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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.

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OBSERVATORIO DE MARINA
DE
SAN FERNANDO.

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*

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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-*

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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*

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appear

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*

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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.*

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In

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

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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 incompt-neſs 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*

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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*

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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 com-*

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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.*

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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,

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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 Catalogüe 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

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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.

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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.

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A Catalogue of the Author's Books.

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*

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A Catalogue of the Author's Books.

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.

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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*

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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.

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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.

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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 Q Q 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

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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 diminished, 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

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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

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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 descends to the height of 29 Digits (I take Digits for Inches throughout all this Tract) and stays at that height, if we mark to what height the Mercury hath ascended into the Tube AB, we may know, that as often as the Mercury in our Gage shall rest at that height, the Air in the same Receiver will be able to sustain onely 29 Digits of Mercury; so that the place in the Gage, or in the Paper semblably divided, must be marked with the figure 29. And so further, every Digit of the descent of the Mercury in the Tube HI may be marked in our Mercurial Gage, and the part AB will be fit to shew all the degrees of the rarefied Air.

But now if the Air be condensed in the Receiver above its wonted pressure, and all ways of its escape be stopped, 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 so much the higher, as the compression of the Air in the Receiver shall be the greater; and how great that is, and what an altitude of the Mercury it can sustain, may easily enough be found out, if the computation be made after the manner following.

It is evident from the Experiments long since published by Mr. *Boyle* in his Answer to *Linus*, That the space possessed by the Air, is diminished in the same proportion, as the compressing force is encreased, and *vice versâ*.

Let then (for Example) the space A be possessed by a certain quantity of Air, when (for instance) the compressing force is F; if now we encrease that force by the addition of G, which is equal to it, it will happen, that our self-same quantity of Air will be reduced to half its space, so that B the remaining space will be the half of the total space A, even as the former pressure F is the half of the total pressure F + G. So further, if we encrease the pressure more by the *Fig.* 2.

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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

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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.

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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 re-ſtraining 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

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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.

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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.

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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

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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.

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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.

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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

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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 screwed 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?

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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 thé 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

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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

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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.

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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 con-

 densed

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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

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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.

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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.

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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

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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.

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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.

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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.

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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.

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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.

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May

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.

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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.

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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 was 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

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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	And Water ſeemed to be expreſſed out of the maſs.
25.		97	
26.		101	
27.		105	*March* 2. Its height was 120
28.		$107\frac{1}{2}$	3. Its height was 121
March 1.		112	4, & 5. It ſtayed at 121

March

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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.

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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

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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 nevertheless, 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.

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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. 9. 10. 11. The height of it was 61, 65, 71, 74

Aug. 13. 14. 15. The height of it was 78, 80, 80

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

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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, 8 } The height of it was } 81, 95

Aug. 9, 10 } The height of it was } 113, 124

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

Aug. 11, 13, 14 } The height of it was } 131, 157, 163

Aug. 15, 16 } The height of it was } 171, 171

17 and the days following the same height remained.

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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 factitious 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 $\frac{1}{2}$ a digit.

17 The height was 1 digit.

18 The height $1\frac{1}{2}$

19 The height almoſt 4 digits.

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

21 The height was $4\frac{1}{2}$

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

26, 27, 30, *Nov.* 2 The height of it was $5\frac{1}{2}$, 6, $6\frac{1}{2}$, $7\frac{1}{2}$

Nov.

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Nov. 6	The height	9	*Nov.* 18	The height	23
8		10	21		26
9	of it was	12	*Dec.* 8	of it was	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

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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 $3\frac{1}{2}$

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	digits
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.

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		The height of it was	digits
Octob.	21		8
	22		10
	23		11
	26		12
Nov.	2		15
	6		17 ½

		The height of it was	digits
Nov.	12		20
	14		20
	16		21
Decemb.	8		26
	16		26 ½
	27		28 ½

Jan. 6. 1678. The height was 32

March 28. 1678. The height was 33 ½.

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 ½.

April 30.

The height of it was 3 ¼.

May 4.

The Mercury was depressed, though no air broke forth, and the Paste was mouldy. The height of it was 2 ½.

		The height of it was	
May	6		2 ¾
	8		3
	10		3 ½
	14		4

		The height of it was	
May	17		4 ½
	20		5
	24		6
	28		8

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June 2	The height	digits 9	*July* 5	The height	digits 13 ½
6	of it was	10	19	of it was	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	5 ½	*May* 24	The height	16
10		7 ½	28		18 ½
14	of it was	10 ½	*June* 2	of it was	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.

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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 ½.

31. There was no change.

June 1, 6, 10 } The height of it was { 3 ½, 4, 4 ½

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, 28, 29, 31, *June* 2 } The height of it was { 4 ½, 5 ½, 7, 9 ½, 12

June 6, 10, *July* 4 } The height of it was { 17, 22, 30

July 19. The Mercury little exceeded 30 digits. This

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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

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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.

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From

From this Experiment it ſeems to follow, That Ale if the Air be wholly excluded from the Veſſel will ferment more ſlowly than if ſome Air were left with it: yet in tract of time, it makes a greater compreſſion, if no place be left for its 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	10	*July*	5	The height	26
	30		16		7	of it was	30
July	1	of it was	19				

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

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

July 1	The ascension was	3	*July* 4	The ascension was	2 $\frac{1}{2}$
2		4	5		3
3		2			

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

July

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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, 15 } The aſcenſion was { 14, 11

16, 17 } The aſcenſion was { 13, 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, 26 } The aſcenſion was { 8, 9

27, 28 } The aſcenſion was { 4, 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.

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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.

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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.

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Sept. 26, 27 } The height of it was } 32 ½, 34

Sept. 29, 30 } The height of it was } 35, 35

October 1. The height remained at 35.

Octob. 2. The height was 36

Octob. 10 The height was 35

5, 6 } The height stayed at 36.

13 The height of it was 32 ½. 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. 1, 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.

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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 } The height { 30 | *Octob.* 10 } The height { 31 1/2
6 } of it was { 31 | 13 } of it was { 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

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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

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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

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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.

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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 $\frac{1}{4}$.

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

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 $\frac{1}{2}$ 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-

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

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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.

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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 ½

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.

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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

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

Octob. 2. The same height continued.

3. The height of it was 52 $\frac{1}{2}$.

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.

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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.

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Aug. 10 { 11 } The height of it was { 6 / 10 } | *Aug.* 13 { 14 } The height of it was { 16 / 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 { 13 } The height of it was { 51 / 61 } | *Aug.* 14 { 15 } The height of it was { 67 / 73 }

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

Aug.

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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

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August 6. The height of the Mercury was 5.

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

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

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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, 5 The height of it was 41, 43 | *Sept.*7, 8 The height of it was 45, 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.

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Aug. 23.

Many of our Fruits appeared rotten and mouldy.

Aug. 24.

The Apricocks were wholly infected with putrefaction and mouldiness.

August 21.

THE THIRD RECEIVER.

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

Aug. 22.

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

Aug. 23.

These Fruits seemed less 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 discern the quantity.

Aug. 26.

Little alteration in the Fruits.

Aug. 28.

The Apricocks began to moisten, yet they were far less altered than those 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 pressure, was 30 digits.

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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.

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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

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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

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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

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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

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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.

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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

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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

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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.

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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

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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 ſinew, appeared wholly putrified.

By this Experiment we have a confirmation, That the quantity of the mouldineſs or ſinew 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.

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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 corrruption 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

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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.

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July

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.

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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.	4	The height of it was	4	*Sept.*	6	The height of it was	$6\frac{1}{2}$
	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.	1	The height of it was	4	*Sept.*	5	The height of it was	19
	2		8		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.

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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,

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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 rarefying, 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

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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.

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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

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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

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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

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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

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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.

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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

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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 revivisence.

EXPERIMENT X.

July 12. 1678.

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

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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.

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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.

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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

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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.

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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 Noon-day 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.

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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.

Weare 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.

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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.

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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

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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 $\frac{3}{4}$ 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-

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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 ¼ 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

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the Cone was not well kindled, seeing at one time it lost only $\frac{3}{4}$, 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.

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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 $\frac{3}{4}$ 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.

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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.

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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

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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.

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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.

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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 $\frac{3}{4}$. Its height was $7\frac{1}{4}$.

Aug. 10	The height of it was	$8\frac{3}{4}$	*Aug.* 18	The height of it was	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 $52\frac{1}{2}$.

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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.

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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.

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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.

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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

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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.

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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 many

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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

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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.

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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-

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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

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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.

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Aug. 26	The height	18	*Aug.* 28	The height	23
27	of it was	21	31	of it was	30
Sept. 1	The height	32	*Sept.* 4	The height	44 1/3
2	of it was	35	5	of it was	45 1/3
3		38 1/3	6		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 1/2.

