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MERTON RUSSELL COTES, J.P., F.R.G.S.

From

The Revd. Chas: H. Cotes M.A. Oxon:
Subwarden of St. Augustine's
College, Canterbury.
Oct 13th/92

To

Merton Russell Cotes Esq. F.R.G.S.
of Bournemouth

with a kinsman's greeting.

J. Turner 1[illegible]

Roger Cotes succeeded Sir Isaac
Newton in the chair of mathematics
at Cambridge. He wrote the Preface
to Newton's Principia. On his
early demise Sir Isaac said
of him, "If Mr. Cotes had lived
we should have known some-
thing.

C. H. C.

HYDROSTATICAL AND PNEUMATICAL LECTURES

BY

ROGER COTES A. M.

Late Profeſſor of Aſtronomy and Experimental Philoſophy at Cambridge:

PUBLISHED WITH

NOTES

By his Succeſſor

ROBERT SMITH LL.D.

Maſter of Mechanicks to His MAJESTY.

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TO

HIS ROYAL HIGHNESS

WILLIAM

DUKE OF CUMBERLAND,

THESE

LECTURES

ARE HUMBLY DEDICATED,

BY

HIS ROYAL HIGHNESS'S

MOST DUTIFUL SERVANT

ROBERT SMITH.

THE EDITOR'S

PREFACE.

THE Course of Hydrostatical and Pneumatical Experiments for which these Lectures were written, was contrived by the Author above thirty years ago, and was one of the first that we had in England of any considerable note. From that time it has been often performed before large assemblies at the Observatory in Trinity College Cambridge; first by the Author in conjunction with Mr. *Whiston*, at that time Professor of the Mathematicks, then by the Author alone, and after his decease by my self: and on these occasions the Lectures were frequently lent out to be transcribed; by which means a copy falling lately into the hands of a Bookseller, was intended to have been printed without my knowledge. By this attempt I was induced to present the Publick with a correct edition of them, printed from the Author's original manuscript.

The general heads of the Course and of the Lectures upon it, may be seen at one view in the following paper, in which every article marked with a Star, referring to a page in the Book, is the subject of a Lecture to be found by that reference. And the whole design is farther explained by the Author in the beginning of his first and twelfth Lectures, where he gives his reasons why he judged it needless to write up-

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As to the method in general of teaching Philoſophy by Courſes of Experiments, (that is, of drawing general truths and concluſions from a ſelect number of ſimple experiments, firſt repreſented to our ſenſes, and then explained to our underſtandings,) it is now ſo much practiſed and approved of by the moſt eminent Profeſſors all over Europe, and has ſo greatly contributed to the propagation and increaſe of knowledge, in the little time it has been duly cultivated, that nothing more need be ſaid to ſhew the uſefulneſs and excellency of it: and as to the Lectures before us, the general ſatisfaction they have given, to all thoſe curious Perſons who have peruſed them in manuſcript, or heard them read, together with the great and eſtabliſhed reputation of their Author, will recommend them to the Publick, much beyond any thing I can offer in their commendation.

In comparing ſome other Works of our Author (*a*) with the Lectures before us, the reader will find this remarkable difference in the ſtyle and manner of writing. In thoſe other works he is generally ſparing of his words and thoughts, that he might not ſeem tedious to able Mathematicians; in theſe he is more liberal and diffuſive in thought and expreſſion, and condeſcends to clear up every appearance of difficul-

(*a*) Harmonia Menſurarum, ſive analyſis & ſyntheſis per rationum & angulorum menſuras promotæ, &c.

ty,

ty, as writing either for beginners or for persons of common underſtandings; and he has rendered theſe Diſcourſes more entertaining and uſeful than ordinary, by enlivening the Science with a mixture of Learning, and the Hiſtory of the Inventions he treats of; in which he has been particularly careful to do juſtice, and to give the deſerved honour to their ſeveral Inventors.

Two or three noble Truths relating to the Preſſure of fluids *(b)*, and the conſtitution of the Atmoſphere *(c)*, the Author has here demonſtrated, from the very firſt mathematical Elements, with ſuch uncommon plainneſs and perſpicuity, as cannot fail of giving pleaſure to all ſorts of mathematical Readers. As to the reſt, I am much miſtaken if Readers of good ſenſe and ſome command of attention, though unſkilled in Mathematicks, may not go through it with as much eaſe and pleaſure, as in reading a piece of hiſtory; and upon making the tryal, they may poſſibly conceive a better opinion of their own underſtandings, with regard to theſe matters, than ever they did before.

Nevertheleſs it may probably be ſurmiſed, as theſe Diſcourſes were written on purpoſe to deduce the properties of fluids from a Courſe of Experiments, actually performed and explained before the reading of each Lecture, that therefore they may not be ſo well adapted to Readers in

(b) Lect. iii. *(c)* Lect. ix. and xiv.

ral, as to thoſe who have ſeen that Courſe, or ſome other of the ſame kind. It muſt indeed be confeſſed, that the latter Readers would have ſome advantage over the former, in as much as the Author intending to ſhew the experiments, belonging to each Lecture, immediately before the reading of it, does not always deſcribe them, but frequently reaſons upon them as things already ſeen and underſtood. But as I have taken due care to ſupply ſuch defects, by giving compleat deſcriptions of the Experiments in Notes, referring to very good Figures of the proper *Apparatus*, I preſume I have entirely removed the ſuſpicion abovemention'd; and have rendered the lectures as clearly intelligible, and as well adapted to the taſte and capacity of all ſorts of readers in the form they now have, as if the matter of the Notes had been every where inſerted in the context of the Lectures, even by the Author himſelf. I have alſo added here and there a few Mathematical Notes, upon ſuch points as the Author had but lightly touched upon, or only mentioned, but thought improper to be introduced into popular diſcourſes.

When he exhibited and explained his Inſtruments for determining the ſtate of the weather, as Barometers, Thermometers and Hygrometers of various ſorts, for a Lecture on this ſubject he conſtantly read Dr. *Halley*'s *Account of the riſing and falling of the mercury in the barometer, upon change of weather*, as what he perfectly approved. With the Doctor's leave I have

therefore

therefore reprinted that excellent Discourse of his from the Philosophical Transactions, and placed it in an Appendix to these Lectures: and have added to it a Translation of Sir *Isaac Newton*'s *Scale of degrees of heat*, taken also from the Transactions; as directing us, among other great curiosities, to such a Construction of Thermometers, as shall cause them to denote the same degree of heat, though they have never been actually compared together: which useful property is still wanting in all the Thermometers I have yet seen. I have therefore fully described this Construction at the end of that admirable paper.

For the uses of Hygrometers Mr. *Cotes* referred his pupils chiefly to Mr. *Boyle*'s Tracts upon that subject.

Lastly, because in the Lecture upon Capillary Tubes many particulars are wanting, which since the Author's decease have been discovered and explained by Dr. *Jurin*, his particular friend, as well as my own, I could not do better, with the Doctor's leave, than close my Appendix with his very ingenious Discourses upon that curious subject: and to make the Book more useful, I have added an alphabetical Index of the matters contained in it.

The

The heads of a Course of Hydrostatical and Pneumatical Experiments, as performed at the Observatory in Trinity-college, CAMBRIDGE.

Hydrostatical tryals and conclusions.

THAT *fluids gravitate* in proprio loco, *the upper parts continually pressing upon the lower; that this pressure is not only propagated downwards, but even upwards and side-ways, according to all possible directions; that a lighter fluid may gravitate upon an heavier, and an heavier upon a lighter.* * Page 1. Lect. i.

That a fluid may sustain a body heavier in specie *than itself, and even raise it up; that a fluid may detain a body lighter in specie than itself, and even depress it; that a competent pressure of a fluid may produce the remarkable phænomena of the Torricellian tube, pump, syringe, siphon, polished plates, and other effects of the like nature.* * Page 10. Lect. ii.

That fluids press according to their perpendicular altitudes, whatever be their quantities, or however the containing vessels be figured; the exact estimate of all manner of pressures; the invention of the center of pressure, upon any proposed plain, reduced to the problem of finding

Pneumaticks illustrated by experiments for the most part tubular, being such as were wont to be made before the air-pump was invented.

The

The more known properties of the air established by the air-pump, and other engines.

thereby: a ſecond enquiry into the ſtate of the atmoſphere. * Page 153. Lect. xiv.

The weight, preſſure and ſpring of the air proved ſeveral ways; by the ſenſe of feeling, by breaking glaſs vials, by the phænomena of bladders, glaſs bubbles, fountains, the gardener's watering pot, the diving bell, &c.

Syphons, ſyringes, poliſhed plates, the Torricellian tube in vacuo; *quickſiver raiſed to the uſual height of the weather-glaſs by the bare ſpring of a little included air;* Otto Guericke's *hemiſpheres.*

The ebullition of liquors in vacuo, *the quantity of air contained in them, the ſuſtentation of fumes and vapours, the deſcent of bodies in* vacuo, *the refraction of air.*

The more hidden properties of the air conſidered by the help of the like engines.

The influence of the air examined as to the cauſes of magnetiſm, the elaſticity of ſprings, the ſphericity of the drops of fluids, the aſcent of liquors in capillary tubes, the reflection of light from the farther ſurface of glaſſes, &c.

The influence of the air as to ſounds, fire and flame, the conſumption of fuel. * Page 168. Lect. xv.

The

CONTENTS
OF THE
APPENDIX.

GEORGE R.

GEORGE the Second, by the Grace of God, King of *Great-Britain, France* and *Ireland*, Defender of the Faith, *&c.* To all, to whom these Presents shall come, Greeting. Whereas Our Trusty and wellbeloved *ROBERT SMITH*, Doctor of Laws, Professor of Astronomy and Experimental Philosophy, in Our University of *Cambridge*, has humbly represented unto Us, that he has, at his own Expence, Printed and prepared for Publication, a Book in Octavo, Intituled *Hydrostatical and Pneumatical Lectures by* Roger Cotes, *A. M. late Professor of Astronomy and Experimental Philosophy at* Cambridge, *published with Notes by his Successor* Robert Smith, *LL. D.* to the Copy of which he has the Sole Right and Title; and has therefore humbly besought Us, to grant him Our Royal Privilege and Licence for the Sole printing and publishing the said Book, for the Term of Fourteen Years; We, being willing to give all due Encouragement to Works of this Nature, that tend to the Advancement of Learning, are graciously pleased to condescend to his Request; and do therefore, by these Presents, so far as may be agreeable to the Statute in that behalf made and provided, grant unto him, the said *ROBERT SMITH*, his Heirs, Executors and Assigns Our Royal Privilege and Licence for the sole printing and publishing the said Book, for the Term of Fourteen Years, to be computed from the Date hereof; strictly forbidding and prohibiting all Our Subjects within Our Kingdoms and Dominions to reprint, abridge or translate the same, either in the like or any other Volume or Volumes whatsoever; or to import, buy, vend, utter or distribute any Copies of the same, reprinted beyond the Seas, during the said Term of Fourteen Years, without the consent and approbation of the said *ROBERT SMITH*, his Heirs, Executors, and Assigns, by Writing under his, or their Hands and Seals first had and obtained, as they, and every of them offending herein, shall answer the contrary at their Perils; Whereof the Commissioners and other Officers of Our Customs, the Master, Wardens and Company of *Stationers* of Our City of LONDON, and all other Our Officers and Ministers, whom it may concern, are to take Notice, that due Obedience be given to our Pleasure herein signified. Given at Our Court at St. *James*'s the Seventh Day of *November*, 1737 in the Eleventh Year of our Reign.

By His MAJESTY's Command,

HOLLES NEWCASTLE.

M^R C O T E S's

HYDROSTATICAL

AND

PNEUMATICAL

LECTURES.

LECTURE I.

That fluids gravitate in proprio loco, *the upper parts continually pressing upon the lower*; *that this pressure is not only propagated downwards but even upwards and sideways according to all possible directions*; *that a lighter fluid may gravitate upon a heavier and a heavier upon a lighter.*

BEFORE we begin our experiments it may be convenient to give some account of the method I intend to follow throughout the whole course, and to allot to each of the four weeks, in which I hope to finish my design, its distinct share of the tryals, by which I propose to make out and clear up those truths and conclusions in hydrostaticks and pneumaticks, which seem to be the most fundamental and of the chiefest importance. Hydrostaticks and pneumaticks have in nature so near a relation to each other, that they ought never to be separated: being therefore both comprehended in one course, they are disposed into that order which was judged to be the most natural for deducing one consequence from another which was proved before, moſt eaſy for performing the experiments, thoſe being uſually placed together which require a very little different *apparatus*, and moſt conformable to the ſucceſſion of times in which the experiments and the truths eſtabliſhed from them appeared in the world.

In this firſt week then I ſhall endeavour to ſettle the grounds of hydroſtaticks, and to account for thoſe various effects which depend upon the preſſure of fluids againſt other fluid and ſolid bodies. This ſcience as it began firſt to be cultivated and was brought to a conſiderable degree of perfection by the ſagacity of the great *Archimedes*, ſo is it the moſt ſolid foundation upon which our modern philoſophers have built their admirable doctrine of the air's preſſure: which notion ſeems firſt to have been ſtarted about an hundred years ago by the famous *Galilæo*, and was happily proſecuted by his ſcholar *Torricellius*, and after him by ſeveral of the moſt eminent *Virtuoſi* of *Europe*. Whoever ſhall attentively conſider the ſeveral phænomena which I ſhall this week prove to depend upon the gravitation of fluids in general, will find it no difficult matter, when we come afterwards to the air, which is a particular fluid, and which I ſhall prove to be not devoid of weight, as it was formerly thought to be, to explain by the gravitation of this ſubtle fluid thoſe once ſurpriſing effects which ſo much puzzled the philoſophers, and compelled them for concealing their ignorance to run into very great abſurdities.

Having aſſigned to this firſt week the buſineſs of hydroſtaticks, in the next I ſhall enter upon pneumaticks, which furniſh us with the knowledge of the nature and properties of the air. I need not here inſiſt upon the uſefulneſs of this ſcience, ſince it cannot but be well known to every one, how much all natural philoſophy depends upon it; there being no

one

one body in the world that has a more univerſal influence upon the general courſe of nature than the air. The famous *Torricellian* experiment was that which firſt alarmed the philoſophick world, and his mercurial tube has juſtly been as much celebrated for opening the way to pneumatical diſcoveries, as his maſter *Galilæo*'s optick tube has been for the advancement it has made in aſtronomy. After *Torricellius* many *Italian*, *French* and *Engliſh* gentlemen made ſeveral other, for the moſt part, tubular experiments, to illuſtrate and confirm the doctrine of the air's preſſure before the air-pump was invented. I have therefore in the ſecond week propoſed to exhibit thoſe tubular experiments which were commonly made before the air-pump was uſed, adding to them ſome others of the like nature that have ſince that time been thought on.

In the third and fourth weeks I deſign to repeat the principal experiments that are wont to be tried with the air-pump and condenſer, ſuch of them at leaſt as the *apparatus* of inſtruments which we have at preſent are capable to exhibit. For though our air-pump be as commodious as any that has yet been made, yet our condenſer is not ſuch as I could wiſh for, and I hope ſome time or other to be furniſhed with an engine which will perform both parts with more advantage than can be expected from our preſent inſtruments, and that the reſt of our *apparatus* may be ſo enlarged as to afford us a greater plenty of experiments. In our third week then I intend to make ſuch tryals as ſeem to be moſt pertinent for eſtabliſhing the principal properties of the air, ſuch as its weight, ſpring, preſſure, reſiſtance, refraction; explaining by theſe properties ſeveral remarkable effects. Now though ſome of theſe properties will be deduced from the tubular experiments of the foregoing week, yet I ſuppoſe that nobody will be diſpleaſed to see such things, which were once and not long ago very much controverted, made out by more ways than one.

To the fourth and last week I have allotted such experiments as shew some very notable effects of the air, which depend upon more hidden properties than those explained in the third week. Amongst these we may reckon the experiments upon the life of animals, upon flame, and others of that nature. And because several effects have been by philosophers ascribed to the air which do not belong to it, I design at the same time to confute those errors by matter of fact and experience. This is the method which I have proposed to myself in our present course of experiments.

Omitting then all further preliminaries, let us begin with hydrostaticks. The signification and reason of the name need not here be explained, nor need I tell you that by hydrostaticks is now commonly understood that part of natural philosophy which considers the equilibrium and pressure of fluids in general, though that word seems to be restrained to water, which is a particular fluid and the most obvious of all others, and by means of which we shall make out most of our following conclusions. For whatever can be proved by experiments and reason to belong to any one body which is both fluid and heavy, upon account of its being fluid and heavy, must belong to all other bodies which are fluid and heavy. If a due caution be observed, we may very safely be allowed to draw general conclusions from experiments, which cannot otherwise be made but upon particular bodies.

Fluidity and gravity being the qualities which are of great concern in hydrostaticks, it will not be amiss to say something concerning each of them in this place. It is beside our purpose to enquire into the

the caufes of thefe two properties of bodies; I fhall only obferve of fluids that they are fuch whofe parts yield to any force exercifed upon them, and by yielding are eafily put in motion amongft one another. But whence this difpofition arifes of giving way to, and being moved by every the leaft impreffion is a problem not readily to be folved; I had rather wholly pafs it by than propofe uncertainties about it. The famous *materia fubtilis* has been a conftant refuge to our modern philofophers as well in this as in all other difficulties, but that way of juggling does now at laft very juftly begin to be out of credit, and ought altogether to be laid afide, till fufficient reafons can be produced for its admittance.

Gravity is well known to be the endeavour and tendency of bodies towards the center of the earth. It is a property of fo univerfal an extent that no one body in the world is yet known to be without it; not air, which as I fhall afterwards fhew, may be weighed in the ballance; nor fumes and vapours, which feem to be lighter than air itfelf by their afcending in it, as I fhall prove by experiment; and the excellent Mr. *Boyle* has found out ways to make even flame itfelf ponderable.

Now though moft men without any difficulty allow thus much, that water and other fluids are really ponderous, and do actually gravitate when taken as a whole body, being convinced by their fenfes that a veffel weighs lefs when empty than when filled with any fluid, and weighs ftill more as it contains more of the fluid, yet notwithftanding this, many eminent men have found much difficulty to believe that the parts of fluids do gravitate *in proprio loco*, as they fpeak, or upon one another. It would be endlefs and tedious to go about to enumerate the feveral prejudices which have been the occafions of this error among the philofophers, whilft they have chosen rather to oppose any truth which came in their way, than forsake the opinion however false they had once resolved to adhere to. But since this error if admitted must of necessity subvert the foundations of all hydrostaticks, and the contrary truth is that by which most of our following conclusions must be explained, I ought not to proceed any further till I have cleared up this matter.

It is evident from daily experience that in any fluid the weight of the whole is equal to the weight of all its parts, of what magnitude or number soever those parts are supposed to be; and if any part be taken from the whole the weight of the whole will be diminished by the weight of that part; if any part be added to the whole the weight of the whole will likewise be increased by the weight of the part which was added, and it seems from hence to be a reasonable conclusion that the weight of the whole is composed of the weights of the several parts, and that the parts do therefore gravitate even in the whole or, according to their way of expressing it, *in proprio loco*. Notwithstanding so obvious and necessary a deduction, the opposers of it have been subtle enough to elude the truth by a distinction which they have invented. They grant that the parts do by an united action cause the gravitation of the whole, but deny that they do singly and separately gravitate *in proprio loco*, so as to compose by that means the whole gravitation. Whenever it can be clearly made out that a number of agents may jointly produce an effect, whilst each singly contributes nothing to that effect, it may then be time to answer this subterfuge.

Notwithstanding so necessary and obvious a deduction, there have been two grand arguments usually produced against the doctrine we assert, which if answered will also render the rest of the

objections

objections invalid, they being for the moſt part reducible to the one or other of thoſe two. It is an experiment obvious to every body that a bucket full of water is lighter in the water than out of it, nor does it weigh more when full in the water, than when empty out of it; therefore they conclude that the water in the bucket becauſe it is within water, its own element, does not gravitate. The other inſtance is taken from divers, who are ſaid to feel no ſenſible preſſure under water, though they often deſcend to very great depths; therefore they again conclude that the parts of water do not gravitate nor conſequently cauſe any preſſure *in proprio loco.* Now granting the matter of fact to be true in both theſe caſes, though it may juſtly be queſtioned as to the buſineſs of diving, yet till they can prove that theſe matters of fact are no other way to be accounted for than by that which they have propoſed, their inferences can by no means be allowed them. I ſhall take an opportunity whilſt we are upon hydroſtaticks, to give the true reaſon why the weight of the water in the bucket ought not to be perceived whilſt the bucket is in water, though it do really at that time retain all its weight which it has when taken out of the water. And as to divers, though we allow that at the depth of thirty two feet under water, they have upon the ſurface of their whole bodies a more than ordinary preſſure of twenty thouſand pounds weight, yet when we conſider the uniformity of that preſſure and its equability, which cauſes no diſlocation of parts, all the external being equally affected with it, and being internally ſupported by the air and other elaſtick fluids, which conſtantly endeavour the more to expand themſelves as they are more compreſt, when we alſo conſider the firm texture of the membranes and other ſolid parts of human bodies and the incredible force they are able to bear, as has been made evident by experiments, we shall not much wonder that divers complain of no sensible pain though they be certainly prest with so great a weight of water, besides the ordinary pressure of the air which our bodies are continually exposed to, which is equal at least to that of water at the depth of thirty two feet, or twenty thousand pounds: so that the whole pressure to which a diver is exposed at thirty two feet under water, is about forty thousand pounds. Since then we have proved that fluids do gravitate *in proprio loco*, and these difficulties do not destroy that assertion, as the proposers of them would make us believe, when they tell us they cannot otherwise be explained, what has been hitherto said might be taken for a satisfactory answer to the objections; yet because this truth is of so great moment I will endeavour to confirm it by two experiments levelled against those two objections, shewing by the first of them that fluids lose nothing of their weight *in proprio loco* (*a*) and by the second that the lower parts of fluids are pressed by the upper, and commu-

(*a*) Fluids lose nothing of their weight *in proprio loco*.

Exp. I. Fig. I, represents a roundish glass bottle, thick enough when empty to sink in water, with a stop-cock cemented to the mouth of it; whereby a quantity of air may be taken out of the bottle by an air-pump, and be hindered from returning by shutting the cock; in order to give passage and room to a quantity of water, equal in bulk to the exhausted air, as soon as the cock shall be opened under water contained in a larger vessel.

The air being exhausted, let the bottle be suspended by a wire to the beam or scale of a ballance, and let it be exactly counterpoised in the air by the weight A in the opposite scale.

Again when the bottle is suspended in a vessel full of water and wholly immersed, let it be counterpoised by a weight B, after A is taken away and reserved.

Then having opened the cock under water, to let it run into the bottle, after the running is over let a third weight C together with B restore the equilibrium of the ballance.

It

communicate that preſſure to bodies expoſed to their contact. I will alſo add other experiments to prove that the preſſure cauſed by the gravitation of fluids, is not only propagated downwards but even upwards and ſideways according to all poſſible directions, (*b*) that a lighter fluid may gravitate upon a heavier, and a heavier upon a lighter. (*c*)

It is plain then that C is the weight of the water let into the bottle, even while it communicates with the water in the veſſel.

Having ſhut up this water in the bottle by turning the cock while under water, take the bottle out of the water, and while it hangs in air at the beam of the ballance, take the weight B from the oppoſite ſcale and reſtore A in its ſtead, then will the weights A and C exactly counterpoiſe the bottle and the water within it.

Which ſhews that this water weighs juſt as much in air as it did before (*in proprio loco* or) in the water of the veſſel, the cock being open.

(*b*) Preſſure is cauſed by the gravitation of a fluid and is propagated every way alike.

Exp. 2. Fig. 2, 3, 4, repreſent tubes bent near their lower ends into various angles. They were firſt filled at their lower orifices with quickſilver, which reſted in the longer legs upon a level with thoſe orifices, and then were dipped into a deep glaſs veſſel filled with water, which while the tubes were deſcending, gradually preſſed the quickſilver from the lower orifices towards the higher, where the water could not enter.

To ſhew that preſſure is propagated even upwards as well as all other ways, dip an open end of a very narrow-bored tube into quickſilver, then ſtopping the upper end with your finger, lift up the tube, and a ſhort column of the quickſilver will hang in the lower end, which column when dipped deeper into water than about fourteen times its own length, will not only be ſuſpended but even preſſed upwards, after your finger is removed from the upper orifice.

(*c*) A lighter fluid may gravitate upon a heavier, and a heavier upon a lighter. The firſt part of this propoſition has juſt been proved by the foregoing experiments, and if any oyl that is lighter than water, be put into the lower end of a bended tube, it will alſo be depreſſed by the water in the veſſel, and forced from the lower orifice towards the higher.

LECTURE

LECTURE II.

That a fluid may ſuſtain a body heavier in ſpecie than itſelf and even raiſe it up, that a fluid may detain a body lighter in ſpecie than itſelf and even depreſs it, that a competent preſſure of a fluid may produce the remarkable phænomena of the Torricellian *tube, pump, ſyringe, ſyphon, poliſhed plates and other effects of the like nature.*

YEſterday it was proved that fluids retain their gravitation *in proprio loco*, and by that gravitation preſſed upon bodies expoſed to their contact, that this preſſure is not only propagated downwards, according to the tendency of heavy bodies, but alſo upwards and ſideways according to all manner of directions.

I deſign this day to make ſome farther experiments concerning the preſſure of fluids, by which I ſhall endeavour to demonſtrate ſome of its more general effects; reſerving the particular and exact eſtimate of all manner of preſſures to my next lecture. In the choice of this day's experiments I have had regard to ſome of the moſt obvious and notable phænomena, that are now a-days explained by the air's gravitation; ſuch as the ſtrong coheſion of poliſhed plates, the ſuſpenſion of quickſilver in weather-glaſſes, the effects of ſyringes, pumps and ſyphons. If we can make it appear, that theſe things not only may depend upon the gravitation of a fluid, but muſt neceſſarily be the conſequence of ſuch a gravitation, and can afterwards prove, that the air itſelf is a gravitating fluid, and can determine the proportion of its ſpecifick weight to that of any other fluid, it will be no difficult matter from theſe things laid together, to evince directly, that the preſſure of the air is the cauſe of thoſe phænomena, and ſeveral others of the like nature. Let us then consult experience, and try whether any thing analogous to what I have been mentioning, be the result of that pressure, which is caused by the gravitation of fluids *(a)*.

These

(a) Here the author made the following experiments and explained them in the sequel of his lecture.

Exp. 1. In Fig. 7. *a b* represents a large round plate of thick brass, whose upper surface being covered with wet leather, is applied so close to the orifice *c d* of the inner vessel, as to hinder the entrance of the water contained in the outer vessel. Now if this plate be held tight against the said orifice, by pulling the wire *e* fixed to the plate, till it be immersed to a sufficient depth of water in the outer vessel, the plate will be supported by the pressure of the water acting upwards, though the hand be taken from the wire. The margin of the inner vessel is made broad enough to hang upon the margin of the outer.

Exp. 2. has already been described, *Page* 9. *Note (b) Sect.* 3.

Exp. 3. Fig. 8, represents a small glass cup having a flat wooden bottom, well planed and fixed to the glass with cement. Upon this bottom place a thick wooden plate, whose under surface is also well planed, and press it against the bottom with your fingers while quicksilver is poured into the cup so as almost to fill it. Then take away your fingers, and the wooden plate will still be detained at the bottom, until you disjoin it by pulling the pin fixed to the middle of it.

Exp. 4. Fig. 9. Any sort of oyl lighter than water, is poured into the shorter leg of a tube bent parallel to the longer, then while the tube descends gradually into a vessel full of water, the oyl will descend into the shorter leg and ascend in the longer.

Exp. 5. Two cylindrical cups containing tinged water, whose surface is about an inch higher in one than in the other, are placed within a larger glass vessel, whose bottom is levelled with bees wax that the cups may stick to it and stand upright. Into the tinged water in the cups put the legs of a glass siphon, having an open pipe inserted into the middle of it, as represented in Fig. 11, and put a wooden cover over the vessel, having a hole in its center to receive this pipe and keep it upright; then through a funnel, inserted into another hole in the cover, pour oyl of turpentine into the larger vessel, till it flows into the cups and rises above the arch of the siphon. The pressure of the oyl upon the tinged water in the cups, will cause the water to pass through the siphon from the higher cup to the lower, till the surfaces of the water be reduced to a level in both the cups.

Exp. 6. The last experiment being easily explicable, is applied to explain the effect of a common siphon, which is nothing else

but

These experiments will need something of an explication; I shall therefore give the reason why the events ought to be such as we have seen, and afterwards infer those consequences which I told you were the principal motives that induced me to make these tryals.

The cause of the firm adherence of the brass plate to the orifice of the glass to which it was applied, is this; that the parts of the water immediately contiguous to the under surface of the plate, were very much pressed by the water which was above the level of the under surface of the plate, and which surrounded the glass to whose orifice the plate was applied, that superior and ambient water communicating a pressure downwards to those parts upon the same level which were directly under it, and these again laterally communicating the pressure they had received, to the other parts upon the same level which were immediately under and contiguous to the plate. For if the parts of the water contiguous to the plate were not as much pressed as the others upon the same level, which were directly under the ambient water, they could not by reaction be able to sustain the pressure of the others, but would give way to the admittance of a further pressure, which must for this very reason be increased till both came to an equipollency. The water immediately contiguous to the under surface of the plate being thus pressed, by communicating its pressure upwards causes this strong union of the plate and the orifice of the glass, notwithstanding the weight of the plate by which it endeavours to descend and to be disunited; which we

but a bended pipe, whose shorter leg being put into any liquor and your mouth being applied to the longer, to suck up some liquor, the rest will continue to ascend in the shorter leg and descend through the longer, till the whole be exhausted from the vessel.

saw

saw it actually did effect when it was not so deeply immersed, and had not so great an altitude of water as was able to cause a sufficient resistance.

Now to know what depth of water is able to cause a sufficient resistance, we ought to know the weight of the plate and whatever else is annexed to it, as in our case the wire and the leather which covers its upper side. We must then immerse it so deep at least that the perpendicular distance of the under surface of the plate and upper surface of the ambient water, be equal to the height of a column of water, whose base is equal to the under surface of plate, and weight equal to the weight of the plate with whatever is annexed to it. For then the pressure of the water against the plate will be a ballance to the weight of the plate against the water, as may (among other ways) be thus made out.

Every part of water which is directly under and contiguous to the plate, receives and communicates a pressure equipollent to that of every equal part of water upon the same level, which is directly under the ambient water, as hath been proved before; now these parts which are under the ambient water receive their pressure from the weight of the column of water, which is perpendicularly incumbent on them, and by their reaction sustain that weight, therefore the pressure which every one of these parts receives and communicates, is equipollent to the weight of its respective superior column; since then the parts of water contiguous to the plate, which are equal to those we have been speaking of, receive and communicate the same degree of pressure, the pressure of these also will be equipollent to the weight of a like column of water; and the sum of their pressures or the force with which they do unitedly sustain the plate, will be equipollent to so many such columns as there are parts of water contiguous to the

the under ſurface of the plate. Now all thoſe columns together are equal to a cylinder or column having that under ſurface of the plate for its baſe, and the perpendicular diſtance of that under ſurface of the plate and upper ſurface of the water for its altitude. If therefore the weight of that cylinder be any thing greater than the weight of the plate, the plate will be ſuſtained; if the plate be not ſo deeply immerſed that the weight of the cylinder be at leaſt equal to the weight of the plate, the plate will be disjoined, by its exceſs of weight, from the orifice of the glaſs; if it be deeper immerſed, the preſſure of the water will by its exceſs of weight, be more than ſufficient to ſuſtain it.

The ſpecifick gravity of braſs to that of water is nearly as eight is to one, I mean that bulk for bulk braſs is about eight times heavier than water; therefore the weight of a cylinder of water will be equal to the weight of a cylinder of braſs, if their baſes be equal and the altitude of the water be eight times as great as the altitude of the braſs. Hence we may conclude that our plate which is of braſs, ought to be immerſed under water at leaſt eight times its thickneſs, to be ſupported by the water. For the ſame reaſon a plate of pure gold, which is the moſt ponderous body we meet with, would require near twenty times its thickneſs. Upon this account it was that Mr. *Boyle* propoſed one of his hydroſtatical paradoxes in theſe words, *That a ſolid body as ponderous as any yet known, though near the top of the water it will ſink by its own weight, yet if it be placed at a greater depth than that of twenty times its thickneſs, it will not ſink, if its deſcent be not aſſiſted by the weight of the incumbent water.*

The other experiment in which the quickſilver which is yet heavier than braſs, was ſuſtained by water, which is a fluid about fourteen times ſpecifically

fically lighter, is explicable the same way. I will therefore only take notice that, whereas I asserted before that an heavier body immersed in a fluid may be either immersed so deep as to be just sustained, or more than just sustained, or so as to be not quite sustained, according to the different weights of the body to be sustained and the column of the fluid t at was before described, the truth of the assertion is in this experiment very manifest. For when we immersed the quicksilver to a just depth, we saw it rest in the pipe without either ascending or descending; if the pipe were thrust deeper, the quicksilver in it was impelled upwards by the force of a more than sufficient pressure of the fluid; if the pipe were raised up above that just depth, the quicksilver by its excess of weight would in part fall out. This experiment will never succeed unless the pipe which contains the quicksilver be of a very narrow bore; for if it be not so, the water will get a passage by the side of the quicksilver, and this changing of places will soon frustrate the event.

The experiment of the wooden plate remaining at the bottom of the vessel filled with quicksilver, would possibly appear strange to some who are unacquainted with the true principles of hydrostaticks, and prejudiced with the false notion of positive, real or absolute levity. For if that imaginary levity were indeed the cause why light bodies ascend in fluids less light than themselves, we should be utterly at a loss in explaining this phænomenon. For what reason can we assign why the wooden plate by the force of its imagined levity should not in this case, as well as in all others, make its way through the body of the quicksilver, which is by so great odds less light than itself. But if this positive levity be rejected as we shall afterwards give further proof that it ought to be (there being no such thing in nature as a positive

tive levity of bodies) and the gravitation of fluids be be admitted, which we have already ſhewn to be conſonant to reaſon and experience, this difficulty will inſtantly vaniſh. For it cannot but be evident that the plate ought always to remain at the bottom, unleſs it be diſplaced and impelled upwards by ſome adventitious force, which in our caſe does not happen, the quickſilver not being able to inſinuate itſelf between the plate and the bottom upon which it reſts, nor conſequently by its preſſure upwards, againſt the under ſurface of the plate, to remove it to the top, which we ſaw it did immediately effect when we permitted it to intervene. It would be yet more difficult from the principle of poſitive levity to give any account why the plate does not only emerge of itſelf, but requires even a conſiderable force to disjoin it from the bottom. It is manifeſt that this cannot otherwiſe be accounted for, but muſt be aſcribed to the gravitation and preſſure of the quickſilver incumbent upon the plate. The force requiſite for their ſeparation being greater or leſſer according as there is a greater or leſſer depth of quickſilver to hinder their ſeparation. It may perhaps ſeem more ſtrange to ſome, as it was a moſt perplexing difficulty to the famous Dr. *More*, that immediately upon their ſeparation there ſhould be not only no further need of any force to raiſe up the plate to the top, but that of itſelf it ſhould very violently emerge; whereas immediately upon their ſeparation, when it firſt begins to aſcend, there is ſtill almoſt as great a weight of incumbent quickſilver to depreſs it, as before the ſeparation there was to detain it. If we ſay it is raiſed upwards by the quickſilver which intervenes upon the ſeparation, and that this intervening quickſilver receives the preſſure, which it communicates upwards to the wooden plate, from the reſt of the quickſilver in the

the vessel, which is not perpendicularly incumbent on the plate, but which surrounds that incumbent quicksilver, being contiguous to the sides of the vessel, and resting upon that annular part of the bottom, which is not covered by the plate; the Doctor will here urge a further difficulty, and will tell us, that the pressure upwards caused by the weight of the ambient quicksilver can by no means equal, much less exceed, the pressure of the incumbent (besides the weight of the plate) that being in quantity much less than this; so that here he supposes he has found out some work for his *Principium Hylarchicum* or his Spirit of Nature.

What has been already said in the explication of the first experiment, may be a sufficient solution of this difficulty; for it was there proved that the force with which the brass plate was suspended, was equal to the weight of a cylinder of the fluid in which it was immersed, whose base was equal to the area of the plate, and altitude equal to the perpendicular distance of the under surface of the plate and upper surface of the fluid, of what breadth soever the ambient fluid were supposed to be: which paradox shall be further explained and experimentally illustrated at our next meeting. In our present case then, the force which impells the wood upwards, is equal to the weight of a cylinder of quicksilver having the under surface of the wood for its base, and the perpendicular depth of that under surface for its altitude; which weight does evidently exceed that of the incumbent quicksilver and plate together, just as much as the weight of the wooden plate itself falls short of the weight of an equal bulk of quicksilver. Quicksilver is fitter to be used in this experiment than water, because it does not adhere to or wet wood as water does, and cannot therefore so easily insinuate itself between the plate and the bottom upon which it rests.

The next experiment that was tryed, was propoſed to evince the ſame thing with this laſt, that fluids may detain and even depreſs a ſpecifically lighter body than themſelves; there was only this difference, that in the former the ſpecifically lighter body was a ſolid, and in the latter a fluid. I ſhall paſs over the explication of the latter, preſuming that by this time the reaſon of it is ſufficiently obvious.

You may remember we immerſed two pipes of different bores in a veſſel of quickſilver, FIG. 10, then pouring on water which could not get into the pipes, their upper orifices being above the water and their lower under the quickſilver, the quickſilver was ſeen to aſcend in the pipes above the level of that in the veſſel to the ſame altitude in both. This effect is an eaſy and plain conſequence of the gravitation and preſſure of water upon the quickſilver. For if we imagine a plane, parallel to the horizon, to paſs by the under orifices of the pipes, it is certain that theſe fluids cannot reſt in any poſition till every equal part of this imaginary plain doth ſuſtain an equal preſſure with the reſt, as has been proved before; now the parts of this plane which are directly under the orifices of the pipes, cannot ſuſtain an equal preſſure with the reſt, unleſs the quickſilver be ſuppoſed to aſcend ſo much in the pipes as to ballance that exceſs of water which preſſes upon the other parts; and although it might ſeem at firſt ſight that a leſſer height of quickſilver would be ſufficient for this purpoſe in the larger pipe than in the ſmaller, a leſſer height of quickſilver in that being equiponderant to a greater in this, yet in reality it is quite otherwiſe, and the heights in both ought to be equal. For though there is a greater weight of quickſilver in the larger pipe, yet we muſt at the ſame time conſider, that this greater weight has a proportionably greater part of the imaginary plane to communicate a preſſure to.

We

We are now come to that ingenious experiment first proposed by M. *Paschal* to manifest that the effect of a syphon may depend upon the gravitation of a fluid. We saw it with our eyes and can therefore no longer doubt of it, that the weight and pressure of the oyl caused the tinged water to take its course from the higher vessel into the lower through the bended pipe. But that you may have the evidence of reason as well as of sense, I will give you that excellent author's own explication of this matter, after I have told you that he instead of our tinged water made use of quicksilver, and instead of our oyl of turpentine he tried his experiment with water. We are to observe, says he, that the oyl gravitating upon the tinged water contained in each vessel, and not all upon that which is contained in the legs of the syphon, it comes to pass that the water in the vessel is compelled by the weight of the oyl, to ascend in each leg to the top of the syphon, and there a sort of conflict must happen betwixt the two ascending columns, each pressing against the other, and that will necessarily prevail which has the greater force. Now which has the greater force may easily be determined, for since the oyl has the greater altitude above the lower vessel by an inch, it must more powerfully elevate the water of the longer leg, than that of the shorter by the force which an inch of altitude gives to it. Whence it seems at first sight to follow, that the water ought to run from the longer to the shorter leg. But we must at the same time consider that the weight of the water in each leg resists the effort of the oyl to raise it up, but both do not resist equally, for the water in the longer leg has a greater altitude by an inch, and so makes a greater resistance by the force which an inch can give it. In the longer leg the water is more powerfully elevated by the force of an inch of oyl, and its aſcent is more powerfully hindered by the force of an inch of water; now an inch of water is more ponderous than an inch of oyl; therefore the water of the ſhorter leg abſolutely ſpeaking is elevated with a greater force, and conſequently it ought to aſcend, and continue to aſcend till the water in both veſſels comes to a level. *(b)*

From hence it appears that the reaſon of the higher veſſel's emptying itſelf into the lower, is that the water is heavier *in ſpecie* than the oyl. The contrary would happen if the ſyphon and the veſſels into which it is immerſed were filled with oyl, and all were immerſed in a veſſel of water; for then it would come to paſs that the oyl of the lower veſſel would aſcend, and paſſing by the top of the ſyphon would deſcend into the upper veſſel, upon the account which I have juſt now mentioned. For the water preſſing continually upon the oyl in the lower veſſel with a greater force, ſince it has by an inch a greater altitude, and the oyl of the lower leg gravitating and reſiſting more by an inch of altitude, it muſt needs come to paſs, ſince an inch of oyl weighs leſs than an inch of water, that the oyl of the lower leg ought to be more forcibly elevated than the oyl of the higher, and therefore the courſe muſt be from the lower veſſel into the higher. Upon the ſame account if the ſyphon were filled with a liquor of the ſame gravity with water, no flux would enſue but all things would remain at reſt.

From

(b) The author has here given us M. *Paſcal's* explication of his own experiment, but in common diſcourſe I remember he explained it as follows.

In FIG. 11. Suppoſing the legs of the ſyphon to be really equal, or which comes to the ſame, ſuppoſing an horizontal plane to paſs through the legs and tinged water in both cups, the parts of this plane within the legs will be equally preſſed by equal columns of tinged

From these experiments I might now in a few words and very easily deduce those inferences, for whose sake they were chiefly proposed, were they not already too obvious to be insisted on. I will therefore only mention them. Supposing then the air to be an heavy fluid, and that the surface of the earth is as much pressed upon by this fluid as if it were every where covered with quicksilver to the height of about 29 inches and an half, or with water to the height of about 34 feet, as we shall hereafter prove; if in the first experiment, FIG. 7, we substitute air instead of water, and instead of the plate of brass applied to the orifice of the glass vessel, two polished planes applied together so closely as to exclude the air from getting between, the lower plane must of necessity be pressed against the upper and kept suspended. So in the last but one of these experiments, in which the quicksilver was raised above its level in the two pipes, FIG. 10, if we substitute the body of a pump for either of those pipes, water in the well for quicksilver in the vessel, air incumbent upon the water in the well for water incumbent upon the quicksilver in the vessel, and observe that as the water in our experiment was hindered from entering into the

tinged water within the legs; but other equal parts of this plane on the outsides of each leg, will be unequally pressed by their incumbent columns though of equal altitudes; because the columns of the higher cup consist of more water and less oyl than those of the lower. The heavier columns will therefore press up the higher water into the leg in its cup, with greater force than the lighter columns can press up the lower water into the leg in its cup, and the excess of the former pressure above the latter will drive the water along the syphon from the higher cup to the lower.

This excess of pressure, which causes the flux, is therefore as the difference in weight of two columns composed of water and oyl, whose common base is equal to the orifice of the syphon, and common height is the difference of the heights of the water in the cups, pipe by the ſides of the pipe, ſo the air is excluded from the cavity of a pump by the ſides of the pump and the ſucker; it will be clear to any one, that water may aſcend, by the preſſure of the external air upon the ſurface of the water in the well, to the height of about 34 feet. The ſyringe is a little pump as the pump is a greater ſyringe, what has been ſaid of the pump may therefore be applied to the ſyringe. The caſe is very nearly the ſame with the Torricellian tube, barometer, or weatherglaſs, in which the quickſilver uſually aſcends to 29 inches and an half; that height of quickſilver being equiponderant to 34 feet of water. This alſo may be taken notice of, that as in our experiment the quickſilver aſcended to the ſame height in both pipes, though of unequal diameters, ſo in pumps and barometers the altitude of the liquors is not altered by any difference of their bores. The laſt experiment is ſo particularly fitted to the common ſyphon that any one may make the application.

LECTURE III.

That fluids preſs according to their perpendicular altitudes, whatever be their quantities or however the containing veſſels be figured; the exact eſtimate of all manner of preſſures; and the invention of the center of preſſure upon any propoſed plane reduced to the problem of finding the center of percuſſion.

We are now to determine the quantity of that preſſure which any ſurface ſuſtains that is expoſed to the gravitation of a fluid: this muſt be done gradually, beginning with thoſe caſes which are moſt ſimple and eaſy, and afterwards proceeding to thoſe which are more complex and difficult. Let a veſſel *abcd* Fig. 12. be propoſed containing any fluid, ſuppoſe water,

water, and let *a b* be the upper surface of the water, and *c d* the bottom of the vessel. The pressure upon any part of that bottom, suppose *g h*, will be equivalent to the weight of a column of water *g h i k*, having the part *g h* for its base and *g i* or *h k*, the depth under water, for its altitude. This seems to be self-evident and may best be proved by the absurdity of any contrary supposition; for if it be said that *g h* sustains a greater weight than that of the column *g h i k*, the excess must come from the adjoining columns *a c g i* and *k h d b*; now for the like reason it ought to be said that *c g* sustains a greater weight than that of the column *a c g i*, and *h d* a greater than that of the column *k h d b*; but if this were true, then would all the parts *c g*, *g h* and *h d* together, of that whole plane *c d*, sustain a greater weight than that of the columns together, or of the whole water which is above it, namely *a c d b*, which is absurd. The like absurdity will follow if it be said that *g h* sustains a lesser pressure than the weight of the column *g h i k*; the weight of that column then which is perpendicularly incumbent upon it, is exactly equivalent to the pressure which it sustains.

This is the quantity of pressure upon the plane *g h* in the case that has already been described. If the figure of the vessel be any way altered, the pressure will still be the same if the perpendicular distance of the plane *g h* from the upper surface of the water contained in the vessel, of whatever figure it be, remain unaltered. Thus in Fig. 13, 14, if *l n g h o m* be a vessel of any irregular figure, and *l m* be the upper surface of the water, and the perpendicular distance of *g h* below *l m* namely *g i* or *h k* be the same as before, the pressure of the vessel'd water *l n g h o m* upon the bottom *g h* will be equal to the same weight of the column *g h i k* as before, though the vesselled water *l g h m* be much less than that column, as in

FIG. 13, or much greater, as in FIG. 14. The pressure is not to be estimated by the quantity of water but by its altitude. For if the quantity of water *lghm* in FIG. 13, be a thousand times less than *ighk*, as we may easily suppose it to be, and the quantity of water *lghm* in FIG. 14, be a thousand times greater than the same *ighk*, then the quantity of this latter will be a million of times greater than that of the former, nevertheless both will equally press upon their bottoms *gh* with a force equivalent to the weight of the perpendicular column *ghik*; which may deservedly be accounted a paradox in hydrostaticks, but may thus, among other ways, be rendered intelligible.

Let us conceive each of those vessels placed in a larger *abcd*; the pressure upon *gh* will be the same whether we suppose the water *lnghom* to be contained in its proper vessel *lnghom*, or, imagining that vessel to be away, we suppose its place to be supplied by the ambient water *acgnl* and *hombd*; for any parcel of water may be conceived to be kept in by the rest of the water, which every way surrounds it as in a vessel, supposing all things at rest. Now in this latter case where we suppose the ambient water *acgnl* and *hombd* to be a vessel to the water *lnghom*, the pressure upon *gh* is equivalent to the weight of the column *ghik*, as has been already made out; therefore in the former case, where the water *lnghom* was contained in its proper vessel, the pressure upon *gh* will be also equivalent to the weight of the same column *ghik*. By the same way of reasoning we may conclude that the water contained in any other more irregular vessel, as *lnghom* in FIG. 15, presses upon the bottom with a force equivalent to the weight of the column of water *ghik*, having the said bottom for its basis, and *gi* or *hk*, the perpendicular distance of the planes *gh* and *lm*, for its altitude.

If the plane *g h* be oblique to the horizon, as in FIG. 16, the preſſure upon *g h* from the water of the veſſel *l n g h o m*, or from that of the veſſel *e g h f*, or from that of the larger veſſel *a c d b*, will ſtill be the ſame, if the upper ſurfaces *l m*, *e f* and *a b* be in the ſame plane or at the ſame altitude above *g h*. The altitude is every where the meaſure of preſſure whatever be the quantity of the fluid, or however the containing veſſel be figured *(a)*.

I ſhould

(*a*) The author gave an experimental proof of theſe concluſions as follows. FIG. 17 repreſents a large mouthed ſyphon inverted and partly filled with water, whoſe ſurface always reſts at the ſame level in both legs; conſequently ſuppoſing the ſyphon to be cut at the bottom of the flexure by an imaginary plane, the water in both legs, however different in ſhape and bulk, preſſes with equal and oppoſite forces againſt that plane; otherwiſe the level of the water would ſoon be altered and deſtroyed.

It has been ſhewn above, that the braſs plate *a b*, in FIG. 7, being about eight times heavier than an equal bulk of water, will by the preſſure of the water underneath it, be ſupported at the mouth of the inner veſſel, if the plate be immerſed under water above 8 times its thickneſs. This being done, let other water be gradually poured through a funnel into the inner veſſel, till its weight and preſſure ſhall cauſe the plate to deſcend, and at that inſtant let the altitude of the inner water above the plate be obſerved, or rather the difference between the altitudes of the inner and outer water, which difference by theory ſhould be about eight times the thickneſs of the plate.

This being obſerved, take up the inner veſſel, and into its mouth ſqueeze a large cork *c d* with a wet leather bound about it, ſo far as to leave a thin ſpace between it and the plate applied to the orifice of the mouth, as repreſented in FIG. 18. A glaſs tube *g h* was firſt ſqueezed through a hole in the middle of this cork and cemented, to it, and then the wire *e f* fixed in the plate was put through the tube. Then while the plate is held by the end of the wire againſt the orifice of the inner veſſel, the whole is let down into the water in the outer veſſel, by whoſe preſſure upwards the plate is again ſupported, as before. But by degrees the ambient water will inſinuate itſelf between the orifice and the plate, into the thin ſpace above it, and this being filled, it will quickly riſe into the tube, where as ſoon as it arrives at the altitude before obſerved in the whole inner veſſel, its weight and preſſure will immediately cauſe

the

I ſhould now proceed to eſtimate the preſſure upon planes which are either perpendicular or oblique to the horizon, but becauſe the ſeveral indefinitely ſmall parts, of which ſuch planes are compoſed, are acted upon with different forces, accordingly as the particles of water, by which they are immediately touched, happen to be at different depths; and ſince the total preſſure is made up of all theſe different forces taken together, we ought before we go any further to conſider, what will be the preſſure which each of theſe indefinitely ſmall parts ſuſtains. Firſt then we are to conſider that every ſmall particle of water, which is at reſt, is preſſed upon equally on all ſides by the other particles which ſurround it, otherwiſe it would yield to the ſtronger force till it were equally preſſed every where; and as it is equally preſſed on all ſides, ſo does it every way by reaction equally preſs whatever is contiguous to it, according to all poſſible contrary directions; for ſhould it preſs leſs than it were preſſed, it muſt neceſſarily yield to the force which is ſuppoſed greater than its own; and ſhould it preſs more than it were preſſed, its force would neceſſarily remove its weaker antagoniſt. Therefore ſince all things are ſuppoſed to be at reſt, we cannot any ways imagine this inequality of preſſure to take place. Now it has been proved before, that the preſſure from above is equivalent to the weight of the incumbent column of water, therefore the preſſure from any other part, or according to any other direction, is alſo equal to the weight of the ſame incumbent column; and ſince action and reaction are equal, the particle itſelf muſt preſs according to all manner of directions with the ſame force, which is equivalent to the weight of the incumbent column.

the plate to deſcend: which ſhews that the preſſure of this ſmaller quantity of water is equal to that of the larger upon the ſame baſe.

