Cold abated.

Nov. 2. The height of the Mercury was 20.

3. The Mercury deſcended a little. It was cold weather.

6. The height of the Mercury was 28. The weather grew hotter.

8. The height of it was 33.

Nov

Nov. 11. The height of the Mercury was 40.

12. The height remained at 40. Some of the Beer wrought out.

16. The height of it was 46.

19. The height of it was 43. But much of the Beer was loſt.

21. The Mercury aſcended not, but the Beer proceeded to work out.

23. When the Beer was almoſt all wrought out, I opened the Receiver, and found the Peaches very ſoft, yet of a grateful taſte, though they had been kept 9 hours in the free Air, after the Receiver was opened.

N. *Theſe Fruits were never quite ripe.*

From this Experiment, if it be compared with the ſecond Receiver in *Exper.* VII. it may be inferred, that Beer doth hinder the Fermentation of Peaches, and the production of Air, if it be in a ſufficient quantity: but here there was but a little Beer contained in the interſtices, which was not able to hinder the fermentation of the Peaches.

EXPERIMENT X.

October 19. 1678.

I included raw Beef in 3 Receivers; the firſt of which was exactly filled with ſtale Beer, forcibly intruded, ſo that the Mercury exceeded its wonted height by 60 digits. The ſecond was alſo exactly filled with ſtale Beer, but here there was no compreſſion made. The third was filled partly with the Beef, and partly with Common Air.

Octob. 20. In the firſt Receiver the Mercury was depreſſed to the twentieth digit beyond its uſual height, though nothing at all had eſcaped out. In the ſecond alſo, it deſcended a little; but in the third, it aſcended ſomewhat.

Octob. 26. In the firſt Receiver the Mercury did ſometimes aſcend,

ascend, and then descend very irregularly; in the second it began to ascend slowly two days ago; in the third it was not moved at all.

Octob. 27. One piece of the same Beef, which was left in the Air, began to have an ill smell; and also the Mercury in the third Receiver began to ascend. In the second it proceeded to ascend by little and little; but in the first it seemed rather to descend.

Nov. 3. The Mercury in the first Receiver ascended not; in the second, the height of it was 20 digits; in the third it was 10 digits.

Nov. 5. I opened all the Receivers, and the two first did not stink at all, yet they had contracted a Smell from the Beer. The Flesh boiled in the same Beer, was found very tender, but its taste was bitter, perhaps by reason of the too great quantity of the Beer. That Beef which was included with common Air, when the Receiver was opened, did presently affect the nostrils with a stinking smell; yet when it was taken out, and accurately smelt too, it scarce seemed to stink. I included the same Flesh in the same Receiver, to trie whether new Air being admitted, would promote corruption.

Nov. 6. The height of the Mercury was 3 digits.

11. The height of it was 9.

25. The height of it was 20 digits.

I opened the Receiver, I found the Flesh so stinking, that I was forced to throw it away.

From this Experiment it seems to follow, that Beer may be convenient for the preservation of Flesh, especially if it be intruded by force into the Receiver; but this compression is soon abated, because the Air compressed in the same Receiver, is apt to enter into and pervade the pores of the Beer by degrees.

EX-

EXPERIMENT XI.

November 12.

I included Beef, as hardly as I was able to do it, in 3 Receivers: Into the first of them I poured Water, mixed with one fortieth part of Salt, which filled up all the interstices which were left betwixt the parts of the Flesh: In the second, some salt Water was in like sort contained; but it was intruded by force, so that the Mercury in the Gage ascended to 15 digits above its wonted height: Into the third Receiver, I poured no Water, and therefore those few interstices which could not be possessed by the Flesh, were left for the Air.

Nov. 13. The Mercury descended in all the Receivers, especially in the second, which had admitted the compressed Liquor.

Nov. 18. The two Receivers, which were not compressed, did not repel the depressed Mercury upward: But as for that whose Mercury had been impelled to 15 digits, and afterwards had descended most of all, it now returned almost to its former height. A piece of the same Beef, being left in the Air, began to have a bad smell.

Nov. 23. In the three Receivers Air was produced a new; but this day in the second the Mercury descended 3 digits, the height of it was 20: in the other two 'twas about 16. I opened the first Receiver, and the Flesh was not corrupted at all.

Nov. 30. I took the Flesh out of the Receiver which was put in without Salt, it did not stink at all; but being boiled, was very tender and of a pleasant taste.

Decemb. 6. I opened the Receiver into which I had forcibly introduced salt Water. The Mercury exceeded its wonted height 25 digits. The smell of the Flesh did strongly affect the nostrils, yet it did not stink. The Flesh put *in vacuo* sent forth many Bubbles, which ceased not, but a pretty while after, the Receiver in which it was included, was taken out of the Pneumatick Engine; yet the Mercury in one hours ſpace, came to the height of 3 or 4 digits. Afterwards I immerſed the ſame Receiver ſo exhauſted, in hot Water, and the Liquor contained therein, did bubble very much, though the Water from which it borrowed all its heat, did not boil at all; but ſo great a quantity of Air was produced, or elſe had entered from without, that the Receiver was quickly full. Afterwards the Liquor contained therein, did not bubble or Boil, though it were immerged in boiling Water. I took out the Fleſh, and found it pleaſant and tender, yet leſs *ſo* than I expected, perhaps becauſe it was not yet boiled enough.

This Experiment teacheth us, that Water, as well as Beer, may conduce to the preſervation of Fleſh.

EXPERIMENT XII.

*Nov.*29. 1678.

I incloſed Oiſters in 4 Receivers; In the firſt the Oiſters were without their ſhells, and filled the whole ſpace as exactly as we could; in the ſecond, the Oiſters, not taken out of their ſhells, were included with common Air: in the third, the Oiſters alſo were included in their ſhells, and the remaining ſpace of the Receiver was exactly filled with ſalt Water. All theſe 3 Veſſels were firmly cloſed with Screws. The fourth Receiver was exhauſted of Air, and it contained 3 Oiſters in their ſhells, and eight taken out of their ſhells. When the Air was pumped out of this Receiver, the Oiſters which were taken out of their ſhells, did emit many Bubbles, and thoſe very great ones; but the 3 others underwent no ſenſible mutation, ſave that one of them did gape.

Nov. 30. In the 3 Recipients which were ſtopped with Screws,

Screws, the Air seemed to be consumed, rather than produced; but the Mercury *in vacuo* ascended a little.

Decemb. 4. Whilest the Weather was cold, the Mercury ascended not; but now when the Cold began to abate, the height of the Mercury in the first Receiver was 7 digits; in the second, none; in the third, 3; in the fourth, 3.

Decemb. 5. The height of the Mercury in the first Receiver was 20 digits; in the second, 1 digit; in the third, 3; in the fourth 5.

Decemb. 7. The height of the Mercury in the first Receiver was 30 digits; in the second, 1 digit; in the third, 3; in the fourth, 8. Other Oisters, left at the same time in the Air, had a bad smell.

Decemb. 9. In the first Receiver the height was 30; in the fourth, 11. The rest were not changed.

Decemb. 13. There was no change in the 3 first Receivers, but in the fourth the height was 14 digits.

Decemb. 20. In the first Receiver the height was 46 digits; in the fourth 24; the rest were not changed.

Decemb. 21. In the first Receiver the height was 52 digits. in the fourth, 25: in the rest no change.

Decemb. 22. The height of the Mercury in the first Receiver was 60; in the fourth, 27: no change in the rest.

Decemb. 27. In the fourth Receiver the height was 29. the rest were not changed.

Jan. 1. 1679 The Oisters in the third Receiver had tinged the Water with a black colour.

Jan. 25. The Mercury *in vacuo* seemed still to remain almost in the same height. But this day some Bubbles were formed in the Turpentine, by the internal Air, about the Commissure of the Cover with the Receiver. Therefore I opened the Receiver, and found the Oisters very stinking; I likewise opened the other Receivers, and found the Oisters of a stinking smell, and turned to a kind of viscous Gelly.

This Experiment seems to inform us, that Fishes do produce less Air than Flesh; and yet, that they will be corrupted, though they are fortified against the Air.

EXPERIMENT XIII.

Nov. 29. 1678.

I exactly filled a Glass Vessel with fresh Butter, not at all salted, and then stopped it with a Screw. A mercurial Gage was included in the same Vessel.

Nov. 30. In the night, the cold being very sharp, the Butter was condensed, for the Mercury came nearer to the aperture of its Gage.

Decemb. 2. The Mercury came nearer and nearer to the aperture of its Gage, perhaps because the Cold did daily increase.

Decemb. 5. The Cold being abated, the Mercury returned almost to its former height; part of the same Butter, being left in the Air, began to have a very bad smell.

Decemb. 7. The Cold again returning, the Mercury did also again come to the top of its Gage. The Butter left in the Air, smelt worse than before, notwithstanding, as yet it was edible.