9. The height of it was 54 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 1/2.

Sept. 13.

I tranſmitted the Air into another Receiver: the height of it was 32 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.

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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.

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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 {75 | *June* 26} The height {90
25} of it was {90 | 28} of it was {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-

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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, 19, 20, 22 } The height of it was { 10, 12, 14, 18

Sept. 23, 24, 26, 28 } The height of it was { 18, 19, 23, 26

Octob: 1. The height of the Mercury was 30.

4. The height of it was 31. 'twas ſomewhat cold.

Octob. 5, 7 } The height of it was { 32, 33

Octob. 9 } The height was 33 ½.

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, 19, 22, 26 } The height of it was { 38, 39 ½, 41, 43

Octob. 29, *Nov.* 2, 5, 20 } The height of it was { 45, 46, 47, 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 $\frac{1}{2}$.

In 10 hours ſpace, the aſcenſion of it was 2 $\frac{1}{2}$.

July 8. In 14 hours the aſcenſion of the Mercury was 1 $\frac{1}{2}$.

In 10 hours the aſcenſion of it was 2 digits.

July 9. In 14 hours the aſcenſion of the Mercury was 2 $\frac{1}{2}$.

In 10 hours its aſcenſion was 1 $\frac{1}{4}$.

July 10. In 14 hours the aſcenſion of it was 1 $\frac{3}{4}$.

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.

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In

In 4 hours the ascension of the Mercury was 4 digits.

July 19. In 14 hours the ascension of the Mercury was 1 ½. 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 ½. and the Paste continued to swell or rise more and more.

In another hours space the ascension of the Mercury was 3 digits and ½.

In 1 hours time the ascension of it was 4 ½ 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.

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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.

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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 ½.

In 8 hours the aſcenſion of it was 1 ½. 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 ½.

21. In 24 hours ſpace the aſcenſion was 5 ½.

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.

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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, 26 } The height of it was { 12, 17 | *July* 27, 28 } The height of it was { 20, 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.

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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 15 16	The height of it was	36 39 41½	hours 24.	17 18 19	The height of it was	44 47 50	hours 24.

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 14 16	The height of it was	5 10 17		*Sept.* 17 19 20	The height of it was	19 23 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.

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Sept. 26	The height of it was	34 ½	*Octob.* 2	The height of it was	39 ½
27		36 ¼	4		39 ½
28		36 ¼	5		40 ½
29		37 ¼	7		41 ½
30		37 ¾	9		42 ½

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

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The Air being transmitted into another Receiver, the height of the Mercury remained at 53 ½.

Aug. 18, 19 } the height of it was { 61, 64 | *Aug.* 20, 21 } the height of it was { 70, 72

The Air being transmitted into another Receiver, the Mercury remained in the height of 61.

Aug. 22, 23 } the height of it was { 68, 74 | *Aug.* 24, 25 } the height of it was { 79, 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, 28, 29 } the height of it was { 60, 68, 75 | *Au.* 30, 31 / *Sept.* 1 } the height of it was { 83, 88, 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.

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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

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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*

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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

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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,

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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.

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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 inessantly 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

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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 liquor 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

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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

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into an exhauſted Receiver, and the Spirit always ſwam on the top; now leſt the Spirit might be ſpilt by bubling above the edges of the Veſſel, I extracted the Air by degrees, and in the beginning great Bubbles came from the Spirit, and but very ſmall ones from the Oil; but after one hours time, the Oil did emit great Bubbles, which being ſmall at bottom, in their aſcent did fill the whole latitude of their Veſſel; and after another hour, ſome Bubbles brake out with ſo great force, that they hit againſt the top of the Receiver.

May 9. I iterated the former Experiment in a Glaſs ſomewhat long and narrow, that I might the better perceive the motion of the Bubbles; and then I ſaw the Bubbles paſſing out of the Oil into the Spirit of Wine, without any great increaſe of their quantity; but being diſtant onely 1 quarter of an inch from the ſuperficies, they were ſuddenly expanded.

EXPERIMENT XIII.

May 3. 1676.

I mixed a certain quantity of *Aqua Fortis* with a quantity of *Spirit of Wine* ſomewhat greater; and then I diſtributed that mixture equally into 3 Glaſs Veſſels, and put three equal pieces of Iron into them, to each Veſſel one. This being done, I included one of the 3 Veſſels *in vacuo*, and there many great ebullitions were made. Then after a quarter of an hour, I took out the Veſſel, and found the liquor black and turbid, whereas the other two Veſſels had their liquor not altered in colour, but onely ſome black powder did appear in the bottom of the liquor.

Of theſe 2 Veſſels, I put one *in vacuo*, and then there aroſe ebullitions, great indeed, but much leſſer than the former: when one quarter of an hour was elapſed, I took the Veſſel *è vacuo*, and found the liquor black indeed, yet ſomewhat leſs ſo than the former; but the liquor which was left always in the Air, did in a manner remain unchanged *May*

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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

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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

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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

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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

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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.

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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.

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In the Lead Cylinder, the Mercury exceeded not the height of 3 digits and ½, 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 common

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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.

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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.

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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.

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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.

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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, 7, 8, 9 } The height of it was } 10, 10, 12, 15

Sept. 11, 12, 13, 14 } The height of it was } 18, 23, 27, 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.

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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, 18 } The height of it was { 10, 15 | *Sept.* 19, 20 } The height of it was { 29, 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

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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, 20, 21	The height of it was	5, 7, 9	*Sept.* 22, 23, 24	The height of it was	11, 12, 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.*

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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.

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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, 27, 28 } The height of it was { 10, 14, 18

Aug. 29, 30, 31 } The height of it was { 21, 25, 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, 27 } The height of it was { 17, 25

Aug. 28, 29 } The height of it was { 28, 30

Aug. 30. The Mercury ascended no higher, because the Receiver was forced from his Cover.

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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, 27, 28 } The height of it was { 9, 12, 15

Aug. 29, 30, 31 } The height of it was { 19, 25, 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.

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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 ½.

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, 29 } The height of it was { 7, 10 | *Aug.* 30, 31 } The height of it was { 12, 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.

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Sept. 11. The height of the Mercury was 6 digits.

Sept. 12, 13, 14	The height of it was	9, 12, 15	*Sept.* 15, 16, 17	The height of it was	20, 25, 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. 24, 25, 26	The height of it was	9, 14, 17	*Sept.* 27, 29, 30	The height of it was	20, 27, 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. 11, 12, 13, 14	The height of it was	4, 7, 10, 12	*Sept.* 15, 16, 17, 18	The height of it was	15, 18, 20, 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

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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

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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.

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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 ſeemedas 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 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.

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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 $\frac{1}{12}$ 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

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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.

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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.

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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.

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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.

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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 ard 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-

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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.

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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.

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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.

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Nov. 26. 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.

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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,

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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.

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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

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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,

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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.

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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

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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.

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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,

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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 Mer-

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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.

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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 ½. The Milk stagnant below the butyrous part, appeared of a red colour.

Jan. 4. The height of the Mercury was 19. Some white serum was 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 af-

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fect 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 $1\frac{1}{2}$.

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Feb. 12. The height of the Mercury in the firſt Receiver was $4\frac{1}{2}$; in the ſecond not above $1\frac{1}{2}$.

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-

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4 digits and ½; 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

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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.

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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

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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.

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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 Va-

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pours, 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

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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

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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.

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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.

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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 *ses* r. *sediment*. p.190.l.20. for 2 *ounces* r. 2 *pounds*.