It is evident then that as fluids preſs according to all poſſible directions, ſo are the preſſures equal according to all directions, if the points of contact in which the preſſures are made be at equal depths. This being allowed we may proceed to what remains.

Suppoſing then that *acdb* in FIG. 19, is a cubical veſſel in which the water reaches to the top, ſo that its upper ſurface be repreſented by *ab*, let it be required to determine the preſſure which one of its ſides *ac* ſuſtains from the included water. This ſide *ac* though repreſented here by a line, to avoid confuſion in the ſcheme, is ſuppoſed to be a ſquare. The meaſure of the preſſure upon every phyſical point of that ſquare, or as it is here repreſented, of that line *ac*, is the altitude of the water above that point; thus the preſſure upon *l* is meaſured by *al*, the preſſure upon *m* by *am*, the preſſure upon *n* by *an*, and the preſſure upon *c* by *ac*, and the ſame may be ſaid for any other points of the line *ac*; therefore the preſſure upon the whole line, or upon all the points of it, will be meaſured by the ſum of ſo many of thoſe altitudes *al*, *am*, *an*, *ac*, as there are points in the line *ac*. Now that ſum may be eſtimated by drawing the perpendicular *lo* equal to *la* from the point *l*, the perpendicular *mp* equal to *ma* from the point *m*, the perpendicular *nq* equal to *na* from the point *n*, and the perpendicular *cd* equal to *ca* from the point *c*. Now it is evident that the ſum of *al*, *am*, *an*, *ac* muſt be equal to the ſum of *lo*, *mp*, *nq*, *cd*, and if from every intermediate point between *a* and *l*, *l* and *m*, *m* and *n*, *n* and *c*, perpendiculars be conceived to be drawn after the ſame method, the ſum of all thoſe perpendiculars will be the meaſure of the total preſſure upon the line *ac*. But the ſum of all thoſe perpendiculars is equal to the area of the triangle *acd*, therefore the area of the triangle *acd* is the meaſure of the preſſure upon the line *ac*.

Now as the line *a c* represents a square, so will the triangle *a c d* represent a prism, having the said triangle for its base, and the side of the square for its altitude. The weight of that prism of water is therefore equivalent to the pressure made against the square, or side of the cube. That prism is equal to half the whole cube, as we learn from Euclid's elements, therefore the pressure against the square is equivalent to half the weight of the whole water contained in the vessel. There are four such sides of a cube besides the top and bottom, and each of those four sides for the same reason sustains the same pressure, therefore all together sustain four times half the weight, that is twice the whole weight of the water. And the bottom, by what has been proved above, does itself sustain a pressure equal to the whole weight of the water; therefore the bottom and sides together of a cubical vessel filled with water, sustain a pressure from the water equal to thrice the weight of it.

I have endeavoured to make the thing as easy as I believe the nature of it will permit, however since that part of this deduction where I told you the triangle *a c d* did at the same time represent the prism when the line *a c* represented the square, might be perhaps a little obscure, I will endeavour to clear up this matter something further. Let then *a c f e* in Fig. 20 represent the square side of the vessel, and *c d g f* represent the square bottom of the same. It was proved before that the pressure exercised upon the line *a c* was measured by the triangle *a c d*; by the same way of reasoning it may be proved that the pressure upon the line *e f* is measured by the triangle *e f g*, and the pressure upon any other line *h i*, which is parallel to these two and situated between them, is measured by its respective triangle *h i k*. If we imagine the square *a c f e* to be made up of an infinite number of such intermediate lines as *h i*, the pressure upon the whole square will be made up of the same infinite number of such equal triangles as *b i k*; now the sum of all those triangles make up the prism *a e g d c f*, and this prism is half the whole cube, as in the former scheme the triangle *a c d* is half the square *a c d b*. If the plane *a c f e* instead of being a square were a rectangled parallelogram, having its side *a e* either longer or shorter than *a c*, it would follow from the principles, that the pressure to which it is exposed, would be equivalent to the weight of a like prism of water having the triangle *acd* for its base, and the side *a e* for its altitude.

I have been hitherto speaking of planes which are either parallel or perpendicular to the horizon; it will be no difficult matter to apply what has been said of perpendicular planes to those which are oblique. Let *a c* in FIG. 21 represent any such oblique plane, and let the upper surface of the water be *a b*. The measure of the pressure upon the point *l* is *l s* the altitude of the water above that point, so *t m* is the measure of the pressure upon *m*, *v n* the measure of the pressure upon *n*, and *x c* the measure of the pressure upon *c*. Erect the perpendiculars *l o*, *m p*, *n q*, *c r* equal respectively to *l s*, *m t*, *n v*, *c x*, and imagine the like construction to be made for all the other points of the line *a c*, and the sum of all those perpendiculars, that is the triangle *a c r*, will be the measure of the pressure upon the whole line *a c*. If this line *a c* be supposed to represent a parallelogram as before, then the triangle *a c r* will as before become a prism, and the weight of that prism of water, which we are taught by Euclid how to measure, will be the pressure sustained by the parallelogram.

I have hitherto supposed that the line *c a* or the parallelogram represented by it, coincides with the surface of the water at *a*; if that does not happen, but the highest part of the line or parallelogram is at some

ſome diſtance from the ſurface, a computation of the preſſure will ſtill be eaſy enough. Suppoſe *m c* in Fig. 21, were the line or parallelogram propoſed; the preſſure upon the line *m c* will be meaſured by the trapezium or four ſided figure *m c r p*, and the preſſure upon the parallelogram repreſented by that line, will be a priſm having that trapezium for its baſe, and the other ſide of the parallelogram, which is ſuppoſed parallel to the ſurface of the water, for its altitude.

From what has been ſaid of theſe few particular inſtances we may now underſtand, that the preſſure upon any plane of whatever figure and ſituation, is equivalent to the weight of a ſolid of water, which is formed by erecting perpendiculars upon every point of the plane propoſed, equal to the reſpective diſtances of thoſe points from the upper ſurface of the water. For the perpendiculars being the meaſure of the preſſure upon the points from which they are erected, the ſum of theſe perpendiculars, or the ſolid formed by them, will be equal to the ſum of the preſſures upon the points, or the total preſſure upon the whole plane.

Or we may thus expreſs the ſame thing after another way, and ſo take in all curved ſurfaces as well as planes; that the preſſure upon any ſurface is equal to the ſum of all the products which are made by multiplying every indefinitely ſmall part of the ſurface into its diſtance from the top of the water. For the preſſure upon each of thoſe parts is equal to a column of water having the part for its baſe, and the diſtance from the top of the water for its altitude; and every one knows who has the leaſt skill in geometry, that thoſe columns are meaſured by multiplying their baſes by their altitudes; therefore the ſum of the products of all thoſe baſes or little parts by their altitudes, or reſpective diſtances from the

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top of the water, will be equal to all the columns upon every little part, and therefore to a body of water whoſe weight will be equivalent to the total preſſure upon the whole ſurface.

Now to find the ſum of all theſe products, or a body of water equal to that ſum, is a very difficult problem in moſt caſes. *Stevinus* in his hydroſtaticks has attempted it only in a few inſtances, and thoſe of plane ſurfaces, and among plane ſurfaces he meddles only with ſuch which he calls regular, nevertheleſs he has gone the fartheſt in this matter of any writer I have met with. To ſupply then this defect I will here lay down another rule, which is not only univerſal, but alſo as eaſy and expeditious as can be deſired.

It is this; the preſſure upon any ſurface whatever, however it be ſituated, is equal to the weight of a body of water whoſe magnitude is found by multiplying the ſurface propoſed into the depth of its center of gravity under water. So the preſſure upon any number of ſurfaces of different bodies, however differently ſituated, is equal to the weight of a body of water whoſe magnitude is found by multiplying the ſum of all thoſe ſurfaces into the depth of their common center of gravity under water.

The demonſtration of this rule may not perhaps be fully underſtood by thoſe who are unacquainted with ſtaticks and the nature of the center of gravity, however I will here produce it, that thoſe who can, may underſtand it, and that others, taking now for true what I ſhall aſſume as demonſtrated by the writers of mechanicks, may afterwards be fully ſatisfied of it, when they come to underſtand the theorem it is grounded upon; which is, that if every indefinitely ſmall part of any ſurface, or number of ſurfaces, be multiplied reſpectively into its perpendicular diſtance from any propoſed plane, the ſum of thoſe products will be equal to the product of the whole ſurface or number of ſurfaces multiplied into the perpendicular diſtance of the center of gravity of the ſingle ſurface, or of the common center of gravity of the whole number of ſurfaces, from the ſame plane. (*b*)

Now taking the upper ſurface of water for that plane to which we refer the indefinitely ſmall parts

(*b*) In FIG. 22, let any number of quantities *a*, *b*, *c*, *d*, repreſent as many weights, hanging at their centers of gravity *a*, *b*, *c*, *d*, by the lines *ao*, *bo*, *co*, *do*, fixed to any horizontal plane *o*, *o*, *o*, *o*; and let *z* be the common center of gravity of all the weights, and *zo* its perpendicular diſtance from that plane; I ſay that $a \times ao + b \times bo + c \times co + d \times do = \overline{a+b+c+d} \times zo$.

For let the common center of gravity of the weights *a*, *b* be the point *x*, and to the line *xo* drawn parallel to the reſt, let *am* and *bn* be perpendiculars. Then by the ſimilar triangles *mxa*, *nxb*, we have $mx : nx :: (xa : xb ::)\ b : a$ by the known property of a center of gravity. Hence $a \times mx = b \times nx$, or $a \times \overline{mo - xo} = b \times \overline{xo - no}$, or, $a \times mo - a \times xo = b \times xo - b \times no$, whence $a \times mo + b \times no = \overline{a+b} \times xo$; which was to be proved in the ſimpleſt caſe of the propoſition.

Now let a weight $x = a + b$ be ſuſpended by a line *xo* in the common center of gravity of *a* and *b*, and likewiſe a weight $y = x + c$ in the common center of gravity of *x* and *c*, and alſo a weight $z = y + d$ in the common center of gravity of *y* and *d*. Then is *z* the common center of gravity of all the weight *a*, *b*, *c*, *d*, firſt propoſed.

Conſequently by what has been proved in the firſt caſe, we have $a \times ao + b \times bo = x \times xo$, and likewiſe $x \times xo + c \times co = y \times yo$, and likewiſe $y \times yo + d \times do = z \times zo$; conſequently $a \times ao + b \times bo + c \times co = y \times yo$, and likewiſe $a \times ao + b \times bo + c \times co + d \times do = (z \times zo =)\ \overline{a+b+c+d} \times zo$, which was to be proved.

Hence if a ſurface or number of ſurfaces of any kind be conſidered as equally ponderous in every equal part, and as divided into indefinitely ſmall parts, ſuſpended by lines, drawn from their centers, perpendicular to any horizontal plane; it is manifeſt that, if every part be multiplied reſpectively into its perpendicular line, the ſum of the products will be equal to the product of the whole ſurface multiplied into the perpendicular diſtance of its center of gravity from the ſaid plane: and that this equality of the products will ſubſiſt even if the ſaid lines be perpendicular to any plane, though not parallel to the horizon.

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of the ſurface which is expoſed to the preſſure we are concerned with, ſince it has been already ſhewn, that the preſſure upon the whole is equivalent to the weight of a body of water which is equal in magnitude to the ſum of all the products, made by multiplying every little part by its diſtance from the upper plane of the water; and this ſum of products, by the ſtatical theorem I have been mentioning, is exactly equal to the product of the whole ſurface or number of ſurfaces multiplied into the diſtance of the center of gravity from the upper plane of the water; it will follow, that the ſame product is the meaſure of a magnitude of water whoſe weight is equivalent to the preſſure required. The ſame rule may be demonſtrated by ſeveral other methods, but I have pitched upon this as the fitteſt for my purpoſe.

Another thing which *Stevinus* propoſes to himſelf, is to determine the center of preſſure upon any plane. Before we can diſcourſe any farther about this we muſt declare what is meant by that center. It is then the point to which if the total preſſure were applied, its effect upon the plane would be the ſame as when it was diſtributed unequally over the whole after the manner before deſcribed; or we may ſay it is that point in which the whole preſſure may be conceived to be united; or it is that point to which if a force were applied, equal to the total preſſure but with a contrary direction, it would exactly ballance or reſtrain the effect of the preſſure. Thus if *a b c d* in FIG. 23, be a veſſel of water and the ſide *a c* be preſſed upon with a force equivalent to twenty pounds of water, this force we have ſeen is unequally diſtributed over *a c*; for the parts near *a* being at a leſſer depth, are leſs preſſed upon than the parts near *c* which are at a greater depth, and therefore the efforts of all the particular preſſures are united in ſome point *z*, which is nearer to *c* than to *a*, and that point *z* is what may be called the center of preſſure: if to that point a force equivalent to twenty pound weight be applied, it will affect the plane *a c* in the ſame manner as before by the preſſure of the water diſtributed unequally over the whole. And if to the ſame point we apply the ſame force with a contrary direction to that of the preſſure of the water, the force and preſſure will ballance each other, and by contrary endeavours deſtroy each others effects. Suppoſe at *z* a cord *z p w* were fixed, which paſſing over the pulley *p*, has a weight *w* of twenty pounds annexed to it, and that the part of the cord *z p* were perpendicular to *a c*; the effort of the weight *w* is equal, and its direction contrary to that of the preſſure of the water. Now if *z* be the center of preſſure theſe two powers will be in equilibrio, and mutually defeat each others endeavours.

It may be worth while to be acquainted with a rule for finding that center in all caſes. We cannot have much help from *Stevinus* in this buſineſs; he undertakes only a few particulars and thoſe which are the eaſieſt, ſuppoſing that his reader will apply the like method to other circumſtances; but they who ſhall endeavour to make ſuch an application, will in moſt caſes find it more difficult than they might poſſibly expect. I have for that reaſon deviſed this general rule which follows.

If any plane which happens to be propoſed be produced till it interſects the upper ſurface of the water produced, if need be, and the line which is the common ſection of the two planes, be made an axis of ſuſpenſion; the center of oſcillation or percuſſion of the plane, as it is ſuppoſed to revolve about that axis, will be the center of preſſure required. Thus if *a c* in Fig. 24, repreſents the plane propoſed, let it be produced till it cuts the plane *g h* in *d*, now if *d* be made the axis of ſuſpenſion of the plane *ac*, the center of percuſſion of the plane *ac* revolving about *d*, will be alſo the center of preſſure upon the ſame plane.

For if the percuſſive forces of every point of *ac* be as the preſſures exerciſed upon thoſe points, then the center of percuſſion muſt needs be the ſame with the center of preſſure; and that the force of percuſſion is every where as the preſſure of the water may thus be proved. The percuſſive force of any point, ſuppoſe *b*, is as the velocity of that point, and the velocity is as the diſtance *bd* of the point from the axis of motion; ſo the percuſſive force of *a* is as *ad*, of *c* as *cd*; ſince then the percuſſive forces of *a*, *b*, *c* are as the lines *da*, *db*, *dc*, and theſe lines are as the lines *ea*, *fb*, *gc*, perpendicular to the ſurface of the water, and theſe laſt lines are as the preſſures upon *a*, *b* and *c*, it follows that the percuſſive forces, taking the interſection *d* for the axis of ſuſpenſion or motion, are reſpectively as the preſſures upon the ſame points; therefore the center of percuſſion or oſcillation is the ſame with the center of preſſure.

The geometers of the laſt age have proſecuted the problem of finding the center of oſcillation very diligently, being excited thereto chiefly by the noble invention of pendulum clocks; the rules they have laid down for that purpoſe are eaſy enough, and the applications they have actually made of thoſe rules are not a few. Having therefore ſhewn how the center of oſcillation may be made uſe of for determining the center of preſſure, I preſume I have by this time ſufficiently cleared up what I propoſed; but for further illuſtration I will add a couple of examples.

Let it be required to find the preſſure which a diver ſuſtains when the center of gravity of the ſurface of his body is 32 feet under water. The ſurface of a middle ſized human body is about 10 ſquare feet. Multiply then 32, the depth of the center under water, by 10 the surface of the body, and the product, or 32 times 10 solid feet, will be a magnitude of water whose weight is equivalent to the pressure which the diver sustains, by the rule before laid down. A cubick foot of water has been found by experiment to weigh 1000 averdupois ounces, therefore 32 times 10 feet, or 16 times 20 feet of water, will weigh 16 times 20000 averdupois ounces or 20000 averdupois pounds. This therefore is the pressure of the water to which a diver at 32 feet depth is exposed.

Again in FIG. 25, let the right angled parallelogram *abcd* be a wall, dam, or pen of timber perpendicular to the horizon, made to keep in a pond of water, whose upper surface reaches to *ab*; let *ab* be 20 feet, and *ac* 12. Let *k* be the center of gravity of the plane; the depth of that center *k* will be equal to half *gh* or half *ac*, that is 6 feet. The area of the plane is found by multiplying *ac* by *ab* or 12 by 20, it is therefore 240 square feet; multiply, according to the rule, the area 240 by *gk* which is 6, and the product will be 1440 cubick feet of water, which weighs so many thousand ounces, that is 90000 pounds; and that is the pressure which the dam *abcd* sustains.

To find the center of that pressure we must make the line *ab*, which is the common section of the dam and the upper surface of the water, the axis of suspension of the plane *abcd*; now it appears by the discovery of *Huygens*, *Wallis* and other geometers that *z*, the center of oscillation of this plane so suspended, will be in the line *gh* which bisects this plane and is parallel to *ac* or *bd*; and that the line *gz* will be two thirds of *gh*, that is 8 feet; and the same point *z* so determined is, as was proved before, the center of pressure required.

LECTURE

LECTURE IV.

Of the ſinking and floating of bodies immerſed in fluids, their relative gravities and levities, their ſituations and poſitions: the phænomena of glaſs bubbles accounted for.

We are now to make our enquiries concerning the ſinking and floating of bodies immerſed in fluids; their relative gravities, their levities, their ſituations and poſitions. This is the ſubject of *Archimedes*'s two books de *Inſidentibus Humido*, of which the Latin tranſlation is yet extant, though the original in Greek be loſt. I will therefore give you the ſubſtance of his doctrine with ſome additions. But becauſe the laſt propoſitions of his firſt book demonſtrate to us the poſtures in which a floating portion of a ſphere will compoſe itſelf, and the whole ſecond book except the firſt propoſition of it, is entirely taken up in determining the like for the parabolick conoeid (which is a ſolid formed by the revolution of a parabola about its axis) I will content myſelf to make out and demonſtrate to you the foundation upon which thoſe theorems of his are grounded, paſſing by the application of it to particular ſolids, as being a matter that belongs more properly to geometry than to hydroſtaticks.

We have already ſeen that fluids preſs upon bodies to which they are contiguous every way, and on all ſides, but the preſſure upon each part is not the ſame; the altitude of the fluid is every where the meaſure of its force; and the ſeveral parts of the ſame body being at different depths, muſt needs be differently affected. We ought therefore to conſider which of all theſe impreſſions will prevail. Now it is evident that the lateral preſſures do all ballance each other, being equal, as ariſing from equal altitudes of the fluid, and oppoſite in their directions; ſo that from theſe the body is no ways determined to any motion. But thoſe parts of the fluid which are contiguous to the under ſurface have a greater altitude, and therefore a greater force than the others which are contiguous to the upper; therefore the body muſt of neceſſity be more violently elevated by the former than depreſſed by the latter, and would therefore aſcend by the exceſs of force were it devoid of gravity. Now it is eaſy to underſtand, that this exceſs of force is equivalent to the weight of ſo much of the fluid as is equal in magnitude to the bulk of the body, being the difference in weight of two columns of the fluid, whereof one reaches to the upper, the other to the under ſurface of the body.—

It has been objected by ſome againſt the gravitation of fluids *in proprio loco*, that bodies immerſed would of conſequence be violently detruded to the bottom, whereas we ſee in fact that the contrary is true; ſome which are ſpecifically lighter than the fluid being even buoyed up by it. Thoſe who make this objection ought at the ſame time to have conſidered, that as the upper parts of the body are depreſſed, ſo are the under more powerfully elevated, and therefore that ſetting aſide the conſideration of the body's own weight, it ought always to aſcend; but taking in that conſideration let us now ſee what the event will be; and this may be eaſily determined. For ſince all bodies endeavour to deſcend by the force of their own weight, and to aſcend by the weight of an equal bulk of the fluid in which they are immerſed, it muſt of neceſſity come to paſs, that if the weight of the body be greater than the weight of an equal bulk of the fluid, it will deſcend with a force that is equal to the difference of thoſe two weights; if the weight of the body be leſs than the weight of an equal

bulk

bulk of the fluid, the weight of the fluid muſt prevail, and carry it upwards with a force that is equal alſo to the difference of the two weights; and this is the cauſe of the deſcent or aſcent of bodies, as they are ſpecifically heavier or lighter than the fluid in which they are immerſed.

The ſame thing is often made out by another method which is ſomewhat different from this which we have made uſe of. We are to ſuppoſe, in what place ſoever within the fluid the body is conceived to be, that there paſſes by it an imaginary plane touching its under ſurface and parallel to the horizon. Now it has been made manifeſt that this fluid cannot compoſe itſelf and be at reſt till every equal part of this plane ſuſtains an equal preſſure; if then the body be of an equal gravity with ſo much of the fluid as is equal to it in bulk, and whoſe place it takes up, the part of the imaginary plane, which is directly under the body will be equally affected by the preſſure of the fluid and body together, which are ſuperior to it, with the other equal parts of the ſame plane, which are preſſed upon by the fluid alone; therefore there can be no reaſon aſſigned why the body ſhould give way either by aſcending or deſcending, but it ought to maintain the place given it. If the body be heavier than ſo much of the fluid as is equal to it in bulk, this part of the imaginary plane which is directly under it, will be preſſed with a greater weight than the other equal parts of the ſame plane, by the exceſs of the body's weight, above the weight of an equal bulk of the fluid; that part muſt therefore yield, and the body muſt deſcend with a force equal to that exceſs. By a a like way of reaſoning we may collect, that if the body be lighter than an equal bulk of the fluid, it will be buoyed up by that part of the plane which is under it, with a force equivalent to the difference in weight of that equal bulk of the fluid and the body;

dy; and upon the same account it must continue to ascend till every part of this imaginary plain, which is conceived to follow it, always touching its under surface, be equally pressed upon; that is, till the body be so far extant above the surface of the fluid, that the weight of a part of the fluid equal in bulk to the part of the body immersed, be equal in gravity to the whole body. And then also the part immersed will be to the whole, as the specifick gravity of the body is to the specifick gravity of the fluid. (*a*)

(*a*) The author used to confirm these conclusions by the following experiments. To try the force of descent of a solid, he used a small glass viol stopt up, having shot enough in it to cause it to descend in water, and a horse-hair tied about the neck of it, by which he suspended it to the scale or beam of a ballance.

To find the weight of a bulk of water equal to this bottle, considered as the solid, he first put it into a narrow cylindrical glass jar, and poured in water enough to cover the bottle; then taking it out, he weighed the jar and water contained; then he placed the jar upon a table, and having immersed the bottle in it, he placed a slip of wet paper upon the outside of the jar, so that the edge of the paper might appear to coincide with the surface of the water, to his eye placed in that surface produced; then taking out the bottle he poured water into the jar, till its surface rose up to the edge of the paper as before. This water then was equal in bulk to the bottle, and its weight he found by replacing the jar in the scale, and adding a separate weight to the former, sufficient to counterpoise the additional water.

Place this counterpoise in one scale and the bottle in the other, and the weight added to make an equilibrium, will be the excess of the bottle's weight above the weight of a bulk of water equal to it.

Then suspending the bottle in water, by the hair, to the arm or scale of the ballance, from the opposite scale take out the weight of the equal bulk of water, and the remaining excess, found above, will just ballance the force of the bottle's descent, by keeping the scales in equilibrio.

The force of ascent of a thin glass bubble, or any solid lighter than the fluid, may be tried by an inverted ballance, placed at the bottom of a large vessel full of water as represented in Fig. 26.

For having found the weight of a quantity of water equal in bulk to the bubble (by immersing it wholly in the water of the cylindrical glass abovementioned) and also the excess of this weight

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If two fluids which will not eaſily mix with each other be contained in the ſame veſſel, ſo that the lighter may float upon the heavier, a ſolid body, which is heavier then the lighter of the two fluids, and lighter than the heavier, will not ſuffer itſelf to be totally immerſed in either of them. If it be placed wholly within the heavier it will aſcend, if it be placed wholly within the lighter it will deſcend for the reaſons before given; and will never reſt in any place till it be ſo diſpoſed, partly within one and partly within the other, that the weight of ſo much of both fluids, whoſe place it poſſeſſes, be equal to the weight of it.

If any one will from hence compute what proportion the parts contained within each fluid bear to each other, or in what proportion the common ſurface of both fluids divides the whole ſolid, he will find, that the part contained within the heavier, muſt be to the part contained within the lighter, as the difference in weight of the ſolid and an equal bulk of the lighter, is to the difference in weight of the ſolid and an equal bulk of the heavier: and that the part immerſed in the heavier, is to the whole, as the difference in weight of the ſolid and an equal bulk of

above that of the bubble itſelf, as before; hang the ſolid by an horſe-hair-loop to an arm of the inverted ballance, and connect the oppoſite arm to that of a common ballance by another horſe-hair; then that exceſs of weight, placed in the oppoſite ſcale, will ballance the force of the bubble's aſcent, by keeping the ſcales in equilibrio.

To ſhew that a quantity of a fluid equal in bulk to the part immerſed of a floating ſolid, is equal in weight to that of the whole ſolid, weigh a larger glaſs jar partly filled with water, and having taken it from the ſcale, let a ſmaller jar float upon the water and mark its altitude with a wet paper, as before; then having taken out the leſſer jar and filled the larger with water up to the mark, replace the larger in the ſcale, and the leſſer jar being put to the former weights in the oppoſite ſcale, will produce an equilibrium.

the lighter, is to the difference in weight of an equal bulk of the heavier and the ſame equal bulk of the lighter. *(b)*

Hence if we would be ſo ſcrupulouſly exact we may eaſily correct that ſmall error in the rule of *Archimedes* which was before delivered for floating bodies; namely, that the part immerſed was always to the whole, as the gravity of the ſolid is to the gravity of the fluid. For ſince the air is an heavy fluid, though it be the leaſt heavy of all others, yet by reſting upon the upper ſurface it has this effect, that in reality it will not permit a ſolid to be altogether ſo deeply immerſed as it would otherwiſe be if the air were removed, which the rule ſuppoſes. Allowing then for the air's preſence we may thus expreſs the proportion: That the part immerſed is to the whole, as the difference in weight of the ſolid and an equal bulk of air, is to the difference in weight of an equal bulk of the fluid and the ſame equal bulk of air. Whoever will compare theſe two rules together, will find that their difference is altogether inconſiderable: we may therefore ſtill very ſecurely make uſe of the old one without any further ſcruple.

The ſum of what has been ſaid comes to this, that if a ſolid be heavier bulk for bulk than the fluid in which it is immerſed, it will ſink till it arrives at the bottom, and the force of its deſcent will be equi-

(b) Let the parts of the ſolid contained within the heavier and the lighter fluid be A and B, in FIG. 27, and the ſpecifick gravities of the reſpective fluids as a and b; then ſince the abſolute gravity or weight of any body is compounded of its magnitude and ſpecifick gravity, the weight of a quantity of the heavier fluid equal in magnitude to the part A, is aA, and the weight of a quantity of the lighter fluid, equal in magnitude to the part B, is bB, and the ſum of their weights is $aA + bB = c \times \overline{A+B}$ ſuppoſing c is as the ſpecifick gravity of the ſolid $A+B$. Hence $aA - cA = cB - bB$, and conſequently $A : B :: c - b : a - c$; and conjointly $A : A+B :: c - b : a - b$.

valent

valent to the difference of its weight and the weight of an equal magnitude of the fluid. If it be lighter than the fluid in which it is immerſed, it will conſtantly aſcend till it be ſo far extant above the ſurface of the fluid, that its whole weight be equal to the weight of that part of the fluid whoſe place it takes up; and the force with which it aſcends will be equivalent to the difference of its weight and the weight of an equal magnitude of the fluid. If it be immerſed in either of two fluids differently heavy, which are contiguous, and it be of a middle gravity between both the fluids, it will move towards their common ſurface, and reſt in ſuch a poſition that the parts of both fluids whoſe place it takes up have an equal gravity with the ſolid itſelf. If a body be of equal gravity with the fluid in which it is immerſed, it will retain any poſition which is given it: and this is the reaſon why a bucket in a well is without difficulty drawn up as long as it is under water, and that we perceive not its weight till it begins to get above the ſurface: by the ſame reaſon a bucket full of wax, which is nearly of the ſame gravity with water, would not be difficult to draw up whilſt under water; now they cannot eaſily anſwer that wax is in its own element, and does not therefore gravitate.

All bodies do actually retain their whole gravity when immerſed in a fluid, but that is rendered ineffectual by the contrary preſſure of the fluid, ſometimes in part and ſometimes altogether, according as that gravity of theirs is greater, equal, or leſs than the gravity of an equal bulk of the fluid, which is the meaſure of the force which reſiſts their deſcent. We may ſay that the gravity of bodies within fluids is of two ſorts, whereof one is abſolute and true gravity, the other apparent and relative. The abſolute gravity is the whole force with which the

body

body tends downwards, the relative and vulgar is only the excess of gravity by which a body has a greater tendency downwards than the ambient fluid. The parts of fluids and all other bodies do gravitate *in proprio loco*, taking gravity in the first sense; according to the latter sense and acceptation of gravity bodies do not gravitate *in proprio loco*, that is, being compared together they do not preponderate, but hindering each others endeavours to descend they keep their places as if they were devoid of gravity. Thus in water, bodies which by their gravity, greater or lesser, do ascend or descend, may relatively and apparently be said to gravitate or levitate, and their relative gravity or levity is the excess or defect by which their true gravity does either exceed the gravity of water, or fall short of it. But if they neither descend by preponderating, nor ascend by yielding to the preponderating water, though they do by their real and absolute weights increase the weight of the whole, yet relatively and in the sense of the vulgar they do not gravitate in the water.

We may also in this place take notice of an objection which is sometimes made use of against the gravitation of fluids *in proprio loco*. They tells us, if fluids gravitate *in proprio loco*, that then a body as it happens to be immersed at different depths would have a different weight, according as it is pressed upon by different altitudes of the same fluid, which does not appear in fact. We may answer that it has and ought to have the same relative weight, though it be pressed upon by different altitudes at different depths. For its absolute weight does every where continue the same, and the relative weight is the excess of that absolute weight above an equal bulk of the fluid. Therefore if the weight of that equal bulk of the fluid be at all depths the same, as it certainly is in fluids which are not compressible, that excess

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and therefore the relative weight, will every where be the ſame alſo.

If the fluid be compreſſible, as air is, and the lower parts be condenſed by the weight of the upper, then indeed the relative weight of a body in the air at the bottom of a valley, will be leſs than its relative weight at the top of a mountain; an equal bulk of air weighing more in the valley than upon the mountain, and conſequently taking more from the real and abſolute weight of the body in the former caſe than in the latter. For the ſame reaſon if a body be weighed in freſh water, and ſea water, its weight will be leſs in the latter than the former, becauſe the exceſs of its real and abſolute weight above the weight of an equal bulk of ſea water, is leſs than the like exceſs above an equal bulk of freſh water; ſea water being about a thirtieth or fortieth part heavier than freſh. Upon this account alſo if two bodies of different ſpecifick gravities be equiponderant and conſequently of different magnitudes, ſuppoſe the one be of copper and the other of lead, and the bulk of the copper be greater than the bulk of the lead, as it muſt be to be of the ſame weight, putting the two metals in oppoſite ſcales of a ballance, we ſhall find them to reſt in equilibrio; but if we place the ballance under water, they will be no longer in equilibrio, and the lead will preponderate. For the abſolute weight of each being diminiſhed by the weight of a bulk of water equal to itſelf, the weight of the copper will be more diminiſhed than the weight of the lead. Thus if two bodies of different ſpecifick gravities be brought to a moſt perfect equilibrium when the air is at lighteſt, they will no longer remain ſo when the air changes and becomes heavier; and this is the foundation of the ſtatical baroſcope deſcribed by Mr. *Boyle* in the Philoſophical Tranſactions.

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The phænomena of glaſs bubbles and images, which are fitted ſeveral ways to aſcend and deſcend in fluids, have been very much celebrated by the philoſophers of the laſt age. The whole myſtery depends upon this, that by a greater or leſſer preſſure on the bladder at the end of the veſſel, Fig. 28, or by heat and cold, there is an alteration made of their weight, and by this alteration of their weight they become ſometimes heavier ſometimes lighter than that part of the fluid whoſe place they poſſeſs, and do therefore, for the reaſons which have been lately mentioned, ſometimes aſcend ſometimes deſcend with a pleaſing variety. Theſe bubbles conſiſt uſually of three different materials; of glaſs which is heavier *in ſpecie* than the fluid, of air which is lighter *in ſpecie* than the fluid, and of the fluid itſelf. As long then as that aggregate of bodies is lighter than an equal bulk of the fluid, it will float, but if it grows heavier than ſo much of the fluid, it muſt neceſſarily ſink. Now when there is any competent preſſure, whether produced by weight or otherwiſe, upon the water in which the bubble is commonly immerſed, becauſe the glaſs is a firm body and the water though a fluid ſuffers no compreſſion, the air included in the bubble, being a ſpringy and very compreſſible body, will be compelled to ſhrink, and thereby poſſeſſing leſs room than it did before, the contiguous water will enter the neck of the bubble and ſucceed in its place; which being a body about 850 times heavier than air, the bubble will thereby become heavier than an equal bulk of water, and will conſequently deſcend; but if the force or preſſure be removed, the impriſoned air will by its own ſpring free itſelf from the intruding water, and the aggregate of bodies that make up the bubble, being thereby grown lighter than an equal bulk of water, will again aſcend. The dilatation and contraction of the included air is there-

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fore the caufe of thefe changes; if then that dilatation and contraction be any other way procured than by preffure (as it may proceed from heat and cold) the event will be the fame. And this may fuffice to account for the phænomena of bubbles.

I promifed in the beginning of this difcourfe to give you the foundation of thofe propofitions of *Archimedes*, in which he demonftrates the pofture of floating bodies; it may thus be expreffed; that all floating bodies affect fuch a pofture, that the center of gravity of the part immerfed, be fituated perpendicularly under the center of gravity of the part extant: the body will otherwife never reft and ceafe to fluctuate. For if the floating body be imagined to be divided into two parts by the furface of the fluid, it will be eafy to conceive, that the part immerfed endeavours to afcend, and the extant part to defcend with equal forces; otherwife the body would be either more or lefs immerfed. Now the part immerfed endeavours to afcend by the perpendicular paffing through its center of gravity, and the part extant to defcend by the perpendicular paffing through its center of gravity; therefore unlefs thofe perpendiculars do coincide, or which is the fame thing, unlefs the center of the part immerfed be fituated perpendicularly under the center of the part extant, there will be no hindrance of thofe endeavours, but a motion will be produced, and for the fame reafon continued till that pofture be obtained; and in that pofture the body will acquiefce, the endeavours being then equal and directly contrary to each other, and thereby reftraining each other.

LECTURE V.

The hydroſtatical ballance explained, with the methods of determining the ſpecifick gravities of all ſorts of bodies thereby.

IT having been proved that bodies aſcend or deſcend in fluids with a force that is equal to the difference in weight of the body immerſed and an equal bulk or magnitude of the fluid itſelf, we are hence furniſhed with a very accurate and eaſy way of finding out the ſpecifick gravities of all manner of bodies whether fluid or conſiſtent, and of comparing them together. Bodies are ſaid to be ſpecifically or *in ſpecie* heavier or lighter one than another, when being equal as to magnitude, the weight of the one does exceed or fall ſhort of the weight of the other. Thus the ſpecifick gravity of quickſilver is about 14 times greater than that of water; for if you take an equal quantity of each as to magnitude, ſuppoſe a pint, the the pint of quickſilver will weigh about 14 times as much as the pint of water.

Several methods have been propoſed and more may be ſtill invented to determine in what proportion bodies differ from one another as to their ſpecifick gravities; yet after all, moſt men have with good reaſon preferred the uſe of the hydroſtatical ballance for exactneſs and convenience. It is very probable that *Archimedes* was the firſt that ever attempted this buſineſs with any ſucceſs, in order to diſcover the cheat of the workman that had debaſed king *Hiero*'s crown, and though the way he then made uſe of, be certainly much inferior to that we have been ſpeaking of by the hydroſtatical ballance (as may be perceived by the account which *Vitruvius* gives of it) yet ſo pleaſed was he to gain his end by any means, that upon this occaſion not being able to contain his

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joy, like a madman leaping from the bath, naked as he was, he is ſaid to have ran about the ſtreets of *Syracuſe*, crying out his Ἕυρηκα wherever he came. I will not here ſtay to enumerate and explain thoſe various methods that have been thought of for finding out the ſpecifick weight of bodies, but will confine myſelf to the buſineſs I have undertaken, and ſhew what helps we have from hydroſtaticks, and how ſuitable they are to our preſent purpoſe.

Firſt then if it be required to find out what proportion the ſpecifick gravity of a fluid and ſolid body have to one another, and the ſolid be heavier than the fluid ſo that it may ſink when immerſed in the fluid, we are to weigh the ſolid both in air and in the fluid. Now it has been proved before, that its weight in the fluid will be leſs than its weight in the air, by the weight of ſo much of the fluid as is equal in bulk to the ſolid; but the ſpecifick gravity of the fluid, is to the ſpecifick gravity of the ſolid, as the abſolute weight of an equal bulk of the fluid, is to the abſolute weight of an equal bulk of the ſolid; therefore the ſpecifick gravity of the fluid, is to the ſpecifick gravity of the ſolid, as the difference in weights of the ſolid in air and in the fluid, is to the weight of the ſolid in the air. If the fluid be common clear water and its ſpecifick gravity be expreſſed by an unit, as is uſual and very convenient upon ſeveral accounts, then to find a number which will expreſs the ſpecifick gravity of the ſolid, we muſt divide the weight of the ſolid in air by the difference of the weights of the ſame in air and in water, the quotient will be the number required.

An example will clear the whole matter. Suppoſe that a piece of copper weighed in air comes to 45 grains, and when weighed in water but to 40 grains; the difference of theſe two weights, which is 5 grains, is equal to the weight of ſo much water as is equal in bulk to the piece of copper. Therefore the ſpecifick gravity of water, is to the ſpecifick gravity of copper, as 5 to 45. The ſpecifick gravity of water is here expreſſed by the number 5, if inſtead of that it were to be expreſſed by an unit, we muſt divide 45, the weight of the copper in air, by 5 the difference of 45 and 40 its weight in air and water, and the quotient, which is 9, will expreſs the ſpecifick gravity of the copper as an unit does that of water, 9 bearing the ſame proportion to 1 as 45 did to 5.

If the ſolid body to be examined be ſpecifically lighter than the fluid, we would compare it with, ſo that it cannot ſink by its own weight, but is continually buoyed up by the heavier fluid, we may by a compound ballance find out its relative levity in the fluid, or the force with which it endeavours to aſcend.

It was yeſterday ſhewn by an experiment made with ſuch a compound ballance, that the force of aſcent is equal to the difference of weights of the aſcending body and an equal bulk of the fluid which invirons it; therefore the weight of ſo much of the fluid as is equal in bulk to the body, is the ſum of two weights, whereof one is the abſolute weight of the body in air, and the other is equal to the force of aſcent, being the weight which is applied to the compound ballance to reduce the aſcending body to an *equilibrium*. Hence it follows that the ſpecifick gravity of the fluid, is to the ſpecifick gravity of the ſolid, as that ſum of the two weights, is to the abſolute weight of the ſolid. If the fluid be common water, and its ſpecifick gravity be expreſſed by an unit, we muſt divide the abſolute weight of the ſolid by that ſum of weights, and the quotient will expreſs the ſpecifick gravity required.

To illuſtrate this by an inſtance, let us ſuppoſe that a piece of dry elm weighs in air 36 grains; this wood being lighter than water, will not of it ſelf ſink in it; let then a weight be applied to the ſuperior beam of the compound ballance to detain it under water and to keep it *in equilibrio*, and the weight neceſſary for

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that purpofe be found to be 24 grains; this weight of 24 grains being as we have already proved, equal to the difference of the weight of the elm and an equal bulk of water; if it be added to the leffer weight of the elm, which was 36 grains, the fum which is 60 grains will be the weight of that equal bulk of water. Therefore the fpecifick gravity of water is to that of elm as 60 is to 36; if inftead of 60 which does now exprefs the fpecifick gravity of water, you would rather make ufe of an unit for that purpofe, we muft divide 36, which is the weight of the elm in air, by 60 the fum which was before mentioned, the quotient 0,6 will exprefs the fpecifick gravity of elm, as an unit does the fpecifick gravity of water; and it is evident, that 0,6 has the fame proportion to an unit that 36 had to 60.

I know not whether this way of examining bodies which are lighter than the fluid they are compared with has ever yet been put in practice, there feeming to be too great a difficulty in making the experiment, which yet may be much leffened, if not taken away, by a well contrived inftrument; however it is certain that the calculation is much more eafy in this method than in that other which I will now defcribe.

To the body which we would examine which is lighter than the fluid with which it is to be compared, we muft annex another body (by tying them together with an horfe-hair or otherwife) which is fpecifically heavier than the fluid; fo that both taken together as one compound body, may be likewife fpecifically heavier than the fluid and fink in it; weighing then the heavier body fingly, and alfo the compound, both in air and in the fluid, we muft thus make our calculation. Subftracting the weight of the heavier body alone in the fluid, from its weight in air, what remains will be the weight of fo much of the fluid as is equal in bulk to the heavier body; again fubftracting the weight of the compound body in the fluid, from its weight in air, what remains will be the weight of ſo much of the fluid as is equal to the compound body in bulk; taking then the former difference from the latter, that is, taking the weight of ſo much of the fluid as is equal in bulk to the heavier body, from the weight of ſo much of the fluid as is equal to the compound body, or heavier and lighter together, what remains will be the weight of ſo much of the fluid as is equal in bulk to the lighter body; and the proportion which this weight bears to the weight of the lighter body in air, will be the proportion of the ſpecifick gravity of the fluid to the ſpecifick gravity of the lighter body. If the number which expreſſes the ſpecifick gravity of the lighter body be divided by the number which expreſſes the ſpecifick gravity of the fluid, the quotient will alſo expreſs the ſpecifick gravity of the lighter, whilſt an unit expreſſes that of the fluid.

Thus if a piece of elm weighs in air 15 grains, having fixed to it a piece of copper that the compound may ſink in water, let us ſuppoſe that the copper alone in air weighs 18 grains, in water 16 grains, the aggregate of the copper and elm in air will be 33 grains; ſuppoſe again we find by making trial of it, that the aggregate in water comes to 6 grains; if we ſubſtract 16 the weight of the copper alone in water, from 18 its weight in air, the 2 grains which remain will be the weight of a bulk of water equal to the copper; alſo if we ſubſtract 6 the weight of the compound in water, from 33 the weight of it in air, the 27 grains which remain will be the weight of a bulk of water equal to the compound. Taking then 2 the weight of water equal in bulk to the copper, from 27 the weight of water equal in bulk to the copper and elm together, the 25 grains which remain will be the weight of the water equal in bulk to the elm. The weight of the elm itſelf in air was 15 grains; therefore water is to elm in ſpecifick gravity as 25 to 15;

now

now as 25 is to 15 ſo is 1 to 0, 6 as any one may find by the rule of three. The third term in the proportion being in our caſe an unit, we are only to divide the ſecond by the firſt, or the weight of the ſolid in the air by the weight of an equal bulk of water, and the quotient will be the fourth term in the proportion, expreſſing the ſpecifick gravity of the ſolid, as an unit expreſſes that of water.

Having by this time I hope ſufficiently explained the method of comparing ſolids and fluids together as to their gravities, I am now to ſhew how ſolids are to be compared with ſolids, and fluids with fluids. This will require but few words being eaſy and obvious enough. Solids may be compared with ſolids by the mediation of a fluid; and fluids may be compared with fluids by the mediation of a ſolid; ſometimes indeed it may happen that a fluid may be weighed as a ſolid body within another fluid with which it will not mix, by placing it in the glaſs bucket; thus may quickſilver moſt conveniently be compared with water (*a*). Suppoſe it were required to determine what proportion the gravity of copper has to the gravity of elm; theſe two cannot immediately be compared together hydroſtatically, but we may as has been already ſhewed compare each of them with water, and then we may conclude that the ſpecifick gravity of copper, is to the ſpecifick gravity of elm, in a proportion, which is compounded of that of the ſpecifick gravity of copper to water, and that of the ſpecifick gravity of water to elm. If copper be to water

(*a*) Fig. 29. repreſents the hydroſtatical ballance, whereby ſolid bodies may be weighed in the glaſs bucket *a*, firſt in air and then in water. In the latter caſe the ſlit *b* in the circular plate, muſt firſt be ſlipt upon the neck *c*, and reſt upon the ſquare ſhoulder underneath; that the weight of the plate, being equal to that of a quantity of water equal in bulk to the empty bucket, may reſtore the equilibrium of the empty ſcales. The ſpecifick gravities of fluids are determinable by the glaſs ball *d*, deſcribed in the ſequel of the lecture.

as 9 to 1, and water be to elm as 1 to 0,6, copper will be to elm as 9 to 0,6 or as 90 to 6, or as 15 to 1.

Suppoſe again two fluids were propoſed to be compared together; let one of them be a parcel of oyl of vitriol bought in the ſhops which you ſuſpect to be not the beſt, and you would examine whether its gravity be to the gravity of water as 17 to 10, as it ought to be if good; comparing it with glaſs by the method before deſcribed, you find its gravity to that of glaſs to be as 7 to 15; comparing the ſame glaſs with water you find the gravity of glaſs to that of water as 3 to 1; hence by compounding the proportions of 7 to 15 and 3 to 1, you know that the gravity of your oyl of vitriol is to the gravity of water as 7 to 5, or as 14 to 10, whereas it ought to have been as 17 to 10. This way of comparing fluids together is univerſal, and may be practiſed with a ballance of any form. The fabrick of our inſtrument does indeed in this particular ſomewhat ſhorten the operation, and therefore it will not be amiſs to ſhew how we make our calculation from it.

The glaſs ball you may remember was heavier than an equal bulk of water, as was evident by its ſinking in it, and by an experiment purpoſely made, its weight was found to be to the weight of an equal bulk of water, as $18\frac{1}{2}$ to 10; oyl of vitriol, which is one of the heavieſt fluids excepting quickſilver, is to water as 17 to 10, therefore the ball may be uſed for examining any liquor that is leſs heavy than oyl of vitriol, ſince it will ſink even in that oyl. The exceſs of weight of the ball above that of an equal bulk of water, was counterpoiſed by an equal exceſs of weight, of the oppoſite ſcale of the ballance, above that of the ſcale to which the ball was fixed; and by that means it was ſuſtained in the water *in equilibrio.* We may conceive the ball ſo ballanced in the water, as if it were a parcel of the water congealed into that ſhape; and therefore if we ſubſtitute for water in the

veſſel

veſſel ſome other liquor of a different gravity, this *equilibrium* will be no longer preſerved; we are therefore continually to put weights into the aſcending ſcale, till we have again reduced the ballance to the ſame ſtate; and the weight we have put into either ſcale, will be the difference in gravity of two bulks, one of water and the other of the liquor to be examined, which are equal to one another, and each equal to the bulk of the ball. This bulk of water has been found to be 803 grains; if therefore we add to 803 the number of grains which were put into the ſcale to which the ball is annexed, or ſubſtract from 803 the number of grains which were put into the oppoſite ſcale, the reſult will be the weight of a bulk of the liquor under examination equal to the ball; and the ſpecifick gravity of water will always be to the ſpecifick gravity of the other liquor, as 803 to the reſulting number. If we divide the reſult by 803 the quotient will expreſs the gravity of the other liquor as an unit expreſſes that of water.

To illuſtrate this by an example, let it be propoſed to find the gravity of milk; immerſing the ball as it is fixed to the ballance in that liquor, we find it neceſſary to put 28 grains into the ſcale to which the ball hangs, in order to reduce the beam to its horizontal ſituation; adding then 28 to 803 the ſum will be 831, and the ſpecifick gravity of water to that of milk, will be as 803 to 831.

Thus then may all bodies of what kind ſoever be compared together as to their intenſive weights; I might have added other methods which are alſo hydroſtatical and fitted to the ſame purpoſe, but thoſe already deſcribed are ſufficient and ſeem indeed to be the moſt convenient. However I will here mention one other way of examining the gravity of fluids which is of very good uſe upon ſome occaſions. The foundation of it is this, that if a body be made ſucceſſively to float upon two fluids of different gravities, the ſpecifick weight of the lighter will be to the ſpecifick weight of the heavier, as the magnitude of the part of the floating body which is immerſed in the heavier, is to the magnitude of the part immerſed in the lighter. For, the bulks or volumes of both fluids that are equal to the parts reſpectively immerſed in them, having the ſame abſolute weight with the whole floating body, as hath been proved before, will be of equal abſolute weights, and conſequently their ſpecifick gravities will be reciprocally as their magnitudes, or which is the ſame thing, reciprocally as the magnitudes of the parts immerſed. If therefore a body of a regular figure could be provided ſo that the part of it which is immerſed, might always be accurately and eaſily meaſured, this way would be expeditious enough. A truly cylindrical glaſs veſſel ſeems to be the fitteſt for this purpoſe; for the part immerſed will be always as its depth. The gravity therefore of the fluid may readily be eſtimated by a proper ſcale of parts in arithmetical progreſſion applied to the ſide of the cylinder, or more readily by inſpection only of another ſcale which might be muſically divided by ways which I will not ſtay here to deſcribe. For, any one that ſhall attempt to get ſuch a cylinder, as will be convenient for his purpoſe, of an exact figure and truly poiſed, that it may always ſtand erect, will perhaps find it more difficult to obtain than at firſt he expected.