Decemb. 24. The Butter had produced no Air; being taken out of the Receiver, it was of a grateful taste, except onely a little of the superficies, which was contiguous to the Leather that was spread over the Cover.

From this Experiment it follows, that Butter may be kept a great while, if it be defended from the contact of the external Air.

EXPERIMENT XIV.

Nov. 30. 1678. I filled two Receivers with Whitings; and that

that no Air might be left in the vacant ſpaces, into the one I poured Wine; into the other, Oiſters, with their juice, without their ſhells; ſo that both the Receivers were exactly filled. When I had afterwards cloſed their Covers with Screws, the Air in the mercurial Gages was compreſſed; but in 3 hours ſpace the Mercury again returned to its former mark.

Decemb. 2. The Cold increaſing, the Mercury came nearer to the aperture of its Gage in both Receivers.

Decemb. 4. The Cold ceaſing, the Mercury aſcended very much in that Receiver wherein the Oiſters were, but in the other Receiver it was not moved.

Decemb. 5. In the Receiver containing the Oiſters, the height of the Mercury was 20 digits; but in the other, it was not yet returned to its wonted height.

Decemb. 7. In the Receiver with Oiſters, the height of the Mercury was 40 digits; in the other, it continued ſtill below its wonted height.

Decemb. 9. The Mercury in both Receivers was changed little or nothing.

Decemb. 20. When the Mercury was changed no more, I opened the Receivers, and both of them were found to be very ſtinking. And this ſeemed new to me in this Experiment, that the Receiver in which the Wine was, had admitted of corruption without production of Air; for hitherto all Bodies, whileſt they were corrupting, had produced Air.

EXPERIMENT XV.

Decemb. 3. 1678.

I put raw Beef into two large Receivers, with Pepper and Cloves; and that no Air might be left in the interſtices, I poured in Beer upon them, and no long time after, I found the preſſure of the Air in the Receivers to be abated, the Mercury in the Gages coming to the open ends.

Decemb.

Decemb. 8. The Mercury did not aſcend in either of the Receivers. I opened the one, that I might boil the Fleſh, it was endued with a ſweet ſmell, contracted from the Cloves; and the Liquor contained in the ſame Receiver, before it was boiled, did ſmell like Hippocras.

Jan. 2. 1679. I opened the other Receiver, and found no Air produced therein; the Fleſh was not at all corrupted, and when I boiled it *in vacuo*, I obſerved, that if a more intenſe fire were kindled, the Air, or ſome Spirits, did make an eruption through the ſtop-cock, which was faſtned to the top of the Receiver. The Receiver, being cooled, all the night, the day after was found almoſt quite empty of Air. The Fleſh was very tender, and well taſted, onely it was a little over-boiled, for it had been kept on the fire 6 full hours.

We have a confirmation by this Experiment, that Beer may be uſeful for the preſervation of Fleſh, eſpecially if the bitter taſte thereof be corrected by ſome Aromaticks.

EXPERIMENT XVI.

Decemb. 4. 1678.

I included 2 Larks, with ſome Beef, in a Receiver, all whoſe ſpaces unpoſſeſſed by the Fleſh, I filled with Ale; and at the ſame time I filled another Receiver with the ſame ſort of Beef, adding Beer alſo, but no Larks were put in with it.

Decemb. 9. Some pieces cut off from the Larks, and expoſed to the Air, began to ſmell ill; but thoſe included in the Receiver, as yet had produced but little Air; for the Mercury was not yet come to 5 digits above its wonted height. In the other Receiver it was not moved.

Decemb. 19. In the Receiver, which contained the Larks, the Mercury aſcended no higher; for the Cover being broken, ſuffered the Liquor to run out. Wherefore I opened the Receiver, and boiled both the Beef and the Larks, which were not at all corrupted,

corrupted, but they seemed very acceptable to the palate; yea the Beef had contracted a pleasant taste, partly from the Larks, and partly from the Beer.

Decemb. 23. I opened the other Receiver, and the boiled Flesh seemed pleasant, yet not so pleasant, as that which was endued with a Venison-like taste from the Larks.

This Experiment shews us, that even tender Birds may be preserved long by the help of Beer or Ale.

EXPERIMENT XVII.

December 14.

I included Apples in 4 Receivers; in the first was an whole Apple, and all the spaces were filled with powdered Sugar: in the second, an Apple was cut in pieces, and the spaces filled with Sugar, as before: in the third an Apple was also cut, but the rest of the Receiver was filled with Water, wherewith $\frac{1}{10}$ part of Sugar was mixed: in the fourth, the Apple was also cut, and the spaces were likewise filled with a solution of one part Sugar, and 5 parts of Water.

Decemb. 21. This day in the first Receiver the Mercury began a little to ascend, yet the Sugar did not melt: in the second Receiver all the Sugar was melted, and the pieces of Apple were shrievelled, also they produced much Air when they were first put into the Receiver: In the 2 other Receivers the Mercury began also to ascend; but in the third, the pieces of Apple were very much corrupted, for their *skin* or rine was taken off.

Decemb. 22. Air was produced in all the Receivers, but the quantities of the Air produced, did not bear the same proportion amongst themselves, as the quantities of the Sugar; for in the second Receiver much Air was produced, but in the fourth the Mercury ascended less than in the third; and besides, in the first some Air was generated.

Decemb. 27. In the three first Receivers the height of the

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Mercury was 10 digits; but in the fourth 'twas onely 6 digits.

Decemb. 31. In the firſt and ſecond Receivers the height of the Mercury was 13 ; in the third the height was 15; in the fourth it was onely 9 digits.

Jan. 2. 1679. In the firſt and ſecond Receivers the height of the Mercury was almoſt 14; in the third, 17; in the fourth, 11.

Jan. 7. In the ſecond Receiver the height of the Mercury was 16 digits; in the third, 36; in the fourth the height of it was 15 : but in the firſt the Mercury had not aſcended, and ſomething had eſcaped out of the Receiver, and therefore I *eaſed* the Screw, that I might diſpoſe of it the better ; and then the Air made an eſcape.

Jan. 9. In the firſt Receiver the height was 6 digits; in the ſecond, 16; in the third, 39 ; in the fourth, 15.

Jan. 17. In the firſt Receiver the height was 13 ; in the ſecond, 19 ; in the third, 56 ; in the fourth, 17.

Jan. 30. In the third Receiver the height of the Mercury was 76 digits, and the Liquor brake out, and therefore I opened it, and found the Fruit to have loſt much of its taſte, but the Water had contracted it, and was pleaſant enough to the palate. In the ſecond Receiver the Mercury aſcended no more. I opened this Receiver alſo, and found the Fruit much more pleaſant in this than the other; yet much of its taſte was imparted to the ambient Sugar, ſo that it was found changed into a very good Syrup.

Feb. 16. The height of the Mercury in the firſt Receiver was 22 digits; but in the fourth, 33. I opened it, and found the Fruit to have loſt much of its taſte, and that the ambient Water had got it, and was thereby turned into a pleaſant drink.

Feb. 27. In the firſt Receiver the height of the Mercury was 30 digits.

March

March 15. In the firſt Receiver the height of the Mercury was not changed, but this day I found ſomething to eſcape out of the Receiver, and therefore I opened it, and found the Apple of a laudable colour, but the Pulp was ſpongy, and had loſt much of its taſte.

This Experiment ſeems to teach us, that Sugar is not ſo fit for the preſervation of Fruits, as Fermented Liquors. See *Exper.* VII.

EXPERIMENT XVIII.

December 23.

I filled a Glaſs Veſſel with Milk, and then ſtopped it with a Screw; and into another Receiver I put a Lark with Milk, and ſtopped it cloſe.

Decemb. 24. This Evening I perceived that the caſeous part was ſevered from the butyrous, in the cloſed Receivers as well as in the Milk, which at the ſame time I had left expoſed to the Air.

Decemb. 27. I found no Air produced in the Receiver which held the Lark; but in the other, the mercurial Gage was ſpoiled.

Decemb. 31. The Mercury aſcended in that Receiver which contained the Lark; but the Milk that was left in the Air at the ſame time that I ſtopped the Receivers, did ſtink 3 days ago.

Jan. 1. 1679. In the Receiver, wherein the Lark was included, the height of the Mercury was 10 digits.

Jan. 2. The height of the Mercury was 14 $\frac{1}{2}$. The Milk ſtagnant below the butyrous part, appeared of a red colour.

Jan. 4. The height of the Mercury was 19. Some white ſewas concreted in the bottom of the Milk.

Jan. 9. The height of the Mercury was 29 digits.

Jan. 25. I opened both Receivers and found the Lark to affect the Noſtrils with a ſtrong, though no fœtid ſmell, yet it had been kept 32 days; when it was boiled it was of a pleaſant taſte. In the other Receiver, the caſeous part of the Milk was ſubacid and grateful, but the butyrous part was not ſowre at all.

This Experiment informs us, that ſometimes Milk may be uſed with good ſucceſs for the preſervation of Fleſh.

EXPERIMENT XIX.