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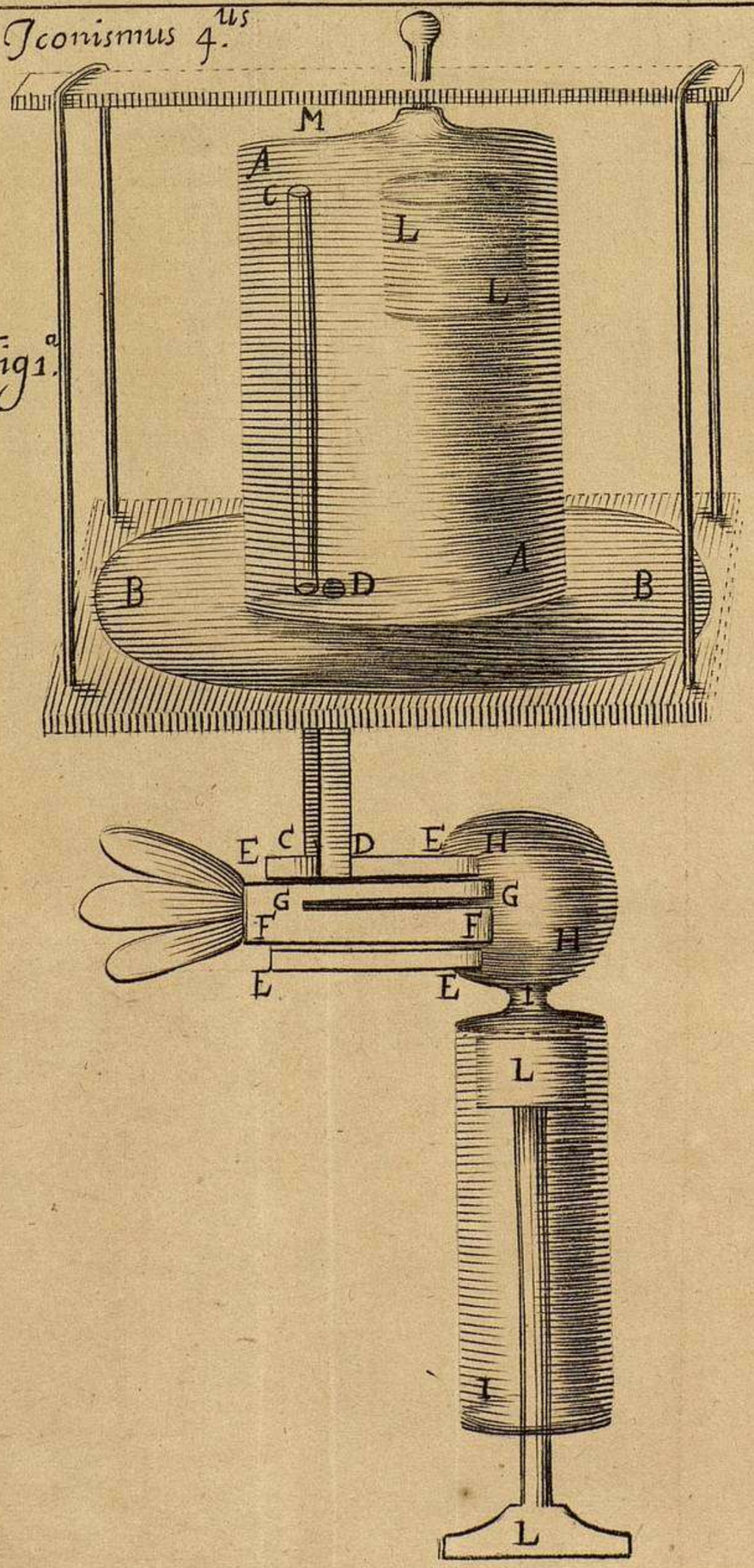
Iconismus 4.us

Fig 1.a



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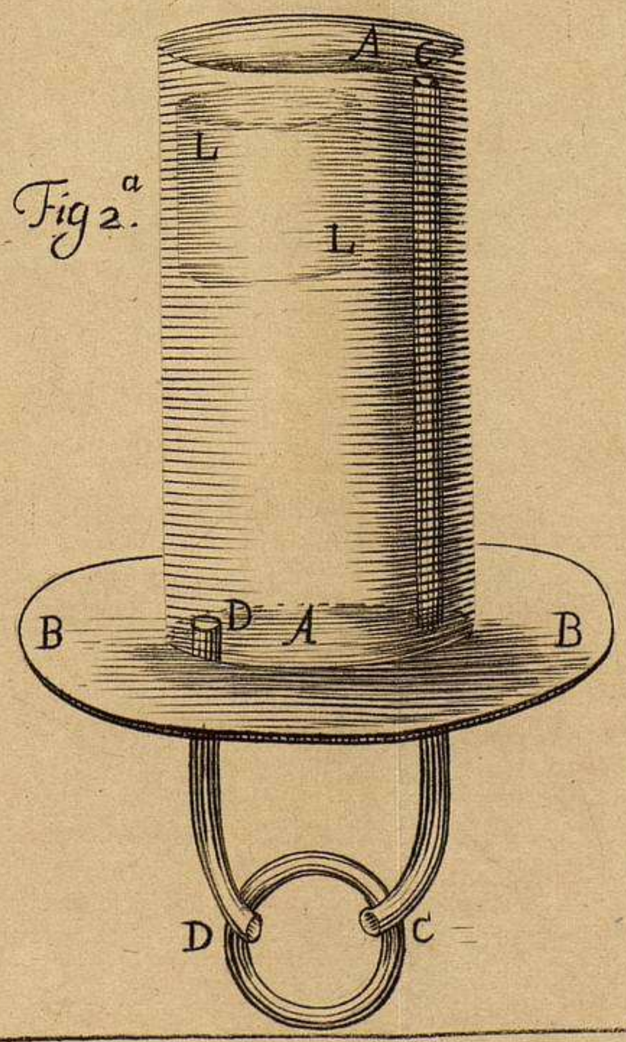

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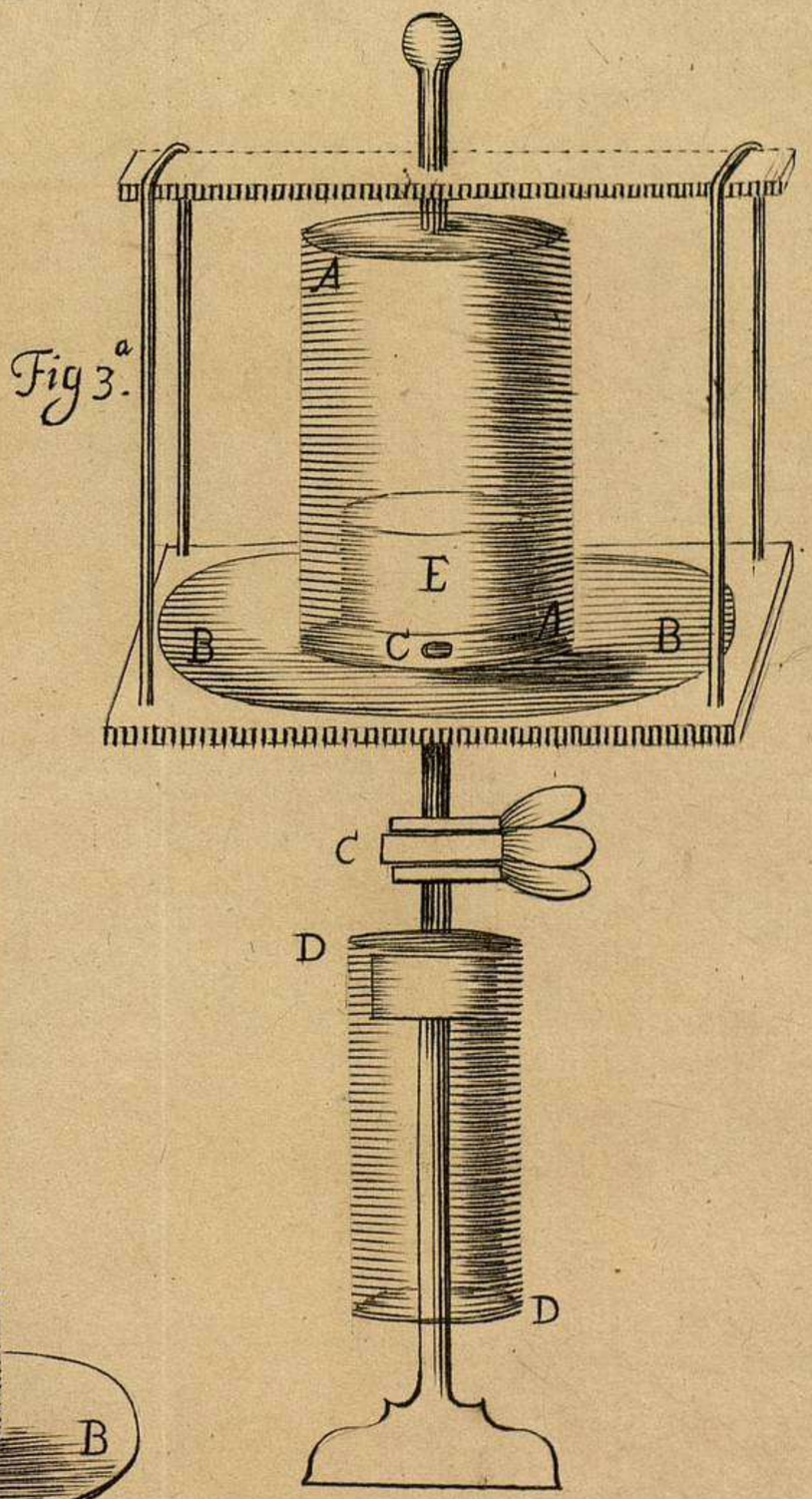