Neverthelеſs upon ſome occaſions this method may be of ſingular uſe. We may examine by this means whether a liquor propoſed be genuine as to its gravity, though we do not learn from hence what is the preciſe quantity of that gravity. The thing is common among chymiſts; they make uſe of an hollow ball of glaſs having a ſlender ſtem or pipe annexed to it, which is ſo poiſed that when the ball is immerſed in any liquor, the ſtem may ſtand erect and be in part extant above the ſurface of it. This

glaſs

glaſs they place in ſeveral liquors which they know by other means to be good in their kind, and put marks upon the ſtem, which ſhew the different degrees of immerſion in the different liquors. Then if any liquor of the ſame name be afterwards to be examined, ſuppoſe it were oyl of tartar *per deliquium*; if the glaſs ſink lower in it than the mark they had formerly made for that oyl, they conclude it has not its juſt gravity, and is probably adulterated with water; but if it ſink not ſo low as the mark for alcohol of wines, they conclude that the alcohol is too heavy, and therefore not ſufficiently rectified (*a*). This inſtrument by a ſmall alteration may be fitted to examine whether ſolids have the true ſtandard weight of their kind, as any one may perceive by the deſcription of Mr. *Boyle*'s eſſay-inſtrument, publiſhed in the Philoſophical Tranſactions.

After all that has been hitherto ſaid, there may yet remain ſome few difficulties to be removed, and ſome cautions to be given. It may happen that the body to be examined may conſiſt of ſmall fragments, or may be a powder, or may imbibe the water it is weighed in, ſo as to appear heavier than it really is, or may be diſſoluble in water. If it be made up of ſmall fragments or be a powder, we muſt of neceſſity in this caſe make uſe of the glaſs bucket, which we are not obliged to do when the body to be weighed is entire, and of a conſiderable magnitude. For then we may, if we think fit, make uſe of a ballance of the uſual form, and by ſuſpending the body with an horſe-hair (which is nearly of the ſame gravity with water) to the under ſide of either of the ſcales, we may weigh it both in air and in water. Putting then a convenient quantity of the fragments or powder into the bucket; we firſt find the weight in air, afterwards we muſt warily and little by little put into the bucket, whilſt it is yet kept in air, and hath the powder

(*a*) See Phil. Tranſ. N°. 384 and 413.

der or fragments in it, a convenient quantity of the ſame water it is to be weighed in, that the liquor may have time to inſinuate itſelf between the dry bodies and even the corpuſcles of the powder, and expel thence the air that was lodged in the intervals betwixt them; which little aerial portions, if not thus ſeaſonably expelled, would upon the immerſion of the veſſel, produce in the water ſtore of bubbles, that would buoy up or faſten themſelves to the fragments or other ſmall bodies, and make the experiment uncertain or fallacious. And if it be a powder that is to be weighed, unleſs it be beforehand throughly wetted, and thereby freed from aerial particles, and reduced to a kind of mud, there is danger that ſome dry corpuſcles of the powder will, when the veſſel is under water, be buoyed up and get out of it, and floating on the ſurface of the incumbent water take off from the true weight, that the immerſed powder ſhould have in that liquor.

If the body to be weighed be ſubject to imbibe water too readily, it may be caſed with a coat of bees-wax, and then we muſt proceed in our calculation according to the method which was before deſcribed for determining the gravities of bodies lighter than water, or the fluid they are weighed in, by adding a more ponderous body; and as there we weighed the heavier body by itſelf in air and in water, and afterwards the compound of the heavier and the lighter both in air and in water, ſo here we muſt weigh the bees-wax before we apply it both in air and water, and afterwards the compound of the bees-wax and the other body both in air and in water. If the body propoſed be diſſoluble in water, we may weigh it in other liquors, which will not diſſolve it.

Mr. *Boyle* tells us upon this occaſion, conſidering that except quickſilver, the viſible fluids we can command are either of an aqueous or of an oily nature, and that moſt bodies whereof we can make ſolutions in liquors of the former, will not (at leaſt ſenſibly) ſuffer themſelves to be diſſolved by thoſe of the latter kind, whilſt a propoſed ſolid is weighing in them; he preſumed that the moſt ſaline bodies, ſuch as allum, vitriol, ſal gem. borax, ſublimate and others, might de commodiouſly weighed in oleous liquors; and amongſt theſe upon ſeveral accounts he made choice of ſpirit of turpentine. If then we follow his choice, we may determine what proport ionthe gravity of the ſolid propoſed has to the gravity of that ſpirit; and by other ways, which have have been already explained, finding the proportion of the gravity of the ſame ſpirit to the gravity of water, by compounding their two proportions, we ſhall obtain the proportion of the ſolid's gravity to that of water.

Moſt men who treat of theſe matters do now adays compare all other bodies with water, whoſe ſpecifick gravity they expreſs by an unit. It may perhaps be objected that we cannot diſcover the proportion between a ſolid body and water in general, but only between the propoſed body and the particular water it is weighed in, becauſe there may poſſibly be a great difference between liquors which are called common water. Mr. *Boyle* has ſupplied us with an anſwer to this objection; I will therefore give you his words. "Having had, ſays he, the opportunity as well as cu-"rioſity upon ſeveral occaſions to examine the weight "of divers waters, ſome of them taken up in places "very diſtant from one another, I found the diffe-"rence between their ſpecifick gravities far leſs, than "almoſt any body would expect. And if I be not "deceived by my memory the difference between "waters, where one would expect a notable diſpari-"ty, was but about the thouſandth part (and ſome-"times perhaps very far leſs) of the weight of either. "Nor did I find any conſiderable difference between "the weight of divers waters of different kinds, as "ſpring water, river water, rain water, and ſnow wa-

" ter; though this last was somewhat lighter than any
" of the rest. And having had the curiosity to procure
" some water brought in England, if I much misre-
" member not, from the river Ganges itself, which
" some travellers tell us from the press is by a fifth
" part lighter than our water, I found it very little, if
" at all, lighter than some of our common waters."

It may also be possibly objected, that we take the weight which bodies have in the air for their absolute weight, whereas their absolute weight is what they would weigh in *vacuo*. I allow that all bodies have less weight in air than their absolute and real weights in *vacuo*; but if we consider that this diminution of real weight is in most bodies but about the thousandth part of the whole, and in metals, which are the heaviest kind of bodies, much less, this objection will cease to be considerable: yet if any one has a mind to be so needlessly accurate in a matter which will not bear that nicety upon other accounts, he may add the number which expresses the specifick gravity of air, to all the other numbers of any table of bodies, and by that means correct the whole error which rises from this source.

Though this way of examining the gravities of bodies hydrostatically be preferable to all others, yet it is not free from some uncertainties. Bodies themselves though they have justly the same name and are referred to the same kind, have not any exact common standard of weight, and some little errors will inevitably arise in physical experiments though made with the utmost accuracy. We must therefore be as cautious as we can. When we weigh any thing in water or any other liquid, great care ought to be taken, that no part of the body to be weighed touch the bottom or sides of the vessel, or rise above the surface; that no bubbles of air stick to and buoy it up; that no drops of the liquor touch or adhere to the scales or beam. Several other cautions there are which may

best

beſt be learned by experience. I therefore deſign to-morrow to practiſe the rules which have been now laid down, and to ſuggeſt ſome further uſes of this ſort of experiments.

A table of the ſpecifick gravity of bodies.

Fine gold	1.9640	Oyl of tartar	1.550
Standard gold	1.8888	Bezoar	1.500
Quickſilver	1.4000	Honey	1.450
Lead	11.325	Gum arabick	1.375
Fine ſilver	11.091	Spirit of nitre	1.315
Standard ſilver	10 535	Aqua fortis	1.300
Biſmuth	9.700	Pitch	1.150
Copper	9.000	Spirit of ſalt	1.130
Caſt braſs	8 000	Craſſamen. of hum. blood	1.126
Steel	7.850	Spirit of urine	1.120
Iron	7.645	Human blood	1.054
Tin	7.320	Amber	1.040
Glaſs of antimony	5.280	Serum of human blood	1 030
A pſeudo-topaz	4.270	Milk	1.030
A diamond	3.400	Urine	1.030
Clear cryſtal glaſs	3.150	Dry box-wood	1.030
Iſland cryſtal	2.720	Sea water	1.030
Fine marble	2 700	Common water	1.000
Rock cryſtal	2.650	Camphire	0.996
Common green glaſs	2.620	Bees wax	0 955
Stone of a mean gravity	2.500	Linſeed oyl	0.932
Sal gemmæ	2.143	Dry oak	0.925
Brick	2.000	Oyl olive	0.913
Nitre	1.900	Spirit of turpentine	0.874
Alabaſter	1.875	Rectified ſpirit of wine	0.866
Dry ivory	1.825	Dry aſh	0.800
Brimſtone	1.800	Dry maple	9.755
Dantzick vitriol	1.715	Dry elm	0.600
Allum	1.714	Dry firr *(a)*	0.550
Borax	1.714	Cork	0.240
Calculus humanus	1.700	Air	0.001$\frac{1}{4}$
Oyl of vitriol	1.700		

(a) Theſe are the ſpecifick gravities of dry wood. For Dr. *Jurin* has obſerved that the ſubſtance of all wood is ſpecifically heavier than water, ſo as to ſink in it, after the air is extracted from the pores and air-veſſels of the wood, by placing it in warm water under a receiver of an air-pump; or if an air-pump cannot be had, by letting the wood continue ſome time in boiling water over a fire. Thus he found alſo, that ſome human *calculi*, contrary to common opinion, are almoſt as heavy as bricks and the ſofter ſort of paving ſtones. *Phil. Tranſ.* N°. 369.

The ſpecifick gravities of human blood, its *craſſamentum* and *ſerum* are taken from very accurate experiments made by the ſame judicious author. *Phil. Tranſ.* N°. 361.

LECTURE

LECTURE VI.

The praxis of the hydrostatical ballance, the specifick gravities of several bodies actually found out, with an account of the various uses of such enquiries.

IT may now be expected, after so much of our time spent in explaining and practising the hydrostatical ballance, that I should give some account of the usefulness of such tryals. To do this in its fullest extent would take up more of our time than we can conveniently spare upon that subject alone. There have been whole books written upon this matter by *Ghetaldus* and Mr. *Boyle*, who have nevertheless left several things untouched, which might have been pertinent enough to their purpose. Any one who has in the least meddled with natural philosophy, must needs be sensible of what great importance it is to be able to compare bodies together, as to their magnitudes, densities, and the quantities of matter they contain. Now this may be done by the help of the instruments we have been describing and making use of; for the density of any body is as its specifick gravity, and the quantity of matter it contains is as its absolute weight; therefore whatever comparisons we can make of the magnitudes, specifick gravities and absolute weights of bodies, the same will hold good as to their magnitudes, densities and quantities of matter considered together. Hence did our famous Sir *Isaac Newton* conclude that water has about 40 times more pores than solid parts. Hence also he made it evident that the forces of bodies to reflect and refract light are very nearly proportional to their densities, excepting that unctuous and sulphurous bodies refract more than others of the same density.

It would be needless to heap up instances of this kind which any thinking person cannot miss of. But though

though it be impoſſible to declare all the uſes there may be of comparing bodies thus together, I will however give ſome certain rules for ſuch compariſons, and ſhew how from any two of theſe three things, magnitude, ſpecifick gravity and abſolute weight given, the other may be determined. Thus will our geometry be enlarged, and we may be able to find out the magnitude of any body however irregular, by its weight and ſpecifick gravity; and the ſame way our ſtaticks may be improved to find out the weight of any body how great ſoever, ſuppoſe it were a whole building, by the magnitude and ſpecifick gravity of the materials which compoſe it. After this I will ſhew how any mixture of any two bodies may be diſcovered, as the allay of gold and ſilver, and other problems of the like nature. In the third and laſt place I will give ſome inſtances of the uſefulneſs of theſe enquiries to phyſicians, chymiſts, apothecaries, jewellers, goldſmiths and others, who by theſe means may be able to judge whether the materials they deal with be rightly qualified for their purpoſe or not.

To determine any one of theſe three things, magnitude, ſpecifick gravity and abſolute weight, the other two being ſuppoſed to be given or known, we may obſerve the following Rules.

1. If bodies compared together be of equal magnitudes, their abſolute weights will be as their ſpecifick gravities.

2. If bodies compared together be of the ſame ſpecifick gravity, their abſolute weights will be as their magnitudes.

3. If bodies compared together have their abſolute weights equal, their magnitudes and ſpecifick gravities will be reciprocally as one another.

Hence if bodies compared together be neither of equal magnitudes, nor of the ſame ſpecifick gravity, nor the ſame abſolute weight, then

4. Their abſolute weights will be in a compound ratio of their ſpecifick gravities and their magnitudes.

5. Their ſpecifick gravities will be in a compound ratio of their abſolute weights directly and magnitudes inverſely.

6. Their magnitudes will be in a compound ratio of their abſolute weights directly, and their ſpecifick gravities inverſely.

To fit theſe rules to our purpoſe, we ought further to know the exact weight of ſome certain magnitude of a determinate body whoſe ſpecifick gravity we can readily compare with that of all other bodies; ſuch I take water to be, which I therefore make choice of. Now a cubick foot of water weighs preciſely 1000 averdupois ounces; I have found it to be very nearly ſo myſelf, and by comparing ſeveral experiments that have been made by others for this purpoſe, I find there is ſometimes an exceſs at other times a defect, but the difference is always inconſiderable. It falls out very happily and is a great eaſe in calculation, that one cubick foot of water, with which other bodies are moſt eaſily compared, and whoſe ſpecifick gravity is commonly expreſſed by an unit, ſhould weigh ſuch a round number of ounces.

We have another convenience by taking the weight thus by ounces, that we can by this means the more eaſily expreſs our deductions by other ancient and modern weights. For it has been ſufficiently proved that the ancient and modern Roman ounce has no ſenſible difference from our averdupois ounce, and it is well known that other ancient and modern weights are moſt eaſily referred to thoſe of the Romans.

Our averdupois ounce contains 437½ grains troy, and our averdupois pound contains 7000 grains troy, ſo that the averdupois ounce is to the troy ounce nearly as 51 to 56, and the averdupois pound to the troy pound nearly as 17 to 14. We may therefore by any of these proportions make a reduction from either of these weights by the other.

It being evident then by experiment that a cubick foot of water weighs 1000 averdupois ounces, we may hence shorten and facilitate the rules which were laid down in more general terms, by compounded proportions, and instead of them make use of these following; which all along suppose the specifick gravities of bodies to be expressed by a scale of numbers wherein 1000 is put for the specifick gravity of water, and their absolute weight to be expressed by the number and parts of averdupois ounces, and their magnitude to be expressed by the number and parts of a cubick foot.

1. The absolute weight of any body is equal to the product which arises by multiplying its magnitude and specifick gravity together.

2. The specifick gravity of any body is equal to the quotient of its absolute weight divided by its magnitude.

3. The magnitude of any body is equal to the quotient of its absolute weight divided by its specifick gravity.

These three rules may be sufficient and are easy enough to be practised: I will give an example fitted to each. Suppose an architect being about to build a church were desirous to know beforehand what weight of lead is requisite to cover it, in order to compute the expence he must be at. He knows by the dimensions he has proposed to himself, that the area to be covered is 30000 feet, and is satisfied by experience that the thickness of an hundreth part of a foot is sufficient; multiplying then 30000 by $\frac{1}{100}$, or dividing it by 100, the magnitude of the lead whose weight he requires will be 300 cubick feet. By making experiment or by some table he finds that the specifick gravity of lead is 11325 at the same time that the ſpecifick gravity of water is 1000. Multiplying therefore according to the firſt rule the ſpecifick gravity 11325 by 300 the magnitude, the product will 3397500, which is the number of ounces that the lead will weigh. There are 35840 ounces in a tun, if therefore the product be divided by the number of ounces, the quotient will be the number of tuns requiſite to cover the whole building, which amounts to 94 and about $\frac{4}{5}$ of a tun.

Suppoſe again for an example of the ſecond rule that a poliſhed parallelopiped of fine marble is in magnitude equal to 4 cubick feet, and when weighed comes to 6 hundred weight and 3 pounds, and it were required to find the ſpecifick gravity of it. An hundred weight being 112 ſingle pounds, 6 hundred weight and 3 pounds will be equal to 675 ſingle pounds, which multiplied by 16 makes 10800 ounces. If then according to the ſecond rule, we divide the weight of marble by 4 its magnitude, the quotient 2700 will be the ſpecifick gravity of marble as 1000 is the ſpecifick gravity of water. By this method we find the ſpecifick gravities of bodies without the help of hydroſtaticks, but as this method can ſeldom be put in practice by reaſon of the irregular figures of moſt bodies which we may have occaſion to examine, ſo is it never ſo accurate as the hydroſtatical way which I have already explained.

The third rule is of excellent uſe for determining the magnitude of any body how irregular ſoever it be, provided we can aſſign both its abſolute and ſpecifick weight. Let ſeveral fragments of coral be propoſed, whoſe ſpecifick gravity we find to be 2690; ſuppoſe their weight be 7 ounces; divide then according to the rule, the abſolute weight 7 by the ſpecifick gravity 2690, the quotient will give the total magnitude of all the fragments equal to $\frac{26}{10000}$ of a cubick foot; to reduce that to cubick inches, multiply

tiply

tiply theſe parts of a cubick foot by 1728, the number of cubick inches contained in a cubick foot, the product will be four inches and very nearly an half, which is the total magnitude of all the irregular fragments which were propoſed to be meaſured: which is an eaſy and very exact way of obtaining the dimenſions of ſeveral bodies which cannot be brought under the rules of geometry.

Another uſe which I mentioned of theſe hydroſtatical tryals, was to diſcover in what proportion any two bodies are mixed together in a compoſition offered to be examined. The *data* requiſite for this purpoſe, are the ſpecifick gravities of the mixture and of the two ingredients; theſe are all of them to be obtained by the hydroſtatical ballance, and are ſufficient for the diſcovery, by the help of the following rules of proportion.

As the difference of the ſpecifick gravities of the mixture and the lighter ingredient, is to the difference of the ſpecifick gravities of the mixture and heavier ingredient, ſo is the magnitude of the heavier to the magnitude of the lighter ingredient; then as the magnitude of the heavier ingredient multiplied into its ſpecifick gravity, is to the magnitude of the lighter ingredient multiplied into its ſpecifick gravity, ſo is the weight of the heavier ingredient to the weight of the lighter.

The reaſon of this laſt rule is obvious enough from what has been ſaid before, and the reaſon of the firſt may eaſily be found out by thoſe who are qualified to underſtand it when demonſtrated; I will therefore paſs it over and propoſe an example.

Let it be the famous one of king *Hiero*'s Crown. Suppoſe then the ſpecifick gravity of the gold, which the king furniſhed his workman with for making the crown were as 19, and the ſpecifick gravity of the crown as it was debaſed, were 16, and the ſpecifick gravity of the silver which the workman used for that purpose were 11, we are from these *data* thus to state our proportion according to the first Rule. As 5 the difference of 16 and 11, the specifick gravities of the mixture and lighter ingredient, is to 3 the difference of 16 and 19, the specifick gravities of the mixture and heavier ingredient, so is the magnitude of the heavier ingredient gold, to the magnitude of the lighter ingredient silver; by which it appears, that of the magnitude of the crown, 3 parts in 8 were silver. Then by the second rule we must say as 95, the product of 5 and 19, the magnitude and specifick gravity of the heavier ingredient gold, is to 33 the product of 3 and 11, the magnitude and specifick gravity of the lighter ingredient silver; so is the weight of the heavier ingredient gold, to the weight of the lighter ingredient silver. By which it appears that of the whole weight of the crown 33 parts in 128, or somewhat more than a fourth part were silver, if the circumstances were really such as we supposed them to be (*a*). Thus may the fineness of coins be examined and the proportion of alloy be determined without any detriment. And what has been said as to metals may be applied to other bodies and even fluids to very good purposes, if due caution be taken.

The third and last thing proposed was to give some instances of the usefulness of these enquiries to physicians, chymists, apothecaries, jewellers, goldsmiths, &c. Mr. *Boyle* has treated this subject very fully in his excellent *Medicina Hydrostatica*, I will therefore

(*a*) The rule abovementioned may be thus found out. Let the magnitudes of the gold and silver in the crown be A and B, and their specifick gravities as a and b; then, since the absolute gravity or weight of any body is compounded of its magnitude and specifick gravity, the weight of the gold is aA, of the silver bB, and of the crown $aA + bB = c \times \overline{A + B}$ supposing c is the specifick gravity of the mixture. Hence $aA - cA = cB - bB$ and consequently $c - b : a - c :: A : B$, which is the rule abovementioned.

transcribe

transcribe some things from that book of his; a few will suffice to encourage the reading of it to those who have not yet done it, and many would be tedious to those who have. Having made it probable in his treatise of gems, that divers, if not most, of the real virtues of precious stones may in great part proceed from the quantities of metalline and mineral substances, that in the state of fluidity or softness were incorporated with the stony matter, which hardened afterwards into a gem, he was hence induced to suspect that divers boles, clayes and other earths, that several minerals not looked upon as metalline, that several stones which are commonly neglected for want of beauty, may yet be endowed with considerable medicinal virtues, and perhaps with greater than the finer gems themselves; upon account of the great quantity of metalline and mineral substances, with which they might be impregnated whilst they were in *solutis principiis*. The method which he proposes for the exploration of fossils, is by finding their specifick gravities. For since the most pure and homogeneous kind of stones, are in gravity to water as about 2 $\frac{1}{2}$ to 1; and tin the lightest of metals is about 7 times heavier than water; if a stony substance be found to exceed in gravity that proportion of 2 $\frac{1}{2}$ to 1, it must be probable that it has in it some adventitious matter of a metalline nature, or is at least commixed with some mineral body more heavy than pure stone; and may therefore very probably be usefully applied to some medicinal purposes. He illustrates this matter by experiments made upon some substances which are found to be useful in physick; such are the *lapis hæmatites* or blood-stone, *lapis lazuli*, the load-stone, and *lapis calaminaris*, which he found to be in gravity to water respectively as 4.15, 3.00, 4.93 and 4.92 to 1.

A second use which he proposes of the hydrostatical way of enquiry, is to find out whether a body propounded as likely to be a ſtone of the mineral kind, be ſo indeed. Thus coral which, ſays he, ſome take to be a plant, others a lithodendron, but moſt reckon it among precious ſtones, is in gravity to water as 2.68 to 1, which favours the laſt opinion. Thus a pearl was found to be in gravity as 2.51; a *calculus humanus* was in gravity as 1.7; a bezoar as 1.5. Theſe two laſt he thinks ought to be diſtinguiſhed from common ſtones, being ſo much lighter, and he therefore chuſes rather to call them animal ſtones than ſingly ſtones. A third uſe which he propoſes, is to diſcover the reſemblance or difference between bodies of the ſame denomination. A fourth uſe is to diſcern genuine ſtones from counterfeit ones, which may be of great help to jewellers. He inſtances in factitious coral and factitious gems, which he found out that way not to be genuine. A bezoar which in appearance ſeemed to be very fair and by no means a counterfeit, and had therefore a great price ſet on it, was the ſame way detected, being found to be as ponderous as a mineral ſtone of the ſame bigneſs, whereas it ought to have been nearly as light again. Thus mercury has been ſometimes found not altogether 13 times and an half heavier than water; at other times it has been obſerved to be ſomewhat above 14 times heavier.

Now that we may obſerve this by the way, here may hence ariſe a notable difference in two weather-glaſſes at the ſame time and in the ſame place, if the mercury of the one be not of the ſame gravity with the mercury of the other, and the difference may amount even to a whole inch. Thoſe therefore who publiſh regiſters of the weather, ought alſo to find out and declare to the world the ſpecifick gravity of the quickſilver they make uſe of in their tubes.

After the ſame method may an eſtimate be made of the goodneſs of any of thoſe ſubſtances that compoſe the *materia medica*, which is of great uſe to phyſicians,

ſicians, chymiſts, apothecaries and druggiſts. Hence alſo may the goldſmith be aſſiſted in judging of the fineneſs of his metals, and the merchant in his choice of ſand gold and other precious commodites which are often counterfeited; and the miner may hence inform his judgment concerning the various ſubſtances he meets with under ground.

To conclude, this excellent philoſopher, about the end of his book lets us know the high value he has for this method in the following words. "As little skill "as I have in hydroſtaticks I would not be debarred "from the uſe of them for a conſiderable ſum of mo-"ney; it having already done me acceptable ſervice, "and on far more occaſions than I myſelf expected "at firſt, eſpecially in the examen of metals and mi-"neral bodies, and of ſeveral chymical productions. "And I have been able more than once or twice to "undeceive artiſts and other experimenters, that *bo-"na fide*, believed they had made, or were poſſeſſors "of, *luna fixa*, as they call it, and other valuable "things; and to make a judgment of the genuineneſs "or falſity, and the degrees of worth and ſtrength "in their kind, of divers richer and poorer metal-"line mixtures and other bodies, ſome ſolid and ſome "liquid, whoſe fair appearances might otherwiſe "have much puzzled, if not deceived me."

LECTURE VII.

The ſeveral phænomena of the Torricellian experiment exhibited and explained.

HAVING exhibited the principal phænomena of the Torricellian experiment *(a)*, I need not uſe many words to evince their dependence upon the gra-

(a) The principal phænomena of the Torricellian experiment, ſo called from the name of its inventor, are repreſented by FIG. 30,

vitation of the air. Let us ſuppoſe that theſe things were altogether new to us, and laying aſide all former prejudices in favour of any hypotheſis, let us try whether a due conſideration of the effects which we have ſeen, may not be ſufficient to lead us into the knowledge of their cauſes. It appears at firſt ſight to be altogether repugnant to the laws of hydroſtaticks, that the quickſilver within the tube ſhould be ſo much more elevated than that within the veſſel into which

in the following manner. Having filled a glaſs tube with quickſilver and covered its orifice with your finger, and inverted it, and immerſed the finger in a veſſel of quickſilver; upon withdrawing the finger from the orifice, the quickſilver will never wholly ſubſide; if the tube be long enough, it will ſubſide in part, till it reſts at a certain altitude, generally between 31 and 28 inches; but if the tube be ſhorter than that altitude, called the ſtandard, it will not ſubſide at all.

It is further remarkable, if ſeveral tubes of various lengths, ſhapes and capacities be thus filled and inverted, that the ſurfaces of the contained quickſilver will reſt exactly upon the ſame level in all, whether held upright or any way inclined. I ſay exactly, provided the bore of none of the tubes be too narrow, and due care be taken in filling them to expel all the little air-bubbles that adhere to their inſides: which may be done by putting a ſlender wire into the tube and ſtirring it up and down; or, if the tube and quickſilver be very clean, as they ought, by leaving about an inch or leſs of the tube unfilled, by covering its orifice with your finger and by inverting it gently, that the air in the vacant part may aſcend gradually along the tube, and ſweep up the little air-bubbles along with it; and laſtly by reverting it gradually to its former poſition and filling it up with quickſilver.

In this latter method take care that the large air-bubble may not aſcend too quick, leſt by its ruſhing againſt the crown of the tube with violence, it ſhould break it. Alſo in emptying the tube, for fear of the like accident, take care firſt to incline in it, then to draw the orifice gently above the ſurface of the ſtagnant quickſilver, and immediately to immerſe it again, ſo as to take in but little air at a time.

This is the way of making a weather-glaſs, which will be compleat when placed in a common frame, having at the ſide a ſcale of three inches, divided into tenths as uſual, and placed at the height of 28 inches above the ſurface of the quickſilver in the baſon.

it was inverted. For imagining an horizontal plane to paſs by the lower orifice of the tube through the the body of the quickſilver within the veſſel, it will be evident that the part of the plane which is contiguous to and placed directly under the ſaid orifice, has a greater weight of quickſilver incumbent upon it than any other equal part of the ſame plane. Now we have often ſeen in the former week, that fluids cannot poſſibly reſt *in equilibrio*, whilſt the equal parts of ſuch an imaginary horizontal plane are unequally preſſed upon, which nevertheleſs happens in our preſent experiments; therefore we muſt neceſſarily conclude, either that the general courſe of things is here interrupted and that theſe phænomena are a ſort of miracle in nature, or that there is, contrary to what does at firſt ſight appear, an equality of preſſure upon every part of our imaginary plane. If there be that equality of preſſure, it muſt proceed either from ſome increaſe of the leſſer or ſome diminution of the greater, or perhaps from both; that is, there muſt either be ſome, as yet by us unheeded preſſure, added to that of the quickſilver in the veſſel, or ſome ſuſpenſion of the quickſilver in the tube, whereby its exceſs of gravitation may be taken off.

Let it then be conſidered how either of theſe ways may be accounted for. If the equality proceeds from ſome preſſure added to that of the quickſilver in the veſſel, it muſt ariſe from ſomething contiguous to the ſurface of the veſſelled quickſilver. Since then the ſurface is contiguous to the air only, nothing but the preſſure of the air can be that additional force I have hitherto been ſpeaking of. The preſſure of the air is therefore one of the cauſes we are to examine. We are alſo to conſider of the other, and to enquire how the exceſs of preſſure of the quickſilver in the tube may be ſuſpended; and here I muſt needs confeſs myſelf to be utterly at a loſs, not being able to imagine any cauſe ſufficient for this effect, that ſhall at the ſame time agree with the reſt of the appearances of nature. *Franciſcus Linus* does indeed imagine he has found out what I deſpair of; let him then be anſwerable for his own conceit, which it may not be improper in this place to give you ſome account of; and I cannot do it better, ſince I have not *Linus*'s book by me, than in the words of Dr. *Power*. His principles are theſe.

1. That there is an inſeparability of bodies, ſo that there can be no vacuities in *rerum natura*.

2. That the deſerted part of the tube is filled with a ſmall film of quickſilver, which being taken off from the upper part of it, is both extenuated and extended through the ſeeming vacuity.

3. That by this extended film or rope, as he calls it, of dilated quickſilver, the reſt of the quickſilver in the tube is ſuſpended, and kept up from falling into the veſſel.

4. That this funicle or rope is exceedingly rarefied and extended by the weight of the pendent quickſilver, and will, upon removal of that violent cauſe which ſo holds it, recontract itſelf into its former dimenſions again, and ſo draw up what body ſoever it hath hold of along with it; as the effluviums of an electrick body, upon its retreat, plucks up ſtraws or any other thing with it, that it is able to weild.

5. That this extenſion of the film of quickſilver is not indefinite, but hath a certain limit beyond which it will not be ſtretched; and therefore if the tube be of an exceeding great height, the quickſilver will rather part with another film and extend that, and ſo a third or fourth till it come to the ſtandard of 29 inches where it reſts, having not weight nor power enough to ſeparate another film from it.

Theſe are his principles, and that you may have a taſte of the application he makes of them, I will add,

that

that his reaſon why the quickſilver in a tube under 29 inches deſcends not at all is this, becauſe it ſticks with its uppermoſt ſurface ſo cloſe to the top of the tube, that there is not weight enough to break that adheſion; the reaſon whereof is, becauſe there is nothing to ſucceed in the room of the deſcending quickſilver, and therefore it firmly ſticks there *ne daretur vacuum.*

In longer tubes it falls to that ſtandard, becauſe then the greater weight of the quickſilver is able to break that link of contiguity or adheſion, and therefore the uppermoſt ſurface of the quickſilver being ſliced off, is dilated into a thin column or funicle, which ſupplies that ſeeming vacuity. Now, ſays Dr. *Power*, for the poſitive arguments to avouch his principles by, he has none at all, only what he fetches *a poſteriore*, from his commodious ſolution of difficulties and ſalving the phænomena better than others have done. This is the hypotheſis of *Linus*, and the only one I have met with that pretends to account for our phænomena by taking off the exceſs of gravitation of the quickſilver in the tube.

Thus much then we have hitherto eſtabliſhed, that nature either ſuſpends her ſettled laws for the production of theſe phænomena, or that there is ſome additional preſſure communicated to the quickſilver in the veſſel, which can be no other, as has been proved, than the preſſure of the air which is contiguous to it; or laſtly that the exceſs of preſſure from the quickſilver in the tubes is by ſome way or other, which I confeſs I cannot diſcover, taken off or rendered ineffectual.

It is unreaſonable to imagine that nature ſhould forſake her wonted paths upon ſo trifling an occaſion; it is certain we have no precedents to warrant ſuch a ſuſpicion; it has indeed been ſtrenuouſly maintained by the ſchools, that nature does at all times ſuſpend

pend any of her laws to prevent a *vacuum*, to which they confidently tell us ſhe has a moſt dreadful averſion. Now by nature they muſt mean, if they mean any thing, either the author of all created beings or the creatures themſelves; if they would be underſtood in the firſt ſenſe, they do unavoidably charge omniſcience its ſelf with incogitancy, ſuppoſing him ſo to have created the world as continually to ſtand in need of miracles for its preſervation; it being in their own power as often as they pleaſe, to make a trifling experiment to put him to the neceſſity of interpoſing to hinder a *vacuum*. If they mean by nature the creatures themſelves, then they muſt of neceſſity fall into another abſurdity, whilſt they ſuppoſe brute matter to be intelligent, and to put its ſelf into action in purſuit of ſome determinate end.

This I preſume may be ſufficient to expoſe that groſs opinion concerning a *fuga vacui*, ſuppoſing it could account for our experiments, which it cannot do by any means. When the length of the tube was leſs than 30 or 29 inches or thereabouts, the quickſilver as we ſaw did not aſcend, and thereby deſert the top of the tube, ſo as to leave a vacuity behind it; here indeed it poſſibly might be pretended that nature went out of her way to prevent a *vacuum*; why then was ſhe not equally concerned to do it when the tube was longer? It muſt at laſt be ſaid that her power is limited and kept in by certain determinate laws.

There remains then but two ways of explaining the phænomena we are concerned with. The preſſure of the air upon the quickſilver in the veſſel, or ſome unknown cauſe which takes off the exceſs of preſſure from the quickſilver in the tube. Let us now try wheher either of them may poſſibly be excluded, ſo that at length we may be certain what to fix upon.

We may make uſe of that ingenious device of Mr.

Azout

Azout as an *experimentum crucis* in our preſent enquiry (*a*). We are chiefly to attend to what happened in the upper tube and ciſtern. It was evident that the quickſilver which remained in the upper ciſtern, after opening the orifice of the lower tube, was free from the contact of the air; if therefore the elevation of the quickſilver in the tubes in the former experiments, did proceed from the preſſure of the air

(*a*) The inſtrument for trying M. *Azout*'s experiment conſiſts of ſeveral parts. In FIG. 31, *a b* is the lower baſon, *b c* the lower tube, *c d e f* the upper baſon, whoſe bottom part is ſkrewed at *c* into a hollow braſs collar cemented round the top of the lower tube, and *e f g* is the upper tube, put through a braſs collar cemented to it at *f* and ſkrewed into the upper part of this baſon. This tube reaches almoſt to the bottom of the baſon, covered over with hard cement; through which, towards one ſide, there paſſes a ſmall pipe *h i*, ſo bent, underneath the cement, towards the middle of the baſon, as to reach down towards the orifice of the under tube *c b*.

The manner of making the experiment is this. The bottom of the lower tube is ſurrounded with two parallel collars of hard cement, having a neck between them, for the convenience of tying a piece of wet bladder very tight over this orifice of the tube. Having taken off the upper ciſtern and tube, firſt fill the lower tube with quickſilver, well purged of air-bubbles; then ſkrew on the upper baſon, and fill it with quickſilver, which will alſo fill the included pipe *h i*; then ſkrew in the upper tube and fill it alſo with quickſilver, well purged of air-bubbles, up to the very top, and tye a wet bladder over it in the ſame manner as below.

The inſtrument being thus filled, with a ſmall penknife immerſed in the quickſilver in the lower baſon, cut ſlits in the lower bladder, for the quickſilver in the tubes to deſcend through; by which means the upper tube will firſt be evacuated and then the upper part of the upper baſon, as low as the upper orifice of the bended pipe; then will this pipe be next evacuated and the upper part of the lower tube, ſo far as to leave no more in it than a column equal to the ſtandard altitude, as in the Torricellian experiment made with a ſingle tube.

But upon unſkrewing the upper tube, ſo as to admit ſome air gradually into the upper baſon, the quickſilver will aſcend to the ſtandard altitude in the upper tube, and deſcend quite to the bottom of the lower tube. Laſtly upon pricking the upper bladder with a pin, the admitted air will depreſs the quickſilver, in this tube alſo, to the level of that in this baſon.

upon

upon the quickſilver in the ciſtern, becauſe in this upper ciſtern there was no ſuch preſſure, there ought to be no ſuch elevation; but if the elevation of the quickſilver in the former experiments did depend upon ſome other unknown cauſe, by which the exceſs of preſſure from the quickſilver in the tube was taken off, then ought the quickſilver in the tube to remain alſo elevated in this experiment, all circumſtances being the ſame here as in the former, excepting that the air is here excluded from the ſurface of the quickſilver in the upper ciſtern. Since then in this deciſive experiment we ſaw that the quickſilver in the upper tube was not elevated as in the former experiments, but fell down to the level of that which was contained in the ciſtern, we may very ſecurely from hence conclude, that its elevation in the former experiments was not owing to any other cauſe, but to the preſſure of the air only.

For a further confirmation of this concluſion, which yet needs not any, we may obſerve, that as the air was permitted gradually to enter into the upper ciſtern, ſo was the quickſilver gradually elevated in the upper tube, till it had attained its ſtandard altitude of about 29 or 30 inches; and at the ſame time the quickſilver in the lower tube was depreſſed gradually, till it was reduced to the level of that in the lower ciſtern. For the air being ſuffered to preſs equally upon the quickſilver in the lower tube and upon that in the lower ciſtern, there is now no reaſon why it ſhould be any longer elevated, as it was when the air preſſed only upon that within the ciſtern. You will hereafter meet with ſtill further proofs and confirmations of the air's preſſure.

It is now our buſineſs to account for that obſervation, which was made upon theſe experiments, that the perpendicular altitude of the quickſilver in the tube above that in the ciſtern is conſtantly the ſame, whatever be the length, wideneſs, figure or ſituation of of it. We have ſeen already that the preſſure of the air is that additional force which counterballances the exceſs of gravity from the quickſilver in the tube; it will follow from hence that, that part of the upper ſurface of the quickſilver in the ciſtern which is contained within the orifice of the tube, is juſt ſo much, neither more nor leſs, preſſed upon by the quickſilver in the tube, as any other equal part of the ſame ſurface is preſſed upon by the weight of the column of air incumbent on it.

We ſaw in the laſt week, that the preſſure of all fluids upon any propoſed plane was according to their altitudes; that as long as the altitude was the ſame, the preſſure was alſo the ſame, though the quantity of the preſſing fluid was never ſo much altered, either by being contained in a veſſel differently figured or differently inclined. Therefore in all the caſes of the experiments which have been made this day, the preſſure of the quickſilver contained in the tubes, however figured or ſituated, is every where the ſame, and every where equal to the preſſure of the air upon the other equal parts of the ſurface of the quickſilver in the ciſtern, becauſe, as we ſaw, the altitude is always the ſame. The elevation of the quickſilver always to the ſame height, ought not then to be urged as an argument againſt what we have proved, that the gravitation of the air is the cauſe of the phænomena in the Torricellian experiment, ſince we have ſhewn it to be a neceſſary effect of that cauſe.

Let us now proceed to thoſe experiments which were made to determine the force requiſite to bear up the inverted tube (*a*). We obſerved that the force

(*a*) In Fig. 32, *a b* repreſents the Torricellian tube immerſed in a baſon of quickſilver, and ſuſpended at the beam of a ballance by the ſtring *a*, lapped about the end of the tube and faſtened to it with hard cement. The tube being counterpoiſed with weights in the oppoſite ſcale, its orifice plays freely within the quickſilver in

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was, as nearly as we could eſtimate it, equal to the weight of the tube and the quickſilver contained in it; abating the weight of ſo much quickſilver as was equal in magnitude to the part immerſed. This phænomenon does at firſt view ſeem to diſagree with what we have hitherto advanced, and to favour the funicular hypotheſis; accordingly the patrons of that hypotheſis have not been wanting to make uſe of it for their purpoſe. For if the preſſure of the air upon the ſurface of the quickſilver in the veſſel, be the true cauſe of the elevation of the quickſilver in the tube, it ſhould ſeem that the weight of the quickſilver in the tube, being ſuſtained by that preſſure of the air, ought not in the leaſt to be perceived by the hand of him who holds up the tube; and that he ought to be ſenſible only of the bare weight of the tube. Since then the weight of the quickſilver, as appears by our experiments, does alſo ſeem to load the hand, it may perhaps be ſuſpected that the quickſilver is connected to the top of the tube by *Linus*'s rope.

But we ought to obſerve that this weight, which at firſt ſight one would be apt to aſcribe to the quickſilver contained in the tube, is not in reality the weight of the quickſilver, that being unqueſtionably ſupported by the preſſure of the air upon the ſtagnant quickſilver of the ciſtern below, but is the weight of the column of air incumbent upon the crown of the

the baſon, and is hindered from aſcending accidentally above it, by a table placed under the oppoſite ſcale. With your finger immerſed in the quickſilver, having covered the orifice of the tube and ſhut up the column within it, take it up and incline it gradually till it be quite inverted, then putting the cloſed end into the quickſilver in the baſon, hang the open end upon the ballance by the ſtring *b*, and the beam will again be in equilibrio as before.

α β repreſents a like tube, ſhorter than the ſtandard altitude, which being hung at the ballance by the ſtring α or β at either end, will alſo counterpoiſe the ſame weight in both poſitions. I believe Dr. *Wallis* was the inventor of this experiment.

tube,

tube, which is equivalent to the weight of the quickſilver contained in the tube, and which we therefore unwarily are apt to look upon as the very weight of that quickſilver it ſelf. For the weight of that air ought in this experiment to be perceived by the ballance, or by the hand of him who held up the tube, ſince it is not counterballanced and ſo taken off by an equal preſſure from below, as it is when the tube is empty; that counterballance being now otherways employed in bearing up the quickſilver within the tube.

There have been ſeveral warm diſputes, but to little purpoſe, about the ſpace in the top of the Torricellian tube, which is deſerted by the deſcending quickſilver, ſome holding it to be an abſolute vacuity, others denying that there is or can be any ſuch thing in nature. Their *materia ſubtilis* is always ready upon any difficulty, and they employ it a thouſand ways as occaſion requires. This we may ſecurely affirm, and it is ſufficient for our purpoſe, that the deſerted ſpace, if it be not altogether free from the common air we breathe, yet is ſo as to all ſenſe; which we may be certain of by inclining the tube; for the quickſilver will again repoſſeſs the ſpace it had formerly quitted, which it could not do, were that ſpace taken up by the air. We may indeed ſometimes, upon inclination, perceive ſome air ſtill lurking behind, but the quantity of it is generally ſo ſmall, if care be taken in making the experiment, that it deſerves not to be regarded.

LECTURE VIII.

Mr. Pascal's *imitation of the Torricellian experiment by water; other experiments of the like nature with fluids variously combined; the pressure of the air shewn by experiment to be different at different altitudes from the surface of the earth.*

I PROCURED the apparatus for that chargeable and troublesome experiment of Mr. *Paschal* (*a*) rather as a curiosity than as absolutely necessary to our purpose; for since the pressure of the air was able to keep the quicksilver in the Torricellian tube, elevated to the altitude of about 29 or 30 inches, as we yesterday saw, we might without making the experiment safely conclude, that the same pressure of the air would be sufficient to keep water elevated in the Pascalian tube to an altitude about 14 times greater; that is, to about 34 feet. For since quicksilver, as we found by the hydrostatical ballance, is about 14 times specifically heavier than common water, so the altitude of water requisite to counterballance the pressure of the air, ought to be about 14 times as great as the altitude of quicksilver requisite for the same purpose. But matter of fact is always more convincing than any reason how well soever it be grounded, and what we see with our eyes is always more satisfactory than any relation of what has been done by others. We have therefore ventured to repeat that noble experiment, which had also been tried by others with success, though not very often by reason of the great difficulty in managing it.

Mr. *Pascal* is said to be the first that attempted it. The learned and industrious Jesuit *Gaspar Schottus* has, in

(*a*) The best way of trying this experiment is sufficiently described in the sequel of the lecture.

in his *Technica curiosa*, given us an account, from a letter of Mr. *Roberval*, of the motives which chiefly induced Mr. *Pascal* to make this tryal. He tells us from that letter, that Mr. *Pascal* did at Roan in Normandy exhibit this experiment with water and wine, in tubes of crystal glass 40 feet long, which were fixed to the mast of a ship, that was contrived to be raised and depressed as need required. He also informs us that the occasion of having recourse to tubes of that length, was, that when several learned men saw the quicksilver in the Torricellian experiment, when it deserted the upper part of the tube, so to descend as always to possess in the lower part of the tube, the altitude of 2 Paris feet and $3\frac{1}{2}$ inches nearly, measured perpendicularly from the surface of the quicksilver in the vessel underneath, they divided into different opinions; and some of them who were peripaticks asserted, that in the upper part of the tube, deserted by the quicksilver, there was contained some spirits evaporated from the quicksilver, which being rarefied did fill that part, thereby helping nature now put to her shifts, against her mortal enemy a *vacuum*. Now Mr. *Pascal*, that he might plainly convince these men of their error, procured (says *Roberval*) crystal tubes of 40 feet in length to be tied to a mast, and engines to be applied as was said before; and having fixed upon a day and a spacious place near the glass-house, he invited all to be present to see wonders.

Now Mr. *Pascal* had privately made a calculation of water and wine, compared with quicksilver as to their gravities, that thence he might find out the altitude due to each of them, so as they might equiponderate; and he found, that taking 2 Paris feet and $3\frac{1}{2}$ inches for the altitude of quicksilver, the altitude due to the water must be 31 feet and about $\frac{1}{7}$; and in like manner the altitude due to the wine must be 31 feet and $\frac{2}{7}$ nearly. Then before he opened any thing of his defign, by queftioning them he eafily made them confefs that there was certainly a greater quantity of fpirits in wine than in water; fo that if the experiment could be made with thofe liquors, the wine would leave a greater fpace in the top of the tube than the water, provided the tubes were of the equal lengths. This being allowed him, the maft was fhewn with the tubes tied to it, which being filled, the one with water, the other with wine, and their orifices being clofed, the maft was erected, and veffels were applied to the orifices, the one filled with wine, the other with water, into which the orifices were immerfed, the tubes ftill remaining full till their orifices were opened; which when done, the liquors contained in the tubes did fo defcend, as that after they came to reft, the altitude of the water in its tube, above the furface of the water in the veffel below, was 31 feet and about $\frac{1}{3}$, but the altitude of the wine was fomewhat greater, being 31 feet and about $\frac{2}{3}$; the upper parts of both tubes remaining to appearance void of any thing, as is ufual in the Torricellian experiment.

The liquors in the tubes were afterwards changed, that being filled with water which was before filled wine, and that with wine which was before filled with water; notwithftanding this, no alteration was obferved as to the altitudes. Thus did he confute his adverfaries from their own conceffions, fhewing them, if their hypothefis were true, that a greater fpace was neceffarily occupied by the fpirits of water than by thofe of wine, and confequently that water was more fpirituous than wine; contrary to all reafon and experience.

Let us now come to thofe experiments which were made by combining two different fluids in the fame tube. The trials we made were with quickfilver and water, quickfilver and air, water and air, and thefe

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are ſufficient to let us underſtand what the event would be if any other fluids be made uſe of. We have ſeen that quickſilver is elevated by the preſſure of the air to about 29 inches and ½, and water to about 34 feet. It is eaſy then to underſtand that a mixture of quick-ſilver and water, muſt needs be raiſed by the ſame preſſure to ſome intermediate altitude; what that altitude will preciſely be, muſt be determined by the particular quantity of water we employ in the experiment.

Let us ſuppoſe that the quickſilver we make uſe of is 14 times ſpecifically heavier than the water, and that the ſtate of the weather is ſuch, that the weight of the air is equipollent to 30 inches of that quickſilver, as it often happens to be; putting then 28 inches of water into the tube, we may thus compute before hand what the event will be: 28 inches of water are equal in weight to 2 inches of quickſilver, 28 inches being equal to 14 times 2 inches of quickſilver, as the ſpecifick gravity of quickſilver is 14 times greater than the ſpecifick gravity of water; but the air was able to ſuſtain 30 inches of quickſilver by ſuppoſition; ſince then theſe 28 inches of water are equal but to 2 of the 30, there remains 28 inches more of quick-ſilver to equal the preſſure of the air. The whole compound then of quickſilver and water will be ſuſtained at the altitude of twice 28 or 56 inches.

Suppoſe again that 14 inches of water were placed in the tube; then becauſe 14 inches of water are equivalent to one of quickſilver; taking that one inch from 30 as before, there will remain 29 due to the quickſilver, ſo that the whole compound of quick-ſilver and water together will ſtand at 43 inches.

If quickſilver and air be to be combined together in the ſame tube, it will be much more difficult to make an eſtimate of the altitude at which the quick-ſilver will ſtand, than in the former caſe; the problem is properly algebraical, and he who will ſolve it muſt do it analytically, for which reaſon I ſhall omit it in this place and invert the problem. Inſtead of computing at what height the quickſilver will ſtand for any propoſed quantity of air lodged in the top of the tube, I will here ſhew how to determine the quantity of air which being placed in the upper part of the tube, will cauſe the quickſilver to reſt at any given altitude leſs than the Torricellian ſtandard.

The rule may be thus ſtated: as the ſtandard altitude of the quickſilver in the Torricellian tube at the time of making the experiment, is to the defect of the propoſed altitude from that ſtandard, ſo is the length of that upper part of the tube which is to be left free from quickſilver after inverſion, to the length of that part of the tube which is to be left free from quickſilver before the inverſion.