Decemb. 24. 1678.

I put a Lark into a ſmall Receiver, and poured Butter upon it, melted with a ſlow fire, till all the ſpaces were exactly filled, then I cloſed the Cover with a Screw.

Decemb. 27. The Mercury approached nearer to the aperture of its Gage; but the Butter ſeemed to be altered, for the loweſt part of it was more yellow, and the middle more white than it ſeemed before the incluſion thereof; the upper part was fluid.

Jan. 5. 1679. The Mercury returned by little and little, to its wonted height.

Jan. 9. The Mercury was ſomewhat higher.

Jan. 28. The Mercury was little changed: I opened the Receiver, and found that part of the Butter which was contiguous to the Leather ſpread over the Cover, to be white, and of a very unacceptable taſte. The Butter which was more remote from the Leather, was yellow and ſomething graveolent, yet it was edible. But the Lark being roaſted, was grateful to the palate, though it had been kept 34 days.

This Experiment ſeems to inform us, that Butter melted and hot, is not ſo ſucceſsfully uſed for the preſervation of Fleſh.

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EXPERIMENT XX.

Jan. 4. 1679.

I included boiled Flesh *in vacuo* in a Receiver stopped with a Screw, and filled the interstices exactly with Broth of the same Flesh, which seemed a little too salt. Whilest I *set* the Screw, all things in the Receiver suffered a compression, and the Mercury ascended to the height of 6 digits into the Gage; but shortly after it returned to its wonted height.

Jan. 28. The Air was more and more consumed, so that the Mercury now descended to 8 digits below its wonted height. I opened the Receiver, and found the Flesh very sweet and tender. The Broth also had a *subacid*, but a very *grateful* taste.

This Experiment informs us, that Flesh, after it is boiled, may be kept long without prejudice, which is a great conveniency in long Voyages at Sea, so that perhaps there will be no need of salted Flesh. For after the raw Flesh hath been kept so long in Vessels stopped with Screws, till Experience shews that there is no danger of its corruption; then it is to be taken out, and being perfectly boiled, is again to be included in the same Receivers: And so without doubt it may be kept for a long time without Salt. See *Exper.* XII.

EXPERIMENT XXI.

Jan. 30. 1679.

I put raw Flesh into 2 Receivers; to the first I added Pepper and Cloves; in the second I mixed nothing, for I was willing to know, whether these spices would promote the production of Air, or retard it.

Feb. 11. The height of the Mercury in the first Receiver was 3 digits; in the second the height of it was below $1\frac{1}{2}$.

Feb. 12. The height of the Mercury in the firſt Receiver was 4 ½; in the ſecond not above 1 ½.

Feb. 13. In the firſt Receiver the height of the Mercury was 6 digits and more; in the ſecond, it was 3 digits. I boiled the Fleſh of the firſt Receiver, after the manner before deſcribed, and it was very pleaſant and tender.

Feb. 14. The height of the Mercury in the ſecond Receiver was 5 digits.

Feb. 19. The height of the Mercury in the ſecond Receiver, was 8 digits.

Feb. 20. The height of the Mercury in the ſecond Receiver was 11 digits. I boiled the Fleſh and found it very tender, though it had ſtaid over the Fire *in balneo mariæ*, onely for 3 quarters of an hour. I put ſome part of this Fleſh, before it was boiled, into a Receiver, and filled all the ſpaces as exactly as I could with the ſame Fleſh, to try how long the Fleſh might be preſerved when the Air was ſo excluded.

Feb. 28. The Mercury aſcended very little.

March 20. The height of the Mercury was about 16 digits. I opened the Receiver, and the Fleſh ſeemed of a pleaſant taſte, yet inclining to corruption.

EXPERIMENT XXII.

February 10.

I put raw Beef into 3 Receivers: In the firſt, the Beef was ſeaſoned with Pepper and Cloves; in the ſecond, it was encompaſſed with ſalt Water; in the third, I put neither Salt nor Spice.

Feb. 19. Four days ago the Mercury aſcended in the third Receiver; in the firſt alſo it began to aſcend; but in the ſecond it was not moved at all.

Feb. 21. In the firſt Receiver the height of the Mercury was 4 di-

4 digits and $\frac{1}{2}$; in the third, 10 digits; but in the ſecond, there was no aſcent at all.

Feb. 25. The height of the Mercury in the firſt Receiver was 6 digits; in the third, 19 digits; in the ſecond, half a digit.

Feb. 26. This night there was no aſcenſion of the Mercury in all the Receivers. I opened the third Receiver, and the Fleſh, after boiling, was found very good.

The former Experiment ſeems to teach us, that Spices do hinder the production of Air; but the preſent Experiment proves the contrary. Whence this contrariety ſhould proceed, I know not; unleſs it be, becauſe, perhaps, I had left a ſpace large enough for the Air in theſe Receivers; but in the former Experiment I filled all as exactly as I could with Fleſh.

March 9. The height of the Mercury in the firſt Receiver was 8 digits; in the ſecond, none.

March 12. The height of the Mercury in the firſt Receiver was 12 digits; in the ſecond, 1 digit.

April 3. The height of the Mercury in the firſt Receiver was 11 digits; but in the ſecond, it exceeded not one digit. I opened the Receiver, and boiling the Fleſh, after my accuſtomed manner, I found it very tender, and of an excellent taſte.

The Corollary from this Experiment ſeems to be, that the ſaltneſs of Water, included with Fleſh, doth hinder the production of Air; but becauſe there was ſo ſmall a quantity of Water, compared with the quantity of Fleſh. I do rather incline to think that leſs Air was produced in the ſecond Receiver, becauſe it was more exactly filled. And indeed if freſh Water had been uſed inſtead of ſalt, the matter ſucceeds after the ſame ſort; but the chief Art to Preſerve Fleſh without Salt conſiſts herein, That all Air be excluded from it, and that there be a great compreſſion in the Receiver.

All theſe Experiments about the preſervation of Aliments,

what

what great uſe they may be of for the tranſporting of Fruits, Veniſon, or other Fleſh from places far remote to great Cities, and for the affording better nouriſhment to Mariners, I leave to the Reader to judge.

ARTICLE XVIII.

Experiments concerning Elixation and Diſtillation in Vacuo.

EXPERIMENT I.

Decemb. 12. 1678.

I Put 2 ounces and 6 drachms of Beef into an empty Receiver, which was able to hold 22 ounces of Water. Then I put it into boiling Water for 3 hours; which being done, I expoſed it to the Air to be cooled for a whole night; afterwards, uſing my Pneumatick Engine, I perceived, that the Air formed in the Receiver, could ſcarce ſuſtain 3 digits of Mercury; and ſo deducting from the Calculation, a man may eaſily find, that Fleſh, whileſt it is boiled, cannot form Air enough to make an entire preſſure in a Receiver capable of holding a double weight of Water: that is, If you include one pound of Fleſh in an emptied Receiver, able to hold 2 ounces of Water, it will not generate Air that can remove the Cover from the Receiver, unleſs heat do confer much to produce the effect; but I confeſs that our Fleſh was not boiled enough.

See the Deſcription of a Veſſel to Boil and Diſtil in Vacuo, pag. 19.

EX-

EXPERIMENT II.

December 23.

I inclosed 3 ounces of raw Beef in a Receiver able to hold 32 ounces of Water; and when it boiled, having been long on the Fire, the Cover was forced from its Receiver, and so suffered the vapours to pass out: but because it was presently shut again, the fire being removed, the Receiver soon lost its internal pressure, so that being set again to the fire, it was a long time before it could force away the Cover the second time. I tried this again and again; yea, unless the Receiver had been exposed to a very strong fire, the Cover would never have been removed; but if the fire be kindled enough, sweet exhalations continually pass out.

Decemb. 24. The Receiver having been cooled during the whole night, was this day, by the use of the Pneumatick Engine, almost wholly evacuated. Whence we seem to have a confirmation, that the divulsion of the Cover, is not made by that Air, which can keep the form of Air, but from the Steams exhaling from the Flesh, and subsiding again therein, if they be hindred from egress, which may easily be performed, if we use not too fierce a fire in the empty Receiver, and so the loss of those sweet smelling vapours may be easily avoided.

EXPERIMENT III.

Jan. 21. 1679.

I put Paste without Leaven into an exhausted Receiver; and also I included another part of the same Paste in another Receiver, full of Common Air. I enclosed these 2 Receivers *in balneo mariæ*, stopped with a Screw; and when they had staid there for 3 hours, having been exposed to a moderate fire, I opened the Receivers: The Paste *in vacuo* I found reddish, as far

far as the superficies; but the other had admitted Water; and the Paste was not boiled enough, and therefore I put both Receivers again *in balneo mariæ*, where they staid an whole night.

Jan. 22. This day in the morning, I found the *balneum mariæ* quite cold; and the Paste, when it was taken out, was boiled enough, but it was covered with no crust. That which was included *in vacuo*, was interspersed with many cavities, but it seemed too insipid; the other contained no cavities, but afforded a more pleasant taste. Both the Receivers were found almost wholly emptied of Air.