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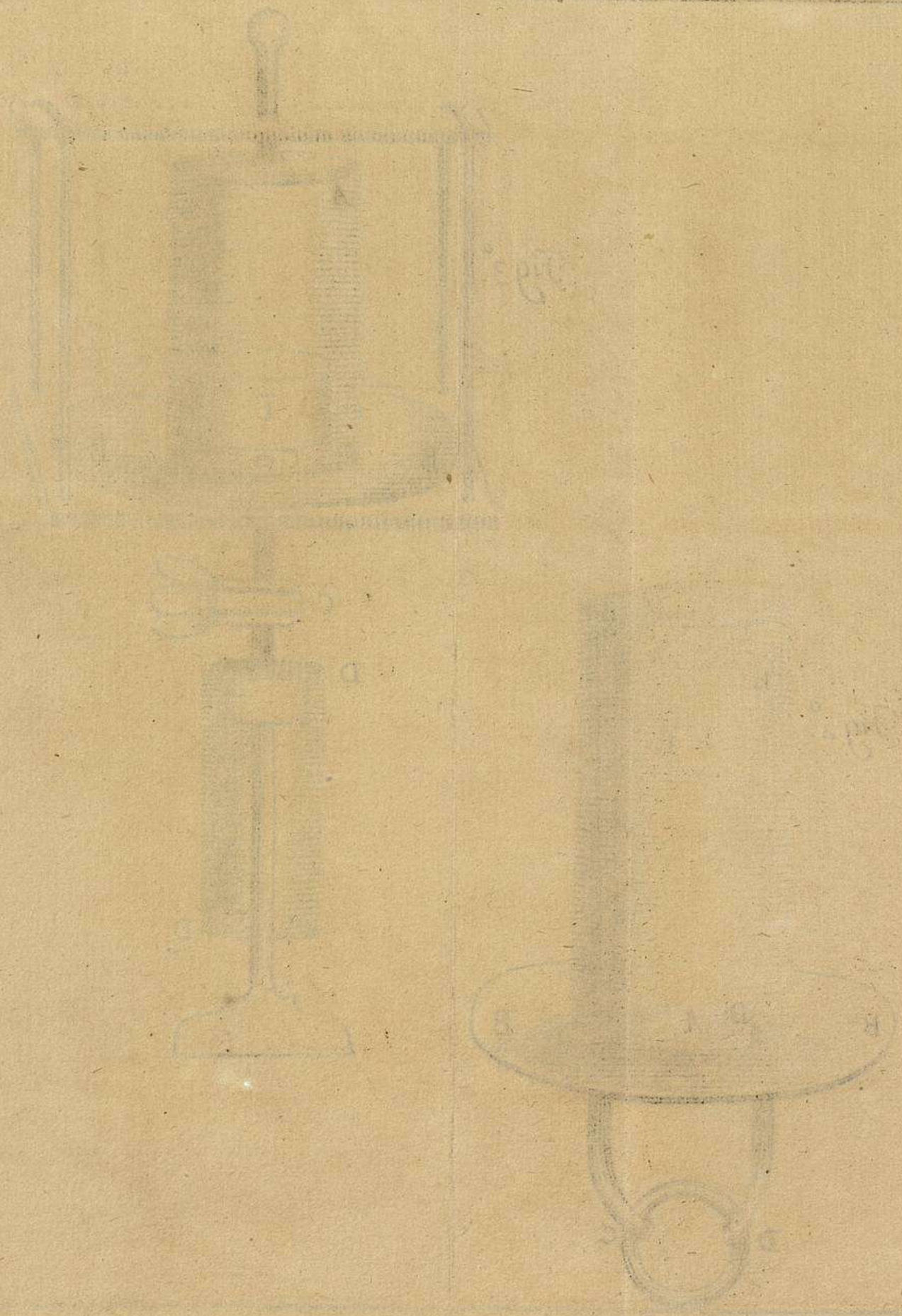

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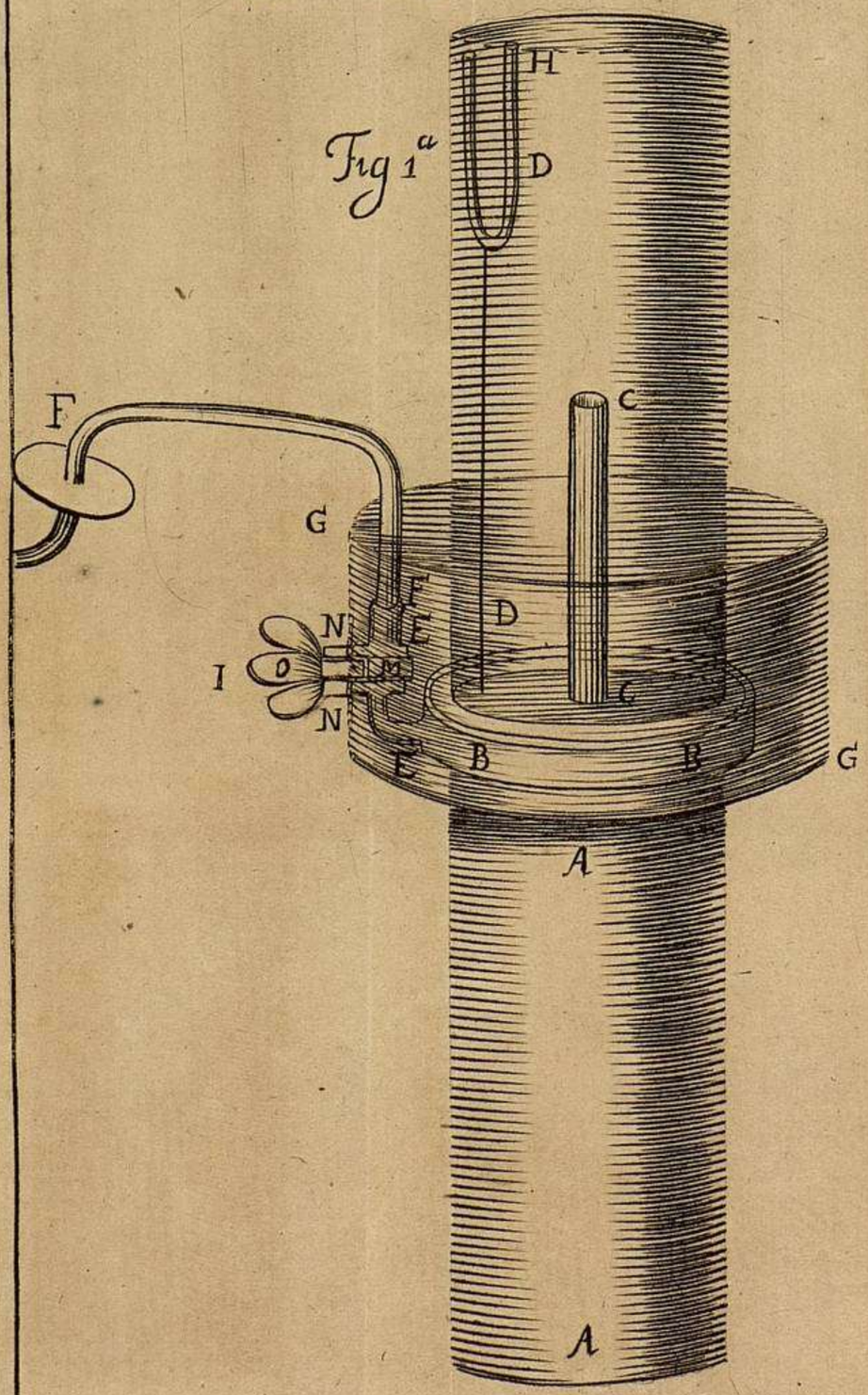