I will firſt endeavour to make this rule clearly underſtood by an example or two, and then give the reaſon of it. Suppoſe at a time when the altitude of the quickſilver in the Torricellian tube or barometer is 30 inches, it were required to fill a tube of 36 inches partly with quickſilver and partly with air, ſo that after inverſion the altitude of the quickſilver may be 20 inches. Let us ſuppoſe that when the tube is inverted, one inch of it is immerſed in the ſtagnant quickſilver of the veſſel below, ſo that there remains but 35 inches above the ſurface of the ſtagnant quickſilver; the defect of the propoſed altitude 20 inches from the ſtandard altitude 30 inches, is equal to 10 inches; the length of the upper part of the tube which is to be left free from quickſilver after inverſion, is 15 inches; for there being but 35 inches of the tube extant above the ſurface of the quickſilver in the veſſel, and 20 of theſe 35 being poſſeſſed by the quickſilver, there will remain 15 free from the quickſilver; we muſt ſay then, by the rule, as 30 inches,

inches, the ſtandard altitude, are to 10 inches the defect of the propoſed altitude from the ſtandard, ſo are 15 inches, the length of the upper part of the tube to be left free from quickſilver after inverſion, to 5 inches, the length of that part of the tube which is to be left free from quickſilver before the inverſion. If then we fill the tube with quickſilver excepting 5 inches which we leave to be poſſeſſed by the air, then cloſing the orifice we invert it, theſe 5 inches of air will readily aſcend to the top of the tube; afterwards immerſing the lower end an inch deep under the quickſilver in the veſſel, when we uncover the orifice, the quickſilver will immediately fall down to the altitude of 20 inches, which was the thing propoſed to be effected; and the air which did juſt before poſſeſs only 5 inches, will now upon the retreat of the quickſilver dilate itſelf to 15.

In this example the tube was 36 inches long, which is more than the ſtandard; ſuppoſe now it were but 24 which is leſs than the ſtandard; let an inch as before be immerſed in the veſſel, there will then be but 23 extant, and let it be propoſed to find out how much air muſt be left in the tube before inverſion, to make the quickſilver reſt at the altitude of 18 inches after inverſion. The defect of 18 inches from the ſtandard is 12 inches; the upper part of the tube which is to be free from the quickſilver, is 5 inches, 18 of the 23 inches extant being to be poſſeſſed by it. Say then as 30, the ſtandard, is to 12, the defect of the altitude propoſed from the ſtandard, ſo is 5, the part free from quickſilver after inverſion, to 2, the part to be left free from quickſilver before inverſion. If then the tube be filled with quickſilver excepting 2 inches, after inverſion the quickſilver will reſt at the altitude of 18 inches.

In order to make out this rule we muſt obſerve, in the firſt place, that there is a ſpring or elaſtical power in the air we live in; by which I mean that our air either consists of, or at least abounds with parts of such a nature, that in case they be compressed and thereby reduced into lesser dimensions, either by the weight of the incumbent part of the atmosphere, or by any other force, they endeavour as much as in them lies, to free themselves from that pressure, and to regain their former dimensions, by bearing against the contiguous bodies that keep them in. This is what any one may observe in a blown bladder; the air contained in it may by the force of his hands be reduced into a lesser space, but then as soon as the force is removed, it will immediately expand itself as before, and you may perceive even whilst you compress it, a very great endeavour to free itself from the violence you offer to it.

In the second place we may observe, that this elastical or expansive power of the air is equivalent to the force which compresses it; for were it less, it would still yield to a further degree of compression, and were it greater, it would not suffer itself to be so much reduced, action and reaction being always equal. It follows from hence, that the elastick power of any small parcel of the air we breathe, is equivalent to the weight of the incumbent part of the atmosphere, that weight being the force which confines it to the dimensions it possesses. Though this assertion may at first view seem a paradox, yet, as was said before, if the elasticity or spring of this small portion of air, were not so great as the weight of the incumbent part of the atmosphere, it would yield to that weight and permit itself to be confined in narrower bounds.

In the third place we may take notice, that by a greater compression, the air is still reduced into lesser dimensions, and on the contrary, its dimensions are enlarged as its compression is diminished. We cannot

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by any reaſoning alone find out what ſort of proportion the dimenſions bear to the compreſſions. The Author of nature might have ordered theſe things otherwiſe than he has done by infinite variations; we muſt therefore by experiments try what is the conſtitution of nature. I deſign therefore at our next meeting to attempt this buſineſs, and I hope then to demonſtrate, that the ſpace which any propoſed quantity of air at any time poſſeſſes, is reciprocally as the force which compreſſes it.

When I ſay the ſpace is reciprocally as the force, I mean that the ſpace is diminiſhed in the ſame proportion in which the force is increaſed, and increaſed in the ſame proportion in which the force is diminiſhed: thus a double force reduces the air into half the ſpace, a triple force reduces it into a third part of the ſpace it poſſeſſed before; ſo half the force permits the air to expand itſelf into double the ſpace, and a third part of the force permits to expand itſelf into a ſpace triple of that which it poſſeſſed before.

Now the force of elaſticity, being as I have already proved, equal to the force of compreſſion, it will follow that the force of elaſticity is reciprocally as the ſpace which the air takes up: which property I will at this time take for granted, and proceed to make out the truth of our rule.

In Fig. 33 and 34, let *a e* be the tube propoſed, *b c* the altitude at which the quickſilver is to ſtand, *b d* the ſtandard altitude in the Torricellian experiment. The preſſure of the atmoſphere upon the ſurface of the veſſel below is, in the Torricellian experiment, ballanced by the column of quickſilver *b d*, or *b c* and *c d* together; in our preſent experiment it is ballanced by the weight of the column *b c* and the preſſure downwards of the air *e c*, contained in the top of the tube, upon the upper ſurface *c* of the elevated quickſilver; which preſſure ariſes not from

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the weight of that included air, for that is altogether inconſiderable, but from its elaſticity or endeavour to enlarge its dimenſions, which it can no otherways do but by depreſſing the quickſilver *cb*; the weight of the columns *b c* and *c d* is therefore equivalent to the weight of *b c* and the elaſtick force of the air *c e*, both being equivalent to the preſſure of the atmoſpere upon the ſurface of the veſſel. The weight of the column of quickſilver *c d* is therefore equivalent to the elaſtick force of the included air *c e*.

Let *e f* be the ſpace which that air in its natural ſtate did poſſeſs, when it was firſt lodged in the top of the tube, before it had depreſſed the quickſilver to *c*, and by its ſpring expanded itſelf to its new dimenſion *e c*. Its elaſtick force whilſt it was in its natural extent *e f*, was equivalent to the preſſure of the atmoſphere, as has been already proved; and therefore equivalent to the weight of the column of quickſilver *b d*.

Thus far then have we hitherto proceeded; we have found that the elaſticity of air in the ſpace of *e c*, is equivalent to the weight in *c d*; that the elaſticity of the ſame air in the ſpace *e f* is equivalent to the weight of *b d*; the elaſticity in *e f* therefore is to the elaſticity in *e c* as *b d* to *d c*.

Now by the property of the air which I told you was to be proved to morrow, that the elaſticity is reciprocally as the ſpace, it follows that the elaſticity of the air in *e f*, is to its elaſticity in *e c*, as the ſpace *ec* to the ſpace *ef*. Therefore as *bd* is to *dc*, ſo is *e c* to *e f*; that is, as the ſtandard altitude in the Torricellian tube at the time of making the experiment, is to the defect of the propoſed altitude from that ſtandard, ſo is the length of the upper part of of the tube which is to be left free from quickſilver after the inverſion, to the length of that part of the tube which is to be left free from quickſilver before the inversion; and this is what I undertook to make out (*b*). The same rule will hold good with a small alteration, if instead of quicksilver we would combine water and air together in a tube of any proposed length.

(*b*) FIG. 33, 34. Having ef to find ec or cb, is the converse of the foregoing proposition, and as the author observed above, is to be solved analytically.

By the allowed property of air, that its elasticity is reciprocally as the space it possesses, we had $ec : ef :: bd : dc$, or $ec \times dc = bd \times ef$; which shews that the question abstractly considered, is only to find a point c, in a given line ed produced, at which the rectangle under ce and cd, shall be equal to the given rectangle under bd and ef. Now since the sides ce, cd are related both alike to their difference de, and consequently both determinable by a like analysis, it would be arbitrary to seek either of them rather than the other; which intimates, that a simpler and better way will be, to bisect their given difference de in g, FIG. 35, and to seek their half sum gc.

Hence we have $ce = cg + gd$ and $cd = cg - gd$, and $ce \times cd = \overline{cg + gd} \times \overline{cg - gd} = \overline{cg}^2 - \overline{gd}^2 = bd \times ef$ by the condition of the problem: which gives this Theorem, $\overline{cg}^2 = \overline{gd}^2 + bd \times ef$, or, $cg = \sqrt{\overline{gd}^2 + bd \times ef}$.

We had an example in pag. 86, where $bd = 30$ inches, $de = 5$, and $ef = 5$. Here $\frac{1}{2}\, de$ or $dg = 2.5$. and, by the Theorem, $cg = \sqrt{6.25 + 150} = 12.5$; whose difference $cd = 10$ and sum $ce = 15$, and the column $cb = 20$ inches.

But instead of the Theorem for arithmetical computation, if a geometrical construction be desired; in FIG. 35, 36, to the standard altitude bd add $dh = ef$, and upon the diameter bh describe a semicircle bih, cutting in i a line di drawn perpendicular to the tube; then bisecting de in g, and joining gi, a circle described with the center g and semidiameter gi, will cut the tube in the point c where the surface of the column of quicksilver will rest.

This will appear from the Theorem $\overline{cg}^2 = \overline{gd}^2 + bd \times ef = \overline{gd}^2 + bd \times dh$ by construction, $= \overline{gd}^2 + \overline{di}^2$ by the known property of the semicircle bih, $= \overline{gi}^2$ by the property of the right angled triangle gdi.

But if a synthetical demonstration be desired, let the circle ci cut the produced tube in k, then $bd \times ef = bd \times dh$ by construction $= \overline{di}^2$ by the known property of the circle bih, $= dc \times dk$ by the like property of the circle cik, $= dc \times ec$; and by resolving length. We muſt then ſay as the ſtandard altitude of the water in the Paſcalian experiment, which is commonly about 34 feet, is to the defect of that propoſed altitude of the water from the ſtandard, ſo is the length of the upper part of the tube which is to be left free from water after inverſion, to the length of that part of the tube which is to be left free from water before inverſion.

We may now go on to the experiment that was made at the top and bottom of the Obſervatory, which affords us as ſenſible an argument for the air's preſſure as can well be deſired, and of the difference of that preſſure at different altitudes (*c*). I muſt confeſs I cannot ſee any objection that may with the leaſt

the firſt and laſt rectangles into a proportion of their ſides, we have *ec* : *ef* :: *bd* : *dc*; therefore, by the property of the air's ſpring, the ſurface of the quickſilver will reſt at *c*.

The problem abſtractly conſidered, has two anſwers, becauſe the circle *c i k* cuts the produced line *de* in two points; whereof *k* has this property in our particular problem, that if the ſpace *e k* be filled with a column of quickſilver, having a *vacuum* above it, its upper ſurface will reſt at *k*, as that of the lower column does at *c*; becauſe the ſpring of the air included in the ſpace *ce*, is equipollent to the weight of a column whoſe length is *e k* or *cd*.

(*c*) Fig. 37. Having with a red hot iron burned a moderate hole length-ways through a cork, and ſqueezed it hard into a glaſs bottle, whoſe bottom is covered with water about an inch deep; and having run a glaſs tube, open at both ends, through the hole in the cork, quite down into the water, and covered the top of the cork with cement, to hinder the air from paſſing by the ſides of it; with your mouth applied to the top of the tube, force ſome air downwards into the water, till it riſes in bubbles above its ſurface, and increaſes the quantity of air within; whoſe ſpring, by preſſing ſtronger now than before on the furface of the included water, will raiſe it in the tube above the mouth of the bottle. Then having placed the bottle in any convenient veſſel, and covered it all over with ſalt or ſand, to keep the included air of the ſame temperature; after the ſurface of the elevated water is quite ſettled at any place of the tube, mark it by tying a thread about the tube or otherwiſe. Then having carried the veſſel up to the higheſt place at hand, you will find the water riſen higher in the tube, and reſting above the mark: which plainly ſhews that the preſſure of the air

degree

degree of probability be urged against it. I shall not therefore go about to obviate any. What I think most pertinent to observe concerning it, is this, that we are not only from hence convinced of the weight of the air, but may hereby also determine the proportion of its specifick gravity to that of water. The difference in height of the two places in which we made the experiment is 54 feet, and that difference in height caused the difference of $\frac{3}{4}$ of an inch in the height of the water. By which it appears that a column of water of $\frac{3}{4}$ of an inch or $\frac{1}{16}$ of a foot, is equiponderant to a column of air of 54 feet having the same basis. Therefore the gravity of water is to that of air, as 54 to $\frac{1}{16}$ or as 864 to 1.

We might have made with the Torricellian tube an experiment like this, to shew the different pressure of the air at different distances from the surface of the earth, had the Observatory been much higher than it is. At the altitude of 54 feet the ascent of the quicksilver would be too small to ground any thing upon, being but about $\frac{1}{20}$ of an inch. It was therefore necessary to make use of the contrivance you have seen, to supply the defect of some very high mountain, upon which had any such been near us, we might have observed a sensible alteration even with the Barometer. Such an experiment was formerly made at the desire and by the direction of Mr. *Pascal*, in the year 1648, upon the Puy de Domme, a very high mountain in France. It was then observed that in ascending 3000 Paris feet, the quicksilver in the tube fell

upon the surface of the water, is now less than it was below-stairs. For the air within the bottle being of the same temperature as before, presses by its spring upon the included water with the same force; which being ballanced in part by a column of water in the tube, now longer and heavier than the former, must have its total ballance made up by the weight of a lighter column of air incumbent upon the heavier column of water.

down

down 3 inches and $\frac{1}{8}$ of an inch. To reduce this to English measure, we may say that ascending 3204 English feet, the height of the quicksilver was abated 3 inches and $\frac{1}{3}$ of an inch. Another experiment like this was made by Mr. *Caswell* upon Snowdon Hill in Wales; he found that the height of 3720 feet abated the quicksilver 3 inches and $\frac{8}{10}$.

It may not be amiss here to add the result of a computation which I made of the weight of all the air which presses upon the whole surface of the earth. If this weight were to be expressed by the number of pounds it contains, that number would be so large as to be in a manner incomprehensible. I will therefore make use of another way of expressing it, by determining the diameter of a sphere of lead, of the same weight with all the air which presses upon the whole surface of the earth. Now that diameter was found to be very nearly 60 miles long. If any one has a desire to make this calculation after me, he may proceed upon these grounds. That the weight of a column of air reaching to the top of the atmosphere, is most commonly equal to a column of water having the same basis, and the altitude of 34 feet; that the semidiameter of the earth is equal to 20949655 feet, and that the specifick gravity of water is to that of lead as 1000 to 11325.

LECTURE IX.

The density and spring of the air proved to be as the force which compresses it, and from hence an enquiry is made into the limits and state of the atmosphere.

It was proved yesterday that the air has a spring or elastical power, by which it constantly endeavours to expand itself; and that the force of that spring is always equivalent to the force by which the air

air is compressed; but we did at the same time take for granted that the space which the air possesses is reciprocally as that force, and consequently its density directly as the same. Let us now try by making experiment, whether the assertion will hold true. The air may be either more rare or more dense, than it is in that constitution of it, which we commonly, but perhaps somewhat improperly, call its natural state. We will therefore by two different sets of experiments make our trials upon it, first when it is more and afterwards when it is less expanded than as we usually breathe it.

Let *a e* in FIG. 33, be a tube hermetically sealed at the end of *e* and open at the end *a*; placing the end *e* downwards, if we fill the whole tube with quicksilver excepting a certain space, which we leave to be possessed by the air, then stopping the orifice *a* and inverting the tube, we permit the included air to ascend into the space *ef*, afterwards immersing the end *a* into a vessel of quicksilver, we open the orifice, which was before closed, the quicksilver will descend to *c*, and the air *e f* will thereby expand itself into the space *e c*. Now if *b d* be the standard altitude in the Torricellian experiment, you may remember it was yesterday proved that the force by which the air was compressed, whilst it was contained in the space *e f*, was equivalent to the weight of the column of quicksilver *b d*; and the force with which it was compressed whilst in the space *e c*, was equivalent to the weight of the column of quicksilver *dc*. If then we find by making experiment that the space *e c* is constantly to the space *ef*, as the force which compresses the air in the space *ef*, is to the force which compresses the same air in the space *ec*, that is, as the weight of the column *b d* to the weight of the column *d c*, or as the length *b d* is to the length *d c*, we may safely conclude that the space which the air when rarefied possesses, is reciprocally as the force which compresses it.

To examine also whether the same proportion holds true for condensed air, we may make use of a tube bent up like that in Fig. 38, whose extremity *n* is supposed to be open, but *g* to be hermetically sealed up. Pouring in then just so much quicksilver as will fill the bottom *ik*, so as to shut up the air *ig* from making its escape, if the surfaces of the quicksilver at *i* and *k* be both upon the level, we may conclude that the pressure of the air *ig* upon the surface *i*, is equivalent to the weight of that part of the atmosphere which presses upon *k*; and therefore that the weight of a column of quicksilver of the standard altitude, is equivalent to the force which compresses the air *ig*. After this if we pour in more quicksilver at *n* till it ascends in the longer leg to *m*, we shall at the same time perceive it to rise in the shorter leg to *h*, and therefore the air which before did possess the space *ig*, will be condensed and reduced to the space *hg*. Now in this case it is evident that the force which compresses the air into the space *hg*, is equivalent to the weight of the column of quicksilver *lm*, besides the weight of a column of the standard altitude, if *l* be upon the level with *h*. If then we find upon trial, that the space *hg* is constantly to the space *ig*, as the force which compresses the air whilst it is contained in the space *ig*, is to the force which compresses the same air whilst it is contained in the space *hg*, that is, as the standard altitude is to the standard altitude and the altitude *lm* together, we may also conclude, that the air when condensed does always possess a space which is reciprocally as the force which compresses it.

Let us try then whether the event will nearly answer what we expect. We may be certain there will be some small difference after all the cautions we can possibly take, unless the bores of the tubes *ec* and *gi*, Fig. 33, 38, be truly cylindrical, which seldom or never happens; and the cause of that difference which may arise is this, that we suppose the spaces *ef* and *ec*, *gi* and *gh* to be to each other as their lengths, which

which supposition is erroneous unless the tubes be perfect cylinders.

Having yesterday made it appear from reason, that the spring or elastick power of the air is as the force which compresses it, and having this day, as far as the unavoidable irregularity of tubes would permit us, shewn by several experiments that the density is also as the said force, the space it possesses being always reciprocally as that force; we are now furnished with sufficient *data* to make our enquiries concerning the limits of the atmosphere, and to determine its state, as to rarity, at different elevations from the earth's surface.

If the air were of the same consistence as to its rarity or density at all altitudes, it would be no difficult thing to set bounds to it. We collected from the experiment which was yesterday made at the top and bottom of the Observatory, that the specifick gravity of water is about 850 times greater than the specifick gravity of air (which thing will hereafter be further examined by an experiment particularly fitted for that purpose) and in the foregoing week we found by the hydrostatical ballance, that quicksilver is about 14 times heavier than water; it follows then of consequence that quicksilver is 14 times 850 degrees heavier than air, that is, 11900 times heavier. We have seen by the Torricellian experiment, that a column of quicksilver of $29\frac{1}{2}$ inches is usually a counterpoise to a column of air, having the same base and reaching to the top of the atmosphere; if therefore the air be every where of the same density as it is here below, its altitude ought as many times to exceed the height of $29\frac{1}{2}$ inches (which is the height of an equiponderant column of quicksilver) as its specifick gravity falls short of the specifick gravity of quicksilver; that is, the height of the atmosphere ought, upon the supposition of an every where uniform density, to be 11900 times $29\frac{1}{2}$ inches, or ſomewhat above $5\frac{1}{2}$ miles.

But it may be eaſily proved that this ſuppoſition does in no wiſe take place. For ſince every region of the air is compreſt by that part of the atmoſphere which is ſuperior to it, and ſince the higher parts have a leſſer weight incumbent upon them than the lower, and ſince the denſity of the air is every where as the force which compreſſes it; it will follow of neceſſity that there is ſtill a greater rarity of the air as it is further diſtant from the ſurface of the earth. How far the air may poſſibly admit of rarefaction and condenſation, has not yet, that I know of, been determined by any one. Mr. *Boyle* has obſerved that it may be ſo dilated as to become 10000 times rarer than it is in its natural ſtate. Dr. *Halley* ſays that he himſelf has ſeen air compreſſed ſo as to be 60 times denſer than it is as we commonly breath it; and Monſieur *Papin* relates that he was a witneſs that Monſieur *Huygens* did once in a glaſs veſſel compreſs air to the ſame degree before the glaſs was broken; yet never could any experimenter determine how much farther air might poſſibly be rarefied or condenſed. However it is certain there are in nature ſome limits which cannot be exceeded. No condenſation can reach ſo far as to cauſe a penetration of parts; and if the rarefaction of the air be ſtill greater, as its diſtance from the ſurface of the earth increaſeth, its ſpring will at length be ſo weakened, that the force with which every particle of it endeavours to tend upwards, from the particles which are next below it, will be weaker than the force of its own gravity which endeavours conſtantly to detain it. The rarefaction of the air muſt therefore be bounded where theſe two oppoſite forces come to ballance each other.

Though this be certainly true that the air cannot poſſibly expand itſelf beyond a certain meaſure upon account

count of its gravity, yet ſince men have not hitherto been able to ſet any bounds to its utmoſt expanſion, it is equally certain, that we cannot poſſibly define the limits of the atmoſphere. For as the air may be more and more rarefied, ſo will the ſame quantity of it, which equals the weight of about 30 inches of quickſilver, be contained in a greater ſpace, and thereby thoſe limits be ſo much the wider.

Notwithſtanding this ſeeming difficulty, we may ſtill collect how much the air is rarefied at any propoſed altitude from the ſurface of the earth, after the following manner.

In Fig. 39, let *xaαpx* repreſent a veſſel reaching from the ſurface of the earth *aα* to the top of the atmoſphere *x*; and let us imagine the ſide *ax* divided into inches *ab*, *bc*, *cd*, &c, and let the lines *bk*, *cl*, *dm*, *en*, &c, be drawn parallel to *aα*. It is evident that the air contained between *bk* and *cl*, is rarer than the air contained between *aα* and *bk*, the former having a leſſer column of air *xclx* incumbent upon it than the column *xbkx*, which preſſes upon the latter. Upon the ſame account the air between *cl* and *dm* is rarer than that between *bk* and *cl*, and that between *dm* and *en* rarer than that between *cl* and *dm*; and thus every ſuperior inch of air is rarer than that below it.

Let us now ſuppoſe that every inch of air is in all parts of it of an equal denſity, or that the air *ak* is every where uniform, but denſer than the air *bl*, which is alſo ſuppoſed to be every where uniform, but denſer than *cm*, and that to be uniform itſelf, but denſer than *dn*, and ſo onwards.

Again let us ſuppoſe that the air *bl* is reduced to a leſſer ſpace *bq*, ſo as to become equally denſe with the air *ak*, which is done by making the ſpace *bq* leſſer than *bl*, in the ſame proportion that the air *bl* is leſs denſe than the air *ak*; after the ſame manner let the air *cm* be reduced to the ſpace *cr*, and the air *dn* to the ſpace *ds*, and ſo onwards, that thus every inch of air may be reduced to the ſame conſiſtence with the air *ak*.

Now it is evident from this conſtruction, that the ſpaces *ak*, *bq*, *cr*, *ds*, &c, will every where be as the denſities reſpectively of the ſeveral inches of air, *ak*, *bl*, *cm*, *dn*; and it is alſo evident, that the quantity or weight of the air which reaches from any one of thoſe ſpaces up to the extremity of the atmoſphere, will every where be as the ſum of all the ſpaces which are ſituated above the ſpace propoſed. Thus the quantity or the weight of air above the ſpace *ak*, will be as the ſum of the ſpaces *bq*, *cr*, *ds*, *et*, *fv*, &c, and the quantity or weight of air above the ſpace *cr*, will be as the ſum of the ſpaces *ds*, *et*, *fv*, &c. For the air being every where reduced to the ſame conſiſtence, the quantity or weight of it will be as the ſpace it poſſeſſes.

Theſe things being laid down I may now without much difficulty proceed to eſtabliſh the concluſion I am at, which is this; That if any number of diſtances from the ſurface of the earth be taken in an arithmetical progreſſion, the denſities of the air at thoſe diſtances will be in a geometrical progreſſion.

For ſince by the experiments which have this day been made, it appears that the denſity of the air is always as the force which compreſſes it, we muſt conclude that the denſity of the air at any diſtance from the ſurface of the earth, is as the quantity or weight of that part of the atmoſphere which is above it. Therefore in our ſcheme, the denſities of the air between *aα* and *bk*, *bk* and *cl*, *cl* and *dm*, &c, are to each other reſpectively as the quantities of air above *aα*, *bk*, *cl*, &c, up to the extremity of the atmoſphere. But we ſaw before that thoſe denſities were as the ſpaces *ak*, *bq*, *cr*, &c, reſpectively, and thoſe quantities of air reaching to the extremity of the atmoſphere were as the ſpaces *xbβqrstvx*, *xcγrstvx*, *xdδstvx* reſpectively; it follows then that the ſpaces *ak*, *bq*, *cr* are to each other reſpectively as the ſpaces *xbβqrstvx*, *xcγrstvx*, *xdδstvx*.

Now the former ſpaces *ak*, *bq*, *cr* are the differences of the latter, and it is well known to thoſe who underſtand any thing of the nature of proportions, when any ſet of quantities are to each other reſpectively as their differences, that then as well the quantities themſelves, as their differences, are in a geometrical progreſſion *(a)*.

The ſpaces *ak*, *bq*, *cr*, are therefore in a geometrical progreſſion, as the diſtances *ab*, *ac*, *ad* are in an arithmetical progreſſion. And as the denſities of the air belonging to theſe three firſt inches, are in a geometrical progreſſion, ſo do the denſities of the air belonging to every one of the other inches, which are ſuppoſed to be continued up to the extremity of the atmoſphere, decreaſe in the ſame geometrical progreſſion, as any one without difficulty may collect by the ſame way of reaſoning.

I have hitherto ſuppoſed for eaſe of conception, that the air is of the ſame denſity in every part of each inch of altitude; nevertheleſs it is certain that every the leaſt variation of altitude cauſes a variation of denſity in the air. The concluſion however will not hereby be diſturbed; for if inſtead of dividing the altitude of the atmoſphere into inches as before, we conceive it now to be divided into its moſt indefinitely minute parts, applying to theſe what we have ſaid above concerning the inches, we ſhall at length deduce the ſame geometrical progreſſion of denſities anſwering to a like arithmetical progreſſion of altitudes.

(a) Suppoſe $a : a-b :: b : b-c :: c : c-d ::$ &c, then converſely we have $a : b :: b : c :: c : d ::$ &c.

Now becauſe the rarity of any body is reciprocally as its denſity, we may alſo conclude that, as the diſtances from the ſurface of the earth do increaſe in an arithmetical progreſſion, ſo do the different degrees of rarity of the air increaſe in a geometrical progreſſion.

This property of the air was firſt, that I know of, obſerved by Dr. *Halley*, but becauſe his demonſtration cannot be underſtood by thoſe who are unacquainted with the nature of the hyperbolick line, and Dr. *Gregory* in his demonſtration of the ſame thing, which may be ſeen in the fifth book of his Aſtronomy, ſuppoſes his reader to be furniſhed with ſo much geometry as not to be ignorant of the properties of the logarithmick line, I have endeavoured to make the thing intelligible by a method which may be eaſy even to thoſe who have never medled with curvilinear figures.

Let us ſee now what help we have from this property, to determine how much the air is really rarefied at any propoſed elevation from the ſurface of the earth.

Since the elevations are the terms of an arithmetical progreſſion as the rarities are the terms of a geometrical, it follows that the elevation is every where proportionable to the logarithm of the rarity. If then by experiment we can poſſibly find the rarity of the air at any one elevation, we may by the rule of proportion find what is the rarity at any other propoſed elevation: by ſaying, as the elevation at which the experiment was made, is to the elevation propoſed, ſo is the logarithm of the air's rarity which was obſerved at the elevation where the experiment was made, to the logarithm of the air's rarity at the elevation propoſed.

Thus I collected from the celebrated French experiment at the *Puy de Domme*, which I yeſterday gave you an account of, that at the altitude of 7 miles the air is somewhat above 4 times rarer than at the surface of the earth. By the same method I collected from the experiment of Mr. *Caswell*, made upon Snowden Hill, that at the same altitude of 7 miles the air is not altogether so much as 4 times rarer than at the surface; the difference on both sides was inconsiderable. We may take a mean therefore and say in a round number, that at the altitude of 7 miles the air is about 4 times rarer than at the surface of the earth.

Sir *Isaac Newton* in his late additions to his Opticks, makes use of this very proportion (*b*), what grounds he went upon is difficult to guess, however I am satisfied of the conclusion from my own computation. Now from what has been already proved, that the rarity of the air is augmented in a geometrical, as the altitude is augmented in an arithmetical progression, it follows that every seven miles added to the altitude, does always require a rarity of the air still 4 times greater. Therefore at the altitude of 14 miles the air is 16 times rarer than at the surface, at the altitude of 21 miles it is 64 times rarer, at the altitude of 28 miles 256 times, at 35 miles 1024 times, at 70 miles about a million of times, at 140 miles a million of million of times, at 210 miles a million of million of millions of times, if the air can possibly expand itself to so large dimensions.

Hence we may easily gather that the air at the altitude of 500 miles, if the atmosphere can reach so far, must necessarily be there so much rarefied, that if a globe of the air we breath in, of an inch diameter, were as much dilated, it would possess a larger space than the whole sphere of Saturn. The semidiameter of the earth is nearly 4000 miles, which is 8 times 5

(*b*) In the last edition he makes it 4 times rarer at the height of $7\frac{1}{2}$ miles, and 16 times rarer at 15 miles, and so on, but gives no reason for this alteration.

hundred miles; with good reaſon then might that excellent philoſopher I have lately been mentioning, tell us in his *Principia*, that the air at the altitude of a ſemidiameter of the earth, is at leaſt ſo wonderfully rarefied as I have decribed it to be at an altitude 8 times leſs.

It appears from the obſervations of aſtronomers of the duration of twilight, and of the magnitude of the terreſtrial ſhadow in lunar eclipſes, that the effect of the atmoſphere to reflect and intercept the light of the ſun, is ſenſible even to the altitude of between 40 and 50 miles. So far then we may be certain that the atmoſphere reaches, and at that altitude we may collect, from what has been already ſaid, that the air is about 10000 times rarer than at the ſurface of the earth. How much farther than this altitude of between 40 and 50 miles the atmoſphere may be extended, I muſt confeſs I am altogether ignorant, there being no *data*, that I know of, from which a greater altitude may indubitably be concluded.

There has indeed been often ſeen in the atmoſphere ſome very luminous parts, even near the zenith about midnight, but I dare not conclude any thing from ſuch appearances. If I ſhould aſſert, as ſome have done, that theſe luminous parts are nothing elſe but ſome terreſtrial exhalations floating in the air at a prodigious altitude, and thereby reflecting the light of the ſun, which they are expoſed to at that great height, to our eyes, it will be next to impoſſible to give any tolerable account, how thoſe exhalations can be denſe enough to reflect ſo copious a light at that vaſt diſtance, and at the ſame time be ſupported by a medium, I may ſay, almoſt infinitely rarer than the air we breath in. It ſeems more probable that theſe extraordinary lights proceed from ſome ſelf-ſhining ſubſtance or aerial phoſphorus.

A ſurpriſing appearance of this kind was ſeen here at Cambridge about 10 of clock at night, and at other very diſtant places, on the 20th of march in the year 1706. It was a ſemicircle of light, of about two thirds of the ordinary breadth of the milky way, but much brighter. The top of it paſſed very near our zenith inclining about 4 or 5 degrees to the north; it croſſed the horizon at a very ſmall diſtance from the weſt towards the ſouth, and again about as far from the eaſt towards the north. It was moſt vivid and beſt defined about the weſtern horizon, and moſt faint about the zenith, where it firſt began to diſappear: there was at the ſame time an *Aurora borealis.* A friend of mine ſaw the ſame appearance in Lincolnſhire, at the diſtance of about 70 miles north of Cambridge; the ſemicircle ſeemed to him to lie in the plane of the æquator. From theſe two obſervations compared together it is eaſy to collect, that the matter from which that light proceeded, was elevated above the earth's ſurface between 40 and 50 miles.

Having now finiſhed what I deſign'd to repreſent concerning the limits and different degrees of rarity of the atmoſphere at different altitudes, I might here conclude; but becauſe it may poſſibly be expected I ſhould add ſomething in this place concerning the cauſe of the air's elaſticity, upon which theſe deductions were grounded, it may not be amiſs to declare here, that of all the ſeveral hypotheſes which have hitherto been ſuggeſted for this purpoſe, that of Sir *Iſaac Newton* ſeems to me to be the moſt probable. He has demonſtrated in the ſecond book of his *Principia*, that if the particles of the air be of ſuch a nature as to recede from each other with centrifugal forces reciprocally proportionable to their diſtances, they will compoſe an elaſtical fluid whoſe denſity will always be as the force which compreſſes it; and any one who

who reads the late additions to his Opticks will perceive that that hypothesis is not advanced without reason.

LECTURE X.

The effects of the weight and spring of the air in syringes, pumps, syphons, polished plates, cupping-glasses, suction, respiration, &c.

I HAVE hitherto been proving that the air has weight, and consequently presses upon all bodies to which it is contiguous. We have found that at the surface of the earth, the pressure of any column of air is equivalent to the weight of a column of quicksilver having the same basis and its altitude of about $29\frac{1}{2}$ inches; or to the weight of a column of water, having the same basis and its altitude about 34 feet; that the pressure is lessened always as the elevation from the surface of the earth becomes greater; that the air has also an elastical power, by which it endeavours as far as is possible to expand itself; that this elastical power of the air is equal to the force which compresses it; that the space it possesses is always reciprocally as that force, and consequently its density directly as the same; that the degrees of density of the atmosphere are different at different altitudes, the air being still rarer as the altitude is greater; that the rarity of the air increases in a geometrical progression as the altitudes increase in an arithmetical one, the air at every 7 miles of height being always 4 times more rare than before.

Let us now come to those effects of the pressure of this subtle fluid, whether caused by its weight or spring, which were formerly thought to proceed from that abhorrence which they say nature ever has of a *vacuum*. Amongst these we may reckon the phænomena of syringes, pumps, syphons, polished plates, cupping-

cupping glaſſes, ſuction, reſpiration and others of the like nature. Mr. *Paſcal* in his little French treatiſe concerning the gravity of the air, has given us a very good account of theſe things. I ſhall therefore for the moſt part make uſe of his explications, ſince it would be needleſs to go about to make new ones. His method is this, he firſt recites the principal effects which were wont to be aſcribed to a *fuga vacui*, and afterwards ſhews that they proceed from the preſſure of the air.

Firſt then a pair of bellows whoſe vents are all well cloſed up, are difficult to be opened; as we attempt to do it we perceive a reſiſtance as if the ſides were glued together. After the ſame manner the ſucker of a ſyringe, which is ſtopped at the bottom, reſiſts the force we apply to draw it out, as if it were ſome way faſtened to the bottom. It is pretended that this reſiſtance proceeds from the abhorrence which nature has of a *vacuum*, which would happen in both caſes, if the ſides of the bellows were disjoined, or the ſucker of the ſyringe drawn out. That opinion is confirmed by this, that the reſiſtance ceaſes as ſoon as the air is permitted to enter.

Secondly two poliſhed bodies applied together are difficult to be ſeparated and ſeem to adhere to each other. It is pretended that this adherence proceeds from the like abhorrence of a *vacuum*, which would happen during the time which the air would take up in coming from the edges to the middle.

Thirdly when the pipe of a ſyringe is immerſed in a veſſel of water, if you draw up the ſucker the water will follow it and aſcend as if it did adhere to it. Thus in a pump which is a longer ſyringe, the water aſcends and follows the ſucker, being raiſed up in the ſame manner. It is pretended that this elevation of water proceeds from the endeavours of nature againſt a *vacuum*, which would happen in the ſpace

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deserted by the sucker, if the water should not ascend, since the air is excluded; which is confirmed by this, that the water will no longer ascend if the engine has any leaks so as to admit the air to come in.

After the same manner if you place the nose of a pair of bellows under water, and open it suddenly, the water will ascend to fill it, because the air cannot, and the experiment will succeed the better if the bellows be entirely closed up. Thus placing your mouth under water and sucking, you may attract the water for the same reason; for the lungs may be compared to a pair of bellows. Thus in respiration we draw in the air, just as a pair of bellows by being opened attracts the air to fill up its cavity. Thus if you place a lighted piece of paper in a glass, and suddenly invert it into a vessel of water, as the flame decreases so will you see the water ascend into the glass; for the air in the glass being rarefied by the flame, when it afterwards comes to be condensed by the cold water, upon contracting its dimensions it will draw up with it some of the water to fill the space it has deserted. Thus do cupping glasses draw the flesh and cause a swelling; for the air in the cupping glass being rarefied by heat, when it comes again to be condensed after the flame is extinguished, it draws in the flesh to fill up the space it has deserted, as before it drew in the water.

Fourthly if you fill a bottle with water, and invert the neck of it into a vessel filled with other water, the water will remain suspended in the bottle without falling out. It is pretended that this suspension proceeds from a *fuga vacui*; for there would necessarily be left a void space if the water should descend, since the air cannot come in to fill it up; which they confirm by this observation, that if the air be suffered to enter by some hole the water will immediately fall down.

Fifthly if syphon be filled with water and its legs be immerſed into two different veſſels of water, it will come to paſs, if one of the veſſels be higher than the other, that the water contained in the higher will aſcend to the top of the ſyphon, and then deſcend into the lower veſſel, ſo that if you continually ſupply the higher veſſel with water, the flux will be perpetual. It is pretended that this elevation of water is to be aſcribed to the endeavours of nature to hinder a *vacuum*, which would happen within the ſyphon, if the water contained in thoſe two leſs ſhould deſcend each into its veſſel below; which actually comes to paſs when the air can come in at the top of the ſyphon through ſome hole.

Many other effects there are of the like nature, which have been omitted as being nearly the ſame with thoſe already deſcribed; in all of them there appears nothing more than this, that all contiguous bodies reſiſt any effort made to ſeparate them, when the air cannot ſucceed them; whether that effort be their own proper weight, as in the examples where water aſcends and remains ſuſpended notwithſtanding its weight; or whether it proceed from ſome force applied to diſunite them, as in the firſt example. Such effects as theſe have commonly been aſcribed to a *fuga vacui*, let us now ſee how they depend upon the preſſure of the air.

To explain how the preſſure of the air is the cauſe of that difficulty we perceive in opening a pair of bellows, whilſt the air has no ingreſs, Mr. *Paſcal* puts his reader in mind of what he had before been diſcourſing of, in his other treatiſe, concerning the *equilibrium* of liquors, that if a pair of bellows whoſe pipe is 20 feet long or more be placed in a deep veſſel filled with water, ſo that the end of the pipe be above the ſurface of the water, it will be very difficult to be opened, and by ſo much the more as the altitude of the water above the ſides of the bellows is the greater; which proceeds manifeſtly from the weight of the ſuperior water. For before any water be poured into the veſſel, there is no difficulty in opening the bellows, but as more and more water is poured in, the reſiſtance is continually augmented, and is always equivalent to the weight of the water which is ſupported. For as no water can enter into the cavity of the bellows, the orifice of the pipe being above the ſurface, it is evident that the ſides cannot be disjoined without raiſing and ſuſtaining the ſuperior maſs of water. Now no body can here ſay that this reſiſtance proceeds from a *fuga vacui*, ſince the air has a free paſſage into the cavity by the orifice of the pipe which is above the water; it is abſolutely certain therefore, that it depends intirely upon the weight of the water.

What has been here ſaid as to the weight of the water, may be applied to any other fluid; for if the bellows be placed in a veſſel filled with wine, there will be the like reſiſtance, and the ſame may be ſaid as to milk, oyl, quickſilver, or any other fluid whatever. It is then a general rule and a neceſſary effect of the gravitation of fluids, that if a pair of bellows be placed in any fluid whatever, ſo that the fluid have no acceſs to the cavity of the bellows, the weight of the ſuperior parts of the fluid will cauſe a reſiſtance to the opening of the bellows. If therefore we apply the general rule to the air in particular, we may ſay that when a pair of bellows is ſo ſtopt as to leave no ingreſs to the air, the weight of the ſuperior maſs of air will cauſe a reſiſtance in opening the bellows, which reſiſtance will ceaſe as ſoon as the air is permitted to enter.

What has been ſaid as to this effect will hold good as to others, in which I may be the more ſuccinct, having enlarged already ſo much upon this. It has been already ſhewn in the former week that the preſ-

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ſure of any fluid may produce effects analogous to thoſe of the ſyringe, pump, ſyphon and poliſhed plates, and the application was at the ſame time made to the air; I need not here therefore inſiſt upon them any longer. That would alſo have been the proper place for this experiment of the bellows under water, could it as eaſily have been made as deſcribed. However the deſcription of it may ſerve at leaſt to illuſtrate the concluſion upon whoſe account it was propoſed by its ingenious author. For it cannot but be evident to any one who is at all acquainted with hydroſtaticks, that the event muſt needs anſwer the deſcription that has been given of it.

The ſame thing alſo may be ſaid of the following experiment or inſtance propoſed by the ſame perſon to illuſtrate the effect of cupping glaſſes. He ſuppoſes that a tube of about 20 feet in length open at both ends, has one end which enlarges itſelf like the mouth of a funnel, applied to a man's thigh at a conſiderable depth under water, ſo as to hinder the water from preſſing upon that part alone of the thigh which is included within the orifice of the tube, to which the air nevertheleſs has a free acceſs by the other end of the tube which is above the ſurface of the water. In this caſe, ſays he, it will come to paſs, that the part included within the orifice of the upper tube, will be conſiderably ſwelled out as if ſomething ſucked it in that place. Now it is plain that this ſwelling can by no means be ſaid to proceed from a *fuga vacui*, ſince the tube is open to the air, and no ſuch thing would happen if there were not any, or but very little, water to preſs upon the reſt of the body. It is therefore moſt certain that this effect depends purely upon the gravitation of the water; for whilſt it preſſes upon all other parts of the body excepting that alone which is covered by the tube, to which it has no acceſs, it forces the blood and other yielding parts to ariſe where

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there is not ſo great a preſſure, and thereby cauſes the ſwelling.

What has been ſaid as to the preſſure of water will hold true as to the preſſure of any other fluid; and therefore the preſſure of the air may cauſe a like ſwelling, if it be greater upon the other parts of the body than upon that to which the cupping glaſs is applied, as it certainly is. For the air within the cupping-glaſs being very much rarefied, and conſequently in part expelled by the heat, when it comes again to its uſual temper, its ſpring will be very much debilitated, and therefore it will preſs leſs forcibly againſt the part of the body under the glaſs, than the external air upon the other parts of the body.

I need not now uſe many words to explain how it comes to paſs, when any one places his mouth under water and ſucks, that the water aſcends; for it is clear that the external air preſſes upon every part of the ſurface of the water, excepting that which is covered by the mouth; and hence it happens, that when the muſcles ſerving for reſpiration, elevate the ribs and enlarge the capacity of the cheſt, the air within having a greater ſpace to fill than before, hath leſs force to hinder the entrance of the water into the mouth, than the external air has to promote that entrance. This alſo is the cauſe of the attraction or ſuction of any liquor by a tube, and it differs very little from the effect of the ſyringe. Thus a ſucking child at the breaſt of its nurſe, draws in its milk; the external air preſſing the breaſts of the nurſe on all parts excepting that which is covered by the child's mouth. Upon the ſame account in reſpiration the air enters into the lungs; for as the cheſt is dilated, ſo is the external air forced in by the weight of the ſuperior part of the atmoſphere; which is ſo intelligible, ſo eaſy and natural, that one would wonder that philoſophers ſhould ever have had recourſe to a *fuga vacui*,

cui, to occult qualities, to causes so foreign and chimerical.

Thus may all the other effects which were once ascribed to a *fuga vacui*, be shewed to depend upon the pressure of the air, as cannot but be evident to those who understand the principles of hydrostaticks, and are satisfied that the air is a gravitating fluid; which thing I hope I have already proved, and shall hereafter further confirm. But as the weight of the air is not infinite, but limited by certain bounds, so are the effects depending thereupon also limited. Thus water cannot by a pump be raised to any proposed altitude. We know that a column of water of about 34 feet in the altitude, is commonly a counterpoise to the pressure of the atmosphere; that therefore is the utmost height to which the air in its mean state of gravity can elevate the water in a pump. If the air happens to be more than ordinarily heavy, the water will ascend something higher, but scarce ever more than 36 feet. If the air be more than ordinarily light, the water will not come up 34 feet, nevertheless the air is seldom so light as not to be able to bear up water so far as 32 feet.

If the operation of the pump did depend upon the *fuga vacui*, as it was commonly believed before *Galilæo*'s time, then it would follow that water might be raised to any altitude how great soever; for why should not nature have as great an aversion against a *vacuum* in one case as in another? And accordingly several who embraced that notion, have very confidently asserted, though they never made the experiment, that it might be raised *ad libitum*. But *Galilæo* observing that there was a certain standard altitude, beyond which no water could be elevated by pumping, took an occasion from thence to call in question the doctrine of the schools concerning the *fuga*, which began from that time to be very much suspected, and in the room thereof he happily ſubſtituted the hypotheſis of the air's preſſure and gravitation. It was to him indeed little better than an hypotheſis, ſince it had not then thoſe confirmations from experiments which were afterwards found out by his ſcholar *Torricellius* and other ſucceeding philoſophers, particularly our excellent Mr. *Boyle*.

What has been ſaid of pumps may be alſo applied to ſyphons. It was formerly looked upon as unqueſtionable, that water might be conveyed over the higheſt mountains by the help of this inſtrument, if the place into which it was to be diſcharged, were but lower than the place from whence it was derived. We are now certain of the contrary by experiments made more than once: 34 feet is commonly the utmoſt height to which water can riſe as well in ſyphons as in pumps. In quickſilver the utmoſt altitude is leſs, being commonly about 29 ½ inches, 29 ½ inches of quickſilver and 34 feet of water being a counterpoiſe to the preſſure of the atmoſphere, upon which thoſe effects depend.

I will add but one inſtance more concerning poliſhed plates; as their coheſion depends upon the limited preſſure of the air, ſo is the force requiſite for their ſeparation alſo limited, and may be thus computed. Since the force requiſite for their ſeparation muſt be equal at leaſt to the force which cauſes their coheſion, that is, to the preſſure of the air, and the preſſure of air upon any baſis is equal to the weight of a column of quickſilver having the ſame baſis and the altitude of about 29 ½ inches, it follows that the force requiſite to ſeparate the plates, ought to be equal at leaſt to the weight of a cylinder of 29½ inches altitude, having the area of the plates for its baſis. By calculating upon theſe grounds, I find that the force requiſite to ſeparate our larger marble plates, is equal to about one hundred weight and ¼; and the force requiſite

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ſite to ſeparate the leſſer braſs plates amounts nearly to 73 pounds weight; and this upon ſuppoſition that they are perfectly well poliſhed and ſo fitted together that no air can intervene. But as they want of that perfection, ſo will a leſſer force be ſufficient to disjoin them.

LECTURE XI.

The phænomena of capillary tubes, glaſs planes, the figures of the ſurfaces of fluids and other things relating to the ſame head, conſidered.

WE are now upon a ſubject abounding with difficulties, which has in vain been attempted by ſeveral modern philoſophers. Many hypotheſes they have invented to account for theſe odd appearances, which if thoroughly examined will be found to be but bare hypotheſes, and in many particulars inſufficient. It has been generally believed, that the unequal preſſure of the air upon the liquor contained in the tube and that in the veſſel, is the cauſe of the aſcent in the tube. For if the preſſure upon the liquor in the tube be leſs than that upon the veſſel, the liquor ought to aſcend ſo far in the tube, that its own weight together with the weaker effort of the air incumbent upon it, be equal to the free and unreſtrained gravitation of the atmoſphere upon the veſſel.

Though they have generally agreed in this, that there is a leſſer preſſure of air upon the liquor in the tube than upon that contained in the veſſel, yet the cauſes they have aſſigned for that inequality are very different. Some have had recourſe to the magnitude of the particles of the air and of the aſcending fluid. Others have believed, that only an inverted cone of air, touching the ſurface of the liquor in the tube with its vertex, and having the upper orifice of the tube for its basis, could press upon the surface contiguous to its vertex. Dr. *Hook* supposes part of the pressure of the air in the tube, to be taken off by its friction, which he supposes must necessarily happen against the sides in so narrow a passage. Other conceits there are which I have omitted, being more concerned to find out, if I can, what is the truth, then to enumerate the groundless fancies of those who seem to me to have missed of it.

Dr. *Hook*'s hypothesis has indeed the fairest shew of probability, and accordingly it has been received with great applause. It may therefore be worth while to give an account of it, and to examine afterwards whether it be such as we may acquiesce in. That there is an inequality of pressure he endeavours to make out from hence, that there is a much greater incongruity of air to glass and some other bodies than there is of water to the same. By congruity he means a property of the fluid body, whereby any part of it is readily united with any other part, either of itsself or of any similar fluid or solid body; and by incongruity, a property by which bodies are hindered from uniting with any dissimilar body. Thus, not to mention several chymical spirits and oyls, which will very hardly, if at all, be brought to mix with one another, if we observe the drops of rain falling through the air, and the bubbles of air which are by any means conveyed under water, or a drop of sallad-oyl swimming upon the water, we cannot be to seek for instances of the incongruity of fluids amongst one another. And as for the congruity or incongruity of liquids with several kinds of firm bodies, they have long since been taken notice of and called by the names of dryness and moisture; though these two names are not comprehensive enough, being commonly used to signify only the adhering or not adhering of water to solid bodies. Thus we may observe that water will

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more readily wet ſome woods than others, that water let fall upon a feather, the whiter ſide of a colewort and ſome other leaves, or almoſt upon any duſty, unctuous or reſinous ſurface, will not adhere but eaſily tumble off from them, like a ſolid bowl; whereas if dropt upon linnen, paper, clay, green wood, &c. it will not go off without leaving ſome part of itſelf behind. So quickſilver, which will very hardly be brought to ſtick to any vegetable, will readily adhere to and mingle with ſeveral clean metalline bodies.

The cauſe which he propoſes of this congruity and incongruity of bodies is, that all fluids are in a ſort of vibrative motion, which he ſays is a ſort of pulſe or ſhake of heat, by which the parts of bodies being made looſe from one another, can eaſily move any way and are thereupon fluid. If in a large diſh ſeveral kinds of ſands be mixed together, we ſhall find that by any vehement agitation, the fine ſand will eject and throw out of itsſelf all the bigger bulks of ſmall ſtones and the like, which will be gathered together into one place; and if there be other bodies in it of other natures, they alſo will be ſeparated into a place by themſelves. In like manner he ſuppoſes the pulſe of heat to agitate the ſmall particles of matter, and thoſe which are of the ſame bigneſs, figure and texture will hold or dance together, and thoſe which are of a different kind will be thruſt out from between them by ſeveral variations of harmony and diſcord. And what has been ſaid as to fluids he ſuppoſes may be attributed to ſolid bodies, to which alſo he applies the like vibrative motion. This is his explication of congruity and incongruity.