EXPERIMENT IV.

February 3. 1679.

I enclosed Paste kneaded with Leaven *in vacuo*, and as soon as it had filled its Receiver with factitious Air, I transmitted it into that Receiver, which I am accustomed to use to boil Flesh *in balneo mariæ*; but when the Paste was thus removed out of one Receiver into another; it pitched or sank very much; yet when it had remained for 3 hours in a fervid *balneo mariæ*, the Bread made of it was interspersed with many cavities, but it was covered with no crust.

Feb. 5. I iterated the same Experiment, but this time the Paste was included *in vacuo*, in the same Receiver, which was afterwards put *in balneo mariæ*, and therefore there was no need to remove the Paste, and to expose it to the Air. Hence it came to pass, that the Bread made thereof, was much lighter than the former.

EX-

EXPERIMENT V.

February. 12.

I included Rosemary with Water in the Vessel described *p.* 19. and when the Air was pumped out, I put the Vessel *in balneo arenæ*, and there came forth a Water endued with a very sweet smell; yea and some drops of essential Oil, smelling very sweet also, and affected with no *Empyreuma*. But when I opened the Stop-cock for to let in the Air, the noise did so soon cease, that I judged much Air was produced from the Rosemary.

*Feb.*13. I put the same Rosemary into the same evacuated Vessel, and administred a more intense fire thereunto, yet I could extract no Oil, neither sweet nor stinking; and besides the Water was less fragrant than the former.

EXPERIMENT VI.

February 10. 1679.

I boiled 1 pound of Flesh *in vacuo*, in the Vessel described *p.*19. which could contain almost 4 pound of Water: the upper part thereof, which was made of Glass, did hold the mercurial Gage, by the help whereof, I perceived that the Mercury had not ascended to the height of 3 digits, though the Flesh had boiled for 3 hours and more. It was not boiled enough, and its taste was ungrateful; and moreover, the Liquor which was formed of the condensed Vapours, afforded also an unpleasant taste.

*Feb.*11. I iterated the former Experiment, but this time I sprinkled the Flesh with Pepper and Cloves; the issue was, that the Mercury ascended to the height of 6 digits, though the Flesh was boiled no longer than the other; it seemed very grateful to the palate, and the Liquor formed from the Vapours, afforded a most pungent taste of Pepper ; but it had contracted nothing ungrateful from the Flesh, as was done in the former Experiment.

From these Experiments made about Elixation and Distillation *in vacuo*, the Corollary seems to be, that such Vessels may be very useful for the Distilling, and boiling of such bodies, which do contain thin, and very volatile Spirits : for all things will be preserved by their help, and nothing will avolate or flie away.

ARTICLE XIX.

Concerning Elixation in Vessels stopped with Screws, by the help whereof, even Harts-horn, and the bones of Fishes, and Four-footed Creatures may be softned.

EXPERIMENT I.

January 29.

Eight days ago I filled a Vessel, stopped with a Screw, with Beef and Water together, and when it had continued, exposed to a moderate Fire, for eight or nine hours *in balneo mariæ*, stopped also with a Screw ; I took the Flesh out of it, but it was boiled a great deal too much, and the Taste of it was very unpleasant. Afterwards, I boiled new Beef in the same Vessel, and after the same manner, save that this was seasoned with Pepper and Cloves, and remained exposed to the Fire, onely for three hours. The issue was, that this Flesh preserved a most pleasant taste; wherefore, that

that I might know whether the excellency of this Flesh above the other, did proceed from the Spices, or from a shorter time of boiling, I boiled other Flesh without Spices for 3 hours, in the same Vessel, and after the same manner: when the Flesh was taken out, it was of a good taste. Whence I conjectured, that the cause of spoiling the first Flesh, was to be chiefly ascribed to the over-boiling: Yet I think that the Spices may be convenient to correct some part of the ungrateful taste; for I left a place for the condensing of the Vapours, in the top of the Vessel, and found that the Liquor there formed, was of an unpleasant taste; but when the Flesh was seasoned with Pepper and Cloves, no such thing was found.

EXPERIMENT II.

Jan. 29.

I boiled Apples, after the same manner as I did the Flesh before described; but I mixed no Water with them. They were set upon a moderate fire almost for 2 hours. They were very soft, and of a very good taste, but some pieces which were laid in the upper part of the Receiver, where the Vapours ascending from the inferiour part, were condensed, were found of an unpleasant taste; and also the drops, formed from the same Vapours, did affect the Nostrils with an ungrateful odour.

EXPERIMENT III.

February 4.

I enclosed Flesh with Pepper and Cloves in a Receiver, stopped with a Screw, but poured no Water in to fill the interstices, onely I compressed the Flesh, as much as I could, and then I put the Receiver *in balneo mariæ*, already hot, and stopped it with a Screw; and when it had remained there, over a moderate fire, for a whole hour, the Flesh was rather over-boiled than under-boiled: But when I opened the *balneum mariæ*, all the Water brake out of it with a great force, *viz.* the Liquor being hot, and hitherto incarcerated, now having freedom given, at length did ſhew its ſtrength.

Feb. 5. I encloſed ſome part of this Fleſh in a Receiver ſtopped with a Screw.

March 12. The Fleſh, which was included 5 weeks ago, was this day found very good. I do not doubt, but that perfect Elixation, was able to contribute ſomething to its preſervation, *viz.* becauſe the ſundry principles, of which Fleſh conſiſteth, had, whileſt the heat continued, exerted their ſtrength upon one another, far better than if the Fleſh, being leſs boiled, by reaſon of the great avolation of parts, had been to be removed from the Fire, as it happens in ordinary coctions. And indeed, by Experiments made about other Bodies, I have found that Elixation, the perfecter it is, doth ſo much the more hinder fermentation. See *Artic.*XVII. *Exper.*XII, XX.

EXPERIMENT IV.

February 10.

I boiled an Ox-foot or *Cow-heel*, after the ſame manner, as I had done the Fleſh above mentioned, but I left the Cow-heel for 4 hours or more, upon a moderate fire. That time being elapſed, and the Veſſels unſtopped, the Fleſh was excellently well boiled, and the bones were ſo ſoft, that they might be cut with a Knife, and eaten like Cheeſe.

Feb. 12. I repeated the ſame Experiment, but the Veſſels remained expoſed to the fire for 12 hours ſpace; and though the Water of the *balneum mariæ* did every where ſecure the Veſſel demerſed in it, yet the Fleſh had contracted a taſte and a ſmell very *Empyreumatical*; but the juice, which in the former Experiment did concrete into a very firm Gelly, in this latter, could not be congealed at all.

By these Experiments it appears, That many bones and hard tendons, which we daily cast away as unprofitable, by the help of *balneum mariæ*, stopped with a Screw, may be converted into good nourishment.

EXPERIMENT V.

February 10.

I boiled a Fish, after the same manner as was described above, *in balneo mariæ* stopped with a Screw, but I mixed no Water therewith. The Fish staid upon the fire two hours, onely; then the Vessel being cooled and opened, the Fish was found of a very good taste, and his bones were so soft that they yielded to the pressure of ones finger, and the head of it could be eaten like its flesh. The juice of it in a short time did concrete into a Gelly of an hard consistence.

This Experiment is very useful for the boiling of Fish which are full of bones.

EXPERIMENT. VI.

February 15.

I put Harts horn into a Receiver which was to be stopped with a Screw, and filled the intervals with Water, I included the Receiver thus stopped, *in balneo mariæ*, stopped also with a Screw, and so exposed it for 4 hours to a moderate fire; when that time was passed and the Vessels opened, the Harts horn was as soft as Cheese; and the juice did soon concrete into a very firm Gelly.

Feb. 17. I repeated the same Experiment, but no Water was included with the Harts-horn, and the fire lasted 6 hours under the *balneum mariæ*; when this was done, the Harts-horn was found very soft, but a little juice had excreted out of it, and *that* did adhere to the external parts of the Harts-horn in the form of drops of Gelly.

The

The Excellency of this *Balneum mariæ* is confirmed by this Experiment: For ſeeing Harts-horn it ſelf can be boiled by the help thereof, without the mixture of Water, there is no doubt but all freſh Water, which is wont to be ſpent in Ships to boil Fleſh, may be preſerved for other uſes of the Mariners. Furthermore, If we add what we have tried about the preſervation of raw Fleſh, and after of that which is boiled. (See *Exper.* III.) Doubtleſs we may conceive great hope, that many inconveniences which are wont to prejudice Mariners, both by reaſon of the ſaltneſs of their meat, and the putrefaction of their Water, will be almoſt wholly remedied and prevented. Neither let any man object that ſo many Veſſels, and ſo exactly ſtopped, are very difficult to be procured; for daily experience doth evince, that very many mechanical inſtruments, far more difficult, may in a little time become very eaſie for uſe, and as eaſily procurable.

FINIS.