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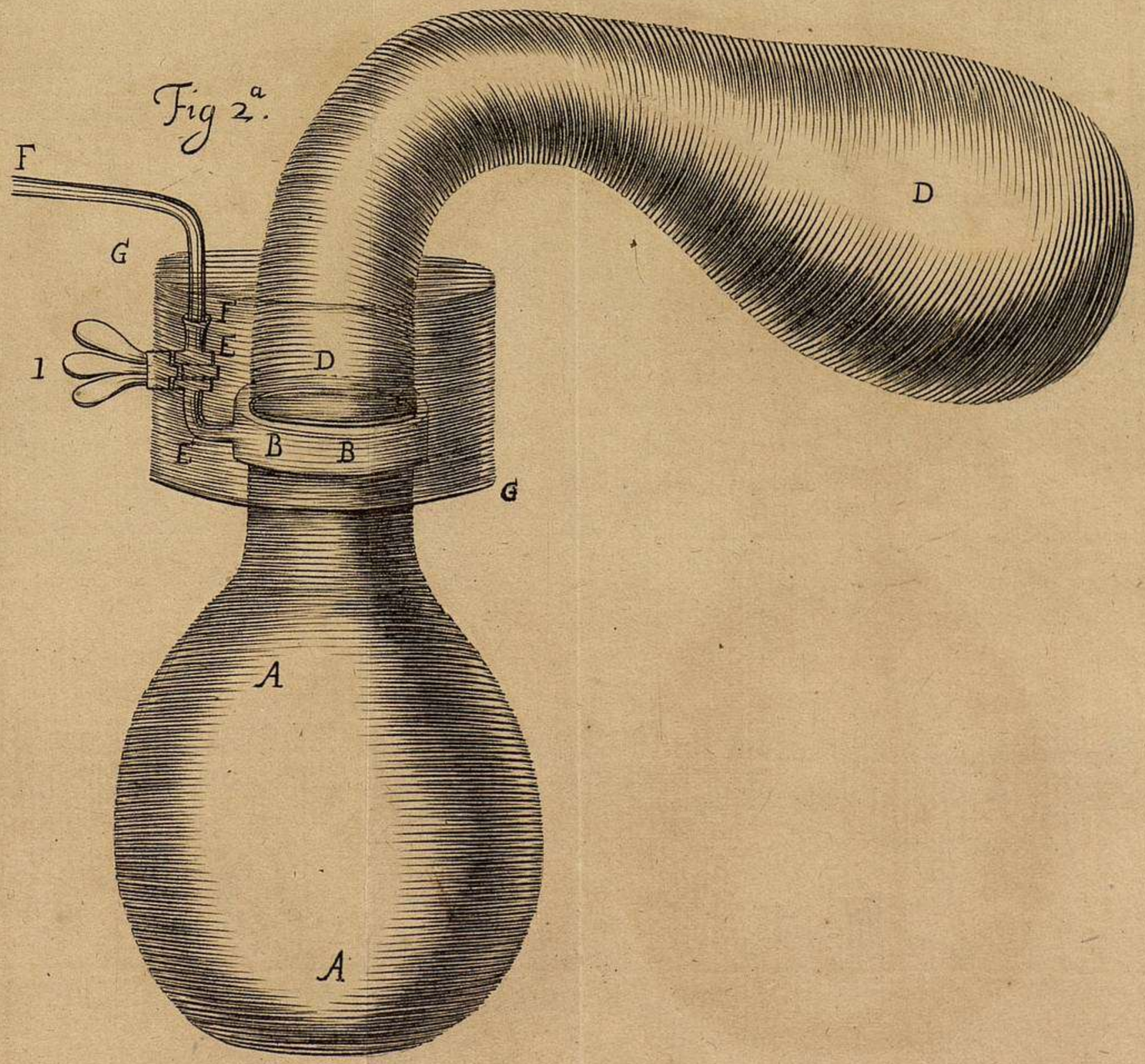

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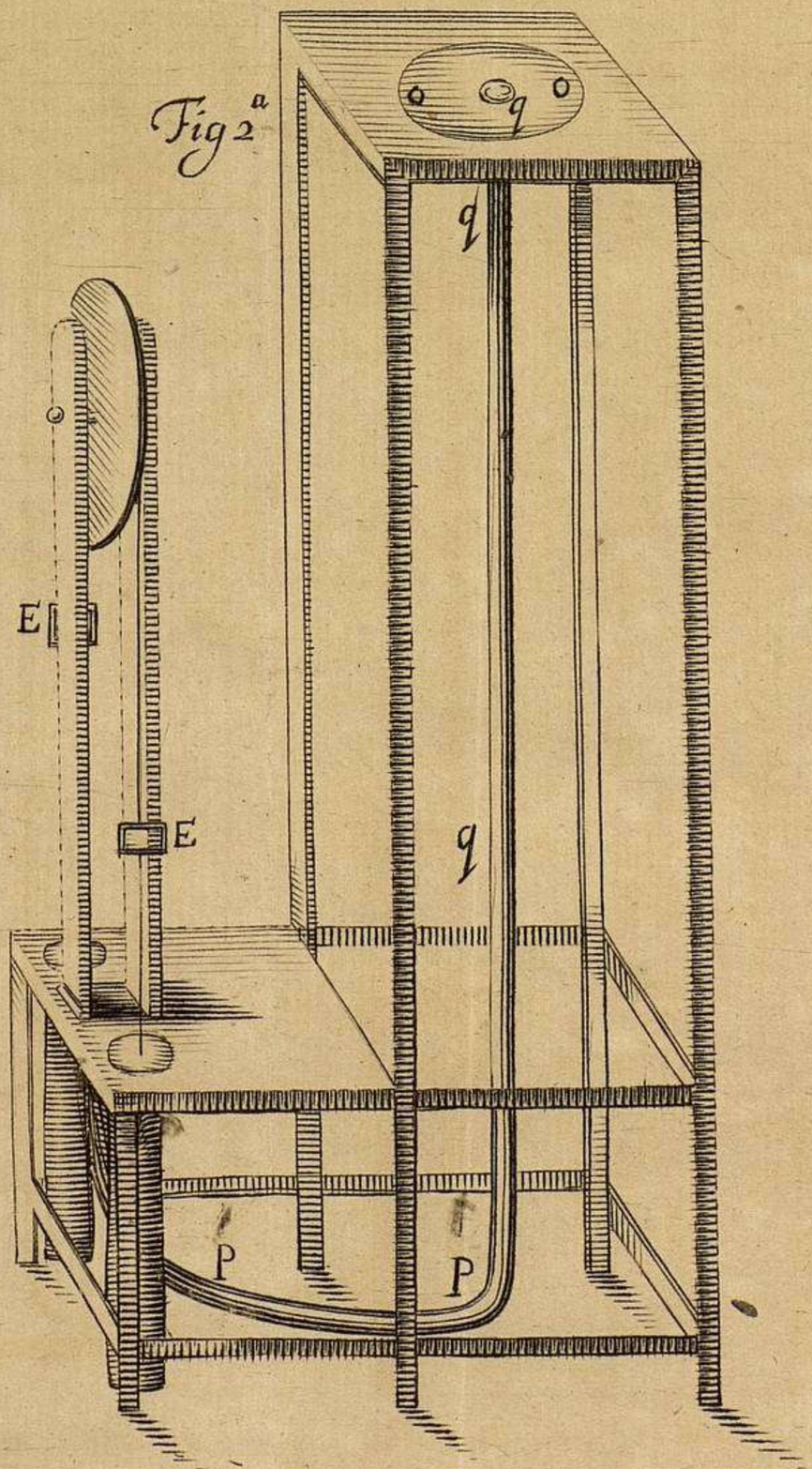

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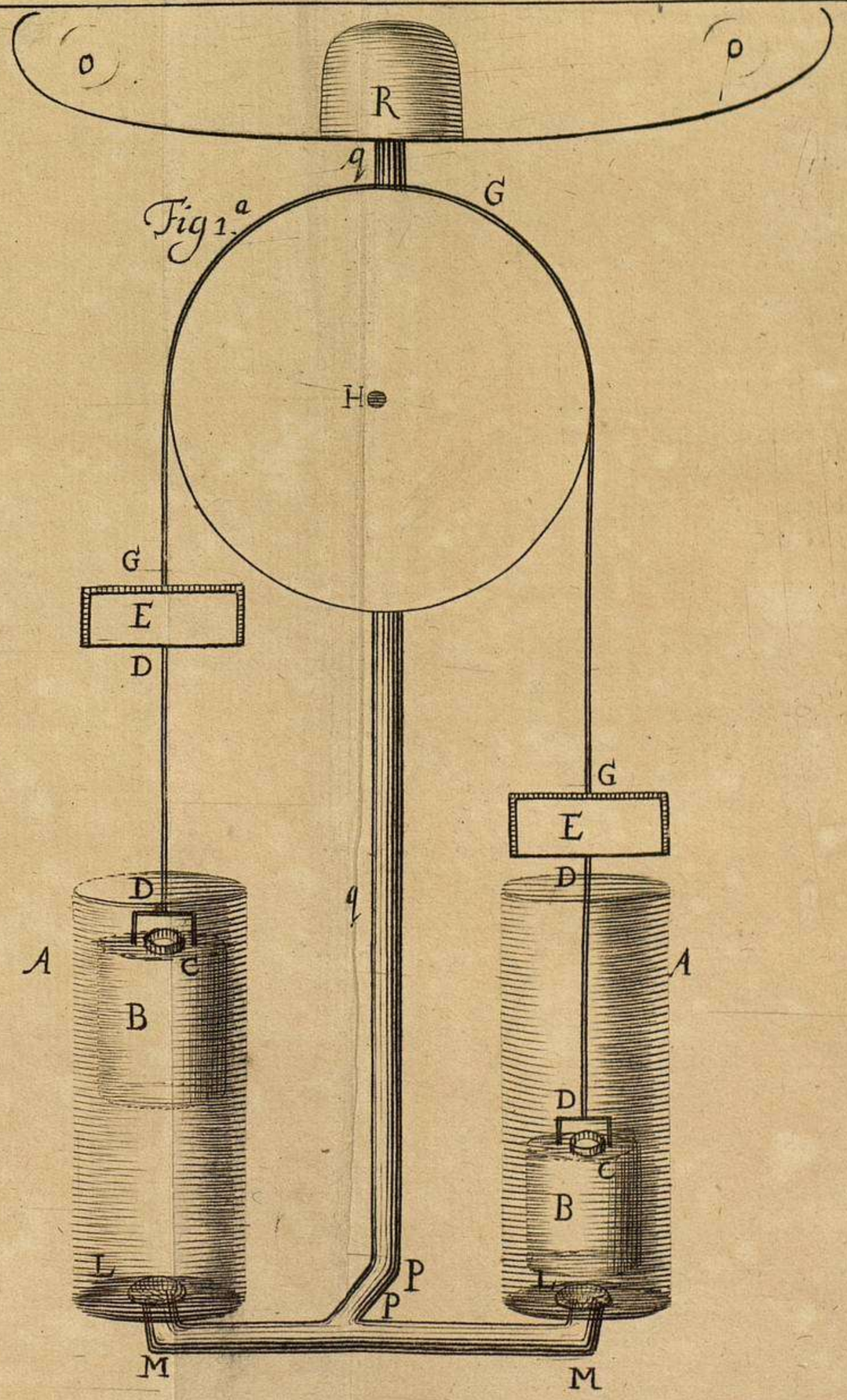