If then we allow, as we eaſily may, that water is more congruous to glaſs than air is, it will follow that water may more eaſily be forced through the narrow paſſage of a ſlender pipe than air. He illuſtrates the thing by the reſemblance of a round ſpring, ſuch as an hoop: for as in a round ſpring there is required an additional preſſure againſt the two oppoſite ſides to reduce it into an oval form, or to force it in between the ſides of an hole whoſe diameter is leſs than that of the ſpring, ſo to alter the ſpherical conſtitution of the air included in the tube, ariſing from its incongruity to glaſs, there is required more preſſure againſt the oppoſite ſides to reduce it to an oval; and to preſs it into a hole leſs in diameter than itsſelf, it requires a greater protruſion againſt all the other ſides, which he found alſo to be true by experiments. Therefore he concluded, that part of the preſſure of the atmoſphere being taken off and ſpent in protruding the air within the cavity of the ſlender tube, it has leſs force to reſiſt the aſcent of the water, which is impelled upwards by the whole force of the atmoſphere, preſſing upon the ſurface of the water in the veſſel into which the lower end of the tube is immerſed; and as a greater part of the preſſure of that column of the atmoſphere, which is incumbent over the upper orifice of the tube, is taken of in protruding the air within the tube when it is ſlenderer, ſo is its reſiſtance to the aſcending liquor ſtill leſs, and conſequently the aſcent is greater.

This is Dr. *Hook*'s account of the matter, which any one may more fully acquaint himſelf with, by reading the ſixth obſervation of his admirable Micrography. It appears at firſt view to be very ſatisfactory, and accordingly it has not wanted its patrons; nevertheleſs I muſt here confeſs that I cannot by any means perſuade myſelf to be of the ſame opinion. Amongſt many others, this is one great reaſon of my backwardneſs to embrace Dr. *Hook*'s hypotheſis, that I have found by making the experiment, and ſeveral others have took notice of the ſame thing before me, that even under a receiver exhauſted of air by

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the air-pump, there is as far as we can perceive a like and equal aſcent of liquors in capillary tubes. Now if the difference of the preſſure of the air upon the liquor contained within the tube and upon that within the veſſel, into which the tube is immerſed, be the true cauſe of the aſcent, we muſt needs be at a loſs in explaining how the ſame aſcent can poſſibly happen in *vacuo*, when there is no air to preſs either upon the liquor in the veſſel or that in the tube.

It may perhaps be anſwered, that we can never by pumping perfectly evacuate the receiver of air, ſo that after all our endeavours there will ſtill a ſufficient quantity remain behind to produce the effect: let it then be conſidered whether that very ſmall quantity which remains behind, can poſſibly be ſaid to be ſufficient. It is reaſonable to conclude, that as the quantity of air under the receiver is diminiſhed, ſo are the preſſures of the air upon the liquor in the veſſel and upon that within the tube proportionably diminiſhed, and conſequently the difference of thoſe preſſures alſo. If then that difference be the cauſe, as is pretended, of the aſcent of the liquor in the tube, as that difference is diminiſhed by pumping, ſo ought the aſcent in like manner to be diminiſhed, which does not happen; therefore that difference is not the cauſe of the aſcent. If it be ſaid, contrary to all reaſon, that the difference of thoſe two preſſures is not diminiſhed as the receiver is evacuated, I might eaſily prove, were it neceſſary, that even the whole preſſure of the air in the receiver upon the liquor in the veſſel, is not able to bear up the liquor in the tube to the height it will aſcend to, if the bore be narrow, when the pump is ſufficiently worked: much leſs then can the exceſs of that whole preſſure upon the liquor in the veſſel above the preſſure upon the liquor within the tube, be ſaid to do ſo, unleſs the part can be proved to be greater than the whole. We have therefore good grounds from the abovementioned event of the experiment made in the evacuated receiver, of which you will hereafter be eye-witneſſes, to diſtruſt that cauſe ariſing from the inequality of the air's preſſure.

It will follow therefore, that there is no ſuch inequality of preſſure as is pretended; for if there were, then would the aſcent in the open free air be greater than in *vacuo*, the inequality of preſſure co-operating in the open air with the cauſe of the aſcent in *vacuo*, whatever it be.

But it may not be amiſs in this place to ſhew a *priori* alſo, that there is not that inequality of preſſure, notwithſtanding that there be required a conſiderable force to protrude the air contained in the tube. The preſſure of the incumbent part of the atmoſphere endeavours to force the included air downwards, and the preſſure of the elevated liquor does at the ſame time endeavour to force it upwards; now theſe two forces are in *equilibrio*, otherwiſe the included air would be more protruded downwards or more upwards, till that *equilibrium* were gained. We muſt therefore neceſſarily conclude that the preſſure of the elevated liquor upwards, is equivalent to the whole weight of the incumbent part of the atmoſphere. Therefore it is preſſed downwards, by the air which is contiguous to it, with a force equivalent to the weight of the incumbent part of the atmoſphere; for were it kept down by a leſs force than that with which it endeavours to aſcend, it would aſcend further. Every equal part of the ſurface of the liquor in the veſſel below, is alſo preſſed downwards by the ſame whole weight of the incumbent part of the atmoſphere. The liquor contained in the tube is therefore expoſed to an equal preſſure with that which is contained in the veſſel; that fancied inequality cannot therefore take place. I have proved both a *poſteriori* and alſo a *priori*, that the inequality of the air's preſſure is not the cauſe of the aſcent of liquors in capillary tubes.

I may add further that the difficulty of protruding the air through the narrow paſſage of the tube, is ſo far from being the cauſe of the liquor's aſcent, that it would rather hinder that effect than promote it. Dr. *Hook* indeed from thence might with ſome probability have given an account why an elevated liquor ſhould not be depreſſed, but the ſpontaneous aſcent of liquors does manifeſtly contradict his opinion, ſince the difficulty the air has to paſs upwards along the tube, when urged by the aſcending liquor, ought more to reſiſt its aſcent than the free and unreſtrained preſſure of the atmoſphere in a tube much larger.

Whether his hypotheſis concerning the congruity and incongruity of bodies, be ſuch as we may ſecurely admit of, I will not ſtay here to examine. I confeſs I ſee not any neceſſity of ſuppoſing the particles of fluids or firm bodies to be perpetually either in a vibrative or any other motion. It may ſo happen, that by accidental motions of the air or other contiguous bodies, the parts of fluids may ſeldom be at reſt; but that a perpetual inteſtine motion is eſſential to fluids, is what has not yet, that I know of, been demonſtrated, though ſome have attempted it.

I have hitherto been only proving what is not the cauſe of the effects we have been conſidering. Let us now try whether we can find out what is the true cauſe. It is a common obſervation, that a drop of water of a certain determinate magnitude, will firmly adhere to the ſurface of glaſs and of other bodies, and even hang pendulous to it, though the ſurface be placed downwards, notwithſtanding the weight of the drop which endeavours to disjoin it. Since then that endeavour is rendered ſome way or other ineffectual, we need not ſcruple to aſcribe a mutual attraction to the glaſs and water: it is evident that congruity alone, in Dr. *Hook*'s ſenſe, is not ſufficient to

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hinder the drop from falling by the force of its own weight; there is something more required than a bare congruity to overcome that force.

What I call attraction, any one if he thinks fit may give another name to; I mean no more by attraction than some power in nature, from what causes soever it proceeds, by which bodies do endeavour to be united to one another. This adhesion of the drop to the surface of the glass and other bodies, has commonly been ascribed to the pressure of the air. We have seen that this pressure is able to keep well polished plates united, notwithstanding a considerable force was used to separate them; but then the air must be excluded from between the surfaces. Now a drop or globule of water will be attracted to a plate of glass, which touches its upper part, notwithstanding the intervention of the air immediately before the attraction; which shews that the case is different from that of polished plates. We may also satisfy ourselves that this adhesion does not proceed from the pressure of the air, by the air-pump, since the same will happen under an exhausted receiver.

Certain it is, whatever be the cause of it, which I pretend not to determine, that there is such an attraction between water and other liquors to glass and several other bodies. And as there are attractions between several particular bodies, so we may observe others mutually to repel each other. Thus do the particles of air seem to fly asunder with forces reciprocally proportionable to their distances, and thereby compose an elastical fluid, whose density is as the force which compresses it. After the same manner it is very probable that air endeavours to recede from several dense bodies. Thus also do several other, as well solid as fluid bodies, seem to repel each other, and this is what Dr. *Hook* took notice of in many bodies which he therefore calls incongruous. Instances e-

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nough of this kind may be ſeen in the laſt query at the end of the laſt edition of Sir *Iſaac Newton*'s Opticks, which I forbear to tranſcribe. Whoever will read thoſe few pages of that excellent book, may find there in my opinion, more ſolid foundations for the advancement of natural philoſophy, than in all the volumes that have hitherto been publiſhed upon that ſubject.

But, to proceed with our drop of water, we may obſerve that the force of the attraction I have been ſpeaking of, is of a certain determinate quantity. If the drop be too big, it will fall off, the force of its own weight being greater to ſeparate it, than the force of attraction to hinder that ſeparation. As the part of the ſurface of the glaſs, to which the drop is contiguous, is larger, ſo will it bear up a greater drop; a larger ſurface having proportionably a greater attraction.

It is eaſy to apply what has been ſaid concerning a plane ſurface, to the inner concave ſurface of a narrow cylindrical tube. It cannot but be evident, that this ought, as well as the other, to attract and hold up a certain weight of water within the tube. The attracting ſurface does in this caſe every way ſurround the drop, and by that means has a much greater advantage to bear it up, than if it were a plane, and could thereby touch it only in one of its ſides. Now as the diameter of the tube becomes leſſer, ſo is this advantage ſtill increaſed; for it is well known that the ſurfaces of cylinders bear a greater proportion to their capacities, as their diameters are more and more diminiſhed, the ſurfaces decreaſing only in the ſame proportion with the diameters, and their capacities decreaſing in a proportion which is duplicate of that of the diameters. Hence it comes to paſs that the water can be held up at a greater altitude as the tube is narrower, which is the thing we were chiefly concerned to account for.

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It is plain from theſe principles, that the event ought to be the ſame in *vacuo* and in the open air; that the ſame quantity of liquor ought to be ſuſpended in the tube when taken out of the veſſel, as was before elevated above the ſurface of the liquor contained in the veſſel; that, if when the tube is taken out of the veſſel, a piece of glaſs be applied to its under orifice, ſo as to touch the ſuſpended liquor, by attraction it will cauſe it to deſcend out of the tube. The experiments of capillary ſyphons are explicable upon the ſame grounds. The liquor aſcends to the top of the flexure by the attraction I have been ſpeaking of, and then by its own weight it deſcends along the other leg. It would be tedious to inſtance in more particulars; the application of what has been ſaid to other caſes, is ſo eaſy that no body can miſs of it.

The reaſon of the different figures of the ſurfaces of fluids, is very obvious and depends upon the ſame principles. Water forms itsſelf into a concave, the ſuperficial parts of it, which are near the ſides of the tube, being attracted upwards to the glaſs. Quickſilver forms itsſelf into a convex, being repelled from the glaſs (*a*), which repulſion is alſo the cauſe why it does not, as water, aſcend above the level in capillary glaſs-tubes, but on the contrary remains below it. If two liquors be placed in the ſame tube contiguous to each other, and both be equally attracted to the ſides of the tube, their common ſurface will be a plane. If one be ſomewhat more attracted than the other, that which is moſt attracted will have a concave ſurface, and the other which is leſs attracted,

(*a*) This repulſion is not real but only apparent and relative. For Dr. *Jurin* has plainly ſhewn, that glaſs attracts the particles of quickſilver, but not ſo ſtrongly as they attract one another; and upon this principle has clearly explained the phænomena of quickſilver in capillary tubes and between glaſs planes. Phil. Tranſ. N°. 363.

muſt of conſequence have a convex, the ſurfaces of the two liquors being contiguous; and thus a liquor whoſe ſurface is concave when expoſed to the air, may have that ſurface changed into a convex, by the contact of another liquor which is more powerfully attracted to the ſides of the containing veſſel than its-ſelf.

It is not difficult to underſtand that the experiments we made concerning the motions of floating bodies are deducible from the like cauſes. If any one would be more particularly informed about this matter, he may conſult Mr. *Marriotte*'s Traité du movements des eaux, or the fourth volume of *Du Hamel*'s Burgundian philoſophy.

From what has been ſaid concerning the aſcent of liquors in capillary tubes, we may eaſily underſtand how filtrations of all ſorts are performed. If a tube be filled with ſand or ſifted aſhes well preſſed together, and one end of it be placed in a veſſel of water, the water will be attracted by the ſand or aſhes, and riſe to a great height above the level of that within the veſſel. Thus if any part of a piece of cap-paper, or a ſpunge, or a piece of bread or ſugar, or of linnen, or of ſeveral other ſubſtances, be wetted, the moiſture will be propagated to the other parts by the power of attraction.

This is the cauſe of the aſcent of ſpirit of wine, oyl, melted tallow and other unctuous bodies, into the wick of a lamp or candle. It is very reaſonable to believe that this is alſo the cauſe of the aſcent of the ſap in trees, and of the various ſecretions of fluids through the glands of animals, and of ſeveral other effects in nature, which any thinking perſon cannot miſs of.

LECTURE

LECTURE XII.

The air-pump and inſtruments for condenſing and transferring air; their fabrick, operation and gages explained.

Having in the firſt week of this courſe by ſeveral deductions and concluſions from experiments, which we thought to be the moſt pertinent, endeavoured to eſtabliſh the true and genuine principles of hydroſtaticks, and to demonſtrate the moſt fundamental properties of fluid bodies in general, we proceeded in the ſecond week to a more particular conſideration of the air; a fluid contrived by the wiſe Author of nature for ſo many various, admirable and excellent ends and purpoſes, and manifeſtly fitted to have ſo univerſal and uſeful an influence upon the whole ſyſtem of bodies we are particularly concerned with, that it very much deſerves our utmoſt diligence and moſt careful examination. It has been already proved, I think beyond any reaſonable contradiction, that this ſubtle element is by no means privileged or exempted from that catholick law of gravitation, to which (as far as appears from obſervations that have hitherto been made by inquiſitive philoſophers) all matter is alike and equally ſubject, of what form or texture ſoever it be; and it is upon the account of this its ponderouſneſs and its fluidity that it is qualified to exhibit all the various appearances of other fluid and heavy bodies, as has been made manifeſt in ſeveral inſtances, when we compared the remarkable phænomena of the Torricellian tube, of pumps, ſyringes, ſyphons, poliſhed plates and ſome other effects of the like nature, with the more common and obvious, and therefore leſs ſurpriſing, effects of groſſer and more ſenſible fluids, ſuch as water and quickſilver. We

We have found moreover that the air is endowed with a very confiderable power of elafticity or fpringinefs, by which it perpetually endeavours to expand itfelf into larger dimenfions, and to remove the obftacles, whatever they be, which confine it to the bounds in which it happens at any time to be contained. We have feen alfo that it exerts this power the more forcibly as it is the more clofely imprifoned and crowded together; and, as if it were defirous of its liberty in the fame meafure in which it wants it, experience has fhewed us that the force it employs to regain its freedom, I mean its elafticity, is ever proportionable to its coarctation or denfity.

From hence was deduced a method of determining its rarefactions in the feveral regions above the furface of the earth. You may remember it was proved that as its altitude increafed by equal intervals or in an arithmetical progreffion, fo the degrees of rarity were augmented in a geometrical progreffion. Thefe affections of the air and fome others, which I need not now recall to your memory, have been already made out in the preceding week. But the proofs we made ufe of, though they are very well fitted to fatisfy and convince thofe who are able to give them an attentive and impartial confideration, yet are they of fuch a nature as to afford fome little fcope for the petty cavils and exceptions of fome philofophers, whofe former prejudices had made it feem their intereft to oppofe them; and being rather the remote deductions of reafon than the immediate impreffions of fenfe, they may have, upon that fcore, the lefs weight and moment to determine the affent of men who are not much accuftomed to abftracted fpeculations, but are generally to be wrought upon and convinced by motives of a more fenfible kind, and lefs different from the vulgar apprehenfions they frame to themfelves of the natural appearances of things.

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We ſhall now proceed in the remaining part of our courſe to another ſet of tryals, which will not be ſo liable to the abovementioned objection. Theſe carrying a more ſenſible evidence along with them, are for the moſt part, ſufficient of themſelves to procure our aſſent and intire conviction without any further reinforcements, or the aſſiſtance of remote inferences to bring them home to our underſtanding. For which reaſon, that I may not give you or myſelf any unneceſſary trouble, or miſpend our time to no uſeful purpoſe, I ſhall endeavour to avoid all needleſs prolixity in my future lectures, and ſhall oftentimes alſo omit to read any, when either the matter has already been formerly treated of, or is of itſelf ſo evident as not to require any further illuſtration, or laſtly, which I confeſs will ſometimes happen, is of ſo difficult a nature that I cannot pretend to ſatisfy myſelf as to the true cauſes of ſuch ſurpriſing appearances; and I am not willing to go about to amuſe you with conjectures and ſeeming probabilities or plauſibly contrived hypotheſes.

It is not long ago ſince this method was very much in vogue, but we have ſeen it of late give way to a ſounder and ſurer manner of philoſophizing. It is no very difficult matter for ingenious men, who have ſufficient leiſure, to frame to themſelves ſuch principles of nature as may ſerve to explain any particular appearance whatſoever. But then the theories which are thus advanced, ought to be looked upon only as philoſophical romances, and the witty fictions of inventive brains, unleſs the truth of thoſe principles and their real exiſtence can be demonſtrated, and put beyond diſpute by proper experiments. Where this cannot eaſily be done, it is the ſafeſt way, if we are deſirous to be free from error and prejudice, to wait till ſome further light may be afforded us by future obſervations. The mind of man indeed is naturally deſirous of ſomething to reſt upon, ſomething in which

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it may acquiesce and on which it may terminate its view; it is a sort of pain to it to be held long in suspense, and therefore we are willing to take up with any fair shew of an hypothesis, rather than continue, as we are apt to imagine, in a greater degree of uncertainty. But we ought to consider (besides that these hasty and ill grounded conclusions argue a certain weakness and levity in us) that by thus greedily catching at the shadow we commonly lose the substance; nothing being so great an obstacle to the reception of truth, when it comes to be proposed, as these darling phantoms which use and custom does at length persuade us to be realities. Thus from the time that the philosophy of *Des Cartes* first appeared, the existence of his *materia subtilis* has been looked upon as a thing not to be questioned; the celebrated feats and wonderful operations of it, have in a manner intoxicated the minds of men, and possessed them with a sort of madness; insomuch that any attempts which have been made against it, have been thought to be of the most dangerous consequence, as tending to subvert the very foundations of all science. Even *Hugenius* himself, that great master of reasoning, as he has upon other occasions shewed himself to be, was drawn aside from pursuing better things, by the fondness he had entertained for this principle; which by a sort of legerdemain could so easily be applied to the solution of the most intricate and perplexing difficulties of nature. One might reasonably have expected that this great man, who appears to have been of a very candid and ingenuous temper, after he had seen and considered the incomparable *Principia* of Sir *Isaac Newton*, and in particular the application of them, in accounting for the heavenly motions to such a wonderful degree of exactness; and so full and clear a demonstration of the insufficiency of the Cartesian vortices, one might have expected, I say, that after

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this further information, he would not have been averse to have altered his sentiments. Notwithstanding this, though he expresses an extraordinary pleasure and satisfaction upon the reading of that admirable book (which he says he looks upon as a surprising instance of the great strength and capacity to which it is possible for the mind of man to arrive) yet we see he could not willingly change his own principles for others; and it was impossible for him to forsake an hypothesis which he had himself very much cultivated, and was so long accustomed to. So great is the force of prejudice that an ill grounded opinion shall often prevail, by long prescription, to obstruct the evidence of a well demonstrated certainty. For my part I think it more adviseable to profess our ignorance where the truth is not yet discovered, than to pretend a knowledge which may for ever hinder us from attaining it.

But to come more immediately to the business of this day, the instruments under our present consideration, which we shall chiefly employ in the remaining part of our course, are the air-pump and condenser. By these we are assisted to make a great variety of tryals, concerning the influence and operation of the air under its most different constitutions, from a very great degree of density to an almost infinite rarefaction.

The condenser is an instrument whose invention is so very obvious, that it was impossible it should escape the curiosity of former ages. It has been so very long in use that I cannot pretend to assign its origin.

The air-pump was first, that I know of, contrived and brought into use by *Otto Guericke* consul of Magdeburg, some time before the year 1654; for then it seems this ingenious gentleman, being employed in a publick negotiation at Ratisbon, had an occasion offered him of shewing his engine to the Emperor and some other princes there present; among whom the Elector and Archbishop of Mentz was particularly delighted with the contrivance of the instrument, and the curious experiments exhibited by it; insomuch that he became very desirous of having such another machine made for his own use. But this could not easily be effected by reason of the short stay they had to make at Ratisbon, and for want of skilful workmen. However he prevailed with the inventor to part with his own *apparatus*, and at his return carried it home with him to Wurtzburgh. Here it was that the learned and diligent Jesuit father *Schottus*, being then professor of the mathematicks in that university, had first the sight of it, together with some other curious and learned persons. The archbishop was pleased himself to give them an account of the engine, and a relation of the experiments he had seen the inventor perform at Ratisbon. These they tryed over several times in his presence, and it was not long before they themselves also made several other new ones of the like nature.

The fame of these first essays was quickly spread abroad by the large correspondence which *Schottus* held with learned men in most parts of Europe, but more particularly in the year 1657, when he published his *Mechanica Hydraulico-Pneumatica*; to which as an appendix he added a distinct and full account of these Magdeburgick experiments as he called them. In the year 1664 he published his *Technica curiosa*, and gave a further relation of other new experiments which had been made since the printing of his former book. After this the famous inventor himself *Otto Guericke*, in the year 1672, was pleased to give a most perfect narrative of his own tryals, in his book which he calls *Experimenta nova Magdeburgica de vacuo spatio*. They who are curious to understand the particular fabrick of these first engines, and to observe the gradual improvements which have been made in these matters abroad, may receive full satisfaction by consulting the books I have been mentioning.

But it is time now that I return to our own countryman, the excellent Mr. *Boyle*, whom I fear I shall be thought to have injured by ascribing these first inventions to a foreigner. The air-pump indeed is so generally known by the name of the *Machina Boyliana*, and the void space produced by it is so commonly called the *vacuum Boylianum*, that many are thereby persuaded to believe, they owe their original contrivance to this English philosopher. For my part I should rather chuse to give another reason for these appellations, by saying that the engine and void space do very justly bear the name of Mr. *Boyle*, since whoever might happen to be the inventor of them, his certainly was the more excellent part, to have first applied them to such admirable and useful purposes: it being confessed on all hands that the glory of the English experiments, has in a manner totally obscured that of the Magdeburgick.

As to the contrivance of the instrument, he does himself ingenuously confess that it was not his own, in his letter written, two years after *Schottus*'s first book was published, to the Lord *Dungarvan*, his nephew, who was then at Paris; in which letter are the following words, which I think not amiss to be repeated to you, that you may the better understand the occasion and manner of his first attempts upon this subject. " I should immediately proceed, says he, " to the mention of my experiments, but that I like " too well the worthy saying of the naturalist *Pliny*, " *Benignum est & plenum ingenui pudoris, fateri per* " *quos profeceris*, not to conform to it, by acquaint" ing your Lordship, in the first place, with the hint " I had of the engine I am to entertain you with. " You may be pleased to remember, that a while be-

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“ fore our ſeparation in England, I told you of a “ book that I had heard of, but not peruſed, pub- “ liſhed by the induſtrious Jeſuit *Schottus*, wherein it “ was ſaid, he related how that ingenious gentleman “ *Otto Guericke*, conſul of Magdeburg, had lately “ practiſed in Germany, a way of emptying glaſs veſ- “ ſels, by ſucking out the air at the mouth of the “ veſſel plunged under water. And you may alſo “ perhaps remember, that I expreſſed myſelf much “ delighted with this experiment, ſince thereby the “ great force of the external air, either ruſhing in at “ the opened orifice of the emptied veſſel, or vio- “ lently forcing up the water into it, was rendered “ more obvious and conſpicuous than in any experi- “ ment that I had formerly ſeen. And though it may “ appear from ſome of thoſe writings I ſometimes “ ſhewed your lordſhip, that I had been ſolicitous “ to try things upon the ſame grounds, yet in re- “ gard this gentleman was beforehand with me in “ producing ſuch conſiderable effects, by means of “ the exſuction of air, I think myſelf obliged to “ acknowledge the aſſiſtance and encouragement, “ which the report of his performance hath afford- “ me. But as few inventions happen to be at firſt “ ſo compleat, as not to be either blemiſhed with “ ſome deficiencies needful to be remedied, or other- “ wiſe capable of improvement, ſo when the engine “ we have been ſpeaking of, comes to be more at- “ tentively conſidered, there will appear two very “ conſiderable things to be deſired in it. For firſt, “ the wind-pump, as ſomebody not improperly calls “ it, is ſo contrived, that to evacuate the veſſel there “ is required the continual labour of two ſtrong men “ for divers hours. And next, which is an imperfe- “ ction of much greater moment, the receiver, or “ glaſs to be emptied, conſiſting of one entire and “ uninterrupted globe and neck of glaſs, the whole "engine is so made, that things cannot be conveyed "into it, whereon to try experiments: so that there "seems but little, if any thing, more to be expected "from it, than those very few phænomena that have "been already observed by the author and recorded "by *Schottus*. Wherefore to remedy these inconve-"niencies, I put both Mr. *Gratorix* and Mr. *Hook* "to contrive some air-pump that might not, like "the other, need to be kept under water, and might "more easily be managed. And after an unsuccess-"ful tryal or two of ways proposed by others, Mr. "*Hook* fitted me with a pump anon to be describ-"ed."

This air-pump of Dr. *Hook*'s contrivance was, it seems, the first that Mr. *Boyle* made use of. It was indeed more perfect than that described by *Schottus* in his *Mechanica Hydraulico-Pneumatica*, yet still it laboured with several imperfections, and was not so commodious in many respects as might be desired; particularly it was furnished but with one single receiver, always fixed to the body of the engine; which therefore it was requisite should be very capacious to be fitted for all manner of tryals. Now this great capacity of the receiver made it necessary to employ a considerable time for its exhaustion; but this was an inconvenience which could not easily be dispensed with in many experiments that required a speedy evacuation; and moreover a variety in the form of the receivers to be made use of, would better suit with the variety of the subjects which were to be enquired into. Hence I suppose it was, that after he had made his first experiments with this engine, and had published them in the form of a letter to his nephew, under the title of *Physico-Mechanical experiments touching the spring of the air and its effects*, he thought it requisite to make an alteration and improvement of his instrument before he proceeded to a further prosecution of his design. The

The deſcription of this ſecond air-pump of Mr. *Boyle*, may be ſeen in the firſt continuation of his Phyſico-Mechanical Experiments. It conſiſted, as the former, only of one ſingle barrel, by which the receiver was evacuated; but this barrel was now contrived to be every way ſurrounded with water, the better to prevent any poſſible regreſs of the air. The receivers, which were now of ſeveral ſhapes and bigneſſes, were cloſed to an iron plate, upon which they were placed by the means of a ſoft cement, and ſo they could eaſily be removed and changed as occaſion required. He had not, it ſeems, as yet thought of that eaſier expedient of fixing them to the table on which they ſtood, by the interpoſition of a wet leather.

The experiments related in the ſecond continuation of this Honourable Author, were made with an engine different from the two former. It was the contrivance of Mr. *Papin*, whoſe aſſiſtance Mr. *Boyle* did alſo make uſe of in the tryals themſelves. This third air-pump was much more convenient than the former, and the advantage lay chiefly in theſe two particulars. Firſt, whereas the former engines had only one ſingle barrel and one ſucker or embolus, this was furniſhed with two barrels and two ſuckers, and theſe two ſuckers being alternately raiſed and depreſſed, cauſed the evacuation to be continual; which effect could not be obtained by a ſingle ſucker, it being neceſſary that the evacuation ſhould ceaſe during the time in which the ſucker is forced in towards the bottom of the barrel. But beſides this advantage of performing the operation in half the time it could be done with a ſingle ſucker, the labour alſo in doing it was exceedingly leſſened. The chief difficulty complained of in ſingle-barrelled pumps, is the very great reſiſtance which the external air makes againſt the ſucker as it is drawn outwards; and this reſiſtance increaſes as the receiver is more and more exhauſted, the counterballance of the internal againſt the external air being thereby more and more diminiſhed; ſo that if the barrel be of a conſiderable wideneſs, it may be impoſſible for the ſtrength of any one man to work the engine any longer. Now this reſiſtance of the external air is entirely taken off by making uſe of two ſuckers inſtead of one. They are ſo connected together by the fabrick of the inſtrument, that as the one deſcends, ſo the other muſt of neceſſity aſcend at the ſame time; and conſequently the reſiſtance of the external air, hindering the aſcent of the one as much as it promotes the deſcent of the other, by contrary effects loſes its force upon both. I cannot illuſtrate this better than by comparing it with a ballance. If a ſingle weight be placed at one of its extremities, we perceive a difficulty in moving the beam to make the weight aſcend, and this difficulty increaſes as the weight is greater. But if you place another weight equal to the former at the oppoſite extremity, the difficulty in moving the beam will entirely ceaſe, how great ſoever the two equal weights may be ſuppoſed to be.

The other particular in which this air-pump excelled the former, was the advantage of its valves. In the two firſt engines, whilſt the ſucker was drawn outwards, you was obliged at the ſame time to turn a ſtop-cock, to make way for the air in the receiver to paſs from thence into the barrel; and when this air was to be excluded from the barrel, as the ſucker was moved inwards, you was obliged again to turn the ſtop-cock, to prevent the air from reverting into the receiver, and at the ſame time to give it a paſſage outwards, a ſtopple or plug was to be removed, which cloſed the hole through which it was to paſs, and then again this hole was to be ſtopped up and the cock to be turned again as more air was drawn

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from the receiver; and this labour to be repeated perpetually ſo long as you continued to work the pump. Now the valves which in the third air-pump ſupplied the place of the plug and ſtop-cock, were undoubtedly much more convenient, in that of themſelves they opened to give the air a paſſage forwards, and ſhut to prevent its return back again. I will not detain you any longer by ſpeaking of the various forms of the engine, and the different contrivances which have been uſed by others. I have not obſerved that any of them whoſe deſcriptions I have ever met with, has been ſo convenient in all reſpects, as the air-pump before us, which was made by that excellent operator the late Mr. *Haukſbee.* I cannot ſay that, in the main, it is at all different from the third of Mr. *Boyle*'s; what little alterations may be obſerved in it, are I think for the better. It would be a loſs of time to go about to deſcribe it by words, when we may better ſee the contrivance of it with our eyes. I will therefore take its ſeveral parts aſunder, and then endeavour to make you underſtand the uſe of each, and the operation of the whole, as clearly and diſtinctly as I can.

LECTURE XIII.

An account of the ſeveral ſucceſſive degrees in which the air is expanded and compreſſed by the air-pump and condenſer.

AT our laſt meeting we took a particular view of the ſeveral parts of which our engines conſiſt. I ſhall therefore ſuppoſe you to be ſufficiently acquainted with the fabrick and contrivance of them, and to underſtand in general the manner of their operations. I ſay in general, becauſe there are ſome particulars which yet remain at this time to be diſcourſed

courſed of; which may alſo very well deſerve your conſideration, and will be of good uſe in order to frame juſt and true apprehenſions of the experiments which will hereafter be made. I ſhall begin with the air-pump, and repreſent to you by what degrees the air contained in the receiver is exhauſted.

It may perhaps, upon the firſt view, ſeem not improbable that an equal evacuation is made at each ſtroke of the pump, and conſequently that the receiver may after a certain number of ſtrokes be perfectly exhauſted; for it muſt be allowed, if an equal quantity of air is taken away at every ſtroke, that the receiver will in time be perfectly exhauſted, how ſmall ſoever thoſe equal quantities, which are continually taken away, may be ſuppoſed to be. Thus if the air which goes out of the receiver at each turn of the pump, be but the hundredth part of what was at firſt included in the receiver, it is certain that a total evacuation will be made after an hundred turns. That things are thus, may at firſt view I ſay, ſeem not improbable, but if we conſider the matter more nearly we ſhall find it to be far otherwiſe.

What I ſhall endeavour to make out to you is this; that the quantities exhauſted at every ſtroke are not equal, but are perpetually diminiſhed, and grow leſſer always ſo long as you continue to work the pump; that no receiver can ever be perfectly and intirely evacuated, how long time ſoever you employ for that purpoſe, notwithſtanding that the engine be abſolutely free from all defects and in the greateſt perfection which can be imagined. It may appear to be a paradox, that a certain quantity of the air in the receiver ſhould be removed at every turn of the pump, and yet that the whole can never be taken away; but I hope I ſhall eaſily ſatisfy you that it is not a miſtake. Laſtly, that I may not ſeem too much to depreciate the value of our engine, I have this further

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to ſay for it, that though it be impoſſible by its means to procure a perfect *vacuum*, yet you may approach as near to it as you pleaſe. By a perfect *vacuum* I mean in reſpect of air only, not an abſolute vacuity in reſpect of every thing which is material; for not to mention what other ſubtle bodies may poſſibly be lodged in our emptied receivers, it is matter of fact that the rays of light are not excluded from thence.

In order to make out theſe aſſertions I ſhall in the firſt place lay down this rule. That the quantity of air which is drawn from the receiver at each ſtroke of the pump, bears the ſame proportion to the quantity of air in the receiver immediately before that ſtroke, as the capacity of the barrel into which the air paſſes from the receiver, does to the capacity of the ſame barrel and the capacity of the receiver taken together.

You may remember that in each barrel there are two valves, whereof the lower is placed at the bottom of the barrel, and the upper is fixed upon the embolus or ſucker. Now the hollow ſpace which lies betwixt theſe valves, when the embolus is raiſed as high as it can go, is what I call the capacity of the barrel; for the other part of the cavity of the barrel, which is above the embolus and the upper valve, is of no uſe in evacuating the receiver, and therefore ought not here to be conſidered. Upon a like account, by the capacity of the receiver I mean, not only the ſpace immediately contained under the receiver, but alſo all thoſe other hollow ſpaces which communicate with it, as far as to the lower valves: ſuch you may remember are the cavity of the pipe which conveys the air to the barrels, and the cavity in the upper part of the gage above the quickſilver. Theſe additional ſpaces are very ſmall and inconſiderable, yet if we would be exact, they alſo muſt be taken

taken into the account and looked upon as parts of the receiver.

Now to underſtand the truth of the rule, we muſt obſerve that as the embolus is moved upwards from the bottom of the barrel, it would leave a void ſpace behind it, but this effect is prevented by the ruſhing in of air from the receiver. The air, you know, by its elaſticity is always endeavouring to expand itſelf into larger dimenſions, and it is by this endeavour that it opens the lower valve, and paſſes into the hollow part of the barrel as the embolus gives way to it, and this it will continue to do, till it comes to have the ſame denſity in the barrel as in the receiver. For ſhould its denſity in the barrel be leſs than in the receiver, its elaſtick force, which is proportionable to its denſity, would be leſs alſo, and therefore it muſt ſtill give way to the air in the receiver, till at length the denſities become the ſame. The air then which immediately before this ſtroke of the pump, by which the ſucker is raiſed, was contained in the receiver only, is now uniformly diffuſed into the receiver and the barrel; whence it appears that the quantity of air in the barrel, is to the quantity of air in the barrel and receiver together, as the capacity of the barrel, is to the capacity of the barrel and receiver together. But the air in the barrel is that which is excluded from the receiver by this ſtroke of the pump, and the air in the barrel and receiver together, is what was in the receiver immediately before the ſtroke; therefore the truth of the rule is very evident, that the quantity of air which is drawn from the receiver at each ſtroke of the pump, bears the ſame proportion to the quantity of air in the receiver immediately before that ſtroke, as the capacity of the barrel into which the air paſſes from the receiver, does to the capacity of the ſame barrel and the capacity of the receiver taken together.

To illustrate this further by an example, let us suppose the capacity of the receiver to be twice as great as the capacity of the barrel; then will the capacity of the barrel, be to the capacity of the barrel and receiver together, as 1 to 3; and the quantity of air exhausted at each turn of the pump, is to the quantity of air which was in the receiver immediately before that turn, in the same proportion. So that by the first stroke of the pump, a third part of the air in the receiver is taken away, by the second stroke a third part of the remaining air is taken away, by the third stroke a third part of the next remainder is exhausted, by the fourth a third part of the next, and so on continually; the quantity of air evacuated at each stroke, diminishing in the same proportion with the quantity of air remaining in the receiver immediately before that stroke: for it is very evident that the third part, or any other determinate part of any quantity must needs be diminished in the same proportion with the whole quantity itself. And this may suffice for the proof of what I asserted in the first place, that the quantities exhausted at every stroke are not equal but are perpetually diminished.

I shall now proceed to shew, that the air remaining in the receiver after every stroke is diminished in a geometrical progression. It has been proved that the air remaining in the receiver after each stroke of the pump, is to the air which was in the receiver immediately before that stroke, as the capacity of the receiver is to the capacity of the barrel and receiver taken together; or in other words, that the quantity of air in the receiver, by each stroke of the pump, is diminished in the proportion of the capacity of the receiver to the capacity of the barrel and receiver taken together. Each remainder is therefore evermore less than the preceding remainder in the same

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given ratio; that is to ſay, theſe remainders are in a geometrical progreſſion continually decreaſing.

Let us return again to our former example, which may afford a ſomewhat different light into this matter. The quantity exhauſted at the firſt turn, you remember, was a third part of the air in the receiver, and therefore the remainder will be two thirds of the ſame; and for the like reaſon the remainder after the ſecond turn will be two thirds of the foregoing remainder, and ſo on continually; the decreaſe being always made in the ſame proportion of 2 to 3; conſequently the decreaſing quantities themſelves are in a geometrical progreſſion.

It was before proved that the quantites exhauſted at every turn did decreaſe in the ſame proportion with theſe remainders; therefore the quantities exhauſted at every turn are alſo in a geometrical progreſſion. Let it then be remembered, that the evacuations and the remainders do both decreaſe in the ſame geometrical progreſſion.

If the remainders decreaſe in a geometrical progreſſion, it is certain you may, by continuing the agitations of the pump, render them as ſmall as you pleaſe, that is to ſay, you may approach as near as you pleaſe to a perfect *vacuum*. But notwithſtanding this, you can never entirely take away the remainder. If it be ſaid that you may, I prove the contrary thus. Before the laſt turn of the pump, which is ſaid wholly to take away the remainder, it muſt be confeſſed there was a remainder; this remainder, by that laſt turn of the pump, will only be diminiſhed in a certain proportion, as has been before proved; therefore it was falſly ſaid to be totally taken away.

It may not be improper in this place to ſay ſomething concerning the gradual aſcent of the quickſilver in the gage, upon which we have made ſome experiments. You have obſerved that as we continue

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to pump, the quickfilver continues to afcend, approaching always more and more to the ftandard altitude in the weather-glafs, which you know is about $29\frac{1}{2}$ inches, being a little under or over according to the variety of feafons. What I fhall now endeavour to make out to you is this; that the defect of the height of the quickfilver in the gage from the ftandard altitude, is always proportionable to the quantity of air, which remains in the receiver; that the altitude itfelf of the quickfilver in the gage, is proportionable to the quantity of air which has been exhaufted from the receiver; that the afcent of the quickfilver upon every turn of the pump, is proportionable to the quantity evacuated by each turn.

In order to underftand thefe affertions you are to confider, that the whole preffure of the atmofphere upon the ciftern of the gage, is equivalent to, and may be ballanced by, a column of quickfilver of the ftandard altitude. Therefore when the quickfilver in the gage has not yet arrived to the ftandard altitude, it is certain the defect of quickfilver is fupplied by fome other equal force, and that force is the elaftick power of the air yet remaining in the receiver; which communicating, as you remember, with the upper part of the gage, hinders the quickfilver from afcending, as it would otherwife do, to the ftandard altitude. The elafticity of the air in the receiver is then equivalent to the weight of the deficient quickfilver; but the weight of that deficient quickfilver is proportionable to the fpace it fhould poffefs, or to the defect of the height of the quickfilver in the gage from the ftandard height; therefore the elafticity of the remaining air is alfo proportionable to the fame defect. And fince it was formerly proved, that the denfity of any portion of air is always proportionable to its elafticity, and the quantity in this cafe is proportionable to the denfity, it follows that the quantity

quantity of air remaining in the receiver, is proportionable to the defect of the quickſilver in the gage from its ſtandard altitude, which was the firſt thing to be proved.

Hence it follows that the quantity of air which was at firſt in the receiver before you began to pump, is proportionable to the whole ſtandard altitude; and conſequently the difference of this air, which was at firſt in the receiver and that which remains after any certain number of turns, that is, the quantity of air exhauſted, is proportionable to the difference of the ſtandard altitude and the beforementioned defect, that is, to the altitude of the quickſilver in the gage after that number of turns; which was the ſecond thing to be proved.

And from hence it follows that the quantity of air exhauſted at every turn of the pump, is proportionable to the aſcent of the quickſilver upon each turn, which was the laſt thing to be made out. And theſe concluſions do very well agree with the experiments, which ſhewed us the quantity of air that was exhauſted, by the quantity of water which afterwards ſupplied the vacant place of that air in our receiver (*a*).

Let it then be remembered, that the quantity exhauſted at each turn, is proportionable to the aſcent of the quickſilver upon that turn; that the whole quantity exhauſted from the time you began to pump, is proportionable to the whole altitude of the quickſilver; that the quantity remaining in the receiver is proportionable to the defect of that altitude from the ſtandard.

To come now to the application of the other experiments which we made this day. We found, by meaſuring, that the ſeveral aſcents of the quickſilver

(*a*) This receiver, like the bottle deſcribed in Exp. 1. pag. 8, had a ſtop-cock cemented to it, to hinder the return of the exhauſted air, and to admit water in its ſtead.

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in the gage, upon every turn of the pump, were diminished in a geometrical progression, and it has just now been proved that the quantities of air exhausted at each turn are proportionable to those ascents. Therefore we may safely conclude from experiment also, what we before collected by a train of reasoning, that the quantities of air exhausted at every turn of the pump, are diminished continually in a geometrical progression.

Furthermore, since those ascents are the differences of the defects from the standard altitude, upon every successive turn of the pump, it follows that the defects also are in the same decreasing geometrical progression. For it is a general theorem, that all quantities whose differences are in a geometrical progression, so long as the quantities continue to have any magnitude, are themselves also in the same geometrical progression. The defects being then in a decreasing geometrical progression, and the quantities of air remaining in the receiver being proportionable, as was lately proved, to the defects, it follows from the same experiments, that the quantities of air which remain in the receiver after every turn of the pump, do decrease in a geometrical progression; which was the other thing concluded also by a train of reasoning.

Before I dismiss the consideration of the air-pump, it remains that I add something concerning the use of the two tables, which I have put into your hands. They are designed to shew the number of turns of the pump, which are requisite to rarefie, in any given proportion, the air contained under any receiver. The first table in particular is fitted for receivers whose capacity is the same with the capacity of the barrel, and the numbers of the first column of it express the degrees of rarefaction, as those over against them in the second column express the number of turns, with their decimal parts, which are requiſite to produce thoſe degrees of rarefaction. Thus for example, if it were required to rarefie the air, under ſuch a receiver, an hundred times above its natural rarity, I ſeek for the number 100 in the firſt column, and over againſt it in the ſecond I find the number 6.644, by which I underſtand that the air will be rarefied an hundred times by 6 turns of the pump and 644 thouſand parts of a turn. So if it were deſired to rarefie the air under the ſame or an equal receiver 10 thouſand times more than in its natural ſtate, I perceive there will be 13 turns and 288 thouſand parts of a turn requiſite for that purpoſe.

TABLE I.

Rarity	Number of turns	Rarity	Number of turns	Rarity	Number of turns
1	0	60	5.907	900	9.814
2	1	64	6	1000	9.966
3	1.585	70	6.129	1024	10.
4	2	80	6.322	2000	10.966
5	2.322	90	6.492	2048	11.
6	2.585	100	6.644	3000	11.551
7	2.807	128	7	4000	11.966
8	3	200	7.644	4096	12.
9	3.170	256	8	5000	12.288
10	3.322	300	8.229	6000	12.551
16	4	400	8.644	7000	12.773
20	4.322	500	8.966	8000	12.966
30	4.907	512	9	8192	13.
32	5	600	9.229	9000	13.136
40	5.322	700	9.451	10000	13.288
50	5.644	800	9.644	16384	14.

The receivers which we ſhall have occaſion to make uſe of in our experiments, are generally much bigger

bigger than the capacity of each barrel of the pump, and by being bigger, will require a greater number of turns than thoſe ſet down in the ſecond column, to rarefie the air in the degrees which are expreſſed in the firſt column. It may perhaps at the firſt view, ſeem not unreaſonable to think that the number of turns requiſite to rarefie the air in any certain degree, ſhould exceed the numbers of the ſecond column, in the ſame proportion by which the capacity of the receiver exceeds the capacity of the barrel. But if the matter be examined more cloſely, it will be found, that the number of turns do not increaſe in ſo great a proportion as the capacity of the receiver does.

What that proportion is, by which the number of turns is truly increaſed, as the capacity of the receiver becomes bigger, may be ſeen by the ſecond table, whoſe firſt column expreſſes the proportion of the receiver to the barrel, as the ſecond does the proportion of the true number of turns to thoſe ſet down in the firſt table. The uſe of it will be more clearly underſtood by an example or two.

Let us ſuppoſe the capacity of the receiver to be 10 times greater than the capacity of the barrel, and that we would find how many turns are requiſite to rarefie the air under ſuch a receiver 100 times more than it is naturally rarefied.

By the firſt table we find, as was ſaid above, that if the receiver were equal to the barrel, the number of turns would be 6.644. But the receiver is 10 times greater. Find therefore the number 10 in the firſt column of the ſecond table, and over againſt it you will ſee the number 7.273 in the ſecond column of the ſame table; by which you perceive that as the receiver is increaſed in a decuple proportion, the number of turns are increaſed not ſo much, but only ſomewhat more than in a ſeptule proportion. Therefore

fore the true number of turns will be found by multiplying the number 6.644 by the number 7.273, and will conſequently be 48.322.

TABLE II.

Capacity of the receiver	Multiplier	Capacity of the receiver	Multiplier
1	1.	60	41.934
2	1.710	70	48.866
3	2.409	80	55.798
4	3.106	90	62.729
5	3.802	100	69.661
6	4.497	200	138.976
7	5.191	300	208.291
8	5.885	400	277.605
9	6.579	500	346.920
10	7.273	600	416.235
20	14.207	700	485.549
30	21.139	800	554.864
40	28.071	900	624.179
50	35.003	1000	693.494

So if it were deſired to find the number of turns of the pump, which muſt be made to rarefie the air 10 thouſand times above its natural ſtate, in a receiver which is 50 times bigger than the capacity of the barrel; over againſt 10000 in the firſt table I find 13.288 and over againſt 50 in the ſecond table I find 35.003, which multiplied together make 465.12; this therefore is the number of turns requiſite for the purpoſe. You need not be ſolicitous about the fractions which are above any certain whole number of turns; they do not mean, that the handle of the pump is to be moved juſtly ſuch a part of a turn as they

ſeem

ſeem to denote, for ſtrictly ſpeaking it need not be moved altogether ſo much, but the difference is inconſiderable, and it would be a loſs of time to inſiſt more particularly about it. It was neceſſary to ſet down the fractions in the tables that no whole number of turns might be loſt in the product, when you come to multiply them together; but when you have found the product, the fractions belonging to it need not be conſidered.

In making theſe tables, that they might not be too large, you ſee I have omitted ſeveral intermediate numbers. However they are ſufficient for the purpoſe for which I deſigned them; which was to give you clearer notions of the operation of our engine. I ſhould here explain to you the grounds upon which they were computed, but I fear the difficulty of the ſubject would not permit me to be generally underſtood. I ſhall therefore omit the doing of it, *(b)* and

(b) In a receiver whoſe capacity is to that of the barrel of the pump, as c to 1, the air will be rarefied r times by a number of turns equal to $\frac{\log. r.}{\log. \overline{c+1} - \log. c.}$

For by any one of the turns, the air will be rarefied or dilated in the ratio of the ſpace c to the ſpace $c+1$, or of 1 to $\frac{c+1}{c}$; and therefore will have as many ſuch equal and ſucceſſive rarefactions, as there are equal and ſucceſſive ratio's in this ſeries $1, \overline{\frac{c+1}{c}}\Big|^1, \overline{\frac{c+1}{c}}\Big|^2, \overline{\frac{c+1}{c}}\Big|^3$, continued to the term $\overline{\frac{c+1}{c}}\Big|^n$, whoſe index n is the whole number of turns. Conſequently the total ratio of all the rarefactions, for which we put 1 to r, is the ſame as the ratio of 1 to $\overline{\frac{c+1}{c}}\Big|^n$, that is, of the firſt term of the ſeries to the laſt. Therefore $r = \overline{\frac{c+1}{c}}\Big|^n$, and by the noted properties of logarithms, the log. $r = n \times \log. \frac{c+1}{c} = n \times \overline{\log. \overline{c+1} - \log. c}$.

whence $\frac{\log. r}{\log. \overline{c+1} - \log. c} = n$.

Corol.

only observe to you of the first table, that if you take any numbers in the first column which are in a geometrical progression, the correspondent numbers of the second column will be in arithmetical progression. It may also be observed of the second table that the disproportion of the correspondent numbers does continually increase from the beginning to the end, how far soever it be continued, but yet does never exceed the disproportion of 13 to 9.

It is time now that we proceed to the condenser. This instrument will not require much to be said concerning it. When I assert that equal quantities of air, namely, as much as the barrel can naturally contain, are intruded into the receiver at each stroke of the forcer, the thing is so very obvious that I believe I need not go about to prove it. For you cannot but easily understand, that as the embolus or forcer is drawn upwards from the bottom of the barrel, there is a vacuity left behind it, till such time as it comes to get above the little hole, which is made in the side of the barrel towards the top of it. For then the external air is permitted to pass freely through that hole into the aforesaid void space, and consequently the barrel will then have as much air in it, as it can

Corol. 1. Hence in Table I, where $c=1$, the air will be rarefied r times by a number of turns equal to $\frac{\log. r}{\log. 2}$.