THE INDEX.

19. *Con*

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OBSERVATIONS.

The Experimental History of Cold begun, to which is subjoined a Dissertation concerning Antiperistasis, together with an Examen of Mr. Hobs's *Doctrine about Cold*; 1665.

* *Hydrostatical Paradoxes*; 1666.

* *The Origin of Forms and Qualities; the second Edition; to which is annexed a Dissertation concerning Subordinate Forms*; 1671.

* *Tracts concerning the Cosmical Qualities of things; Cosmical Suspicions; the Temper of the Marine Regions; the Temper of the Subterranean Regions, and of the Bottom of the Sea*; 1671.

* *An Essay concerning the Origin and Vertues of Gems*; 1672.

A Tract containing New Experiments between Flame and Air; together with an Hydrostatical Dissertation; 1672.

* *Some Essays concerning the wonderfull Subtilty and Efficacy of Effluviums, and their determinate Nature*; 1673.

Some Tracts consisting of Observations concerning the Saltness of the Sea; with a Sceptical Dialogue concerning the Nature of Cold both positive and privative; 1674.

Tracts containing some Suspicions concerning some Occult Qualities of the Air; with an Appendix touching Celestial Magnets, &c. 1674.

An Introduction to the History of particular Qualities in the Philosophical Transactions; N. 63. p. 2057.

* *Of the Excellency of the Mechanical Hypothesis*; N. 103. p. 53.

Experiments and Observations concerning the Mechanical Production and Origin of several particular Qualities; together with some Reflexions upon the Hypothesis of Acid and Alcaly; 1675.

The Sceptical Chymist, or Chymico-physical Doubts and Paradoxes about those Experiments, whereby vulgar Spagyrists do labour to evince, that Sal, Sulphur and Mercury are the genuine Principles of things; to which, viz. *in this* 2d. *Edition, sundry*

Experiments and Conſiderations are ſubjoined concerning the Producibleneſs of Chymical Principles; 1680.

* *A Continuation of New Phyſico-mechanical Experiments*; the ſecond Part; 1680.

Theſe are the Philoſophical Works of our Author hitherto publiſhed; what he hath wrote in Divinity belongs not properly to this place; not to mention ſeveral Diſſertations of his which you may find here and there interſperſed among the Philoſophical Tranſactions publiſhed in Print.

THE TRANSLATOR TO THE READER.

Though the First Part of the Physico-Mechanical Experiments *of this Honorable Author was published by him in the* English *Tongue, as was also, some years after, his First* Continuation *of the same, yet so welcomly were they entertained by the Curious, especially in Transmarine parts, that the First Part hath been long since published in the* Latin *Tongue; and the First* Continuation *is also translated into the same Tongue, though (for reasons, in part mentioned at the end of the Publisher's* Advertisement to the Reader *prefixed before this* Tract) *not yet Printed.*

This Second Continuation *of the aforesaid* Experiments *speaks* Latin *at the first hand; but that all those Three Tracts might be clothed with one habit, it was the desire of some ingenious Persons, that it might also be rendred into* English; *which Province hath been recommended to me by the Bookseller.*

I may without vanity affirm, that I have an advantage beyond some others, in reference to the Versions of any Tracts *of*

this

this Noble Author, either out of English *into* Latin, *or out of* Latin *into* English, *in that, by reason of the vicinity of my habitation, I have conveniencie, at fitting seasons, to consult the Author himself, about his sense in any place which may be doubtfull to me; which I mention, not onely to declare the Candour and Condescension of so Eminent a Person, but also to account for any Mistakes I may be guilty of, which are therefore rendred the more inexcusable in me, and properly to be laid at my own door.*

It is not to be doubted, but that, if the Honorable Author of these following Experiments *had himself at first drest them up in their* English *habit, they had appeared far more terse and polite than my inability can trim them up to, yet, besides the necessary Inconveniences which attend all Translations of Books, (especially those which treat of Nice and Curious Subjects) which I alledge for part of my Apology, I do farther relieve my self with that saying of the Orator,* Si quis summa desperet, tamen est pulchrum in secundis tertiisve consistere. *Quintil.*

And in reference to the Elegancies of this Noble Author, in the English *as well as other Languages (of which he is so great a Master) I may farther add with the same Orator,* Ut transeundi spes non sit, magna tamen est dignitas consequendi. *'Tis sufficient honour for me to write after his Copy.* In fine *I conclude with the Poet;*

Veniam pro laude peto, laudatus abunde,
Si fastiditus non tibi, Lector, ero.

OBSERVATIONS.

FINIS.

ERRATA.

PAg.16.l.3. add in margine, *F* 4. p. 20.l.15. for *EE* r.*AA*.p.35.l.26.for 5 *hours* r. 5 *minutes*. p.40.l.3.dele *almost*. p.41.l.8. for *June* 1. r.*June* 2. p.60.l.10. for 10 r.11 *digits*. p.51.l.26. for *Sept*.1. r.*Sept*.15. p.68.l.penult. for *no mouldiness* r. *much mouldiness*. p.75.l 18.for *July* 14. r.*June* 14. p.82.l.13. for 6 $\frac{1}{2}$.r.6 $\frac{1}{3}$. p.96.l.11. dele *XI*. p.101.l.21. for *May* 11. r.*May* 19. p.151.l.23. for *July* r.*June*. p.154.l.13. for 28 r.29. p.109.l.ult. dele *any*. p.168.l. antepenult. for *weighed*, r. *filled*. p.129. l.28. after *later*, insert *then*. p.185.l.antep. for *se-* r.*sediment*. p.190.l.20. for 2 *ounces* r. 2 *pounds*.

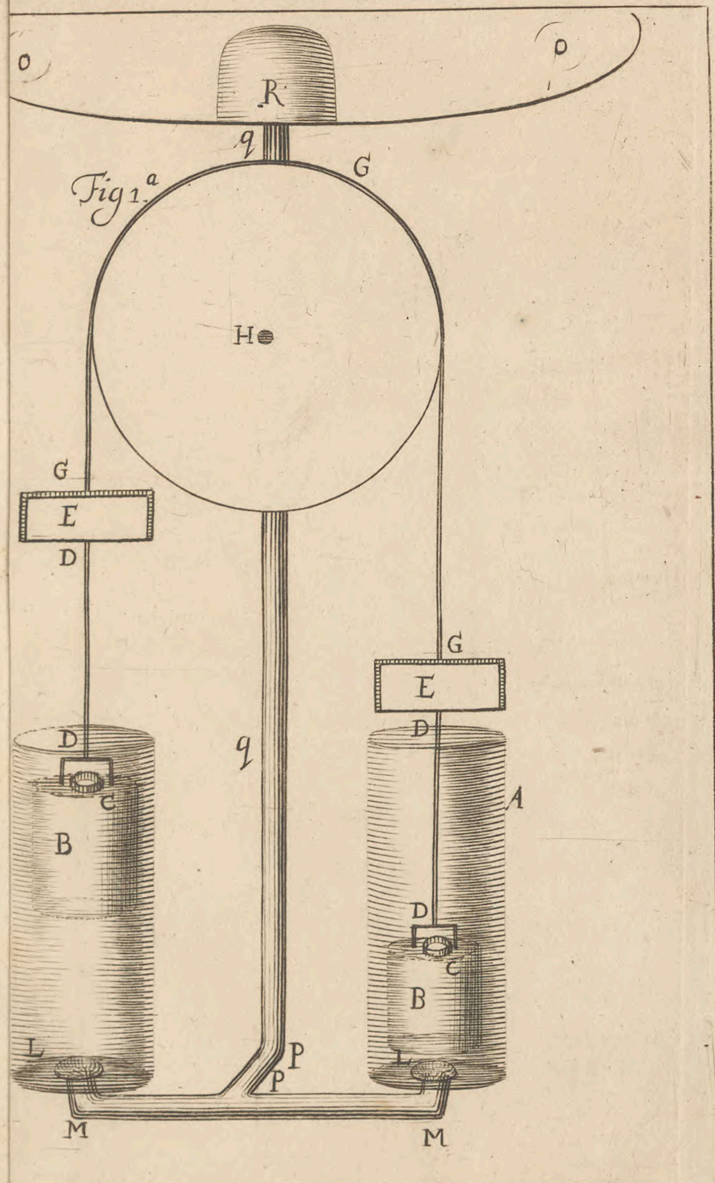
Fig 1.a
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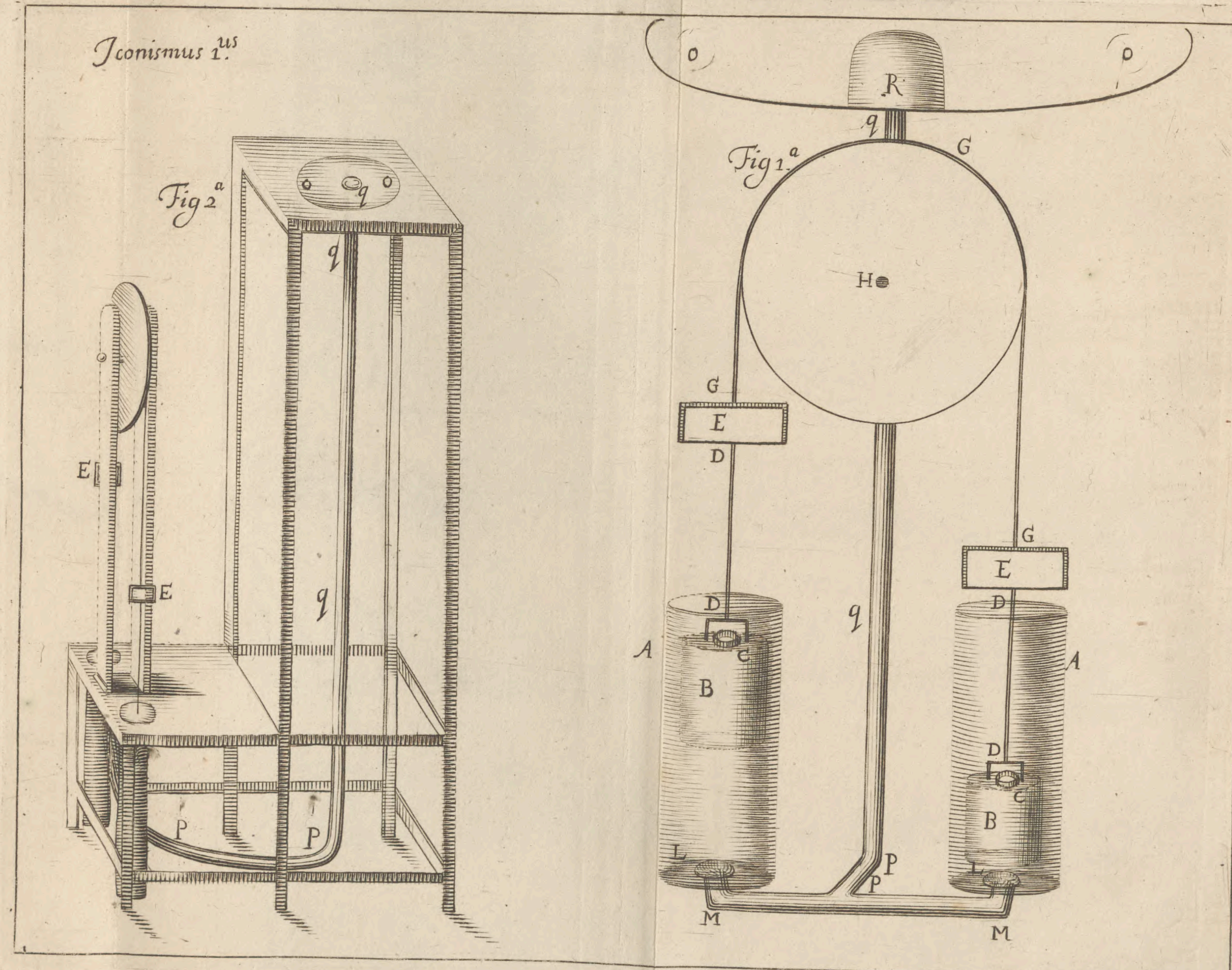