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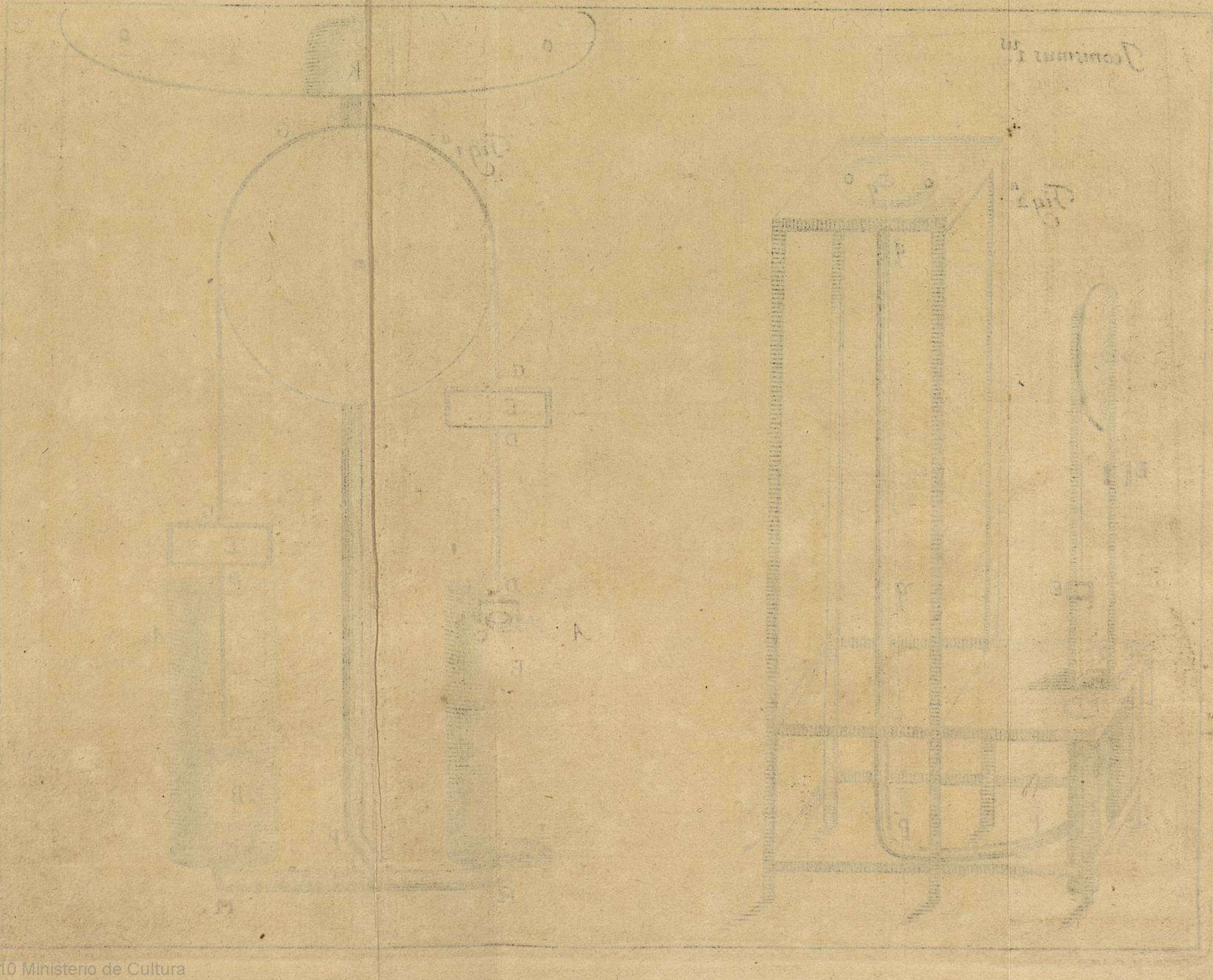

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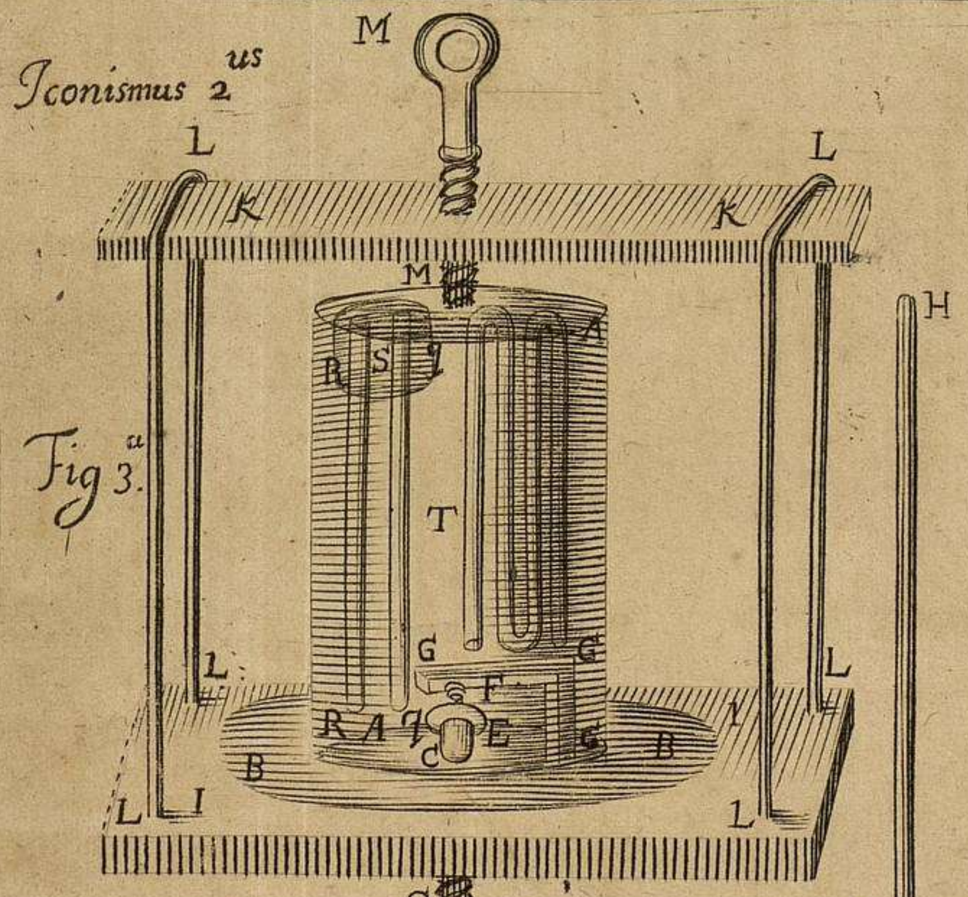