Corol. 2. Call that number t, and in Tab. II, where c has any proposed value, put m for the corresponding multiplier; then since the author makes $m\,t=n$, we have $m=\frac{n}{t}=\frac{\log. 2}{\log. \overline{c+1}-\log. c}$.

In the author's example $r=100$ & $c=10$, whence by *Corol.* 1, $t=\frac{\log. 100}{\log. 2}=\frac{2.00000}{0.30103}=6.644$ turns; and by *Corol.* 2, $m=\frac{\log. 2}{\log. 11-\log. 10}=\frac{0.3010300}{0.0413927}=7.273$, & $m\,t=n=48.342$ turns.

natu-

naturally contain. And as the forcer is moved downwards, this air is compreſſed, and by compreſſion is more and more condenſed, till at length the force of its elaſticity becomes greater than the elaſtick force of that which is contained within the receiver, and thereby it will open the valve and make way for itſelf to enter totally into the receiver, as it is continually puſhed forwards by the deſcending embolus. Since then the quantities intruded at each ſtroke of the forcer are equal, it manifeſtly appears that the quantities in the receiver, and conſequently the degrees of condenſation, do increaſe in an arithmetical progreſſion.

Let us now examine by what ſteps the quickſilver in the gage advances at each ſtroke. What I ſhall endeavour to prove as to this matter is this, that as the quickſilver is moved forwards in the gage upon every ſucceſſive ſtroke of the forcer, the ſpaces at the end of the gage, which are yet left free from the quickſilver, do decreaſe in a muſical progreſſion.

But in the firſt place it may not be amiſs to explain in ſome meaſure the nature of muſical progreſſions, ſince theſe are not generally ſo well underſtood as thoſe which we call arithmetical and geometrical progreſſions. In order to do this, I ſhall propoſe an inſtance which firſt gave occaſion for the name.

It is a thing well known among muſicians, if three chords or ſtrings, in all other reſpects alike, be of different lengths, and thoſe lengths be to each other in proportion as the numbers of 6, 4 and 3, that the ſounds of thoſe ſtrings will expreſs the principal and moſt perfect of the muſical concords, namely, an eight, a fifth and a fourth. Thus the ſound of the laſt will be an octave to the ſound of the firſt, and the ſound of the ſecond a fifth to the ſound of the firſt, and the ſound of the laſt a fourth to the ſound of the ſecond. Hence theſe numbers 6, 4 and 3, which expreſs the proportions of thoſe muſical ſtrings, were ſaid not improperly to be in a muſical progreſſion. Now it was eaſy to be obſerved, that theſe numbers were reciprocally proportional to three other numbers reſpectively, *viz.* 2, 3 and 4, which were in arithmetical progreſſion; and thence it came to paſs, that any other ſeries of numbers was ſaid to be in a muſical progreſſion, which had the ſame property of being reciprocally proportional to a ſeries of numbers in arithmetical progreſſion.

That therefore is a ſeries of muſical proportionals which is reciprocal to another ſeries of arithmetical proportionals.

But beſides this, you may obſerve another property belonging to the above mentioned numbers 6, 4 and 3, viz. that the firſt is to the third, as the difference of the firſt and ſecond, is to the difference of the ſecond and third. And this property does equally belong to all other numbers, which are reciprocally as a ſeries in arithmetical progreſſion, that is, to all other numbers which are in a muſical progreſſion.

Hence if any two ſucceeding terms be given, the third may be found by dividing the product of the firſt and ſecond, by the difference which ariſes in ſubſtracting the ſecond from the double of the firſt. Thus in the progreſſion 6, 4 and 3, the product of the firſt and ſecond terms 6 and 4 is 24, and the difference which ariſes by ſubſtracting the ſecond term 4 from 12, the double of the firſt, is 8, and the quotient which emerges by dividing the product 24 by the difference 8 is 3, the third term in the progreſſion required.

I ſhall now go on to ſhew that the ſpaces unpoſſeſſed by the quickſilver at the end of the gage, decreaſe in a muſical progreſſion.

It muſt be obſerved therefore, that the quickſilver of the gage is contiguous on one ſide to the air

within

within the receiver, and on the other ſide to the air which is ſhut up at the end of the gage, and that the denſity of the air in both places is equal. For were the denſity of the air in the receiver greater than the denſity of the air at the end of the gage, its elaſtick force would alſo be greater, and by that exceſs of force the quickſilver would be moved on further towards the end of the gage, till the forces, and conſequently the denſities, became equal. After the ſame manner if the denſity of the air at the end of the gage, were greater than the denſity of the air within the receiver, the quickſilver would be moved backwards from the end of the gage, till the denſities became equal. It is manifeſt therefore that the denſities are equal in both parts when the quickſilver in the gage is at reſt. Therefore ſince the denſity of the air in the receiver, upon every ſucceſſive ſtroke of the forcer, was increaſed in arithmetical progreſſion, it follows that the denſity of the air at the end of the gage, is likewiſe increaſed in the ſame arithmetical progreſſion. But the ſpace which that air poſſeſſes, is diminiſhed in the ſame proportion by which the denſity is increaſed, or in other words, the ſpaces are reciprocally as the denſities, therefore the ſpaces are reciprocally as a ſeries of terms in arithmetical progreſſion; which is the ſame thing as to ſay, the ſpaces are in a muſical progreſſion. And this concluſion we found alſo to agree with our experiments.

LECTURE XIV.

A parcel of air weighed in a ballance; its ſpecifick gravity to that of water determined thereby.

THE gravitation of the air has in the foregoing week been ſufficiently proved by ſeveral different methods. We have ſeen a great variety of ap-

pearances

pearances which are very clearly and naturally accounted for upon that principle, and cannot be explained upon any other. We might therefore very juftly conclude it to be altogether true and exactly agreeable to nature, upon the force of that evidence alone. But the immediate evidence of fenfe has always fomething in it, which does more powerfully affect and convince us, than the united ftrength of the greateft number of inferences drawn out by a feries of reafoning. And upon this account we may fay, that the experiment we have now been making, has afforded us a clearer and more cogent proof of the weight of the air, than any of thofe confiderations we have hitherto been engaged in, or even than all of them taken together. For what can be further expected or defired to evince an heavy body to be really fuch, than to feel (if I may fay fo) the weight of it in the ballance. This experiment itfelf or fome other to the fame effect, might at any time fo eafily have been tried, that I believe you will be much more inclined to wonder, how it could have been poffible for any fect of philofophers to have doubted of the gravitation of the air, than to doubt of it in the leaft yourfelves.

It muft be confeffed that *Ariftotle* himfelf does fomewhere in his writings affert this gravitation, and to prove his affertion he appeals to the experiment of a bladder full blown; which, fays he, weighing more than the fame bladder when it was flaccid, is a manifeft token of the weight of the air contained in it. But it is certain, however unreafonable it may feem, that his followers departed from their mafter, and maintained the contrary for feveral ages together. *Galilæo* feems to have been the firft among the modern philofophers, who durft venture to oppofe them in this matter. I formerly gave you an account of the obfervation he had made concerning pumps:

this

this was a ſufficient hint to a perſon of his ſagacity, to call into queſtion the commonly received doctrine of the ſchools. But the inquiſitive genius of this great man was not long to be held in ſuſpenſe. He ſoon reſolved upon proper experiments which might afford him a full and perfect ſatisfaction in this buſineſs. What thoſe experiments were, I am now to tell you.

He took a glaſs veſſel of a large capacity, having a very narrow neck, to which he applied a cover of leather and faſtened it as cloſe as was poſſible to the neck. Through the middle of this cover he put a ſlender tube, cloſing the leather to it as exactly as he could. Then with a ſyringe he forced into the glaſs veſſel, through the tube, as great a quantity of air as he conveniently might, ſo as not to endanger the breaking of the glaſs. This being done he weighed the veſſel, with all this compreſſed air in it, as carefully as he could, by the help of a moſt exact ballance; making uſe of very fine ſand for a counterpoiſe. Afterwards, by uncovering the tube, he permitted the air, which had been with violence forced into the veſſel, to make its eſcape out again; and then he applied the veſſel to the ballance a ſecond time, and finding it to be lighter than before, he took away part of the ſand, till at length he had reduced the beam to an equilibrium again. By this means he was entirely convinced of the ponderoſity of the air in general, and was ſatisfied that the weight of ſo much of it as had eſcaped upon opening the veſſel, was equal to the weight of that ſand which he had taken away and reſerved apart by itsſelf. This indeed was very evident, but yet it was not poſſible from this experiment, to form any concluſion concerning the preciſe and determinate weight of any particular quantity of air, in compariſon with the weight of other bodies: for it could not certainly be known,

known, what the quantity of that air was which had made its escape, and which was equal in weight to the sand he had reserved. In order therefore to pursue this matter somewhat further, he contrived two other different methods of making the experiment. The first was after this manner.

He took another vessel in all respects alike to the former, which had also a cover of leather fixed to it. Through this cover he caused to pass into this second vessel, the end of the tube which stood out of the former vessel and was closed up, and at the same time he took care to bind the cover of the second vessel as exactly as was possible to the tube. This being done, the necks of the vessels respected each other, and had a communication by means of the tube, so soon as the closed end of it, which lay in the second vessel, was opened. Now the end of the tube was opened by a long and slender iron-pin, passing through a small hole made at the bottom or opposite end of the second vessel. But I ought to tell you, that before these vessels were thus joined together, the second was filled with water, and the weight of the first, with all its included and compressed air, was found by a counterpoise of sand as before. Things then being thus disposed and prepared, and the end of the tube being opened by means of the iron-pin, it is easy to understand that the compressed air will rush forth with violence into the second vessel, till that which remains behind in the first vessel be reduced to its natural density. Now as the air forces its way into the vessel filled with water, it must necessarily happen, that part of this water will be expelled, through the hole at the bottom of the vessel, to make way for the air; and the bulk of the water which is expelled, will be equal to the bulk of the air which came forth from the first vessel into the second. The weight of that air, which came out from the first vessel,

veſſel, is to be found by weighing the firſt veſſel again after that air is gone out of it; and this weight of air, compared with the weight of that equal bulk of water which was expelled and reſerved, will give the proportion of the ſpecifick gravity of air to the ſpecifick gravity of water. By this method *Galilæo* tells us, he found that air, was about 400 times lighter than water.

The other method which he propoſes, is ſomewhat more expeditious, it requiring only one veſſel, which muſt be fitted up as the former of thoſe two, which have been juſt now deſcribed. But here inſtead of forcing in more air than the veſſel does naturally contain, he chuſes rather to force in water, whereby the air contained in the veſſel is compreſſed and condenſed, care being taken that no part of it eſcape out, as the water is forced in. Suppoſing then a ſufficient quantity of water to be forced into the veſſel, which may conveniently enough be as much as will fill $\frac{3}{4}$ of it, the whole muſt be weighed with great exactneſs. Then the neck of the veſſel is to be placed upwards, and the tube being opened, the air will be at liberty to expand itſelf, and ſuch a part of it will go out, whoſe bulk is equal to the bulk of water which was forced in. This being done, the veſſel muſt be weighed again, and this laſt weight will be ſo much leſs than the former, as the weight of the air which went out amounts to; and conſequently the ſpecifick gravity of air will be to the ſpecifick gravity of water, as that defect is to the weight of the water which was forced into the veſſel.

Such were the contrivances of this admirable Italian philoſopher at a time when the reſt of the world had altogether different notions of theſe matters, and long before our air-pumps or barometers were invented. It is a ſatisfaction to underſtand by what methods theſe enquiries firſt began. But beſides this, you have alſo another advantage from the account I have been

been giving you; my meaning is, that you will from hence be enabled to frame to yourſelves apprehenſions of various other ways of doing the ſame thing, different from thoſe I have been relating, or that which we have been practiſing. It would be endleſs to deſcribe the ſeveral methods which have been propoſed by curious and inquiſitive men, or to tell you what methods I could myſelf contrive to the ſame purpoſe. The firſt experiment of this nature which was generally taken notice of, and became in its time very famous, was that of *Merſennus.* Omitting therefore all others, I ſhall content myſelf with the account of his manner of trial.

He procured to himſelf an Æolopile, being an hollow globe of braſs with a very ſlender neck. This he placed in the fire till it became red-hot, and immediately weighed it by a ballance whilſt it remained ſo. Afterwards he let it cool and then weighed it again; and finding its weight to be greater than before, he concluded that the exceſs was the weight of that air which had been expelled by the heat, and had been permitted to return again upon the cooling of the globe. Thus he was ſatisfied that the air was a ponderous body, but in what meaſure it was ſo, he could not by this experiment alone determine. He therefore repeated the trial again, and found the weight of the globe when it was red-hot to be the ſame as before. Then he placed the neck of it under water and ſuffered it to cool in that poſture. Which being done he found his globe to be almoſt filled with water, and knowing the bulk of that water to be the ſame with the bulk of the air which was expelled with the heat, by weighing that water and comparing its weight with the weight of the air found by the former experiment, he concluded the ſpecifick gravity of air to be about 1300 times leſs than the ſpecifick gravity of water.

You

You cannot but take notice of a very great difference between this conclufion and that of *Galilæo*, which I mentioned above. *Galilæo* makes the air to be 400 times lighter than water, and *Merfennus* makes it to be 1300 times lighter. If we take a mean between thefe conclufions we muft fay it is 850 times lighter; and this agrees very well with later and more exact obfervations. You have already feen what the proportion was which we deduced from our experiment. But I ought to tell you that if our bottle had been bigger, we might the more fecurely have depended upon it.

We have formerly in fome of our firft courfes, made ufe of a bottle about 8 times as capacious as that with which we made our prefent trial, and I can affure you we always found the proportion of water to air to be between the proportions of 800 and 900 to 1, but it generally approached fomewhat nearer to 900. That larger bottle was procured for us by the late Mr. *Haukfbee*, who himfelf made the experiment before the Royal Society with the fame, at a time when I happened to be there prefent. I can therefore witnefs for the diligence and care with which he made it. The proportion which was found by his experiment was that of 885 to 1. The method by which he proceeded was the very fame with ours, and therefore I may fuppofe you to underftand it perfectly. But becaufe the recital of it may ferve to fix it the better in your memories, I fhall here fubjoin his own account of the whole procefs.

" I took, fays he, a bottle which held more than " 3 gallons, but how much more, we have no occa- " fion at prefent to take notice of, and of a form " fomething oval; which figure I made choice of " for the advantage of its more eafy libration in wa- " ter. Into this bottle I put as much lead as would " ferve to fink it below the furface of the water. And

" the

" the reaſon why I choſe rather to have the weight " of lead encloſed within the bottle, than fixed any " where on the outſide, was, to prevent the incon- " veniencies which in the latter caſe muſt needs have " aroſe from bubbles of air: for theſe bubbles would " have inevitably adhered to, and lurked in great " plenty about the body of the weight, had it been " placed on the outſide, which muſt have cauſed " ſome errors in the computations of an experiment " that required ſo much exactneſs and nicety. Theſe " things thus provided, the bottle containing com- " mon air cloſed up, was by a wire ſuſpended in " the water, at one end of a very good ballance, and " was counterpoiſed in the water by a weight of " 358½ grains in the oppoſite ſcale. Then being " taken out of the water and ſcrewed to the pump, " in 5 minutes time it was pretty well exhauſted, the " mercury in the gage ſtanding at near 29½ inches. " After which, having turned a cock that ſcrewed " both to the bottle and the pump, and ſo prevent- " ed the air's return into it again, it was taken off " from the pump, and ſuſpended as before, at one " end of the ballance in the water. And now the " weight of it was but 175½ grains; which therefore " ſubſtracted from 358½ grains (the weight of the " bottle with the encloſed air, before it had been " applied to the air-pump) gave for the difference " 183 grains; which difference muſt conſequently " be the weight of the quantity of air, drawn from " the bottle by the pump. Having thus determined " the weight of the exhauſted air, the cock was open- " ed under water, upon which the water was at firſt " impelled with a conſiderable violence into the bot- " tle (though this force abated gradually afterwards) " and continued to ruſh in, till ſuch a quantity was " entered, as was equal to the bulk of the air with- " drawn. And then the bottle, being examined by

" the

" the ballance again, was found to weigh 162132 " grains; from which ſubſtracting $175\frac{1}{2}$ grains (the " weight of the bottle with the ſmall remainder of " included air, after it was taken from the air-pump) " there remains $161956\frac{1}{2}$ grains, for the weight of " a maſs of water equal in bulk to the quantity of air " exhauſted. So that the proportion of the weights " of two equal bulks of air and water, is as 183 to " $161956\frac{1}{2}$, or as 1 to 885.

" There are two things particularly obſervable " in this experiment. Firſt, that in making it after " this manner, one need not be very ſolicitous a- " bout a nice and accurate exhauſtion of the receiv- " er: the ſucceſs of the experiment does not at all " depend upon it; for to what degree ſoever the ex- " hauſtion be made, it muſt ſtill anſwer in propor- " tion to the quantity taken out. Neither can any " more water poſſibly enter into the receiver, than " what will juſt ſupply the place, and fill up the " room, deſerted by the exhauſted air. Secondly, " the ſeaſon of the year is to be conſidered in making " this experiment. I made it in the warm month of " May, the mercury in the barometer ſtanding at " the ſame time at $29\frac{7}{10}$ inches. From whence it is " reaſonable to conclude, that a ſenſible difference " would ariſe, were it to be tried in the months of " December or January, when the ſtate and conſti- " tution of the air is uſually different from what it " is in the forementioned month".

Thus far Mr. *Haukſbee*; and here I might conclude what ſeems to me ſufficient to have been ſaid upon this occaſion. But before I do ſo, it may not be amiſs in this place to make our enquiries once more concerning the ſtate of the atmoſphere and the different degrees by which the air is rarefied at different altitudes above the ſurface of the earth. You remember it was proved in the foregoing week, that the density of the air was diminished in a geometrical progression as the altitude of it was increased in an arithmetical progression. The truth of that rule depends upon this supposition, that the gravity of bodies is the same at all distances from the center of the earth. But it has been proved and put beyond dispute by Sir *Isaac Newton*, in his *Principia*, that the gravity of bodies is not exactly the same at all distances from the center, but is diminished as the distance increases; so that the quantity of it is always reciprocally proportionable to the square of the distance. From hence it easily appears, that when the altitude of the air above the surface of the earth, is very great and very considerable in respect of the earth's semidiameter, the rule which I formerly gave you will be far from being true; but if the altitude be small and inconsiderable, as the altitudes of our highest mountains must be confessed to be, it will still be sufficiently exact, and as such it is proposed by Dr. *Halley* in the Philosophical Transactions, and by Dr. *Gregory* in his Astronomy, and generally received by others without any exceptions. However, it may be worth our while to see what consequences will arise upon the truer hypothesis, which supposes, as I said above, the gravity of bodies to be diminished in the same proportion by which the square of their distance from the center of the earth is increased. In treating of this matter, I fear I shall not be generally understood, yet I hope I shall make the thing as easy as the nature of it will permit.

In FIG. 40, let *C* represent the center of the earth, *CA* its semidiameter, *AB* a part of its surface, and let the line *CAD* be produced up to the extremity of the atmosphere. In this line imagine the points *D*, *E*, *F* to be placed infinitely near to each other, and take as many other points *d*, *e*, *f* in such a manner, that the distances *dC*, *eC*, *fC* shall be reciprocally proportionable to the distances *DC*, *EC*, *FC* respectively, or in such manner, that the distances *dC*, *eC*, *fC* shall be less than the semidiameter *AC*, in the same proportion by which the respective distances *DC*, *EC*, *FC* are greater than the same semidiameter; the distances of the lesser letters from the center being diminished in the same proportion by which the distances of the corresponding greater letters from the center are increased.

Upon the points *A*, *d*, *e*, *f* erect the perpendiculars *AB*, *dp*, *eq*, *fr*, and suppose the length of these perpendiculars to be proportionable to the density of the air in *A*, *D*, *E*, *F* respectively, so that the density of the air at *A* shall be represented by the perpendicular *AB*, the density of the air at *D* by the perpendicular *dp*, the density at *E* by the perpendicular *eq*, and the density at *F* by *fr*.

This being done, I am now to prove, that if the distances *CF*, *CE*, *CD* be taken in a musical progression, and consequently the distances *Cf*, *ce*, *cd*, be in an arithmetical progression, as being reciprocally proportionable to the former distances, the perpendiculars *fr*, *eq*, *dp*, and consequently the densities of the air in the places *F*, *E*, *D*, which are analogous to the perpendiculars, will be in a geometrical progression.

In the first place then, because the distances of the lesser letters from the center, are reciprocally as the distances of their correspondent greater letters from the same, it is manifest that *Cd* is to *Ce* as *CE* is to *CD*, and consequently the difference of *Cd* and *Ce*, is to the difference of *CE* and *CD*, as *Ce* to *CD*, or (because the points *E* and *D* are supposed to be infinitely near to each other) as *Ce* to *CE*, or (because *Ce* is less than *CA* in the same proportion by which *CE* is greater than *CA*, and consequently *Ce*, *CA* and *CE* are continual proportionals) as *CAq* is to *CEq*.

It is evident then, that *de* (the difference of *Cd* and *Ce*) is to *DE* (the difference of *CE* and *CD*) as *CAq* is to *CEq*.

Therefore if the distance *CE* remain unaltered, and consequently the proportion of *CAq* to *CEq* remain unaltered, the proportion of *de* to *DE* will also remain unaltered, and consequently *de* will be as *DE*; that is, *de* will be increased and diminished in the same proportion with *DE*.

But if *DE* remain unaltered, because it is always greater than *de* in the proportion by which *CEq* is greater than *CAq*, it follows that *de* must necessarily be diminished in the same proportion by which *CEq* is increased, and increased in the same proportion by which *CEq* is diminished; or in other words, it must always of necessity be reciprocally as *CEq*.

Whence it follows, that if neither *DE* nor *CE* remain unaltered, *de* will be as *DE* directly and as *CEq* reciprocally.

But the bulk of air between the places *D* and *E* is as *DE*, and the gravitation of the same is reciprocally as the square of *CE*, its distance from the center; therefore *de* is as the bulk and gravitation together of the same; and consequently since *eq* is as its density, the product of *de* and *eq* or the area *deqp* will be as the product of its density, bulk and gravitation, that is, as its force to compress the inferior air.

And the sum of all such areas below *dp* will be as the sum of such forces of all the air above *D*, that is, as *dp* the density of the air at *D*; for you know the density of the air is always as the force which compresses it.

Since the perpendicular *dp* is as the sum of all the little areas below itself, and the perpendicular *eq*, for the same reason, is as the sum of all below itself, it follows that the difference of *eq* and *dp* is as the difference of those sums, which difference is the area *eqpd*.

Thus

Thus far then we have proceeded: we have found that the difference of the perpendiculars *eq* and *dp* is as the area *eqpd* comprehended by thoſe perpendiculars.

Let us now ſuppoſe the diſtances *CF*, *CE*, *CD*, and ſo on, to be taken in a muſical progreſſion, and then, as was ſaid above, the diſtances *Cf*, *Ce*, *Cd*, and ſo on, will be in an arithmetical progreſſion; and therefore all the intervals *de*, *ef* will be equal, and conſequently the areas *eqpd*, which have thoſe equal intervals for their baſes, will be as their altitudes *eq*.

Hence the difference of *eq* and *dp*, which was as the area *eqpd*, will be as *eq*, and conſequently *dp* will be as *eq*. In other words, the two perpendiculars, which terminate the little area included between them, do every where bear the ſame given proportion to each other: that is, the proportion of *fr* to *eq* is the ſame with the proportion of *eq* to *dp*, and conſequently the perpendiculars *fr*, *eq*, *dp*, and ſo on, are in a geometrical progreſſion.

But theſe perpendiculars expreſs the denſity of the air at the places *F*, *E*, *D*, and ſo onwards. Therefore thoſe denſities are alſo in a geometrical progreſſion, which was the thing to be proved.

To proceed further; ſince *Cd* is to *CA* as *CA* is to *CD*, it follows that *Ad* is to *AD* as *CA* to *CD*, or in other words, that *Ad* is leſs than *AD* in the ſame proportion by which the ſemidiameter of the earth is leſs than the diſtance of the point *D* from the center.

Conſequently to find the length of *Ad*, we muſt diminiſh the altitude *AD* in the proportion of the ſemidiameter of the earth to the ſum of the ſemidiameter and the altitude, for which reaſon I ſhall call *Ad* the diminiſhed altitude of the point *D*; and upon the ſame account *Ae* may be called the diminiſhed altitude of the point *E*, and *Af* the diminiſhed altitude of the point *F*; and so if *h* be the point which corresponds as above to the point *H*, *Ah* will be the diminished altitude of the point *H*.

Now it is easy to observe that as the distances *Cd*, *Ce*, *Cf* are in arithmetical progression, so are also the diminished altitudes *Ad*, *Ae*, *Af*.

And from hence there arises this Theorem. That if the diminished altitudes be taken in arithmetical progression, the densities of the air will be in a geometrical progression.

Therefore if the rarity of the air at any one altitude, suppose at *H*, be known, you may easily enough find its rarity at any other altitude, suppose at *D*. For as the diminished altitude of the point *H*, is to the diminished altitude of the point *D*, so will the logarithm of the air's rarity at *H*, which is supposed to be known, be to the logarithm of the air's rarity at *D*, which was to be found.

The whole difficulty of the business is therefore reduced to this; to find the rarity of the air at some one altitude as at *H*. This may be done as I formerly shewed you, by carrying the barometer to the top of some very high mountain, and observing the descent of the quicksilver. Such were the experiments made upon the Puy de Domme in France and Snowdon Hill in Wales, which I made use of the last week when I discoursed of this subject.

But the method I shall now describe to you is more expeditious, and depends upon the experiment which we made this day. It appears, as I said, by many such experiments compared together, that the weight of air is to the weight of water as 1 to about 850. Therefore a column of air whose height is 850 inches or 70 feet and 10 inches, will be equal in weight to a column of water upon the same basis, whose height is 1 inch. Let us suppose that *AH*, the height of the point *H* above the surface of the earth is 70 feet and

10 inches; then becaufe the ftandard height of water in the Pafcalian tube is 34 feet or 408 inches, and this height of water is a ballance to the preffure of the whole atmofphere upon the furface of the earth, it is manifeft that the weight of the whole column of air, which is fuperior to the point *A*, is equal to the weight of a column of water upon the fame bafis, whofe height is 408 inches. Take from the weight of the whole column of air, the weight of that part of the column which reaches from *A* up to *H*, and which was fhewn to be equal to one inch of water, and the weight of the remaining part of the column which is above the point *H*, will be equal to the weight of 407 inches of water. Therefore the force with which the air at *A* is compreffed, is to the force with which the air at *H* is compreffed, as 408 to 407; and the rarity of the air at *H*, is to the rarity of the air at *A*, in the fame proportion.

You may perceive that this method fuppofes the air to be of the fame denfity in every part of the fpace *AH*, which is not exactly true; but in fo fmall an altitude as that of 70 feet, the error is altogether infenfible. However, if you have a mind to proceed with the utmoft accuracy, you may do fo, by making the altitude *AH* as fmall as you pleafe *(a)*.

(a) Both in this and the former folution, pag. 102, it appears, that to find the air's rarity at any propofed altitude, it is neceffary firft to determine it by experiment at fome one altitude. But the Author has improved thefe folutions in his *Harmonia Menfurarum*, fo as to find the rarity at any propofed altitude without that determination; which may be looked upon as no fmall curiofity, and alfo as a particular proof, among many more in that admirable book, of the great excellency of the Author's general method of reducing problems to the Meafures of Ratios and Angles, and thence immediately to the tables of Logarithms, Sines and Tangents; as being the fhorteft and eafieft way, both in theory and practice, to an actual folution. As an inftance of this I will give his folutions of the problem before us, in the form of the following Rules for computation, omitting the geometrical reprefentation of them as requir-

LECTURE XV.

Air is the medium that propagates ſounds: their velocity and manner of propagation.

FROM the experiments we have been making it is very eaſy to infer this concluſion, that the air is that medium by which all ſounds are propagated from ſonorous bodies and conveyed to our ears. As the air was in part exhauſted from our receiver, ſo was the ſound of the bell proportionably diminiſhed; and when we ſeemed to have made an almoſt perfect evacuation, the ſound alſo at the ſame time ſeemed almoſt entirely to ceaſe. On the other hand as the quantity of air was augmented by the condenſer, the ſound received a like augmentation, and as the augmentation of the air became yet greater, ſo did that of the ſound alſo, and ſeemingly in the ſame meaſure. It is therefore highly reaſonable to

ing a previous knowledge of the Author's definitions of the meaſures abovementioned.

Putting *l* for the logarithm of the rarity required at any propoſed altitude *a*, *m* for the conſtant logarithm 0.4342944819, *h* for the height of an homogeneal atmoſphere, reduced every where to the denſity of the air we breath, and *s* for the ſemidiameter of the earth; upon the hypotheſis of an uniform gravity of the air at all altitudes, we have $l = \frac{m}{h} \times a$; but upon the true hypotheſis of a decreaſing gravity, as the ſquare of the diſtance from the center increaſes, we have $l = \frac{m}{h} \times \frac{s}{s+a} \times a$.

In the ſame book the Author has alſo given us a general ſolution of this problem, as ſimple as theſe particular ones, ſuppoſing gravity to be as any given power of the diſtance from the center.

The Author's eſtimate of the height *h* in page 97, is 29254 feet; Sir *Iſaac Newton* makes it 29725 feet, taking his quickſilver to water as $13\frac{2}{3}$ to 1, and water to air as 870 to 1, when that quickſilver is 30 inches high in the barometer. *Phil. Princip.* lib. 2. prop. 50. Schol.

conclude,

conclude, that the air is the true and only medium by which all ſounds are propagated from one place to another; for the cauſe muſt anſwer to its effect, and the effect to its cauſe; as the one is augmented or diminiſhed, ſo muſt the other neceſſarily be augmented or diminiſhed in the ſame proportion.

But it may perhaps be ſaid, that the air concurs only to the production of ſounds and not to their propagation. For poſſibly by the abſence of the air, the ſonorous body may undergo ſuch a change in its parts, as to be rendered incapable of being put into thoſe motions which are abſolutely requiſite to excite in us the ſenſation of ſound. This, I ſay, may perhaps be objected to us, and if we admit the objection, it muſt be confeſſed that our experiments do not prove ſo much as was intended to be collected from them. For my part I muſt acknowledge that the ſuppoſition upon which this objection proceeds, is to me altogether inconceivable; for according to my apprehenſions of the matter, the preſence of the air ſhould rather obſtruct and in time deſtroy thoſe tremulous motions of the ſonorous body, than any way contribute to their production. However, for your further ſatisfaction, I ſhall here add an account of an experiment made ſome years ago by Mr. *Haukſbee*, and publiſhed in the Philoſophical Tranſactions; by which it will appear that ſounds actually produced, cannot be tranſmitted through a *vacuum*.

" I took, ſays he, a ſtrong receiver armed with a " braſs hoop at the bottom, in which I included a " bell as large as it could well contain. This re- " ceiver I ſcrewed ſtrongly down to a braſs plate with " a wet leather between, and it was full of common " air, which could no ways make its eſcape. Thus " ſecured it was ſet on the pump, where it was co- " vered with another large receiver. In this manner " the air contained between the outward and inward

“ receivers was exhauſted. Now here I was ſure, “ when the clapper ſhould be made to ſtrike the “ bell, there would be actually ſound produced in “ the inward receiver; the air in which was of the “ ſame denſity with common air, and could ſuffer no “ alteration by the *vacuum* on its outſide, ſo ſtrong- “ ly was it ſecured on all parts. Thus all being rea- “ dy for trial, the clapper was made to ſtrike the “ the bell; but I found that there was no tranſmiſ- “ ſion of it through the *vacuum*, though I was ſure “ there was actual ſound produced in the inward re- “ ceiver.” And from this experiment he very juſtly concludes, that air is the only medium for the propagation of ſounds.

Now to make us underſtand the manner by which this propagation is performed, philoſophers have generally had recourſe to that very obvious inſtance of a ſtone, or any other heavy body, thrown into a pond of ſtagnating water. For as the ſurface of the water forms itſelf into circular waves, which are ſucceſſively propagated from the ſtone as from a center, and are continually dilated in their progreſs, becoming ſtill greater and greater as they are further removed from the center, till at laſt they reach the banks of the water and there vaniſh, or daſhing themſelves againſt it are reflected back again; ſo they tell us, that the tremulous motion of bodies which is requiſite for the production of ſounds, does excite in the air the like undulations, which are alſo propagated to very great diſtances in ſucceſſive rings, every way incompaſſing the ſonorous body; and theſe undulations meeting with our organs of hearing, impreſs upon them a certain tremor, which does neceſſarily excite in our minds the ſenſation of ſound.

It muſt be allowed, that this example is proper enough to illuſtrate and repreſent to us thoſe inviſible motions of the air, by which the conveyance of founds is made from one place to another. But the comparifon ought not to be carried on too far; for it is very certain it will not hold good in every refpect, and fome philofophers of great note, have overfhot the mark by endeavouring to make out a more exact correfpondence than was needful.

I fhall here take notice only of two particulars, in one of which an agreement may be obferved in thefe motions of air and water; in the other a difagreement. It is eafy to perceive upon the furface of fuch a pond as I have been fpeaking of, that the watry undulations are propagated not only directly forwards, but if any obftacle happen to be placed in their way fo as to obftruct their progrefs, they will bend their courfe about the fides of the obftacle, and dilate themfelves by an oblique motion, into that part of the pond which lies immediately behind the obftacle; which part of the furface of the pond muft have remained perfectly fmooth, and could never have participated of this undulating motion, if it were not otherways propagated than by ftraight lines proceeding directly from the central body which firft excited it. Imagine a partition to be drawn crofs the pond, from the one fide of it to the other, by which it may be divided into two parts, and in the middle of this partition conceive a fmall aperture to be made, by which the water on the one fide of the partition, may have a communication with that on the other fide of it. Then if the water on either fide be put into an undulating motion, the waves will continue to extend themfelves till they arrive at the partition, and there will in part be reflected back again, and in part be permitted to pafs through the aperture; and after their paffage you will perceive them to be regularly dilated from the aperture as from a center, and to fpread themfelves over the whole furface of the pond which lies behind the partition; not by a direct

rect motion from the place in which they were at firſt excited, for this cannot be by reaſon of the partition, but by an oblique and lateral progreſs, their courſe being bent as they paſs through the aperture.

In the ſame manner it may be obſerved, that thoſe undulations of the air by which ſounds are conveyed from place to place, are not only propagated in ſtraight lines, proceeding directly from the ſonorous body, but if any obſtacle happen to be interpoſed, they alſo bend their courſe about the obſtacle, and arrive at the ears of the hearer by an oblique motion. Thus two perſons may very well hold a diſcourſe with each other, though a very high wall be between them, and it is certain the ſound in this caſe is not carried from the one to the other by a direct motion, but after it has aſcended from the ſpeaker to the top of the wall, its courſe is there bent, and ſo it proceeds down again to the hearer.

Thus if a gun be diſcharged on the one ſide of a mountain, we may eaſily hear the ſound of it on the other ſide; though it be very certain that this ſound could never reach our ears unleſs it were propagated obliquely over the top of the mountain, or by the ſides of it. That theſe things are matter of fact every body's own obſervation will convince him. But philoſophers are ſometimes led on by prejudice, to argue even againſt matter of fact. That we may therefore oppoſe them in their own way, it will be worth our while to examine a little into the nature of the thing, and to ſee what concluſion we may poſſibly infer from thence.

In order to this I ſhall in the firſt place endeavour repreſent to you, as clearly and diſtinctly as I can, the manner by which theſe undulations are produced in the air and continued; for thoſe upon the ſurface of water need not here be any further inſiſted on, their oblique propagation ſeeming indeed never to have been called into queſtion. We

We are therefore to underſtand, that the parts of the ſonorous body, being put into a tremulous and vibrating motion, are by turns moved forwards and backwards. Now as they go forwards they muſt of neceſſity preſs upon the parts of the air to which they are contiguous, and force them alſo to move forwards in the ſame direction with themſelves; and conſequently thoſe contiguous parts will at that time be condenſed; then as the parts of the ſonorous body return back again, the parts of the air which were juſt before condenſed, will be permitted to return with them, and by returning they will again expand themſelves. It is manifeſt therefore, that the contiguous parts of the air will go forwards and backwards by turns, and be ſubject to the like vibrating motion with the parts of the ſonorous body.

And as the ſonorous body produces a vibrating motion in the contiguous parts of the air, ſo will theſe parts thus agitated, in like manner produce a vibrating motion in the next parts, and thoſe in the next, and ſo on continually. And as the firſt parts were condenſed in their progreſs and relaxed in their regreſs, ſo will the other parts, as often as they go forwards, be condenſed, and as often as they go backwards, be relaxed. And therefore they will not all go forwards together and all go backwards together; for then their reſpective diſtances would always be the ſame, and conſequently they could not be rarefied and condenſed by turns; but meeting each other when they are condenſed, and going from each other when they are rarefied, they muſt neceſſarily one part of them go forwards whilſt the other goes backwards, by alternate changes from the firſt to the laſt.

Now the parts which go forwards, and by going forwards are condenſed, conſtitute thoſe pulſes which ſtrike upon our organs of hearing and other obſta-

cles

cles they meet with; and therefore a fucceffion of pulfes will be propagated from the fonorous body. And becaufe the vibrations of the fonorous body follow each other at equal intervals of time, the pulfes which are excited by thofe feveral vibrations, will alfo fucceed each other at the fame equal intervals. You fee then that the undulations of the air confift in a fucceffive and interchangable rarefaction and condenfation of its feveral parts, as thofe of water confifted of fucceffive and interchangable afcents and defcents of the feveral parts of the water: the pulfes or denfer parts of the air correfpond to the afcents of the water, and as thofe elevated parts of the water defcend again by the force of their gravity, fo thefe denfer parts of the air expand themfelves again by the force of their elafticity.

This further may be obferved, that though the pulfes are carried on to very great diftances by a direct progreffive motion, yet the fpaces in which the parts of the air perform their vibrations, may be very fmall and inconfiderable. To propagate the pulfes, it is not requifite that the whole body of the air fhould be moved on directly forwards as in the cafe of winds; for by fuch a motion, as was faid above, the feveral parts of the air would always retain their refpective diftances from each other, and confequently they could not be fucceffively and interchangably rarefied and condenfed, and thereby no pulfe would be made. It is therefore abfolutely neceffary that they move backwards and forwards by turns; and how fmall foever the fpace is in which their vibrations are performed, it will be fufficient to caufe a fucceffive condenfation of the parts; in which fucceffive condenfation the progrefs of each pulfe confifts.

It now remains to be proved that thefe pulfes and undulations are propagated not only directly forwards, according to the tendency of the motion of the

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the parts of the ſonorous body, but ſideways alſo, ſpreading themſelves obliquely into the neighbouring regions of the air, which would otherwiſe remain at reſt, as lying out of the tract of their direct motion. This neighbouring air, which borders on the ſides or edges of that tract, being in its natural ſtate of expanſion, will be rarer than the air which makes the pulſes, and denſer than that in the intervals between the pulſes. It muſt therefore neceſſarily come to paſs, that the air of the pulſes will expand itſelf laterally into thoſe parts of the bordering ſpaces which are over againſt the pulſes, and thoſe parts of the bordering air, which are over againſt the intervals of the pulſes, will for the ſame reaſon dilate themſelves laterally into the intervals; and thus the bordering air will become denſer over againſt the pulſes, and rarer over againſt the intervals, and ſo partake of that undulating motion which was at firſt directly propagated from the ſonorous body.

And as this air which immediately borders upon the tract of direct motion, owes its undulations to the air contained in that tract, ſo does it after the ſame manner communicate the like undulations to the air which is next to it on the other ſide, and that communicates the like to the next, and ſo on continually. And thus the undulations are propagated into all the neighbouring regions; not always indeed by a motion proceeding in ſtraight lines from the ſonorous body, but if thoſe regions happen to lie out of the tract of direct motion, or to be placed behind ſome obſtacle, the propagation is made by ſuch a lateral and oblique diffuſion as has been above deſcribed. It is evident therefore from the conſideration of their nature, as well as from experiment and matter of fact, that theſe aery undulations agree with thoſe of water in being propagated not only directly forwards, but alſo obliquely, ſo as thereby to

pervade

pervade the ſpaces which lie behind any obſtacle which may be oppoſed to their progreſs. And the ſame may be ſaid of any other ſort of motion or preſſure which is conveyed by the intervention of any fluid. And this was the firſt of thoſe two particulars which I thought fit to take notice of.

If the account I have been giving of this matter, from the philoſophy of Sir *Iſaac Newton*, may any way contribute to a clearer conception of the manner by which ſounds are tranſmitted through the air, it will be no loſs of time to have inſiſted ſo long upon it. But beſides this advantage, I had further in my view the deciſion of a notable queſtion among philoſophers, concerning the propagation of light. Some have thought that the body of the ſun does all around it, inceſſantly caſt out from itsſelf, with an almoſt incredible ſwiftneſs, thoſe very fine and delicate particles of matter, which after they have traverſed ſo vaſt a diſtance, impreſs upon our organs of ſeeing that peculiar motion which is requiſite to excite in our minds the ſenſation of light. Others on the contrary, and thoſe the more numerous, have believed that the lucifick particles which immediately affect our ſenſe of ſeeing, are by no means themſelves derived from the body of the ſun, but their motion only; and this motion they imagine to be communicated to them from the ſun, by the mediation of a very ſubtle and ætherial fluid, whereof they themſelves make a part.

Des Cartes indeed who held a *plenum*, thought it ſufficient to make the action of light conſiſt in a preſſure only, which he conceived to be inſtantaneouſly conveyed from the ſun by means of that *plenum*. But this notion was afterwards confuted by a ſurpriſing diſcovery of Mr. *Roemer*, who made it evident from obſervations of the eclipſes of Jupiter's ſatellites, that the propagation of light was not inſtantaneous, ftantaneous, though its fwiftnefs was found to be almoft beyond comprehenfion, as requiring no longer a time than that of half a quarter of an hour to pafs from the fun to the earth, which fpace a cannon ball would be 25 years in defcribing.

Hereupon *Hugenius* propofed a new hypothefis, in which he fuppofes this very fwift, but not inftantaneous, motion to be propagated from the fun by fucceffive undulations of the æther, in all refpects alike to thofe of the air, by which the motion of founds is tranfmitted from fonorous bodies. When we take a particular view of the feveral parts of this hypothefis, it appears to be fo very ingenioufly contrived, and fo handfomely put together, that one can hardly forbear to wifh it were true. But it is very manifeft, from the obfervation I have lately been making, that neither this nor any other hypothefis can be fo, which fuppofes the progrefs of light to depend only upon the agitations of a fluid medium, conveyed fucceffively from the luminous body to our fenfes. For if any fuch hypothefis were true, the confequence of it would be this, that light would not only go directly forwards in ftraight lines from the luminous body, but might alfo diffufe itfelf obliquely, after the manner of founds, when any obftacle happened to be interpofed, and fo it might dilate itsfelf into the fpaces which lie behind the obftacle; and thus we fhould have a perpetual day even at midnight, and a total eclipfe of the fun's light would be a thing altogether impoffible. I might here add fome further confiderations concerning the motion of light, but fince I cannot do it without making this digreffion too long, I fhall omit it and return to the fubject I have undertaken.

The fecond particular then, which I propofed to take notice of, was concerning a certain difagreement of the undulations of air and thofe upon the ſurface of water; I mean in reſpect of the velocity with which they are moved forwards. It has been found by many repeated obſervations, and is generally agreed on, that all ſounds are tranſmitted through the air with one certain, determinate velocity; the greatneſs or ſmallneſs of the ſound not in the leaſt contributing to the acceleration or retardation of its motion. Now it might be expected that the undulations of water ſhould alſo, in like manner, ſpread themſelves from the central body, which excited them, how different ſoever the weight, magnitude and force of that central body might happen to be, with one fixed and determinate velocity, though that velocity were different from the velocity of ſounds, as being propagated through a different medium.

Gaſſendus indeed, who imagined a perfect correſpondence between theſe waves of water and thoſe of the air, by which ſounds are conveyed, was of opinion that the matter ſtood thus; but his opinion has juſtly been cenſured by the famous Florentine Academy del Cimento, who found it would not anſwer upon trial. They tell us on the contrary they have obſerved by frequent experiments, that by how much the ſtone is larger and the force greater, wherewith it is thrown into the water, by ſo much the circles approach the ſhore ſwifter.

Sir *Iſaac Newton* in his excellent *Principia Philoſophiæ* has carried this matter ſomewhat further. He conſiders the nature of theſe waves and the manner by which they are formed; and from that conſideration he determines their velocity *a priori*. His concluſion is this, that as theſe circles are excited by a greater force, and conſequently their diſtances from each other become greater, their velocities will be increaſed in a ſubduplicate proportion of their diſtances. Thus if the force be increaſed ſo much, that the diſtances of the waves become four times as great

as

as before, their velocities will be double; if the diſtances become nine times as great, the velocity will be triple; if the diſtances are ſixteen times as great, the velocity will be quadruple, and ſo on. And more particularly he ſhews that the velocity in all caſes is ſuch, that if the length of a pendulum be taken equal to the diſtance of the waves, thoſe waves will deſcribe a ſpace equal to their diſtance whilſt that pendulum performs its vibration. And therefore if the diſtance of the waves be 39.2 inches, they will deſcribe that ſpace in a ſecond of time; but if the diſtance be greater or leſs than 39.2 inches, the ſpace deſcribed in a ſecond of time will be augmented or diminiſhed in the ſubduplicate proportion of that by which the diſtance is augmented or diminiſhed.

His manner of making out theſe concluſions is very curious, and I believe I might render it ſufficiently intelligible; but ſince it is not abſolutely neceſſary to my purpoſe, I rather chuſe to refer you to his book. That which more immediately belongs to our preſent conſideration, is the velocity of ſounds, and this is alſo determined *a priori*, from its cauſes, by the ſame incomparable philoſopher. The train of reaſoning which he makes uſe of on this occaſion is ſo wonderfully ſubtile and requires ſo very cloſe an attention, that I think it not proper in this place to enter into every particular of it; and it may perhaps be ſufficient to underſtand in general the method by which he proceeds.

You have already ſeen, that in order to form thoſe undulations which are requiſite for the conveyance of ſounds, every particle of the air muſt be moved forwards and backwards by turns, within a certain very ſmall ſpace. He goes on yet further, and ſhews that this progreſſive and regreſſive motion is not uniform, but is by degrees accelerated and retarded; and in particular that the laws by which this acceleration and retardation is regulated, are exactly the ſame with thoſe to which the motion of a pendulum is ſubject.

This being demonſtrated, he conceives the atmoſphere to be reduced to ſuch an uniform ſtate, that its denſity in every part of it may be the ſame with the denſity of the air at the ſurface of the earth. The height of the atmoſphere ſo reduced, you remember, was formerly proved to be about $5\frac{1}{2}$ miles. To this height he imagines a pendulum to be equal in length, and then making his enquiry concerning the proportion of the times in which the airy particles and that pendulum perform their reſpective vibrations, by comparing the ſpaces deſcribed and the forces with which they are deſcribed together, he finds that the time of the particles of air, is to the time of the pendulum, as the diſtance of the waves of air from each other, or the latitude of the pulſes, is to the circumference of a circle whoſe ſemidiameter is the length of the pendulum, or that height of the atmoſphere which was before mentioned.

Now the pulſes by going forward deſcribe a ſpace equal to their latitude in the time that each particle of the air performs its courſe.

Therefore the time in which the pulſes deſcribe their own latitude, is to the time in which the pendulum performs its vibration, going forwards and returning back again, as the latitude of the pulſes, is to the abovementioned circumference.

And from hence he deduces this concluſion ; that the velocity of theſe aery pulſes, or which is the ſame thing, the velocity of ſounds, is of ſuch a quantity, as to deſcribe a ſpace equal to the circumference of a circle whoſe ſemidiameter is the height of the atmoſphere, in the time that a pendulum, whoſe length is the ſame with that height, performs its vibration, by going forwards and returning back again. Or to expreſs the ſame thing by a ſomewhat

differ-

different but eafier manner, that the velocity of founds is equal to the velocity acquired by an heavy body in falling from half that height of the atmofphere, fuppofed to be of the fame uniform denfity in all its parts.

From this conclufion he proceeds to his computation, and after all due allowances are made, he finds that the number of feet which founds defcribe in a fecond of time, is 1142; which agrees with the moft exact obfervations. And this again is another full and perfect proof, that the air alone, and not any other more fubtile fluid, which may be imagined to be interfperfed through the body of it, is the proper vehicle of founds.

It muft indeed be confeffed, that thofe who have obferved the motion of founds, have not always agreed in their meafures; but then their difagreement is to be afcribed to a want of exactnefs in the methods they ufed, or to the fmallnefs of the diftances at which their trials were made. I fhall here give a relation of fome obfervations which may beft be depended on.

Caffini, *Picard* and *Roemer*, three excellent members of the French Academy of Sciences, made their experiment at the diftance of about a mile and an half, and found that the fpace defcribed in a fecond of time was 1172 feet. The Florentine Academy del Cimento made their trial at the diftance of about 3 miles, and found that the fpace defcribed in a fecond of time was 1148 feet. Dr. *Halley* and Mr. *Flamfteed*, by an obfervation made at the fame diftance concluded upon 1142 feet. And this laft determination is confirmed by the moft exact enquiries of the Reverend Mr. *Derham* Rector of Upminfter in Effex, and Fellow of the Royal Society, who has lately publifhed in the Philofophical Tranfactions, a particular treatife upon this fubject; giving an account of ſeveral obſervations made by himſelf with the utmoſt care and diligence, for the ſpace of three years together, at various diſtances from one mile to more than twelve. We may therefore very ſafely conclude, that the velocity of ſounds is of ſuch a quantity, as to deſcribe very nearly 1142 feet in a ſecond of time.

I ſay very nearly, becauſe it is certain this velocity may be a little augmented or diminiſhed by favouring or contrary winds, and by heat or cold, notwithſtanding what ſome philoſophers have ſaid to the contrary. It is very well known, that winds are nothing elſe but a body of the air moved forwards, with a direct progreſſive motion. If therefore that body of air be moved the ſame way with the pulſes of ſound contained in it, the pulſes by participating of that motion, will be accelerated, if the contrary way, the pulſes will be retarded; ſo that the velocity of the ſound will in the former caſe be augmented, in the latter be diminiſhed, juſt ſo much as the velocity of the wind amounts to.