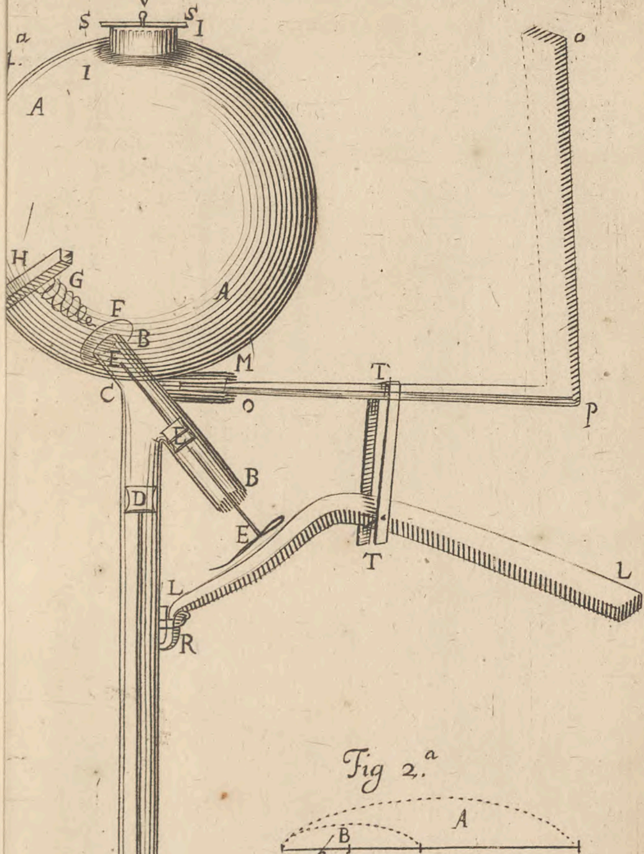

Fig 2.a

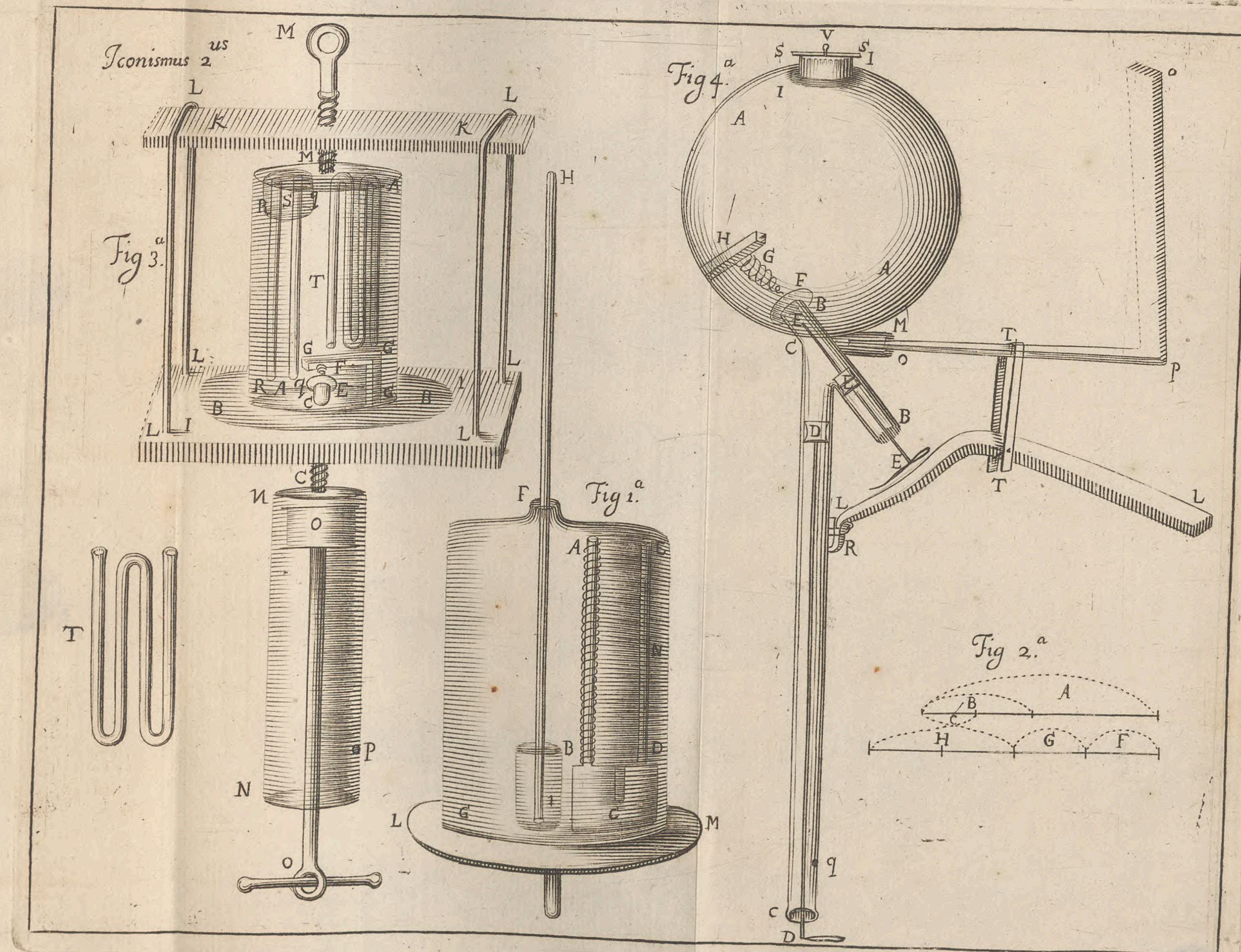
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Fig 3.a
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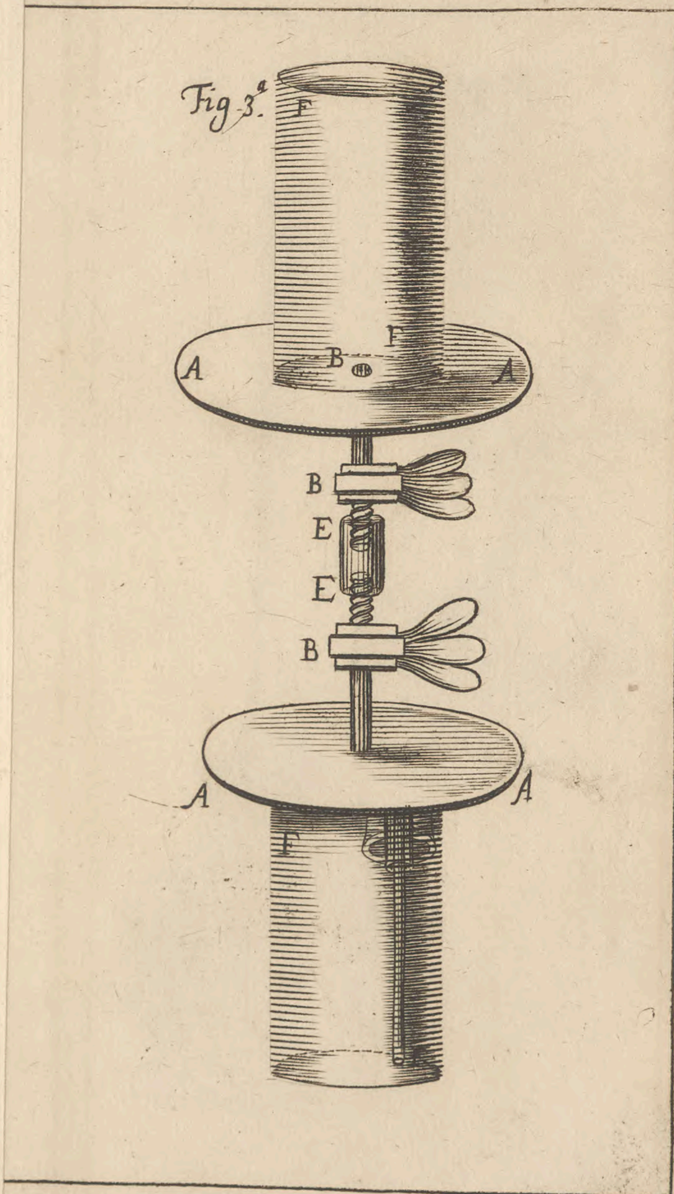