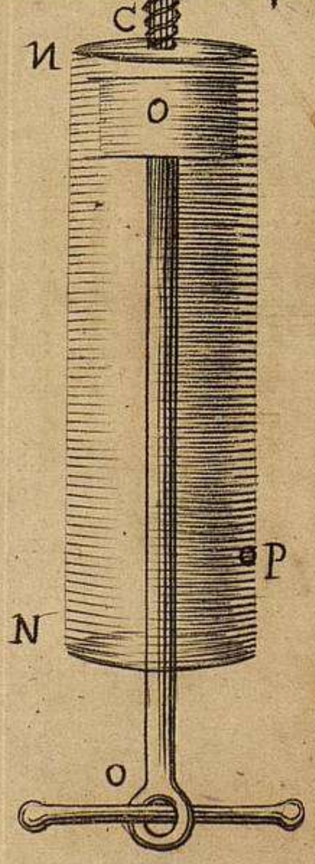

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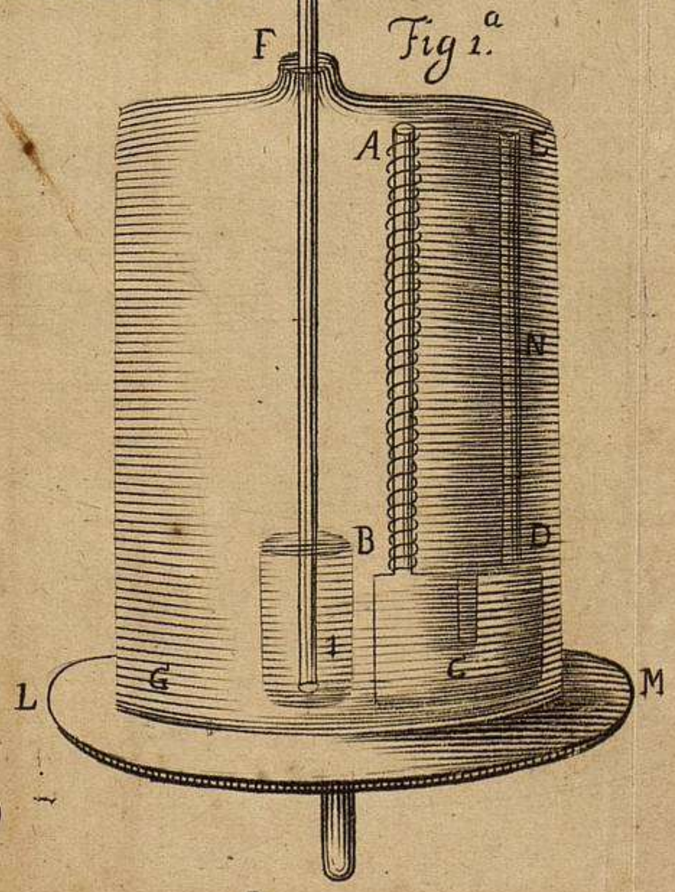

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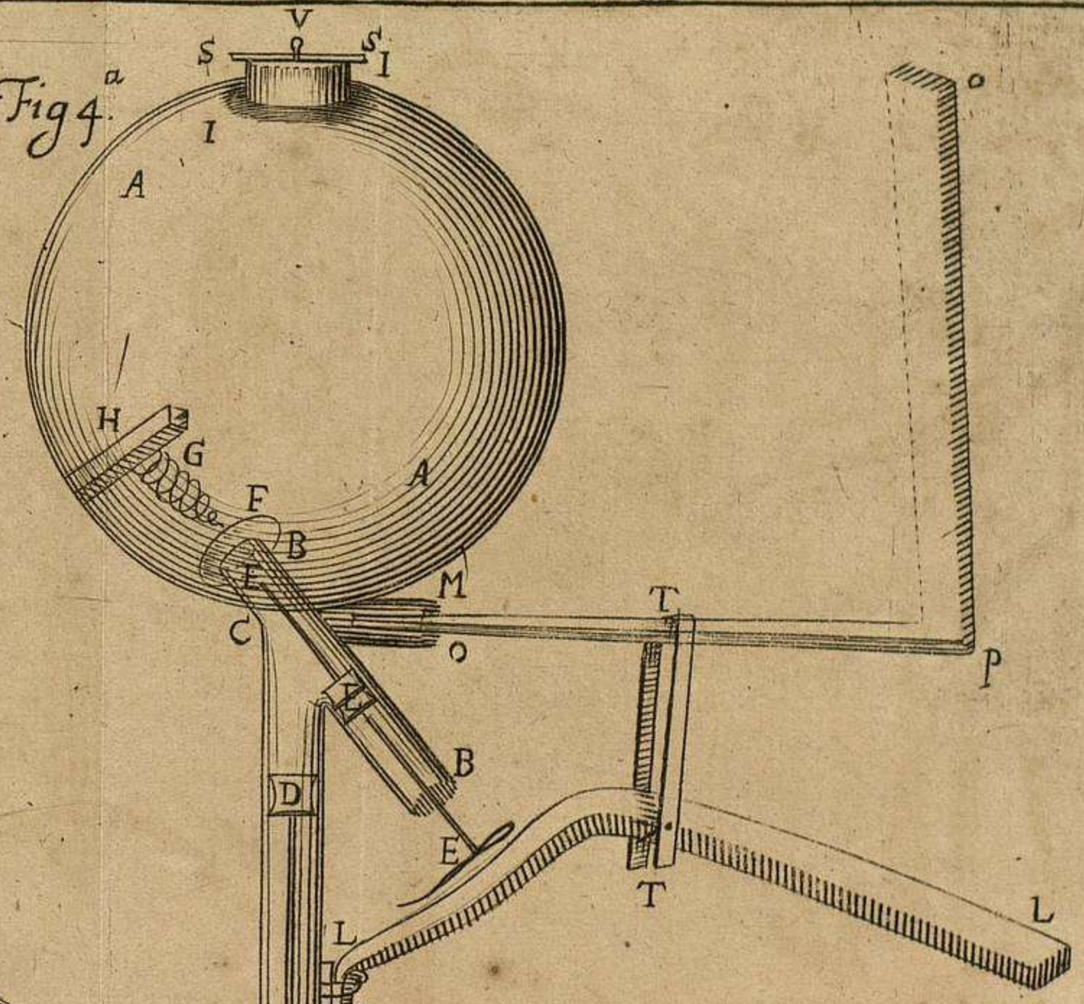

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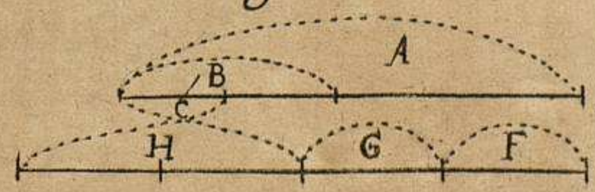

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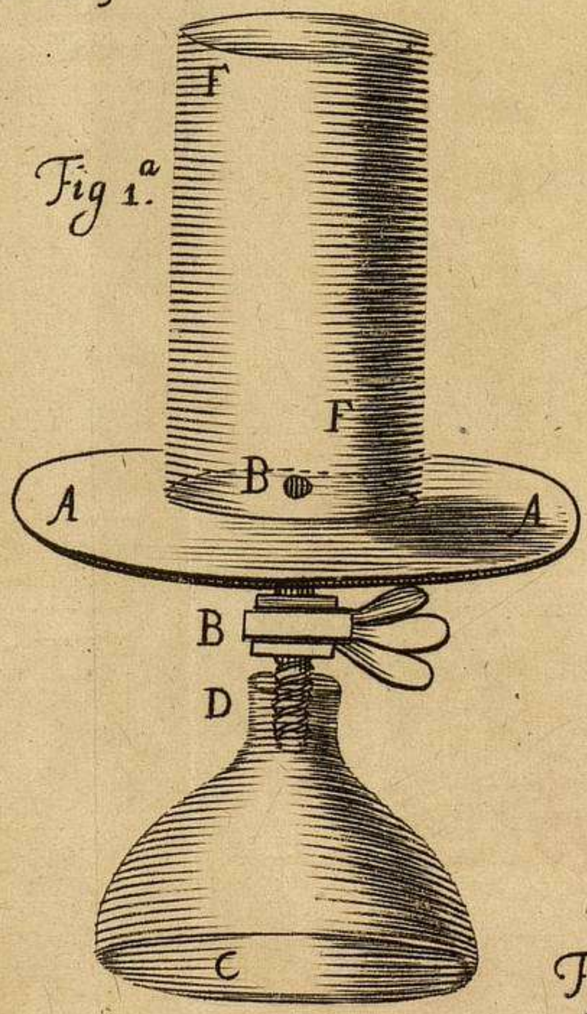
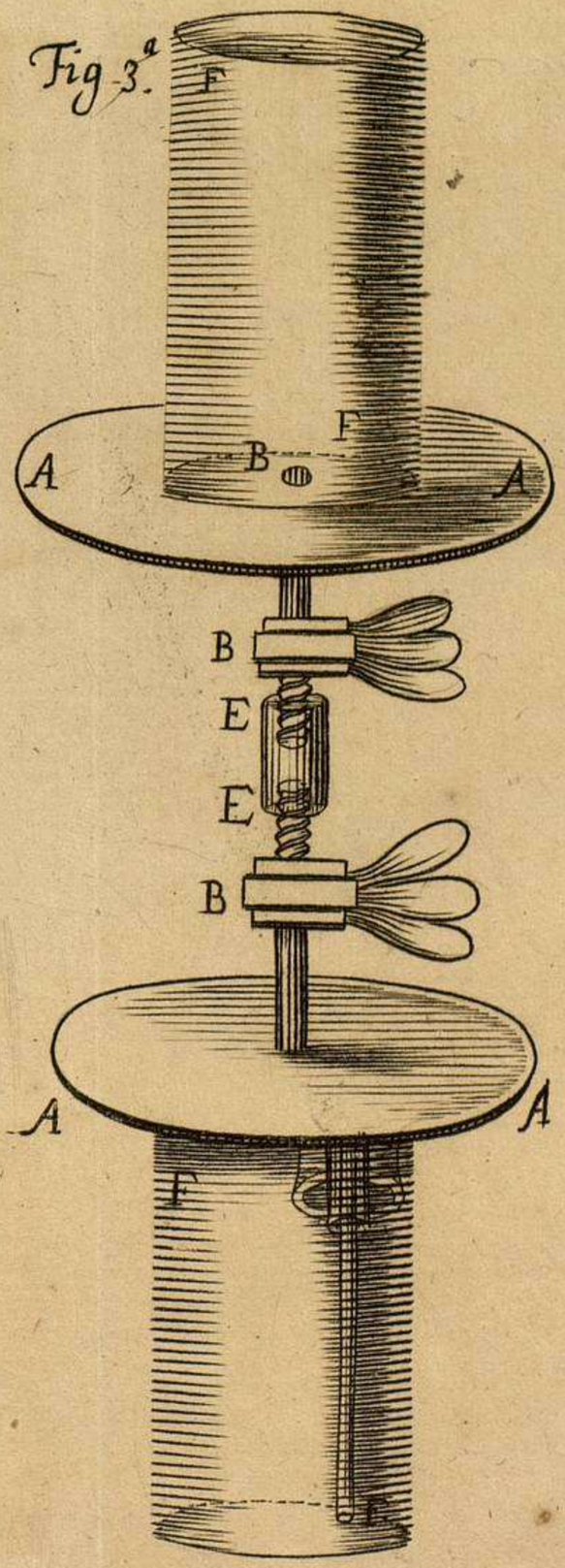