The Florentine Academy and ſome others, who have deſignedly made experiments for this purpoſe, have not been able to obſerve that winds had any ſenſible influence upon the velocity of ſounds; and thence it came to be generally believed, that there was not any the leaſt acceleration or retardation upon that ſcore. But Mr. *Derham* has at length undeceived us. He aſſures us, that by many certain obſervations he has found an alteration of ſwiftneſs, which though it be ſmall, is yet ſufficiently ſenſible in thoſe very large diſtances at which he made his trials. He tells us he has alſo made many experiments concerning the velocity of winds, and in particular he ſays, that a ſtorm ſo exceedingly violent as almoſt to overturn a windmill, which ſtood near the place where he made his obſervation, was found by many repeated trials to move not above 66 feet in a ſecond.

Whence

Whence it is eaſy to underſtand that more moderate winds can cauſe but a very ſmall change in the velocity of ſounds.

Let us now go on to conſider the effect of heat and cold. Since by heat the air contiguous to the ſurface of the earth is expanded, it is manifeſt that the height of the atmoſphere, ſuppoſed to be every where of the ſame denſity with this contiguous air, will be increaſed in proportion to that expanſion; and therefore the velocity of ſounds, which is equal to the velocity acquired by an heavy body in falling from half that height, will be increaſed in a ſubduplicate proportion of the ſame expanſion. And the like may be ſaid as to the effect of cold, namely, that the velocity of ſounds will thereby be diminiſhed in a ſubduplicate proportion of the air's contraction.

Hence from ſome obſervations made upon the expanſion and contraction of the air from its greateſt degree of heat in our climate, to its greateſt degree of cold, I find that the middle velocity of ſounds may be increaſed or diminiſhed about a thirtieth part of the whole; and by that means they may move about 38 feet more, or ſo much leſs than 1142 feet in a ſecond of time, accordingly as the ſeaſon is either hot or cold to an extremity *(a)*.

Mr. *Derham* tells us he could never obſerve any

(a) By Mr. *Haukſbee*'s experiments (Phil. Tranſ. N°. 315.) the proportions of the greateſt, middle and leaſt expanſions of common air in this climate, are expreſſed by theſe numbers, 144, 135, 126, which are as theſe numbers 32, 30, 28, as appears by dividing by 4,5. Suppoſing then the middle height of the uniform atmoſphere to contain 30 equal parts, the greateſt height will be 32 and the leaſt 28 ſuch parts. Therefore by what has been ſaid above, the middle velocity of ſounds will be increaſed in the ſubduplicate ratio of 30 to 32, that is, in the ratio of 30 to 31 very nearly; which increaſe is $\frac{1}{30}$ of the middle velocity, or $\frac{1}{30}$ of 1142 feet, or 38 feet in a ſecond of time; and ſo much may the middle velocity be diminiſhed.

change of velocity occaſioned by heat or cold, but we ought not from thence to conclude, againſt the theory, that there is none. I am unwilling to queſtion either his diligence or fidelity, and therefore I chooſe rather to ſay, that poſſibly at the times when he made his trials, the quality of the ſeaſon might not be very intenſe, and conſequently the change might be ſo ſmall as to eſcape his obſervation; which may eaſily be admitted, ſince at the utmoſt it amounts but to the thirtieth part of the whole velocity.

What he further adds concerning the variation, of the height of the mercury in the barometer, namely, that this has no influence upon the motion of ſounds, may be depended on with more ſecurity; for this is alſo confirmed by the theory.

It is certain that the height of the atmoſphere, ſuppoſed to be reduced to the ſame ſtate of denſity with the air we breath in here below, is not any ways altered upon thoſe variations of the barometer. For though the quantity of that uniform atmoſphere be often changed, yet ſetting aſide the conſideration of heat and cold, the denſity of it is always changed in the ſame proportion, and therefore the height of it does always remain unaltered; and conſequently the velocity acquired by falling from half that height, which is equal to the velocity of ſounds, does alſo remain unaltered.

Hence it it is eaſy to underſtand, that the tranſmiſſion of ſounds is equally ſwift through a rarer or denſer air, ſuppoſing the elaſticity of it to be augmented or diminiſhed in the ſame proportion with its denſity; which always comes to paſs, excepting when that proportion is a little diſturbed by heat or cold. I ſhall here conclude what I think ſufficient to have been ſaid concerning the propagation of ſounds.

LECTURE XVI.

Air ſometimes generated, ſometimes conſumed; the nature of factitious airs; exploſions in vacuo, *diſſolutions, fermentations, &c.*

THOUGH the experiments we have been making are ſufficient to convince us, that air may in very great quantities be produced from bodies which ſuffer any conſiderable alteration in the texture of their parts, whether that alteration be made by almoſt inſenſible degrees, as in putrefactions and very ſlow fermentations, or whether it be made more ſwiftly, as in ſome diſſolutions, or even almoſt inſtantaneouſly, as in the exploſion of gunpowder; yet for the wonderfulneſs of it, I ſhould have added one further trial made ſome time ago by Dr. *Slare*, had not the danger of it deterred me from attempting to repeat it. His own account of the experiment is as follows.

" We took, ſays he, half a dram of the oyl of carui-ſeeds and poured it into a little gally-pot, and " put a dram of our compound ſpirit of nitre, in a " ſmall vial, into the ſame gally-pot, and placed over " it a glaſs that held three pints upon Mr. *Papin*'s " exhauſting engine; and having ſoon cleared it of " the air, we turned up the vial in order to ſee what " effect would enſue, in the *vacuum*, upon this mixture; but in the twinkling of an eye the receiver " was blown up, and the mixture in a flame, which " ſtupendous phænomenon ſurpriſed and frightened " us all. Nor did I ever ſee or hear of the like by any " mixtures made in *vacuo*, though I have my ſelf " ſeen a thouſand. For if we look into thoſe many " admirable experiments made by the immortal Mr. " *Boyle*, the removal of the air did almoſt always " extinguiſh light and fire and flame. The blowing

" up

" up of the glaſs does alſo make the experiment the
" more aſtoniſhing, and puzzles one how to account
" for ſo great a quantity of air as was produced from
" theſe liquors, which amounted only to a dram and
" an half; for here was required not only air enough
" to fill up the capacity of the veſſel, but alſo there
" was required ſo great a preſſure within, as did ex-
" ceed that great incumbent weight of air that preſſ-
" ed upon this capacious glaſs without, whoſe dia-
" meter was ſix inches and the depth above eight;
" for otherwiſe it would not have thrown it up into
" the air. If we review and conſider well the phæ-
" nomena of this experiment, we may find the re-
" ſiſtance of ſome hundred weight, that was coun-
" tervailed, and not only ſo, but with a much greater
" force exploded. That it was not produced by any
" expanſion of the common air, for that was ſeen to
" riſe out of the liquors themſelves and was drawn
" out of them in their ſeparate ſtate by the exhauſt-
" ing engine, which ſuffers no elaſtical air to lie con-
" cealed in any liquors. That it was produced in an
" inſtant by the mutual colliſion and agitation of theſe
" active and ſelf-expanding liquors. That it was not
" abſolutely generated *de novo*, but that the air was
" antecedently there, we may reaſonably believe, al-
" though in a very differing ſtate from what it is in
" when in *pleno*. For all that the exhauſting engine
" does, is to deliver the air from a ſtate of compreſ-
" ſion, by leaving it to ſtretch itſelf like a bladder,
" that has full liberty to ſwell up, and has no hard
" body to ſtraighten or oppoſe its expanſion: ſo that
" we have cauſe to conclude our liquors to be fur-
" niſhed with this ſort of air, which being by the
" accenſion of theſe two liquors put to a new and
" violent motion, does expand itſelf *de novo*, and
" to that degree as to anſwer ſo great an effect as is
" above-mentioned. The circumſtances of which
phæno-

"phænomenon will allow me to call this mixture a "ſort of liquid gun-powder". Thus far Dr. *Slare.*

I ſhall now proceed to give you an account of thoſe curious and uſeful obſervations of Mr. *Boyle*, which make up the ſecond Continuation of his Phyſico-Mechanical Experiments. Theſe being the beſt and almoſt only trials which have yet been made concerning factitious airs, are very proper to give us what further light may be had concerning their nature and properties. I ſhall endeavour to make my extract as ſhort as conveniently I can, that it may not be too tedious. Thoſe who are deſirous to ſee the particulars of each experiment, may conſult the book itſelf at their leiſure with greater advantage.

ARTICLE I. *Several ways uſed to help the production of air.*

Bread by itſelf does not readily produce any air in *vacuo*, but being very much moiſtened and a little kneeded, it yields a ſufficient quantity; and thence it was concluded, that water is a fit diſſolvent to draw forth air out of bread. The experiment was alſo tried by burning it in *vacuo* with a burning-glaſs, and by this means much air was generated, which did ever and anon break out as by fulmination. And from theſe trials it was thought probable, that the air contained in bread is ſo cloſely confined, that no eaſy operation can give it a full diſcharge; but if any thing could diſſolve and looſe that knot, it might then produce great effects.

Dried grapes bruiſed and put into water, being included in a receiver produced much more air than others without water. It appears therefore, that water is a fit medium to elicit air out of them; but it was obſerved, that the production of air did not begin preſently upon the affuſion of water, but that it proceeded on with greater ſwiftneſs after the parts

of

of the water in five or six days time had more deeply sunk into and pervaded the grapes.

Pears were included in two different receivers in *vacuo*, and it was found that in one of them, which was exposed to the rays of the sun, much more air was generated in the same time than in the other. Whence it was conjectured, that the production of air is very much promoted by the heat of the sun.

Bruised grapes in *vacuo* with spirit of wine produced more air than without that spirit. Whence it appeared, that spirit of wine doth advance the production of air from bodies included in *vacuo*; though by other experiments it appears wholly to hinder that production from bodies included in common air.

From experiments made with apples both boiled and raw, both with sugar and without it, in larger and smaller receivers, it was concluded that sugar, the crudity of the fruit, and the largeness of the receiver do all contribute to the production of air.

ART. II. *Several ways to hinder the production of air.*

Paste made with bread-corn-meal, without leaven, was put into an empty receiver, and afterwards the receiver was placed in a certain apartment with a good fire in it, yet the paste produced no air in ten hours space. Hence it was thought, that if paste hath once suffered too much cold, it can scarce recover its faculty of fermenting. For at another time paste made without leaven in the summer season, produced very much air in a short time in *vacuo*.

Dough kneeded with leaven had a quantity of spirit of wine poured upon it, to try whether fermentation would be hindered by that means, and it was concluded by the event, that the spirit did hinder the production of air.

By

By ſome trials upon pears it was collected, that fruits included in a receiver, with very much compreſſed air in it, cannot produce ſo great a quantity of factitious air as in a medium leſs denſe. It was alſo further collected that artificial air is ſometimes produced by iterated turns, and as it were by reciprocations, and that the changes of heat and cold, though they are not the ſole cauſes of ſuch reciprocations, yet ſeem to contribute much thereunto.

The ſame things were concluded from experiments made with paſte. Raiſins of the ſun ſteeped in vinegar, were placed in an emptied receiver, and it was thereby found, that vinegar was an hindrance to fermentation and the production of air.

Plums and apricots, many of them being cut aſunder, were placed in two receivers, in one of which was air produced from cherries, and in the other common air; and it was found by this experiment, that the artificial air of cherries was a great hindrance to the apricots, that they could not produce air; yet notwithſtanding, it was thought to advance the alteration of their colour and firmneſs, and to be good to preſerve their taſte.

Grapes included in common air with ſpirit of wine and without it, ſhewed that in common air the ſpirit of wine doth hinder fermentation; though by other experiments it was found to promote it in *vacuo*.

Some peaches were included in an exhauſted receiver, and together with them ſome ſpirit of wine, which could not touch the peaches unleſs it were elevated in the form of vapours. A like quantity of peaches was alſo included in another unexhauſted receiver without ſpirit of wine. And it was found by this experiment, that the very vapours of ſpirit of wine, do ſomewhat hinder fermentation and the production of air; but much leſs than the ſpirit itſelf.

From experiments made upon paſte with leaven and

and without it, with ſpirit of wine and without it, it was concluded, that leaven doth rather hinder than help the production of air, if the paſte be not made in a place hot enough; and that ſpirit of wine doth very much prejudice the production of air, and the rather if the paſte be wrought with the ferment; and moreover that paſte without ferment in tract of time will produce no leſs air than paſte with ferment.

New ale was included in a receiver exactly filled that ſo no air might be left, and another quantity of the ſame ale was included in another receiver wherein ſome room was allowed for the air; and from the experiment it ſeemed to follow, that ale if the air be excluded from the veſſel, will ferment more ſlowly than if ſome air were left in; yet in tract of time, it makes a greater compreſſion if no place be left for its dilatation.

Green peaſe in an emptied receiver with ſpirit of wine and without it, ſhewed that ſpirit of wine doth hinder the production of air from peaſe.

ART. III. *The effects of artificial air are different from the effects of common air.*

From two experiments made upon cherries it was concluded, that in artificial air fruits do produce leſs air, and ſo they keep their colour and their taſte better: it was alſo obſerved, that cherries do contain much air in them, and that they produce it very irregularly.

A trial was made with unripe grapes in common air and in factitious air produced from pears, and it was from thence concluded that factitious air was fit to alter the colour of fruit and to preſerve its taſte.

A mixture of common air and air produced from cherries, was found to preſerve oranges better than common air alone.

Two pieces of beef were placed in different receivers,

ceivers, in one of which was common air, in the other cherry-air; and it was concluded from the comparifon of the events, that cherry-air is a great hindrance to the production of air from flefh.

Two onions were put into a receiver full of common air, to fee whether vegetation would increafe the quantity of air or diminifh it. Two other onions were put into a receiver with air produced from pafte; and it was gathered from the event, that artificial air doth not at all hinder vegetation, and that not only the fenfible bignefs of the body, but alfo the quantity of air is increafed by vegetation.

Unripe grapes were included in common air and in factitious air of pears; and the comparifon confirmed the efficacy of artificial air to alter the colour of fruits. But it was obferved in this experiment, that it prejudiced the prefervation of tafte, and promoted the production of air, contrary to what had happened in fome of the former experiments.

Gilliflowers were included in three different receivers, one of which was exhaufted, another had common air in it, and the third contained artificial air of pafte; and it was obferved that factitious air renders the change of colour more fpeedy, yet it prevents mouldinefs even as a *vacuum* doth the fame.

From an experiment made in two receivers with common air and with artificial air of cherries, it was found, that the alteration of colour and firmnefs in apricots is promoted by cherry-air, and that fome part of fuch air is deftroyed in the beginning.

Plums cut afunder were put in three different receivers, the one exhaufted, another full of goofberry-air, the third full of common air; and by this experiment artificial air feemed to have promoted alteration.

Some peaches were put into a receiver with common air mixed with air produced from grapes, and the grapes themſelves were included in the ſame receiver, that the common air might be the better ſaturated with the artificial; and from the circumſtances of this experiment it was concluded, that common air doth corrupt bodies, yet it doth ſo much leſs, if it be mixed with factitious air.

Equal parts of pears cut aſunder were placed in four different receivers, one of which was full of common air cloſed up, another was full of common air but not exactly cloſed, another contained cherry air, and the laſt was evacuated. It was obſerved, that corruption doth not begin in free air ſooner than in included air; but when it is begun, it is much more increaſed and more ſpeedily, the included air ſeeming to be ſooner ſatiated. The aptitude of artificial air for the ſoftning of fruits was alſo obſerved. And it ſeemed probable, that the production of air was here promoted by the artificial air, though it had ſucceeded otherwiſe with apricots in the other trials.

Apricots were included in four different receivers, one had common air and was cloſed, another had common air but was left open, the third had a mixture of air produced from pears, the fourth contained common air but pretty much compreſſed. It was from hence concluded, that the quantity of corruption doth depend on the quantity of air, and alſo that in factitious air alteration is made quicker, but in tract of time the corruption is far greater in common air.

Art. iv. *The effects of compreſſed air are different from the effects of common air.*

Onions ſet to grow in common and condenſed air, ſhewed, that a little compreſſion doth not prejudice

thoſe

those bodies which are to be expanded by vegetation.

Tulips and lark-spurs placed in common and condensed air shewed, that compression in some plants doth hinder putrefaction and moulding.

The two halfs of an orange were included in compressed and common air; and it was confirmed by this experiment, that compressed air may somewhat retard corruption, yet in progress of time it was made probable by other experiments, that the quantity of corruption doth depend upon the quantity of air.

Equal quantities of roses were included in common and compressed air; and from hence it seemed to follow, that compressed air is something fitter for the alteration of colour than common air.

The two halfs of an orange were again included in common and compressed air; and from the circumstances of the experiment it was concluded, that the quantity of mouldiness doth depend on the quantity of air.

Two mice were included in common and compressed air; and it was found that the mouse in the common air had consumed something of that air. By comparing the times they each lived under their confinement, it was judged that compressed air was fitter than common air for the prolongation of life; but it is to be observed that this compression was not very great.

The experiment was tried with flies, and they seemed not to be sensible of a small compression, nor indeed are they much prejudiced by a rarefaction of the air, unless there be an almost compleat *vacuum*.

The experiment was made also with frogs, but nothing could with certainty be concluded from it.

Another trial like the former was made with a dissected orange, and it was again confirmed, that the quantity of mouldinefs doth depend on the quantity of air. But it was obferved, that the mouldinefs did appear a little later in the compreffed air than in the common, though afterwards it increafed much more.

The like was again concluded by experiments made upon rofes, the parts of a limon and upon gilliflowers.

A moufe was put into a receiver with common air, only to try whether he would produce or confume air. From the event it was concluded, that living animals confume air, but dead ones produce it.

Pears of the fame fort were included in compreffed air, in common air and in *vacuo*; and it feemed to follow from the event, that in a great compreffion a lefs quantity of air is produced.

From fome other experiments made upon animals, it was found that a very great compreffion of air is noxious and even mortiferous to them.

ART. V. *The effects of artificial air upon animals.*

A bee, with diftilled vinegar and pulverized coral, were put into a receiver, and the air being wholly exhaufted, matters were fo ordered that the coral fell down into the glafs of vinegar. But the air produced from thence, did not reftore any power of motion to the bee; but when fhe was expofed to the open air, in a little time fhe began to move herfelf. Hence it was fufpected that artificial air is unfit for the life of animals.

Two flies were included in a receiver out of which the common air being exhaufted, fome goofberry-air was made to fupply its place. Afterwards two other flies were included in *vacuo*, but with this difference, that common air was reftored to thefe latter flies. The event was, that the latter flies recovered

vered thereby their power of motion, which they had loſt in *vacuo*, but the former in the factitious air remained irrecoverably dead. The experiment was repeated with the ſame ſucceſs; and this was thought to be an high confirmation that artificial air is noxious to the life of animals.

Three receivers being filled with air produced from paſte, a perfumed cone was kindled and put into one of them, which being ſtopped, the fire within a minute of time went out. Then by blowing with a pair of bellows, the artificial air was expelled from the receiver, and the fire was again put into it as before, and now it burned bright for a pretty long time, though the receiver was ſhut as ſpeedily and as accurately as before. Into the ſecond receiver a fly was put, and preſently ſeemed to be dead, but being expoſed to the ſun, ſhe recovered again in a ſhort time. Then common air being blown into the receiver, the fly included as before ſuffered no inconvenience thereby. The ſame experiment was tried with the ſame fly in the third receiver, being filled with artificial air, and the ſame ſucceſs followed, excepting that the fly being now longer included, could not ſo ſoon as before be recovered to health again. Hence it appeared that artificial air is not only prejudicial to the life of animals, but to flame alſo.

Several other ſuch experiments were tried with various animals, from whence it was concluded as before, that factitious air is very hurtful to their life; but if mixed with common air it doth not ſo readily produce its effects; it appearing to be ſo much the more hurtful as it is the more free from that mixture. It was alſo made evident, that factitious air is a greater enemy to animals than a *vacuum* itſelf, and thence it was collected that it kills by ſome venemous quality, and not only by the defect of common air. Air produced from cherries was found to be somewhat less hurtful to frogs than that produced from paste; air produced from goosberries less hurtful to mice than air produced from gun-powder; air produced from pease less hurtful to snails than air produced from paste.

Art. VI. *Animals in vacuo.*

A butterfly being put into an emptied receiver was almost three hours before it was deprived of its faculty of motion. Then the air being let in, it recovered itself again. After this it was bound by one of its horns with a thread, and so it was suspended in the receiver, and it was carried very freely from one part of it to another by clapping its wings; but after the air was extracted again, the clapping of its wings was in vain, for it could not move the thread in the least from being perpendicular.

By an experiment made upon flies in very much rarefied air, it was concluded that a small quantity of air may suffice for insects to breath in.

Snails were included so long in *vacuo* till they seemed to have lost all power of motion, and in that state they produced some air, though they were not so perfectly dead as to be past recovery.

Fly-blowings or the eggs of flies were placed under a receiver in air much rarefied, and it was found by the event, that insects may be produced and may live, if not in *vacuo*, yet at least in highly rarefied air.

By another experiment of the like nature it was concluded, that insects could not be generated and live in *vacuo*, though they might in rarefied air; which thing was also confirmed by a farther experiment.

Vinegar

Vinegar full of thofe very fmall eels which may be difcovered in it by microfcopes, was for fometime included in *vacuo*, and another part of the fame vinegar was kept in the open air; the eels which had been kept in *vacuo* were all found to be dead, though the others in the open air were as brifk as at firft. Hence it was evident, that even thofe very diminutive animals are alfo affected with the prefence and abfence of the air.

Art. VII. *Contains fome experiments concerning the confumption of fuel by fire in compreffed air.*

It was concluded from thofe experiments, that the quantity of matter confumed in a given fpace of time did nearly anfwer to the quantity of compreffed air.

Art. VIII. *Fire ufed to produce air.*

Paper befmeared with fulphur was burnt in *vacuo*, and fome air was thereby produced, which was not at all diminifhed for two whole days. This air was afcribed to the paper, for it was found by other trials, that no air is produced out of fulphur alone.

Some air was produced from harts-horn burnt in *vacuo*, but part of this air was in a fhort time deftroyed again, and the other part which preferved its elafticity for a full hour after the burning-glafs was removed, feemed afterwards not to lofe it at all.

Amber produced no air even by being burnt.

Camphire in *vacuo* was placed over a digefting furnace, and though it was fublimated into flowers, yet no air was produced.

Sulphur vivum was melted in *vacuo* by a burning-glafs; but the fumes of it did not appear to contain any air.

Paſte that had been included in *vacuo* for nine days and ſeemed to have emitted all its air, was endeavoured to be fired with a burning-glaſs. The ſubſiding fumes had tinged the ſurface of the paſte with a curious yellow colour, and it was conjectured that ſome air was produced.

Art. IX. *Concerning the production of air in vacuo.*

Dried grapes and dried figs were placed in *vacuo*, and it was concluded from the event, that dried fruits in *vacuo* produce very little air.

Apricots appeared to produce their air almoſt as eaſily in their wonted preſſure as in *vacuo.*

By comparing the events of cherries in *vacuo*, when whole and when diſſected, it was concluded, that ſome diſſected fruits do ſooner produce their air than whole and undivided ones.

Cabbages cut in pieces were put into an emptied receiver, and it was thought from the circumſtances of the experiment, that bodies when they putrify, have already produced almoſt all their air.

The ſame was confirmed by another experiment made upon apples.

Two equal quantities of milk were put into two glaſs receivers of equal bigneſs; the one was left in the free air, the other was evacuated. And it was obſerved, firſt, that the coagulation of milk, when the air is extracted therefrom, is ſomewhat retarded. Secondly, that the butter, whey and cheeſe are mixed with one another confuſedly in the air, but in *vacuo* they keep their diſtinct places, and one ſwims upon the top of the other. Thirdly, that the putrefaction of milk is hindered or very much retarded in *vacuo.* Laſtly, that milk by long continuance in *vacuo*, is made unfit to generate worms, even in common air.

A like experiment was made with urine. By comparing the quantity of air produced in this experiment, with that produced in the former, it ſeemed, that urine, which is an excrementitious humour, contains leſs air in it than milk which is an alimental humour. And moreover the efficacy of the air to corrupt urine was here very obſervable.

Paſte very much diluted and without leaven, being put into a glaſs veſſel, was placed in an emptied receiver, and though the veſſel which contained it were not half full before all the air was exhauſted, yet the ſame day the paſte had ſwollen above the brims of the veſſel. The next day the paſte continued to ſwell more and more, and was interſperſed with many cavities. The third day the paſte was much more tumid than before, and much air was generated from it. The fourth day in the morning the cover was found to be ſeparated from the 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 it was thought that bread thus made was very light.

A quantity of beef was put into an exhauſted receiver, and it was concluded from the event, that fleſh, whilſt it putrifies, doth produce much more air than before it putrifies, though the contrary was before obſerved of fruits.

From an experiment made upon gooſberries it ſeemed to follow, that theſe fruits after they have produced all their air, admit very little alteration; as if that air itſelf were the cauſe of corruption.

By an experiment made upon dried plums it was confirmed, that dried fruits are very unfit to produce air.

A trial was made with nut-kernels, and it appeared, that air may without ſenſible putrefaction be produced from fruits even of an hard conſiſtence.

ART. X. *Concerning the production of air above its wonted preſſure.*

An experiment made with gooſberries ſeemed to prove, that gooſberries contain much air in them, which as ſoon as it is freed from the uſual preſſure, 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.

An experiment made with paſte ſeemed to prove, that air may be produced out of paſte in compreſſed air as well as in *vacuo.*

Horſe-beans contain much air, which they produce very irregularly, both in *vacuo* and under a moderate preſſure.

Gooſberries produce their air regularly enough unleſs ſomething be extracted out of the receiver, for then they acquire ſtrength to produce new air more ſpeedily.

Grapes produce not all their air but in a long tract of time.

Pears ſeemed to produce their air, as it were by paroxyſms or fits.

ART. XI. *Various experiments.*

Melted lead and melted tin produced no air in *vacuo.* It was obſerved by the way, that the ſurface of theſe melted metals which were included in a braſs veſſel, was concave in *vacuo* after concretion, though in the common air it be convex.

Water

Water ſaturated with ſalt was placed in *vacuo*, to try whether it would there be converted into cryſtals, as is uſual in the free air, but it was found it would not.

Air produced from gooſberries was put into an evacuated receiver furniſhed with a mercurial gage. It was found that in the ſpace of half a year, no change was made in the height of the mercury, and conſequently the ſpring of this factitious air was not altered in ſo long a time.

A vial capable of containing 7 ounces, 5 drams and 3 grains of water, was exhauſted of its air and weighed; then the bladder which covered its orifice was pierced with a needle, and thus being filled with air again, it was found to be $4\frac{1}{2}$ grains heavier than before, whence it followed that water was about 800 times more ponderous than an equal bulk of air.

Aqua-fortis with fixt nitre were placed in a receiver, which being exhauſted, the one was poured into the other, and much air was thereby produced.

Spirit of wine was found to be very ſenſibly condenſed by a moderate degree of cold, but not at all by a very great compreſſion.

Spirit of wine and oil of turpentine were cleanſed of air, then a quantity of the ſpirit of wine being put into a glaſs, ſome drops of the oil of turpentine were ſuperadded to it, which ſwimming upon the ſpirit, were whirled about as is uſual by an odd motion. Afterwards the veſſel was placed in an exhauſted receiver, and though no ebullition was made, nor any bubbles appeared, yet the drops continued to be moved in *vacuo* as in the open air. Hence it ſeemed to follow, that the cauſe of the motion of the drops is not to be aſcribed to the diſſolution of them, for all diſſolutions in *vacuo* are wont to produce bubbles,

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A glaſs containing ſpirit of ſal-armoniac and the filings of copper was placed in *vacuo*. In a month's time the blew colour given to the ſpirit by the copper was almoſt quite vaniſhed, but upon the admiſſion of the air it quickly returned.

A mixture of aqua-fortis and ſpirit of wine was diſtributed equally into three glaſſes, into which three equal pieces of iron were put. One of theſe veſſels being included in *vacuo*, a great many ebullitions were made in it. After a quarter of an hour the veſſel was taken out again, and the liquor was found to be black and turbid, whereas the other two veſſels had their liquor not altered in colour, but only ſome black powder did appear at the bottom of them.

Spirit of ſal-armoniac with filings of copper were again placed in *vacuo*, and after the ſpirit had ceaſed to emit any bubbles, the filings were mixed therewith, which cauſed many bubbles to break forth again; but they were ſo far from producing any air, that on the contrary they conſumed that which was there before.

Art. XII. *Artificial air deſtroyed.*

Air produced from cherries was tranſmitted into a receiver full of common air. It was concluded from the event, that air produced from fruits, at the beginning is in part deſtroyed; but the reſt can keep the form of air very long.

Sal-armoniac was put into a receiver with a ſufficient quantity of oil of vitriol, then the air being exhauſted, the ſalt was put into the oil, whereupon a great ebullition preſently followed, and the mercury in the gage ſhewed a good quantity of air to be generated; but this by the ſame gage ſoon after appeared to be deſtroyed again. The experiment was repeated, and both the production and deſtruction were

were ſlower than before. Afterwards oil of vitriol alone was put into a receiver, in which only a fifth part of air was left to, try whether the oil without ſal-armoniac would diminiſh the elaſtical force of the air; but it fell out contrary, that the force of the air was increaſed. It was confirmed by theſe trials, that ſome artificial airs may be deſtroyed, but it was thought to deſerve a further enquiry, why this deſtruction happens ſometimes ſooner and ſometimes later.

ART. XIII. *Experiments concerning the different celerity of air produced in vacuo or in common air.*

From theſe experiments made with paſte, the kernels of filberds, raiſins of the ſun and onions, it was concluded, that ſome bodies do more eaſily produce their air in *vacuo* than in common or rarefied air.

ART. XIV. *The difference betwixt whole or entire, and bruiſed fruits.*

Bruiſed pears did not produce air ſo ſoon as entire ones. The ſame was found to hold as to bruiſed apples and unripe grapes bruiſed; but ripe grapes bruiſed had the contrary effect. By another experiment upon apples it was concluded, that bruiſed fruits do produce leſs air in *vacuo* than ſound ones, contrary to what happens in the common air. The reaſon whereof was thought to be this, that bruiſed fruits 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.

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ART. XV. *Contains some experiments,*

By which it seemed, that the air at divers times is diversly affected; so that sometimes it hath a power to hinder corruption and sometimes to promote it; sometimes it readily produces mouldiness, at other times it is unfit for that purpose.

ART. XVI. *Contains experiments,*

By which it appears, that some bodies, even in vessels hermetically sealed, may lose part of their weight by being exposed to the beams of the sun concentered with a burning-glass.

ART. XVII. *Of the preservation of bodies in compressed liquors.*

Many experiments are contained under this article; the conclusions made from them are as follow.

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.

That liquors may grow sour though no spirits have evaporated from them.

That fruits cannot be long kept in pulp of apples by reason of the great production of air.

That the juice of crude grapes cannot conveniently be used for the preservation of fruits, for the same reason.

That fermented liquors may be useful for the preservation of fruits, as being unfit to produce air.

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

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to enter into and pervade the pores of the beer by degrees.

That water as well as beer may conduce to the preſervation of fleſh.

That fiſhes produce leſs air than fleſh, and yet that they will be corrupted, though they be fortified againſt the air.

That butter may be kept a great while if it be defended from the contact of the external air.

That corruption may ſometimes happen without production of air.

That even tender bird's fleſh may be preſerved long by the help of beer or ale.

That ſugar is not ſo fit for the preſervation of fruits as fermented liquors.

That milk may ſometimes be uſed with good ſucceſs for the preſervation of fleſh.

That butter melted and hot is not ſo ſucceſsfully uſed for the preſervation of fleſh.

That fleſh after it is boiled, may be kept long without prejudice, which is a great convenience at ſea, ſo that perhaps there may be no need of ſalted fleſh. For after the raw fleſh hath been kept ſo long in veſſels ſtopped with ſcrews, as experience ſhews 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. 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 (*a*).

(*a*) The reaſon why ſpirit of wine preſerves fleſh, and other things immerſed in it, from corruption may be, that the ſpirit ſucks up and conſumes the air lodged in the pores of the fleſh. For it has been found that ſpirit of wine will imbibe a bubble of air as large as your thumb in about two hours, which is more than water will do in a much longer time, though it be firſt well purged of air by boiling it.

ART. XVIII. *Contains experiments concerning elixation and diſtillation in vacuo.*

ART. XIX. *Contains ſome experiments concerning elixation in veſſels ſtopped with ſcrews.*

By which it appears, that even harts-horn and the bones of fiſhes and four-footed creatures may be ſoftened and converted into good nouriſhment.

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APPENDIX.

NUMB. I.

The reaſon of the riſing and falling of the mercury in the barometer, upon change of weather, by Dr. Halley (*a*).

TO account for the different heights of the mercury at ſeveral times, it will not be unneceſſary to enumerate ſome of the principal obſervations made upon the barometer.

1. The firſt is, that in calm weather, when the air is inclined to rain, the mercury is commonly low.

2. That in ſerene, good, ſettled weather the mercury is generally high.

3. That upon very great winds, though they be not accompanied with rain, the mercury ſinks loweſt of all, with relation to the point of the compaſs the wind blows upon.

4. That *cæteribus paribus*, the greateſt heights of the mercury are found upon eaſterly and north-eaſterly winds.

5. That in calm froſty weather the mercury generally ſtands high.

6. That after very great ſtorms of wind, when the quickſilver has been low, it generally riſes again very faſt.

7. That the more northerly places have greater alterations of the baroſcope than the more ſoutherly.

8. That within the tropicks and near them, thoſe accounts we have had from others, and my own obſervations at St. Helena, make very little or no vari-

(*a*) Reprinted from *Lowthorp*'s Abridg. of the Philoſophical Tranſactions, vol. II. pag. 20.

ation of the height of the mercury in all weathers.

Hence I conceive, that the principal cauſe of the riſe and fall of the mercury, is from the variable winds which are found in the temperate zones, and whoſe great inconſtancy here in England is moſt notorious.

A ſecond cauſe is the uncertain exhalation and precipitation of the vapours lodging in the air, whereby it comes to be at one time much more crouded than at another, and conſequently heavier; but this latter in a great meaſure depends upon the former. Now from theſe principles I ſhall endeavour to explicate the ſeveral phænomena of the barometer, taking them in the ſame order I laid them down.

1. The mercury's being low inclines it to rain, becauſe the air being light, the vapours are no longer ſupported thereby, being become ſpecifically heavier than the medium wherein they floated; ſo that they deſcend towards the earth, and in their fall, meeting with other aqueous particles, they incorporate together and form little drops of rain. But the mercury's being at one time lower than at another, is the effect of two contrary winds blowing from the place where the barometer ſtands; whereby the air of that place is carried both ways from it, and conſequently the incumbent cylinder of air is diminiſhed, and accordingly the mercury ſinks. As for inſtance, if in the German ocean it ſhould blow a gale of weſterly wind, and at the ſame time an eaſterly wind in the Iriſh ſea, or if in France it ſhould blow a northerly wind, and in Scotland a ſoutherly, it muſt be granted me that, that part of the atmoſphere impendent over England would thereby be exhauſted and attenuated, and the mercury would ſubſide, and the vapours which before floated in thoſe parts of the air of equal gravity with themſelves, would ſink to the earth.

2. The greater height of the barometer is occasioned by two contrary winds blowing towards the place of obſervation, whereby the air of other places is brought thither and accumulated; ſo that the incumbent cylinder of air being increaſed both in height and weight, the mercury preſſed thereby muſt needs riſe and ſtand high, as long as the winds continue ſo to blow; and then the air being ſpecifically heavier, the vapours are better kept ſuſpended, ſo that they have no inclination to precipitate and fall down in drops; which is the reaſon of the ſerene good weather, which attends the greater heights of the mercury.

3. The mercury ſinks the loweſt of all by the very rapid motion of air in ſtorms of wind. For the tract or region of the earth's ſurface, wherein theſe winds rage, not extending all round the globe, that ſtagnant air which is left behind, as likewiſe that on the ſides, cannot come in ſo faſt as to ſupply the evacuation made by ſo ſwift a current; ſo that the air muſt neceſſarily be attenuated when and where the ſaid winds continue to blow, and that more or leſs according to their violence; add to which, that the horizontal motion of the air being ſo quick as it is, may in all probability take off ſome part of the perpendicular preſſure thereof: and the great agitation of its particles is the reaſon why the vapours are diſſipated, and do not condenſe into drops ſo as to form rain, otherwiſe the natural conſequence of the air's rarefaction.

4. The mercury ſtands the higheſt upon an eaſterly or north-eaſterly wind, becauſe in the great Atlantick ocean, on this ſide the 35th degree of north latitude, the weſterly and ſouth-weſterly winds blow almoſt always Trade, ſo that whenever here the wind comes up at eaſt and north-eaſt, it is ſure to be checked by a contrary gale as ſoon as it reaches the o-

cean; wherefore, according to what is made out in our ſecond remark, the air muſt needs be heaped over this iſland, and conſequently the mercury muſt ſtand high, as often as theſe winds blow. This holds true in this country, but is not a general rule for others where the winds are under different circumſtances; and I have ſometimes ſeen the mercury here as low as 29 inches upon an eaſterly wind, but then it blew exceeding hard, and ſo comes to be accounted for by what was obſerved upon the third remark.

5. In calm froſty weather the mercury generally ſtands high, becauſe (as I conceive) it ſeldom freezes but when the winds come out of the northern and north-eaſtern quarters, or at leaſt unleſs thoſe winds blow at no great diſtance off; for the northern parts of Germany, Denmark, Sweden, Norway, and all that tract from whence north-eaſtern winds come, are ſubject to almoſt continual froſt all the winter; and thereby the lower air is very much condenſed, and in that ſtate is brought hitherwards by thoſe winds, and being accumulated by the oppoſition of the weſterly wind blowing in the ocean, the mercury muſt needs be preſt to a more than ordinary height; and as a concurring cauſe, the ſhrinking of the lower parts of the air into leſſer room by cold, muſt needs cauſe a deſcent of the upper parts of the atmoſphere to reduce the cavity made by this contraction to an *æquilibrium*.

6. After great ſtorms of wind, when the mercury has been very low, it generally riſes again very faſt. I once obſerved it to riſe $1\frac{1}{2}$ inch in leſs than 6 hours after a long continued ſtorm of ſouth-weſt wind. The reaſon is, becauſe the air being very much rarefied, by the great evacuations which ſuch continued ſtorms make thereof, the neighbouring air runs in the more ſwiftly to bring it to an *æquilibrium*; as we

we ſee water runs the faſter for having a great declivity.

7. The variations are greater in the more northerly places, as at Stockholm greater than at Paris (compared by Mr. *Paſcall*) (*a*) becauſe the more northerly parts have uſually greater ſtorms of wind than the more ſoutherly, whereby the mercury ſhould ſink lower in that extream; and then the northerly winds bringing the condenſed and ponderous air from the neighbourhood of the pole, and that again being checked by a ſoutherly wind at no great diſtance, and ſo heaped, muſt of neceſſity make the mercury in ſuch caſe ſtand higher in the other extream.

8. Laſtly, this remark, that there is little or no variation near the equinoctial, as at Barbadoes and St. Helena, does above all others confirm the hypotheſis of the variable winds being the cauſe of theſe variations of the height of the mercury; for in the places above named there is always an eaſy gale of wind blowing nearly upon the ſame point, viz. E.N.E. at Barbadoes, and E.S.E. at St. Helena, ſo that there being no contrary currents of the air to exhauſt or accumulate it, the atmoſphere continues much in the ſame ſtate: however upon hurricanes (the moſt violent of ſtorms) the mercury has been obſerved very low, but this is but once in two or three years, and it ſoon recovers its ſettled ſtate of about $29\frac{1}{2}$ inches.

The principal objection againſt this doctrine is, that I ſuppoſe the air ſometimes to move from thoſe parts where it is already evacuated below the *æquilibrium*, and ſometimes again towards thoſe parts where it is condenſed and crouded above the mean ſtate, which may be thought contradictory to the laws of ſtaticks, and the rules of the *æquilibrium* of

(*a*) *Equilibre des liqueurs.*

fluids. But thoſe that ſhall conſider how when once an impetus is given to a fluid body, it is capable of mounting above its level, and checking others that have a contrary tendency to deſcend by their own gravity, will no longer regard this as a material obſtacle; but will rather conclude, that the great analogy there is between the riſing and falling of the water upon the flux and reflux of the ſea, and this of accumulating and extenuating the air, is a great argument for the truth of this hypotheſis. For as the ſea over againſt the coaſt of Eſſex, riſes and ſwells by the meeting of the two contrary tides of flood, whereof the one comes from the S. W. along the channel of England, and the other from the north, and on the contrary ſinks below its level upon the retreat of the water both ways, in the tide of ebb; ſo it is very probable, that the air may ebb and flow after the ſame manner; but by reaſon of the diverſity of cauſes whereby the air may be ſet in moving, the times of theſe fluxes and refluxes thereof are purely caſual, and not reducible to any rule, as are the motions of the ſea, depending wholly upon the regular courſe of the moon.

NUMB. II.

A scale of degrees of heat by Sir ISAAC NEWTON *(a)*.

The signs and descriptions of heats.

Equal parts of heat.		
0.		THE heat of air in winter, when water begins to freeze. This heat may be exactly determined by placing a thermometer in compressed snow when it begins to thaw.
0, 1, 2		The heats of the air in winter.
2, 3, 4		The heats of the air in spring and autumn.
4, 5, 6		The heats of the air in summer.
6		The heat of the air at noon about the month of July.
12	1	The greatest heat which a thermometer can acquire in contact with a human body: the heat of a bird hatching her eggs is much the same.
$14\frac{3}{11}$	$1\frac{1}{4}$	Almost the greatest heat of a bath that a person can bear while his hand is immersed and constantly agitated for some time. The heat of the blood just let out of the body is almost the same.
17	$1\frac{1}{2}$	The greatest heat of a bath that a person can bear, while his hand is immersed and kept constantly at rest for some time.
$20\frac{2}{11}$	$1\frac{3}{4}$	The heat of a bath in which floating wax, after it has been melted, begins by

(a) Translated from the original in the Philosophical Transactions, N° 270.

		cooling to loſe its fluidity and tranſparency.
24	2	The heat of a bath, by which floating wax is ſo heated as to melt, and continue in fuſion without ebullition.
$28\frac{6}{11}$	$2\frac{1}{4}$	A middle degree of heat between that wherewith wax melts and water boils.
34	$2\frac{1}{2}$	The heat with which water boils vehemently, and a mixture of 2 parts of lead, 3 of tin and 5 of biſmuth grows ſtiff by cooling. Water begins to boil with a heat of 33 parts, and in boiling ſcarce ever exceeds a heat of $34\frac{1}{2}$. Drops of hot water falling upon hot iron, ceaſe to bubble, when the iron has 35 or 36 parts of heat, and of cold water, when the iron has 37 parts.
$40\frac{4}{11}$	$2\frac{3}{4}$	The leaſt heat with which a mixture of 1 part of lead, 4 of tin and 5 of biſmuth, will melt and continue in fuſion.
48	3	The leaſt heat with which a mixture of equal parts of tin and biſmuth melts. This mixture by cooling grows ſtiff with 47 parts of heat.
57	$3\frac{1}{4}$	The heat with which a mixture of 2 parts of tin and 1 part of biſmuth melts; as alſo a mixture of 3 parts of tin and 2 of lead: but a mixture of 5 parts of tin and 2 parts of biſmuth, grows ſtiff by cooling in this heat; and ſo does a mixture of equal parts of tin and biſmuth.
68	$3\frac{1}{2}$	The leaſt heat with which a mixture of 1 part of biſmuth and 8 parts of tin melts.

		melts. Tin by itself melts with a heat of 72 parts, and grows stiff by cooling in a heat of 70 parts.
81	$3\frac{3}{4}$	The heat with which bismuth melts, as also a mixture of 4 parts of lead and 1 part of tin. But a mixture of 5 parts of lead and 1 part of tin, after it has been melted, grows stiff by cooling in this heat.
96	4	The least heat with which lead melts. It grows hotter and melts with a heat of 96 or 97 parts, and grows stiff by cooling in a heat of 95 parts.
114	$4\frac{1}{4}$	The heat with which burning bodies by cooling cease to be visible in a dark night, and on the contrary by heating begin to shine in the same degree of darkness, but with so faint a light as is scarce sensible. With this heat a mixture of equal parts of tin and regulus martis, and also a mixture of 7 parts of bismuth and 4 parts of that regulus, grows stiff in cooling.
136	$4\frac{1}{2}$	The heat with which burning bodies shine in a dark night but not at all in the twilight. With this heat a mixture of two parts of regulus martis and 1 part of bismuth, and also a mixture of 5 parts of regulus martis and 1 part of tin, grow stiff by cooling. Regulus alone grows stiff with a heat of 146 parts.
161	$4\frac{3}{4}$	The heat wherewith bodies burning in the twilight, just before sun-rise or after sun-set, shine manifestly, but not at all or but very obscurely in broad daylight.

192	5	The heat of burning coals in a ſmall fire made of bituminous pit-coal, and not blown with the bellows. Iron heated as hot as poſſible in this fire, has the ſame heat as the fire itſelf. But the heat of a ſmall fire made of wood, is a little greater, having 200 or 210 parts; and the heat of a large fire is ſtill greater, eſpecially if blown with bellows.

In the firſt column of this table we have degrees of heats in arithmetical progreſſion, beginning from the heat with which water juſt begins to freeze, as the loweſt degree of heat, or as a limit common to heat and cold, and conſidering the external heat of a human body as conſiſting of 12 equal parts.

In the ſecond column we have degrees of heats in geometrical progreſſion; the firſt degree (12) is the external heat of a human body adjuſted by our ſenſes, the ſecond (24) is double the firſt, the third (48) is double the ſecond, the fourth (96) is double the third and the fifth (192) double the fourth *(b)*.

By this table it appears, that the heat (34) of boiling water, is almoſt three times greater than the heat (12) of a human body, and the heat (72) of melting tin ſix times greater, and the heat (96) of

(*b*) I underſtand the Author's ſenſe of this paragraph as follows. "In the ſecond column we have" a ſcale of indices or exponents of "degrees of heat in geometrical progreſſion." The numbers 1, $1\frac{1}{4}$, $1\frac{1}{2}$, $1\frac{3}{4}$, 2, &c. in the ſecond column, being in arithmetical progreſſion, are a ſcale of logarithms, or meaſures of the ratio's of the heats expreſſed by the correſponding numbers 12, $14\frac{3}{11}$, 17, $20\frac{2}{11}$, 24, &c. in the firſt column; which being in geometrical progreſſion, may be ſoon found by taking $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$ of the logarithm of 2, or of the ratio of 1 to 2, and multiplying the correſponding abſolute numbers, in the table of logarithms, by 12. Then by doubling the 4 laſt terms of the geometrical progreſſion ſo found, you get the 4 next, and by doubling theſe you have the 4 next, and ſo on to the end of the ſcale.

melting lead eight times greater, and the heat (146) of melting regulus about twelve times greater, and the heat (200) of a common fire about sixteen or seventeen times greater than the heat of a human body.

The table was constructed by the help of a thermometer and red-hot iron. By the thermometer I found the measures of all the heats as far as that which melts tin, and the measures of all the rest by red-hot iron. For the heat which the iron communicates to contiguous cold bodies in a given time, that is, the heat which it loses in a given time, is as the whole heat of the iron. Consequently if the time of its cooling be divided into any equal parts, the corresponding heats [both lost and retained] will decrease in a geometrical progression, and therefore may easily be found by a table of logarithms *(c)*.

First then by a thermometer made with linseed-oil, I found when the thermometer was placed in melting snow, if the oil took up 10000 equal parts of space, that the same oil, being afterwards rarefied and dilated by the first degree of heat, or that of a human body, took up 10256 such parts, and by the heat of water just beginning to boil 10705 parts, and by the heat of water boiling vehemently 10725 parts, and by the heat of melted tin, when by cooling it began to stiffen to the consistence of an amalgama 11516 parts, and when quite stiff 11496 parts.

Therefore the oil was rarefied and dilated in the ratio of 40 to 39 by the heat of a human body, in the ratio of 15 to 14 by the heat of boiling water, in the ratio of 15 to 13 by the heat of melted tin beginning to stiffen and coagulate by cooling, and in the ratio of 23 to 20 by the heat of tin just grown quite stiff.

(c) See pag. 101. paragraph. 1.

The

The rarefaction of air by an equal heat was ten times greater than that of the oil, and the rarefaction of the oil about fifteen *(d)* times greater than that of spirit of wine.

By these experiments, taking the heats of the oil to be proportional to its rarefactions *(e)*, and for the external heat of a human body writing 12 parts, the heat of water just beginning to boil, comes out 33 such parts, and of water boiling vehemently 34 parts, and of tin either beginning to melt or to stiffen into an amalgama 72 parts, and of tin just become quite stiff and hard 70 parts.

Having found these heats, in order to determine the rest, I heated a piece of iron of a sufficient thickness, till it became red-hot, and taking it from the fire with the tongs likewise red-hot, I immediately put it in a cool place where the wind blew constantly, and upon it I laid particles of diverse metals and other fusible bodies, and noted the several instants of time when by cooling they lost their fluidity and began to coagulate, and lastly when the heat of the iron became equal to the external heat of a human body.

(d) The original runs thus, *Rarefactio aeris æquali calore fuit decuplo major quam rarefactio olei, & rarefactio olei quasi quindecim vicibus major quam rarefactio spiritus vini*, in which I conceive there is some mistake. For Dr. *Halley*, by an experiment described in the Philos. Transact. N° 197. found that when spirit of wine began to boil (after which it has no regular expansion) it had increased itself a 12th part of its bulk when cold in winter time; and by my own trial I have found, that with the heat of spirit of wine beginning to boil, the linseed oil in my thermometer had increased itself about 53 thousandth parts of its bulk when placed in thawing snow, or to make a juster comparison, about $\frac{51}{1002}$ parts of its bulk when cold in winter time. Therefore the increment of the spirit, is to that of the oil, produced by equal heats, as $\frac{1}{12}$ to $\frac{51}{1002}$ or 501 to 306 or about 5 to 3.

(e) That is, to the increments of its bulk, as appears by the numbers in the two last paragraphs but one.

Then

Then upon this principle, that the excesses of the heats of the iron and coagulating particles, above the heat of the atmosphere, found by my thermometer, were in a geometrical progression, when the times were taken in an arithmetical progression, I determined all those heats.

I placed the iron not in a stagnating air, but in a wind blowing uniformly, that the air heated by the iron might immediately be driven away by the wind, and that cool air might continually succeed it with an uniform motion. For by this means equal parts of the atmosphere were heated in equal times, with degrees of heat proportionable to the heats of the iron.

Now the heats so determined were to one another in the same ratio's as the heats found by my thermometer, and therefore the principle I assumed, that the heats of the oil were proportionable to its rarefactions, is a true principle *(f)*. So far Sir *Isaac Newton*.

Hence we learn the construction of a thermometer, which being once adjusted by experiment to any one degree of heat in the Author's scale, shall determine the rest artificially, and also the proportions of any other heats to those in the scale.