Fig. 3.ª
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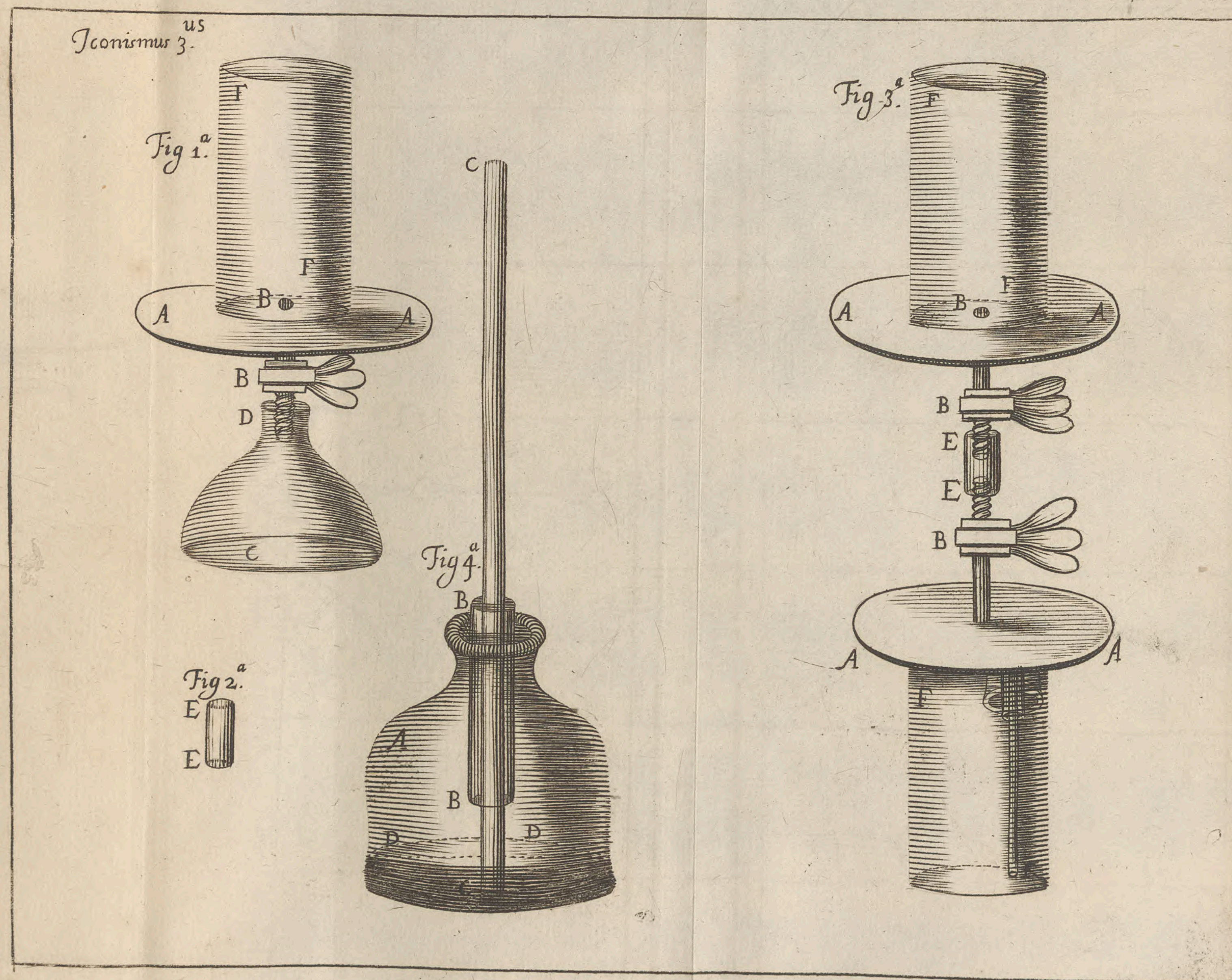
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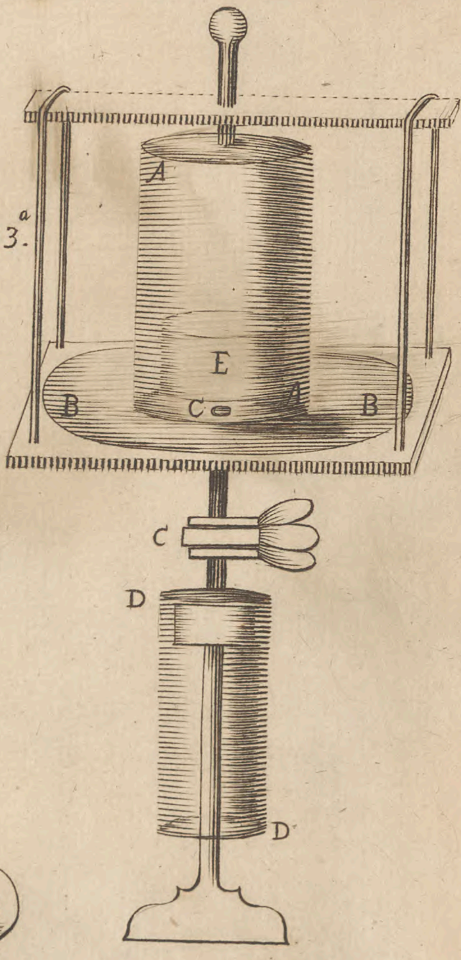