Fig 2a.

Fig 4a.

Fig 3a.



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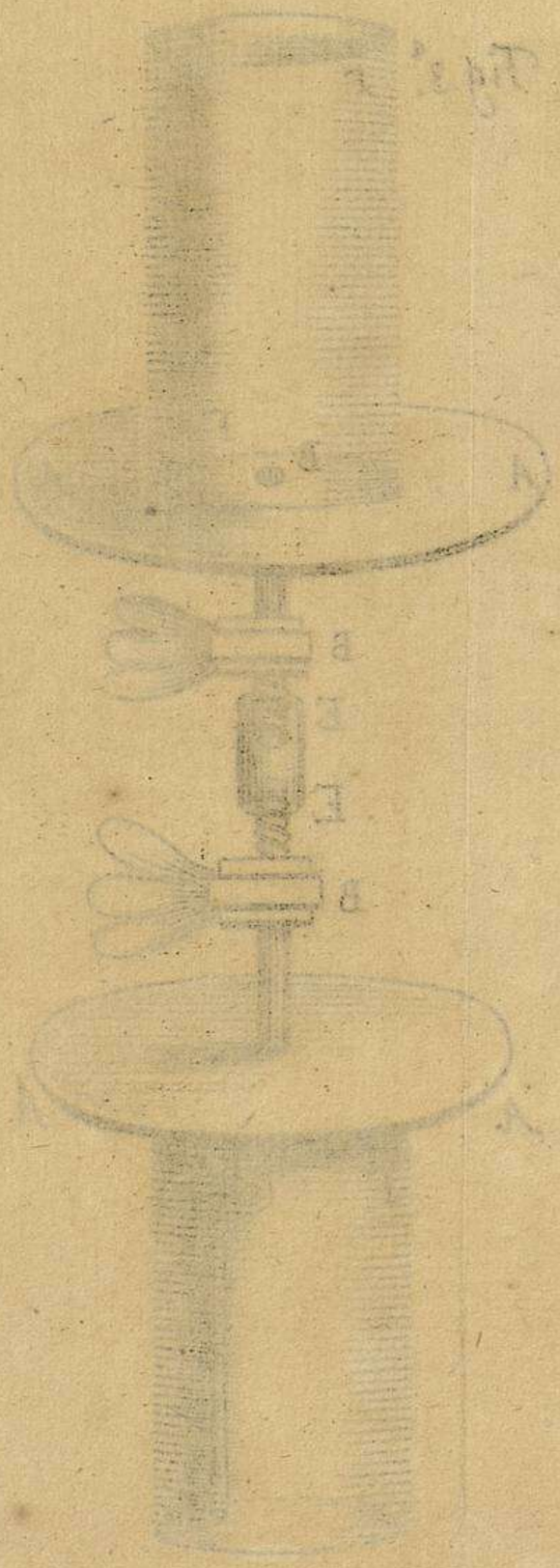

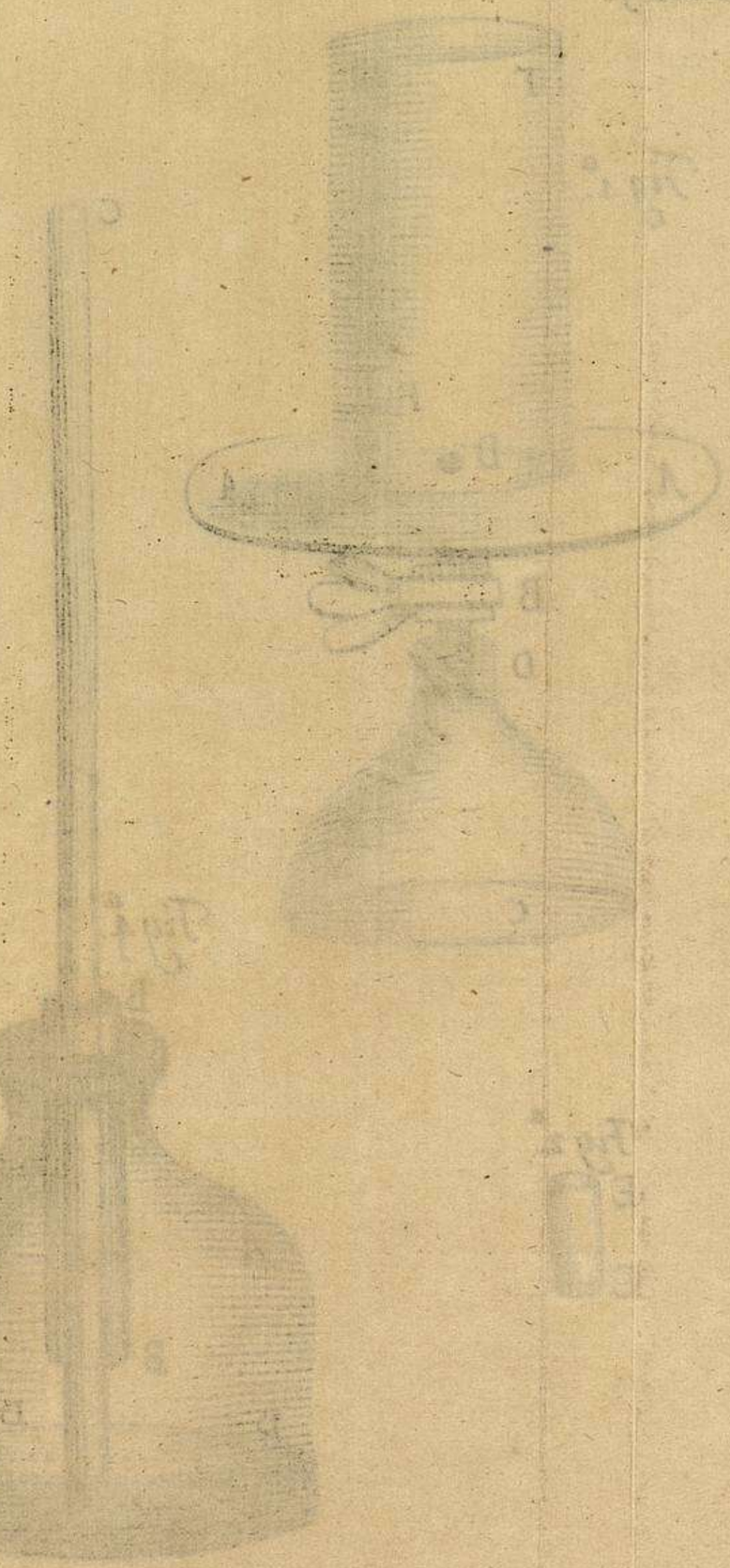

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