For this purpose, since a tube seldom happens to be perfectly cylindrical, it must be distinguished into parts equal in capacity, if not in length, as follows. First weigh the empty tube, then having filled its ball and about a ninth or tenth part of the tube with quicksilver, weigh it again and deduct the former weight from the latter; the difference is the weight of the inclosed quicksilver, which gives the weight of one hundredth part of it.

(f) This property of the rarefaction of linseed-oil was afterwards confirmed by an experiment made by Dr. *Brook Taylor*, and described in the Philosoph. Transact. N° 376.

Mark the tube with a file at the surface of the inclosed quicksilver, and with an hundredth part of its weight, weigh out 8 or 9 parcels of the like quicksilver, and pour them one after another upon the inclosed quicksilver, marking the tube successively at the surface of each parcel.

Then with your compasses compare the intervals of the marks, and if they be equal to one another, divide each of them into ten equal parts, otherwise make the parts increase or decrease as the intervals do.

Thus the capacity of the tube will be distinguished into thousandth parts of that of the ball and contiguous part of the tube reaching up to the first mark.

Then put the tube into a frame, and by the side of it place a scale of thousandth parts exactly corresponding with the opposite marks upon the tube; and writing 1000 over against the first mark, number the rest in their order, as in Fig. 41.

On the opposite side of the tube over against the numbers 1000, 1012.8, 1025.6, 1038.4, 1051.2, 1064, 1076.8, &c. in arithmetical progression, write 0, 6, 12, 18, 24, 30, 36, &c. also in arithmetical progression, where putting marks, divide their several intervals into six equal parts if the intervals be equal, otherwise into parts proportionable to the intervals. And along the side of this scale, at the proper divisions answering to the numbers in the first column of the Author's scale, write the names of the several bodies whose degrees of heat are expressed by those numbers.

The scale for the given tube being thus constructed, what remains to be done is only to pour in linseed-oil, and adjust it to such a quantity, that, when the thermometer is placed in the heat of any one body marked upon the scale, and has acquired it very slowly and uniformly in every part, the surface of the

the oil may rest exactly at the mark belonging to that heat; that is, at 0 if the ball be placed in compressed snow just thawing, or at 34, if in water just beginning to boil; and so for any other.

Artificers generally fill their thermometers with a glass-funnel, whose pipe is drawn out, like a capillary tube, to a length and slenderness sufficient to enter the tube of the thermometer and reach down to its ball. And if they happen to pour in too much liquor, they insert an empty capillary tube, which will attract the liquor by little and little, till a due quantity be left behind.

By a linseed-oil thermometer well graduated and adjusted as above, many more may soon be made with oil or any other expanding fluid, without the trouble of graduating their tubes by equal quantities of quicksilver. For having filled the balls and a convenient part of the tubes, with the fluids proposed, place them all together in a skillet of cold water, and while it is warming as gently as possible, when the oil in the standard thermometer shall arrive successively at the several divisions of its scale, at the same instants of time mark the new tubes at the several heights of their fluids; and form a scale for every tube that shall correspond to those marks. Then while the liquors subside by cooling gently, examine whether they correspond at the respective marks.

It is easy to understand how, by the help of quicksilver as above, the scale of thousandth parts in the standard thermometer, may be continued below the mark 1000; from which the scales of the new tubes may be continued downwards, by placing them all together in some freezing mixture, provided the liquors in the tubes be incapable of freezing. These parts of the scales are necessary for registering degrees of cold.

A

A thermometer that ſhall vary very ſenſible by every ſmall variations of heat and cold, as thoſe of the atmoſphere, muſt have a large ball in proportion to the bore of the tube; and that the heat or cold may ſooner diffuſe itſelf, even to the innermoſt parts of the included liquor, the ball ſhould not be ſpherical but oblong and flatted like a French flaſk. Thermometers intended for different uſes ſhould have tubes of different ſizes and lengths, for comprehending a greater or ſmaller number of degrees of heat, ſuitable to the intended uſes. And it may not be improper to place a ſpirit of wine thermometer in the ſame frame with that of linſeed-oil, by which the larger intervals in the new ſcale may be determined; and theſe may be ſubdivided into ſmaller, anſwering to equal capacities of the tube, as above.

Being very much pleaſed with the ingenuity of the Author's method of meaſuring heats, and his care and judgment in ſelecting ſo great a variety of particulars as ſhould compoſe a ſcale ſo remarkably regular, for further ſatisfaction in the accuracy of his meaſures, I formerly made an oil thermometer in the manner I have been deſcribing; and having placed it in ſeveral heats mentioned by the Author, beginning from that of water juſt freezing and increaſing to that of its boiling vehemently, I found they raiſed my oil exactly enough to the marks determined upon my ſcale; and judging Sir *Iſaac Newton* to be the Author of this admirable paper, as well by the ſtyle and manner of it, as by its analogy to ſome paſſages in his writings, I was afterwards confirmed in my opinion by talking with him about it, and mentioning the agreement of the ſcale with my own experiments. Which I now mention as an experimental proof of the propoſition I began with, and as an inducement to thoſe that uſe thermometers in philoſophical enquiries, to conſtruct them in the manner

ner here deſcribed, at leaſt till a better be invented; in order to render their experiments intelligible and uſeful to the world. For it cannot but appear to a thinking perſon, even from the theory alone, that all thermometers of this conſtruction, notwithſtanding any difference in the ſhapes and capacities of the tubes, muſt needs agree together artificially, to the ſame degree of exactneſs, as bodies of the ſame name and ſort, and in the ſame circumſtances, can agree together in the quantity of their ſenſible qualities. And this I think is all the information a thermometer can give us, and what I preſume has been hitherto wanting, in thoſe at leaſt that I ever met with.

NUMB. III.

An account of ſome experiments ſhown before the Royal Society; with an enquiry into the cauſe of the aſcent and ſuſpenſion of water in capillary tubes, by Dr. Jurin *(a).*

SOME days ago a method was propoſed to me by an ingenious friend, for making a perpetual motion, which ſeemed ſo plauſible, and indeed ſo eaſily demonſtrable from an obſervation of the late Mr. *Haukſbee*, ſaid to be grounded upon experiment, that though I am far from having any opinion of attempts of this nature, yet I confeſs I could not ſee why it ſhould not ſucceed. Upon trial indeed the fallacy diſcovered itſelf. But as ſearchers after things impoſſible in themſelves are frequently obſerved to produce other diſcoveries, unexpected

(a) Reprinted from the Philoſoph. Tranſact. N° 355.

by

by the inventer; ſo this propoſal has given occaſion, not only to rectify ſome miſtakes into which we had been led by that ingenious and uſeful member of the Royal Society above-named, but likewiſe to detect the real principle by which water is raiſed and ſuſpended in capillary tubes, above the level.

I. My friend's propoſal was as follows. In Fig. 42 let *a b c* be a capillary ſiphon, compoſed of two legs *ab*, *bc*, unequal both in length and diameter; whoſe longer and narrower leg *ab* having its orifice *a* immerſt in water, the water will riſe above the level, till it fills the whole tube *ab*, and will then continue ſuſpended. If the wider and ſhorter leg *bc*, be in like manner immerſt, the water will only riſe to ſome height as *fc*, leſs than the entire height of the tube *bc*.

This ſiphon being filled with water, and the orifice *a* ſunk below the ſurface of the water *de*, my friend reaſons thus.

Since the two columns of water *ab* and *fc*, by the ſuppoſition, will be ſuſpended by ſome power acting within the tubes they are contained in, they cannot determine the water to move one way or the other. But the column *bf*, having nothing to ſupport it, muſt deſcend, and cauſe the water to run out at *c*. Then the preſſure of the atmoſphere driving the water upward through the orifice *a*, to ſupply the vacuity, which would otherwiſe be left in the upper part of the tube *bc*, this muſt neceſſarily produce a perpetual motion, ſince the water runs into the ſame veſſel, out of which it riſes. But the fallacy of this reaſoning appears upon making the experiment.

Exp. 1. For the water, inſtead of running out at the orifice *c*, riſes upward towards *f*, and running all out of the leg *bc*, remains ſuſpended in the other leg to the height *ab*.

Exp. 2. The ſame thing ſucceeds upon taking the ſiphon out of the water, into which its lower orifice

orifice *a* had been immerſt, the water then falling in drops out of the orifice *a*, and ſtanding at laſt at the height *ab*. But in making theſe two experiments it is neceſſary that *ag* the difference of the legs exceed *fc*, otherwiſe the water will not run either way.

Exp. 3. Upon inverting the ſiphon full of water, it continues without motion either way.

The reaſon of all which will plainly appear, when we come to diſcover the principle by which the water is ſuſpended in capillary tubes.

II. Mr. *Haukſbee*'s obſervation is as follows. In Fig. 43 let *abfc* be a capillary ſiphon, into which the water will riſe above the level to the height *cf*, and let *ba* be the depth of the orifice of its longer leg below the ſurface of the water *de*. Then the ſiphon being filled with water, if *ba* be not greater than *cf*, the water will not run out at *a*, but will remain ſuſpended.

This ſeems indeed very plauſible at firſt ſight. For ſince the column of water *fc* will be ſuſpended by ſome power within the tube, why ſhould not the column *ba*, being equal to, or leſs than the former, continue ſuſpended by the ſame power?

Exp. 4. In fact, if the orifice *c* be lifted up out of the water *de*, the water in the tube will continue ſuſpended, unleſs *ba* exceed *fc*.

Exp. 5. But when *c* is never ſo little immerſt in the water, immediately the water in the tube runs out in drops at the orifice *a*, though the length *ab* be conſiderably leſs than the height *cf*.

Mr. *Haukſbee* in his Book of Experiments has advanced another obſervation, namely, that the ſhorter leg of a capillary ſiphon, as *abfc*, muſt be immerſt in the water to the depth *fc*, which is equal to the height of the column, that would be ſuſpended in it, before the water will run out at the longer leg.

Exp. 6. From what miſtake this has proceeded, I cannot imagine; for the water runs out at the longer leg, as ſoon as the orifice of the ſhorter leg comes to touch the ſurface of the ſtagnant water, without being at all immerſt therein.

III. I proceed now to enquire into the cauſe of the aſcent and ſuſpenſion of water in capillary tubes.

That this phænomenon is no way owing to the preſſure of the atmoſphere, has been, I think ſufficiently proved by Mr. *Haukſbee*'s experiments.

And that the cauſe aſſigned by the ſame perſon, namely, the attraction of the concave ſurface, in which the ſuſpended liquor is contained, is likewiſe inſufficient for producing this effect, I thus demonſtrate.

Since in every capillary tube the height, to which the water will ſpontaneouſly aſcend, is reciprocally as the diameter of the tube, it follows, that the ſurface containing the ſuſpended water in every tube is always a given quantity: but the column of water ſuſpended, is as the diameter of the tube. Therefore, if the attraction of the containing ſurface be the cauſe of the water's ſuſpenſion; it will follow, that equal cauſes produce unequal effects, which is abſurd.

To this it may perhaps be objected, that in two tubes of unequal diameters, the circumſtances are different, and therefore the two cauſes, though they be equal in themſelves, may produce effects that are unequal. For the leſſer tube has not only a greater curvature, but thoſe parts of the water, which lie in the middle of the tube, are nearer to the attracting ſurface, than in the wider. But from this, if any thing follows, it muſt be, that the narrower tube will ſuſpend the greater quantity of water, which is contrary to experiment. For the columns ſuſpended are as the diameters of the tubes.

But as experiments are generally more ſatisfactory in things of this nature, than mathematical reaſonings,

ings, it may not be amiſs to make uſe of the following, which appear to me to contain an *experimentum crucis.*

In Fig. 44, the tube *cd* is compoſed of two parts, in the wider of which the water will riſe ſpontaneouſly to the height *bf*, but the narrower part, if it were of a ſufficient length, would raiſe the water to a height equal to *cd*.

Exp. 7. This tube being filled with water, and the wider end *c* immerſt in the ſtagnant water *ab*, the whole continues ſuſpended.

Exp. 8. The narrower end being immerſt, as in Fig. 45, the water immediately ſubſides, and ſtands at laſt at the height *dg* equal to *bf*.

From which it is manifeſt, that the ſuſpenſion of the water in the former of theſe experiments is not owing to the attraction of the containing ſurface; ſince, if that were true, this ſurface being the ſame, when the tube is inverted, would ſuſpend the water at the ſame height.

IV. Having ſhewn the inſufficiency of this hypotheſis, I come now to the real cauſe of that phænomenon, which is the attraction of the periphery, or rather of the ſmall annular portion of the inſide of the tube, to which the upper ſurface of the water is contiguous and coheres.

For this is the only part of the tube, from which the water muſt recede upon its ſubſiding, and conſequently the only one, which by the force of its coheſion or attraction, oppoſes the deſcent of the water.

This likewiſe is a cauſe proportional to the effect, which it produces; ſince that periphery, and the column ſuſpended, are both in the ſame proportion as the diameter of the tube.

Though from either of theſe particulars it were eaſy to draw a juſt demonſtration, yet to put the matter out of all doubt, it may be proper to confirm this assertion, as we have done the former, by actual experiment.

Let therefore, in FIG. 46, *edc* be a tube, like that made use of in the 7th and 8th experiments, except that the narrower part is of a greater length; and let *af* and *bg* be the heights, to which the water would spontaneously rise in the two tubes *ed* and *dc*.

EXP. 9. If this tube have its wider orifice *c* immerst into the water *ab*, and be filled to any height less than the length of the wider part, the water will immediately subside to a level with the point *g*; but if the surface of the contained water enter never so little within the smaller tube *ed*, the whole column *dc* will be suspended, provided the length of that column do not exceed the height *af*.

In this experiment it is plain, that there is nothing to sustain the water at so great a height, except the contact of the periphery of the lesser tube, to which the upper surface of the water is contiguous. For the tube *dc*, by the supposition, is not able to support the water at a greater height than *bg*.

EXP. 10. When the same tube is inverted, as in FIG. 47, and the water is raised into the lower extremity of the wider tube *cd*, it immediately sinks, if the length of the suspended column *dh* be greater than *gb*; whereas in the tube *de* it would be suspended to the height *af*. From which it manifestly appears, that the suspension of the column *dh* does not depend upon the attraction of the tube *de*, but upon the periphery of the wider tube, with which its upper surface is in contact.

V. For the sake of those, who are pleased with seeing the same thing succeed in different manners, we subjoin the two following experiments, which are in substance the same with the 9th and 10th.

In FIG. 48, *abc* is a siphon, in whose narrower

and

and ſhorter leg *ab*, if it were of a ſufficient length, might be ſuſpended a column of water of the height *ef*; but the longer and wider leg *bc* will ſuſpend no more than a column of the length *gh*.

Exp. 11. This ſiphon being filled with water, and held in the ſame poſition, as in the figure, the water will not run out at *c* the orifice of the longer leg, unleſs *dc* the difference of the legs *ab* and *bc*, exceed the length *ef*.

Exp. 12. If the narrower leg *bc* be longer than *ab*, as in Fig. 49, the water will run out at *c*, if *dc* the difference of the legs exceed *ef*; otherwiſe it will remain ſuſpended.

In theſe two experiments it is plain, that the columns *dc* are ſuſpended by the attraction of the peripheries at *a*, ſince their lengths are equal to *ef*, or to the length of the column, which by the ſuppoſition thoſe peripheries are able to ſupport; whereas the tubes *bc* will ſuſtain columns, whoſe lengths are equal to *gh*.

VI. Though theſe experiments ſeem to be concluſive, yet it may not be improper to prevent an objection, which naturally preſents itſelf, and which at firſt view may be thought ſufficient to overturn our theory.

For ſince a periphery of the tube *ed*, in Fig. 46, is able to ſuſtain no more than a column of the length *af*, contained in the ſame tube; how comes it to ſuſtain a column of the ſame length in the wider tube *dc*, which is as much greater than the former, as the ſection of the wider tube exceeds that of the narrower?

Again, if a periphery of the wider tube *dc*, in Fig. 47, be able to ſuſtain a column of water in the ſame tube, of the length *bg*; why will it ſupport no more than a column of the ſame length in the narrower tube *ed*?

Which queries may likewise be made with regard to the 11th and 12th experiments.

The answer is easy, for the Moments of those two columns of water are precisely the same, as if the sustaining tubes *ed* and *cd*, were continued down to the surface of the stagnant water *ab*; since the velocities of the water, where those columns grow wider or narrower, are to the velocities at the attracting peripheries, reciprocally as the different sections of the columns.

Exp. 13. From which consideration arises this remarkable paradox, that a vessel being given of whatsoever form, as *abc*, in Fig. 50, and containing any assignable quantity of water, how great soever; that whole quantity of water may be suspended above the level, if the upper part of the vessel *c* be drawn out into a capillary tube of a sufficient fineness.

But whether this experiment will succeed, when the height of the vessel is greater than that, to which water will be raised by the pressure of the atmosphere, and how far it will be altered by a *vacuum*, I shall give an account in my next paper.

Having discovered the cause of the suspension of water in capillary tubes, it will not be difficult to account for the seemingly spontaneous ascent of it; for since the water that enters a capillary tube, as soon as its orifice is dipt therein, has its gravity taken off by the attraction of the periphery, with which its upper surface is in contact, it must necessarily rise higher, partly by the pressure of the stagnant water, and partly by the attraction of the periphery immediately above that, which is already contiguous to it.

NUMB. IV.

An account of ſome new experiments relating to the action of glaſs tubes upon water and quickſilver, by Dr. Jurin *(a).*

I. IN the foregoing diſcourſe, preſented to the Royal Society, I maintained that the ſuſpenſion of water in a capillary tube was owing to the attraction of a ſmall annular ſurface on the inſide of the tube, which touched the upper part of the water. Among the ſeveral experiments made uſe of to prove this aſſertion, was that of a glaſs funnel of ſeveral inches diameter, having its ſmall end drawn out into a very fine tube, which funnel being inverted and filled with water, the whole quantity of water therein contained was ſuſtained above the level by the attraction of that narrow annulus of glaſs, with which the upper ſurface of the water was in contact.

Soon after that diſcourſe was printed, came out a book publiſhed by a learned and ingenious member of this Society, in which that experiment was accounted for in the following manner.

FIG. 51. *If there be a funnel, as ABC, full of water, and whoſe wide end ſtands in a veſſel of water as BC; and the top of the funnel A ends in a capillary tube open at A, the whole water will be ſuſtained: the pillar Aa by the attraction of the circle of glaſs within the tube immediately above it; and all the reſt of the pillars of water, as Ff, Dd, Ee, Gg, &c. in ſome meaſure by the attraction of the parts of the glaſs above them, as F, D, E, G: and that the ſmall pillars or threads of water Dd and Ee, do not ſlide down to Ff and Gg, and ſo go*

(a) Reprinted from the Philoſoph. Tranſact. N° 363.

quite down, seems to be owing to their cohesion with the pillar Aa, which is sustained by the capillary tube A: for if you break off the said tube at DE, the whole water will presently sink down.

As this solution was very different from what I had before given, and the reputation of that gentleman, whose great knowledge in experimental philosophy is generally known, was sufficient to give weight to any of his opinions; I thought myself under an obligation to examine his account of the experiment, in order either to demonstrate its insufficiency, or to retract my own solution. Accordingly at the next meeting of the Society, I produced the following experiment.

In Fig. 52, the funnel *afgbc*, whose lower part *bcfg*, was cylindrical to a considerable height, and whose top was drawn out into a fine tube at *a*, being filled with water to the height *bf*, so that the surface of the water *fg*, did not reach to the arched part of the funnel; I touched the end *a* with a wetted finger, whereby a small quantity of water being insinuated into the capillary tube at *a*, the water contained in the funnel was suspended above the level of the water in the cistern *de*, as in the former experiment.

In this experiment it is manifest, that the little columns, into which we may suppose the cylinder of water, *fgbc*, to be divided, are no way sustained by the attraction of the arched part of the glass above them, since they have no contact with it. Nor is there any such middle pillar of water, which, by its contact with the tube at top, is both sustained itself, and helps to support the pillars about it. Upon the supposition of which two particulars, that gentleman's solution was founded.

This experiment may be thus accounted for. The cylinder of water *fgbc*, by its weight ballances a part of the pressure of the atmosphere, which is incumbent

bent on the water in the ciſtern, and endeavours to force that cylinder upwards. The reſt of that preſſure is ballanced by the ſpring of the air, *afg*, which is included between the cylinder of water *fgbc*, and the little column of water in the capillary *a*. But, as this air by its ſpring preſſes equally every way, it muſt ballance as much of the preſſure of the atmoſphere upon the little column of water at *a*, as it does of that upon the water in the ciſtern. The remainder of the preſſure of the atmoſphere upon the column of water at *a* is ſuſtained by the force, with which that column adheres to the capillary tube, which therefore does exactly ballance the weight of the cylinder of water *fgbc*, and is the real, though not the immediate, cauſe of its ſuſpenſion.

The experiment ſucceeds in the ſame manner, when a column of quickſilver is raiſed into the funnel, inſtead of the column of water *fgbc*, the top of the tube being touched with a wet finger as before. But then the height of the quickſilver in the funnel muſt be as much leſs than that of the water, as its ſpecifick gravity is greater.

I proceed now to acquit myſelf of a promiſe I made in the diſcourſe abovementioned, of examining whether the experiments therein contained would ſucceed in *vacuo*; and whether water could be ſuſpended in a wide tube by means of a capillary at top, at a greater height, than what it can be raiſed to by the preſſure of the atmoſphere.

In order to this, I boiled ſome water, and afterwards purged it of its air, by means of the air-pump; which being done, thoſe experiments all ſucceeded in the exhauſted receiver, in the ſame manner as in the open air.

The 13th experiment in particular, was made with a tube of about 35 inches in length, and a quarter of an inch diameter, the top of it being drawn out into

into a fine capillary. Which being filled with water purged of its air, as before mentioned, the whole quantity continued ſuſpended in the exhauſted receiver.

This plainly ſhews, that the ſucceſs of that experiment does not depend upon the preſſure of the air, ſince the ſmall quantity of air left in the receiver was by no means capable of ſuſtaining the water at ſo great a height, and conſequently that the height, at which water may be ſuſpended in this manner, is not limited by that preſſure.

But here I muſt not omit taking notice of a conſiderable difficulty, which preſents itſelf to thoſe who attentively conſider this experiment. In order to make which the better appear, it will be proper to obſerve, what happens, when a ſimple capillary tube is filled with water purged of air, and incloſed in the exhauſted receiver.

In this caſe the whole column of water contained in the tube, *acb*, Fig. 53, is ſuſpended by the attraction of the *annulus* at the top of the tube, *a*: and though that *annulus* does not immediately act upon any part of the water, except what is either contiguous to it, or ſo near as to be within the ſphere of its attraction, which extends but to a very ſmall diſtance; yet it is impoſſible, that any other part of the water, as for inſtance that at *c*, ſhould part from the water above it, and ſink down; becauſe its deſcent is oppoſed by the attraction of the contiguous *annulus* at *c*. For this being equal to the upper *annulus* at *a*, is capable of ſuſtaining a column of water of the length *ab*, and conſequently is more than ſufficient for ſupporting the column of water below it, *cb*. From which it is plain, that no part of the water contained in the tube can poſſibly deſcend, unleſs the upper part, aſſiſted by the weight of the water

ter below it, be ſufficient to overcome the attraction of the *annulus* of glaſs at *a*.

But in ſuch a compound tube, as that made uſe of in our experiment, *acb*, FIG. 54, the caſe is very different, and it does not eaſily appear, why in a *vacuum* any part of the water in the wider part of the tube, as for example at *c*, ſhould not leave that which is above it and deſcend; ſince the *annulus* at *c* is by much too wide to ſuſtain a column of water of ſo great a length as *cb*.

The beſt anſwer I can give to this difficulty is, that the coheſion between the water contained in the capillary and that below it, is ſufficient to ballance the weight of the column ſuſpended. But how far this coheſion may depend upon the preſſure of a medium, ſubtile enough to penetrate the receiver, is worthy of conſideration. For though ſuch a medium will pervade the pores of the water, as well as thoſe of the glaſs, yet it will act with its intire preſſure upon all the ſolid particles, if I may ſo call them, of the ſurface of the water in the ciſtern; whereas ſo many of the ſolid particles of the water in the tube, which happen to lie directly under the ſolid particles of the water above them, will thereby be ſecured from this preſſure; and conſequently there will be a leſs preſſure of this medium upon any ſurface of the water in the tube below the capillary, than upon an equal ſurface of the water in the ciſtern. So that the column of water ſuſpended in the tube may be ſuſtained by the difference between thoſe two preſſures. This explication ſeems to be favoured by the following experiments, which may all be accounted for in the ſame manner, though I ſhall anon mention another cauſe, which contributes to the ſucceſs of the firſt and ſecond.

The firſt I ſhall mention is the famous experiment of the ſuſpenſion of mercury purged of air, to the

height

height of 70 or 75 inches, in the Torricellian-tube, in the open air. To which we may add the suſtaining of mercury, likewiſe purged of air, within the exhauſted receiver, as related by the learned Monſ. *Papin* in his *Continuation du Digeſteur*. I forbear to mention the ſuſpenſion of water purged of air in the *Vacuum*, which he deſcribes in the ſame book; becauſe there is little difference between that experiment and our own above-mentioned; the very top of the arched part of his tube, which top we may ſuppoſe as ſmall as we pleaſe, ſupplying the place of the fine capillary at the top of our tube. But we muſt not omit the experiments made by the famous Monſ. *Huygens*, and deſcribed by him in Philoſoph. Tranſact. N° 86, of the cohering of poliſhed plates, with a conſiderable force in the exhauſted receiver; as likewiſe of the running of water and mercury, when purged of air, through a ſiphon of unequal legs in the *vacuum*: all which he accounts for from the ſame principle, and much in the ſame manner, as we have uſed for explaining the experiment above.

III. As to the exiſtence of ſuch a medium, I ſhall content myſelf to refer to what has been ſaid by our illuſtrious Preſident in the Queries at the latter end of the laſt edition of his Opticks. And as I have lately had the honour to entertain the Society with ſome experiments upon quickſilver, which were exactly the reverſe of thoſe made by Dr. *Taylor*, the late Mr. *Haukſbee* and myſelf, upon water; by which I am now enabled to throw this whole affair into a little ſyſtem by itſelf, I ſhall lay it down in the following propoſitions, the proof of which is contained in the experiments annexed.

Prop. 1. *The particles of water attract one another.*

This, I think, is now univerſally acknowledged, and therefore needs no demonſtration; the ſphericity of the drops of rain, and the running of two

drops

drops of water into one another upon their contact, manifeſtly proving it.

Prop. 2. *The particles of quickſilver attract one another.*

This is likewiſe manifeſt from the ſpherical figure, into which a drop of mercury forms itſelf upon a table; and from two of them immediately running together, as ſoon as they come to touch.

Prop. 3. *Water is attracted by glaſs.*

This plainly appears from all the experiments, that we have ſhewn upon this ſubject.

Prop. 4. *Quickſilver is attracted by glaſs.*

Exp. 1. If a ſmall globule of quickſilver be laid upon a clean paper, and be touched with a piece of clean glaſs; upon drawing the glaſs gently away, the quickſilver will adhere to it, and be drawn away with it. And if the glaſs be lifted up from the paper, the quickſilver will be taken up by it, in the ſame manner as a piece of iron is drawn up by the loadſtone, and will ſtick to the glaſs by a plain ſurface of a conſiderable breadth, in propoation to the bulk of the drop, as manifeſtly appears by an ordinary microſcope. Then if the glaſs be held a little obliquely, the drop of mercury will roll ſlowly upon its axis along the under ſide of the glaſs, till it comes to the end, where it will be ſuſpended as before.

Exp. 2. If a pretty large drop of mercury be laid upon a paper, and two pieces of glaſs be made to touch it, one on each ſide; upon drawing the glaſſes gently from each other, the drop of mercury will adhere to them both, and will be viſibly drawn out from a globular to an oval ſhape; the longer axis paſſing through the middle of thoſe ſurfaces, in which the drop touches the glaſſes.

Prop. 5. *The particles of water are more ſtrongly attracted by glaſs than by one another.*

This manifeſtly appears from the riſing of water in ſmall tubes above the level. For when the water

begins

begins to riſe into a capillary tube, all the particles of water, which touch the ſmall *annulus* at the bottom of the tube, muſt have quitted the contact of the other water, and have riſen contrary to their gravity, to come into contact with the glaſs. After the ſame manner the other experiments of Dr. *Taylor*, Mr. *Haukſbee* and myſelf, upon this ſubject, are eaſily explicable. For upon a careful examination, it will be found in them all, that ſome parts of the water quit the contact of the other water, and join themſelves to the glaſs.

Prop. 6. *The particles of quickſilver are more ſtrongly attracted by one another, than by glaſs.*

Exp. 1. If a ſmall tube as *ab*, Fig. 55, open at both ends, be dipt into a glaſs veſſel filled with mercury, and be held cloſe to the ſide of the veſſel, that the riſe of the mercury within it may appear; the mercury will partly enter into the tube, but will ſtand within it at ſome depth, as *ce*, below *cd* the ſurface of the quickſilver in the veſſel, and this depth will always be reciprocally as the diameter of the tube.

In this experiment a column of quickſilver of the height *ce* endeavours to force the mercury higher into the tube; and as glaſs has been already proved to attract quickſilver, the attraction of the annular ſurface on the inſide of the tube, which is contiguous to the upper part of the mercury, will likewiſe conſpire to farther its aſcent. What oppoſes the aſcent of the quickſilver, is the power, by which that part of it, which endeavours to riſe into the glaſs, is drawn back by the attraction of the other mercury, with which it is in contact laterally, and this does not only ballance the attraction of the glaſs, but likewiſe the weight of a column of mercury of the height *ce*, and conſequently this attraction is conſiderably ſtronger than the attraction of the glaſs.

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The caufe therefore, that fufpends the weight of the column of mercury *ce*, being the difference between the attraction of the annular furface of the tube at *e*, and that of an equal furface of the quickfilver in the ciftern, from which the mercury, that endeavours to rife into the tube, muft recede, in order to unite itfelf to fuch an annulus of the glafs, will always be proportional to that annular furface, or to the diameter of the tube. And fince the column fuftained muft be proportional to the caufe that fufpends it, that column muft likewife be as the diameter of the tube. But the column fufpended is as the fquare of the diameter of the tube, and the height *ce* conjointly; from which it follows, that the height *ce* muft be as the diameter of the tube reciprocally, as it is found to be by experiment.

The experiment of the afcent of water above the level in a capillary tube, is juft the reverfe of this.

Exp. 2. Quickfilver being poured into the inverted fiphon *acb*, Fig. 56, one of whofe legs *ac* is narrower than the other *cb*; the height *ce*, at which the mercury ftands in the wider leg *cb*, is greater than the height *cd*, at which it ftands in the narrower leg *ca*.

On the contrary, water ftands higher in the narrower leg, than in the wider.

Exp. 3. In Fig. 57, *abcd* reprefents a rectangular plane of glafs, which makes one fide of a wooden box. On the infide of this is another glafs plane of the fame fize, which at the end *ac* is preft clofe to the former, and opens to a fmall angle at the oppofite end *bd*. When mercury is poured into this box to any height as *ce*, it infinuates itfelf between the two glafs planes, and rifing to different heights between the glaffes, where the opening is greater or lefs, it forms the common hyperbola *cgf*; one of whofe afymptotes *ef* is the line on which the furface

of

of the mercury in the box touches the inner glafs; the other is the line *ac*, in which the planes are joined. This hyperbola being carefully examined by Mr. *Haukſbee* and myſelf, the rectangles *ehg*, whereſoever taken, proved always equal to one another, to as great an accuracy as could be expected, when the planes were opened to any conſiderable angle: but when the opening was very ſmall, the inequalities of the planes, though the beſt I could procure, bearing a greater proportion than before to the diſtance between them, occaſioned a ſenſible variation. Which, by the way, I take to be the reaſon, why the ordinates found the late Mr. *Haukſbee*, in in examining the curve produced in a contrary ſituation, upon dipping two glaſs-planes ſo joined into ſpirit of wine, do not anſwer to thoſe of the hyperbola.

Exp. 4. In Fig. 58, *ab* is a perpendicular ſection through two glaſs planes joined at *a*, and opened to a ſmall angle at *b*; *c* repreſents a pretty large drop of mercury, the larger the better, which being made to deſcend as far as *c*, by holding the planes in an erect poſture, with the end *a* downwards, retires from the contact of the planes to *d*, upon inclining the planes towards an horizontal ſituation; and the diſtance *cd* becomes greater or leſs, as the planes are more or leſs inclined towards the horizon.

A drop of any oily or watery liquor moves the contrary way, as has been ſhewn by the late Mr. *Haukſbee*.

Exp. 5. In Fig. 59, *ab* is a tube open at both ends, and a foot or two in length, whoſe lower part is drawn out into a fine capillary at *b*. This tube being filled with mercury, the whole column of quickſilver will be ſuſtained in it, provided the capillary tube at *b* be ſufficiently ſmall. But if the mercury in the end *b* be ſuffered to touch any other mercury, it runs all out of the tube. If, without letting it touch

any

any other mercury, a ſmall part of the end *b* be broken off, the mercury will run out, till it comes to ſome leſſer height as *bc*, at which it will again ſtop, the height *bc* being nearly in a reciprocal proportion to the diameter of the ſmall end of the tube.

The ſeventh experiment in the former paper is the reverſe of this.

Exp. 6. Is the ſame in ſubſtance with the former, but made with a large glaſs-funnel *ab*, inſtead of a tube, Fig. 60.

The reverſe of this in water is the thirteenth experiment in the former paper.

In all theſe experiments it is eaſily ſeen, that the effect is owing to the difference between the two attractions, by which mercury tends to glaſs and to its own body; they being always oppoſed to one another, ſo that a particular explication is no way neceſſary. But perhaps it may ſave ſome little trouble to the reader, to remove the following objection, which will readily occur to him.

In the experiments brought to demonſtrate the fourth propoſition, the globule of mercury adheres to the glaſs in a plane ſurface, which cannot be done without increaſing the ſurface of the globule, and conſequently removing ſome of its particles from the contact of one another. If therefore they tend more ſtrongly to one another than to the glaſs, why do they not recede from the glaſs, and aſſume a figure perfectly ſpherical, that they may all have the greateſt contact with each other?

To this we may anſwer, that the power by which mercury is attracted either by glaſs, or by other mercury, is proportional to the attracting ſurface; and therefore, though, *cæteris paribus*, the tendency of mercury to glaſs, is not ſo ſtrong as its tendency to other mercury, yet in this caſe a much greater number of mercurial particles coming into contact with the glaſs, than what recede from the contact of one to another, it is no wonder that the attraction of the glaſs prevails, and cauſes the globule to adhere to it. For the number of mercurial particles which loſe their contact with the other mercury, is no more than what makes up the difference of ſurface, which ariſes from changing the figure of the drop; whereas the particles, which by this means come to adhere to the glaſs, are all thoſe that conſtitute the plane ſurface, in which the globule touches it.

Which conſideration ought likewiſe to be applied to the ſuſpenſion of quickſilver in glaſs-tubes, either at extraordinary heights in the open air, or at leſſer heights in a *vacuum*, as above-mentioned. For the top of the tube being ſpherical, or nearly ſo, it will be found, that the contact of the mercury with the extremity of the tube, is to the contact with other mercury, which would be gained by its leaving the top of the tube, and deſcending a very ſmall ſpace in a ratio infinitely great; and conſequently that the contact of the mercury with the top of the tube is one cauſe of its ſuſpenſion.

Corol. 1. From this propoſition it appears, that in a barometer made with a narrow tube, the quickſilver will never ſtand at ſo great a height as in a wider. Which accounts for the phænomenon ſo often mentioned, in the yearly hiſtory of the Royal Academy of Sciences at Paris, by Monſ. *De la Hire*; that in the barometer, which he conſtantly made uſe of for his annual obſervations, the quickſilver did not riſe ſo high, as in another he kept by him, by about three lines and a half, which is near a third of an inch our meaſure: for he tells us, that the tube of his barometer is very ſmall. So that there is no need to have recourſe to any peculiarity, either in the quickſilver or the glaſs of which that tube was made; or

to

to an unperceived remnant of air left in the tube, from ſome of which cauſes that effect, and ſome others of the ſame kind were imagined to proceed.

Corol. 2. In a barometer made with a ſmall tube, the mercury will riſe and fall irregularly. For, as the height of the mercury depends partly upon the diameter of that part of the tube that touches the upper ſurface of the mercury, it is plain, that the unavoidable inequalities in the diameter of the tube will be more conſiderable, in reſpect to the whole diameter; and conſequently will affect the height of the mercury more in a ſmall tube than in a wider. And this I take to be the reaſon why it is ſo very difficult, not to ſay impoſſible, to make two barometers which ſhall exactly agree in the height of the quickſilver in all conſtitutions of the air, eſpecially if the tubes be very narrow. This irregularity is ſtill more conſiderable in the pendent barometer, in which the quickſilver moves through a large ſpace, in order to make a ſmall alteration in the length of the column ſuſpended. The ſame conſideration is eaſily extended to thoſe levels, that depend upon the riſing of mercury to the ſame height, in the oppoſite legs of a bent tube; an inſtrument of which kind has been lately offered. And as the effect is juſt contrary in levels made with water or ſpirit of wine, due regard ought to be had to this property in the conſtruction of thoſe inſtruments, by making the tubes ſufficiently wide, in order to diminiſh the error as much as poſſible.

ERRATA.

Page

9, NOTE (*b*) sect. 2. insert Fig. 5.
9, Note (*c*) insert Fig. 6.
13, line 12, r. the plate.
20, last line, r. within the legs, when the syphon is full, will *&c.*
40, Note (*a*) line 3, for vial, r. bottle.
41, line 4, r. than.
42, Note (*b*) line 8, read, $\overline{A+B}$.
44, line 24, read, tell.
59, line 6, r. might be.
60, line 3, r. into *England.*
61, in the Table, r. Dry Maple ——— 0. 755.
62, line 32, r. sulphureous.
75, line 17, r. posteriori.
76, line 22, for ascend, r. descend.
83, line 17, r. Peripateticks.
84, line 6, *dele* the.
84, line 24, r. filled with.
88, line 24, r. breath.
109, line 10, for less, read legs.
109, line 31, r. *æquilibrium.*
113, line 14, *dele* the.
113, line 21, r. up to 34.
133, line 23, r. afforded.
240, line 12, r. found by, *dele* in.

I N D E X.

The numbers refer to the pages, and the letters (*a*) (*b*) &c. to the Notes.

A.

H.

I.

L.

M.

O.

P.

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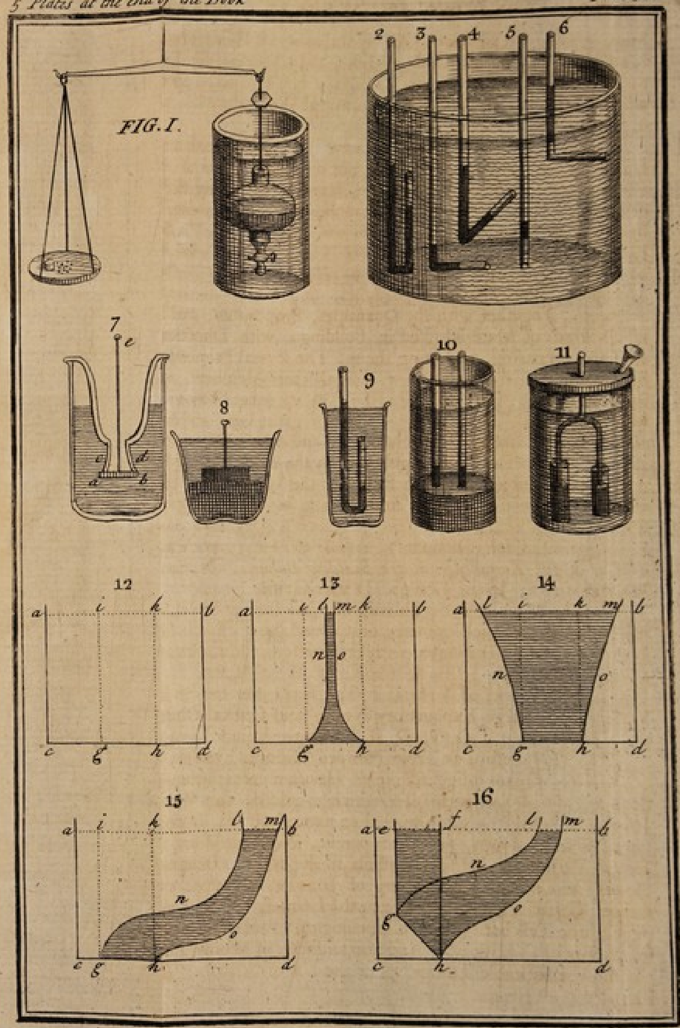
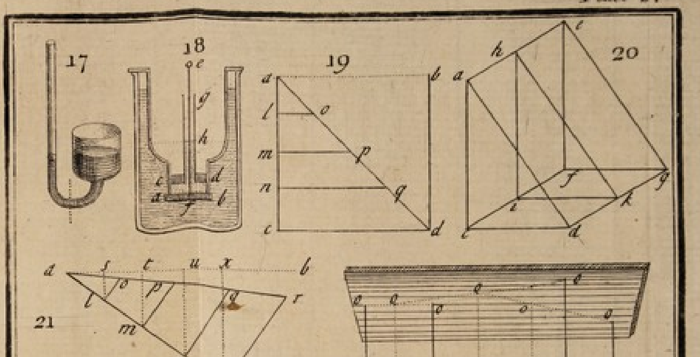


FIG. I.
2
3
4
5
6
7
e
c
d
a
b
8
9
10
11
12
a i k b
c g h d
13
a i l m k b
n o
c g h d
14
a l i k m b
n o
c g h d
15
a i k l m b
n
o
c g h d
16
a e f l m b
n
o
g
c h d

Plate 2.



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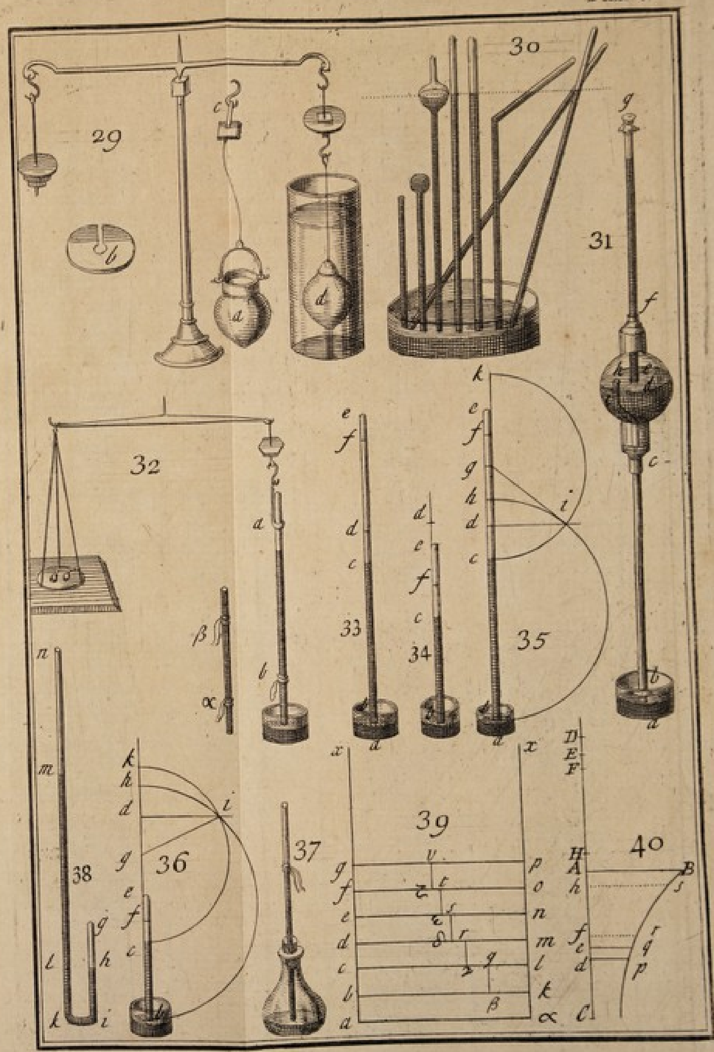

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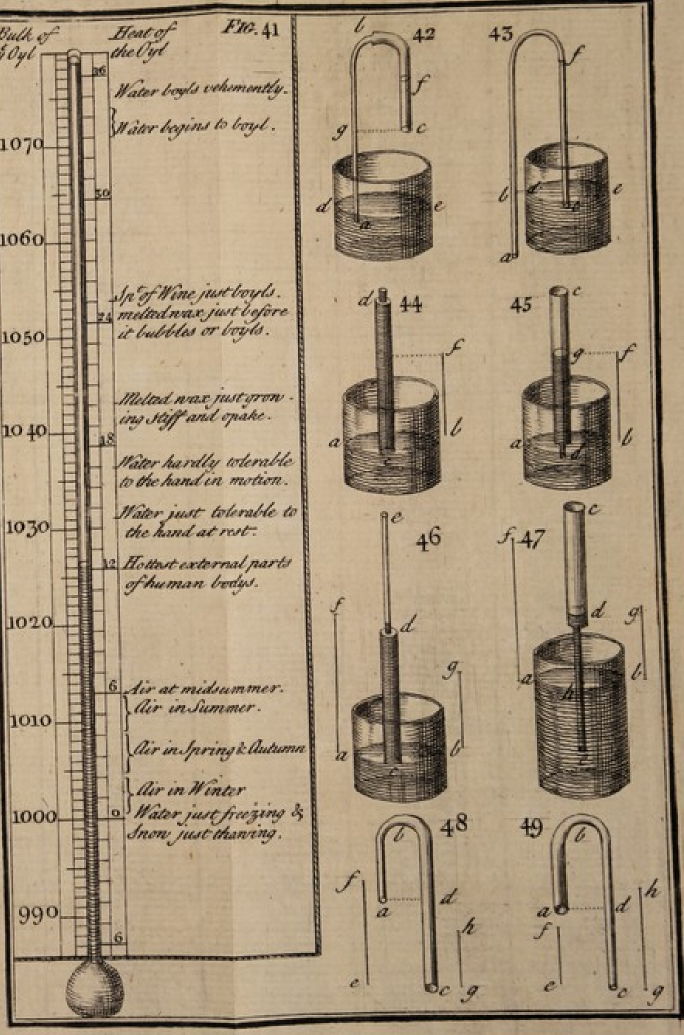
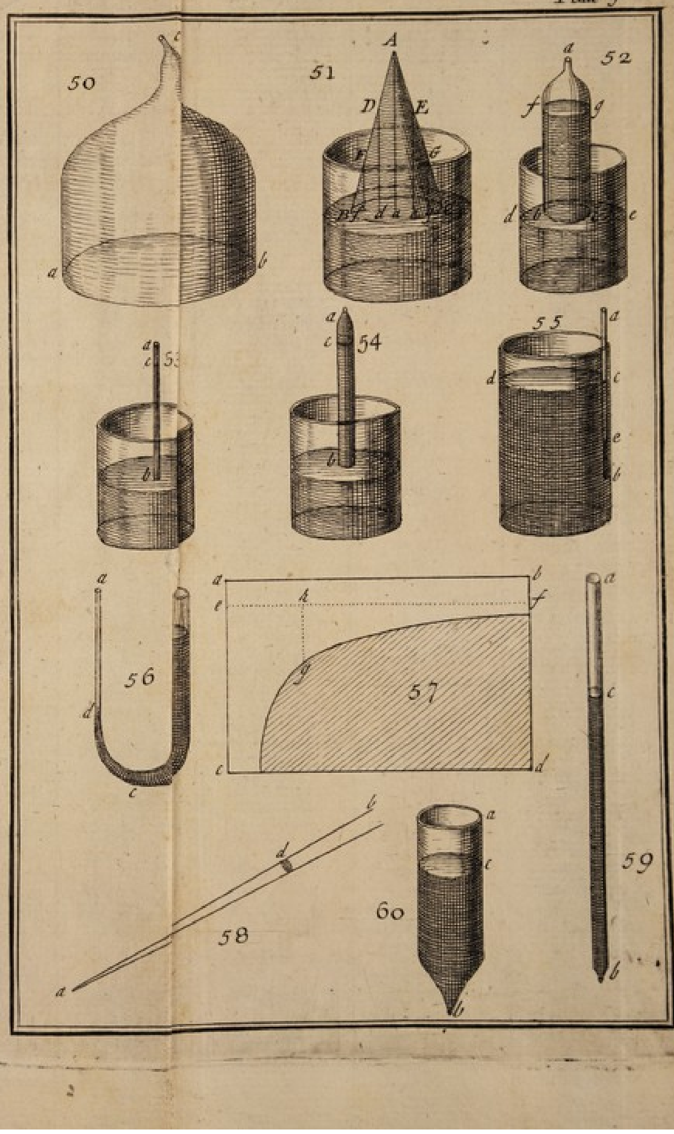


Bulk of ÿ Oyl
Heat of the Oyl
FIG. 41
Water boyls vehemently.
Water begins to boyl.
Sp.t of Wine just boyls.
melted wax just before it bubbles or boyls.
Melted wax just grow-ing stiff and opake.
Water hardly tolerable to the hand in motion.
Water just tolerable to the hand at rest.
Hottest external parts of human bodys.
Air at midsummer.
Air in Summer.
Air in Spring & Autumn
Air in Winter
Water just freezing & Snow just thawing.
1070
1060
1050
1040
1030
1020
1010
1000
990
36
30
24
18
12
6
0
6
42
43
44
45
46
47
48
49

Plate 5



[illegible] mercury [illegible]
[illegible] than mercury [illegible]
[illegible] as [illegible]
[illegible]
[illegible] distance [illegible]

The following experiments on the [illegible] the surface of glass.

[illegible] is brought [illegible] with [illegible] water when [illegible] is raised [illegible]

[illegible] of the [illegible] is the [illegible] on [illegible] paper.

[illegible] experiments it is evident [illegible] owing to the difference between the [illegible] [illegible] mercury [illegible] being always [illegible] that a particular explication is [illegible] [illegible] the following [illegible] with [illegible].

[illegible] brought [illegible] position the globule of mercury [illegible] the glass plane surface, which [illegible] increasing the [illegible] of the globule, and consequently [illegible] in its [illegible] the [illegible] of one another [illegible] [illegible] one another [illegible] [illegible] from the glass, [illegible] perfectly spherical, [illegible] the [illegible] contact with each other.

[illegible] we may believe, that the power [illegible] mercury is attracted either by glass, or by other mercury, is proportional to the [illegible] and therefore, [illegible] the [illegible] of mercury to glass, is not so strong as [illegible] to other mercury, [illegible] in this case a [illegible]