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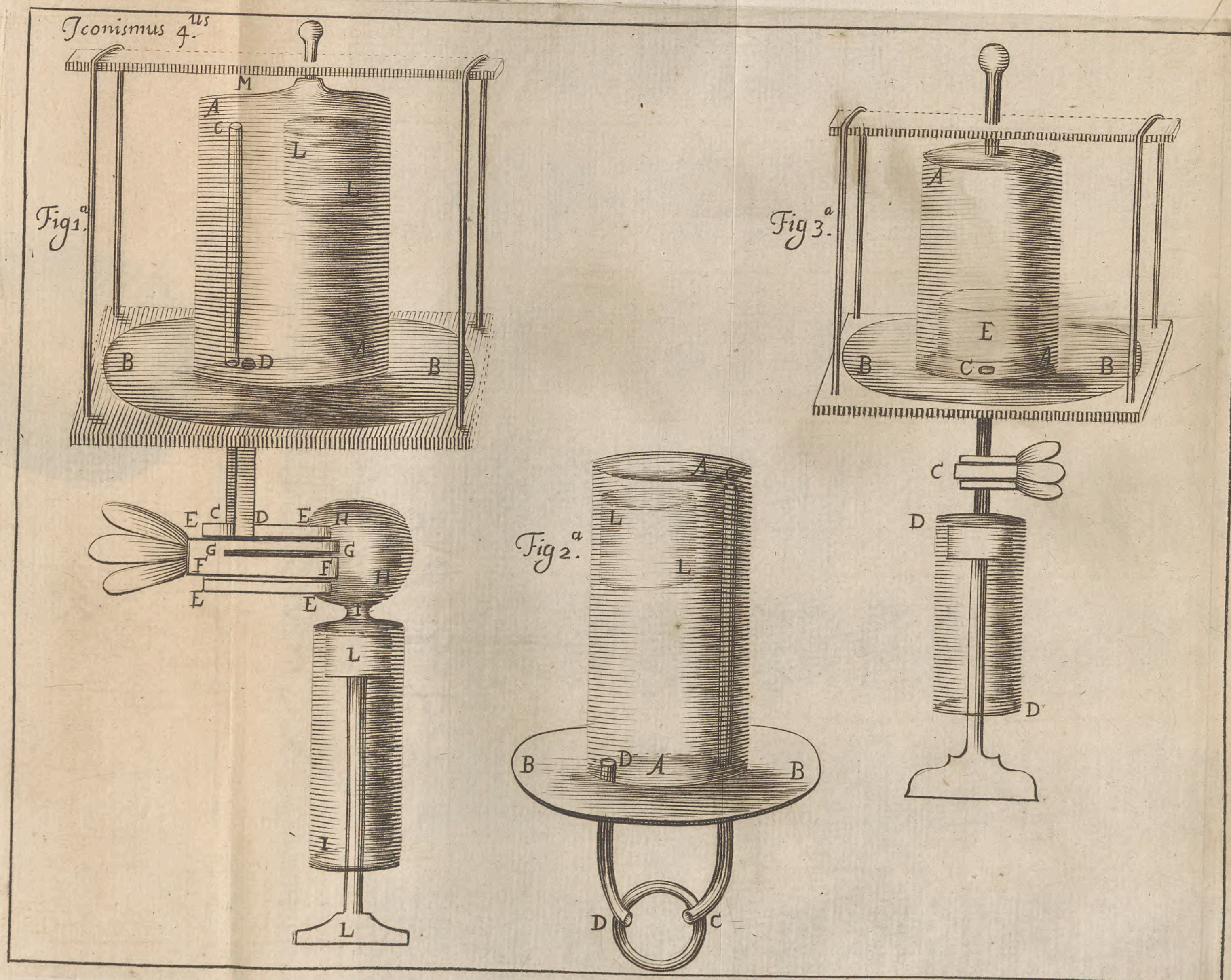
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Fig 1.a
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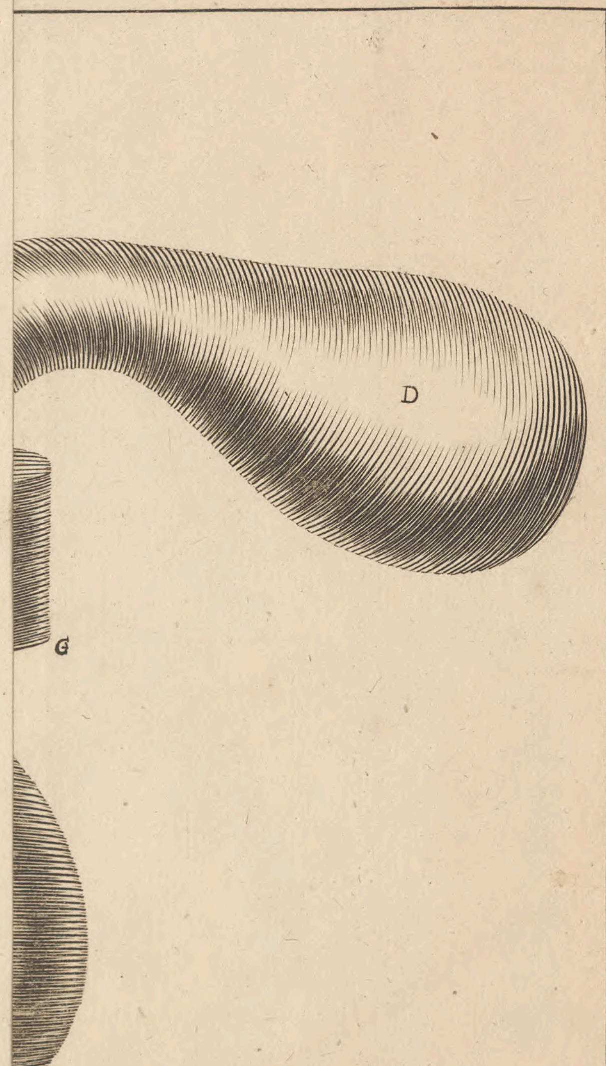
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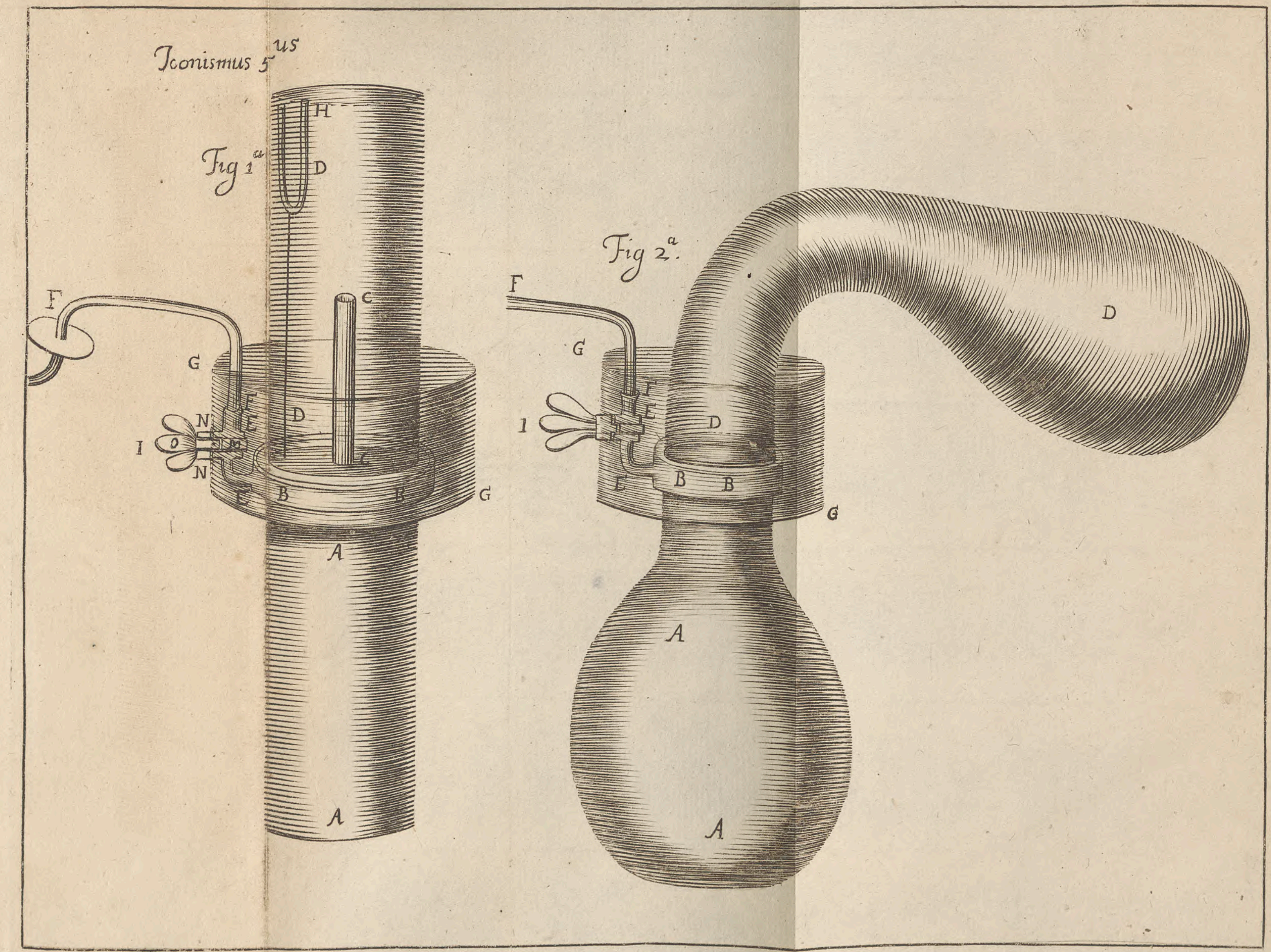
Iconismus 5us
Fig 1a
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Fig 2a
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